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Joppe de Ree
Eleonora Nillesen

Universiteit Utrecht

Utrecht School of Economics

Tjalling C. Koopmans Research Institute
Utrecht School of Economics
Utrecht University

Vredenburg 138
3511 BG Utrecht
The Netherlands
telephone +31 30 253 9800
fax +31 30 253 7373
website www.koopmansinstitute.uu.nl

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How to reach the authors

Please direct all correspondence to the first author.

Joppe de Ree
Utrecht University
Utrecht School of Economics
Vredenburg 138
3511 BG Utrecht
The Netherlands.
E-mail: j.deree@econ.uu.nl

Eleonora Nillesen
Wageningen University
Hollandseweg 1
6706 KN Wageningen
The Netherlands.
E-mail: eleonora.nillesen@wur.nl

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Joppe de Ree[a]
Eleonora Nillesen[b]

[a]Utrecht School of Economics
Utrecht University

[b]Wageningen University

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Abstract
This paper considers the impact of foreign aid on the risk of civil conflict. Previous studies on this topic have not properly addressed the problem of endogeneity between aid and conflict as well as the distorting influences of country specific time invariant effects. We propose GDP levels of donor countries as new and powerful instruments for foreign aid flows in the conflict regression. Aid flows are often defined as a fixed percentage of Donor's GDP hence they are strongly correlated. Changes in donor GDP constitute an exogenous shock to aid received by developing countries, in the sense that it is unrelated to the endogenous aid allocation process. Hence, the identification strategy does not pick up covariation due to aid rationing in the prelude to war. In addition, we condition on a number of macro factors to rule out other possible channels through which donor GDP affects conflict. We find a statistically significant and economically important negative effect of foreign aid on the risk of civil conflict. We estimate that a ten percent increase in foreign aid decreases the risk of civil conflict by six to nine percent using different specifications.

Keywords: Civil conflict, Foreign aid, Sub-Saharan Africa

JEL classification: D74, F35, O55

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1 Introduction

Between 1946-2002 not less than 1.37 million battle-related deaths occurred in 47 civil wars in sub-Saharan Africa (Lacina and Gleditsch, 2005). Civilian casualties resulting from civil wars even outnumber these figures by far. For example during the Rwandan genocide not less than 800.000 people were killed, foremost civilians. These statistics illustrate the importance of research trying to understand the causes of civil conflict.

Collier and Hoeffler (1998, 2004) were among the first to recognize the importance of economic factors in explaining conflict. Rebel organization may be regarded as businesses of some sort that indeed make efficient financing decisions in order to sustain its own viability. In that respect low per capita income, badly performing institutions, dependence on primary commodity exports are associated with increased risk of civil conflict. These studies are illustrative for the increasing interest to investigate a range of economic and political factors that may explain civil war occurrence.[1] Studying the effects of aid flows contributes to this line of research. The relevance of the subject is twofold. In accordance to the rebel-financing argument, aid resources may be a good 'prize' to capture by rebels thereby feeding instability. On the other hand, when in fact aid flows decrease the likelihood of conflict, development assistance may be a direct tool to break out of a conflict trap. The effects of foreign aid flows on civil war incidence have only incidentally been discussed, notwithstanding the fact that the average sub-Saharan African country receives a substantial 5% of official development assistance (ODA) as a percentage of GDP.

In this paper we present an empirical analysis of the effects of foreign aid flows on the risk of civil conflict in sub-Saharan Africa. We estimate a model that explains the incidence of conflict out of a measure of aid flows and a set of important controls, using an annual panel data set of 39 sub-Saharan African countries covering a period of 19 years.[2] We intend to improve on earlier work by Collier and Hoeffler (2002), by emphasizing the importance of fixed effects estimation and the use of instrumental variable techniques to control for

[1] Other empirical studies include those by Miguel, Satyanath, and Sergenti (2004a) who explore the causal impact of economic shocks on the incidence of civil conflict and find a negative statistically significant effect of economic shocks; Fearon and Laitin (2003) studying the effect of ethnic and religious diverse states on risk of civil war effect, and finding that not countries that are more ethnically or religiously diverse are associated with a higher risk of civil war but weak military states, characterized by low per capita income, large populations, rough terrain and political instability; and Hegre (2002) testing and confirming the theory that both solid democratic and harsh autocratic regimes are associated with less civil war than those that are considered to be at an intermediate level of democracy.

[2] We have averaged Official Development Assistance (ODA) relative to GDP over period $t-5$ to $t-1$ and took *log*'s.

the endogeneity of aid flows in the conflict regression. Our empirical results yield a decisive rejection of standard OLS or probit regressions and identifies a stabilizing role of foreign aid.

It is important to discriminate between indirect and direct effects of aid on conflict. Aid may impact conflict through its effects on economic conditions, as increasing income per capita is expected to decrease the probability of conflict when economic alternatives for potential rebels evolve and improve (Collier and Hoeffler, 1998). Moreover, increased income reduces a country's dependence on primary commodity exports which marginalizes looting opportunities for rebel groups and hampers their chances of survival (Collier and Hoeffler, 1998).[3].

One of the fundamental arguments for aid donation is to improve economic conditions. An extensive and body of literature studies the effectiveness of aid on economic growth. Much cited examples include the influential research by Burnside and Dollar (2000) who claimed that aid only works in countries with good policies, and reactions to this result by amongst others Collier and Dollar (2002), Easterly (2003) and Dalgaard, Hansen, and Tarp (2004). To date however empirical evidence is only tentative and has not led to consensus about how aid affects growth. Analogously, we do not know how aid affects conflict, through economic growth. In spite of some of the theoretical arguments above (i.e. higher income increases opportunity costs for rebels hence the probability of starting a fight will decrease; or aid increases income per capita leading to a lesser dependence on primary commodities) which intuitively make sense, the exact transmission mechanisms are not well understood. This paper does not assess the importance of these and other possible indirect channels through which aid might have an impact on conflict. We focus on empirical support for a direct effect instead. Indirect channels through which aid might affect conflict are controlled for in our regressions.

Studies focussing on direct effects of foreign aid on the risk of civil conflict builds on two fundamental hypotheses. The first hypotheses states that foreign aid augments the government's access to financial resources, thereby inducing rent-seeking behavior by rebel groups. The extended budget increases the rebels' incentives to try and appropriate the resources of the state. This mechanism proposes a destabilizing effect of foreign aid flows as it increases the risk on conflict. Examples of models that have been developed and analyzed following this line of argument include those by Grossman (1991, 1992, 1999).

The second hypothesis claims that aid flows relax the government budget

[3]Note however that Fearon (2005) shows that this result is not robust, and, to the extent that there is an effect, this only holds for those countries where oil production comprises a large share in primary commodity exports

constraint, such that government military expenditure, assumed to be a normal good, increases. A forceful government army then discourages rebel groups from pursuing a violent course, such that foreign aid, as a result, decreases the risk on civil conflict. Note that the validity of this argument is conditional on the assumption that aid is sufficiently fungible into military expenditure. Devarajan, Rajkumar, and Swaroop (1999) examine the extent of aid fungibility in sub-Saharan Africa and indeed find supporting evidence for this assumption. Collier and Hoeffler (2002) develop a model formalizing the idea of an increased government budget, translating into increased military spending, but fail to find empirical support for their theory. Our paper does provide empirical evidence that supports the argument of Collier and Hoeffler as we improve on their identification strategy, by recognizing the importance of fixed effects and other endogeneity issues.

The objective of our paper is to identify and estimate a causal effect of foreign aid flows on the risk of conflict. Identification of causal effects typically hinges on specific sets of over-identifying restrictions on the data. Some of these restrictions can be easily tested, but others cannot, and are therefore genuine assumptions. The identification of our objective is no different in this respect. As we are never certain but merely convinced about a set of assumptions, we report the estimates of four different models that produce causal parameters under four different sets of identifying assumptions. Moreover we perform some robustness checks on our fourth and preferred specification.

The identification of a causal effect from the empirical specification in Collier and Hoeffler (2002) depends on strong exogeneity assumptions on all regressors that, to our belief is highly improbable. We consider the endogeneity problem regarding aid flows to be threefold. First, we believe that country-specific unobserved factors will affect both aid flows and the risk of civil war. This may include all kinds of unobserved measures of grievances, norms or institutions. Second, we address the endogenous allocation process of aid in the aid and conflict relationship. It is likely that time varying unobserved factors affect both the amounts of aid received as well as the risk of conflict. For example, the temporary presence of a 'bad leader' often urges donors to ration aid flows while it simultaneously increases the likelihood of conflict. A third type of endogeneity, which is in fact closely related to the 'bad leader' issue is simultaneity. Donors are likely to reduce monetary aid in countries actually experiencing civil conflict, and allocate aid flows elsewhere. Not appropriately controlling for the endogenous aid allocation process leads to spurious inference when the suggested negative correlation is wrongly interpreted as a risk reducing effect of aid on civil conflict. Collier and Hoeffler (2002) recognize this and have used lagged aid flows in their attempts to overcome the simultane-

ity problem. We consider this strategy not to be satisfactory because donors are likely to anticipate increasing possibilities of future conflicts and adjust aid flows well before war breaks out.[4] Strong persistence in the residuals of the conflict regressions supports the anticipation argument and invalidates this identification strategy.[5]

We propose 'Donor GDP' as a new and powerful instrument for foreign aid flows in the conflict regression. Aid flows are often defined as a percentage of Donor's GDP, hence both are strongly correlated. Almost by definition, changes in donor's GDP is unrelated to the endogenous aid allocation process on the recipient country level. However, donor's GDP may affect conflict through other macro-related channels that itself are related to conflict. We solve for these potential violations of the exclusion restriction by including variables as oil prices, measures of trade and a dummy for the cold-war years. The main empirical finding is as follows: foreign aid is directly affecting the incidence of civil conflict in sub-Saharan Africa negatively and significantly. A ten percent increase in foreign aid decreases the probability of civil conflict by about six to nine percent.

2 Data description

We use data on civil conflict from the Armed Conflict Database, recently developed by the Peace Research Institute Oslo (PRIO) and the University of Uppsala, henceforth referred to as the PRIO/Uppsala data set.[6] Work on the data set was supported by The World Bank's Development Economics Research Group as part of its project on The Economics of Civil War, Crime, and Violence. The data set has been widely used since it was made available, for example by Miguel, Satyanath, and Sergenti (2004a). The PRIO/Uppsala data set defines civil conflict as 'a contested incompatibility which concerns government and/or territory where the use of armed force between two parties, of which at least one is the government of a state, results in at least 25 battle-related deaths.'[7] A dummy variable that is unity in case of civil conflict is the primary dependent variable in the regression analyses. About 26% of our observations report

[4] In econometric terms: aid flows should not be regarded as a predetermined variable.

[5] Collier and Hoeffler (2002) do not report estimates on residual autocorrelation. They do, however, use clustered standard errors suggesting that residuals may be autocorrelated. The presence of residual autocorrelation would invalidate their identification strategy as they state that current aid flows are correlated with current errors.

[6] See Gleditsch, Wallensteen, Eriksson, Sollenberg, and Strand (2002).

[7] Note that although often used interchangeably, according to the definition in the PRIO/Uppsala data set 'civil wars' are different from 'civil conflicts' as 'conflict' also includes minor and intermediate conflicts with a threshold of at least 25 but fewer than annual 1000 battle-related deaths, whereas the term 'civil war' refers only to those cases where there are at least 1000 annual battle-related deaths (Lacina and Gleditsch, 2005).

internal conflicts of this type, whereas 16% of our observations report internal wars with over 1000 annual deaths.[8]

The primary explanatory variable is foreign aid, measured as official development assistance (ODA) in proportion to GDP.[9] This measure reflects the magnitude of aid flows relative to other resources at a governments disposal. We calculate the foreign aid variable as a five-year average of official development assistance flows relative to recipient GDP up to period $t-1$. Both quantities are measured in current US$. We have constructed *log*'s of the ratio such that the estimated coefficients are elasticities.[10]

GDP data that we use as instruments for aid flows in the conflict regression we have drawn from the Penn World Tables. We have constructed *log*'s of five year averages of donor GDP measured in current US$, to instrument the *log* of the five year average of aid to GDP ratio of the recipient country. We are using current instead of real GDP as an instrument which is the appropriate measure to instrument a ratio of which both factors are measured in current US$.

The remaining data includes a set of country control variables similar to those used by Collier and Hoeffler (2002); Miguel, Satyanath, and Sergenti (2004a). Data on controls are drawn from Fearon and Laitin (2003); Miguel, Satyanath, and Sergenti (2004a); the Penn World Tables and the World Development Indicators. We control both to cover indirect channels through which aid affects conflict, and to rule out potential violations of the exclusion restrictions on the instruments. Control variables include: a variable 'peace duration' measuring the number of peace years up to period $t-1$ (using the PRIO/Uppsala data on conflict dated back to 1960); a ratio of primary commodity exports to GDP (both linear and squared) to proxy natural resource dependence; the *log* of real per capita income measured at $t-1$; measures of democracy calculated from the Polity IV data set; ethnolinguistic and religious fictionalization; oil exports measured as a percentage of total merchandize exports [from (Miguel, Satyanath, and Sergenti, 2004a)]; the *log* of the proportion of a country that is mountainous; the *log* of the national population measured at $t-1$; a linear time trend; trade as a ratio of total GDP (only included for a robustness check, because this variable is not available for all observations); a dummy that is unity in the cold war years; and oil prices measured in 1982 US$. For the 2SLS

[8] Using either one of these variables as the dependent variable in the regression does not change results quantitatively (results not shown). The results presented in table (2) are based on the the civil conflict variable with at least 25 annual deaths.

[9] 'Official Development Assistance relates to aid flows originating from countries belonging to the OECD Development Assistance Committee, including grants or loans to developing countries undertaken by the official sector, with the promotion of economic development and welfare as the main objective at concessional financial terms excluding grants, loans and credit for military purposes'(OECD, 2006).

[10] When we include the aid to GDP ratio, without taking *log*'s, as one of the explanatory variables, the results are qualitatively similar. This parameter however, cannot be interpreted as an elasticity.

Figure (1A) reveals no obvious relationship between aid flows and the conflict rate in averages. In fact, the between correlation the aid variable and the conflict rate is close to zero and insignificant. Excluding outliers (countries that receive over 20% of ODA as a percentage of GDP) still yields an insignificant correlation. To us the absence of significant between correlation is a result of the extreme complexity of the matter. Both the aid-giving procedures, and particularly the arise of civil conflict are subject to many influences such that correlation in averages are easily blurred with noise. Neglecting the influence of these unobserved forces is generally a source of bias as the unobserved forces are likely to affect both aid flows and conflict.

Figure (1C) shows a negative but insignificant correlation between GDP and the conflict rate. Dropping South-Africa as being a typical outlier(figure (1D)), however identifies a negative correlation that is significant at the 5% level.[11] It suggests that that poor countries are experiencing more conflict than rich countries (correlation coefficient: -0.32 and significant at the 5% level). Clearly this fact alone is not conclusive for causality, but it surely is plausible as better economic conditions provide more and better alternatives to fighting. The possibility that the direction of causality is reversed however should not be overlooked.

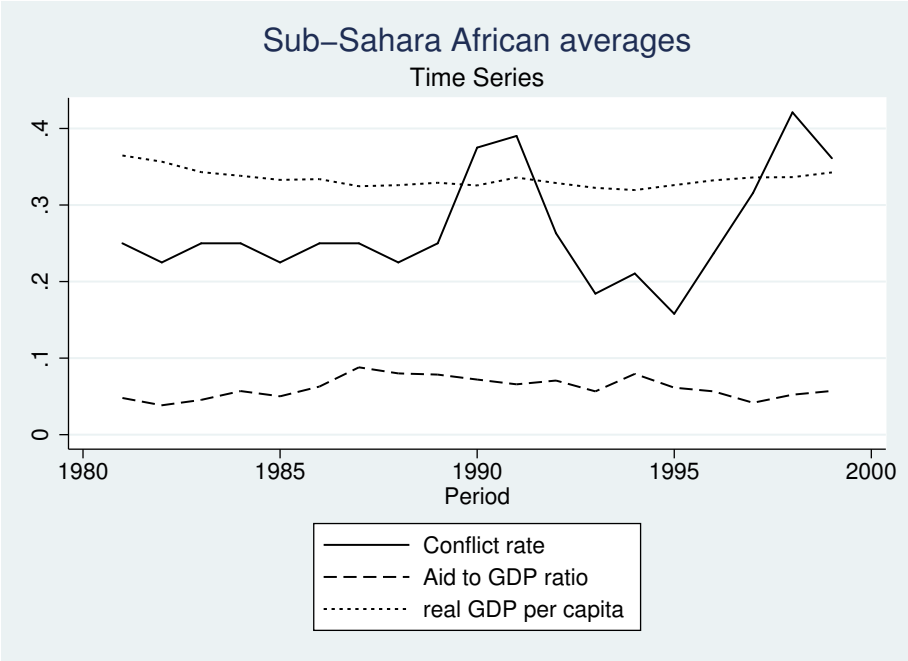
Figure (2) presents averages over time for the whole sub-Saharan African region and shows other interesting patterns. GDP per capita is presented as a ratio to three times the sub-Saharan African average. This transformation generates a measure of GDP such that the average value of the series is of same order of magnitude as the other two series.

The average aid to recipient-GDP ratio has grown on average over the sample period 1981-1999. The data however suggests some stabilization of the ratio after 1990. Stabilization of this ratio could be the result of aid and recipient GDP both fluctuating at a similar rate, but as GDP (in real PPP adjusted terms) stays fairly constant this explanation is ruled out. Conflict rates were relatively stable in the eighties after which there is one major fluctuation in the nineties. After a peak around the end of the cold war, the sub-Saharan conflict rate decreased to less than 20% from which it grew steadily to about 40% in 1999.

In conclusion both figure (1) and figure (2) reveal no evident relationship between aid flows and the conflict rate in averages. If there is a relationship between aid and conflict it should be identified from the within country variation over time. Fortunately, there is considerable variation of this type. The within country (unconditional) standard error of the aid to GDP ratio is about 0.03 such that on average 95% fluctuates between +/- 0.06 of the country spe-

[11] South-Africa is relatively rich, but has serious problems with civil conflict arising from the Apartheid regime.

Figure 2:



cific mean. Despite the absence of between country correlation, within country correlation between aid and conflict is significantly negative.[12] The negative within country correlation is difficult to interpret as we have merely removed the fixed effect. In the next section (3) we present our solutions to solving the other major problems concerned with identifying causal, interpretable, effects.

3 The Empirical Analysis

We define a model to explain the incidence of conflict:

$$C_{it} = 1\left(\beta_1 A_{i,\overline{t-5}} + \beta_2 PD_{it} + X'_{it}\beta_3 + (\alpha_i + \varepsilon_{it}) > 0\right) \tag{1}$$

The dichotomous conflict variable C_{it} equals unity when the right hand side passes a threshold, here normalized to zero. The explanatory variables include our measure of $A_{i,\overline{t-5}}$, which is formally defined as the *log* of a five year average of the aid to GDP ratio:

$$A_{i,\overline{t-5}} = \ln\left(\frac{1}{5}\sum_{l=1}^{5}\frac{Aid_{it-l}}{GDP_{it-l}}\right) \tag{2}$$

[12] An OLS regression of the conflict measure on the aid measure and a set of country dummies yields an estimate associated with the aid measure which is negative and significant at the 5% level.

'Peace duration' PD_{it}, measures the number of peace years up to period $t-1$. Equation (1) represents a dynamic model because peace duration is a function of lagged conflict variables. The dynamic nature of the model is important for the choice of the econometric specifications. X_{it} represents the set of important, possibly endogenous control variables. The error term consists of a time invariant effect α_i and a time varying effect ε_{it} which is possibly autocorrelated.

The primary objective throughout the remainder of the empirical analysis is to estimate a causal effect from aid flows on the conditional possibility of conflict. The objective is formally notated as follows:

$$\frac{\partial}{\partial A_{i,\overline{t-5}}} P\left(C_{it}=1|\Omega_{it}\right) \tag{3}$$

The choice of our set conditioning variables (= Ω_{it}) depends on specific exogeneity assumptions, and varies across specifications.

To estimate parameters we rely on a linear probability specification of equation (1).

$$C_{it} = \beta_1 A_{i,\overline{t-5}} + \beta_2 PD_{it} + X'_{it}\beta_3 + (\alpha_i + \varepsilon_{it}) \tag{4}$$

By adopting a linear probability model (LPM) we give up on using more sophisticated, but computationally burdensome probit type specifications. Moreover, equation (1) represents a dynamic model including a fixed effect, such that standard probit modeling does not apply. Using a similar model as we do, Hyslop (1999) shows that linear specifications produce rather similar outcomes than probit/logit specifications[13]. Informally, linear probability models assume that the conditional expectation function of conflict, conditional on the relevant explanatory variables is a linear function. Probit/logit and LPM's often produce rather similar outcomes, because the conditional distribution function 'looks' rather linear around its expected value. Typically, most of the draws from any conditional distribution function lie within reasonable distance from the expected value.

3.1 Four Regression Models of Conflict

We estimate four different versions of equation (4). All four regressions produce causal parameter estimates under four different sets of assumptions (see table (2) for the regression results). We basically adopt a step-by-step strategy as we move from an implausible set of assumptions to, in our view, the most plausible set of assumptions. In practice this means that we attempt to get rid of the endogeneities that distort the regression results of the most simple

[13]While our model and the model used by Hyslop (1999) are similar, Hyslop (1999) studies labor force participation.

model. The first model (OLS(1)) estimates equation (4) with OLS. This specification is similar to the specification estimated by Collier and Hoeffler (2002).[14] OLS(1) produces qualitatively similar outcomes as Collier and Hoeffler (2002) and serves as a benchmark to interpret the results of the three subsequent models. The similarity between results indicate that our data has similar properties as Collier and Hoeffler's. This fact adds to our confidence in the reliability of our data set. In consequence the results obtained from the three subsequent regression models stem from differences in specification rather than from specific features of the data.

In Section (1) and (2) we have briefly motivated that neglecting fixed factors and other endogeneity issues in regressions is likely to produce inconsistent estimates of causal effects. OLS(1) would yield inconsistent estimates because the unobserved country effect α_i are likely to be correlated with (some of) the explanatory variables. OLS(2) controls for these fixed factors by including country dummies. In 2SLS(3) and 2SLS(4) we instrument economic growth, GDP and aid measures with rainfall measures and 'Donor GDP' respectively. 2SLS(3) and 2SLS(4) differ in ways of dealing with the fixed effect. In 2SLS(3) we have included country dummies in the regression, whereas in 2SLS(4) we adopt a typical dynamic approach by first differencing equation (4).

3.1.1 The OLS Regressions

The results of the two OLS regressions are presented in the first two columns of table (2). From OLS(1) we infer no significant association between our measure of aid and the incidence of civil conflict. Conflict is however strongly negatively associated with peace duration and real per capita income, and nonlinearly with dependence on primary commodity exports. These results mirror Collier and Hoeffler's findings (Collier and Hoeffler, 2002), with the exception that the association with primary exports dependence is different. We find the non-linearity to be reversed relative to Collier and Hoeffler's outcomes, as our estimates indicate a U-shape, instead of the (expected) inverted U-shape. Besides, our relationship is much weaker than the effects found by Collier and Hoeffler (2002). Differences in results may be explained by differences in data. Collier and Hoeffler (2002) focus on world data and the onset of conflict, while we use a sample of sub-Saharan African countries and the incidence of conflict.

The OLS(1) parameter estimates are consistent estimates of the causal effects defined by definition (3) under the following set of assumptions:

$$E\left[\alpha_i + \varepsilon_{it} | A_{i,\overline{t-5}}, PD_{it}, X_{it}\right] = 0 \tag{5}$$

[14] Using probit as Collier and Hoeffler do instead of a linear probability model does not change the results much. (results available upon the authors request.)

Because of the dynamic nature of our model, the above assumption is quite restrictive as, amongst other things, it implies that $\alpha_i = 0$ and that the ε_{it}'s are not autocorrelated. Under assumption (5) any persistence in unobserved causes of conflict is ruled out. The validity of assumption (5) can be easily tested by a simple t-test on residual autocorrelation. Table (2) shows that the null hypotheses of no residual autocorrelation is strongly rejected. Due to the autocorrelated disturbances, 'peace duration', which is a function of lagged dependent variables, will be correlated with present error term, such that the estimates of all model parameters are consequently biased.

Additionally, it is most likely that due to residual autocorrelation, lagged aid flows, lagged GDP and lagged growth lose their exogenous status as well. It is typically accepted that current measures of aid and GDP are correlated with current errors (i.e. donors ration aid flows in times of war). Errors that are strongly correlated over time are more than likely to introduce correlation between lagged aid, GDP and growth measures and the current errors, which in turn violates assumption (5). We conclude that the significant findings of OLS(1), although maybe intuitive, should be interpreted as (conditional) correlations rather than causal effects.

There are two alternative explanations for the autocorrelated residuals from OLS(1). First, the ε_{it}'s are correlated over time and capture for example the come and go of a strong rebel leader. Second, one country is just more likely to ending up having conflict than another, because of differences in unobserved fixed country specific characteristics (e.g. legal and political systems, size, culture etc.). The problem is of course that these unobserved factors also correlated with aid flows. When autocorrelation is highly persistent it seems reasonable to account for these unobserved fixed factors.

OLS(2) includes country dummies to control for fixed effects, while simultaneously making an effort to eradicate residual autocorrelation. The fit of the model increases dramatically from 35% to 58%, indicating the importance of fixed factors in explaining conflict. Unlike OLS(1), OLS(2) identifies a small, but significant association between aid and conflict. The strong correlations between GDP and the dependence on primary commodity exports on the other hand, are no longer visible. This either suggests that there is no such effect, or that the within country covariation of GDP and conflict is not strong enough to identify it. We do not draw any premature conclusions on this subject and hold on to the latter interpretation.[15]

For a causal interpretation of the OLS(2) estimates the following strict exo-

[15] Between country covariation of GDP and conflict is not employed when country fixed effects are included in the regression. The absence of a "visible" effect of poverty, here characterized as having low levels of GDP, on conflict may therefore be attributed to the lack of within country variation.

geneity assumption should hold:

$$E\left[\varepsilon_{is}|A_{i,\overline{t-5}}, PD_{it}, X_{it}, \alpha_i\right] = 0 \qquad \forall t, s \tag{6}$$

Strict exogeneity is a necessary assumption, because the α_i's are unobserved and need to be estimated by including country dummy variables.[16] When the number of time periods T is large enough and the ε_{it}'s are not autocorrelated assumption (6) may be replaced with a much weaker assumption:

$$E\left[\varepsilon_{it}|A_{i,\overline{t-5}}, PD_{it}, X_{it}, \alpha_i\right] = 0 \tag{7}$$

While one might argue that $T = 19$ is 'large enough' to accept that assumption (7) is sufficient for causality, the dummies did not account for all residual autocorrelation. Analogue to OLS(1), an autocorrelation test on the residuals of OLS(2) exposes large and significant residual autocorrelation (see table 1). As a result, both assumption (6) and (7) are violated because peace duration being a function of lagged dependent variables is correlated with current errors.

Evidently, by including dummies in the regression we have only partially solved the endogeneity problem. The significant residual autocorrelation suggests a dynamic relationship between economic output variables, foreign aid flows and the incidence of conflict. Rebel uproar or the temporary presence of malfunctioning governments in one of the recipient countries, captured by ε_{it}, indicates increased possibility of future conflict. Donor countries observe this and respond, by decreasing aid flows. If war indeed breaks out the observed negative correlation would be wrongly interpreted as a causal effect. Lagging aid flows or economic output variables does not solve this problem as the unobserved factors are highly persistent over time (even after correcting for fixed effects). When the described endogenous mechanism is true, it is another important reason why assumption (7) is violated and OLS(2) yields biased estimates of the parameters of interest.

Omitted variables or reversed causality issues in the aid and conflict relationship obscure the interpretation of the parameter estimates in OLS(1) as well as in OLS(2). It is likely that output or growth measures interact in a similar way as these measures also deteriorate in case of increased rebel activity or when conflict sets in. This mechanism produces negative correlations between lagged output variables and the present error term in the OLS regression models. We attempt to solve these problems using instrumental variable techniques.

[16] Including dummies in the regression is equivalent to estimating the within transformed model.

3.1.2 The 2SLS Regressions

When the exogeneity assumptions (5) and (6) are violated we enter the world of Instrumental Variable (IV) techniques. We have argued that at least two endogeneity concepts are likely to disrupt the interpretability of the OLS(1) and OLS(2) outcomes. First, the omitted variables problem as unobserved time varying factors such as the presence of bad leaders affecting both aid flows and the likelihood of conflict. And second, the issue of simultaneity (i.e. reversed causality), when aid flows are rationed when countries are in conflict. IV techniques are used to treat the endogeneity infected relationships.

Both omitted variables and simultaneity are similar concepts in the sense that they both invalidate assumptions (5) and (6) and receive the same treatment. However, the interpretation of the model parameters depends on the type of endogeneity. The simultaneity concept is associated with models that consist of a system of equations. One equation explains conflict and another one that explains aid flows. Both aid flows and conflict are then simultaneously determined within the system. When simultaneity is important we are basically estimating one equation (i.e. equation (1)) of a system of equations, such that only a subset of parameters of the complete system is identified. To obtain for example the size of the effect from conflict on aid flows we would need additional information. Estimating the parameters of equation (1) merely yields the 'immediate' effect from aid on conflict, but leaves 'general equilibrium' effects unspecified. When on the other hand the omitted variable problem is the only source of endogeneity, the model is fully identified using the extra information gained from the instruments. The interpretation of the parameters is therefore straightforward in the sense of equation (3).

To deal with the endogeneity problem of aid in the conflict regression we propose GDP levels of large western donor countries as instruments. The intuition behind this idea is that the inflow of aid to the whole sub-Saharan region on average is a linear function of donor's GDP, as donor countries attempt to commit to a constant fraction of GDP for development assistance in total, this being independent of any war in any individual recipient country. The distribution among recipient countries however is subject to (time varying) state of affairs in recipient countries, such as the appearance of a strong rebel leader, and is therefore endogenous. Potential instruments that measure characteristics of specific recipient countries, and are somehow related to aid flows, suffer from the important drawback that they are quite easily correlated with some of the unobserved factors causing conflict, and should be selected with great caution.

As a baseline case we report the estimation result of the regressions using the GDP of the United States, which is by far the largest donor in absolute

terms. As a robustness check we report the estimation results when GDP levels of European donors are used (section (3.1.4)). To our knowledge, using donor GDP as an instrument for foreign aid flows is new. We estimate two IV models that are different in how they control for fixed effects. 2SLS(3) includes country dummies in equation (4):

$$C_{it} = \beta_1 A_{i,\overline{t-5}} + \beta_2 PD_{it} + X'_{it}\beta_3 + D'_i\gamma_i + \varepsilon_{it} \tag{8}$$

D_i is a country dummy that is unity for country i. 2SLS(4) first differences equation (4), such that α_i, the country fixed effect drops out:

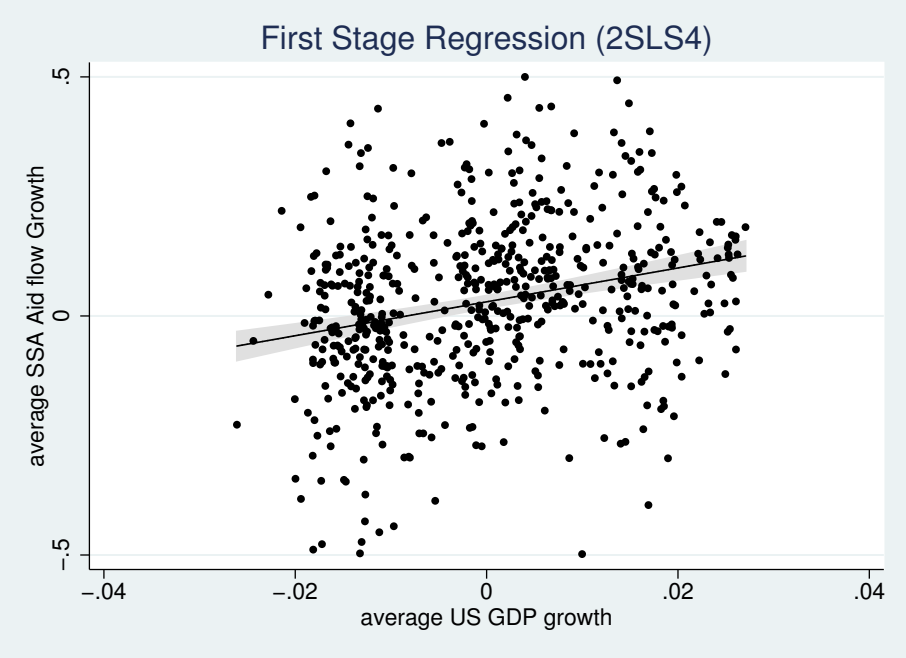
$$\Delta C_{it} = \beta_1 \Delta A_{i,\overline{t-5}} + \beta_2 \Delta PD_{i,t} + \Delta X'_{it}\beta_3 + \Delta\varepsilon_{it} \tag{9}$$

The essence of IV regression is to use information (the instrument) that is affecting the dependent variable only through its effect on the endogenous regressor. A good instrument should therefore satisfy two properties: with all other important variables controlled for, the instrument should be correlated with the endogenous regressor (i.e. good first-stage explanatory power), and uncorrelated with the error term of the model (i.e. the exclusion restriction should not be violated).

Good first stage explanatory power in the above two models simply means that donor's GDP is partially correlated with our measure of aid (i.e. correlation after controlling for other factors affecting conflict X_{it} or ΔX_{it}). On 24 October 1970, the United Nations General Assembly adopted Resolution 2626, The International Development Strategy for the Second United Nations Development Decade. With this resolution, developed countries agreed to increase their official development assistance to developing countries to a level equivalent to 0.7% of their Gross National Product at market prices, and to do their best to meet these goals by 1975 (see paragraph (43) of the resolution). Even though just a small group of countries has ever reached the 0.7% it is likely that aid flows and donor GDP levels are correlated. Table (1) reports the first stage regressions of 2SLS(3) and 2SLS(4). U.S. GDP has strong partial explanatory power on the aid to GDP ratio of sub-Saharan African countries (the F-statistics are well above 10 in both model 2SLS(3) and 2SLS(4)). The partial correlation between U.S. GDP levels and the aid to GDP ratio's of SSA countries is graphically represented in figure (3). The clear positive relationship between donor's GDP and the aid flows summarizes the first requirement for a good instrument.

Satisfying the exclusion restriction is the second necessary requirement for a good instrument. In practice this means that the error term of the model

Figure 3: Positive partial correlation between the change in *log* average U.S. GDP and the change in the *log* of the average aid to GDP ratio in sub-Saharan African countries. (Figure displays a 95% confidence band around the linear fit)



should be uncorrelated with the instruments. However, to infer causality we need the stronger assumption of mean independency. For both models 2SLS(3) and 2SLS(4) we write down these familiar conditions for causality. For 2SLS(3) we need:

$$E\left[\varepsilon_{is}|DGDP_{\overline{t-5}}, PD_{it}, X_{it}, \alpha_i\right] = 0 \qquad \forall t, s \tag{10}$$

For 2SLS(4) we need:

$$E\left[\Delta\varepsilon_{it}|\Delta DGDP_{\overline{t-5}}, C_{it-1}, \Delta PD_{it-1}, \Delta X_{it}\right] = 0 \tag{11}$$

$DGDP_{\overline{t-5}}$ is a vector of levels of donor GDP and is defined analogous to our measure of aid: we have averaged GDP of a donor country from period $t-5$ up to $t-1$ and calculated *log*s.

As usual, the conditions (10) and (11) are assumptions that allow us to interpret the parameter estimates as causal effects. However, the likelihood that these conditions hold depends greatly on the variables that are included in the vector X_{it} (or ΔX_{it}). To address this issue we have considered a large number of potentially important conditioning variables that if neglected, could violate the above conditions and disrupt the interpretability of the results. We

have included measures of GDP, growth, dependence on primary commodity exports, dependence on world oil prices (when the country is an oil exporter), a dummy for the cold war years and a time trend. Measures of trade flows would be another factor that could be related to U.S. GDP, and in one way or another, to conflict. Due to lack of available data on measures of trade data we did not include it in the baseline model. We have included measures of trade as a robustness check, but it is not changing the results.

A strong argument in favor of our instrument is that it is unlikely to be systematically related to recipient country properties that affect conflict, such as rebel movement or sudden instances of poverty, draught etc. (These unobserved factors are extremely persistent[17] and cause a lot of trouble in the OLS regressions, but it also limits the choice for a instrument that is specific to recipient countries). The strength however, comes at a cost. As our instrument is not specific to recipient countries it is impossible to include time dummies to capture world wide (or sub-Saharan African wide) changes to the incidence of conflict that are directly related to donor's GDP and are not captured by any of the conditioning variables.

Many have argued that at least two of the important conditioning variables, i.e. GDP growth and GDP per capita, are likely to be endogenous as well. We need however, to include these variables because of their dynamic interplay with conflict and aid flows. We draw upon Miguel, Satyanath, and Sergenti (2004a) by instrumenting lagged economic shocks with shocks in rainfall. This strategy successfully identified the impact of economic shocks on the incidence of civil conflict. Rainfall shocks are credible instruments for economic growth in sub-Saharan Africa for two reasons. First, countries in sub-Saharan Africa are heavily dependent on rainfed agriculture, and second, agriculture forms a major component of the countries' gross domestic product.[18] For a description of the rainfall data and a more detailed discussion of the validity of the instrument the reader is referred to Miguel, Satyanath, and Sergenti (2003) and Miguel, Satyanath, and Sergenti (2004a).

Whereas Miguel, Satyanath, and Sergenti (2004a) employs rainfall shocks to instrument economic shocks, we instrument levels of GDP with levels in rainfall as well. For our research purpose it is important to control for GDP levels to exclude potential indirect channels through which aid affects conflict. To estimate the model parameters consistently we need to instrument both aid flows and GDP levels. Unlike rainfall shocks, rainfall levels are easily correlated with geographical positioning or other important fixed factors, which

[17] We find that these factors exhibit (close to) unit root type dynamics

[18] The median contribution of the agricultural sector in sub-Saharan Africa is 28% of GDP (Worldbank, 2006).

themselves may be factors in explaining conflict. To consider rainfall levels as instruments we therefore need to condition on country effects, which we do in 2SLS(3) and 2SLS(4). Consequently the parameters of the model are identified by the within country variation in rainfall, which is uncorrelated with fixed factors by construction.

3.1.3 Empirical Results

2SLS(3) yields a significant negative relationship of the aid to GDP ratio with an estimated elasticity of -.25. We also report a significant negative effect of lagged growth of -2.1, replicating earlier results of Miguel, Satyanath, and Sergenti (2004a). Like in OLS(2), including dummies does not eliminate the residual autocorrelation, such that peace duration is correlated with the errors. Moreover, including dummies introduces a new source of bias, due to small sample correlation between the peace duration variable and the within transformed error term that is known as Nickell-bias (Nickell, 1981).[19] With having a 'standard' lagged dependent explanatory variable in the model, this bias is of order $1/T$ with T being the time dimension in years. With relatively weak residual autocorrelation the Nickell-bias typically vanishes when T gets large. Peace duration differs however from 'standard' lagged dependent variables, as it is constructed out of a potentially much larger set of lagged conflict variables. Moreover, the extreme persistence in the errors induces bias that is no longer of order $1/T$, but of a larger unknown order.

The autocorrelated residuals as well as the Nickell bias, emphasize the need for a more suitable estimation technique for dynamic models. The typical strategy to control for fixed effects in a dynamic model is to first-difference equation (4). Subsequently, we instrument the change of the peace duration variable with its own lags and lagged conflict dummy-variables to eradicate the Nickell-type bias. 2SLS(4) in table (2) shows the results of the first difference estimation. The estimates differ markedly from the OLS(1), OLS(2) and 2SLS(3) regressions and the results of Collier and Hoeffler (2002). 2SLS(4) shows a significant and clear-cut negative effect of foreign aid on the incidence of conflict that is economically important. A ten percent increase in aid relative to GDP decreases the probability of conflict by about nine percent. While Collier and Hoeffler (2002) find only indirect effects of aid on conflict (through its effect on output), we show that after correcting for various endogeneity issues, aid has an important direct effect as well.

Autocorrelation in the changes in ε_{it} is ruled out by assumption (11) and

[19] This is a problem for OLS(2) as well.

proposes a testable restriction on the residuals.[20] We have tested for residual autocorrelation and were note able to reject.[21] The test shows that changes in ε_{it} are uncorrelated, which is another important result of our paper. When $\Delta\varepsilon_{it}$ is not autocorrelated, ε_{it} itself exhibits (close to) unit root type of dynamics. Even after correcting for fixed effects, unobserved factors such as bad leaders or increased rebel activity are extremely persistent. This fact should not be neglected in regression analysis. In practice this finding has two implications. First, lagging aid flows, and GDP measures in level equations is not useful, unless you believe that these variables are strictly exogenous. Second, the Nickell bias is no longer of order $1/T$, but of a much larger unknown order. Even for relatively large panels as ours, controlling for fixed effects with dummies (or within transformation) generates important biases.

The effects of the Nickell bias are revealed through the significant positive parameter on peace duration in 2SLS(4). Nickell shows that when the true parameter associated with the lagged dependent variable is positive, the bias introduced by within transformation is negative (Nickell, 1981). This implies that when in fact peace duration is positively related to the risk of conflict, regression of the conflict variable on the peace duration variable easily reveals a negative estimate. This result may seem counterintuitive at first, because it sharply contrasts with most empirical findings in the conflict literature [See for example Collier and Hoeffler (2004) and Fearon (2005)]. A positive relation between conflict and peace in the preceding years could however be explained by a phenomenon called 'strategic retreat' meaning that rebel groups use periods of peace or latent conflict to get organized. This is a conceivable interpretation as rebel groups nowadays do not differ much from professional organized crime, hence the recruitment process and actual set up of a rebel group requires great organizational efforts which are more likely to succeed when countries are in relative peace than at war.[22]

3.1.4 Robustness Checks

We have verified the robustness of the 2SLS(4) results by using different subsets of our instruments and by including measures of trade in the regression. We have included different combinations of GDP series of four major western donors. Table (1) and table (2) reports results using the U.S. GDP series to instrument aid flows.

[20] if $\Delta\varepsilon_{it}$ would be autocorrelated PD_{it-1} and C_{it-1} would not be valid instruments.

[21] When this assumption would have been rejected, Hyslop (1999) proposes a simple strategy to account for residual autocorrelation.

[22] See for example Gates (2002) who presents a model derived from the economics of organized crime to investigate rebel recruitment and allegiance.

Using GDP levels of three large European aid donors (France, Germany and the Netherlands) show a similar picture. The size of the parameter estimate on our aid measure decreases a bit to -.7, but remains significant at the 5% level. Still the Hansen-J test is not indicating endogeneity of our instruments. Moreover, the Cragg-Donald test rejects under-identification at the 5% level. Including both U.S. GDP and the European GDP series in the instrument list yields similar results, but the estimated effect of aid flows becomes -.6. The fact that the estimated coefficient is decreasing in the number of instruments may result from a well known finite sample property of IV estimation. IV estimators are biased toward the OLS estimators, with the size of the bias increasing in the number of over-identifying restrictions (see for example (Angrist, Imbens, and Krueger, 1999)). Still, the negative sign of the effect is preserved using different sets of instruments.

Furthermore we have included lagged measures of trade as this is another potential channel through which GDP of donor countries may influence the incidence of conflict. Trade measures are insignificant in the conflict regression and do not change the results qualitatively. The results of these regressions are not reported as about 15% of the observations were lost due to missing data.

4 Conclusion

Our analysis shows that when endogeneity issues are appropriately controlled for, foreign aid has direct negative effects on the risk of civil conflict. A ten percent increase in foreign aid reduces the risk of conflict by about six to nine percent. These results are at odds with those from earlier studies that have attempted to establish a relationship between aid and civil war [e.g. Collier and Hoeffler (2002)]. In their empirical applications aid does not directly effect conflict, but only reduces its risk through the indirect effect upon national income and reduced primary commodity dependence. Our study is different as we explicitly attempt to solve some important endogeneity issues. Accounting for fixed effects and using donor GDP as a powerful instrument for aid flows base our results.

Collier and Hoeffler (2002) reason that aid flows facilitate governments to develop a decent army, which suppresses rebel opportunities. However, suppression of violent rebel groups is not necessarily a good thing, as aid flows also supports leadership that is unwanted by society. We suggest therefore that our evidence should serve as a starting point for more in-depth research to further understand the channels through which aid influences the likelihood of conflict. When Collier and Hoeffler (2002) suggest that we should

observe increased military spending after periods of high aid flows. It would be interesting to test whether this is indeed so.

Nevertheless, foreign aid flows may be an important tool to policy-makers and aid agencies in preventing future conflict. Reducing the likelihood of conflict is of course important in its own right, but may have strong positive effect on the economic environment as well. The reduced risk on conflict consequently reduces the risk on capital investments. Collier (1999) elaborates: 'the sheer scale of capital flight from capital-hostile environments suggests that its effects on economic performance are likely to be large'. It is likely that household decision-making is also positively affected as investments in small scale businesses or education become increasingly profitable.

We would like to conclude with an empirical by-product of our research. We are able to explain a substantial part of the incidence of conflict. However, we note that the part that we cannot explain is extremely persistent over time. This empirical fact greatly limits the usefulness of empirical specifications in levels (e.g. OLS(1), OLS(2) and 2SLS(3)). Due to time dependence of the error terms, lagged endogenous variables (even two or three period lags) such as lagged income are likely to be endogenous variables as well. In this setting, and especially when instruments are unavailable, we would opt for empirical specifications in first differences (2SLS(4)).

5 APPENDIX 1

- *Aid to GDP ratio*. We construct a ratio of current aid to current GDP, both measured in current US$. In the regressions we use a five year average of the ratio and constructed the natural logarithm.

    - *Aid*. (www.oecd.org/dac/stats/idsonline). "usd-amount". This measure is derived by converting current aid flows in current US dollars, using current exchange rates (you attain the face value US dollar amount).
    - *recipient GDP*. We have obtained current GDP in US$ from the World Development Indicators. When observations were missing we imputed current GDP measures from the PWT 6.1 ($US\$ = \frac{I\$*PPP_t}{xrat_t} * pop * 1000$). Both measures are highly similar when the data overlap.

- *Donor GDP*. Penn World Tables. We calculate GDP of the donor countries in current US$, using the following transformation: $US\$ = \frac{I\$*PPP_t}{xrat_t} * pop * 1000$.

- per capita *GDP* for sub-Saharan African countries. The source for this was the Fearon and Laitin Database. Per capita GDP has been measured in 1000's of 1985 International (=PPP-adjusted) Dollars. The data set is available at http://www.stanford.edu/ jfearon/.. jprrepdata.zip

- *Civil Conflict*. Data on civil conflict has been acquired from the Armed Conflict Database developed by the Peace research Institute Oslo, Norway and the University of Uppsala, Sweden and is available at http://www.prio.no

- *Peace Duration*. We construct the variable from the the civil conflict variable dating back to 1960. When a country was in conflict at period $t-1$, its peace duration at period t is zero. The peace duration variable of countries that are in peace grows by one every year.

- *primary commodity dependence*. Fearon and Laitin data set. The missing observations are interpolated and extrapolated.

- *GDP growth* $t-1$. based on per capita GDP for sub-Saharan Africa , from Fearon and Laitin database.

- *Revised polity score* Levels of democracy are measured using an index from the Polity IV data set developed by the Center for International Development and Conflict Management (CIDCM)at Penn State University. Polity

is the difference between Polity IVs measure of democracy minus its measure of autocracy. Values range from 10 to 10. A detailed description of the constructed index is available at http://www.cidcm.umd.edu/inscr/polity/

For all of the following variables we used data from Miguel, Satyanath, and Sergenti (2004a) which in turn were drawn from Fearon and Laitin (2003). A description of the data can be found in Miguel, Satyanath, and Sergenti (2004b) and some additional information is available from http://www.stanford.edu/group/ethnic/workingpapers/addtabs.pdf

- *Ethnolinguistic fractionalization* Ethnic-linguistic fractionalization based on the Atlas Marodov Mira. Source: Fearon and Laitin (2003)

- *Religious fractionalization* Data used from the CIA factbook. Source: Fearon and Laitin (2003)

- *Oil-exporting country* Data was drawn from the World Development Indicators (WDI) on fuel exports as a percentage of merchandize exports, which is available for five year periods from 1960 and annually from 1980. Missing years prior to 1980 and after 1960 were linearly interpolated where possible. Source: Fearon and Laitin (2003)

- ln *mountainous* Percent Mountainous Terrain. Available data from A.J. Gerard for the World Banks Economics of Civil War, Crime, and Violence project and own estimated values for those countries not included in Gerard's work by making use of the difference (in meters) between the highest and lowest elevation points in each country as provided in the CIA factbook. Source: Fearon and Laitin (2003)

- ln *national population* $t-1$ Log of population lagged one year. Source: Fearon and Laitin (2003)

- *Real oil prices* http://research.stlouisfed.org/. Federal reserve bank St. Louis.