MANCHESTER 1824

The University of Manchester

Economics Discussion Paper

EDP-0633

Aid Effectiveness: The Role of the Local Elite

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November 2006

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First complete draft: October 13, 2006
This version: October 13, 2006

Abstract

We study the importance of the local elite as a determinant of the effectiveness of foreign aid in developing countries. An "extractive" elite will misuse aid flows, an issue that is probably as old as foreign aid itself. We proxy for the existence of an "extractive" elite by using an historically determined variable: the percentage of European settlers in colonial times. Our econometric results clearly show the importance of this factor and its robustness to a wide set of alternative aid-growth relationships advanced in the literature.

Keywords: Foreign aid; Elite; Economic growth
JEL Classification: C23; F35; F43; O11

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1 Introduction

The effectiveness of foreign aid in developing countries is a topic that has kept researchers in economics busy in the recent years. While the subject is by no means a new one[1], a recent revival took place in which the question has shifted from "Is aid beneficial?" to "Under what conditions can aid be expected to be beneficial?". In econometric terms this is traduced by the inclusion of interaction terms between aid and other factors that can affect the effectiveness of aid in a growth-regressions framework.

The most influential work along these lines is the one by Burnside and Dollar (2000). These authors' claim, that aid increases economic growth in countries with good fiscal, monetary and trade policies, sent shock waves throughout the concerned policy circles[2]. The result of Burnside and Dollar (2000) is provocative, but further research was quick to point out a lack of robustness in their regressions[3]. While the policies-aid effectiveness relationship advanced by these authors can be cast into doubt, it is clear that their work has inspired many others in an attempt to understand the conditions under which foreign aid is most beneficial.

Thanks to a vibrant research production, we can now relate the effectiveness of aid to several factors. First, a seemingly robust finding is that there are diminishing returns to aid. In econometric terms this is reflected in a negative coefficient on an aid squared term whenever it is included in growth regressions (Dalgaard and Hansen 2001, Hansen and Tarp 2001, Lensink and White 2001).

Aid also seems to matter more in the presence of large negative shocks. Guillaumont and Chauvet (2001) construct an index for environmental vulnerability, which includes the variability of agricultural production and the

[1] See Hansen and Tarp (2000) for a review of three decades of literature on the subject.
[2] See Easterly (2003) for an account and discussion.
[3] See, in particular, Easterly et al. (2004).

trend and variability of the terms of trade. They find that countries suffering from high environmental vulnerability grow slower but also that aid is more effective in them. A related result is obtained by Collier and Dehn (2001), who study periods characterized by a negative terms of trade shock (defined as those located at the bottom 2.5% of the distribution). Their results suggest that aid can be particularly important during these periods.

A different situation in which aid might be of great use is in the years following an armed conflict. Collier and Hoeffler (2002) study such episodes and advance that the growth effect of aid is strongest between 4 and 7 years after the end of a conflict, reverting then to its normal value. This suggest that the bulk of aid flows should not arrive immediately after the conflict, when the country does not have the capacity to allocate it properly, but a few years afterwards.

A final factor that appears to be strongly related to the effectiveness of aid is the climate. Dalgaard et al. (2004) include the fraction of land in the tropics in their growth regressions and show that their interaction with aid has a strong negative effect, while the effect of other factors such as macroeconomic policies tend to disappear with its inclusion. Roodman et al. (2004), after carefully testing several competing factors, find that the fraction of land in the tropics is one of the best predictors of aid (in)effectiveness.

We contribute to this empirical literature by concentrating on a factor which seems to be understudied with respect to its obvious importance: the role of the local elite. By elite we understand a relatively small part of the population with a disproportionate share of the country's political and economic power. Aid flows cannot be converted into the goods and services they have been allocated for without the intermediation of the local government and local firms, themselves under the control of this elite. Thus, the attitudes and incentives of the elite are crucial and will determine how much of the aid is diverted and how much reaches its final goal.

A downside of the empirical literature on aid effectiveness is that the mechanisms behind the statistical relationships are not necessarily well understood or explicitly spelled out. We believe that the role of the local elite stands out by having a simple and unambiguous working mechanism whose importance is rather uncontroversial. An "extractive" elite will simply use aid to finance its own consumption or its pet projects by taking advantage of the fungibility of aid flows[4]. Countries that have the bad luck of being governed by this type of elite will see their aid flows largely wasted.

Compare this straightforward link with the much-talked role of macroeconomic policies from Burnside and Dollar (2000). As far as we can tell, these authors have not claimed that low inflation, government surpluses or openness to trade *per se* will make aid more effective. Inflation might be problematic if it increases uncertainty and reduces the incentives to invest, but this does not mean that aid given for building schools will be wasted. Trade barriers can be harmful by not allowing a country to specialize in what it does best, but how is this related to the allocation of aid to the people who really need it? As it turns out, authors have not been claiming that it is the precise policies of inflation, fiscal stance or trade liberalization that make aid beneficial. Instead, the idea would be that a country that chooses the right macro policies will also be a good manager of foreign aid. Best practice in one area should be correlated with best practice in other areas.

But should it? A key difference between responsible macroeconomic policies and good management of foreign aid is the group of people that benefits from them. While macroeconomic policies will end up improving the lot of everyone in the economy, foreign aid is often directed towards the very poor and therefore good aid management will be disproportionately beneficial to the lowest part of the income distribution. While the interests of the govern-

[4]In this respect, Boone (1996) shows that aid tends to finance consumption instead of investment.

ment and the poor might be aligned when it comes to chose macroeconomic policies, the situation can be the exact opposite at the moment of using foreign aid. Thus, good policies and aid mismanagement might well coexist in a country. Furthermore, the determining factor would be the attitudes of the people in the government, once again the elite.

The role of the elite has been studied in several theoretical contributions. Boone (1996) and Adam and O'Connell (1999) use a similar theoretical framework to show that an "elitist" political regime will waste foreign aid, meaning that it will allocate aid to consumption instead of investment. Even dimmer outcomes are present in the models of Svensson (2000) and Economides et al. (2004), where the rent-seeking behavior of parts of the population increases when the amount of funds that can be appropriated is enlarged by aid funds. This shifts resources away from productive uses and can result in a net loss for the economy.

These theoretical treatments of the role of the elite do not have a proper counterpart in the empirical literature. The reason might lie in the difficulty to measure not whether an elite exists (since it is always the case that some groups have more political and economic power than others) but whether this elite will reflect the interests of all the society or only of its own members. Researchers have addressed the problem at least indirectly, by estimating the effect of democracy (Svensson 1999) or political instability (Chauvet and Guillaumont 2002) on aid effectiveness. But these variables are at best loosely correlated with the presence of an extractive elite. It is not difficult to come up with examples where, despite the regular holding of elections, political power remained in a handful of people. Similarly, a stable political environment might be the outcome of an unchallenged dominance of a small group.

A central contribution of our paper is to use the percentage of European

settlers to total population in colonial times as a proxy for the existence of an extractive elite. As is well-known, colonialism sent waves of Europeans throughout most of today's developing world in different amounts. Where Europeans settled in few numbers, they tended to leave once the country became independent. On the other hand, in the countries of high European settlement most economic resources and political power remained in the hands of European descendants after independence and in general until today. Historical accounts, like Engerman and Sokoloff (2002) for the Americas, reveal that this type of elite repeatedly put its own interests before those of the rest of the population and that this phenomenon is prevalent even today.

Thus, we hypothesize a negative relationship between the amount of European settlement and the effectiveness of aid. Remark that we are nowhere suggesting that European settlement is the most important, let alone the only, factor determining the existence of an "extractive" elite. We use European settlement because, in addition to being a reasonable proxy for this type of elite, it has the important characteristic of being historically determined and therefore much less prone to endogeneity problems than most other variables. We do not believe that countries of high European immigration are the only ones affected by this problem, we simply propose that using the percentage of European settlers as a regressor will help us identify the effect of an extractive elite on aid effectiveness.

We provide strong empirical support for our thesis by showing that the effect of aid on growth tends to be much diminished in countries of high European settlement. This result holds over a large battery of robustness checks where we consider different regression specifications, different measures of aid and challenge our results by including many of the control variables previously proposed in the literature.

Our approach links this paper not only with the aid effectiveness literature

but also with a growing set of papers studying the effects of colonialism on economic outcomes. This line of research has provided evidence on how the colonial past can explain today's differences in income levels (Acemoglu et al. 2001, 2002, Feyrer and Sacerdote 2006) or inequality (Angeles 2006).

The remainder of the paper is organized as follows. In section 2, we present in detail our empirical methodology and describe the data. Section 3 reports our results and their implications. In section 4, we conduct the robustness tests of the benchmark findings. The last section of the paper offers some concluding remarks.

2 Methodology and Data

2.1 Methodology

Our main interest is to examine the growth impact of aid conditional upon the behavior of the local elite. In other words, we investigate to what extent local elites, political and/or economic, by mis-directing aid funds to non-productive (and self-benefiting) uses diminish the growth-enhancing effect of foreign assistance. Although the rationale behind this idea is also present in Boone (1996), our methodological approach diverts in a significant way. By proxying the local elites as the percentage of European settlers to total population in colonial times, we can capture the adverse effect these elites impose on the growth-effectiveness of aid.

The majority of today's aid-recipient countries were colonized. The central hypothesis of this paper is that the Europeans who settled in the colonized countries evolved into a powerful local elite and that the influence of these elites is related to the extent of European settlement. Thus, in countries where the percentage of European settlers was well below 1% most of the land and productive resources remained in the hands of the autochthonous people. Europeans were simply not numerous enough to directly run

the economy and profited instead by means of taxes and commerce. On the other hand, in the countries where Europeans settlers accounted for a large minority of 20-30% of the population they took direct control of all land and mining resources, along with all political power.[5],[6]

We advance that this elite has played a primal role in the receipt and allocation of foreign financial assistance and has consistently put its own interests in front of those of the rest of the country. Aid effectiveness should then be negatively related to the importance of this elite, as proxied by our colonialism-related variables.

We test our central hypothesis by complying to the current trend in the related literature and utilize panel data techniques. Specifically, we employ the following model specification:

$$g_{it} = \alpha + \beta_1 Aid_{it} + \beta_2 Settlers_{it} + \beta_3 (Aid^* Settlers)_{it} + \sum_{j=1}^{m} \gamma_j X_{j,it} + \sum_{k=1}^{n} \delta_k D_{k,it} + u_{it}, \tag{1}$$

where g_{it} denotes per capita GDP growth rate in country i at time t, Aid represents a measure of aid receipts, and $Settlers$ is the percentage of European settlers in total population in colonial times for all aid recipient countries. For non-colonized countries $Settlers$ takes the value of 0. $X_{j,it}$ describes a list of control variables that are commonly found to explain a substantial variation in the data. These are the logarithm of initial income, an indicator of institutional quality from the International Country Risk Guide, the fraction of land in the tropics, indicators of fiscal (budget

[5] See Angeles (forthcoming) for a more extended discussion of this topic.

[6] Note that there exist four colonies were Europeans became the majority of the population: Australia, Canada, New Zealand and the US. In this case it would be wrong to talk about Europeans constituting an elite since an elite should be formed by a minority of the population. If these countries were included in our dataset we would need to set them in a separate group. But this is not the case since they are not aid-recipients.

balance), monetary (inflation), and trade (Sachs-Warner openness) policies, and a proxy for political instability. Finally, $D_{k,it}$ are the dummies controlling for regional differences (Sub-Saharan Africa and East Asia).

A negative value of the coefficient β_3 would indicate that the growth effects of aid are reduced in countries were European settlement was relatively large in colonial times. We will take this as evidence of the negative role of an extractive elite on aid effectiveness.

The panel estimations we use are based on techniques that address possible endogeneity of the right-hand-side variables. The first method is a standard two stage least squares estimation (2SLS). The instruments we use can be categorized into lags of potentially endogenous variables and into additional exogenous variables. Consistent with the literature, we consider one lag of the endogenous variables as instruments. The exogenous variables used to instrument for aid are drawn from Hansen and Tarp (2001), Burnside and Dollar (2000), and Clemens et al. (2004). These are a dummy for the Franc zone, a dummy for Central American countries, a dummy for Egypt, arms imports relative to total imports lagged one period, the logarithm of population, and M2 as a fraction of GDP lagged one period.

The second method is the system GMM estimation developed by Blundell and Bond (1998) and popularized in the aid-growth literature by Dalgaard et al. (2004) and Roodman (2004). This technique eliminates the impact of time invariant and slowly changing variables, such as regional dummies and institutions, while it accounts for possible endogeneity by a rich set of endogenous instruments. This estimator treats the model as a system of equations, in first-differences and in levels, where the endogenous variables in the first-difference equations are instrumented with lags of their levels and the endogenous variables in the level equations are instrumented with lags of their first differences. A difficulty associated with system GMM, however, has to do with the choice of the number of lags of the endogenous and predetermined

variables. For instance, Dalgaard et al. (2004) starting with twice lagged endogenous regressors use an unrestricted set of lags, while Reddy and Minoiu (2006) prefer five time periods of lags. In addition, Rajan and Subramanian (2005) have shown the fragility of some of the results by simply limiting the number of lags of the instrumented endogenous and predetermined variables from unrestricted to three.

To enhance the robustness of our results and to avoid this kind of criticism, we follow Roodman (2004) and Rajan and Subramanian (2005) in the way we set the lags to be used for instrumentation in our system GMM estimation. Originally, similar to Dalgaard et al. (2004) we use unrestricted lags starting with two, and thereafter we drop the size of the maximum lags to three. Finally, acknowledging Roodman's (2004) comment that the number of instruments we use could be too large that "they can overfit the instrumented variables, biasing the results towards those of the OLS", we restrict the number of instruments to be less than the number of countries in the regression. However, instead of reducing directly the lags of the endogenous instruments we collapse the instruments.[7]

Both the instrumental variable approaches we use to estimate the diminishing effect of aid on growth due to the presence of the local elite are tested for the validity of the used instruments with two specification tests. First, Hansen's (1982) J test of over-identifying restrictions is employed to examine the exogeneity of the instruments. It is worth mentioning that this test is consistent in the presence of heteroscedasticity and autocorrelation of any pattern.[8] Second, with regard to serial correlation, taking into account the bias it can cause to both the coefficients and the standard errors, we check

[7] As the Stata manual states, this means that instead of creating one instrument for each time period, variable, and lag distance, we create one instrument for each variable and lag distance.

[8] Failure of the null hypothesis suggests that the set of instruments is incomplete implying omitted variables bias.

all of our regressions for first and second order degree with the Arellano and Bond (1991) test. When first-order serial correlation is present, we use clustered standard errors by country making them robust to serial correlation. In addition, to avoid dynamic panel bias we instrument for regressors that are not strictly exogenous. These include initial income, institutional quality, openness, budget balance, and inflation. For the system GMM, however, since first-differencing induces first-order serial correlation in the transformed errors, the appropriate check regards only the absence of second-order serial correlation.

We also complement the above tests and checks of our methodological approach with a procedure that identifies multiple outliers (Hadi 1992). These outliers correspond to the partial scatter of growth with the multiplicative regressor $Aid^{*}Settlers$. This allows us to investigate whether our relationship of interest is affected by some observations that carry an excess weight. Note, however, that this technique is applicable only in the 2SLS framework since there is no comparable procedure in the GMM setting.

As discussed, the estimation techniques that we use account for the potential endogeneity of our aid measures and of a set of the control variables $X_{j,it}$. One variable that we do not instrument for, however, is $Settlers$. This requires that the number of European settlers in the colonies were not a function of the rate of economic growth today nor of any variable affecting growth today that is omitted in equation (1). Fortunately, we can be reasonably confident that this condition is met. The pattern of European settlement was certainly affected by factors that can be related to present-day growth, like climate (see Gallup et al. 1999) and institutions (Acemoglu et al. 2001); but equation (1) includes both climate and institutions as control variables. And surely one would not argue that European settlers were so foresighted as to make their decision to move into a colony in the 19th century or before based on this colony's rate of economic growth during the late 20th century.

2.2 Data Sources

We investigate our hypothesis by adopting the data set developed by Roodman (2004), which depends heavily on that of Easterly et al. (2004). It covers 171 aid recipient countries over the period 1958-2001. This set has been compiled in order to test the robustness of many of the results established in the aid-growth literature. Therefore, it is the most comprehensive data set not only in terms of country and period coverage, but also with respect to the breadth of regressors and instruments used. It contains three measures of aid receipts that allow us to test the robustness of our argument. These are effective development assistance (EDA)-to-real GDP, net overseas development assistance (ODA)-to-exchange rate GDP, and ODA-to-real GDP.[9] Unless we state otherwise the measure we use is EDA-to-real GDP. In addition, the data set brings together a large set of variables that have been found in the past to affect the impact of aid on economic growth (e.g., climate, economic policies, political instability, vulnerability of economic environment, etc). In this way, we have a natural benchmark of studies to compare our findings and claims against.

We expand the above mentioned data set with data on European settlement in colonial times drawn from Angeles (2006, Table 1) which, in turn, are mainly based on Etemad (2000) and McEvedy and Jones (1978). This data constitutes our prime proxy for the size and importance of an extractive elite. We also provide an alternative proxy for the elite: the descendants of the original European settlers as a percentage of total population in present times.[10] This allows us to control whether our results are robust to different

[9] For a detailed description of these measures, see Roodman (2004).

[10] The source for this measure is the CIA World Factbook 2006. Note that in the case of Latin American countries the exercice is complicated by the large extent of racial mixture. The only way through this difficulty is by compromising: we calculate the percentage of European descendants as the percentage of people of white race plus one half the percentage of people classified as "mestizo" (mixed white and amerindian races) or "mulatto" (mixed white and black).

ways of measuring the weight of the elite.

Data availability determines the set of countries we can include in our regressions. We cover up to 76 countries and the data are averaged over 11 four-year intervals (1958-61 to 1998-2001).

3 Benchmark Findings

We begin our analysis by estimating equation (1) with simple OLS where the set of control variables is as described above. At this stage we examine the non-linear impact of aid on growth conditional on the presence of the local elite without controlling for the possible endogeneity of aid. As we move to the right of Table 1 we progressively allow more regressors to be endogenous. The results in column (1) are not surprising as to the influence the variables included in sets X_j and D_k are found to exert on economic growth. Specifically, apart from the evidence of conditional convergence, location in Sub-Saharan Africa, a higher fraction of land in the tropics, higher inflation, and greater political instability are associated with slower growth. Being situated in East Asia and having a higher institutional quality indicator, on the other hand, are conducive to faster economic growth. We also find that although running a budget surplus and being more open to international transactions are beneficial to growth, are not so to a statistically significant degree. Turning our attention to the variables of interest, we find strong evidence in support of our main thesis. Aid is found to have a positive impact on growth. However, this impact is diminished by the presence of the local elite as indicated by the negative and strongly significant estimate of the aid interaction term.

In columns (2) to (4) we apply 2SLS and instrument for the potentially endogenous regressors. These are limited to Aid and $Aid^{*}Settlers$ in column (2) and expanded to initial income, institutional quality, openness, budget

balance, and inflation in column (3). Controlling for endogeneity improves the fit of the regression without dramatically altering the results. The only difference is that it renders aid insignificant, although still with a positive coefficient. All the other coefficients are similar in magnitude and significance across the two regressions. This holds in particular for the multiplicative term that describes the conditional effect of aid on growth. The specification tests acknowledge the validity of the instruments since Hansen's (1982) J statistic cannot reject their exogeneity at the 5% level. In addition, the Arellano-Bond (1991) test, although rejecting the hypothesis of no first-order serial correlation in the error term just for regression (2), fails to reject the hypothesis of no second-order autocorrelation in both regressions at the 5% level.

Column (4) shows that our findings are not driven by outliers. The Hadi (1992) procedure classifies 10 observations as outliers (listed at the bottom of Table 1). Removing these observations strengthens the fit of the model, improves substantially the explanatory power of the instruments, and leaves the results intact.

In columns (5) to (7) we adopt an alternative instrumental estimation approach, which has been deemed to be superior to 2SLS, the system GMM. As in Daalgard et al. (2004), we remove by first differencing the impact of time invariant factors (dummies for Sub-Saharan Africa and East Asia, fraction of land in the tropics, and settlers) and variables that change very slowly (institutional quality and political instability). In this way, all included regressors are considered to be potentially endogenous and are controlled for with their lagged levels as instruments.

Regression (5) uses all possible lags of the endogenous variables as instruments starting from the second lag, while regression (6) limits the maximum number of lags to three. Finally, regression (7) uses the same set of lags as

regression (5) but collapses the set, as in Roodman (2004). Using this estimation procedure continues to support our underlying conclusions. That is, the results highlight the negative and significant coefficients of $Aid^{*}Settlers$ regardless of the number of instruments used. In addition, these results are found to be consistent while the effect of aid, along with openness and budget surplus seem to be now significantly positive. The final point to note from our benchmark findings in Table 1, is that the coefficient in our variable of interest is fairly stable along the different regressions, even though we use a variety of estimation techniques and different sets and number of instruments. The task of the next section is to treat the robustness of our findings in a more detailed manner.

4 Robustness of the Benchmark Findings

This section examines the sensitivity of our baseline results by conducting the following three exercises. First, we consider alternative estimation specifications regarding the incorporation of the local elite and also use a different measure of its size to examine the validity of our findings. Second, we carry out regressions with alternative measures of aid. Finally, we explore the robustness and strength of our results *vis-á-vis* a wide number of prominent aid-growth studies by jointly considering the aid interaction effects they propose along with ours. Our basic finding survives all these tests and clearly indicates the importance of the local elite as a determinant of the effectiveness of foreign aid in the growth prospects of developing countries.

4.1 Alternative Specification and Measures of Local Elite

As explained before, our regressions include both colonized and non-colonized countries; non-colonized countries simply take the value of 0 for $Settlers$. This specification might not be the best since countries that were colonized

and received few settlers (variable $Settlers$ close to 0) and non-colonized countries ($Settlers$=0) are assumed to be almost the same ceteris paribus. One might expect instead that colonialism would have an impact even in the cases of very limited settlement. The criticism can be extended to suggest that the extent of European settlement might not matter and that our variable $Settlers$ is just picking up the effect of being colonized.

We take this issue seriously and propose two ways to deal with it. First, we modify our original specification to treat colonized and non-colonized countries separately. The first regression we run is described by:

$$g_{it} = \alpha + \beta_1 Aid_{it} + \beta_2 Settlers_{it} + \beta_3 (Aid^* Settlers)_{it} + \beta_4 NonSet_{it} \quad (2)$$
$$+\beta_5 (Aid^* NonSet)_{it} + \sum_{j=1}^{m} \gamma_j X_{j,it} + \sum_{k=1}^{n} \delta_k D_{k,it} + u_{it},$$

where $NonSet$ is a dummy variable taking the value of 1 for those countries in the set that have not been colonized, and all other variables are as defined in equation (1). Thus, if aid efficiency really differs in the countries that were never colonies this should show up in the coefficient β_5.

The second method we use is simply to keep the specification in (1) but to restrict the set of countries to those that have had a colonization experience in the past.

Table 2 presents the estimates of these specifications.[11] They appear in columns (2) and (3) respectively, while column (1) reproduces the second column of Table 1 with the original measure of settlers to ease comparison. The results continue to support our thesis: the coefficient of $Aid^* Elite$ is negative and significant in both regressions (though only at the 10% level in column 2). The coefficient of $Aid^* NonSet$ in regression (2) appears to be not

[11] Note that in this table we have a variable called *Elite* in the place of *Settlers*. This is because in this table we use different proxys for the elite. Thus, on columns 1 to 3 *Elite=Settlers*, while on columns 4 and 5 *Elite=Descendants* (see main text).

statistically significant. This result can be interpreted as bringing support to our thesis: colonized countries with low European settlement do not differ significantly from non colonized ones. Thus, it is the presence of settlers that makes colonialism problematic for aid efficiency because it creates a powerful local elite.

Another criticism that one might have towards our approach is that the size of the Europeans in total population might have changed considerably between colonial times and the present. The correct variable to look at would then be the percentage of the population of European descendants, measured today. We investigate this by considering another proxy for the local elite: the percentage of the population that is of European descendent today (*Descendants*). While we welcome this as an opportunity to test the robustness of our hypothesis to a different proxy for the elite it must also be noted that there are also reasons for not preferring this measure to the one we have used until now. While it is true that the percentage of descendants refers to present times one must also be aware that many of the European descendants might be regarded more as a middle class than as an elite. This is particularly true in Latin America, where a large proportion of the population is actually of mixed race (European and Amerindian or European and Black) and where in some cases the European descendants became the vast majority of the country (e.g., Argentina, Chile and Uruguay).

With these complications in mind, we offer two alternative specifications with the *Descendants* variable. In the first we simply use *Descendants* instead of *Settlers* in equation (1). The second takes into account that in some countries the descendants cannot constitute an elite because they have become the majority of the population:

$$g_{it} = \alpha + \beta_1 Aid_{it} + \beta_2 Descendants_{it} + \beta_3 (Aid^* Descendants)_{it} + \beta_4 HighDesc_{it} \quad (3)$$

$$+\beta_5(Aid^* HighDesc)_{it} + \sum_{j=1}^{m}\gamma_j X_{j,it} + \sum_{k=1}^{n}\delta_k D_{k,it} + u_{it},$$

where $HighDesc$ is a dummy that takes the value of 1 when European descendants constitute the largest proportion of the population.[12]

The results of these regressions are given in columns (4) and (5) of Table 2 respectively. The use of the alternative proxy for local elite does not change our conclusions in any meaningful way. The aid-elite interaction coefficient remains negative and significant in both regressions and its value and significance increases when we account for the countries with high fractions of descendants.[13] This last finding is in accordance with our prior that European descendants are also part of the middle class in the countries with $HighDesc$=1. When we do not treat these countries separately the coefficient β_3 is smaller because these countries have a higher aid-effectiveness than what one would expect given their level of descendants.

To summarize, our alternative regression equations and additional measures of the local elite do not seem to alter the main finding of our analysis. Moreover, the change in the magnitude of the aid-elite interaction term is intuitively appealing and accords well with our expectations. Finally, note that the results are preserved when we also control for outliers using the Hadi (1992) procedure (not shown, but available upon request).

4.2 Alternative Measures of Aid

In all preceding analysis we have used EDA-to-real GDP (as in Burnside and Dollar 2000, Collier and Dehn 2001, Dalgaard et al. 2004) as our preferred measure of aid flows. The literature, however, includes two more measures:

[12] These countries are Argentina (97%), Brazil (72.95%), Chile (89.5%), Puerto Rico (80.5%), and Uruguay (92%).

[13] This illustrates that *Descendants* is not an inappropriate alternative proxy for elite size, given that the correlation between *Settlers* and *Descendants* is 0.83.

net ODA-to-real GDP (Clemens et al. 2004 and Rajan and Subramanian 2005) and net ODA-to-GDP converted to dollars using market exchange rates (Hansen and Tarp 2001 and Guillaumont and Chauvet 2001). Table 3 depicts simple correlations between these three measures of aid and shows that they are highly correlated. Therefore, switching between aid measures is not expected to have a substantial impact on the benchmark results.

This expectation is echoed in the results shown in Table 4. There appear three regressions per alternative measure of aid, and as Table 1, increasingly more variables are controlled for endogeneity as we move to the right. All six regressions describe a significant and fairly stable coefficient for the aid-settlers interaction variable, although the degree of significance drops at the 10% level for the two system GMM estimations. One important difference, however, between these regressions, that use ODA, and the ones in Table 1, that use EDA, is that now in 5 of the 6 regressions the direct effect of aid on growth is found to be positive and significant. The way aid is measured, could therefore be a significant characteristic in assessing the potential growth benefits of foreign aid. This, however, does not influence the idea we advance in this paper.[14] It is also worthwhile to mention that the findings of Table 4 remain unchanged even when we use the alternative estimation specifications and measures of elite as advanced in the previous section.

4.3 Comparison with the Recent Empirical Literature

In the voluminous literature that tackles the relationship between aid and growth, one can distinguish two types of studies that support aid effectiveness. Those that claim that aid has on average a positive but diminishing growth impact independent of any country characteristics, and those that

[14] An additional result that emerges from the specification tests, described in the methodological section, is that as we instrument for more endogenous variables the Arellano-Bond (1991) test fails to reject the hypothesis of no first-order serial correlation in the error term even at the 10% level (columns (2) and (5)).

suggest that the effectiveness of aid hinges upon such characteristics. The first strand of studies includes, among others, Hadjimichael et al. (1995), Durbarry et al. (1998), Hansen and Tarp (2000, 2001), and Clemens et al. (2004), while the "conditional" strand became richer in recent years by providing a wide range of intuitively palatable characteristics. These have been represented by macro indicators, such as inflation, budget balance, and openness (Burnside and Dollar 2000); export price shocks (Collier and Dehn 2001); political instability (Chauvet and Guillaumont 2002); warfare and policy (Collier and Hoeffler 2002); democracy (Svensson 1999, Kosack 2002); and climatic circumstances (Dalgaard et al. 2004). This section examines the validity of our findings by including the aid-elite interaction term in a wide set of regressions that represent these alternative aid-growth relationships that have been advanced in the literature.

Table 5a demonstrates this process by focusing on the policy environment suggested by Burnside and Dollar (2000), BD from now on, and used in an extended data set by Easterly, Levine, and Roodman (2004), ELR. Column (1) reproduces the BD main result, where aid by itself is not significant (even has a negative coefficient) but the aid-policy interaction term is positive and significant.[15] In column (2), we add our aid-elite interaction term (along with elite) to the BD specification and as in our benchmark results we find it to be negative and significant. The aid-policy term is also found to be greater in magnitude and significance compared to the original BD regression. We then include in the dataset the five observations that have been deemed as outliers in BD, and we observe in column (3) that although the aid-policy coefficient is reduced by ten-fold and becomes insignificant, the aid-elite coefficient remains large and significant.

[15] Note that this regression corresponds to Table 4, column (5) of BD, where five outliers have been excluded from the sample (Gambia 1986-89, 1990-93; Guyana 1990-93; and Nicaragua 1986-89, 1990-93). As in ELR, if we include these observations the significance of the aid-policy term breaks down.

Columns (4) to (6) follow the same pattern as (1) to (3), but now the data set is that of ELR, who have updated and extended the BD set. We use the full sample over 1970-1997 and find, in accordance with ELR, that in all regressions both the aid and the aid-policy coefficients enter insignificantly. Therefore, once more the BD finding is proven to be fragile to the use of additional data. Our result, however, is robust and the estimated coefficient of $Aid^{*}Settlers$ is pretty stable and becomes significant at the 5% level when the data set includes the outliers.

Table 5b presents more regressions of the comparison of our main finding with the recent literature. From this point forward, however, we do not use the model specification that each study utilizes but we return to our preferred growth model. Columns (1) to (3) turn to the results of Dalgaard et al. (2004), DHT, where climatic differences play the central role in the effectiveness of aid. Based on Roodman's (2004) data set, we have managed to obtain DHT's main finding even with a different control set. This appears in column (1), where aid is found to have a negative impact on growth in the tropics.[16] Columns (2) and (3) amend the regression equations with the elite and the aid-elite terms, and regression (3) also extends the sample to all the available observations from Roodman's set (2004). We observe that the aid-elite term is consistently negatively significant at the 5% level, corroborating our thesis. Moreover, the direct effect of aid on growth now becomes greater and significant, and the aid-tropics term turns to insignificant and much smaller in size. These results may imply that our measure of the local elite could represent a better proxy for deep structural characteristics, such as institutions, compared to the climatic circumstances.

Regressions (4) and (5) incorporate the aid-elite term in the Collier and

[16] By using their instruments, these results are very close to Table 3, column (2) of DHT, where they also control only for the endogeneity of aid and the aid interaction term. We obtain similar results if we alternatively use as instruments the ones that appear at the bottom of column (2), Table 5b.

Dehn (2001), CD, specification. CD examine the provision of aid as a function of the export shocks that hit aid recipient countries. They conclude that well-timed aid increases have a beneficial effect on the impact of negative export shocks on growth. Our findings support their results since in all our related regressions the $\Delta Aid^{*}Negative\ Shock$ coefficient proves to be positive and significant. As far as it concerns our interaction term of interest, the $Aid^{*}Settlers$ coefficient continues to be negative and significant at the 5% level.[17]

This is also the case in regression (6), where we utilize the Chauvet and Guillaumont (2002), CG, specification, who, among other things, examine the extent to which aid effectiveness is influenced by political instability. Notice in particular the high value of the estimated $Aid^{*}Settlers$ coefficient. However, in contrast to their results, we find that aid given to politically unstable countries is effective since political instability could be considered as a form of economic vulnerability, so that aid contributes in ameliorating its effects. In addition, again in contrast to CG, we find the political instability variable to be significant and to increase substantially in size with the addition of the aid-political instability interaction term.[18]

The final table that contrasts our main argument with findings of the existing literature is Table 5c. In regression (1) we include aid squared in accordance to Hansen and Tarp (2000, 2001), HT, to see if this will change our main finding. The aid-elite term enters significantly with the correct sign, while there is no evidence of diminishing returns of aid. The following three regressions are in the spirit of Collier and Hoeffler (2002), CH, who propose that aid effectiveness is influenced by the timing of its distribution

[17] Regressions (4) and (5) yield similar results if we use instead the pooled distribution of forecasting errors for the commodity export price index. For more details see Roodman (2004).

[18] Note that these discrepancies in the results could be due to the different measurement of political instability and of the size of our sample, which is at least twice as large as their sample. See CG's Table 2 for more details.

as expressed by the number of years following the end of a civil war. By using three dummies that capture the time that elapsed from civil war, they suggest that aid is beneficial during the first post-conflict *decade* but not immediately after (first three years) the end of the conflict.[19] Our results in columns (2) to (4) confirm their conclusions as shown by the positive and significant coefficients of aid interacted with the post-conflict 1 and post-conflict 2 dummies and also by the statistically insignificant coefficient of the aid-peace onset term. In addition, notice the constancy and significance at the 5% level of the $Aid^{*}Settlers$ coefficient.[20]

The last two columns of Table 5c illustrate the validity of our findings in comparison to Svensson (1999). He uses the degree of political and civil liberties in recipient countries to show that aid is effective in more democratic countries. Our results are not supportive of his findings, however, since both indicators of democracy yield insignificant coefficients both for democracy itself and for democracy interacted with aid. But in both cases our aid-elite multiplicative term has the correct sign and is highly significant. A potential explanation is that our measure of elite encapsulates political power and the dominance of a small group of people in a more adequate manner than the institutionalized check the two democracy indicators are supposed to impose on governmental power. Therefore, the inclusion of the aid-elite term in the regressions deems the aid-democracy coefficients insignificant.

In sum, our basic finding that the effectiveness of foreign aid is much diminished by the misuse of funds by an "extractive" local elite, has not been invalidated even when we use a series of estimation specifications that take into account a large number of explanations generated in the existing literature. This means that our main argument either provides a complementary

[19] The three dummies are: peace onset, that assumes a value of 1 if a conflict ended in that period (which also includes the immediate post-conflict years), and post-conflict 1 (2), that takes a value of 1 one (two) year(s) after civil war has ended.

[20] We follow Roodman (2004) and include in our regressions only the $Aid^{*}Post\text{-}Conflict$ dummy and not the triple interaction term $Aid^{*}Post\text{-}Conflict^{*}Policy$.

explanation to some of the already existing ones (Collier and Dehn 2001; Collier and Hoeffler 2002), or it constitutes a better representation of some of the variables used to capture specific characteristics of recipient countries (Burnside and Dollar 2000; Dalgaard et al. 2004; Svensson 1999). Finally, note that the results presented in Table 5 (b, c) do not limit themselves in the use of EDA/real GDP as a measure of aid. Quantitatively similar results were also obtained with the two additional measures: ODA/real GDP and ODA/exchange rate GDP (available upon request).

5 Concluding Remarks and Discussion

Our aim in this paper has been to contribute to the large literature on aid effectiveness by analyzing the role of a key factor that one would readily admit as important but might deem too difficult to measure: the local elite. The existence of an elite is not to be regarded as a negative thing by itself. It is the particular circumstances and motivations that can make this part of the population to behave in a manner that is beneficial only for the minority. Thus the term "extractive" elite.

A strength of the paper is to identify a set of circumstances in which the elite of the country turned out to be of the extractive type and, more to the point of our discussion, would tend to divert flows of foreign aid for its own benefit. These circumstances are: having a colonial past and receiving a relatively large amount of European settlers. Of course this should not be understood as implying that only Europeans form extractive elites in developing countries. It just happens that, colonialism being such a widespread phenomenon and the technological and military advantage of Europe over other regions being so large at the time, this particular pattern can be found in several cases.

What our empirical analysis shows is that our proxy for the presence

and strength of an extractive elite, is a very robust explanatory factor of aid effectiveness. As we have explained, this continues to hold while we change specifications, the measure of aid, the proxy for the elite and the list of control variables and interaction terms with aid. In particular, our thesis holds when we control for many of the other factors that have been associated with aid effectiveness in the literature.

The empirical results give confidence to our claim in this paper: that the elite's attitudes are of first importance for the benefits that can be obtained (or forgone) from foreign aid.

Recent views on foreign aid in the relevant policy circles stress the importance of targeting aid. That is, if we identify the factors that predict high aid effectiveness then we should concentrate aid flows in the countries where these factors are present. If we are to interpret our results in these terms, the outcome would hardly be encouraging. Indeed, the variable we use to identify extractive elites is historically determined; countries cannot get rid of it. Using this variable to enlarge an imaginary "check list" that countries must go through in order to receive aid would set aside many countries forever.

But this is not the method we would recommend for targeting aid. Check lists can be damaging if the evidence behind them is shaky or if the countries can manipulate their numbers in their favor. Instead, we would just stress that the importance of the local elite should not be overlooked. There is no easy way to evaluate this point in practice, but nobody said that proper aid allocation would be easy. A colonial past with European settlement puts a country in a "risk group", but societies change and can outgrow this type of problems. There is just no substitute for a careful examination of a country's circumstances when deciding on aid allocation.

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Table 1
Benchmark Findings

	(1) OLS	(2) 2SLS	(3) 2SLS	(4) 2SLS	(5) GMM-SYS	(6) GMM-SYS	(7) GMM-SYS
Initial GDP per capita (log)	-0.663	-0.777	**-1.24**	**-1.67**	**0.564**	**0.976**	**2.18**
	(0.090)	(0.085)	**(0.007)**	**(0.000)**	**(0.189)**	**(0.058)**	**(0.059)**
Sub-Saharan Africa	-2.12	-1.98	-2.30	-2.30			
	(0.002)	(0.006)	(0.002)	(0.002)			
East Asia	2.51	2.44	2.73	2.76			
	(0.001)	(0.000)	(0.000)	(0.000)			
Institutional quality	0.267	0.366	**0.394**	**0.492**			
	(0.010)	(0.001)	**(0.004)**	**(0.001)**			
Tropical area	-0.884	-1.16	-1.10	-1.33			
	(0.080)	(0.027)	(0.013)	(0.011)			
Openness (Sachs-Warner)	0.035	0.042	**0.277**	**0.103**	**2.31**	**2.25**	**3.28**
	(0.908)	(0.886)	**(0.463)**	**(0.797)**	**(0.001)**	**(0.002)**	**(0.000)**
Budget balance	10.05	9.30	**4.06**	**5.36**	**15.83**	**24.92**	**26.02**
	(0.105)	(0.123)	**(0.617)**	**(0.514)**	**(0.012)**	**(0.000)**	**(0.060)**
Inflation	-2.40	-2.28	**-1.91**	**-1.96**	**-1.86**	**-1.07**	**-0.478**
	(0.000)	(0.000)	**(0.019)**	**(0.039)**	**(0.000)**	**(0.077)**	**(0.548)**
Political instability	-1.91	-1.81	-1.81	-1.47			
	(0.005)	(0.003)	(0.004)	(0.025)			
Settlers	0.021	0.017	0.033	0.041			
	(0.322)	(0.440)	(0.191)	(0.123)			
EDA	0.295	**0.172**	**0.180**	**-0.079**	**0.150**	**0.333**	**0.848**
	(0.028)	**(0.177)**	**(0.168)**	**(0.714)**	**(0.190)**	**(0.011)**	**(0.011)**
EDA * Settlers	-0.027	**-0.021**	**-0.029**	**-0.033**	**-0.013**	**-0.014**	**-0.026**
	(0.003)	**(0.026)**	**(0.002)**	**(0.094)**	**(0.061)**	**(0.048)**	**(0.081)**
Countries / Obs	68 / 487	67 / 449	66 / 414	66 / 405	76 / 530	76 / 530	76 / 530
Number of Instruments	-				323	147	60
R-square	0.318	0.438	0.434	0.475			
Hansen J-test (p-value)	-	0.305	0.495	0.723	1.000	1.000	0.150
AR(1) test (p-value)	0.021	0.007	0.180	0.068	0.000	0.000	0.000
AR(2) test p-value	0.386	0.589	0.505	0.367	0.306	0.278	0.274
Additional exogenous variables used as instruments	-	Frz, centam, egypt, arms1, lpop, m21	Frz, centam, egypt, arms1, lpop, m21	Frz, centam, egypt, arms1, lpop, m21	-	-	-
No. of lags of endogenous variables used as instruments	-	One	One	One	Unrestricted starting with two time lags	Two and three time lags	Unrestricted starting with two time lags and collapse the set

Notes: p-values in parentheses based on robust and clustered standard errors. Constant term not reported. Instrumented variables are in bold type. In regression (4) multiple outliers to the partial scatter of growth with EDA*settlers are removed using the Hadi (1992) procedure. The 10 outliers are GNB 1986-89; BOL 1986-89, 1990-93 & 1994-97; JOR 1974-77 & 1978-81; GAB 1974-77; NIC 1990-93, 1994-97, 1998-01.

Table 2
Alternative Measures of Local Elite

	(1) Settlers	(2) Settlers & Non-colonized	(3) Positive Values of Settlers	(4) Descendants	(5) Descendants & High-descendants
Initial GDP per capita (log)	-0.777	-0.628	-0.654	-0.784	-0.870
	(0.085)	(0.240)	(0.262)	(0.093)	(0.118)
Sub-Saharan Africa	-1.98	-2.41	-2.75	-1.91	-1.83
	(0.006)	(0.002)	(0.000)	(0.010)	(0.025)
East Asia	2.44	2.39	1.24	2.50	2.67
	(0.000)	(0.000)	(0.115)	(0.000)	(0.000)
Institutional quality	0.366	0.304	0.392	0.327	0.370
	(0.001)	(0.033)	(0.002)	(0.003)	(0.006)
Tropical area	-1.16	-1.17	-0.520	-1.07	-1.26
	(0.027)	(0.026)	(0.365)	(0.037)	(0.063)
Openness (Sachs-Warner)	0.042	0.149	0.235	0.204	0.198
	(0.886)	(0.638)	(0.517)	(0.503)	(0.523)
Budget balance	9.30	9.01	7.46	7.65	6.76
	(0.123)	(0.136)	(0.232)	(0.216)	(0.271)
Inflation	-2.28	-2.31	-2.06	-2.29	-2.31
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Political instability	-1.81	-1.98	-1.70	-1.86	-1.68
	(0.003)	(0.002)	(0.001)	(0.001)	(0.017)
Elite	0.017	0.023	0.010	0.011	0.020
	(0.440)	(0.382)	(0.696)	(0.124)	(0.152)
EDA	**0.172**	**0.559**	**0.373**	**0.133**	**0.168**
	(0.177)	**(0.225)**	**(0.241)**	**(0.389)**	**(0.301)**
EDA * (Elite)	**-0.021**	**-0.040**	**-0.061**	**-0.015**	**-0.019**
	(0.026)	**(0.098)**	**(0.032)**	**(0.054)**	**(0.021)**
Non-colonized		0.440			
		(0.640)			
EDA * non-colonized		**-0.755**			
		(0.293)			
High-descendants					-0.002
					(0.999)
EDA * high-descendants					**-29.02**
					(0.738)
Countries / Obs	67 / 449	67 / 449	53 / 383	66 / 442	66 / 442
R-square	0.438	0.401	0.369	0.431	0.419
Hansen J-test (p-value)	0.305	0.199	0.331	0.346	0.271
AR(1) test (p-value)	0.007	0.014	0.003	0.008	0.010
AR(2) test (p-value)	0.589	0.666	0.649	0.582	0.471
Additional exogenous variables used as instruments	Frz, centam, egypt, arms1, lpop, m21	Frz, centam, egypt, arms1, lpop, m21	Frz, centam, egypt, arms1, lpop, m21	Frz, centam, egypt, arms1, lpop, m21	Frz, centam, egypt, arms1, lpop, m21
No. of lags of endogenous variables used as instruments	One	One	One	One	One

Notes: p-values in parentheses based on robust and clustered standard errors. Constant term not reported. Instrumented variables are in bold type.

Table 3
Simple Correlations of Aid Measures

	EDA/real GDP	ODA/real GDP	ODA/exchange rate GDP
EDA/real GDP	1.00		
ODA/real GDP	0.95	1.00	
ODA/exchange rate GDP	0.91	0.93	1.00

Note: correlations correspond to the number of observations in Table 1, column (1) and (2).

Table 4
Alternative Measures of Aid

	ODAPPPGDP			ODAXRGDP		
	(1)	(2)	(3)	(4)	(5)	(6)
	2SLS	2SLS	GMM-SYS	2SLS	2SLS	GMM-SYS
Initial GDP per capita (log)	-0.624	**-1.16**	**1.54**	-0.805	**-1.21**	**1.61**
	(0.184)	**(0.012)**	**(0.061)**	(0.106)	**(0.019)**	**(0.108)**
Sub-Saharan Africa	-2.23	-2.45		-2.51	-2.93	
	(0.001)	(0.001)		(0.004)	(0.004)	
East Asia	2.58	2.81		2.46	2.71	
	(0.000)	(0.000)		(0.000)	(0.000)	
Institutional quality	0.349	**0.386**		0.373	**0.376**	
	(0.001)	**(0.004)**		(0.001)	**(0.010)**	
Tropical area	-1.17	-1.33		-1.02	-1.11	
	(0.028)	(0.010)		(0.079)	(0.070)	
Openness (Sachs-Warner)	-0.017	**0.510**	**3.19**	0.069	**0.258**	**3.01**
	(0.955)	**(0.471)**	**(0.00)**	(0.830)	**(0.529)**	**(0.004)**
Budget balance	10.37	**4.87**	**18.64**	9.33	**7.00**	**20.87**
	(0.074)	**(0.537)**	**(0.061)**	(0.131)	**(0.459)**	**(0.025)**
Inflation	-2.28	**-2.05**	**-1.14**	-2.14	**-1.36**	**-0.741**
	(0.000)	**(0.005)**	**(0.118)**	(0.000)	**(0.184)**	**(0.316)**
Political instability	-1.84	-1.82		-1.84	-1.93	
	(0.004)	(0.005)		(0.001)	(0.002)	
Settlers	0.021	0.039		0.040	0.046	
	(0.371)	(0.134)		(0.148)	(0.098)	
AID	**0.277**	**0.232**	**0.524**	**0.112**	**0.124**	**0.207**
	(0.020)	**(0.040)**	**(0.028)**	**(0.094)**	**(0.120)**	**(0.045)**
AID * Settlers	**-0.015**	**-0.020**	**-0.018**	**-0.015**	**-0.015**	**-0.006**
	(0.043)	**(0.005)**	**(0.087)**	**(0.037)**	**(0.021)**	**(0.099)**
Countries / Obs	67 / 449	66 / 414	76 / 530	67 / 447	66 / 412	76 / 526
Number of Instruments			66			66
R-square	0.436	0.433		0.423	0.418	
Hansen J-test (p-value)	0.200	0.420	0.159	0.275	0.686	0.113
AR(1) test (p-value)	0.005	0.145	0.000	0.003	0.101	0.000
AR(2) test p-value	0.486	0.473	0.332	0.306	0.225	0.332
Additional exogenous variables used as instruments	Frz, centam, egypt, arms1, lpop, m21	Frz, centam, egypt, arms1, lpop, m21	-	Frz, centam, egypt, arms1, lpop, m21	Frz, centam, egypt, arms1, lpop, m21	-
No. of lags of endogenous variables used as instruments	One	One	Unrestricted starting with two time lags and collapse the set	One	One	Unrestricted starting with two time lags and collapse the set

Notes: p-values in parentheses based on robust and clustered standard errors. Constant term not reported. Instrumented variables are in bold type.

Table 5a
Comparison with the Recent Literature

	(1) BD Original	(2) BD Amended	(3) BD with Outliers Amended	(4) ELR Original Full sample 1970-97	(5) ELR Amended Full sample 1970-97	(6) ELR with outliers Amended Full sample 1970-97
Initial GDP per capita (log)	-0.907	-0.755	-0.758	-0.868	-1.06	-0.382
	(0.163)	(0.254)	(0.211)	(0.076)	(0.031)	(0.536)
Sub-Saharan Africa	-1.28	-3.04	-2.46	-1.21	-2.61	-2.73
	(0.126)	(0.020)	(0.032)	(0.044)	(0.007)	(0.004)
East Asia	1.15	1.19	0.953	1.13	0.404	0.736
	(0.041)	(0.206)	(0.246)	(0.027)	(0.626)	(0.326)
Institutional quality	0.664	0.341	0.383	0.322	0.180	0.158
	(0.000)	(0.130)	(0.063)	(0.009)	(0.375)	(0.384)
Ethnic fractionalization	-0.725	-0.696	-1.10	-0.472	-0.575	-0.348
	(0.372)	(0.548)	(0.332)	(0.525)	(0.606)	(0.745)
Assassinations	-0.414	-0.305	-0.346	-0.286	-0.175	-0.186
	(0.118)	(0.038)	(0.036)	(0.259)	(0.447)	(0.330)
Ethnic fractionalization * assassinations	0.713	0.355	0.494	0.011	-0.248	-0.269
	(0.109)	(0.292)	(0.153)	(0.986)	(0.692)	(0.668)
M2/GDP (lagged)	0.017	-0.004	0.000	0.008	-0.009	-0.012
	(0.273)	(0.797)	(0.988)	(0.471)	(0.550)	(0.512)
Policy index	0.735	0.621	0.790	1.10	1.57	1.24
	(0.000)	(0.017)	(0.005)	(0.000)	(0.010)	(0.003)
Settlers		0.011	-0.004		0.006	-0.001
		(0.802)	(0.892)		(0.874)	(0.965)
EDA	**-0.323**	**0.109**	**0.117**	**-0.494**	**0.353**	**0.538**
	(0.369)	**(0.844)**	**(0.814)**	**(0.350)**	**(0.706)**	**(0.352)**
EDA * policy	**0.176**	**0.322**	**0.037**	**0.011**	**-0.328**	**-0.189**
	(0.092)	**(0.049)**	**(0.817)**	**(0.957)**	**(0.485)**	**(0.403)**
EDA * Settlers		**-0.084**	**-0.054**		**-0.075**	**-0.076**
		(0.094)	**(0.068)**		**(0.098)**	**(0.041)**
Countries / Obs	56 / 270	54 / 263	54 / 268	61 / 345	58 / 326	59 / 337
R-square	0.448	0.443	0.429	0.397	0.407	0.411
Hansen J-test (p-value)	0.117	0.566	0.309	0.201	0.705	0.693
AR(1) test (p-value)	0.213	0.455	0.277	0.032	0.044	0.031
AR(2) test p-value	0.904	0.853	0.873	0.142	0.100	0.170
Additional exogenous variables used as instruments	As in BD, Table 4, Col. (5), 2SLS	Frz, centam, egypt, arms1, lpop, lpoppolicy	Frz, centam, egypt, arms1, lpop, lpoppolicy	As in ELR, Table 2, Col. (2), 2SLS, row (5)	Frz, centam, egypt, arms1, lpop, lpoppolicy	Frz, centam, egypt, arms1, lpop, lpoppolicy

Notes: p-values in parentheses based on robust and clustered standard errors (columns 1 and 4 only robust standard errors to replicate original results). Constant term and period dummies not reported. Instrumented variables are in bold type.

Table 5b
Comparison with the Recent Literature

	(1) DHT Original	(2) DHT Amended	(3) DHT Amended & Extended	(4) CD Absolute shocks	(5) CD Shocks to GDP	(6) CG
Initial GDP per capita (log)	-0.607	-0.834	-0.949	-0.893	-1.18	-0.784
	(0.184)	(0.223)	(0.065)	(0.045)	(0.000)	(0.121)
Sub-Saharan Africa	-1.44	-2.34	-2.32	-2.04	-2.31	-2.23
	(0.005)	(0.020)	(0.011)	(0.020)	(0.003)	(0.012)
East Asia	1.99	2.25	2.49	2.78	2.76	2.58
	(0.000)	(0.004)	(0.001)	(0.000)	(0.000)	(0.000)
Institutional quality	0.289	0.273	0.385	0.348	0.378	0.329
	(0.027)	(0.086)	(0.003)	(0.001)	(0.000)	(0.005)
Tropical area	-0.893	-0.790	-0.887	-1.06	-1.05	-1.10
	(0.059)	(0.167)	(0.100)	(0.079)	(0.082)	(0.069)
Openness (Sachs-Warner)	0.847	0.693	0.048	0.494	0.367	0.451
	(0.039)	(0.143)	(0.875)	(0.181)	(0.333)	(0.481)
Budget balance	7.61	7.14	8.61	7.85	9.07	5.12
	(0.238)	(0.297)	(0.167)	(0.204)	(0.102)	(0.453)
Inflation	-2.13	-2.09	-2.11	-2.05	-2.05	-2.07
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.003)
Political instability	-2.44	-2.18	-1.70	-2.03	-1.98	-7.47
	(0.000)	(0.000)	(0.003)	(0.002)	(0.002)	(0.040)
Settlers		0.046	0.039	0.043	0.054	0.067
		(0.156)	(0.113)	(0.117)	(0.072)	(0.093)
EDA	**0.203**	**0.335**	**0.422**	**0.119**	**0.085**	**0.082**
	(0.196)	**(0.098)**	**(0.028)**	**(0.387)**	**(0.578)**	**(0.804)**
EDA * tropical area	**-0.450**	**-0.095**	**-0.141**			
	(0.029)	**(0.826)**	**(0.741)**			
EDA * Settlers		**-0.067**	**-0.062**	**-0.054**	**-0.076**	**-0.115**
		(0.044)	**(0.044)**	**(0.049)**	**(0.020)**	**(0.030)**
Positive shock				1.84	-1.40	
				(0.094)	(0.819)	
Negative shock				-2.03	-7.14	
				(0.049)	(0.385)	
ΔEDA * negative shock				0.049	0.028	
				(0.000)	(0.026)	
ΔEDA * positive shock				0.001	0.001	
				(0.854)	(0.913)	
lagged EDA * neg shock				0.009	0.006	
				(0.060)	(0.404)	
lagged EDA * pos shock				0.011	0.026	
				(0.121)	(0.000)	
EDA * political instability						**7.52**
						(0.076)
Countries / Obs	60 / 355	57 / 340	67 / 449	65 / 391	65 / 391	67 / 447
R-square	0.406	0.395	0.430	0.502	0.519	0.094
Hansen J-test (p-value)	0.180	0.474	0.333	0.806	0.790	0.412
AR(1) test (p-value)	0.032	0.014	0.010	0.059	0.084	0.026
AR(2) test p-value	0.706	0.297	0.316	0.632	0.512	0.745
Additional exogenous variables used as instruments	As in DHT, Table 3, Col. (2), 2SLS	Frz, centam, egypt, arms1, lpop, m21	Frz, centam, egypt, arms1, lpop, m21	Frz, centam, egypt, arms1, lpop, m21	Frz, centam, egypt, arms1, lpop, m21	Frz, centam, egypt, arms1, lpop, m21
No. of lags of endogenous variables used as instruments		One	One	One	One	One

Notes: p-values in parentheses based on robust and clustered standard errors. Constant term not reported. Instrumented variables are in bold type.

Table 5c
Comparison with the Recent Literature

	(1) HT	(2) CH Post-conflict 1	(3) CH Post-conflict 2	(4) CH Peace onset	(5) Svensson Political rights	(6) Svensson Civil liberties
Initial GDP per capita (log)	-0.878 (0.091)	-0.900 (0.055)	-0.926 (0.059)	-0.879 (0.067)	-0.811 (0.089)	-0.755 (0.118)
Sub-Saharan Africa	-2.36 (0.006)	-2.29 (0.005)	-2.22 (0.003)	-2.38 (0.004)	-2.58 (0.003)	-2.70 (0.002)
East Asia	2.47 (0.001)	2.49 (0.001)	2.53 (0.000)	2.49 (0.001)	2.43 (0.001)	2.32 (0.002)
Institutional quality	0.368 (0.001)	0.374 (0.000)	0.381 (0.001)	0.378 (0.000)	0.343 (0.003)	0.346 (0.003)
Tropical area	-0.986 (0.087)	-0.924 (0.122)	-0.943 (0.109)	-0.970 (0.100)	-1.11 (0.087)	-1.02 (0.114)
Openness (Sachs-Warner)	0.069 (0.821)	0.019 (0.951)	-0.038 (0.903)	0.088 (0.780)	0.181 (0.588)	0.243 (0.464)
Budget balance	8.69 (0.159)	8.51 (0.169)	9.08 (0.141)	8.15 (0.196)	8.35 (0.157)	8.07 (0.176)
Inflation	-2.14 (0.000)	-2.10 (0.000)	-2.08 (0.001)	-2.24 (0.000)	-1.93 (0.001)	-1.94 (0.000)
Political instability	-1.76 (0.002)	-1.71 (0.003)	-1.66 (0.003)	-1.76 (0.002)	-1.80 (0.005)	-1.86 (0.003)
Settlers	0.035 (0.187)	0.036 (0.140)	0.039 (0.116)	0.042 (0.120)	0.034 (0.237)	0.040 (0.193)
EDA	**0.350 (0.557)**	**0.259 (0.147)**	**0.260 (0.224)**	**0.355 (0.051)**	**0.905 (0.345)**	**1.19 (0.334)**
EDA * Settlers	**-0.058 (0.059)**	**-0.062 (0.014)**	**-0.063 (0.014)**	**-0.069 (0.020)**	**-0.065 (0.044)**	**-0.070 (0.029)**
EDA squared	**-0.004 (0.942)**					
Post-conflict dummy		-0.474 (0.559)	1.001 (0.080)	0.527 (0.578)		
EDA * post-conflict dummy		0.640 (0.000)	0.378 (0.030)	0.377 (0.407)		
Democracy indicator					0.061 (0.733)	0.220 (0.387)
EDA * democracy indicator					**-0.103 (0.543)**	**-0.158 (0.475)**
Countries / Obs	67 / 449	67 / 449	67 / 449	67 / 449	67 / 381	67 / 381
R-square	0.428	0.435	0.433	0.424	0.400	0.394
Hansen J-test (p-value)	0.379	0.406	0.360	0.370	0.275	0.284
AR(1) test (p-value)	0.007	0.006	0.004	0.003	0.249	0.221
AR(2) test p-value	0.398	0.391	0.330	0.368	0.245	0.236
Additional exogenous variables used as instruments	Frz, centam, egypt, arms1, lpop, m21	Frz, centam, egypt, arms1, lpop, m21	Frz, centam, egypt, arms1, lpop, m21	Frz, centam, egypt, arms1, lpop, m21	Frz, centam, egypt, arms1, lpop, m21	Frz, centam, egypt, arms1, lpop, m21
No. of lags of endogenous variables used as instruments	One	One	One	One	One	One

Notes: p-values in parentheses based on robust and clustered standard errors. Constant term not reported. Instrumented variables are in bold type.