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Aiding violence or peace? The impact of foreign aid on the risk of civil conflict in sub-Saharan Africa

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ABSTRACT

This paper considers the impact of foreign aid flows on the risk of civil conflict. We improve on earlier studies on this topic by addressing the problem of the endogenous aid allocation using GDP levels of donor countries as instruments. A more structural addition to the literature is that we efficiently control for unobserved country specific effects in typical conflict onset and conflict continuation models by first differencing. The literature often overlooks the dynamic nature of these types of models, thereby forcing unlikely i.i.d. structures on the error terms implicitly.[1] As a consequence, malfunctioning institutions, deep-rooted political grievances, or any other obvious, yet unobserved and time persistent determinants of war are simply assumed away. We find a statistically significant and economically important negative effect of foreign aid flows on the probability of ongoing civil conflicts to continue (the continuation probability), such that increasing aid flows tends to decrease civil conflict duration. We do not find a significant relationship between aid flows and the probability of civil conflicts to start (the onset probability).

1. Introduction

Between 1946 and 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. During the Rwandan genocide for example, at least 800,000 people were killed, foremost civilians. These statistics illustrate the importance of research trying to understand the causes of civil conflict. Whereas reducing the likelihood of conflict is obviously important in its own right, it may also have strong positive effects on the economic environment. Lowering the risk of 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. In a recent book Collier (2007) identifies the 'conflict trap' as one out of four important traps plaguing a large part of the developing world.

Collier and Hoeffler (1998, 2004a) were among the first to emphasize the interplay between economic factors and civil conflict. Rebel organizations may be regarded as businesses of some sort that make efficient financing decisions in order to sustain their own viability. From that perspective, low per capita income, badly performing institutions, dependence on primary commodity exports have been associated with higher risk of civil conflict. These studies are illustrative for the increasing interest to relate a range of economic and political factors to civil war occurrence.[2] Studying the effects of foreign aid flows contributes to this line of research. In accordance to the rebel-financing argument, aid resources may be a good 'prize' for rebels to capture and hence feed instability. As a consequence, aid may

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[1] i.e., i.i.d. error structures in linear models, or conditional independence of observations in non-linear probit/logit type models.

[2] Other empirical studies include those by Miguel et al. (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 (2003b) 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.

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contribute to pushing (already conflict ridden) societies (deeper) into misery. On the other hand, when aid flows somehow decrease the likelihood of conflict, development assistance will help breaking conflict traps. Effects of foreign aid flows on civil war 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.

The fundamental argument for aid donation is to improve upon economic conditions for growth. Over the years an extensive body of literature has been studying this relationship. Much cited examples include an influential paper by Burnside and Dollar (2000) that claims that aid works predominantly in countries with good economic policies, and reactions to this result, by amongst others Collier and Dollar (2002), Easterly (2003) and Dalgaard et al. (2004). One of the robust findings in the empirical literature on civil conflict however, is that poorer countries (as opposed to richer countries) suffer more from civil conflict. Aid flows may therefore be related to conflict through its effects on economic conditions. Collier and Hoeffler (1998) argue that increasing income per capita is expected to decrease the probability of conflict when economic alternatives for potential rebels evolve and improve. Moreover, the 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] Fearon and Laitin (2003b) on the other hand offer another explanation for the well-known negative correlation between GDP per capita and civil conflict, by interpreting GDP per capita as a proxy for the state's overall financial, administrative, police and military capabilities. In spite of these theoretical arguments, the transmission mechanisms from aid to GDP are still under debate. In this paper we therefore do not assess the importance of these and other possible indirect channels through which aid might impact on conflict. The objective of this paper is to estimate the direction and size of a direct impact of aid flows on the risk of civil conflict. Potential indirect channels are controlled for in our regressions.

Studies that analyze direct relationships between foreign aid flows and civil conflict build on two fundamental hypotheses. The first hypothesis argues 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 these resources by reallocating time from production to insurrection. 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).

The second hypothesis claims that aid flows relax the government budget constraint, such that military expenditure (as a normal good) increases. Collier and Hoeffler (2007) recently found empirical support for this relationship and estimate that about 40% of foreign aid transfers into military expenditures. A powerful government army may discourage rebel groups from pursuing a violent course, such that foreign aid, as a result, decreases the risk of civil conflict. Note that the validity of this argument is conditional on the assumption that aid is sufficiently fungible into military expenditure.[4] Collier and Hoeffler (2002) develop a model formalizing the idea of an increased government budget, translating into military expenditure, but fail to find an empirical relationship between aid and conflict onset.[5] By contrast, this paper provides empirical evidence in favor of their argument as we improve on their identification strategy by recognizing the importance of fixed effects and the endogenous properties of aid allocation.

Aid flows are likely to be endogenous with respect to conflict. Donors may predict changes in the likelihood of conflict outbreak or conflict ending by observing changes in rebel movement or government action, and re-optimize the allocation of aid funds. Parameter estimates on aid flows, or even lagged aid flows, in a conflict regression would therefore capture a mix of effects. These parameter estimates would fold together the causal impacts from aid to conflict, with correlations due to the endogenous aid allocation mechanism. We propose ‘donor GDP’ as a new and powerful instrument for foreign aid flows in the conflict regression to separate out both effects. Aid flows are often defined as a percentage of Donor's GDP, hence both are strongly correlated. Almost by construction, changes in donor's GDP are not related to the endogenous aid allocation process at the recipient country level.

The main empirical finding is as follows: foreign aid is directly affecting the probability of civil conflict continuation (i.e., the probability of having conflict at t, conditional on having conflict at $t-1$), negatively and significantly. Aid flows therefore reduce the duration of civil conflicts in sub-Saharan Africa. A 10% increase in foreign aid is estimated to decrease the probability of continuation by about 8% points (the unconditional probability of conflict continuation is about 90%). We do not find a significant effect of aid flows on the onset probability (i.e., the probability of having conflict at t, conditional on having peace at $t-1$). The question whether our empirical results have normative interpretations remains however unanswered in this research. Foreign aid typically supports any leadership, hence also leadership that is unwarranted by the majority of the population. Van de Walle (2001) notes that African regimes are surprisingly durable (i.e., less leadership turnover than in any other continent) despite economic stagnation and other problems that are associated with the continent. One of his explanations for this phenomenon relates to the large amounts of aid that flow into many of these countries. Van de Walle (2001) argues that the political elites of some African countries allow just enough reform to satisfy donors, but subsequently utilize the ‘residual’ funds in a predatory fashion to maintain themselves in power. From a normative perspective, suppression of rebel groups is therefore ambiguous. The positive effects of keeping death and destruction under control might be simply outweighed by the negative effects of having an unwarranted government.

In Section 2 we discuss the data set and some important stylized facts. In Section 3 we discuss the identification strategy and report our regression outcomes. Section 3.1 considers different identification strategies as well as its underlying (implicit) assumptions. Sections 3.2 and 3.3 report regression results of models in levels and in first differences respectively. Section 4 concludes.

2. Data description

For the empirical exercise 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. 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 (Gleditsch et al., 2002). 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.’[6] A dummy variable that is unity in case of civil conflict of this type is the primary dependent variable in the

[3] Note however that Fearon (2005) claims 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.

[4] Devarajan et al. (1999) for example study aid fungibility in sub-Saharan Africa and indeed find that aid is at least partially fungible.

[5] Collier and Hoeffler (2002) study a world wide sample and do not find a direct effect of aid flows on the onset probability.

[6] 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).

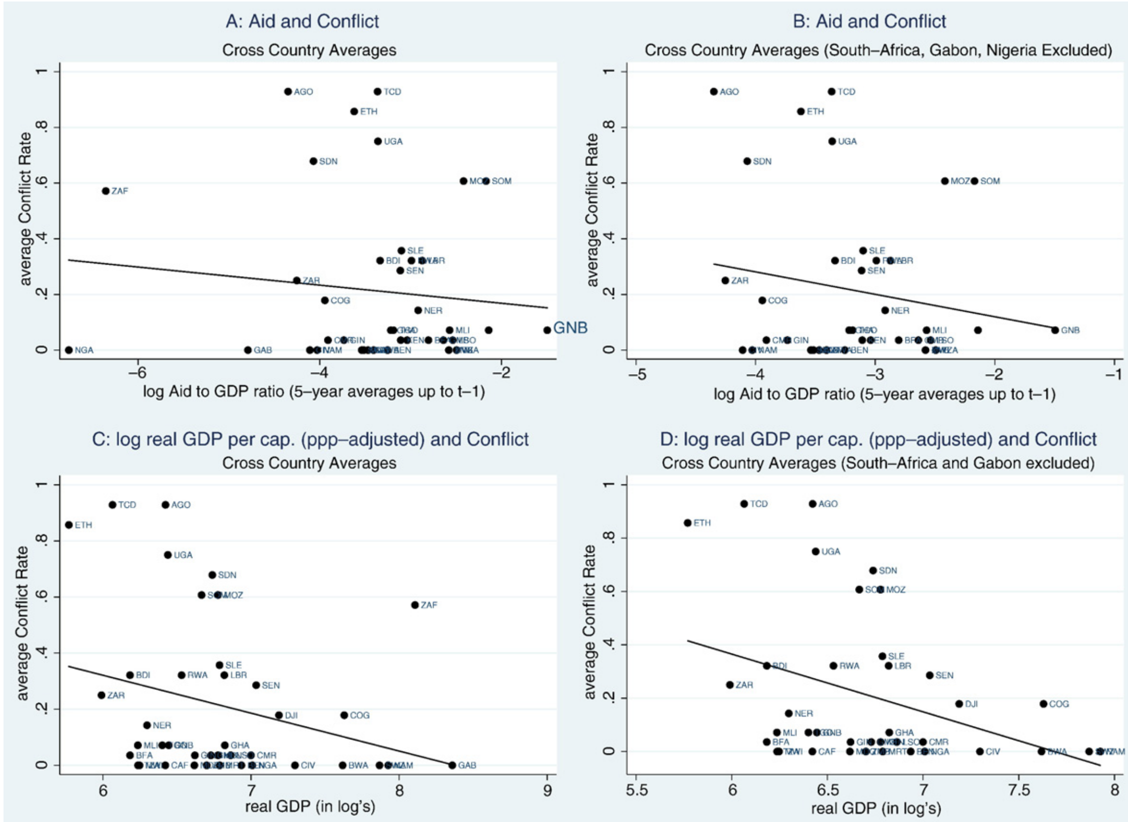

Fig. 1. Between country correlations between aid and conflict (A and B) and between GDP per capita and conflict (C and D).

regression analyses. Even in sub-Saharan Africa, civil conflicts (>25 battle deaths) are relatively rare as about 23% of our observations report civil conflicts of this type.

Selecting the appropriate dependent variable in empirical analysis is important. Collier (2007) argues that aid flows matter in different ways for different kinds of rebel violence by hypothesizing that the possible destabilizing effects of aid are less credible for rebellion as it may take many years for rebels to get their hands on the money that they are after. The potential of aid raising the risk of coups d'etat is much higher: “a coup is virtually over as soon as it has begun, and if it is successful, the aid is there for the taking” (Collier, 2007; p. 105). Coups however are not specifically coded by PRIO/Uppsala in this data set and are not considered in this study. The focus of this paper is on the >25 battle deaths civil conflict variable as opposed to the high threshold variable of >1000 battle deaths. This decision is obviously subject to taste. To our judgement however, studying the lower threshold variable is more appropriate for this sample as the median conflict in sub-Saharan Africa is of the smaller type. This type of civil conflicts therefore cause death and destruction, but report less than 1000 battle-related deaths per year.

It is nevertheless important to be careful when interpreting these conflict variables and hence the outcomes from the empirical exercises. It is not obvious that a zero after a one implies the ending of a civil conflict. A zero after a one may also indicate that conflict intensity drops temporarily, in order to flare up one or two years later (this argument holds for both high and low thresholds). The PRIO/Uppsala data set has just recently tried to account for this issue by coding whether a particular conflict has ended. We do not use this information in this paper but instead argue that the standard conflict dummy variable has a simple and consistent interpretation in our analysis: the onset/continuation probabilities we estimate in this paper are interpreted as the probability that a conflict crosses the 25 battle deaths threshold conditional on whether there was such a conflict the year before. Conflicts may have ended or they may not. This careful interpretation of the conflict variable relates quite well to the military financing argument as strong government armies might suppress rebel insurrection rather than solving underlying grievances.

The primary explanatory variable of interest in this study is foreign aid, measured as Official Development Assistance (ODA) in proportion to GDP.[7] This measure reflects the magnitude of aid flows relative to other resources at a government's 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 easily interpreted. A percentage change in the variable translates to a percentage point change in the probability.[8]

GDP data that we use to instrument aid flows were drawn from the Penn World Tables and the World Development Indicators. We construct *log*'s of five year averages of donor GDP measured in current US$, to instrument the *log* of the five year average of aid to GDP ratio of the recipient country. We are using current GDP as an instrument which is the appropriate measure to instrument a ratio of which both factors are measured in current US$.

The remaining data includes a set of country control variables similar to those used by Collier and Hoeffler (2002), Miguel et al. (2004a), and Fearon and Laitin (2003b). Data on controls are obtained from Fearon and Laitin (2003b) and Fearon (2005). 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

[7] 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).

[8] When we include the aid to GDP ratio, without taking *log*'s, as one of the explanatory variables, the results are qualitatively similar.

variables include: peace and conflict duration dummies (using the PRIO/Uppsala data on conflict dated back to 1973); 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 $t-1$; real per capita growth measured at $t-1$; measures of democracy calculated from the Polity IV data set; ethnolinguistic and religious fractionalization; a dummy for oil exporters; the *log* of the proportion of a country that is mountainous; the *log* of the national population measured at $t-1$; time trends (linear, squared, cubic); 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$. The resulting data set covers 39 sub-Saharan African countries from 1981 to 1999, providing 699 observations. For more detailed description of our data the reader is referred to Appendix A.

2.1. Stylized facts

In order to anticipate the classical mistake of confusing lack of variation for the absence of causal relationships, we have a closer look at some key properties of our variables. Fig. 1 summarizes the correlation between cross-country averages of the log aid to GDP ratio, log real GDP per capita, and the conflict rate. The data is constructed by averaging over the available time period (the conflict rate is the time average of the dummy variable that is unity when a country is having civil conflict). Each observation is accompanied with its World Bank country code.

Fig. 1A reveals no obvious relationship between the aid to GDP ratio and the conflict rate in cross-country averages. In fact, the correlation between the aid variable and the conflict rate is close to zero and insignificant. Excluding outliers (countries that receive less than 1% of ODA as a percentage of GDP, i.e., South-Africa, Gabon and Nigeria) still yields an insignificant correlation. The absence of significant between country correlations may result from the extreme complexity of the matter. Both aid-giving procedures and emerging civil conflicts are subject to many influences such that correlations in averages are easily blurred with noise. Neglecting the influence of these unobserved forces is generally a source of bias as they are likely to affect both aid flows and conflict simultaneously.

Whereas there is no between country correlation between the aid to GDP ratio and the conflict rate, we do observe correlation between real GDP per capita (in *log*'s) and the conflict rate that is significant at the 10% level (Fig. 1C). Excluding the relatively rich countries (i.e., South-Africa and Gabon) identifies an even stronger negative correlation that is significant at the 5% level (Fig. 1D).[9] These outcomes indicate that poor countries are experiencing more conflict than rich countries. Clearly, this correlation alone is insufficient to infer causality, whereas causality surely is plausible as better economic conditions provide more and better alternatives to fighting. However, the possibility that the direction of causality is reversed, or that for example (unobserved) institutional matters affect both, should not be ignored.

Fig. 2 presents averages over time for the whole sub-Saharan African region and reveals some other interesting patterns. GDP per capita (in *log*'s) is normalized to fit into Fig. 2.[10] The aid to GDP ratio, as well as GDP per capita, remain relatively stable over the sample period. Conflict rates however, have increased over time. After the stable eighties we observe increased activity around the end of the cold war. Where conflict rates seem to settle down around 1995, violence picks up again towards the end of the century.

Nevertheless, both Figs. 1 and 2 do not show any clear relationship between aid flows and the conflict rate. Both between country variation and aggregate time series variation may not be the type of variation you would want to look at. Apparently, relationships between aid and

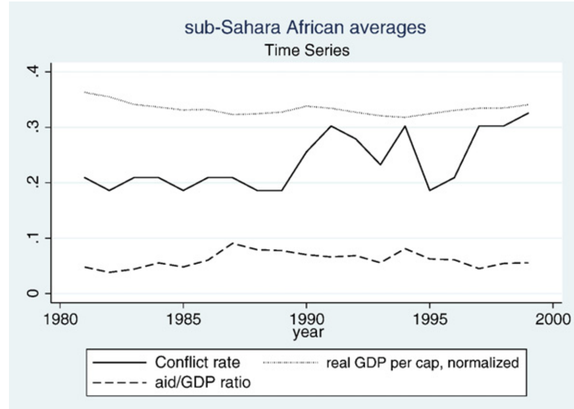

Fig. 2. Sub-Saharan African averages over time.

conflict (if any) 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 specific mean. Despite the absence of between country and time series correlation, within country correlation between aid and conflict is significantly negative [see Fig. 3].[11]

Fig. 3 shows that within countries, more aid (in proportion to GDP) is associated with less conflict. The negative within country correlation however is not easy to interpret as we only remove the country and time specific effects. The negative correlation may be due to a conflict offsetting effect of aid that has been hypothesized in the Introduction. However, the same correlation could also result from aid rationing when countries are at war. Furthermore, the correlation is also inconclusive about whether aid flows are associated with shorter conflicts (as it is related to the duration of conflict), or that aid flows prevent wars from even starting (when it affects the onset of conflict). In Section 3 we present our solutions to solving these, and other problems concerning the interpretation of these raw correlations. We report conditions under which we can identify causal, interpretable, effects.

3. The empirical analysis of conflict

The objective of this paper is to identify and to estimate causal effects of aid flows on both onset and the continuation probability of conflict in sub-Saharan Africa. Identifying causal effects from non-experimental data depends, as always, on specific sets of identifying restrictions on the data (i.e., modeling assumptions). For example, to identify a causal effect from aid on conflict onset Collier and Hoeffler (2002) rely on conditional independence of observations, or on its counterpart in linear models: i.i.d. assumptions on the errors. To our belief, these assumptions are implausible as both random and fixed effects are ruled out, as well as any other serial correlation in the unobserved component. In addition Collier and Hoeffler (2002) assume that aid allocation is exogenous with respect to future conflict (i.e., they assume that donors do not anticipate the likelihood of future conflict outbreak). We argue in this section that neglecting these issues may bias the estimated parameters and could be the reason why they fail to find correlations between aid flows and onset.

In our opinion, the analysis of the relationship between aid and conflict depends critically on modeling three concepts explicitly: *Dynamics* (state and duration dependence). The importance of modeling dynamics is typically picked up by the literature as many papers

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

[10] *log* GDP per capita is normalized with three times the sub-Saharan African average.

[11] The OLS regression that is associated with Fig. 3 regresses conflict dummy on log aid to GDP ratio (averaged over 5 years up to period $t-1$) and a set of country and time dummies. The parameter estimate associated with aid which is negative and significant at the 1% level.

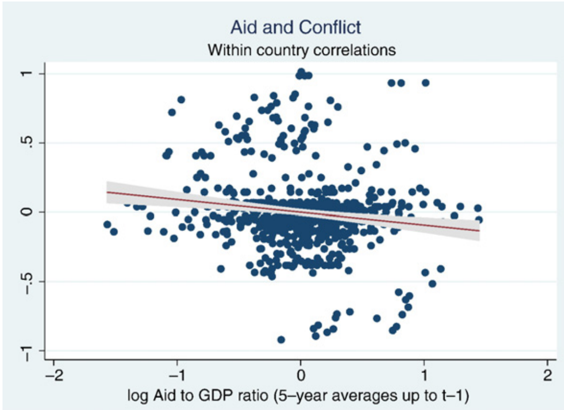

Fig. 3. Within country correlations are identified by regressing both the aid to GDP ratio as well as the conflict dummy on a full set of time and country dummies. The residuals of both regressions are plotted against each other.

distinguish between onset and duration models.[12] *Time invariant effects* (either fixed or random). An issue that has been largely neglected by the empirical literature on civil conflict, whereas political, cultural, or other (partly) unobserved factors are likely to be important determinants of conflict. The problem with neglecting time invariant factors is that they are also related to the amounts of the aid countries receive. We finally advocate the use of instrumental variable analysis to address the *endogenous process of aid allocation*. Donors are likely to anticipate increased likelihood of conflict outbreak, for example when they observe increased rebel activity, and reallocate aid flows elsewhere. We incline to the explanation that aid flows are *reduced* when countries are more likely to end up in conflict. A priori however, there is no reason why donors would not *increase* aid flows to war-prone countries for strategic reasons (e.g., Alesina and Dollar (2000) and Alesina and Weder (2002) discuss the importance of strategic interests of donor countries). After all, some of the most conflict ridden societies are also some of the largest recipients of aid. The working of endogeneities in dynamic models as we use here is not straightforward and will be discussed when we interpret the empirical results in Section 3.3.1. Either way, this type of mechanisms creates correlations between aid and conflict that we would like to eliminate in our empirical analysis.

Dynamic concepts as state and duration dependence seem important, because long sequences of conflict years take turns with long sequences of peace years. The probability of having conflict next year is therefore positively (cor)related with having conflict this year. Death and destruction intensifies hatred among civilians, who then may be more easily recruited by rebels, and also among soldiers, who then may be more determined to continue fighting. The alleged state dependency is an important reason why the empirical literature tends to focus on onset and duration separately. Collier and Hoeffler (2004a,b) however point to another reason of studying onset and duration separately. Many factors that tend to explain onset do not explain duration, and vice versa. It makes sense for example that aid flows transfer into military expenditure more rapidly when a country is at war than when it is in peace. In accordance with the military financing argument (Collier and Hoeffler, 2002), aid flows may not prevent civil conflict from starting, yet it may shorten its duration. This is in fact what we infer from our empirical findings. Moreover, parties that are now fighting may, as war continues, and more destruction takes place, update beliefs about whether it is all worth the sacrifices and how likely it is that there are going to win (Elbadawi and Sambanis, 2002). This learning mechanism changes from year to year, and is likely to be a complicated function of duration. It is therefore important to be flexible in modeling duration dependence. We model both conflict and peace duration dependence by including duration dummies.

In addition to modeling the dynamic structure we emphasize the need to account for unobserved time invariant factors (either correlated[13], or uncorrelated[14] with the vector of regressors $\mathbf{x}_{it}$). These factors concern measures of political grievances, culture, poverty, institutions and the like. It is likely that some of these unobserved factors affect both aid flows and the likelihood of conflict simultaneously, such that we would like to get rid of these factors in estimation. However, even when there is no correlation at all between the unobserved effects and $\mathbf{x}_{it}$, the parameters of standard onset or continuation models are estimated with bias. This is because onset and continuation models are essentially dynamic models, such that by construction, time invariant effects are correlated with the lagged dependents. In a typical onset model all lagged dependent variables are zero and hence drop out in estimation. The effects on the estimated parameters however, is often overlooked. This study is the first that we know of that efficiently eliminates the fixed/random effects in onset and continuation models by first differencing the regression equations.

We would like to stress here that if fixed (or even random) effects matter, they are correlated with lagged dependent variables – this includes duration variables – by construction. We argue in this section that lagged dependent variables tend to pick up variation that should either be attributed to the regressors or to the fixed effects. We cite Griliches (1961) in this section as he has derived properties of these biases in standard linear models. Griliches (1961) comes up with an explanation why residuals (as estimators of the errors) appear less serially correlated than the true errors. It is known from the empirical literature on conflict that including a lagged dependent variables will erase most – or all – of the serial correlation from the residuals. The residuals however, are biased estimates of the true errors in this case, such that the absence of residual serial correlation is by no means evidence for the absence of serial correlation in the errors (e.g., as a result of fixed effects).[15] Correlation between the fixed effect and the explanatory variables is an additional and probably important issue.

We finally argue that the process of aid allocation is endogenous with respect to conflict. Failing governments or increased rebel activity urge donors to reconsider their commitments while these factors simultaneously affect the likelihood of conflict. So even when time *in*variant effects are controlled for, time varying unobservables in the conflict model are likely to be correlated with aid flows (i.e., omitted variable bias). Closely related to omitted variable bias is the issue of reversed causality (i.e., simultaneity). Donors tend to reduce monetary aid in countries that are actually experiencing civil conflict, and allocate aid flows elsewhere. Lagging aid flows will account for reversed causality bias, but may be insufficient to tackle omitted variable bias due to aid rationing in anticipation of conflict.

There is some evidence for both anticipation and reversed causality as regressing a conflict dummy on current aid flows, lagged aid flows (a 5 year average up to period $t-1$), and a full set of time and country dummies yields both aid variables negative and significant (results not shown but available on request). Both during civil conflict and prior to civil conflict, aid flows are significantly lower than on average. Neglecting the endogenous properties of aid allocation leads to spurious inference when the suggested negative correlation would be wrongly interpreted as a risk reducing effect of aid. Our instrument, donor's GDP, is strongly related to aid flows to the sub-Saharan African region, but not to the conflict causing factors within aid recipient countries.[16]

[12] We explicitly thank the editor and one of the referees of this journal for excellent comments on this issue and proposing different modeling strategies.

[13] Often called fixed effects.

[14] Often called random effects.

[15] To perform a meaningful test on error autocorrelation, one needs consistent estimates of the errors under the null hypothesis (no residual autocorrelation) and under the alternative hypothesis (residual autocorrelation). It is clear that under the alternative hypothesis the residuals are biased estimates of the errors (Griliches, 1961).

[16] We solve for potential violations of the exclusion restriction by including other macro variables such as oil prices, trends (linear, quadratic, cubic), measures of trade (in a robustness check) and a dummy variable for the cold-war years.

3.1. Integrating onset and continuation models

We follow Elbadawi and Sambanis (2002) by modeling the probability of observing conflict c_{it} conditional on a vector $\mathbf{x}_{it}$ and a lagged conflict dummy $c_{i,t-1}$.[17] This modeling approach integrates the separate onset and duration approaches. Our baseline, Eq. (1) models two out of three concepts discussed in Section 3 explicitly: dynamics (i.e., state and duration dependence) and it allows for fixed or random effects. The endogenous properties of the aid allocation process will be discussed in Section 3.3. For estimation we rely on Linear Probability Model specifications (LPM).

We define the probability of observing conflict at period t conditional on a vector of regressors $\mathbf{x}_{it}$, peace duration (pd_{it}), conflict duration (cd_{it}), a lagged conflict dummy ($c_{i,t-1}$) and an onset and a continuation time invariant effect (α_i^{on}) and (α_i^{cont}). We condition the fixed effects conditional on conflict status thereby following the empirical literature where onset and duration (or continuation in this paper) are considered separate processes (e.g., Collier and Hoeffler, 2004a,b):[18]

$$Pr\left(c_{it}=1|\mathbf{x}_{it},\mathrm{pd}_{it},\mathrm{cd}_{it},c_{i,t-1},\alpha_i^{\mathrm{on}},\alpha_i^{\mathrm{cont}}\right)=\left(\beta^{\mathrm{on}}\mathbf{x}_{it}+\mathrm{pd}_{it}+\alpha_i^{\mathrm{on}}\right)\times 1\left(c_{i,t-1}=0\right)+\left(\beta^{\mathrm{cont}}\mathbf{x}_{it}+\mathrm{cd}_{it}+\alpha_i^{\mathrm{cont}}\right)\times 1\left(c_{i,t-1}=1\right) \tag{1}$$

The dichotomous conflict variable c_{it} is unity when a country i is experiencing conflict at period t. $\mathbf{x}_{it}$ typically contains all sorts of possibly endogenous covariates of which aid flows is the primary variable of interest in this paper. $\mathbf{x}_{it}$ is defined as follows:

$$\mathbf{x}_{it}=\begin{bmatrix}1\\ \mathbf{z}_{it}\\ a_{i,\overline{t-5}}\end{bmatrix} \tag{2}$$

$\mathbf{x}_{it}$ contains a 1 to model a constant. $\mathbf{z}_{it}$ is a vector of exogenous regressors. Aid flows $a_{i,\overline{t-5}}$, are defined as the *log* of a five year average of the aid to GDP ratio:

$$a_{i,\overline{t-5}}=\ln\left(\frac{1}{5}\sum_{l=1}^{5}\frac{\mathrm{aid}_{i,t-l}}{\mathrm{GDP}_{i,t-l}}\right) \tag{3}$$

pd_{it} and cd_{it} denote peace and conflict duration respectively. Duration is dependence is modeled with dummies.

$$\mathrm{pd}_{it}=\sum_j\gamma_{\mathrm{p}}^j\mathrm{peaceD}_{it}^j$$

$$\mathrm{cd}_{it}=\sum_j\gamma_{\mathrm{c}}^j\mathrm{conflictD}_{it}^j$$

γ_{p}^j and γ_{c}^j are parameters to be estimated. Duration dummies are coded 1 when a country has experienced peace/conflict for at least j years:

$$\mathrm{peaceD}_{it}^j=1\left(c_{i,t-1}=0,c_{i,t-2}=0,\ \mathrm{N}\ ,c_{i,t-j}=0\right)$$

$$\mathrm{conflictD}_{it}^j=1\left(c_{i,t-1}=1,c_{i,t-2}=1,\ \mathrm{N}\ ,c_{i,t-j}=1\right)$$

Our regressions include both conflict duration and peace duration dummies for j=1, 2, 5, 10.[19] We have tested whether including the omitted dummies is significantly improving the fit, but it does not.

We would like to stress that modeling onset and continuation in one model is in fact equivalent to estimating onset and continuation models separately on selected samples, which is normal practice in the empirical literature on civil conflict. Our strategy here however facilitates imposing and testing parameter restrictions across onset and continuation parameters. For policy purposes for example, it would be interesting to know whether there are significant differences between the effects on onset and on continuation. The conditional probability of observing conflict contains two linear components that are multiplied by two indicator functions respectively (i.e., $1(c_{i,t-1}=0)$ and $1(c_{i,t-1}=1)$). These indicators are the data selection criteria that are typically used in standard onset and continuation models. Conditioning the probability of conflict on being at peace at $t-1$ yields an onset model:[20]

$$Pr\left(c_{it}=1|\mathbf{x}_{it},c_{i,t-1}=0,\mathrm{pd}_{it},\mathrm{cd}_{it},\alpha_i^{\mathrm{on}},\alpha_i^{\mathrm{cont}}\right)=\begin{cases}\beta^{\mathrm{on}}\mathbf{x}_{it}+\mathrm{pd}_{it}+\alpha_i^{\mathrm{on}} & \text{if } c_{i,t-1}=0\\ . & \text{otherwise}\end{cases} \tag{4}$$

Conditioning the probability of conflict on having conflict at $t-1$ yields an continuation model:

$$Pr\left(c_{it}=1|\mathbf{x}_{it},c_{i,t-1}=1,\mathrm{pd}_{it},\mathrm{cd}_{it},\alpha_i^{\mathrm{on}},\alpha_i^{\mathrm{cont}}\right)=\begin{cases}\beta^{\mathrm{cont}}\mathbf{x}_{it}+\mathrm{cd}_{it}+\alpha_i^{\mathrm{cont}} & \text{if } c_{i,t-1}=1\\ . & \text{otherwise}\end{cases} \tag{5}$$

The mechanical consequences for estimation of this selection are fundamental and are discussed in Section 3.2.

The primary objective throughout the remainder of the empirical analysis is to estimate separate direct causal effects from aid flows on the conditional possibility of onset and continuation. The objectives of this paper are formally defined as follows:

$$\text{onset}:\quad \frac{\partial}{\partial\mathbf{x}_{it}}Pr\left(c_{it}=1|\mathbf{x}_{it},c_{i,t-1}=0,\mathrm{pd}_{it},\mathrm{cd}_{it},\alpha_i^{\mathrm{on}},\alpha_i^{\mathrm{cont}}\right) \tag{6}$$

$$\text{continuation}:\quad \frac{\partial}{\partial\mathbf{x}_{it}}Pr\left(c_{it}=1|\mathbf{x}_{it},c_{i,t-1}=1,\mathrm{pd}_{it},\mathrm{cd}_{it},\alpha_i^{\mathrm{on}},\alpha_i^{\mathrm{cont}}\right) \tag{7}$$

From Eq. (1) we know that the parameter vectors β^{on} and β^{cont} are exactly the causal effects on onset and continuation that we are interested in.

With introducing the linear structure of Eq. (1) we give up on more sophisticated, but computationally burdensome nonlinear specifications. Estimating nonlinear models of the probit/logit type are typically problematic when endogenous right hand side variables are combined with fixed effects. In nonlinear models, time invariant unobserved heterogeneity cannot be eliminated by some simple transformation of the data. Allowing for these effects would require additional distributional assumptions. Moreover, we do not know of nonlinear models that account for fixed effects, endogenous regressors as well as serial correlation in the transitory errors that we seem to find using LPM's. Hyslop (1999) however, shows that dynamic linear probability specifications with fixed effects produce rather similar outcomes as probit/logit specifications.[21] Moreover, LPM's have the additional advantage that IV techniques, that we think are necessary to address the endogeneity problem that concerns aid allocation, are more easily applied.

The key property of linear probability models is that the conditional expectation function of a binary outcome variable is assumed to be a linear function of the regressors. The fact that probit/logit and LPM's often produce rather similar outcomes is because the conditional

[17] Fearon and Laitin (2003a) report estimates of a similar model in one of their additional regression tables.

[18] Note, that the fixed effects need not be conditional on conflict status from a technical point of view.

[19] j=1 duration drops out when modeling state dependence.

[20] The onset and continuation probabilities that we are interested in may be conceived in different ways. We estimate probabilities of getting into war, conditional on either being in peace or at conflict a period earlier. The probabilities may be interpreted as a hazard rates and relate to duration simply by integration. We could also interpret these probabilities as the two probabilities we need for a two-state conditional Markov switching model.

[21] While our model and Hyslop (1999)'s model are similar, Hyslop (1999) studies labor force participation.

distribution function tends to 'look' rather linear around its expected value, while at the same time most draws from any conditional distribution function are 'close' to the expected value. For estimating parameters we use the following baseline estimating equation derived from Eq. (1).

$$c_{it} = \left(\beta^{\text{on}}\mathbf{x}_{it} + \text{pd}_{it} + \alpha_i^{\text{on}}\right) \times 1\left(c_{i,t-1} = 0\right) + \left(\beta^{\text{cont}}\mathbf{x}_{it} + \text{cd}_{it} + \alpha_i^{\text{cont}}\right) \times 1\left(c_{i,t-1} = 1\right) + \varepsilon_{it} \quad (8)$$

ε_{it} is an error that is allowed to correlate over time. The subsequent sections estimate the β parameters under different sets of assumptions. Moreover, we discuss the potential pitfalls when parameters of the above model are (falsely) restricted.

3.2. Regressions in levels and associated pitfalls

We are estimating the parameters of Eq. (1) under different sets of identifying assumptions. Likewise, the obtained estimates may be interpreted as causal effects only under these respective sets of assumptions. Not all of the identifying assumptions that we introduce are equally appropriate however. In this section for example, we do not control for the unobserved heterogeneity that is presumably important in conflict analysis. Our claim is that these results are therefore difficult to interpret. We do however report these results as this identification strategy is commonly (and perhaps wrongly) accepted by the literature. These LEVEL regressions reported in Table 1 do not account for fixed or random effects and are the type of models that are often estimated in the

Table 1
Foreign aid and civil conflict (models in levels)

Variable	Dependent variable: civil conflict dummy					
	LEVEL(1a)	LEVEL(1b)	LEVEL(2)	LEVEL(3)	LEVEL(4)	
					Onset	Contin.
Aid$_{\overline{t-5}}$	−0.007	−0.007	0.001	0.001	0.012	−0.02
	[0.022]	[0.050]	[0.014]	[0.014]	[0.016]	[0.048]
Primary commodity dependence	−1.817***	−1.817	−0.399	−0.14	−0.192	−0.058
	[0.467]	[1.245]	[0.308]	[0.283]	[0.289]	[1.361]
Primary commodity dependence sqrd.	3.954***	3.954*	0.941	0.358	0.398	0.434
	[0.885]	[2.259]	[0.615]	[0.561]	[0.551]	[3.234]
GDP growth$_{t-1}$	−0.057	−0.057	0.116	0.198	−0.173	0.42
	[0.292]	[0.263]	[0.152]	[0.170]	[0.170]	[0.275]
log real GDP per cap.$_{t-1}$	−0.245***	−0.245**	−0.071**	−0.044	−0.036	0.004
	[0.037]	[0.099]	[0.029]	[0.029]	[0.035]	[0.123]
Polity$_{t-1}$	0.004	0.004	0.004	0.003	0.003	−0.009
	[0.003]	[0.006]	[0.003]	[0.002]	[0.003]	[0.008]
Ethnolinguistic fractionalization	0.107	0.107	0.014	−0.015	−0.033	0.045
	[0.093]	[0.288]	[0.060]	[0.056]	[0.063]	[0.263]
Religious fractionalization	−0.307***	−0.307	−0.09	−0.071	−0.132	0.062
	[0.114]	[0.332]	[0.090]	[0.080]	[0.091]	[0.242]
Oil exporter	0.146	0.146	0.064	0.057	−0.003	0.277
	[0.126]	[0.239]	[0.073]	[0.071]	[0.081]	[0.292]
Oil exporter × Oil Prices$_{\overline{t-5}}$	−0.001	−0.001	−0.001	−0.001	0.003	−0.014**
	[0.005]	[0.007]	[0.003]	[0.003]	[0.004]	[0.007]
Real Oil Prices$_{\overline{t-5}}$	−0.003	−0.003	−0.004	−0.004	−0.003	−0.006
	[0.005]	[0.003]	[0.003]	[0.003]	[0.003]	[0.005]
Cold war	0.024	0.024	0.039	0.02	0.006	0.08
	[0.063]	[0.058]	[0.041]	[0.040]	[0.044]	[0.087]
log mountainous	0.054***	0.054	0.016	0.01	0.003	0.05
	[0.013]	[0.039]	[0.010]	[0.009]	[0.010]	[0.058]
log national population$_{t-1}$	0.028	0.028	0.009	0.002	0.006	−0.004
	[0.021]	[0.061]	[0.013]	[0.012]	[0.015]	[0.051]
peaceD^2				−0.042	−0.04	
				[0.142]	[0.141]	
peaceD^3				−0.166	−0.157	
				[0.119]	[0.120]	
peaceD^5				−0.058	−0.057	
				[0.062]	[0.062]	
peaceD^{10}				−0.001	−0.007	
				[0.032]	[0.033]	
conflictD^2				0.233*		0.203
				[0.132]		[0.130]
conflictD^3				0.231**		0.222**
				[0.101]		[0.099]
conflictD^5				−0.038		−0.043
				[0.039]		[0.073]
conflictD^{10}				−0.055		−0.113*
				[0.048]		[0.059]
Internal conflict$_{t-1}$			0.736***	0.22*	−0.274	
			[0.036]	[0.127]	[1.014]	
Linear time trend	−0.001	−0.001	−0.003	−0.004	−0.003	
	[0.008]	[0.006]	[0.006]	[0.006]	[0.006]	
Constant	1.803***	1.803*	0.61**	0.744***	0.727**	
	[0.378]	[0.918]	[0.288]	[0.286]	[0.327]	
t-test on no f.o. residual autocorrelation (p-val.)	0.00	0.00	0.03	0.851	0.621	
R-squared	0.16	0.17	0.60	0.65	0.66	
Observations	699	699	699	699	699	

Note. LEVEL(1a), LEVEL(2), LEVEL(3), LEVEL(4) report robust standard errors in brackets allowing for heteroskedasticity. LEVEL(1b) reports clustered standard errors allowing for error correlation within countries and for heteroskedasticity. ***, **, * indicate significance at the 1, 5 and 10% level.

literature. In Section 3.3 we get rid of the biases that distort the regression results of the models in levels. The IV-FD (Instrumental Variables-First Differences) regressions reported in Table 2 eliminate fixed effects by first differencing and use instrumental variables techniques to deal with biases due to endogenous aid allocation.

We have argued that neglecting dynamics, fixed or random effects, as well as the endogeneity of aid allocation is likely to produce inconsistent estimates of the causal effects we are interested in. Model LEVEL(1a) of Table 1 neglects all of these issues and just regresses the conflict dummy on a set of $\mathbf{x}_{it}$'s with OLS. Regressing the conflict dummy on a vector of explanatory variables $\mathbf{x}_{it}$, while in fact the true model is represented by Eq. (1) is effectively estimating the following LPM:

$$c_{it} = \overline{\beta}\mathbf{x}_{it} + \eta_{it}^{\mathrm{LEVEL}(1)} \tag{9}$$

where

$$\eta_{it}^{\mathrm{LEVEL}(1)} = \left(\mathbf{x}_{it}\left(\beta^{\mathrm{on}} - \overline{\beta}\right) + \mathrm{pd}_{it} + \alpha_i^{\mathrm{on}}\right) \times 1\left(c_{i,t-1} = 0\right) + \left(\mathbf{x}_{it}\left(\beta^{\mathrm{cont}} - \overline{\beta}\right) + \mathrm{cd}_{it} + \alpha_i^{\mathrm{cont}}\right) \times 1\left(c_{i,t-1} = 1\right) + \varepsilon_{it} \tag{10}$$

which is a straightforward reformulation of Eq. (8). If and only if the following set of assumptions is satisfied, the LEVEL(1) parameter estimates are consistent estimates of the causal effects as defined by definitions (6) and (7):

$$E\left[\eta_{it}^{\mathrm{LEVEL}(1)}|\mathbf{x}_{it}\right] = 0 \tag{11}$$

Condition (11) is highly restrictive as it typically rules out all dynamic issues discussed before, as well as the correlation between the fixed effects and $\mathbf{x}_{it}$. No dynamics implies no discrimination between onset and continuation probabilities, which seems inconsistent with the data and the theory as we have argued before.

Even though assumption (11) is restrictive, the LEVEL(1a) and LEVEL(1b) regressions from Table 1 produce some of the familiar correlations empirical studies on civil conflict tend to find (e.g., Collier and Hoeffler, 2002). LEVEL(1a) reports a significant negative correlation with real per capita income, and a nonlinear relationship with primary commodity exports dependence. The nonlinearity in this study however, is different with respect to Collier and Hoeffler (2002)'s findings. Our estimates yield a U-shape, instead of the (expected) inverted U-shape. The effect is minimized when primary commodity dependence comprises about 20% of GDP, which is about the sub-Saharan African average. Allowing for error correlation within countries in LEVEL(1b) merely identifies the well-known and important empirical fact that poor countries experience more conflicts than rich(er) countries. The dependence on primary commodity exports is no longer significant at the 5% level. In accordance with Collier and Hoeffler (2002)'s findings, we do not find any significant correlations with aid flows, which is in line with the Fig. 1 correlations. Our results are rather similar to those of Collier and Hoeffler and indicate that both data sets exhibit similar features. The regression results of Section 3.3, should therefore be attributed to differences in specification rather than to differences in data. It should be mentioned that Collier and Hoeffler (2002) use world data at five year intervals, and explain onset rather than incidence.

As we have argued before, many authors explicitly recognize the importance of modeling internal dynamics of conflict and estimate onset and continuation (or duration) models separately. LEVEL(2), LEVEL (3) and LEVEL(4) of Table 1 are straightforward extensions of LEVEL(1) and extend dynamics bit-by-bit. LEVEL(2) includes a lagged dependent in the regression and allows for state dependence accordingly (i.e., the constant in the regression is then allowed to vary from onset to continuation). The lagged dependent variable is highly significant and LEVEL(2) explains about 60% of the data as opposed to the 17% of the LEVEL(1) specifications. LEVEL(3) adds duration dummies to allow for peace and war duration. LEVEL(4) estimates our baseline regression model by allowing all marginal effects of $\mathbf{x}_{it}$ to vary from onset to continuation. LEVEL(4) models dynamics and controls, yet does not solve for unobserved heterogeneity as the unobserved country effects (either fixed or random) remain in the error term :

$$c_{it} = \left(\beta^{\mathrm{on}}\mathbf{x}_{it} + \mathrm{pd}_{it}\right) \times 1\left(c_{i,t-1} = 0\right) + \left(\beta^{\mathrm{cont}}\mathbf{x}_{it} + \mathrm{cd}_{it}\right) \times 1\left(c_{i,t-1} = 1\right) + \eta_{it}^{\mathrm{LEVEL}(4)} \tag{12}$$

where

$$\eta_{it}^{\mathrm{LEVEL}(4)} = \alpha_i^{\mathrm{on}} \times 1\left(c_{i,t-1} = 0\right) + \alpha_i^{\mathrm{cont}} \times 1\left(c_{i,t-1} = 1\right) + \varepsilon_{it} \tag{13}$$

for causal interpretation of the OLS estimates of LEVEL(4) we need:

$$E\left[\eta_{it}^{\mathrm{LEVEL}(4)}|\mathbf{x}_{it}, c_{i,t-1}, \mathrm{pd}_{it}, \mathrm{cd}_{it}\right] = 0 \tag{14}$$

To obtain consistent estimates of β^{on} and β^{cont} we need the errors to have expectation zero, conditional on $\mathbf{x}_{it}$ and, perhaps more interestingly, on the lagged dependent $c_{i,t-1}$ and the duration variables. It is essential to note that this requirement rules out any autocorrelation in the error terms.[22] The important conclusion from Eq. (14) is therefore that standard onset or continuation models in levels (implicitly) assume that malfunctioning institutions, deep-rooted political grievances, vicious rebel leaders or other fixed unobservables cannot be important determinants of civil war. Not to mention that these factors are likely to correlate strongly with measures of aid flows, yielding an additional source of bias in the LEVEL estimates.

It stands out from the LEVEL regressions of Table 1 that the $\mathbf{x}_{it}$'s seem to have very little explanatory power in addition to modeling dynamics. In fact, the $\mathbf{x}_{it}$'s are jointly *in*significant in the LEVEL(4) regression. Moreover, we find that the residual autocorrelation is eradicated when state and duration dependence are both accounted for. These results have been a little puzzling at first as we did not find the familiar correlations that have been found in numerous other studies on onset,[23] combined with residual autocorrelation due to the unobserved effects. Even though we do not find any autocorrelation in the *residuals* of the LEVEL(3) and LEVEL(4) regressions, the absence of autocorrelation in the *errors* is hard to justify from a practical point of view. Without a doubt, the unobserved factors mentioned before (and there are many more of these factors) are important determinants of civil conflict. Estimating standard probit/logit or linear probability models in levels however, require that these unobserved effects do not, in any way, relate to conflict. A consistent explanation for this phenomenon (i.e., no residual autocorrelation and weak explanatory power of the regressors) is reported by Griliches (1961). Griliches (1961) derives biases of the OLS parameter estimators in (stationary) dynamic models with autocorrelated errors. Errors that are positively autocorrelated (e.g., due to time invariant unobserved effects) yield attenuated parameter estimates associated with the $\mathbf{x}_{it}$'s, and blow up parameter estimates associated with the lagged dependent. A by-product of the biased parameter estimates is that the implied residuals appear less serially correlated than the true errors. However, explicit formulas for this type of biases have not been derived in linear probability or nonlinear probit/logit models yet and is beyond the scope of this paper. Some of these properties however stand out from our regression results.

Another potential violation of assumption (14) is due to correlation between the unobserved time invariant effects and some important covariates such as aid flows. Alesina and Dollar (2000) for example show that donors have all kinds of political and strategic motivations for giving country *a* more than country *b*, in addition to a recipient country's economic need and policy performance. Whereas Alesina

[22] In a nonlinear environment, such as probit or logit, similar requirements can be defined.

[23] Note that we do find these correlations in LEVEL(1).

Table 2
Foreign aid and civil conflict (IV models in first differences)

Variable	Dependent variable: civil conflict dummy					
	IV-FD(1)		IV-FD(2)		IV-FD(3)	
	Onset	Contin.	Onset	Contin.	Onset	Contin.
$\text{Aid}_{\overline{t-5}}$	0.401	−1.501***	0.18	−0.78**	0.203	−0.808**
	[0.277]	[0.490]	[0.235]	[0.360]	[0.165]	[0.320]
Primary commodity dependence					−1.057	−1.383
					[1.095]	[1.881]
Primary commodity dependence sqrd.					0.902	−2.284
					[1.220]	[3.639]
GDP growth_{t-1}					−0.3*	−0.451**
					[0.174]	[0.208]
log real GDP per cap._{t-1}					0.479**	0.946***
					[0.223]	[0.299]
Polity_{t-1}					0.003	−0.011
					[0.005]	[0.013]
Oil exporter					−0.084	..
					[0.142]	..
Oil exporter × Oil $\text{Prices}_{\overline{t-5}}$					0.005	−0.009
					[0.006]	[0.016]
Real Oil $\text{Prices}_{\overline{t-5}}$					0.002	−0.005
					[0.004]	[0.009]
Cold war					−0.069	−0.068
					[0.068]	[0.064]
log national population_{t-1}					0.077	−0.019
					[0.819]	[0.847]
peaceD^2			0.215***		0.211**	
			[0.060]		[0.103]	
peaceD^3			0.04		0.036	
			[0.066]		[0.077]	
peaceD^5			−0.049		−0.045**	
			[0.070]		[0.020]	
peaceD^{10}			−0.041		−0.034**	
			[0.079]		[0.015]	
conflict^2				−0.282***		−0.295**
				[0.060]		[0.115]
conflict^3				0.013		0.037
				[0.085]		[0.055]
conflict^5				−0.189**		−0.091*
				[0.090]		[0.051]
conflict^{10}				0.041		0.031
				[0.109]		[0.074]
Internal conflict_{t-1}	..		..		..	
Linear time trend	0.025		0.030**		0.026	
	[0.015]		[0.014]		[0.024]	
Constant	..		..		..	
t-test on no f.o. residual autocorrelation (p-val.)	0.24		0.56		0.57	
R-squared			..		..	
Root MSE	0.28		0.23		0.23	
F-Stats on instrument sign. (first stage reg.)	47.33	10.89	27.04	18.74	27.35	15.98
Observations	602		602		602	

Note. Robust standard errors are in brackets allowing for heteroskedasticity. Etholinguistic fractionalization, religious fractionalization and log mountainous area are dropped (i.e., they are first differenced out). Instrumental variables used to instrument foreign aid flows: changes in log's of five year averages of nominal US GDP, interacted with $1-c_{i,t-1}$ and $c_{i,t-1}$ for the onset and continuation model respectively. ***, **, * indicate significance at the 1, 5 and 10% level.

and Dollar (2000) have established correlations between aid flows and a lot of *observables* there are probably many more *unobservable* factors determining the size of aid flows. In relation to this study for example, we suppose that donors in general would prefer to support non-violent regimes in favor of violent regimes (ceteris paribus). It seems likely that aid flows are correlated with at least some of the unobserved determinants of conflict. The empirical literature on conflict however, tends to neglect the effects of country specific unobserved heterogeneity on estimating parameters in onset and continuation models.

Fixed effects in linear dynamic models are typically dealt with by applying a first difference transformation to the model in levels.[24] First differencing estimating Eq. (8) eliminates the unobserved country effects, hence solves for both the correlation between the α_i's and the lagged dependents and the as well as correlation between the α_i's and the $\mathbf{x}_{it}$'s.

3.3. Regressions in first differences

The remainder of the paper is about models that are estimated in first differences, and more specifically on the effects of foreign aid flows on the onset and the continuation probability. First differencing Eq. (8) eliminates the fixed effects and yields the following regression model:

$$\begin{aligned}\Delta c_{it} &= (\beta^{\text{on}}\Delta\mathbf{x}_{it} + \Delta\text{pd}_{it}) \times 1(c_{i,t-1} = 0, c_{i,t-2} = 0) \\ &\quad + (\beta^{\text{cont}}\Delta\mathbf{x}_{it} + \Delta\text{cd}_{it}) \times 1(c_{i,t-1} = 1, c_{i,t-2} = 1) + \Delta\varepsilon_{it} \qquad (15)\end{aligned}$$

Eliminating the α_i's solves the endogeneity problems due to correlations between the $\mathbf{x}_{it}$'s, lagged dependents and the α_i's. OLS regressions

[24] An inconsistent way of dealing with the fixed effects would be including i dummies in the regression to estimate them (i.e., FE/LSDV estimation). This identification strategy requires all regressors to be strictly exogenous and is violated by construction in dynamic models.

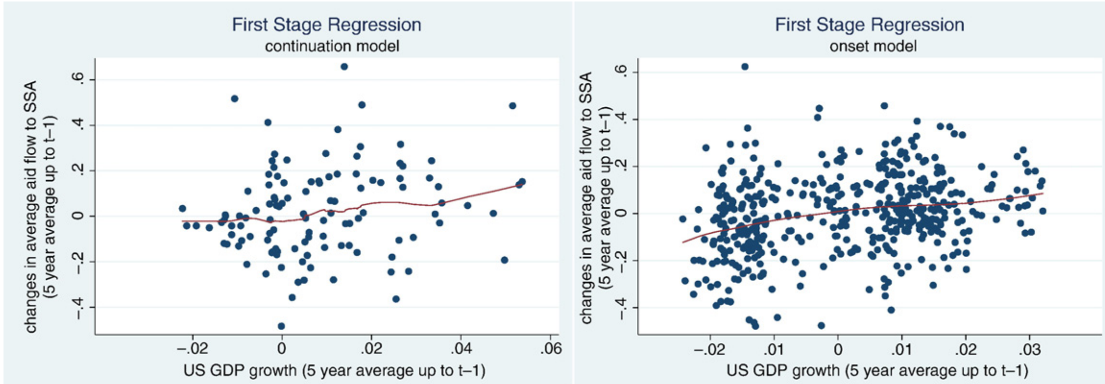

Fig. 4. Positive partial correlation between the change in *log* average U.S. GDP and the change in the *log* of the average aid to GDP ratio in sub-Saharan African countries.

on the Eq. (15) yield consistent estimates of the parameters of interest under the following assumption:

$$E\left[\Delta\varepsilon_{it}|\Delta\mathbf{x}_{it}, c_{i,t-1}, c_{i,t-2}, \Delta c_{i,t-1} = 0\right] = 0 \tag{16}$$

With Eq. (15) we claim to have the framework right for estimating the effects we are interested in. However, we have argued that the endogeneity of aid flows is not only due to correlation with the fixed effects. Rebel uproar or the temporary rule of violent leadership in recipient countries, all captured by ε_{it}, indicate increased likelihood of future conflict and urge donors to suspend or reallocate funds. If as a consequence war indeed breaks out, the observed correlations in first differences could be mistaken for a causal effect. Lagging aid flows in regressions solves this problem only partially. Increased rebel activity is not necessarily causing war outbreak immediately. Donors however, easily anticipate increased likelihoods of conflict (by observing increased rebel activity) and reallocate aid flows before conflicts actually flare up. In econometric terms, lagging aid flows solves for simultaneity bias (i.e., reversed causality), but not for omitted variable bias due to anticipation. We would indeed like to include variables measuring rebel activity. When the endogenous aid allocation mechanism is true, assumption (16) is untenable.

The standard treatment procedure for endogenously determined variables is to use instrumental variables (IV) techniques to separate out the anticipation effect from the true causal mechanism that we are interested in. For this we use additional information (i.e., an instrument) that is correlated with aid flows, but not with the endogenous allocation mechanism. A proper 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 regression model (i.e., the exclusion restriction should not be violated).

We propose GDP levels of large western donor countries as instruments for aid flows. 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. 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 only a small group of countries has ever reached the 0.7% it is likely that aid flows and donor GDP levels are correlated. Donor countries attempt to commit to a constant fraction of GDP for development assistance in total, independent of any war in any individual recipient country. The distribution among recipient countries however is subject to the political situation in recipient countries and perhaps to the strategic interest of the donor and is therefore endogenous. Some authors have constructed instruments for foreign aid building on a similar rationale (see for example Tavares, 2003; Rajan and Subramanian, 2005; Collier and Hoeffler, 2007).

Good first stage explanatory power in the above model means simply that donor's GDP is partially correlated with our measure of aid (i.e., correlation after controlling for other factors affecting conflict). The nonlinear fit between U.S. GDP levels and the aid to GDP ratio's of sub-Saharan African countries (based on the IV-FD(3) model of Table 2) is presented graphically in Fig. 4. Both curves are reasonably linear as well as increasing as we would have expected, and are in fact highly significant when a linear regression line is fitted (the *F*-statistics are above 10, see Tables 2 and 3). The positive partial correlation between donor's GDP and the aid flows summarizes the first requirement for a good instrument.

Table 2 reports our estimation results using GDP of the United States to instrument aid flows. U.S. GDP works particularly well in the first stage as it is by far the largest donor in absolute terms. As a robustness check we discuss our estimation results using GDP levels of the United Kingdom and Japan (Section 3.3.2).[25]

Satisfying the exclusion restriction is the other necessary requirement for a good instrument. In practice we need the error term of the estimating equation to be uncorrelated with the instruments. However, for a causal interpretation of the parameter estimates we need the stronger assumption of mean independency. Table 2 estimates the model in first differences, where aid flows are considered endogenous. The IV-FD regressions are consistent estimates of the causal parameters that we are interested in under the following assumptions (i.e., the exclusion restrictions):

$$E\left[\Delta\varepsilon_{it}|\Delta z_{it}, \Delta\mathrm{DGDP}_{\overline{t-5}}, c_{i,t-1}, c_{i,t-2}, \Delta c_{i,t-1}\right] = 0 \tag{17}$$

$\mathrm{DGDP}_{\overline{t-5}}$ 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 subsequently.

Whereas failing regimes or changes in rebel movement are omitted variables and cause trouble in the OLS regressions (in first differences), their influence also limits the choice set for potential instruments. Potential instruments that are specific characteristics of recipient countries, and are somehow related to aid flows, are quite easily correlated with some of the many factors causing conflict as well. U.S. GDP is unlikely to be systematically related to specific characteristics at the recipient country level and is therefore a good candidate for a proper instrument. This is also the reason why we chose not to interact our instrument with for example the cultural and geographical distance that are used by e.g., Tavares (2003) and Rajan

[25] Using GDP levels of other major donors like Germany and France did not work as well in the first stage.

Table 3
Foreign aid and civil conflict (first stage regressions)

Variable	Dependent variables:					
	Onset model: $\Delta\log \text{aid}_{\overline{t-5}} \times (1-c_{t-1})$					
	Continuation model: $\Delta\log \text{aid}_{\overline{t-5}} \times c_{t-1}$					
	IV-FD(1)		IV-FD(2)		IV-FD(3)	
	Onset	Contin.	Onset	Contin.	Onset	Contin.
$\Delta\log \text{GDP US}_{\overline{t-5}} \times (1-c_{t-1})$	3.555***	0.243	2.534***	0.226	2.507***	0.371
	[0.517]	[0.221]	[0.488]	[0.268]	[0.480]	[0.268]
	(6.88)	(1.09)	(5.20)	(0.84)	(5.23)	(1.38)
$\Delta\log \text{GDP US}_{\overline{t-5}} \times c_{t-1}$	2.576***	1.821***	1.859***	1.310***	1.879***	1.190***
	[0.457]	[0.552]	[0.550]	[0.302]	[0.536]	[0.299]
	(5.64)	(3.30)	(3.38)	(4.33)	(3.51)	(3.98)

Note. Robust standard errors are in brackets. Robust t-statistics are in parenthesis. For reasons of space the conditioning variables are suppressed. Results are available upon request. ***, **, * indicate significance at the 1, 5 and 10% level.

and Subramanian (2005)[26] in their studies. The exogeneity of cultural and geographical distance within our specific context is debatable. Cultural ties (common language, colonial ties etc.), geographical positioning, as well as civil conflict are highly clustered in a spatial sense. This empirical fact is likely to produce correlations between conflict and these interaction variables that we typically do not want to mix up with any potential effects of aid flows. Note that the above mentioned authors study phenomena different to conflict, such that their identification strategy may be appropriate for their particular purposes.

The strength of our instrument however comes at a cost. Because our instrument is not specific to recipient countries it is impossible to include time dummies to capture sub-Saharan African wide changes to the incidence of conflict. This would be problematic when these shocks are directly related to donor's GDP and are not sufficiently captured by any of our conditioning variables. It is therefore essential to include variables as GDP, economic growth, dependence on primary commodity exports, dependence on world oil prices (especially when the country is an oil exporter), and a dummy for the cold war years as controls. 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 in one of the robustness checks, but it is not changing the results.

3.3.1. Main regression results

We have estimated three versions of Eq. (15) using three different sets of controls. The regression results are reported in Table 2. IV-FD(1) includes just our aid measure as well as a constant in order to pick up trending behavior in some of the key variables (see Fig. 2). IV-FD(2) models duration dependence explicitly using dummies. IV-FD(3) adds a full set of controls to the duration dummies. Allowing both for dynamics, fixed effects and using IV techniques for the aid measure reveals some interesting and new empirical patterns that are otherwise blurred by hard-to-interpret between country correlations.

All three IV specifications yield a significant and clear-cut negative, and economically important effect of foreign aid flows on the conflict continuation probability. A 10% increase in aid relative to recipient GDP decreases the probability of conflict continuation by about 8% points in both IV-FD(2) and IV-FD(3). The effect is significantly different from the effect on onset at the 1% level (test results not shown). The stripped down model IV-FD(1) estimates the marginal effects of aid flows almost twice as large (a 10% increase in aid is associated with a 15% point drop in the continuation probability). This outcome suggests that their are indeed some indirect aid effects on the conflict probability that are subsequently picked up by the duration dummies or the controls. Whereas Collier and Hoeffler (2002) only find 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. Yet, Collier and Hoeffler's results are not contradicted here as we also do not find a relationship of aid flows with the onset probability. We do however, have a different identification strategy by first differencing and instrumenting and therefore add to the empirical evidence on the effects on onset as well.

We have also estimated Eq. (15) without instrumenting aid flows (results not shown, but available on request). Aid flows appeared only significant at 10% on the continuation probability in the OLS equivalents of models IV-FD(2) and IV-FD(3). The parameter estimate we find however, is about −0.17 and significantly smaller than in the IV regressions. The parameter estimates associated with the other regressors are not affected by instrumenting aid flows. Instrumenting aid therefore identifies a larger effect (in absolute terms) as compared to the OLS estimates in first differences indicating that endogeneity is important. The negative impact of the OLS estimates (not reported) indicates that relatively high levels of aid are associated with ending conflicts (i.e., the number of battle deaths dropping below the 25 threshold). When we instrument aid flows we find an even stronger negative relationship suggesting that donors direct aid away over the course of civil conflicts. Donors might lose confidence that their money is used for the 'right' purposes so when in the end conflicts terminate, donors are giving less than they would have done otherwise. Eliminating this reallocation effect identifies a stronger negative causal impact on the continuation probability of aid flows. However, it is typically hard to read the donor's minds, moreover comparing IV and OLS outcomes requires speculation about all 'endogenous mechanisms'. An equally valid explanation of our findings would be for example the presence of important measurement error in the foreign aid to GDP ratio.

The negative effects on continuation are quite substantial. Whereas the unconditional continuation probability is about 90%, a structural increase in aid flows by 10% would decrease the continuation probability to about 82%. Predicting what a similar-sized drop in aid flows would do however is not straightforward to derive, as probabilities may not exceed 100%. We should keep in mind that by construction, marginal effects on probabilities are nonlinear (or zero) on the regressor's domain, simply because probabilities are bounded between zero and one. The substantive magnitude of the marginal effects therefore flattens off in the neighborhood of 0 or 1. Linear Probability Models assume these nonlinearities away, merely for estimation purposes. A drawback of the linear probability approach is that the model is able to predict outside the permitted interval. We do not however, find any evidence for off-the-charts predictions. The predicted probabilities of the IV-FD(3) fall neatly within the −1 to 1 interval. The conflict variable in first differences is either −1, 0 or 1, such that predicting within the −1 to 1 range is

[26] Whether an aid recipient country is a former colony of a donor may be regarded as a measure of cultural proximity.

correct. We find that both onset and continuation is negatively related to the lagged economic growth rate, supporting the Miguel et al.'s (2004a) findings. The size of our estimates however, is smaller than theirs.[27] More surprisingly perhaps, we find that lagged economic output per capita is positively related to both the onset and the continuation probability. This result is surprising as it drives into one of the most established findings in the literature on conflict: civil war is more likely in poor countries. Note that we do have these well-known negative correlations in our data [see Table 1: LEVEL(1), LEVEL(2), as well as Fig. 1]. The IV-FD findings on GDP per capita should therefore not be interpreted as a particularity of our data, but as a result from our identification strategy and an interesting subject for future research.[28] Models that neglect fixed effects typically report negative relationships with GDP per capita and find no relationship with aid flows. Our results suggest that unobserved fixed factors may therefore be held accountable for these typical findings. The unexpected positive GDP per capita effect in both the onset and the continuation model in first differences can be explained by a build up of GDP levels in the peace years before conflict sets in, and decreasing GDP during conflict years, as a result from the destruction of human and physical capital. A definitive judgement about this pattern requires more in-depth research and is left for future projects.

The estimates on the duration dummies show an interesting patterns concerning periods of peace and conflict. Peace seems particularly fragile in the early years after civil conflict has ended, when the onset probability is significantly higher. This effect however may result from the fact that our conflict variable is not necessarily capturing peace, but perhaps only a relatively peaceful period in between two conflicts. However, when countries survive this unstable period, the onset probability becomes significantly smaller. The duration dependence seems to work in the opposite direction on the continuation probability. Especially in the early stages of conflict, conflicts have a relatively high probability to end. Here also we could think of a situation where civil tension is just there, and comes to an eruption every once and a while. However, when conflicts continue past this point, the probability of ending decreases. We have not included all possible duration dummies in the regressions in order to save on degrees of freedom. We have tested whether including the omitted dummies is significantly improving the fit, but it is not.

3.3.2. Robustness checks

We have verified the robustness of the IV-FD(3) results by using different subsets of our instruments (i.e., GDP levels of other important donor countries) and by including measures of trade in the regression.[29] We also include higher order trends in the regression addressing the possible trending behavior in both our measure of aid, our instruments as well as our measure of conflict. We pay extra attention to the period around the end of the cold war when a sudden increase in the number of civil wars is observed.

Including GDP levels of two other large donors (i.e., Japan and the United Kingdom) separately in the instrument set works fine and yields similar outcomes. Including GDP levels of all three donors in the instrument set together also yield similar results. Moreover, we were not able to reject the over identification test on the additional instruments (Hansen-*J* test). Using GDP levels of other important donors such as Germany or France did not work very well in the first stage regression (e.g., using GDP of France would yield similar results, but only significant at 10%).

We have included lagged measures of trade as it is another potential channel through which GDP of donor countries could influence the incidence of conflict. Nevertheless, 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. Clearly, our instruments do not allow for including time dummies in the regression (time dummies are not identified simultaneously with the other model parameters). We have included quadratic and cubic trends in order to anticipate spurious correlation due to trending behavior of some of the variables of interest. Including trends did not change the results quantitatively. However, the significant positive effect of recipient GDP on onset disappeared. Excluding the linear trend that is in our baseline specification, also did not change the results qualitatively.

The sudden increase of civil conflicts around the end of the cold war may also be 'accidentally' related to our instruments and aid flows. We have addressed this possibility by adding time dummies for this period (i.e., 1989, 1990, 1991 and 1992) and by leaving out these years. Both strategies did not weaken the aid effects on continuation. In fact, the effects we find were a little stronger.

4. Conclusion

Our analysis shows that when endogeneity issues (i.e., random/fixed effects, the endogeneity of aid allocation process) are appropriately controlled for, foreign aid is found to have a direct negative impact on the probability of an ongoing civil conflict to continue (the continuation probability). In other words, our results suggest that increasing aid flows decreases the duration of conflict. We do not however, find a relationship between aid flows and the probability of a civil conflict to start (the onset probability). The size of these effects is economically important. We estimate a marginal effect on the continuation probability of −0.8, implying that a structural increase in aid flows by 10% decreases the continuation probability by 8% points.

To our knowledge this research is the first that efficiently accounts for unobserved heterogeneity in onset and continuation models by first differencing the regression equations. The standard empirical approach on onset and to a lesser extent continuation (because scholars often model duration directly) is to presume i.i.d. errors in linear models, or conditional independence of observations in non-linear probit/logit type models. This standard assumption is highly restrictive as widespread grievances or institutional issues are at least partially unobserved and simply assumed away. The elimination of fixed effects by first differencing and the use of donor GDP as a powerful instrument for aid flows base our results.

We do not find our results directly 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)]. Like our study, Collier and Hoeffler (2002) do not find an effect on onset, but merely argue that aid impacts on conflict through its effect upon national income and reduced primary commodity dependence. They do not however, consider the probability of conflict continuation in their studies. Yet, our study is different as we explicitly attempt to solve some important endogeneity issues. We therefore add to the literature not only by identifying a significant effect of aid flows on continuation, but also by restating that there is no evidence for aid flows to have an effect on onset.

Our results are supporting the theoretical hypothesis from Collier and Hoeffler (2002) as well as some of the empirical results from Collier and Hoeffler (2007). Collier and Hoeffler (2002) reason that aid flows facilitate governments to develop a strong army due to fungibility of aid into military expenditure. Their recent 2007 paper finds empirical evidence for this mechanism and estimates that about 40% of foreign aid transfers into military expenditure (Collier and Hoeffler, 2007). Our results suggest that aid translates into military expenditure more effectively when a country is at war, or when a country is experiencing increased likelihoods of war. It seems intuitive

[27] We are able to use Miguel et al.'s (2004a) rainfall instruments in a stripped down version of our model in first differences, and replicate their substantive findings on economic growth (results not shown but available on request). The model we use in our regressions is more extensive in terms of dynamics, and therefore also a lot more demanding on the instruments.

[28] Not instrumenting aid flows does not affect these findings.

[29] We have not reported these results in our baseline model due to missing data. Results available on request.

that the marginal benefits from investment in the military is larger during war than at peace. A government army suppressing violent rebel groups however, is not necessarily a good thing, as aid flows supports any leadership, also those that are unwarranted by society. Nevertheless, foreign aid flows may be an important tool to policy-makers and aid agencies in preventing future conflict.

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Appendix A

- *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 ($\text{US\$} = \frac{\text{I\$*PPP}_t}{\text{xrat}_t}$*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: $\text{US\$} = \frac{\text{I\$*PPP}_t}{\text{xrat}_t}$*pop*1000.
- Per capita *GDP* for sub-Saharan African countries. The source for this was the Fearon and Laitin (2003b) Database. Per capita GDP has been measured in 1000s 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.
- *Duration variables*. We construct the variable from the civil conflict variable dating back to 1973 (if data is available). Peace duration calculates the number of peace years from the start of a peace period. Conflict duration calculates the number of conflict years from the start of a conflict.
- *Primary commodity dependence*. Fearon (2005) 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 (2003b) database.
- *Real oil prices*. http://research.stlouisfed.org/. Federal reserve bank St. Louis.
- For all of the following variables we used data that were drawn from Fearon and Laitin (2003b). A description of the data can be found in Miguel et al. (2004b) and some additional information is available from http://www.stanford.edu/group/ethnic/workingpapers/addtabs.pdf.
- *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/ Source: Fearon and Laitin (2003b).
- *Ethnolinguistic fractionalization*. Ethnic-linguistic fractionalization based on the Atlas Marodov Mira. Source: Fearon and Laitin (2003b).
- *Religious fractionalization*. Data used from the CIA factbook. Source: Fearon and Laitin (2003b).
- *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 (2003b).
- In *mountainous* Percent Mountainous Terrain. Available data from A.J. Gerard for the World Bank's "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 (2003b).
- In *national population t*−1. Log of population lagged one year. Source: Fearon and Laitin (2003b).

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