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Did the aid boom pacify Sub-Saharan Africa?[1]
Ex-post evaluation using a near-identification approach

by

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ABSTRACT: The incidence of civil war in Sub-Saharan Africa since the turn of the century is less than half of what it was on average in the last quarter of the 20th century. This paper shows that the aid boom triggered by 9/11 played a key role in achieving purposefully this result using panel data for 46 African countries over four decades. It applies a near-identification approach to test the aid-conflict trade-off, taking due account of asymmetric information between the donors and the econometrician. Preference proxies are used in the first-stage to elicit the relevant hidden information.

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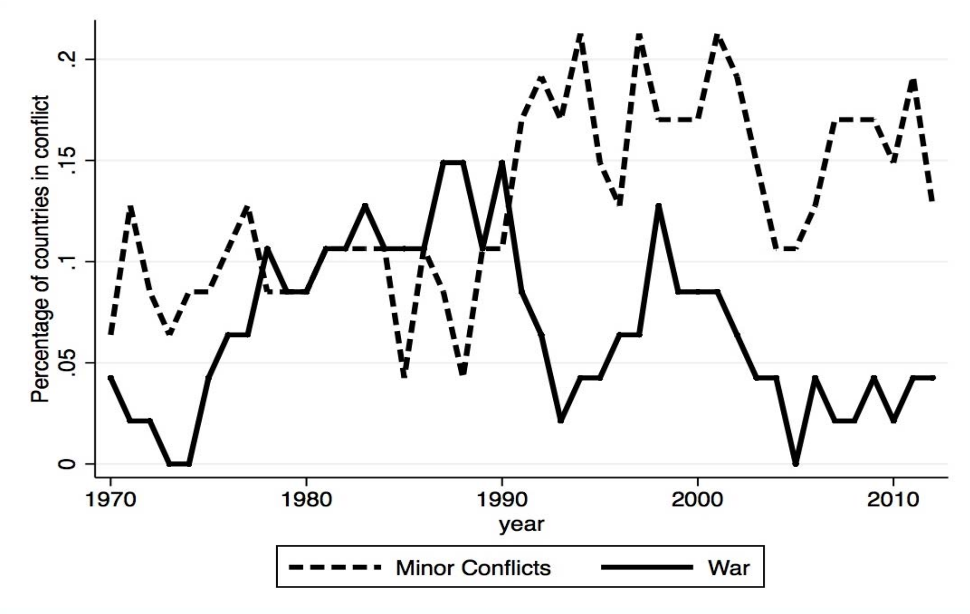
1. Introduction

The 21st century marks a striking contrast for Sub-Saharan African countries relative to the last quarter of the previous one. A kind of African renaissance occurred with remarkable economic performances occurring in a sizable number of countries. Radelet (2010) coined the expression “Emerging Africa” to refer to 17 countries that achieved sizable growth performances in terms of GDP per capita since 1996 and he documents a list of factors that might explain this welcome recovery. The African countries made obviously significant progress on a broad range of fronts, thus raising hopes of a sustained economic growth over many years to come. In a recent publication, a team of IMF economists uses the expression “Sub-Saharan African Frontier Markets” to refer to those countries that made the most progress in financial markets development (IMF, 2013).

Figure 1: Percentage of countries in internal or internationalized conflicts



Data Source: PRIO-Uppsala

This African renaissance is clearly linked to the spectacular improvement in governance in many Sub-Saharan African countries since the fall of apartheid in South Africa and the election of Nelson Mandela at the presidency. Nevertheless, Sub-Saharan Africa is still associated in many people’s minds with civil war and other forms of armed violence. The last 25 years of the 20th century saw a massive increase in the incidence of civil wars in that part of the world, as shown by figure 1. The continuous line describes the number of countries suffering from major civil wars, whether internationalized or not, where more than 1000 battle-related deaths occurred each year. The broken line refers to minor conflicts where the number of fatalities was above 25 per year and less than 1000. Eyeballing the data suggests that civil strife started in the wake of the commodity boom of the 1970s and continued unabated for many years until the end of the century. More than 7% of the countries were at war on average during that period, when counting only the major wars. The curves suggest as well that the 1990s witnessed some reduction in conflict lethality with a temporary fall in the number of countries affected by a major conflict, more or less compensated by an increase in the number of minor conflicts. The end of the century saw the number of major conflicts soar again to its previous level, without any significant fall in the number of minor ones[2].

The average number of major conflicts fell spectacularly since the turn of the century to less than half of what it was in the previous 31 years, as shown in table 1. Explaining this spectacular turn around is the main focus of the present paper. However, this major achievement is partly tarnished by the rise in minor conflicts.

Table 1:

Mean conflict frequencies per period

	1970-2000	2001-2012
Mean Civil Wars	0.07	0.029
Mean Civil Wars & Minors	0.163	0.182

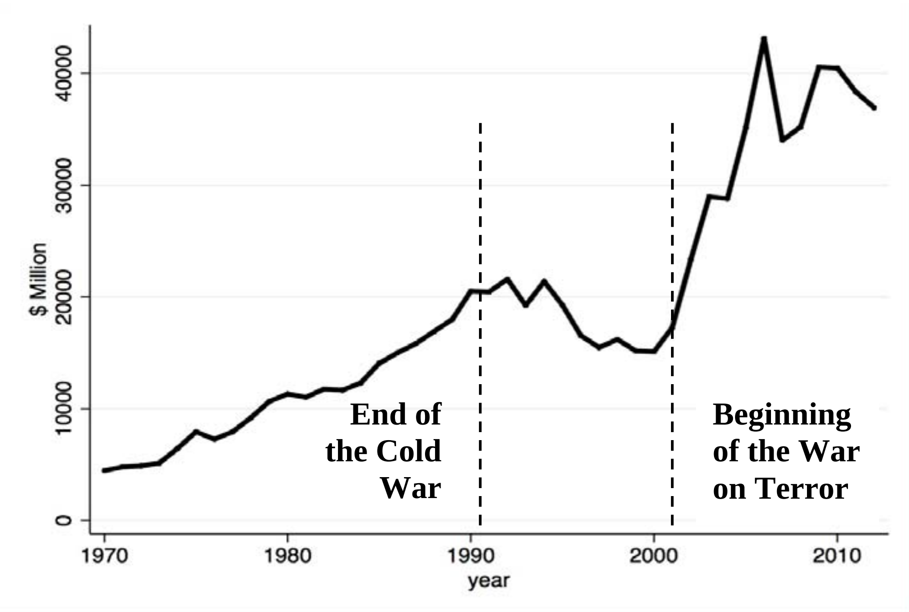
Data Source: PRIO-Uppsala

The econometric analysis presented below supports the view that the aid boom that occurred in the wake of 9/11 and the beginning of the war on terror, depicted at figure 2, was a major determinant of this fall in the incidence of civil war in Sub-Saharan Africa. The aid series is the standard ODA (Official Development Assistance) from the World Bank's African Development Indicators, deflated by a price index that reflects the international purchasing power of the aid money for a representative African economy. We use the manufactures unit value (MUV) index of the exports to low- and lower-middle income countries by the top 15 industrialized countries. The key points are that this index includes the prices of the Chinese and Indian exports that have drastically increased their market shares over the last few decades, thus increasing massively the purchasing power of African commodity exports and aid flows on the world market, and that it is independent of any African country-specific shocks. Three different episodes can be contrasted over these forty years or so. The Cold War

[2] Bates (2008a, 2008b) presents a rich descriptive material on civil wars in "late-century" Africa (1970-1995), pointing out in particular that some countries were at war over the whole period while others did not suffer from any episode of civil war. His empirical analysis uses the existence of private militias as the dependent variable.

saw a steady increase in foreign aid, which grew fourfold over a couple of decades. This ended abruptly in 1991, and foreign aid to Africa fell by about 25% in real terms during the subsequent decade. The 9/11 shock interrupted the downward slide and the War on Terror triggered a massive aid boom, as the aid flow to Africa increased by about 170% in real terms over less than a decade and seems to be bound to remain high in the near future[3].

Figure 2: The 21st century aid boom



Data Source: World Bank. **Note:** Deflated by the Manufactures Unit Value Index of G15 exports to low- and middle-income countries in US $.

This spectacular achievement of foreign aid to Africa stands in sharp contrast to the "aid-ineffectiveness" literature that started with Boone (1996) and Burnside and Dollar (2000), claiming that foreign aid failed to reach its objectives of fighting poverty and boosting economic growth in recipient countries. This diagnosis has been challenged successfully by Arndt et al. (2015) who find that aid positively affects economic growth and some other relevant outcomes, using an instrumental variable approach. Asmus et al. (2017) investigate whether sector-specific aid-targeting improves its effects on growth. Moreover, revealed preference theory suggests another interpretation pointing to a potential hidden agenda behind the philanthropic objectives, as six decades of aid disbursement by rich countries suggest that they were getting something in return. Alesina and Dollar (2000) argue that foreign aid seems

[3] Fleck and Kilby (2010) show that total U.S. bilateral aid experienced a boom starting with the war on terror and they bring out a distinctive change in its determinants, becoming less dependent on "need". Boutton and Carter (2014) also found that US foreign aid has changed since 9/11, becoming more effective against terrorism. Bermeo (2018) provides some historical background to this three-period breakdown.

to pay for political alignment of recipient governments. Azam and Berlinschi (2010) found that rich donors, mainly OECD members, are actively using foreign aid to reduce immigration from low- and lower-middle income countries, as the amount disbursed is endogenous in an equation explaining the number of immigrants in the donors' countries. Azam and Thelen (2010) show that foreign aid is reducing purposefully the number of transnational terrorist events originating in recipient countries as its endogeneity cannot be rejected. Boutton and Carter (2014) show that US foreign aid is successfully focused on countries whose terrorists directly threaten the US but not on protecting its allies[4].

The present paper also tries to discover what foreign aid is good for by looking at its impact on the incidence of civil war in Sub-Saharan Africa, an issue first addressed empirically by Collier and Hoeffler (2002) and de Ree and Ellisen (2009)[5]. From a theoretical perspective, Azam and Saadi-Sedik (2004) present a game-theoretic model where aid is used to buy compliance when the threat of sanctions is made ineffective by a too high cost of imposing them when challenged to do so. The basic framework is an incentive model where the foreign power promises to pay some (aid) money in exchange for the recipient's effort at reducing violence. Azam and Mesnard (2003) and Azam (2006) analyze the government's choice between war and peace in a similar kind of model with special reference to Africa. In these models, the government cannot use a perfectly credible commitment strategy and civil war erupts when the potential rebels expect a higher payoff from rebelling than what the government can promise credibly to transfer to them or invest in deterrence. A similar framework is used by Besley and Persson (2009, 2011) to investigate the choice between peace, repression and civil war from both a theoretical and an econometric point of view. Bates (2008a, 2008b) presents empirical tests of some of its main predictions, emphasizing political institutions and rulers' ethno-regional origins. None of these studies have tested the impact of foreign aid. The two levels of contracting briefly sketched above can be combined à la Azam and Thelen (2010) to produce a model where the foreign power is delegating to the recipient government the task of dealing with the potential rebels in return for a transfer. The key implication of such a framework is that the donor can tilt the balance in favor of peace by making it cheaper than war for some recipient governments. Some implicit or explicit contract

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[4] This brief review makes no mention of the huge literature evaluating the impact of aid-financed projects at the micro-level.

[5] Collier and Hoeffler (2002) analyze the aid/conflict-onset relation world-wide without instrumenting aid. We improve on de Ree and Ellisen by (i) using country-specific instruments rather than continent-wide instruments for foreign aid (they use donors' GDP as instruments), (ii) by instrumenting also domestic GDP p.c. and (iii) by using an extended sample covering almost four decades.

can be offered to enable the latter to credibly promise to give more resources to the potential rebels or to invest in more deterrence than they would without aid.

Such a framework has direct implications for the identification strategy that must be used to estimate the causal impact of foreign aid on the probability of conflict. This is shown in the next section using a simple model that sketches the implications of the endogenous determination of the allocation of foreign aid across recipient countries for econometric purposes. It is an application of the near-identification approach presented in Azam (2016), building on Fisher (1965) and Nakamura and Nakamura (1981). It brings out the type of instrumental variables, called 'preference proxies', which are required to give the econometrician a good chance of testing whether an aid-conflict trade-off exists and whether donors are in fact exploiting it to reduce the incidence of civil war. It also shows that the Nakamura and Nakamura (1981) variant of the Hausman (1978) test is a gauge of the instruments' contribution to improved identification. Section 3 presents the empirical analysis using panel-data with an unbalanced sample of 46 African countries over about four decades. Controlling for both time-invariant country effects and continent-wide time dummies, it shows that foreign aid is a significant inhibitor of civil war in Africa and that donors are using it for that purpose, among other objectives. However, the persistence of the total number of conflicts, including minor ones, leans in favor of a qualified answer to the question raised in the title of this paper. Section 4 shows that the aid boom reduced the lethality of African conflicts by cutting drastically the number of major conflicts, without affecting significantly the overall number of conflicts. Another limit to foreign aid's pacifying power brought out in section 4 is that it has no significant spill-over effects on the neighboring countries, thus requiring a finely designed country-by-country approach. The subsequent section 5 unbundles the continent-wide effects by looking at the impact of various key time-series variables like commodity prices and natural disasters that affect the continent as a whole. Section 6 briefly concludes.

2. Near-identification of the aid-conflict trade-off

No attempt is made at presenting a general theoretical background about the identification of policy trade offs with a special application to aid policy. This section presents instead a simple model that brings out three of the problem's main points, namely:

(i) OLS cannot identify the aid-conflict tradeoff unless the donor is incredibly passive or uninformed,

(ii) This problem can be mitigated by extracting the relevant information from the donors' observed behavior using preference proxies to elicit the hidden information they use,

(iii) The Nakamura and Nakamura (1981) variant of the Hausman (1978) test can be used to assess the validity of the preference-proxies used to improve identification.

2.1. *The Setting.*

The econometrician wants to evaluate "aid effectiveness" in the field of conflict prevention. The impact of aid policy *a*, which is a continuous variable, on a variable measuring the incidence of conflict *w*, is embedded in a linear equation in which *e* and ε are not observed by the econometrician while *x* is an exogenous shift (control) variable or a list of several of them:

$$w = \alpha + \beta x - \gamma a + \delta e + \varepsilon . \tag{1}$$

Unless otherwise specified, all the Greek-letter parameters are defined as positive. For the sake of simplicity, assume that $E(e) = E(\varepsilon) = 0$; if they had non-zero expected values, the latter would simply be added to the intercept to yield the equivalent specification with a different α. The aid effectiveness claim is that γ is non zero and works in the desirable direction, say $\gamma > 0$ for the sake of concreteness. Then, in order to test it, the econometrician needs to identify (1) as closely as possible.

There is no reason to assume without further testing that the representative donor is incompetent and stupid. The "Public Choice revolution" of the 1960s, initiated by people like James Buchanan and Gordon Tullock, has exerted a sufficient influence on the profession to preclude the credible use of this type of assumptions without testing. Let us make instead the following three assumptions:

Assumption 1 (Asymmetric Information)**:** The representative donor observes x, a and e before making her decision**,** and then she observes *w* ex post, while the econometrician only observes w, x and a ex post.

Assumption 2 (Efficient Information Processing): The donor makes the best use of her information so that $E(e\varepsilon) = 0$.

Assumption 3 (Quasi-Linear Loss Function**):** The donor seeks to minimize the following objective function taking the expected value of (1) as the constraint:

$$\min_a L \equiv \pi a + (1/2)(\theta + E(w))^2 \text{ s.t. } E(w) = \alpha + \beta x - \gamma a + \delta e, \tag{2}$$

where $E(w)$ is a shorthand notation for $E(w|\, x, a, e)$. Assumptions (i) and (ii) are natural to make for any rational-choice scientist. Assumption (iii) is not very general, but it is chosen because it yields convenient predictions that keep the resulting econometric specifications linear and tractable. The linear cost of a in (2) captures the fact that foreign aid is usually such a small share of fiscal revenues that assuming an increasing marginal cost would strain credulity. In this specification, $\theta > 0$ is a preference parameter of the donor, which is her private information. A higher θ means that the policy maker is more sensitive to the outcome variable w, as:

$$\frac{\partial L}{\partial E(w)} = \theta + E(w) \text{ and } \frac{\partial^2 L}{\partial \theta \partial E(w)} = 1 . \tag{3}$$

Figure 3 depicts a case where an interior optimum exists, with $\alpha + \beta x + \delta e > (\pi/\gamma) - \theta > 0$. Corner solutions may also exist outside this range.

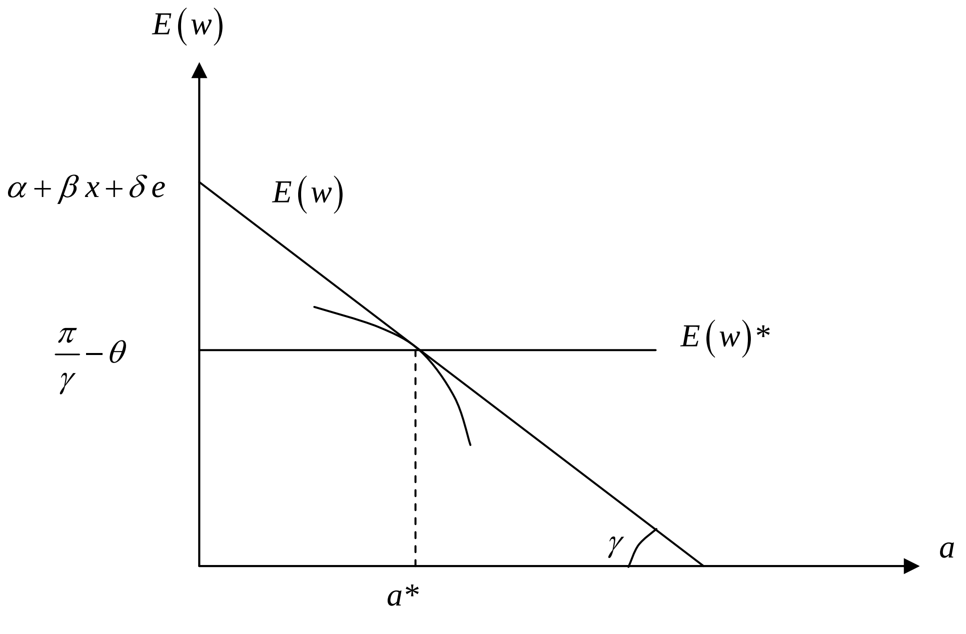

Figure 3: Donor’s optimum.

The downward-sloping straight line labeled $E(w)$ represents (1) with ε set to zero. The downward-sloping concave curve depicts an indifference curve derived from the loss function (2). The optimum point is classically found where the indifference curve is tangent to the constraint. The first-order condition for this minimization exercise (2) reads:

$$E(w)^* = \frac{\pi}{\gamma} - \theta . \tag{4}$$

Hence, the policy-maker's optimum choice is found at the intersection of the deterministic part of (1), i.e., setting $\varepsilon = 0$, with a straight line whose equation may be written as $E(w) = \pi/\gamma - \theta$. This line describes the target conflict level chosen by the donor. It is represented in figure 3 as the horizontal line labeled $\pi/\gamma - \theta$. Figure 3 may be used to bring out the key part played by variations in the policy maker's preferences in identifying $E(w)$. A *ceteris paribus* increase in θ would entail a downward shift of $(\pi/\gamma) - \theta$, thus tracing out the $E(w)$ line. Unfortunately, these changes in θ cannot be inferred directly from observing changes in the donor's behavior.

Substituting for $E(w)^*$ in the first-order condition and rearranging the terms allows us to derive the reduced-form equation for a^* that describes the donor's aid allocation rule chosen in the interior solution:

$$a^* = \frac{\gamma\,\alpha - \pi}{\gamma^2} + \frac{\beta}{\gamma} x + \frac{\theta + \delta\, e}{\gamma} . \tag{5}$$

Outside the $\alpha + \beta\, x + \delta\, e > (\pi/\gamma) - \theta > 0$ range, $a^* = 0$ if θ is "too small" and $E(w) = 0$ if θ is "too large". Notice that this aid-allocation rule (i) includes some of the donor's information that is not available to the econometrician, namely θ and e, while it does not include ε, and that (ii) its coefficients are mongrel parameters of the relevant parameters of the structural equation (1) and the donor's loss function (2). Notice that these parameters' denominator γ measures the difference between the slopes of (4) and (1). Still, it turns out that (5) provides the foundation of the solution to the identification problem as explained in the next sub-section.

2.2. The Identification Problem.

Now, assume that the econometrician has a sample of observations of w, x and a, as well as a few other variables used below, indexed implicitly by $i \in S$. Given the information available to him, the econometrician may try to estimate:

$$w = \alpha + \beta x - \gamma a + f \,, \tag{6}$$

where $f \equiv \delta e + \varepsilon$. This "random disturbance" term is in fact a function of a, x and θ because one can substitute for δe from (5) after rearranging the terms to write:

$$\delta e = \frac{\pi - \alpha \gamma}{\gamma} - \beta x - \theta + \gamma a^* . \tag{7}$$

Therefore, substituting (7) into (6) yields a second relation between w and a (and ε as well), which is the econometric counterpart of (4):

$$w = (\pi/\gamma) - \theta + \varepsilon . \tag{8}$$

However, θ is not observed so that (8) is not directly useful for the econometrician. If there are several donors involved in producing the outcomes captured by this sample, or if the donor's preferences vary over time, then unobserved θ will in general vary across $i \in S$. Let us define $\bar{\theta}$ as its mean and $\eta \equiv \theta - \bar{\theta}$ as its deviations from the mean, with $\sum_{i \in S} \eta = 0$. Then (8) may be written as:

$$w = (\pi/\gamma) - \bar{\theta} + (\varepsilon - \eta) . \tag{9}$$

Although it is very poor, (9) may be regarded as a potential econometric equation where neither x nor a turned out significant. It follows that any linear combination of (6) and (9) has the same structure as the former; this precludes identification of (6) using the usual parameter restriction approach. Notice that the difference between the coefficients of a in (9) and in (6) is γ, which may thus be interpreted as the bias that would result from estimating (9) rather than (6). There is no reason to expect that the deviations from the mean of the policy makers' preference parameters will be correlated with ε and it is thus natural to assume that $E(\varepsilon \eta) = 0$. Then the following identification failure proposition can be proved simply (see online appendix).

Proposition 1: Equation (6) cannot in general be identified by OLS as the latter will select a linear combination of (6) and (9) instead. The weight given to (6) for a large enough sample, which may be called its degree of identification, will be:

$$\lambda_{OLS} = \frac{E\left(\eta^2\right)}{E\left(\eta^2\right) + \delta^2 E\left(e^2\right)} < 1. \tag{10}$$

The intuition for proposition 1 is that OLS will minimize the sum of squared residuals of a linear combination of (6) and (9) by assigning the weights λ_{OLS} to (6) and $1-\lambda_{OLS}$ to (9), as shown by Azam (2016) and in the online appendix. Equation (10) shows that perfect identification will only be achieved by OLS if either $\delta^2 E\left(e^2\right) = 0$ or if $E\left(\eta^2\right) \to \infty$, two unrealistic assumptions. Figure 3, can again be used to understand intuitively this identification failure result. An unobserved increase in *e* would shift the $E(w)$ (downward-sloping) line upwards. Hence, variations in *e*, given the other variables and parameters, will in fact trace out the $(\pi/\gamma) - \theta$ line rather than the $E(w)$ one. Comparing the two comparative-statics predictions with respect to the unobserved parameters $\{\theta, e\}$ helps us understand that OLS will give more weight to (6), the larger are the variations of θ, and hence of η, relative to those of e, and vice versa. In other words, our identification failure diagnosis simply boils down to an omitted variable bias, the omitted variable e being a piece of information used by the donor but unobservable to the econometrician. In contrast, unobserved variations of the representative donor's preferences η over the sample are playing on the side of the econometrician who tries to estimate the aid-conflict trade off.

However, this omitted variable problem might be mitigated by using a good proxy that could be substituted to unobserved *e* in order to provide a consistent estimate of (1). The main lead to find it is obvious from (5). We observe that $a*$ is responding to the increase in *e* by a seemingly unexplained increase, given θ and the variables observed by the econometrician. The endogeneity of $a*$ thus entails that the latter will reveal changes in unobserved *e* as unexplained deviations from the fitted values of $a*$ that the econometrician can estimate given his available information set, provided some good enough proxy for θ is found. This shows the way to follow in the search for a proxy for e.

2.3. *Signal Extraction and Test.*

Assume now that the econometrician's data set includes one or more variables that are liable to be jointly correlated with θ while they are not included in *x*. Let our econometrician assume that:

$$\theta = \rho + \mu z + \zeta, \text{ with } E(\zeta) = E(\zeta e) = E(\zeta \varepsilon) = 0 . \quad (11)$$

Notice that the signs of $\{\rho, \mu\}$ are unknown. Then, he can use z as an instrument, in the standard terminology, although it should more properly be called a preference proxy, as θ is not observed directly. The first-stage equation reads:

$$a = \hat{b}_0 + \hat{b}_1 x + \hat{b}_2 z + \hat{g} . \quad (12)$$

Table 2 shows what each of these coefficients is estimating in the present framework. It is clear that these are complicated mongrel parameters whose exact significance and statistical properties are far from obvious, because the expected value of a ratio is not equal to the ratio of the expected values of its numerator and its denominator. Notice in particular that the maximum potential bias to be corrected comes as the denominator of these coefficients, which will thus be smaller, the greater the stakes of the identification process. Moreover, the correct standard error of the underlying parameters is anybody's guess. Hence, the standard practice of testing the significance of the coefficients of the first-stage equation must be considered with caution for diagnosing identification.

Table 2:

The first-stage mongrel parameters

$\hat{b}_0$	$\hat{b}_1$	$\hat{b}_2$	$\hat{g}$
$\frac{\gamma(\alpha+\rho)-\pi}{\gamma^2}$	$\frac{\beta}{\gamma}$	$\frac{\mu}{\gamma}$	$\frac{1}{\gamma}(\delta e + \zeta)$

Note: This table shows the correspondence between the parameters of (11) and the deeper parameters of the model.

If our econometrician now includes $\hat{g}$ in his second-stage equation as in:

$$w = \alpha + \beta x - \gamma a + \phi \hat{g} + \nu , \quad (13)$$

then ϕ is an estimate of γ so that ϕg is an estimate of $\delta e+\zeta$, which captures most of the donor's private information, and proposition 2 follows (see appendix for the proof).

Proposition 2: OLS applied to (13) will estimate a linear combination of (13) and (9), giving the former a weight equal to:

$$\lambda_{PP} = \frac{E\left(\eta^2\right)+E\left(\varepsilon^2\right)}{E\left(\eta^2\right)+2E\left(\varepsilon^2\right)} > \frac{1}{2}. \tag{14}$$

Therefore, the two-stage approach sketched above, which boils down to an application of the control-function approach, may improve the econometrician's prospects of identifying (1) and hence the impact of a on w, resulting in $\lambda_{PP} > \lambda_{OLS}$, if:

$$\delta^2 E\left(e^2\right) > \frac{E\left(\eta^2\right)E\left(\varepsilon^2\right)}{E\left(\eta^2\right)+E\left(\varepsilon^2\right)}. \tag{15}$$

Moreover, this approach provides the natural test of closer identification by testing whether $\phi = 0$ in (13). This is essentially what the Hausman (1978) test of endogeneity, as reformulated by Nakamura and Nakamura (1981), does. Following the same steps as in Azam (2016), *mutatis mutandis*, one can easily show that:

$$\phi = \left(\frac{\lambda_{PP} - \lambda_{OLS}}{\lambda_{PP}}\right)\gamma. \tag{16}$$

Hence, the key message of this exercise is that if ϕ is not significant in (13) it may either mean (i) that there is no aid-conflict trade off or the donor is not interested in using aid strategically for reducing the risk of conflict or (ii) that the instruments have failed to provide enough information to identify (13) more closely than OLS. Otherwise, one may conclude that the donor is exploiting the trade off and that the preference proxies used in the first stage have improved identification. The key outcome of the first-stage OLS estimation is that the estimated residuals that it produces are correlated with e and not with ε. They really capture the relevant private information of the policy maker. In other words, the key requirement of this two-stage approach to identifying policy trade-offs is that the residuals of the first-stage equation must be orthogonal to the proxy variables that capture the policy maker's preferences in order to extract the signal of the unobserved information she uses in making her decisions. This is what must guide the econometrician in his choice of “instruments” and it is safer to use several preference-proxies to make sure that the relevant unobserved information so captured is as little contaminated by the policy-maker's preferences as possible. This is likely to mitigate also the additional problem raised by the presence of ζ in $\hat{g}$, which makes the latter a noisy estimate of δe. This entails a measurement-error problem and hence an attenuation bias, which is in fact related to the so-called 2SLS bias. Therefore, the two-stage approach sketched above is admittedly biased towards rejecting endogeneity and the validity of the preference-proxies used. This in turn enhances the confidence that may be placed in the model when the opposite diagnosis occurs.

3. Estimating the foreign-aid/civil-war trade-off.

This section tests the core hypothesis that the aid boom of the 21st century played a key part in abating civil war in Africa in the 2000s using an unbalanced panel of 46 countries over about four decades, starting mostly in 1970[6]. South Sudan and Somalia are not included because of missing data especially regarding the level of gross national product. The series of Angola, Cabo Verde, Comoros, Equatorial Guinea, Eritrea, Ethiopia, Guinea, Mauritius, Mozambique, Namibia, São Tomé and Principe, South Africa, Tanzania and Uganda do not start in 1970. However, our results are robust if we exclude South Africa and to several other restrictions, e.g., if we only use countries for which more than two thirds of the years are available, i.e., excluding Eritrea, Guinea, São Tomé and Principe and Tanzania.

The war data come from the UCDP/PRIO Armed Conflict dataset (Gleditsch et al. 2002). They include the number of civil wars with at least 1,000 casualties and of minor armed conflicts with more than 25 deaths (and less than 1000) in a year over the 1970-2012 period. The aid flows are represented by the classic ODA (Official Development Assistance) from the World Bank's African Development Indicators (ADI). This captures the actual disbursements of the aid money, which is often released by tranches in the wake of commitments. As mentioned above, the aid series is deflated by the MUV index of the top 15 industrialized countries' manufactures exports to low- and lower-middle income countries from the World Bank Commodity Price Data (The Pink Sheet). Additional variables like GDP p.c. (also deflated by MUV) and population size (both from ADI) are used to disentangle the effects of foreign aid from those of low income per capita and population size that are known to be correlated with it.

[6] Blattman and Miguel (2010) provide a comprehensive survey of the econometric analysis of civil wars that would make a literature review superfluous in this paper. They emphasize the need to control carefully for endogeneity, in particular for GDP, what was somehow overlooked in the early empirical literature on civil war.

Table 3.a

Internal and internationalized conflicts (control functions)

	Civil Wars		Civil Wars & Minors	
	(1)	**(2)**	**(3)**	**(4)**
Log GDP p.c.	-0.0831***	0.0965	-0.1026***	-0.2692***
	(0.01)	(0.07)	(0.02)	(0.09)
Log Pop.	-0.2568***	-0.2400***	-0.2915***	-0.3150***
	(0.06)	(0.07)	(0.08)	(0.09)
Log ODA p.c.	-0.0475***	-0.1481***	-0.0408***	0.0065
	(0.01)	(0.04)	(0.01)	(0.06)
Res. Log GDP p.c.		-0.1890***		0.1779*
		(0.07)		(0.09)
Res. Log ODA p.c.		0.1035**		-0.0476
		(0.04)		(0.06)
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Nb. of Obs.	1761	1761	1761	1761
Joint Res. T.	-	9.03***	-	3.72
F. stat	2.19***	2.22***	2.34***	2.28***

Note: Columns (1) and (3): Fixed Effects linear probability estimation using robust standard errors (in parenthesis). Columns (2) and (4): Hausman test for endogeneity using the residuals from the first-stage equation presented at table 4, robust standard errors. Stars refer to the standard convention: {***, **, *} mark the significance levels {1%, 5%, 10%}.

3.1. The Pacifying Impact of Aid in Africa.

Table 3.a presents various findings regarding the determinants of the probability of conflict in a country-year. The model used is a linear probability model[7] including fixed effects that control for time-invariant country characteristics and time dummy variables that control for continent-wide shocks[8]. Columns (1) and (3) do not control for endogeneity and robust standard errors are presented in parenthesis. Column (1) restricts the analysis to civil wars entailing at least 1000 battle-related deaths per year while column (3) adds the minor

[7] We have also experimented with a Logit model, yielding qualitatively the same conclusions. The latter has two drawbacks relative to the linear probability model: (i) when applied to panel data, it excludes from the sample all the countries that did not incur any civil war or minor conflict over the period, entailing a risk of a selection bias, requiring a two-stage approach on top of the two-stage approach required for controlling endogeneity, and (ii) its coefficients cannot be interpreted immediately and need to be translated into comparable coefficients to the ones from table 1 using fairly conventional scalars (see, e.g., Hsiao, 1986).

[8] Section 5 below presents an attempt at unbundling these continent-wide effects to bring out additional policy tools available to rich countries.

conflicts that produce at least 25 battle-related deaths per year. These regressions are highly significant and the three continuous variables included are all significant at the 1% threshold.

On the face of it, the estimate of the impact of ODA p.c. on the probability of a civil war in a given country-year in column (1) means that a doubling of per capita ODA would reduce the probability of a civil war affecting the recipient country in any given year by 4.75 percentage points (%). This is slightly higher than the fall in the mean frequency of civil war shown at table 1. To evaluate correctly the meaning of this number, one must bear in mind that the average such probability for the whole sample is 5.7%. Hence, foreign aid is found here to be highly effective at abating civil war. The other two continuous variables, i.e., GDP p.c. and population seem to be even more powerful inhibitors of civil war. However, a doubling of these variables is not the relevant thought experiment to use in these cases. Hence, a 1% increase in population is here predicted to reduce the risk of civil war by 2.57%, assuming that GDP and ODA increase in the same proportion, which is not an insignificant impact either. As far as GDP p.c. is concerned, the estimated impact would be 0.83% for a 10% increase. Hence, given the relevant ranges of variation of these variables, ODA p.c. comes up as a key policy variable for the sake of preventing civil wars.

Given such a significant policy trade-off, however, one might argue that the international community is probably exploiting it in fact already to determine its allocation of foreign aid across countries and across time with a view to control civil violence in recipient countries. This would happen if the rational policy-makers were at least as clever as the econometricians and wanted to reduce the potential collateral damage of civil wars to their political and economic interests in Africa. This would require a different identification strategy than at columns (1) and (3) if they benefitted from some information on the risk of civil war in the different sample countries that is not available to the researcher. In that case, the near-identification approach sketched in section 2 must be used to handle the potential endogeneity bias resulting from the donors rationally using such information for making their aid-allocation decision. Besides, some less strategic motivations might also be present and require similar econometric precaution, e.g., if donors cannot deliver normally foreign aid to a country when the latter is at war, entailing a reverse-causation problem. Moreover, columns (1) and (3) do not control either for the likely endogeneity of GDP p.c.. There is a fair presumption that reverse causation is at work as the occurrence of violent conflict is bound to disrupt economic activity and to reduce GDP in the country where it takes place. It is also likely that some unobserved time-variant country-specific shocks have a simultaneous impact on output and on the probability of violent conflict. Hence, GDP p.c. must probably be treated as endogenous as well.

Columns (2) and (4) present the Hausman endogeneity test as reformulated by Nakamura and Nakamura (1981) showing that this cannot be rejected here for either variable. They thus perform the part of the control function sketched in section 2[9]. The residuals from the reduced-form equations explaining foreign aid per capita and GDP per capita presented in table 4 at columns (5) and (6) capture in a synthetic fashion the impact of unobserved variables on donors' behavior and on GDP p.c. and they are orthogonal to the included exogenous variables and instruments, by construction. They are especially significant in column (2), relative to civil wars, while they are less so at column (4), when minor conflicts are included. The coefficient of ODA p.c. is higher in absolute value in column (2) when the residuals from the first-stage equations are included than otherwise, suggesting that these aid shocks reveal some relevant information that is unavailable to the econometrician and that affects positively and simultaneously the amount of aid delivered and the probability of civil war. The sum of the two coefficients for ODA p.c. and its residuals is almost equal to the estimates of the impact of ODA p.c. at column (1). This suggests that these residuals perform like a control function and correct the endogeneity bias present in column (1). This seems to reveal that donors respond to some information that signals an increased risk of civil war (and that is unobserved by the researcher) by stepping up their delivery of foreign aid to the affected country. After controlling for foreign aid's endogenous response, column (2) shows that foreign aid is strongly effective for reducing the risk of civil war, with an impact that is somewhat underestimated in column (1). This estimate shows that the reduction in the risk of civil war found at table 1 could in fact be achieved by an average increase in foreign aid by about 25% since the turn of the century relative to the previous period, *ceteris paribus*. Comparing the estimates found at columns (1) and (2) for the impact of ODA seems to suggest that the latter is overestimated at column (2). However, section 5 provides empirical arguments to the contrary by showing that foreign aid was effective at abating civil war in Africa despite two massive exogenous shocks that stacked the odds against peace. By contrast, GDP p.c. looses its significance at column (2), and the estimated coefficient changes its sign, while its residuals are strongly significant. This clearly shows that the negative

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[9] This procedure yields the same estimates as 2SLS when the regressors in question are endogenous as shown by looking at table 3.b that presents the conventional 2SLS findings. The coefficients and the standard errors are the same up to the fourth decimal, although we haven't done the bootstrapping advised by Wooldridge (2010, p.118). For a linear model, the two approaches are thus indistinguishable, but 2SLS makes it easier for STATA to compute the various tests of instruments validity presented there.

impact of GDP p.c. found at column (1) is probably only capturing reverse causation rather than any meaningful behavioral impact.

Table 3.b:

Conventional 2SLS with tests of instruments validity[10]:

	Civil Wars (2.b)	**Civil Wars & Minors** (4.b)
Log GDP p.c.	0.0964	-0.2692***
	(0.07)	(0.09)
Log Pop.	-0.2399***	-0.3150***
	(0.07)	(0.09)
Log ODA p.c.	-0.1481***	0.0064
	(0.04)	(0.06)
Country FE	Yes	Yes
Year FE	Yes	Yes
Nb. of Obs.	1761	1761
Under-Identification Test	45.40***	45.40***
(Kleibergen-Paap rank LM statistics		
Weak Idenitification Test	15.22	15.22
(Kleibergen-Paap rank Wald F Stat)		
Endogeneity test	8.63**	3.60
Sargan Test	0.12	0.33
(overidentification test of all instruments)		
Root MSE	0.2057	0.28
F Stat.	1.55***	1.97***

Table 3.b presents the same results produced by standard 2SLS for columns (2) and (4), excluding Res. Log GDP p.c. and Res. Log ODA p.c., and adding the standard tests of instruments' validity (Kleinbergen-Paap, Sargan, etc.). The estimates are differing only at the fourth decimal and the conclusions are the same, namely that foreign aid is effective at preventing major conflicts but not at reducing the incidence of minor conflicts.

[10] **Under-identification test (Kleibergen-Paap rank LM statistic):** the test is equivalent to the Cragg and Donald test but more appropriate with robust covariance estimator. We reject the null and thus the matrix is full rank and we have identification. **Weak identification test (Kleibergen-Paap rank Wald F Stat):** the test is equivalent to the test of Stock and Yogo but more appropriate with robust covariance estimator. The null hypothesis tested is that the estimator is weakly identified in the sense that it is subject to bias. The statistic is equal to 15.22, a *F* value above 10 indicates that the null can be rejected and thus there is no weak-instrument problem in estimation (A5.1) and (A5.2). **Endogeneity test:** the null hypothesis tested is that the specified endogenous regressors can actually be treated as exogenous. We reject the null in (2.b) but not in (4.b). **Sargan test:** test of over-identifying restrictions also known as the Hansen *J* statistic. The null hypothesis tested is that the full set of orthogonality conditions are valid. We do not reject the null for both equations.

The aid endogeneity finding tells us something about the type of information that donors use to make their allocation decisions across recipient countries. The signal that they get about the increased risk of civil war in a given country/year is an early-warning device that gives them a first-mover advantage for controlling civil strife. The coefficient of the ODA residuals in column (2) tells us how much higher would be the risk of civil war in a given country/year had the aid flow not increased in response to the early-warning signal that the donors received when the risk arose. This might arguably be viewed as a tribute to the intelligence-gathering performed by the donors to inform their aid allocation, which justifies the use of the near-identification approach sketched at section 2.

3.2. First-Stage Reduced-Form Determinants of Foreign Aid and GDP p.c.

Table 4:

First-stage equations for Log ODA p.c. and Log GDP p.c.

	Log ODA p.c. (5)	**Log GDP p.c. (6)**
Log Pop.	0.1080	0.0428
	(0.24)	(0.14)
Trend * French Colonies	-0.0265***	0.0002
	(0.01)	(0.00)
Trend * UK Colonies	-0.0081	0.0135***
	(0.01)	(0.00)
Nb. of Natural Disasters	0.0407***	-0.0199***
	(0.01)	(0.01)
Country FE	Yes	Yes
Year FE	Yes	Yes
Nb. Obs.	1761	1761
F stat	7.5168***	62.6406***

Note: OLS with robust standard errors.

Table 4 presents the reduced-form equations estimated to produce the residuals used in the Hausman endogeneity test in table 3.a's columns (2) and (4). Although these first-stage estimates are mongrel parameters that cannot be understood as causal, as shown at (5), they suggest that the allocation of foreign aid across country/years mainly responds to two key stimuli: (i) Donors are providing some implicit insurance against natural disasters, here measured by the number of such disasters according to the international Disaster Database (EM-DAT). This includes natural disasters categorized as geophysical (e.g. earthquake), meteorological (storm), hydrological (flood), climatological (e.g. drought and wildfire) and biological (epidemic) events. Foreign aid responds positively to the occurrence of such shocks. Table A2 in the appendix shows that such natural disasters do not affect the risk of civil war directly and thus satisfy the exclusion restriction for instrumental variables. Not surprisingly, this variable has a significant negative impact on GDP p.c.[11]. We may conclude that donors have some genuine humanitarian motivations beside the political objectives that we emphasize here because natural disasters do trigger a positive response of aid without impacting directly on the probability of conflict at the country level. Section 5 below provides some qualification to this statement.

(ii) Donors are also using foreign aid strategically to prevent African economies from being drafted into major world-wide conflicts and to purchase their alignment. The war on terror revived the aid flow to Africa, which had lapsed by about 25% in the wake of the end of the Cold War, as seen at figure 2. This must be interpreted in conjunction with the two trend variables capturing the evolution of aid to the French and British former colonies, showing that the share of traditional donors is declining over time while non-traditional ones, like China and the U.S., have become sizable players in recent years, whereas they were mostly absent until the turn of the century[12]. Still, former British colonies get (insignificantly) less aid over time but tend also to grow faster, while such a correlation is not present for former French colonies. Appendix tables A3 and A4 show that these variables pass the exclusion test and are thus appropriate instruments.

4. Some limits on foreign aid's pacifying impact

This section helps us to delimit more precisely what foreign aid achieves regarding the control of internal conflicts in Sub-Saharan Africa. It first shows that foreign aid has no significant impact on the overall number of conflicts taking place in SSA over the sample period once the minor ones are included and that it was not allocated with a view to perform such a task. Second, it shows that the allocation of aid across countries is carefully designed on a country-by-country basis as its pacifying impact does not spill over to neighboring countries. Hence, no argument can be made to increase foreign aid to SSA in order to internalize some positive externality beyond its impact on the recipient country.

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[11] Miguel et al. (2004) is the classic reference on the use of climatic variables, rainfall in particular, as instruments for estimating the impact of GDP on the risk of conflict.

[12] Fleck and Kilby (2010) and Boutton and Carter (2014) provide some clues about the changing role of US aid across these two periods. Dreher and Fuchs (2012) analyze China's foreign aid, showing that it does not deserve the label 'Rogue Aid'.

4.1. The Persistence of Minor Conflicts.

Columns (3) and (4) add minor conflicts to civil wars, thus increasing sizably the number of conflicts to be explained as seen at table 1. In column (3), foreign aid seems slightly less powerful for abating minor conflicts than at column (1), while GDP p.c. and population are more powerful instead. However, aid looses entirely its significance when endogeneity is controlled for at column (4). Moreover, the residuals of the reduced-form ODA p.c. equation are not significant either in this case. This suggests that donors do not respond in the same fashion when minor conflicts are involved as they do for civil wars, as neither ODA p.c. nor its residuals are significant at column (4). It might be so either because it is more difficult to collect useful intelligence about them or because there is nothing much at stake for donors in this type of conflict. When minor conflicts are added, at column (4), the negative impact of GDP p.c. remains significant (unlike at column (2)) when controlling for endogeneity and it becomes stronger than at column (3). Its residuals are significant at the 10% threshold, with a positive sign. This suggests that there are unobserved shocks that affect positively both GDP p.c. and the risk of minor conflict, while the basic impact of GDP p.c. is negative. In other words, while economic development seems to reduce the risk of minor conflicts in the long run, short run unexpected booms might instead spread havoc.

There are thus significant differences between the determinants of civil wars, understood here as major conflicts, and of minor conflicts. This comes out here despite the fact that the former are included in the dependent variable used when minor conflicts are also included at columns (3) and (4), suggesting that these differences are strongly meaningful. This provides some support for the view of Collier (2000) and Bueno de Mesquita (2013) that the escalation phenomenon, whereby a minor conflict can evolve into a major one, deserves to be analyzed in its own right. However, our findings support the view that the amount of foreign aid received by the government is a key determinant that can prevent this escalation, while their analyses focus entirely on the rebel side. Further research is certainly needed to clarify the relative roles of rebels and government in the escalation process[13].

4.2 Insignificant Spillovers.

[13] Kalyvas and Balcells (2010) provide an analysis of some qualitative changes that affected the pattern of internal conflicts in the world when the cold war ended. They emphasize the interactions between governments and rebels that entailed a reduction in the size of the forces involved in many countries. Their period of analysis ends in 2004, before the aid boom entailed by the war on terror became obvious.

Table 5 shows that the level of foreign aid received by each country's neighbors has no significant impact on its own risk of conflict, despite the cross-border effects estimated by Bates (2008b). In table 5, this negative finding holds for both civil wars and the total number of conflicts, Minors included. This suggests that aid-recipient governments do not (or cannot) control significantly the cross-border activity of guerrillas based on their own territory. Table A.5 confirms these findings by standard 2SLS with the same tests of instruments' validity as in table 3.b.

Table 5:

Test of cross-border spillovers

	Civil Wars		Civil Wars & Minors	
	(7)	**(8)**	**(9)**	**(10)**
Log GDP p.c.	-0.0828***	0.0953	-0.1025***	-0.2705**
	(0.01)	(0.07)	(0.02)	(0.09)
Log Pop.	-0.2604***	-0.2414***	-0.2928***	-0.3167***
	(0.06)	(0.07)	(0.08)	(0.09)
Log ODA p.c.	-0.0473***	-0.1485***	-0.0408***	0.0061
	(0.01)	(0.04)	(0.01)	(0.06)
Log ODA p.c. Neighbors	0.0056	0.0017	0.0020	0.0021
	(0.01)	(0.01)	(0.01)	(0.01)
Res. Log GDP p.c.		-0.1873***		0.1800*
		(0.07)		(0.09)
Res. Log ODA p.c.		0.1041**		-0.0469
		(0.04)		(0.06)
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Nb. of Obs.	1761	1761	1761	1761
Joint Res. Test	-	8.85***	-	3.73
F stat	2.16***	2.18***	2.31***	2.26***

Note : The columns present the findings of the same kind of estimation as in table 3.a, just adding the logarithm of the average level of foreign aid per capita in neighboring countries (deflated by the MUV index) in those estimations and in the reduced-form equations. For the islands of the sample the closest countries are used as neighbors. Stars refer to the standard convention: {***, **, *} mark the significance levels {1%, 5%, 10%}.

5. Unbundling continent-wide effects

In tables 3-5, continent-wide effects are controlled for using time-dummy variables. This is the appropriate method to use in panel data analysis as it controls both for observable and unobservable variables that affect simultaneously all the sample countries. However, researchers and policy makers may be also interested in unbundling these effects, with a view to identify the key continent-wide shocks that affect significantly the incidence of civil war in

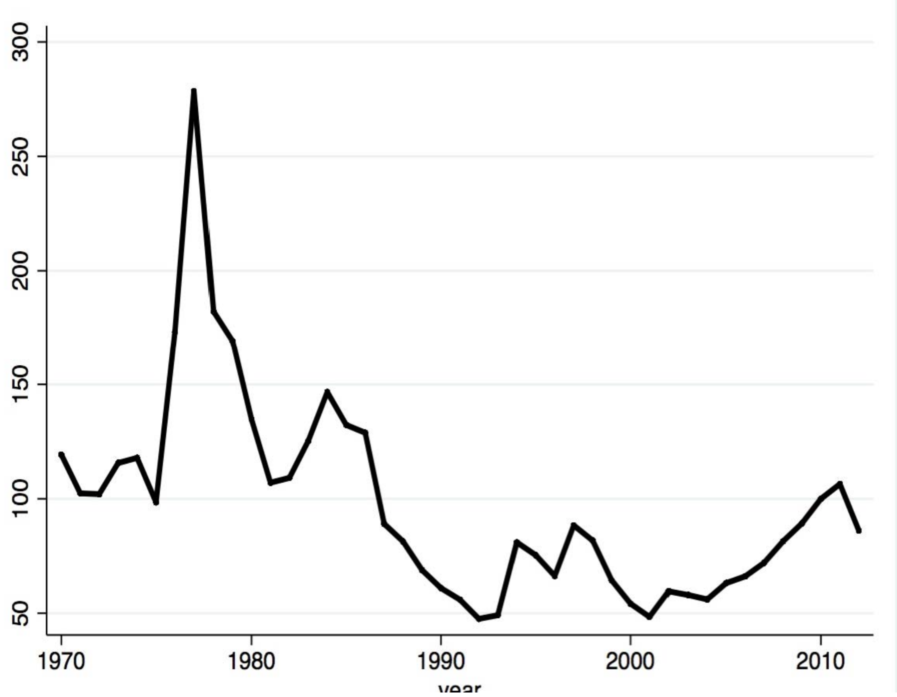
Sub-Saharan Africa. In particular, Besley and Persson (2009) have found significant impacts of export and import prices while Fearon (2005) and Humphreys (2005) emphasize fuel exports. Similarly, Bates (2008b) finds some impact of the price and output of oil. Brückner and Ciccone (2010) show that major shortfalls in international commodity prices are significant determinants of civil war onsets in Sub-Saharan Africa. As discussed among others by Azam (2006), wide swings in commodity prices are liable to change drastically the relative affluence of different ethno-regional groups in African countries and can upset the established political equilibrium. This is especially relevant since the turn of the century, as the war on terror in the wake of 9/11 and the ensuing monetary policy pursued by the FED triggered a commodity boom of the same order of magnitude as the historical oil shock of the 1970s. We now test whether these commodity-price shocks explain a significant share of the impact of the continent-wide shocks captured by the time dummies at table 3. The latter's coefficients measure how much higher (or lower) was the probability of conflict in an average African economy due to the combination of continent-wide shocks that occurred each year. The commodity prices and the composite indexes that we use come from the World Bank Commodity Price Data (The Pink Sheet) as does the MUV index used for deflating them[14].

Beside commodity prices, we also test the impact of natural disasters at the continent-wide level. In the spirit of Miguel et al. (2004), Hsiang et al. (2013) have performed a meta-analysis of 60 primary studies of the links between climate variables and conflict outcomes. These primary studies are taken from a wide range of fields and cover a very deep historical time span. They conclude that a one-standard deviation increase in temperature or towards more extreme rainfalls entails a 14% median increase in the incidence of conflict between groups. In order to capture this kind of effects, we use again the EM-DAT natural disaster index presented above, but aggregated at the sub-continent-wide level this time. However, these natural disasters are bound to affect commodity prices, especially in the agricultural sector where they can affect both the demand and supply sides. Figure 4 represents the time series of a World Bank index for tropical beverages, where the impacts of climatic shocks can be seen by eyeballing the curve. The main peaks are associated with major El Niño or La Niña events. The 1972-73 and 1982-83 El Niño droughts triggered a sizable price hike while the widely forecasted and announced El Niño event of 1997-98 had a more moderate impact. The largest price hike was due to the 1976 frost of the Brazilian coffee crop, due to a La Niña

[14] Unlike Bates (2008b), Fearon (2005) and Humphreys (2005) we do not use commodity outputs beside their prices implicitly captured by the time dummies in our second-stage equations because (i) they are probably endogenous, and (ii) their impacts are probably well captured by GDP p.c. and country fixed effects. Modeling commodity supply is a cottage industry of its own and trying to do this here would take us too far afield.

cold episode, while a more modest cold episode occurred in 2011. Still, macroeconomic shocks in industrialized countries are also affecting these prices.

Figure 4: Beverage prices and El Niño Events



Data Source: World Bank. **Note:** Deflated by the Manufactures Unit Value Index of G15 exports to low- and middle-income countries in US $.

5.1. Unexpected Price Effects.

We experimented first with a number of individual commodity prices producing disappointing results. Although these exercises always yield statistically significant equations, the estimates are not robust in general, depending on the list of the included prices. This is due to the strong collinearity of commodity prices, while agricultural prices are also strongly correlated with the aggregate number of natural disasters. Table 6 illustrates the problem using a parsimonious specification regressing the coefficients of the time dummies from table 3 on different combinations of the log of three commodity prices (gold, crude oil and cotton) and the aggregate number of natural disasters[15]. The first set of five columns uses these time effects from the Civil Wars equation at column (2) and the second set uses the data when the Minors are included as in column (4). The first column of each set includes jointly the three

[15] Brückner and Ciccone (2010) single out the prices of oil and cotton in their fn 1 for explaining the onset of civil wars in Angola (1998) and Sudan (1983).

commodity prices and Natural Disasters as regressors. The next two columns [(12) – (13)] and [(17) – (18)] alternate by eliminating either the price of gold or that of crude oil, while the subsequent two columns [(14) – (15) and (19) – (20)] eliminate in turn the price of cotton or the aggregate number of natural disasters. The findings are typical of a multicollinearity problem as neither the prices of gold and oil nor the price of cotton and the aggregate number of natural disasters are ever jointly significant. However, the prices of gold and oil in the (17) – (18) pair come up with the opposite signs of those in (14) – (15). The table appended at the foot of table 6 presents the results of applying the Davidson-MacKinnon (1981) *J*-test for selecting among non-nested hypotheses to each pair of collinear variables. On purely statistical grounds, these tests lean in favor of choosing the price of gold and the price of cotton for the case of civil wars, while the J-tests fail to choose between the different equations when minor conflicts are included. However, there are no strong analytical reasons to make such choices and a broader perspective seems advisable. These findings suggest on the one hand that it is the general movement of extractive-commodity prices that matters for the incidence of African civil wars rather than the price of oil per se, which has often been found significant in previous empirical studies, as mentioned above; on the other hand, they suggest that the impacts of natural disasters are largely transmitted to agricultural prices, while the latter also reflect to some extent the state of the world market and the policies pursued by rich countries.

Table 6:

Time effects and commodity prices

	Time Effects from Civil Wars – Column (2)					Time Effects from Civil Wars & Minors – Column (4)				
	(11)	**(12)**	**(13)**	**(14)**	**(15)**	**(16)**	**(17)**	**(18)**	**(19)**	**(20)**
Log Gold Price	0.159***	0.156***		0.148***	0.171***	-0.037	-0.062***		-0.060	-0.042
	(0.04)	(0.02)		(0.04)	(0.05)	(0.04)	(0.02)		(0.04)	(0.04)
Log Crude Oil Price	-0.003		0.095***	-0.008	-0.018	-0.024		-0.047***	-0.002	-0.019
	(0.03)		(0.02)	(0.03)	(0.04)	(0.03)		(0.01)	(0.03)	(0.03)
Log Cotton Price	-0.208***	-0.209***	-0.219***	-0.234***		0.073**	0.070**	0.076**	0.021	
	(0.05)	(0.04)	(0.06)	(0.05)		(0.03)	(0.03)	(0.03)	(0.03)	
Nb. Natural disasters	0.001	0.001	-0.000		0.002***	0.002***	0.001***	0.002***		0.001**
	(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)	(0.00)		(0.00)
Cold War	-0.046	-0.047	-0.057	-0.059*	-0.099***	-0.066**	-0.070**	-0.063*	-0.091***	-0.47*
	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
War on Terror	-0.100***	-0.101***	-0.093**	-0.081***	-0.114**	0.069**	0.063**	0.068**	0.107***	0.074**
	(0.04)	(0.03)	(0.04)	(0.03)	(0.05)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Intercept	-0.498***	-0.488***	0.182**	-0.413**	-0.704***	0.162	0.241*	0.001	0.333**	0.234
	(0.17)	(0.14)	(0.09)	(0.16)	(0.20)	(0.16)	(0.12)	(0.05)	(0.16)	(0.18)
Nb. Obs.	42	42	42	42	42	42	42	42	42	42
R2	0.805	0.805	0.719	0.795	0.685	0.769	0.762	0.759	0.686	0.738
***F* stat**	26.680***	32.290***	27.318***	29.839***	21.377***	31.838***	29.926***	39.826***	21.912***	30.859***

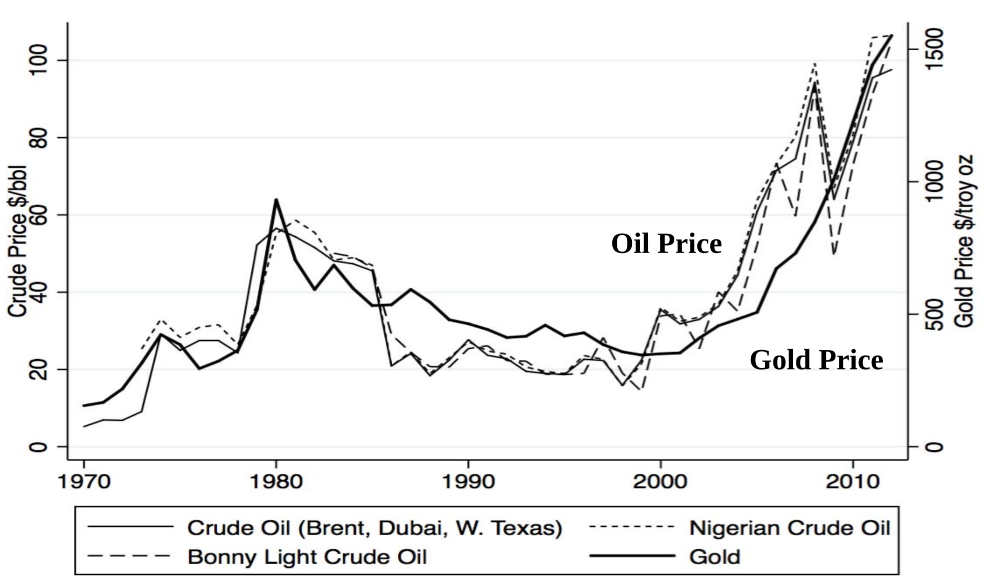
Note : OLS with robust standard errors (* p < 0.1 ,** p < 0.05,*** p < 0.01)

Davidson MacKinnon *J*-tests:

Test H0	*J*-test	Conclusion
H0: (12) vs. H1: (13)	-0.11	Equation (12) is preferred to (13)
H0: (13) vs. H1: (12)	4.52***	
H0: (14) vs. H1: (15)	1.39	Equation (14) is preferred to (15)
H0: (15) vs. H1: (14)	4.58***	
H0: (17) vs. H1: (18)	0.92	Neither equation (17) nor (18) is preferred
H0: (18) vs. H1: (17)	1.04	
H0: (19) vs. H1: (20)	3.44***	Neither equation (19) nor (20) is preferred
H0: (20) vs. H1: (19)	2.34**	

Figure 5 helps us to draw the implications of this finding. It depicts the time series of different kinds of crude oil prices (left scale), including an index of Nigerian oil and the price of Bonny light, the typical crude oil in the Gulf of Guinea, all deflated by the same MUV index. All these series have basically the same time profile with a long period of stagnant low prices during 1986-99 bracketed by two massive booms of roughly the same magnitude in percentage terms. The time profile of the price of gold (right scale) is very similar, although the two booms seem to have a shorter duration. Comparing these profiles to the civil war data depicted at figure 1 immediately shows that the gold/oil price swings raised a special challenge since the turn of the century. The first oil shock saw a massive and long-lasting increase in the number of civil wars, while the second one occurred when the civil war series had a quantum fall. Therefore, the findings of table 6 suggest that foreign aid's achievement at pacifying Sub-Saharan Africa was even more spectacular than acknowledged above, as it had to face the war-promoting impact of the second extractive commodity price boom. This finding is corroborated below using composite price indexes instead of individual prices.

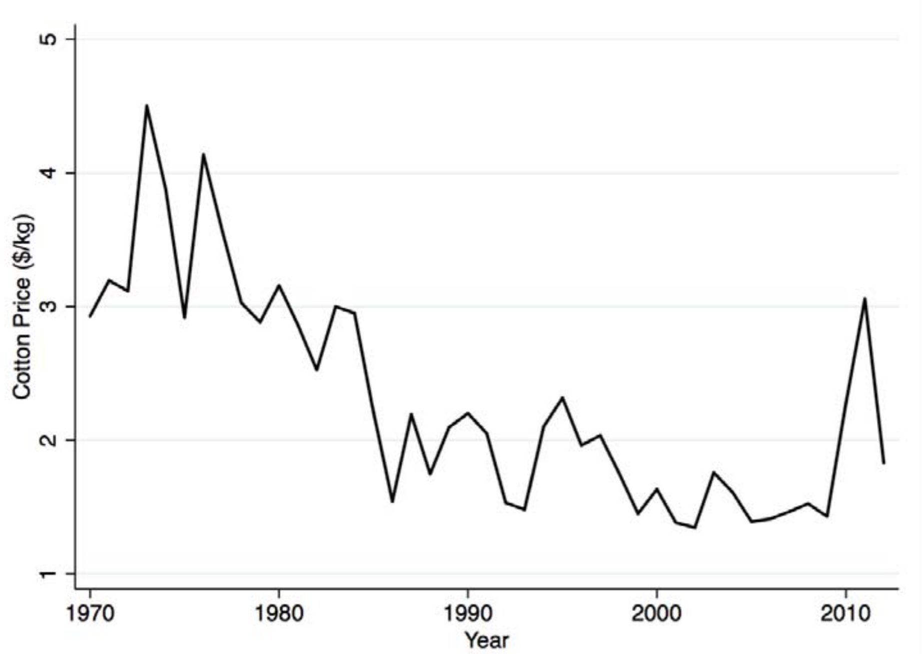
Figure 5: The real prices of oil and gold



Data Source: World Bank. **Note:** Deflated by the Manufactures Unit Value Index of G15 exports to low- and middle-income countries in US $.

The price of cotton is a very robust determinant of the incidence of civil wars in Sub-Saharan Africa and it seems to play a key part at the sub-continent-wide level. Cotton is widely grown in most countries of West Africa, in Chad and Sudan, and in several countries in Southern and East Africa. The estimates show that an increase in the price of cotton reduces significantly the probability of civil wars, with a different and not robust impact when minor conflicts are included. Figure 6 shows that the real price of cotton was particularly low from the mid-1980s to the late 2000s. The world market for cotton is well known for its distortions as the US government has traditionally been subsidizing its own producers over most of the sample period, with a sizable downward impact on the market price for poor countries' producers[16]. Among others, Bourdet (2004) provides a rich analysis of the market environment of some African cotton-exporting countries and its distortions. In addition, China has also paid its producers a subsidy aimed at compensating them partly for the US policy. This suggests that the US government has a powerful lever for enhancing the chances of peace in Africa by changing its price-subsidy policy for cotton. Still, climatic shocks also seem to affect the price of cotton as discussed about tropical beverage prices at figure 4.

Figure 6: Real price of cotton



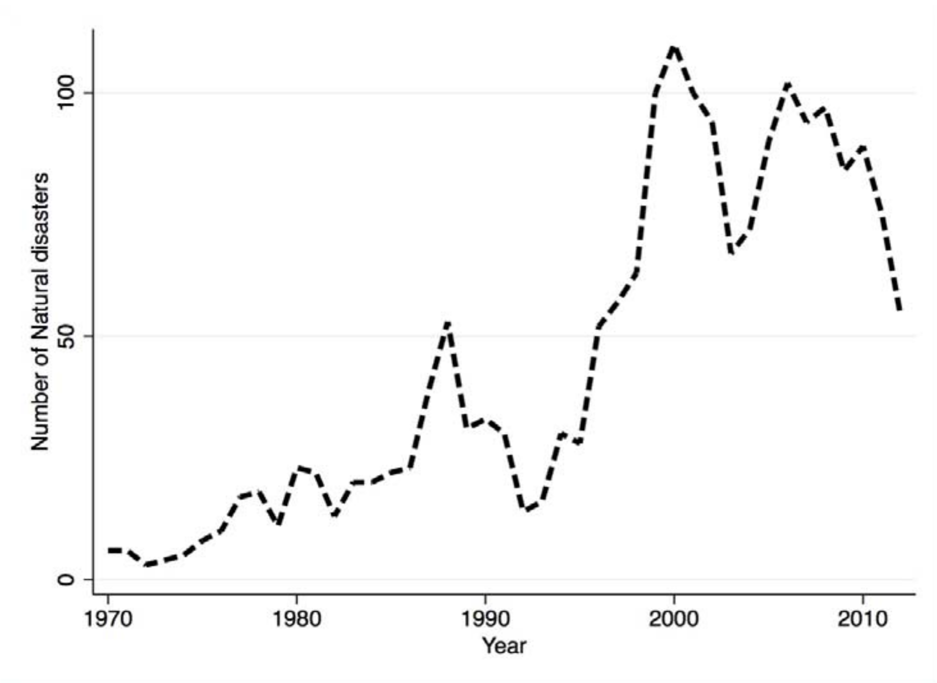
Data Source: World Bank. **Note:** Deflated by the Manufactures Unit Value Index of G15 exports to low- and middle-income countries in US $.

In addition to the commodity prices, table 6 brings out the war-promoting impact of Natural Disasters not for major conflicts, but when the Minors are included. However, table 6

[16] The WTO's Dispute Settlement Body ordered the U.S. government to eliminate its cotton production subsidies in 2005. The latter lost its appeal in 2009, but the battle goes on. In 2010, the U.S. government offered the Brazilian Cotton Institute $147.3 million a year as temporary bilateral agreement to give the U.S. some time to adjust its policy. Brazil is now threatening some forms of retaliatory measures against U.S. cotton subsidies. Meanwhile, nothing was offered to African producers (Langevin, 2014). Hence, the U.S. could simultaneously cut their aid to Africa and their domestic cotton subsidies, thus alleviating their fiscal deficit problem, without affecting the risk of war in SSA.

shows that the price of cotton and the total number of natural disasters are quite (negatively) correlated, as can be checked visually by comparing figures 6 and 7, so that Natural Disasters becomes significant even for Civil Wars if the price of cotton is excluded. This negative correlation does not seem to follow from a simple market mechanism, as the local consumption of cotton is small relative to exports, and might reflect instead the pricing behavior of the state-owned companies that market cotton in many African countries. It might also reflect migration-induced supply effects, as some population may respond to natural disasters by moving to cotton-growing areas. Changes in the levels of subsidies in the U.S. or China might also occur if some related natural disasters are affecting their farmers at the same time. Still, the price of cotton seems to be a more robust determinant than natural disasters at the continent-wide level as far as Civil Wars are concerned.

Figure 7: Aggregate number of natural disasters in Sub-Saharan Africa



Source: EM-DAT.

Although the coefficient of Natural Disasters is fairly small in every column of table 6, falling below 0.03 percentage points, it is far from negligible because of the number of disasters involved. Figure 7 shows the time profile of the number of such natural disasters in Sub-Saharan Africa that peaks at 110 in 2000. Therefore, the accumulation of natural disasters happening in a given year can increase sizably the risk of Civil Wars & Minors even with the small coefficient estimated at table 5, *ceteris paribus*. However, we know from table 4 that foreign aid does respond to natural disasters at the country level and from table 3 that this can prevent a sizable increase in the risk of civil war. Notice however by comparing figures 2 and 5 that the massive increase in the number of natural disasters starting in the second half of the 1990s took place in the context of a fall in foreign aid to Africa while the aid boom only started in 2001 with the launch of the war on terror. At the same time, the real price of cotton was falling to the all time low of the 2000s as shown by figure 6. This might explain why figure 1 displays a big spike in the civil war series in the last few years of the 20th century. This fairly mixed impact of natural disasters at the continent-wide level seems to concur with the fact that we could not find any significant country-specific impacts of domestic natural disasters on the risk of war at table A2.

The findings of table 6 control also for the three-period breakdown highlighted at figure 1. The cold war period does not display a robustly higher probability of civil war in the continent than the low-aid period of the 1990s, as long as the price of cotton is controlled for. Most of the time, the coefficient is negative and not significant. In contrast, it has a significantly pacifying impact when minor conflicts are included. We find a different result concerning the war on terror, which has a significant pacifying impact as far as civil wars are concerned, but has the opposite effect when minor conflicts are included.

5.2. Using Composite Price Indexes.

In order to circumvent the uncertainty mentioned above about the use of individual commodity prices, we also performed a similar exercise using composite indexes. The results are presented in table 7 for Civil Wars and Civil Wars & Minors and they seem to corroborate the findings of table 6 to a large extent, while they yield some additional insight.

The price indexes in this data set are presented with three different levels of aggregation. The second and third levels are derived by disaggregating the composite price indexes of the higher level into a small number of indexes, creating a kind of Russian doll hierarchy. In table 7, we use some indexes from these different levels of aggregation. We have selected these various price indexes after a gradual testing process. For example, at the same level as the Energy Price and Precious Metal Price used in table 7, there is an index of Non-Energy Price. The latter is decomposed at the next level of disaggregation into three sub-indexes, namely Fertilizers Prices, Metals & Mineral Prices and Agricultural Prices. We use these five composite price indexes in table 7 together with the aggregate number of natural disasters. Then, the findings are presented in two sets of three columns, for Civil Wars and Civil Wars & Minors, respectively. Collinearity problems also arise with these series and we handle them like in table 6 by performing Davidson-MacKinnon *J*-test in order to find the most robust specification. However, we restrict here the test to the Agricultural Price/Natural Disasters pair.

Most findings are very close to those found at table 6. For example, the Energy Price index is never significant in table 7, confirming the insignificance of the price of oil found at table 6. The price of precious metals has a significant positive impact in the first set of columns, and not in the second, confirming the result found at table 6 for the price of gold. We find at table 7 for Agricultural Price a result similar to that found for the price of cotton in table 6. The latter index is also quite collinear with the number of natural disasters. In this case, a market-mechanism interpretation of the negative correlation between these two variables is more appealing as many of the goods whose prices are included in the index have a sizable domestic market while natural disasters are bound to reduce domestic demand. Like at table 6, the *J*-test selects the specification with the agricultural price index against the one with Natural Disasters for Civil Wars, while it remains undecided rather than the other way around when Minors are included at table 7. In both cases Fertilizers Price is insignificant in the *J*-test-preferred equation while Metal & Mineral Price is not significant when Minors are included in table 7..

These findings roughly confirm that the risk of civil war increases when gold and precious metals prices rise while higher agricultural prices tend to reduce it. The latter suggests that food aid may have a detrimental impact on peace via its depressing impact on staple crop prices, beside the cotton-price subsidy problem discussed above. Hence, these findings provide some support to those of Nunn and Qian (2014) showing that US food aid tends to increase the incidence and duration of civil conflicts. The number of Natural Disasters at the continent-wide level has roughly the same impact in table 7 as in table 6, with positive coefficients of the same order of magnitude as in table 6, which are only significant in column (23) for Civil Wars when agricultural prices are excluded and in columns (24) and (26) when minor conflicts are included. The impacts of the war dummies differ slightly between the two tables as the Cold War is significant for all conflicts in table 7 while the war on terror looses its significance for civil wars at table 7.

Table 7: Time effects and composite price indexes

	Civil Wars – Column (2)			Civil Wars & Minors – Column (4)		
	(21)	**(22)**	**(23)**	**(24)**	**(25)**	**(26)**
Log Energy Price	0.010	0.014	0.038	-0.024	0.006	-0.038
	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)
Log Precious Metal Price	0.170***	0.165***	0.188***	-0.019	-0.054	-0.028
	(0.04)	(0.04)	(0.05)	(0.04)	(0.06)	(0.06)
Log Fertilizer Price	0.007	0.008	-0.136***	-0.055**	-0.043	0.017
	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.02)
Log Metals & Mineral Price	0.023	0.030	-0.074	-0.057	0.004	0.009
	(0.05)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)
Log Agricultural Price	-0.401***	-0.415***		0.200**	0.082	
	(0.10)	(0.06)		(0.08)	(0.06)	
Nb. Natural Disasters	0.000		0.001*	0.002***		0.001**
	(0.00)		(0.00)	(0.00)		(0.00)
Cold War	-0.056**	-0.060**	-0.095***	-0.061**	-0.094***	-0.041
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
War on Terror	-0.037	-0.033	-0.045	0.071**	0.106**	0.075**
	(0.03)	(0.02)	(0.04)	(0.03)	(0.04)	(0.03)
Intercept	1.310***	1.349***	0.311***	-0.383*	-0.056	-0.117
	(0.26)	(0.23)	(0.12)	(0.21)	(0.19)	(0.11)
Nb. Obs.	42	42	42	42	42	42
R2	0.873	0.872	0.775	0.785	0.684	0.733
***F* stat**	37.278***	44.448***	52.127***	27.266***	17.587***	23.818***

Note: OLS with robust standard errors (* p < 0.1 ,** p < 0.05,*** p < 0.01)

Davidson-MacKinnon *J*-Test

Test H0	*J*-test	Conclusion
H0: (22) vs. H1: (23)	0.43	Equation (22) is preferred to (23)
H0: (23) vs. H1: (22)	4.15***	
H0: (25) vs. H1: (26)	3.76***	Neither equation (25) nor (26) is preferred (25)
H0: (26) vs. H1: (25)	2.56**	

6. Conclusion.

The empirical analysis reported in this paper supports the view that the aid boom that started in the wake of 9/11 played a key part in abating civil wars in Sub-Saharan Africa. This occurred despite the increased tensions raised by two major exogenous price shocks that stacked the odds against peace at the continent-wide level since the turn of the century. The oil shock of the 2000s was in fact a broad-based extractive commodity boom that raised the risk of civil war in Africa. At nearly the same time, the price of cotton and the composite agricultural price index went through a trough that was also threatening peace. The econometric analysis leading to this conclusion involves three intermingled steps from which a "rectangular causality flow" can be derived. Three major exogenous variables, namely the occurrence of natural disasters, the launch of the war on terror and the evolutions of some commodity prices are imposing shocks on three interdependent endogenous variables, namely foreign aid, GDP p.c. and the risk of civil war. Donors aim at controlling violence in Sub-Saharan Africa to avoid ripple effects on their own economic or political interests, a pressing concern in the war on terror. To achieve this, they mainly offer foreign aid to African governments as a reward for avoiding the outburst of civil conflict within their sphere of influence. In addition to humanitarian motives, donors know that natural disasters are probably a major cause of violent conflict through their impacts on agricultural prices and their aid-allocation behavior reflects this connection. They step up aid-disbursement when such disasters occur. In addition, donors use some information that is not available to the econometrician. Our two-stage panel-data analysis captures this information synthetically, by using the residuals of a reduced-form equation explaining aid disbursements. These residuals affect simultaneously their aid allocation and the probability of war for each recipient country/year. This aid strategy has been highly effective since the turn of the century, as the incidence of civil war in Sub-Saharan Africa is nowadays on average less than half of what it was in the last quarter of the 20^{th} century. Unfortunately for African people, the aid boom only started with the launch of the war on terror, while the need for it arose already in the 1990s from a humanitarian point of view when the incidence of natural disasters began to rise significantly. Some commodity prices are creating additional shocks to this system. In particular, the unbundling of time effects performed above points out that high prices for cotton and for agricultural products are also pacifying factors on which rich countries exert a measure of control by subsidizing their own producers and through food aid. Moreover, the aid boom does not seem to have affected the risk of minor conflicts as the fall in the number of major conflicts has been compensated by a small rise in the number of minor conflicts, which are less lethal by definition. Hence, the aid boom has contributed to contain civil violence despite adverse commodity price developments and a sizable increase in the number of natural disasters in Sub-Saharan Africa.

This begs the question of the likely evolution of the aid flow to Sub-Saharan Africa, especially since Osama Bin Laden's death on May 2, 2011. This event is unlikely to put an end to the fight against terrorism because violent Jihadists are not about to disappear and should keep the West weary for many years to come. This prediction seems to be supported by the recent events in Syria and Iraq, where ISIS tried unsuccessfully to carve a new state for the Sunni Muslims under Islamic rule. Among other places, the current conflict in Libya is also vindicating this prediction. This renewed instability should keep the flow of foreign aid to Sub-Saharan Africa steady for some years, as Western powers will certainly strive to avoid leaving new stateless areas where Al-Qaeda, ISIS and its affiliates could flourish and recruit disgruntled fighters.

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Supplementary material for online publication

APPENDIX

Proof of proposition 1: The idea of the proof is to show that there exists a linear combination of (6) and (9) having a smaller sum of squared residuals (*SSR*) than that resulting from estimating either (6) or (9) alone if the sample is large enough. Define $\lambda \in [0,1]$. Then we can write in obvious notation a linear combination of (6) and (9) which has the same structure as (6)[17]:

$$\begin{aligned} w &= \lambda w_{(5)} + (1-\lambda) w_{(8)} = \\ &\lambda\alpha + (1-\lambda)\left(\frac{\pi}{\gamma} - \overline{\theta}\right) + \lambda\beta x - \lambda\gamma a + \varepsilon - \eta + \lambda(\eta + \delta e) \end{aligned} . \tag{A.1}$$

It follows from this identical structure that identification of (6) cannot come from restrictions on structural parameters and must be assessed by looking at the residual variances. Then, OLS will choose λ (and other parameters) such that:

$$\begin{aligned} \min_{\lambda} SSR &= \sum\nolimits_{i\in S}\left(\varepsilon - \eta + \lambda(\eta + \delta e)\right)^2 = \sum\nolimits_{i\in S}\left(\varepsilon^2 + \eta^2\right) \\ &+ \lambda^2 \sum\nolimits_{i\in S}\left(\eta^2 + \delta^2 e^2 + 2\delta\eta e\right) + 2\lambda\sum\nolimits_{i\in S}\left(\varepsilon\eta + (\varepsilon - \eta)\delta e - \eta^2\right) \end{aligned} . \tag{A.2}$$

Taking expectations, knowing that $E(\varepsilon\eta) = E(\varepsilon e) = E(\eta e) = 0$ yields:

$$E(SSR) = E(\varepsilon^2) + E(\eta^2) + \lambda^2\left(E(\eta^2) + \delta^2 E(e^2)\right) - 2\lambda E(\eta^2). \tag{A.3}$$

Minimizing (A.3) and rearranging the terms of the first-order condition yields the weight given to (6) by OLS as (10).

[17] The classic reference on this approach is Fisher (1965), while Desai (1976) clearly shows how to use it.

Proof of proposition 2: Notice first that ν will be an estimate of ε because the inclusion of $\hat{g}$ is controlling for $\delta e+\zeta$, so that $\sum_{i\in S}\nu^2$ will be an estimate of $\sum_{i\in S}\varepsilon^2$. Then, using the same approach as in the proof of proposition 1, as well as the identifying parameter restriction that w is excluded from (13), we can write the linear combination of (13) and (9) as:

$$\begin{aligned} w=\lambda\, w_{(12)}+(1-\lambda)\, w_{(8)} &= \left(\lambda\,\alpha+(1-\lambda)\left(\frac{\pi}{\gamma}-\bar{\theta}\right)\right) \\ &\quad +\lambda\,\beta\, x-\lambda\,\gamma\, a+\lambda\,\phi\,\hat{g}+\lambda\,\nu+(1-\lambda)(\varepsilon-\eta) \end{aligned} \qquad \text{(A.4)}$$

Its structure is identical to that of (13), so that we again need to look at the random disturbance terms to assess identification. The sum of squared residuals of (A.4) reads:

$$\begin{aligned} \sum_{i\in S}\left(\lambda\,\nu+(1-\lambda)(\varepsilon-\eta)\right)^2 &= \sum_{i\in S}\left(\lambda^2\,\nu^2+(1-\lambda)^2\left(\varepsilon^2+\eta^2-2\,\varepsilon\,\eta\right)\right) \\ &\quad +2\sum_{i\in S}\lambda\;\nu(1-\lambda)(\varepsilon-\eta) \end{aligned} \qquad \text{(A.5)}$$

Then, because $E\left(\nu^2\right)=E\left(\varepsilon^2\right)$ and $E(\varepsilon\,\eta)=E(\varepsilon-\eta)=E(\nu)=0$, the expected value of the sum to be minimized is equal to $\lambda^2\, E\left(\varepsilon^2\right)+(1-\lambda)^2\left(E\left(\varepsilon^2\right)+E\left(\eta^2\right)\right)$. The latter is minimal when (14) holds.

Table A1:

Summary statistics

	N	Nb. of Countries	Minimum	Maximum	Mean	Standard Deviation
Civil Wars	1761	46	0	1	0.057	0.232
Civil Wars & Minors	1761	46	0	1	0.169	0.375
log GDP p.c.	1761	46	3.91	9.61	6.454	1.017
log ODA p.c.	1761	46	-4.2	6.81	3.865	1.004
log Pop.	1761	46	10.89	18.94	15.373	1.501
Nb. Of Natural Disasters	1761	46	0	12	1.006	1.487

Table A2:

Natural disasters exclusion test

	Civil Wars		Civil Wars & Minors	
	(A2.1)	**(A2.2)**	**(A2.3)**	**(A2.4)**
Log GDP p.c.	-0.0847***	0.0863	-0.1024***	-0.2921***
	(0.01)	(0.07)	(0.02)	(0.10)
Log Pop.	-0.2368***	-0.2359***	-0.2941***	-0.3057***
	(0.06)	(0.07)	(0.09)	(0.09)
Log ODA p.c.	-0.0463***	-0.1400***	-0.0410***	0.0248
	(0.01)	(0.04)	(0.01)	(0.07)
Nb. Natural Disasters	-0.0088*	-0.0020	0.0012	-0.0046
	(0.01)	(0.01)	(0.01)	(0.01)
Res. Log GDP p.c.		-0.1789**		0.2008**
		(0.08)		(0.10)
Res. Log ODA p.c.		0.0954**		-0.0659
		(0.05)		(0.07)
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Nb. Of Obs.	1761	1761	1761	1761
Joint Res. Test	-	6.36**	-	3.94
F stat	2.19***	2.18***	2.29***	2.24***

Table A3:

Trend*French colonies exclusion test

	Civil Wars		Civil Wars & Minors	
	(A3.1)	**(A3.2)**	**(A3.3)**	**(A3.4)**
Log GDP p.c.	-0.0810***	0.0435	-0.1014***	-0.3888*
	(0.02)	(0.16)	(0.02)	(0.23)
Log Pop.	-0.2667***	-0.2263***	-0.2971***	-0.2841***
	(0.07)	(0.07)	(0.09)	(0.10)
Log ODA p.c.	-0.0464***	-0.2113	-0.0402***	-0.1362
	(0.01)	(0.18)	(0.01)	(0.25)
Trend * Fr. Colonies	0.0006	-0.0019	0.0003	-0.0043
	(0.00)	(0.01)	(0.00)	(0.01)
Res. Log GDP p.c.		-0.1361		0.2975
		(0.16)		(0.23)
Res. Log ODA p.c.		0.1666		0.0951
		(0.18)		(0.25)
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Nb. of Obs.	1761	1761	1761	1761
Joint Res. T.	-	9.19***	-	3.49
F stat	2.15***	2.18***	2.29***	2.24***

Table A4:

Trend*UK colonies exclusion test

	Civil Wars		Civil Wars & Minors	
	(A4.1)	**(A4.2)**	**(A4.3)**	**(A4.4)**
Log GDP p.c.	-0.0906***	0.1906	-0.0914***	-0.0563
	(0.02)	(0.25)	(0.02)	(0.37)
Log Pop.	-0.2386***	-0.2404***	-0.3183***	-0.3159***
	(0.07)	(0.07)	(0.09)	(0.09)
Log ODA p.c.	-0.0490***	-0.1394***	-0.0386***	0.0262
	(0.01)	(0.04)	(0.01)	(0.07)
Trend * UK	0.0014	-0.0014	-0.0021*	-0.0032
Colonies	(0.00)	(0.00)	(0.00)	(0.01)
Res. Log GDP p.c.		-0.2832		-0.0350
		(0.26)		(0.37)
Res. Log ODA p.c.		0.0947**		-0.0673
		(0.05)		(0.07)
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Nb. of Obs.	1761	1761	1761	1761
Joint Res.Test	-	7.16**	-	0.94
F stat	2.17***	2.18***	2.32***	2.24***

Table A5:

Conventional 2SLS for cross-border spillovers with tests of instruments validity

	Civil Wars ***(A6.1)***	***Civil Wars & Minors*** ***(A6.2)***
Log GDP p.c.	0.0953	-0.2705***
	(0.08)	(0.09)
Log Pop.	-0.2414***	-0.3167***
	(0.08)	(0.09)
Log ODA p.c.	-0.1485***	0.0061
	(0.04)	(0.06)
Log ODA p.c. Neighbors	0.0017	0.0021
	(0.01)	(0.01)
Country FE	Yes	Yes
Year FE	Yes	Yes
Nb. of Obs.	1761	1761
Under-Identification Test		
(Kleibergen-Paap rank LM statistics	45.46***	45.46***
Weak Idenitification Test		
(Kleibergen-Paap rank Wald F Stat)	15.26	15.26
Endogeneity test	8.43**	3.60
Sargan Test	0.12	0.33
(overidentification test of all instruments)		
Root MSE	0.2057	0.28
F Stat.	1.50**	1.98***

Note: See note to table A5.

Additional references

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