When does more aid imply less democracy? An empirical examination

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Abstract

Foreign aid flows have increased considerably during the last decades, targeting, apart from development objectives, goals related to democracy. In this paper we investigate whether aid has affected the political regime of recipient countries. To this end, we use annual data on Net Official Development Assistance covering 64 aid-recipients. Because of data limitations, we cover the period 1967-2002. We find that aid flows decreased the likelihood of observing a democratic regime in a recipient country. This effect is sensitive to economic and social conditions. The negative relation between aid and democracy is moderated when aid flows are preceded by economic liberalization. Aid from the U.S. has a non-significant effect on the political regime of recipients.

JEL classifications: D70, F35, C25

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1. Introduction

Our objective is to investigate empirically the relationship between aid flows and democracy. This relationship has attracted considerable attention for two main reasons. First, democracy has increased in prevalence, which is reflected in studies that have explored empirically the determinants of democratization (see Muller, 1995; Barro, 1999; Feng and Zak, 1999; Przeworski et al., 2000; Acemoglu et al., 2005, 2007; Epstein et al., 2006; Borooah and Paldam, 2007; Papaioannou and Siourounis, 2008). Second, foreign aid, which is oriented to poorer –and typically more autocratic– countries, includes non-developmental goals related to the democratization of recipients.

The literature has pointed out that foreign aid can affect the political regime of the recipient by promoting democratic institutions, good governance and the rule of law. This effect takes mainly place through the strengthening of channels that encourage democracy, such as income and education levels (see, among others, Lipset, 1959; Almond and Powell, 1965; Barro, 1996; Doucouliagos and Paldam, 2008, 2011a). Another major channel is conditionality (see for example, Crawford, 1997; Hopkins, 2000).

The general picture from the empirical studies of aid and democratization of recipients is not clear-cut. Goldsmith (2001) found a positive, statistically significant relationship between Official Development Assistance (ODA) from Western donor countries and the level of democracy in forty-eight recipients of sub-Saharan Africa between 1975 and 1997. Dunning (2004) demonstrated that the small positive effect of foreign aid on democracy is limited to the post-Cold War period, a finding that highlights the importance of the geopolitical context in conditioning the causal effect of development assistance. Crawford (1997) analyzed 29 instances of politically motivated aid sanctions over the 1990-1996 period to find that these measures did not induce democratization. Knack (2004) used cross-section data covering the period 1975-2000 and also finds no evidence that aid promotes democracy.[1] Djankov et al. (2008) claim that the effect of aid on democratic institutions is clearly

[1] At the beginning of the 1990s, aid donors began to focus on "good governance" in the form of increased efficiency of state institutions and changes in the institutional and legal framework regulating the market and the private sector. In a parallel manner, donors placed emphasis on other indirect aspects of (non-)democratisation, such as bureaucracy, corruption and other harmful social activities; for instance, it is often claimed that aid flows result in briberies of public officials due to lack of sound public procurement and the associated discretion in

negative and even outweighs the corresponding adverse effect of natural resources.

In this paper we focus on domestic factors influencing the effect of foreign aid on the political regime. Bräutigam and Knack (2004) claimed that the adverse effects of aid were more severe with low democratic accountability. In particular, excessive aid flows can hinder the solution of collective action problems inherent in reform efforts, create moral hazard for both recipients and donors, perpetuate a "soft budget constraint" and a "tragedy of the commons" with regards to the future budget, and weaken the development of local pressures for accountability and reform, as aid revenues do not depend on the taxes raised from citizens and business. Therefore, aid is likely to impede democratization by hampering governmental accountability and undermining citizens' control over governing parties through payment of taxes.[2] Likewise, when aid is non-discretionary, democratic oversight has no role (Bräutigam, 2000).[3] Moreover, in countries with high ethnic fractionalization, aid transfers may spur competition among interest groups by increasing the size of available resources and inducing corruption and rent seeking, which in turn lead to less representative political institutions (see Grossman, 1992; Svensson, 2000; Alesina and Weder, 2002). Anecdotal evidence, surveyed by Easterly (2006), shows that the democratic effect of aid differs substantially between recipients and depends critically on domestic conditions.

Our study uses annual data for 64 aid recipients over the period 1967-2002 in the context of a two-stage discrete-response framework, which takes into account the potential endogeneity of aid and

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awarding contracts. Bräutigam (2000), Svensson (2000), Knack (2001) and Bräutigam and Knack (2004) have examined the link between aid and quality of governance, and have found that aid increases corruption and hampers bureaucratic quality.

[2] Friedman (1958) has first suggested that foreign aid provided to governments increases the relative size of public sector activities and acts as a substitute for tax revenues; this effect is supported empirically by Remmer (2004).

[3] Democracies usually require budgets and public investment programs to be approved by parliaments. Yet despite their interest in supporting new democracies, donors tend to fund projects outside of the budget, and thus outside of any possible review by parliament or central ministries (Sobhan, 1996). In Ghana, for example, the democratically negotiated 1992 Constitution stipulated that "the Government of Ghana cannot contract a foreign loan without the approval of parliament." Later the ruling party amended this to make an exception for "small foreign loans", which then allowed a number of agreements to be signed outside of parliamentary scrutiny (Ayittey, 1998). Thus, the imposing and authoritarian character of aid programs can be related to the violation of domestic democratic institutions and is primarily due to aid dependence of recipients. On the other hand, in a number of cases democratically elected governments should possibly be by-passed during the process of aid allocation, as they have been involved in strong political conflicts in their area (e.g. the Hamas regime in Gaza).

is suitable for analyzing non-linearities. We find that foreign aid flows decrease the probability of observing a democratic regime in the recipient country. We then establish that the negative marginal effect of aid flows on the democratization process of recipients is not uniform, but rather depends on the economic and social environment of the recipient country; the more unfavorable this environment is for democracy, the more adverse are aid flows to democracy. These results are robust to definitions of democracy and aid measures, alternative empirical specifications, and sensitivity tests.

We take our investigation further by asking whether the effect of aid on political liberalization is affected by economic liberalization. The classical rationale, which goes back to Schumpeter (1950), Lipset (1959) and Hayek (1960), is that countries that have liberalized their economies by allowing free trade and capital flows can enhance the efficient allocation of resources, raise income and induce economic development that in turn fosters demands for democracy. Brown (2005) argued that the potential effect of aid on democratization is greatest at the early stages of liberalization, when the resources and inertia for change are lowest. Giavazzi and Tabellini (2005) used data from 140 countries over the period 1960-2000 and found that the presence and timing of economic and political liberalizations affects structural policies, such as the control of corruption and property rights, by inducing governments to introduce new –or improve– existing institutions. We investigate whether the political effect of foreign aid differs between non-liberalized and liberalized economies and we provide evidence that the adverse effect of aid on democratization of recipients is moderated when aid flows are preceded by economic liberalization.

We contribute to the literature in the following ways. First, after taking into account the significant heterogeneity of aid recipients, we establish that the negative effect of aid on democratic institutions depends on the recipients' social and economic stance. This differentiation is important because it can explain the stylized fact that aid impedes democracy in some cases, but seems to be less (or not at all) harmful elsewhere. Second, we highlight the interactions of political liberalization and aid flows with economic adjustment in the recipient country and show that the effect of financial transfers on the recipient's political progress interacts with economic reforms.

Our investigation extends to 2002 only because of data limitations related to two key explanatory variables. First, income inequality, a major determinant of democracy in the empirical literature, is

limited by the availability of the Theil index up to 2002, which is the dataset with greatest coverage compared to alternatives such as the Gini index. Second, economic liberalization data, compiled by Sachs and Werner (1995) and updated by Wacziarg and Welch (2008), are only available until 1999, although country and time coverage is still greater compared to alternatives such as the Fraser Institute dataset. The updated binary index by Wacziarg and Welch (2008) has been established in the literature as a proxy for economic reforms; see, among others, the recent studies by Imam and Salinas (2008), Christiansen et al. (2009), Fugazza and Fiess (2010), Bhattacharyya (2011), and Coricelli and Maurel (2011). Thus, the quality of the data is a significant asset of the analysis and, in addition, renders our results comparable to existing studies of the aid-democracy nexus.

The paper is organized as follows. Section 2 outlines the econometric model and describes the data. Section 3 reports and discusses the empirical results. Section 4 examines the interaction of aid flows with economic liberalization. Section 5 concludes the paper.

2. Econometric methodology and data

In this section we briefly outline the econometric methodology and describe the data. The list of aid recipients and donor countries is given in the Data Appendix. The detailed presentation of the statistical model and the main statistics of the variables is given in the Technical Appendix to the paper.

2.1. Econometric methodology

Most empirical studies on the democracy-aid nexus have relied on averaged cross-section data, mainly in an attempt to circumvent the low within-country variability in democracy levels, whereas the analysis is usually performed using linear probability models. However, cross-sectional analysis within the context of linear models can be subject to several drawbacks, such as limited robustness in the presence of non-linearities and parameter heterogeneity. Although there are a number of econometric techniques to address these caveats, the situation is likely to be particularly acute when it comes to democracy modeling; democracy is often documented to occur globally in massive and infrequent waves, which in turn indicates the presence of strong nonlinearities in political

developments. A classical example is the well-documented surge of democratization involving Sub-Saharan Africa, Latin America and the Caribbean, as well as Eastern Europe, since the late 1980s known as the "third wave" of democratization (Huntington, 1991). The social and political unrest witnessed in several MENA countries in 2011 provided the potential for a new democratization wave. Second, the allocation of aid is likely to be subject to simultaneity bias, as it may be affected by the donors' interests regarding the political regime of the recipient country. The most clear evidence on the endogeneity of aid comes from Alesina and Dollar (2000) who state that *"..countries that have democratized have received a surge in foreign aid, immediately afterwards*[…] *The typical democratizing country gets a 50% increase in aid"*. This conclusion has been confirmed by Doucouliagos and Paldam (2011b). The authors use meta-regression analysis to reveal the relative importance of competing motives for giving aid and provide strong evidence that donors are heavily influenced both by the recipients' record of human rights and the degree of democracy, with democracy having a greater effect on aid allocation decisions. On the contrary, the recipients' humanitarian needs are less important to donors than good behaviour reflected in the aforementioned political indicators.

To confront these issues, we adopt a Two-Stage Instrumental Variables discrete-response framework, which is suitable for analyzing non-linear patterns in the data at hand and for controlling for potential endogeneity of aid flows. Our setup also incorporates random effects to account for country-specific unobserved features and to control for heterogeneity of aid recipients. Specifically, we assume that the political regime of recipient country i at time t is described by a binary variable, Y_{it}, which takes the values 0 or 1 denoting that the recipient is autocratic or democratic respectively. Moreover, we assume that the endogenous regressor, namely AID_{it}, can be written as a function of a set of exogenous instruments (uncorrelated with the political regime) and omitted characteristics, u_{it} (correlated with the political regime). We then model the effect of aid on the political regime within the following general two-stage empirical setup:

$$\textit{Stage 1:}\ \hat{u}_{it} = AID_{it} - \left(\hat{a}_0 + \sum_k \hat{b}_k X_{it} + \sum_r \hat{c}_r Z_i \right) \tag{1}$$

$$\textit{Stage 2: } P\left(Y_{it}=1\right)=G\left(a+\beta AID_{it}+\sum_{k}\gamma_{k}X_{it}+\lambda\hat{u}_{it}\right) \tag{2}$$

where AID_{it} denotes a measure of aid received by recipient country i at time t, $\boldsymbol{X}_{it}$ includes a set of k observable characteristics of country i, Z_i is a vector of r time-invariant instruments of AID_{it} that are excluded from the regime regression but are closely related to aid giving, and $\hat{a}_0$ and a denote constant terms.[4] Stage 1 is a reduced-form specification used to explain the endogenous part of aid receipts. Stage 2 is a random-effects logit model that estimates the probability of observing a democratic regime, where G is the logistic function taking values between zero and one. By applying a Wald test on λ coefficient we can test for the endogeneity of AID_{it}. Assuming that the random effects are uncorrelated with the explanatory variables, estimation of (2) via Maximum Likelihood (ML) is unbiased and consistent.

2.2. Data

To estimate equations (1) and (2), we use annual data for 64 aid-recipient countries. To proxy for the dependent variable (political status) we follow Przeworski et al. (2000), who in turn follow Schumpeter (1950) by defining democracy as a regime in which "key government office", defined as the executive and the legislature, are both filled by "contested elections".[5] Conversely, dictatorships are regimes in which either the executive or the legislature are not filled by contested elections. Contestation implies that multiple parties compete, incumbents have some probability of losing the elections, and all parties comply with the results of the elections. Przeworski et al. (2000) have developed a dichotomous measure of regime first proposed in Alvarez et al. (1996) and then updated in Cheibub et al. (2010), denoted by *DD*. The reversed *DD* dummy variable employed here is coded 1 for democracies and 0 for dictatorships. Transition years are coded as the regime that emerges in that

[4] In subsection 3.1 we provide an extensive review on the determinants of aid that are likely to serve as instruments in the present setup.

[5] The dichotomous regime classification adopted here is superior on many grounds compared to alternatives like the Freedom House index that are available over a longer time period. First, it provides a better grounding in political theory, second, it relies on observables rather than subjective judgements, third, it distinguishes between systematic and random errors, and fourth, country coverage is more extensive; for a detailed comparison between *DD* classification and existing alternatives, see Cheibub et al. (2010).

year.

To account for foreign assistance we use the standard measure of aid, as provided by the Organisation of Economic Cooperation and Development (OECD). This measure corresponds to Net Official Development Assistance, which is the net disbursement amount, i.e., disbursements minus amortisation, of those flows classified as Official Development Assistance, a conventional term introduced by the OECD. Official Development Assistance includes Grants or Loans to countries and territories on developing countries which are: (a) undertaken by the official sector; (b) with promotion of economic development and welfare as the main objective; (c) at concessional financial terms (if a loan, having a Grant Element of at least 25 per cent).[6] We scale Net Official Development Assistance with the recipients' GDP (denoted by *AID*), both measured in current US dollars, which is the usual weighting mechanism recommended to obtain a proxy for this form of transfers.

A large number of control variables are used to capture economic, political, social, institutional and religious determinants of democracy and various country-specific characteristics. The choice of these variables is mainly dictated by the existing theoretical and empirical literature, and is adjusted according to the data availability for the period under consideration.

The literature on the determinants of democracy usually includes income as a determinant of democracy level. Lipset's (1959) modernization hypothesis is that "*the more well-to-do a nation, the greater the chances that it will sustain democracy*". Gundlach and Paldam (2009) find a long-run causal effect of income on the degree of democracy. However, recent empirical work by Knack (2004) and Acemoglu et al. (2007) shows that this association evaporates once one controls for factors that simultaneously affect income and democracy. Similarly, Przeworski and Limongi (2000) have argued that there is no systematic relationship between economic factors and the appearance of democratic regimes; however, there is a significant relationship between economic factors and the likelihood of a country remaining a democracy. In empirical applications, modernization enters in several forms but the majority of studies employ the level of education (see, among others, Barro, 1999; Knack, 2004;

[6] In addition to financial flows, technical co-operation is included in aid. Grants, Loans and credits for military purposes are excluded. Transfer payments to private individuals (e.g. pensions, reparations or insurance payouts) are in general not counted.

Acemoglu et al., 2005). We use the percentage of literate population to total population aged 15-24 provided by the World Bank (World Development Indicators, WDI).

According to the literature, democracy is expected to emerge out of a strategic face-off between the rich minority that is inimical to democracy due to fear of redistribution and the poor majorities who try to extract democratic concessions from the rich; thus countries with higher income inequality tend to be less democratic (Acemoglu and Robinson, 2006). We use the Theil index to control for the effects of income inequality on democratization due to data availability (source: UTIP-UNIDO University of Texas inequality project). The Theil index provides more annual data than the Gini index, but time coverage stops in 1999. This limits our sample coverage and excludes more recent years, for which data on most explanatory variables (including regime classification and aid flows) are available.[7] Since income inequality is a core control variable of our model, we use the simple average of the last 5 years to generate values for the years 2000-2002, on the grounds that this variable is highly time-persistent.

Although most of the traditional work on the determinants of democracy has focused on the domestic attributes of countries, external factors related to the degree of openness, such as international trade, are also likely to influence the prospects of democracy through the spread of innovative ideas and the adoption of more liberal political systems; see Huntington (1991), Whitehead (1996), and Gleditsch (2002).[8] We use the standard measure of trade openness, namely the sum of exports and imports to GDP, to capture the extent of this influence (source: WDI).

According to an argument broadly termed as "the curse of natural resources", oil-rich countries tend to adopt less democratic ways of governance partly because abundance of natural resources enables the state to buy off society with low taxation and high welfare spending and thereby allay popular demand for political accountability. Rents from natural resources can also distort modernization by spurring the expansion of national income without inducing the socioeconomic

[7] Another explanatory variable that further restricts our data span is the economic liberalization index (see the discussion in section 4).

[8] The empirical evidence on this argument is mixed. Li and Reuveny (2003), Rigobon and Rodrik (2005), and Giavazzi and Tabellini (2005) find no impact of trade openness on democracy or assess an adverse effect. In contrast, Rudra (2005) and Papaioannou and Siourounis (2008) argue that the effect of trade openness on democratization is positive.

changes that usually accompany an increase in wealth and that are likely to favor democracy (Karl, 1997; Ross, 2001; Jensen and Wantchekon, 2005). To control for this effect we add a dummy variable that equals unity for oil-exporting countries (source: Easterly and Kraay, 2000).

The military character of a country is also regarded as an indicator of its political regime (Crenshaw, 1995; Kimenyi and Mbaku, 1996; Ross, 2001), on account of the major role of the military in the establishment, maintenance and overthrow of governments. For instance, history shows that the main reasons why democratic systems of government are overthrown are military: conquest or military coup. Strong defense is therefore required to prevent or deter conquest, but a strong military can increase the threat of military coup, so a delicate balancing act is required. In the present analysis averaged military expenditures (as a percentage of GDP) enter regressions to control for the military country-specific characteristics of the recipient countries (source: WDI). In accordance with the empirical literature, we expect social divisions to affect the democratization process negatively, since democracy is less likely to prevail in countries that are socially divided and lack cultural and linguistic coherence (see Horowitz, 1993). We employ the ethnolinguistic fractionalization index to proxy for the number of competing groups and for the degree of conflict within society (Barro, 1999; Clague et al, 1996).[9] Due to unavailability of annual data, we use each country's average value over 1960-1980 throughout based on the assumption that institutional factors change slowly over time. We then introduce a dummy variable taking a value of unity whenever the ethnolinguistic fractionalization index for a country exceeds 0.5 (source: Annett, 2001).

According to some studies, geographic position is a factor that contributes to the shaping of political institutions. For instance, it has been argued that in temperate zones the climate is healthier and agriculture is more productive, thereby enabling a faster development process that facilitates the improvement of institutions (Sachs, 2005).[10] We follow this approach and we let the absolute value of latitude (normalized between 0 and 1) enter the estimated specification (source: CIA Factbook). We

[9] Notice that the indices do not measure the "intensity of conflict" between groups but rather, for a given number of ethnic groups in society, the probability that two randomly selected individuals from the country in question will not belong to the same ethnic group with a higher value reflecting a greater degree of fractionalization.

[10] La Porta et al. (1999) have established empirically that the latitude of a country has a strong positive effect on government performance, especially when one controls for economic performance.

also introduce religious beliefs and we add dummy variables to proxy for Islamic and Catholic countries (source: www.adherents.com). In 2002, 38 out of 47 Islamic countries (80%) were rated as non-democratic according to the *DD* classification of political regimes. Borooah and Paldam (2007) and Potrafke (2011) confirm that Islamic countries are less likely to be democratic. Our sample also contains a limited number of countries where Jewish (Israel) or Hindu (India, Mauritius, and Nepal) populations are the majority.

Another important issue is that regimes consolidate over time and become self-sustaining (see Muller, 1995; Barro, 1999; Acemoglu et al., 2005, 2007). We examine a simplified version of Barro's (1999) "democracy convergence" hypothesis according to which the political regime of a country converges gradually over time toward a (moving) target. Knack (2004) also included an index of initial regime to capture regression-to-the-mean effects attributed to the limited opportunity of democratic countries to increase their ratings. We use the initial values of the dependent variable or the first available observation as a proxy of initial political conditions. This specification enables regime ratings to be conditional on their starting values and also helps dealing with serial correlation often met in the dynamic modelling of political regimes. Finally, we add a dummy variable to indicate the Post-Cold war period when democracy experienced a sharp increase worldwide as a result of externally-influenced transparent, participatory, and accountable political and economic systems, the abandonment of dictators from the West and the acceptance of free trade, human rights, and the rule of law as norms. In the years immediately after the fall of the Berlin Wall in 1989 and the collapse of the Soviet Union in 1991, democracies increased from about 40 percent of all states to 60 percent. See Bratton and Van de Walle (1997), Kirschke (2000), and Solt (2001).

3. Empirical results

In this section we present the instrumentation strategy for aid and we report the empirical results for equations (1) and (2).

3.1. Instrumenting for aid flows

In order to explore the potential endogeneity of aid, we regress aid on various pre-aid factors that have

been pointed out as major aid allocation criteria; Boone (1996), Alesina and Dollar (2000), Burnside and Dollar (2000), Easterly et al. (2004), and Knack (2004) have shown that there are several instruments for aid that can be used to address endogeneity problems. Specifically, there is ample evidence that donors direct aid to low-income countries, but also that they are influenced by the population size, with more populous countries receiving less aid ("country-size bias").[11] Thus, one should expect a negative correlation between aid and both income and population levels. We follow these studies and use initial income (measured by the log of real per capita income in the beginning of the period or the first available observation) to capture recipients' needs and initial population (in logarithms) to capture donors' interests (source: WDI).[12] Moreover, in contrast to the altruistic belief that aid is primarily motivated to assist the poor, substantial evidence also points towards political and geopolitical factors, such as strategic alliances of donor countries, as major driving forces behind aid programs (see Maizels and Nissanke, 1984; Frey and Schneider, 1986; and Trumbull and Wall, 1994). To control for these strategic interests, we use the standard political dummy variables that help capture the importance of a recipient to a particular donor (see Boone, 1994, 1996; Burnside and Dollar, 2000; Easterly et al., 2004; and Knack, 2004). These dummies include Sub-Saharan Africa (to which most European aid is directed), the Franc zone countries, Egypt (over the period, regarded as an ally of the U.S.), and Central American countries (also in the U.S. sphere of influence).

Estimation results of the first-stage equation (1) are reported in columns denoted by (a) in Table 1. In columns (1a) and (2a) foreign aid is measured as a percentage of the recipient's GDP (*AID*) and estimates correspond to the full sample and the outliers-free sample, respectively.[13] In these regressions foreign aid is regressed on a set of instruments comprised by pre-aid factors, regional dummies, and the explanatory variables of the corresponding second-stage democracy regression. As

[11] There are several reasons why the size of the recipient country may be an important determinant of aid flows. First, both international institutions and bilateral donors hesitate to transfer large nominal amounts, as they will come under much greater public scrutiny than relatively smaller amounts. Second, small countries may have relatively higher influence in some international organizations with the most obvious example being the voting process at the United Nations. Third, small countries may be more willing to sell their influence, as they may gain more from joining a coalition than by acting independently.

[12] Notice that the fact that initial values of income are employed (instead of current ones) renders less likely that causality runs from foreign aid to income.

[13] We detect outliers following Hadi (1994), where the cutoff probability is 0.05.

expected, we find that foreign assistance is systematically directed to small and low-development countries. In addition, Egypt and Sub-Saharan countries enjoy more aid *ceteris paribus*. However, we find no evidence that countries located in Central America or the Franc Zone receive proportionally more assistance. Finally, there appears a negative structural break in the amount of aid per capita during the Post-Cold-War period.

Concerning endogeneity, the Wald coefficient tests on the *Predicted residuals* variable always lead to rejection of the null hypothesis that foreign aid is exogenous, indicating that endogeneity is present in the data at hand. The validity of the instrumentation approach is checked by first evaluating the explanatory power of the selected instruments using an F-test to assess their joint significance. The null hypothesis of the test is that the instruments set is weak and instruments are considered strong and relevant if the F-statistic exceeds 10, as suggested by Staiger and Stock (1997). The reported values of the statistic always exceed the conventional threshold implying that the selected set of instruments is not weak. Also, to test if instrumental variables are exogenous, i.e. uncorrelated with the error term of the probability regression, a Sargan/Hansen-type test of overidentifying restrictions is performed, where the null hypothesis is that the selected instruments are validly excluded from the second-stage regression. The reported chi-squared statistics of the test always lead to non-rejection of the null hypothesis that the selected instruments are exogenous. Thus, we can safely infer that the above instrumentation method is valid and that endogeneity of aid is properly addressed within the present empirical setup.

3.2. Regression results

In this subsection we describe the main estimation results and we analyze the underlying differential effect of aid flows in the presence of non-linearities. Estimation of the second-stage equation (2) is performed via Maximum Likelihood and preliminary results are reported in columns denoted by (b) in Table 1.

First, the findings on the estimated coefficients of the control variables corroborate Knack (2004) and Acemoglu et al. (2005) who showed that the association between political change and economic conditions falls out once one controls for factors that simultaneously affect income and democracy.

Trade openness, geographic position, military expenditures and ethnolinguistic fractionalization are also not found to affect democracy. On the contrary, the dominant religion seems to be a crucial determinant of political developments with Muslim countries enjoying less democracy compared to Catholic ones. Our results are also supportive of the democracy surge of the Post-Cold War period. Finally, we find that the political stance of a country is highly contingent on the initial political conditions implying considerable persistence in democracy levels.

Turning to the effect of aid on democratization, estimation results show that foreign aid exerts a negative and statistically significant effect on the probability of observing a democratic regime in the average-case recipient country. In particular, a one-unit increase in average *AID* (from 5.95% to 6.95% of the recipient's GDP) is expected *ceteris paribus* to decrease the predicted probability of observing democracy by roughly 15% (column 1b).[14] A valid concern is, however, that this result might be seriously affected by the presence of outliers given that the allocation of aid exhibits large variation across recipients.[15] We thus replicate estimation excluding Hadi outliers from the sample (column 2b).[16] Results verify the negative effect of aid on democracy, whereas the rest coefficients remain virtually unaffected. Here, a one-unit increase in average *AID* (from 5.46% to 6.46% of the recipient's GDP) is expected *ceteris paribus* to decrease the predicted probability of observing democracy by roughly 18%.

In addition, we investigate whether the aforementioned negative effect of aid is altered when flows are expressed in per capita terms. The majority of empirical studies on the growth impact of aid conclude that aid has systematically failed in boosting growth in the recipient countries and emphasize that this result is not contingent on the scaling mechanism of aid. However, the *political* effect of aid may depend on the population size of the recipient country rather than on its economy size for two reasons. First, a heavily populated developing country requires, ceteris paribus, more aid than a less populated one (McGillivray, 1989). Second, expressing assistance in per capita terms might be of particular importance since macroeconomic data for developing countries rarely reflect the actual size

[14] The marginal effect of aid is calculated at the sample mean of the explanatory variables.

[15] For instance, foreign aid amounts on average to 5.95% of the recipient's income in the developing world, but in some countries it exceeds 50%.

[16] Bahrain, Egypt, Israel, Kuwait, Nicaragua, Oman and Saudi Arabia are excluded from estimation.

of their economies due to illegal and other underground or unreported activities with discrepancies reaching sometimes 70% of GDP (Schneider and Enste, 2000; Schneider, 2005). Therefore, we also use alternatively Net Official Development Assistance per capita (in constant 2002 prices) as an explanatory variable, denoted by *AID_PC*, and we replicate the logit estimations for the total and the outliers-free sample (columns 3b and 4b of Table 1).[17] The estimated coefficients on *AID_PC* are negative and statistically significant confirming the inverse effect of aid on the political regime of recipients.

Standard goodness-of-fit measures are used to assess the validity of estimated regressions. The first goodness-of-fit measure is the *Pseudo-*R^2 that indicates a substantial explanatory power for the models at hand. To test for the joint significance of all control variables, we employ a Wald test, according to which we can always reject the null hypothesis that the slope coefficients of the regression are jointly not significantly different from zero. Next, we test for the presence of random effects using a Likelihood Ratio test, where the null hypothesis corresponds to *Rho*=0, i.e. the panel level variance component is not important and consequently the model does not improve the pooled model significantly. The estimates of *Rho* indicate that the random-effects estimator is superior to the pooled estimator at standard significance levels for all specifications. Finally, we report the *percent correctly predicted* that equals the percentage of times the predicted Y_{it} matches the actual Y_{it}. In order to evaluate the overall ability of the model to predict both zero and unity values we calculate a weighted index of the percent correctly predicted. The regressions reported in tables predict over three quarters of the actual political outcomes correctly indicating a rather strong predictive power of the empirical models.

Next, we explore whether the effect of aid differs between bilateral and multilateral aid flows. Several studies have shown that the impacts between these forms of aid are likely to be different; see, for instance, Maizels and Nissanke (1984), Frey and Schneider (1986), Alesina and Dollar (2000). In particular, we focus on aid flows by the United States and by multilateral agencies, which include multilateral development banks (e.g. the World Bank), United Nations agencies, and regional

[17] The results for the normalized variables are reported, as original variables yielded large standard errors due to extreme dispersion.

groupings (e.g. European Union agencies).[18] Estimation results are presented in Table 2 and show that the negative effect persists in the two sets of regressions. In particular, aid from the US appears with a negative (although insignificant) coefficient, whereas aid from multilateral agencies exerts a negative and highly significant effect on the probability of observing a democratic regime in the recipient countries. A potential explanation for this finding is that aid by the United States is far smaller than that of multilateral agencies, amounting on average to 0.7% of recipients' income relative to 2% by multilateral agencies, and thus its detrimental effect on democracy is found to be smaller.

Finally, to assess the robustness of the adverse effect of aid, we also performed sensitivity tests following the literature. To deal with sample selection and reverse causality problems, we examined only countries that were classified initially as non-democratic (Papaioannou and Siourounis, 2008). Also, we removed time trending from foreign aid and democracy that could give rise to a spurious relationship between these variables, we examined the effect of aid in the Post-Cold War period only (Knack, 2004); we added historical explanatory variables, namely the date of independence and the nature of institutions immediately after the end of the colonial period (for former colonies) or at the date of national independence (for non-colonies), proxied by the constraints on the executive (Acemoglu et al., 2007); we examined former colonies only (Acemoglu et al., 2002); we controlled for prior colonization by Europeans (Rigobon and Rodrik, 2005); we used alternative measures of democracy, namely Freedom House and Polity2 data; and we replicated estimation in the context of the Barro's (1999) dynamic specification. The estimated coefficients for *AID* always retained their negative sign and were statistically significant.[19]

3.3. Aid flows and the probabilities of observing democracy in the recipient country

The discrete-response random-effects model of our analysis implies that the probability of observing democracy in country *i* at time *t* is a function of the observable explanatory variables and

[18] We thank a referee for this suggestion. See the Data Appendix for a detailed listing of multilateral agencies. Notice that, according to the OECD classification, "multilateral agencies" correspond to international institutions with governmental membership which conduct all or a significant part of their activities in favor of development and aid recipient countries. A contribution by a DAC Member to such an agency is deemed to be multilateral if it is pooled with other contributions and disbursed at the discretion of the agency.

[19] The full set of estimates is available in the Technical Appendix to the paper.

unobservable factors captured in the country-specific, time-invariant random component. However, in contrast to the linear probability model, logit models are non-linear in the parameters and, as is well known, the estimated coefficients cannot be interpreted as marginal effects. Therefore, to evaluate the change in the predicted probability of observing democracy $\hat{P}(Y_{it}=1)$ in response to a one-unit change in aid, we scale the estimated coefficients of *AID* by the factor *exp(w)/[1+exp(w)]*^{2}, where $w \equiv \hat{a} + \hat{\beta} AID_{it} + \sum_k \hat{\gamma}_k X_{it} + \hat{\lambda}\hat{u}_{it}$. Obviously, the scaling factor is observation-contingent, thus yielding a non-constant marginal effect of *AID* on democracy.

To highlight the magnitude of the results reported in Table 1, we explore the magnitude of the marginal effects of aid at some plausible levels of the explanatory variables (Table 3). In particular, we explore several scenarios in order to find out how *AID* affects the probability of observing democracy in both "favorable" and "unfavorable" environments (under which these probabilities are ex post high and low respectively) during the Post-Cold War period. The first scenario explores the effect of *AID* in Muslim countries with oil-exporting activity and high ethnolinguistic fractionalization levels that have entered the sample as non-democratic (case 1.1). Given these assumptions the predicted probability of observing democracy is expected to be considerably low; Iran, Kuwait and Qatar belong to this category. We next explore an intermediate scenario that concerns countries with catholic beliefs, no revenues from oil exporting activity and relatively coherent societies that moreover have entered our analysis as non-democratic (case 1.2). Argentina, Burundi, Cape Verde, Honduras, Lesotho, Rwanda, Seychelles are classified under this category. Finally, we explore a "favorable" environment that is similar to the previous one but with initially democratic countries (case 1.3); such countries are, for instance, Costa Rica, Malta, and Uruguay.[20] We also report as a benchmark the marginal effect of *AID* estimated at the sample means of the regressors.

Column (2) in Table 3 reports the associated probabilities for cases 1.1, 1.2 and 1.3. We find that aid always exerts a negative effect on the probability of observing democracy. In the benchmark case a one-unit increase in average *AID* is expected *ceteris paribus* to decrease the predicted probability of observing democracy by roughly 17%. However, the magnitude of this effect varies considerably. The

[20] The calculations have relied on the outliers-free regression (2b) of Table 1.

magnitude of the negative effect is larger in countries under the first ("unfavorable" environment) scenario (23.5%), whereas it is moderated in the intermediate scenario and is substantially smaller under the "favorable" scenario. A one-unit increase in average *AID* is expected to decrease the predicted probability of observing democracy by roughly 14% in the intermediate group and by only 5% in the third group. These figures imply that the negative effect of aid flows on the political regime of recipient countries is not uniform, but depends on the general economic and social environment in the recipient country. The more unfavorable this environment is for democracy, the more disastrous are aid flows to democracy.

4. The effect of economic liberalizations

Our analysis so far has shown that foreign aid is an important determinant of democracy in recipient countries and that the marginal effect of aid flows depends crucially on the domestic conditions. In this section, we investigate whether economic reforms work along with foreign aid towards the democratization of recipients. Our investigation is motivated by Fidrmuc (2003), Giavazzi and Tabellini (2005) and Persson and Tabellini (2006); these studies establish that there are significant interactions between political and economic liberalizations, with causality running more often from the former to the latter, whereas countries perform better in terms of many macroeconomic variables, like growth and investment, when democratic reforms are preceded by economic liberalizations (rather than vice versa).[21] As pointed out by the authors, political and economic reforms have typically been studied separately, although a bulk of anecdotal evidence indicates that their interaction is important. In this vein, a related question in our setup is whether and, if so, to what extent foreign aid works jointly with economic liberalization towards the democratization of recipients.

To investigate this hypothesis, we follow closely Giavazzi and Tabellini (2005) and we define economic liberalizations as discrete and comprehensive policy changes that increase the scope of the

[21] This means that, as pointed by Giavazzi and Tabellini (2005) and Persson and Tabellini (2006), one cannot rule out a reverse relationship with causality running from economic to political liberalizations or the existence of feedback effects. This possibility was investigated by standard tests that led to rejection of the hypothesis that a reverse or feedback relationship between these variables exists, thereby confirming that the relationship runs merely from economic to political liberalizations.

market in allocating goods and services. We use the dataset compiled by Sachs and Werner (1995) and updated by Wacziarg and Welch (2008) to capture economic liberalizations. The dataset is available until 1999, which restricts our sample to the 1967-1999 period.[22] According to the definition put forward by these authors, a country is considered closed to international trade if one of the following conditions holds: (i) average tariffs exceed 40%, (ii) non-tariff barriers exceed 40% of imports, (iii) the black market premium on the exchange rate exceeds 20%, (iv) exports are to a large extent controlled by a state monopoly.[23] In turn, we refer to an economic liberalization as the event of the country becoming open (in which case none of these conditions holds), given that it was closed in the previous year.

Since our aim is to investigate whether foreign aid works in conjunction with economic liberalizations, the latter are introduced with a five-year lag both individually and in the form of an interaction term with foreign aid. Table 4 presents the results. Specification (1) shows, first, that accounting for the effect of economic liberalizations does not affect the negative effect of foreign aid. Second, economic liberalizations enter with a negative and statistically significant coefficient in the estimated regression. However, their interaction effect with aid is significantly positive indicating that the net effect of *AID* will differ between liberalized and non-liberalized economies.

In particular, given the negative coefficient of *AID* and the positive sign of the interaction term, we expect that *AID* should damage democracy at a greater extent in economically non-liberalized countries than in liberalized ones. To further explore this empirical finding, in Table 3 we compare the probability change in democratisation between non-liberalized and liberalized countries over the 1967-1999 period (rows 2.1 and 2.2). In countries where economic liberalization has taken place, a one-unit increase in *AID* decreases the predicted probability of observing democracy by approximately 13%. In

[22] The data are available at www.stanford.edu/~wacziarg/downloads/liberalization.xls. Another measurement of economic liberalization is available through the Economic Freedom of the World Indexes provided by the Fraser Institute. These indicators cover a wide spectrum of economic freedom areas, apart from international trade, such as personal choice, voluntary exchange, freedom to compete, and security of privately owned property. However, the Fraser dataset is available from 1970 to 2000 only on a five year basis.

[23] A country is also considered closed if it has a socialist economic system, which does not apply to our case. Notice that after the inclusion of economic liberalization in the estimated regression, trade openness becomes -as expected- statistically insignificant. We thus omit the latter variable from the estimated specification without any significant change in the rest of the results.

contrast, in countries where no economic liberalization has taken place, a one-unit increase in *AID* decreases the predicted probability of observing democracy by approximately 38%. Hence, we find that aid is roughly three times more disastrous for democracy in a country under a non-liberalized economic environment.

Finally, as an additional robustness test of the previous finding we run two separate logit regressions corresponding to observations where the economic liberalization dummy equals unity and zero respectively (see columns (2) and (3) of Table 4). We then perform a Wald test on the null hypothesis that the coefficients of *AID* are not statistically different between the two populations. Since the chi-square statistic of the test equals 4.22 (critical value: 3.84) we can safely reject the null hypothesis. Evidently, aid flows exert a statistically significant differential (smaller) effect on democratization in countries that have opened up their economy.

5. Conclusions

We have investigated empirically the effect of foreign aid on the political regime of the recipients. We addressed this issue within the context of a two-stage binary response model that allowed for non-linearities in the process of democratization as well as for the endogeneity of aid allocation. Using a sample of 64 aid recipients for the period 1967-2002, we found considerable evidence that aid flows affect negatively the probability of observing democracy in recipients. This result is robust to the distributional assumptions, the choice of the controls set, the presence of outliers, the definition of democracy, the scaling of aid flows, and the sample selection. We also established that the negative effect of aid flows on the political regime of recipients is more intense in unfavorable environments for democracy, but is moderated when aid flows are preceded by economic liberalization. Finally, aid from the U.S. has a non-significant effect on the political regime of the recipients, whereas aid from multilateral agencies has a negative and statistically significant effect.

There are a number of possible explanations for our results. It is often argued that aid assists autocratic regimes in maintaining power (often by simply symbolizing the approbation and active support by a foreign power) or hinders good governance by triggering government inefficiency and corruption in the public sector. In practice, foreign aid produces a revenue flow that may generate

corruption, rent-seeking activities, and civil wars. People in power will engage in rent-seeking activities in order to appropriate these windfall resources and as a result will try to exclude others from engaging in the government decision-making process; hence, political institutions become less democratic and less consensual. On the other hand, the evidence on the reduction of the negative effect of aid when the recipient has reformed its economy implies that, in a more competitive environment in which the private sector is relatively more effective, the adverse mechanisms triggered by aid inflows can be largely eliminated.

Another reason for aid failure is related to the dysfunction and corruption of the public sector and its ties with government (Hayek, 1945; Abed and Gupta, 2002; Prokopijevic 2006; Easterly, 2009). Perhaps effectiveness of aid can be increased by engagement of the private sector, such as private voluntary organizations (PVOs), consulting firms and other business units, in the aid industry. The role of the private sector has been very important in carrying out aid programs. CARE/USA, for example, a PVO, manages over 600 projects in 73 countries funded by the United States Agency for International Development (USAID).

As in existing empirical literature, we did not attempt to identify the mechanisms through which aid hampers the political regime. Despite our finding of robustness of the negative effect of total aid, it is possible that some aid categories have not adversely affected democracy in recipients. Foreign aid encompassed activities such as infrastructure financing, support for education and health, and environmental improvement. Some forms of aid are more vulnerable to misuse by autocratic regimes and may be directed to activities that ensure the sustainability of the regime rather than the welfare of the population; see Gupta et al. (2001), Gupta et al. (2002). Finkel et al. (2007) focused on democracy and governance related, rather than total, assistance provided via the United States Agency for International Development (USAID) and found that it promoted the democratization of recipients. A disaggregated analysis of the democratic effect of aid seems therefore warranted and the empirical investigation of these channels remains open to more refined research.

Data Appendix

Recipients

Data Set 1.a (64 countries):

Algeria, Argentina, Bahrain, Bangladesh, Belize, Benin, Bolivia, Botswana, Brazil, Burkina Faso, Burundi, Cameroon, Cape Verde, Central African Republic, Chile, Cote d' Ivoire, Cyprus, Ecuador, Egypt, Fiji, Ghana, Guatemala, Honduras, India, Indonesia, Iran, Israel, Jordan, Kenya, Kuwait, Lesotho, Malawi, Malaysia, Malta, Mauritania, Mauritius, Mexico, Morocco, Mozambique, Namibia, Nepal, Nicaragua, Nigeria, Oman, Pakistan, Panama, Paraguay, Peru, Philippines, Rwanda, Saudi Arabia, Senegal, South Africa, Sudan, Tanzania, Thailand, Togo, Tunisia, Turkey, Uganda, Uruguay, Venezuela, Zambia, Zimbabwe.

Data Set 1.b (57 countries) includes Data Set 1.a minus:

Bahrain, Egypt, Israel, Kuwait, Nicaragua, Oman, Saudi Arabia.

Donors

1. Development Assistance Committee (DAC) member countries: Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, Korea, Luxembourg, Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, United Kingdom, United States and the Commission of the European Communities

2. Multilateral Agencies: African Development Bank (AfDB), African Development Fund (AfDF), Asian Development Fund (AsDF), Asian Development Bank (AsDB), Caribbean Development Bank (CarDB), European Bank for Reconstruction and Development (EBRD), European Commission (EC), Global Environment Facility (GEF), Global Fund for AIDS, TB and Malaria (GFATM), Montreal Protocol, Nordic Development Fund, International Bank for Reconstruction and Development (IBRD), International Development Association (IDA), Inter-American Development Bank (IDB), IDB Spec. Fund, IMF Trust Fund, IMF, International Fund for Agricultural Development (IFAD), United Nations Development Programme (UNDP), United Nations Population Fund (UNFPA), United Nations High Commissioner for Refugees (UNHCR), United Nations Children's Fund (UNICEF), United Nations Relief and Works Agency (UNRWA), United Nations Transitional Authority (UNTA), World Food Programme (WFP), Council of Europe, Arab Agencies,

3. Non-DAC member countries: Czech Republic, Hungary, Iceland, Poland, Slovak Republic, Turkey, Arab Countries.

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TABLE 1. The effect of foreign aid on the political regime

Variables	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
AID		**-0.33****		**-0.28****				
		(0.14)		**(0.14)**				
AID_PC						**-3.61****		**-4.09****
						(1.84)		**(2.08)**
Initial GDP	-3.20***		-3.08***		-0.02		-0.04**	
	(0.46)		(0.46)		(0.05)		(0.02)	
Initial Population	-3.68***		-4.10***		-0.32***		-0.26***	
	(0.40)		(0.26)		(0.04)		(0.01)	
Sub-Saharan Africa	4.23***		3.70***		-0.11***		0.02**	
	(0.61)		(0.47)		(0.02)		(0.01)	
Egypt	3.32***		Dropped		0.02**		Dropped	
	(1.04)				(0.01)			
Central America	-1.92***		-3.30***		-0.08***		-0.01	
	(0.67)		(0.42)		(0.02)		(0.01)	
Franc Zone	-2.68***		-0.76		-0.11***		-0.03***	
	(0.75)		(0.56)		(0.02)		(0.01)	
Literacy Rate	-0.12***	-0.08***	-0.08***	-0.07**	-0.02	1.41***	-0.03***	1.09***
	(0.01)	(0.03)	(0.01)	(0.03)	(0.03)	(0.41)	(0.01)	(0.40)
Trade Openness	0.00	0.00	-0.01	0.00	0.21***	0.99*	0.08***	0.63
	(0.01)	(0.01)	(0.01)	(0.01)	(0.05)	(0.56)	(0.01)	(0.39)
Military Expenditures	0.07	-0.05	-0.35***	-0.46	0.11**	-0.11	0.02	-1.44
	(0.06)	(0.22)	(0.11)	(0.35)	(0.05)	(0.80)	(0.02)	(1.19)
Oil Exporting activity	-3.02***	-3.35	0.06	-1.69	-0.18***	-1.43*	-0.03***	-0.70
	(0.42)	(2.11)	(0.32)	(1.95)	(0.03)	(0.75)	(0.00)	(0.64)
Income inequality	0.06	-0.11*	0.25***	-0.04	-0.01	-0.16	0.05***	0.30
	(0.04)	(0.06)	(0.04)	(0.07)	(0.04)	(0.23)	(0.01)	(0.27)
Muslim Dummy	0.34	-4.33***	2.05***	-2.84**	-0.00	-1.81***	0.05***	-1.17**
	(0.40)	(1.31)	(0.32)	(1.26)	(0.03)	(0.60)	(0.01)	(0.57)
Catholic Dummy	2.52***	2.30*	2.83***	2.64**	-0.14***	-0.23	-0.01	0.25
	(0.45)	(1.27)	(0.38)	(1.27)	(0.03)	(0.62)	(0.01)	(0.58)
Ethnolinguistic Fractionalization	0.83**	0.61	1.57***	1.11	-0.09**	-0.13	0.05***	0.78
	(0.41)	(1.29)	(0.35)	(1.31)	(0.04)	(0.68)	(0.01)	(0.60)
Latitude	-0.17	0.16	0.34	0.69	-0.13***	0.04	0.05***	0.87
	(0.25)	(0.61)	(0.21)	(0.63)	(0.04)	(0.54)	(0.01)	(0.55)

TABLE 1. (continued)

Post-Cold War Period	0.99* (0.60)	1.40*** (0.39)	0.16 (0.44)	0.95** (0.38)	-0.12*** (0.03)	0.93*** (0.29)	-0.06*** (0.01)	1.08*** (0.20)
Initial Political Regime	-1.71*** (0.29)	6.28*** (1.39)	-1.26*** (0.20)	5.16*** (1.33)	0.07*** (0.02)	2.71*** (0.53)	-0.01 (0.01)	1.99*** (0.49)
Predicted residuals		0.34** (0.14)		0.32** (0.15)		3.54* (1.91)		4.02* (2.26)
No of countries (observations)	64 (1832)		57 (1537)		66 (1874)		57 (1582)	
F-statistic of excluded instruments	61.01		124.02		15.64		100.59	
Sargan-Hansen X^2 statistic (Prob)		10.86 (0.06)		3.71 (0.45)		4.32 (0.50)		5.95 (0.20)
Second-Stage X^2 (Prob)		225.37 (0.00)		204.61 (0.00)		213.19 (0.00)		198.29 (0.00)
Rho - LR test (Prob)		0.79 (0.00)		0.77 (0.00)		0.79 (0.00)		0.76 (0.00)
R^2 / Pseudo-R^2	0.43	0.40	0.57	0.37	0.28	0.38	0.55	0.33
% Correctly Predicted		83.6%		82.4%		82.2%		80.5%
Average Foreign Aid		5.95		5.46		55.92		37.76
Predicted Probability $\hat{P}(Y_{it}=1)$		0.25		0.26		0.24		0.27
Percentage Change in $\hat{P}(Y_{it}=1)$		-14.8%		-18.1%		-1.67%		-2.22%
Marginal Effect of average Foreign Aid		-0.037*** (0.000)		-0.047*** (0.005)		-0.004*** (0.001)		-0.006*** (0.001)

Notes:

1. *For each pair of regressions column (a) reports first-stage OLS estimates of foreign aid using White's heteroskedasticity-consistent covariance matrix, see equation (1) in the text. Column (b) reports second-stage logit coefficients obtained from Maximum Likelihood estimation of equation (2), which models the probability that country i is democratic in year t. Estimation assumes random effects.*
2. *In columns (1a)-(2b) foreign aid is measured as a percentage of the recipient's GDP (AID), whereas in columns (3a)-(4b) aid is measured in per capita terms (AID_PC). The estimates of columns (1a)-(1b) correspond to Dataset 1.a. The estimates of columns (2a)-(2b) correspond to the outliers-free Dataset 1.b. Specifications (3a)-(3b) correspond to Dataset 1.a plus Mongolia and Syria. Estimates of (4a)-(4b) correspond to Dataset 1.b minus Cape Verde, Israel and Malta (see Data Appendix for country coverage). In columns (4a)-(4b) all explanatory variables have been standardized.*
3. *Estimations correspond to the period 1967-2002. Values in parentheses denote standard errors unless otherwise indicated. *, **, *** correspond to statistical significance at 10%, 5%, and 1%, respectively. A constant term was included in all regressions. Marginal effects and Predicted Probabilities are estimated at the sample means of the explanatory variables. The percentage change in $\hat{P}$ is the percentage change in the Predicted Probability that the average-case country is democratic at time t in response to a one-unit increase in the amount of average foreign aid. The marginal effect of average foreign aid corresponds to the change in the Predicted Probability that the average-case country is democratic at time t in response to a one-unit increase in the amount of average foreign aid. For the Sargan-Hansen test of overidentifying restrictions the null hypothesis that the excluded instruments are valid instruments, i.e., uncorrelated with the error term and correctly excluded from the estimated equation obtained via Generalized Two-Stage Least Squares estimation.*

TABLE 2.
The effect of US Aid and Multilateral Aid on the political regime

Variables	**(1a)**	**(1b)**	**(2b)**	**(2b)**
US AID		**-0.95** **(1.88)**		
Multilateral AID				**-0.82**** **(0.36)**
Initial GDP	-0.44*** (0.12)		-1.15*** (0.19)	
Initial Population	-0.05 (0.07)		-1.32*** (0.13)	
Sub-Saharan Africa	0.18** (0.08)		1.72*** (0.26)	
Egypt	1.76*** (0.38)		0.08 (0.37)	
Central America	0.46** (0.20)		-1.12*** (0.21)	
Franc Zone	-0.24** (0.11)		-1.08*** (0.31)	
Literacy Rate	-0.01*** (0.00)	-0.04 0.03)	-0.05*** (0.00)	-0.09*** (0.03)
Trade Openness	0.01*** (0.00)	0.01 (0.02)	0.00 (0.00)	0.00 (0.01)
Military Expenditures	0.13*** (0.03)	0.05 (0.34)	-0.01 (0.02)	-0.06 (0.23)
Oil Exporting activity	-0.78*** (0.14)	-1.27 (2.61)	-0.59*** (0.15)	-2.75 (2.02)
Income inequality	-0.01 (0.01)	-0.15*** (0.06)	0.05*** (0.01)	-0.09 (0.06)
Muslim Dummy	-0.26*** (0.07)	-3.72*** (1.37)	-0.15 (0.15)	-4.56*** (1.34)
Catholic Dummy	0.29** (0.11)	2.13 (1.49)	1.18*** (0.19)	2.38* (1.28)
Ethnolinguistic Fractionalization	-0.18* (0.11)	1.12 (1.41)	0.18 (0.15)	0.59 (1.29)
Latitude	-0.02 (0.07)	0.46 (0.66)	0.16* (0.09)	0.12 (0.61)
Post-Cold War Period	-0.06 (0.14)	1.24*** (0.39)	0.60*** (0.22)	1.57*** (0.43)
Initial Political Regime	0.05 (0.08)	7.13*** (1.39)	-0.63*** (0.10)	6.28*** (1.39)
Predicted residuals		1.14 (1.89)		0.84** (0.37)
No of countries (observations)	61 (1585)		64 (1809)	
F-statistic of excluded instruments	14.96		60.77	
Sargan-Hansen X^2 statistic (Prob)		10.47 (0.06)		4.30 (0.51)
Second-Stage X^2 (Prob)		210.84 (0.00)		225.15 (0.00)

TABLE 2. (continued)

Rho - LR test (*Prob*)		0.80 (0.00)		0.79 (0.00)
R^2 / *Pseudo-*R^2	0.19	0.33	0.44	0.40
% Correctly Predicted		81%		83%
Average Foreign Aid		0.73		2.00
Predicted Probability $\hat{P}(Y_{it}=1)$		0.29		0.24
Percentage Change in $\hat{P}(Y_{it}=1)$		-10.34%		-40.42%
Marginal Effect of average Foreign Aid		0.030 (0.040)		0.097*** (0.010)

Notes:

1. *For each pair of regressions column (a) reports first-stage OLS estimates of foreign aid using White's heteroskedasticity-consistent covariance matrix, see equation (1) in the text. Column (b) reports second-stage logit coefficients obtained from Maximum Likelihood estimation of equation (2), which models the probability that country i is democratic in year t. Estimation assumes random effects.*
2. *In columns (1a)-(1b) US AID denotes Net Official Development Assistance provided by the United States, whereas in columns (2a)-(2b) Multilateral AID denotes Net Official Development Assistance provided by multilateral agencies, both as a percentage of the recipient's GDP (for a listing of the agencies see the Data Appendix). Estimations correspond to the period 1967-2002. See also Table 1.*

TABLE 3. The effects of a unitary change in aid as % of GDP on the probability of observing a democratic regime: Differential recipients and regimes

	(1)	(2)	(3)	(4)
	Environment	**Predicted Probability** $\hat{P}(Y_{it}=1)$	**Marginal effect of *AID***	**% change in** $\hat{P}(Y_{it}=1)$
1. Benchmark (variables at means, 1990-2002)	Average political, economic and social conditions	0.30	-0.053*** (0.007)	-17.2%
1.1. "Unfavorable" environment, 1990-2002	Muslim, oil exporters, high ethnolinguistic fractionalization, initially non-democratic	0.05	-0.012*** (0.003)	-23.5%
1.2. Intermediate environment, 1990-2002	Catholic, non-oil exporters, low ethnolinguistic fractionalization, initially non-democratic	0.44	-0.061*** (0.008)	-13.8%
1.3."Favorable" environment, 1990-2002	Catholic, non-oil exporters, low ethnolinguistic fractionalization, initially democratic	0.81	-0.038*** (0.008)	-4.7%
2.1. Non-liberalized economies, 1967-1999	Non-liberalized at *t*-5	0.22	-0.085*** (0.018)	-38.1%
2.2. Liberalized economies, 1967-1999	Liberalized at *t*-5	0.09	-0.013*** (0.004)	-13.3%

Note:
The estimates for groups 1, 1.1, 1.2, and 1.3 are based on the outliers-free regression (2b) of Table 1. Standard errors are in parentheses.

TABLE 4. The effect of aid as % of GDP (*AID*) on the political regime: Economic liberalization

Variables	**(1)**	**(2)**	**(3)**
AID	**-0.49*** (0.10)**	**-0.26*** (0.06)**	**-0.12*** (0.03)**
Literacy Rate	0.00 (0.01)	-0.02** (0.01)	-0.01 (0.01)
Military Expenditures	-0.06 (0.16)	0.23** (0.10)	0.02 (0.03)
Oil Exporting activity	-1.62* (0.89)	-1.34*** (0.45)	-2.16*** (0.43)
Income inequality	0.11** (0.05)	0.13*** (0.04)	-0.06*** (0.02)
Muslim Dummy	-0.98 (0.63)	-0.70* (0.41)	-2.80*** (0.39)
Catholic Dummy	3.38*** (0.49)	1.29*** (0.43)	1.01*** (0.27)
Ethnolinguistic Fractionalization	3.33*** (0.45)	-0.95*** (0.36)	0.78*** (0.25)
Latitude	3.70 (2.29)	-3.92** (1.99)	4.53*** (1.12)
Initial Political Regime	1.62*** (0.39)	2.09*** (0.35)	2.36*** (0.40)
Post-Cold War Period	4.71*** (0.54)	4.14*** (0.51)	2.15*** (0.23)
Economic Liberalization (*t*-5)	-2.94*** (0.67)		
AID Economic Liberalizations* (*t*-5)	0.34*** (0.09)		
Predicted residuals	0.56*** (0.11)	0.34*** (0.07)	0.13*** (0.03)
No of countries (observations)	44 (1081)	35 (682)	56 (889)
F-statistic of excluded instruments	64.5	40.10	40.10
Second-Stage X^2 (*Prob*)	182.59 (0.00)	126.74 (0.00)	226.00 (0.00)
Rho - LR test (*Prob*)	0.69 (0.00)	-	-
R^2 */ Pseudo-*R^2	0.28	0.30	0.47
% Correctly Predicted	74.00%	81.53%	88.42%
Average AID	5.50	5.45	5.45
Predicted Probability $\hat{P}(Y_{it}=1)$	0.15	0.19	0.22
Percentage Change in $\hat{P}(Y_{it}=1)$	-32.47%	-20.81%	-9.43%
Marginal Effect of average AID	-0.050*** (0.007)	-0.039*** (0.009)	-0.021*** (0.005)

Notes: ***Estimates correspond to the period 1967-1999 following data availability of the economic liberalization variable****. Columns (1)-(3) are second-stage logit estimates. Specifications (2) and (3) correspond to the subset of observations for non-liberalized and liberalized economies, respectively. The chi-squared statistic of the Wald test for the null hypothesis on the equality of AID coefficients between specifications (2) and (3) equals 4.22 (critical value: 3.84).*

When Does More Aid Imply Less Democracy?

An Empirical Examination

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TECHNICAL APPENDIX

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Appendix: The statistical model for the democracy-aid nexus

In the Appendix we develop the econometric framework used to assess the impact of aid flows on the political regime. We adopt a binary response setup that can capture the non-linear pattern of the political developments and we follow Petrin and Train (2003) to incorporate the hypothesis that aid flows are determined endogenously.[1] Our setup also incorporates random effects to account for potential country-specific unobserved features. In particular, assume that the political regime is described by a binary variable, $Y_{it,}$ which takes the values 0 or 1 if country i is autocratic or democratic respectively at time t. We then assume that these values are determined by an unobservable latent variable, Y_{it}^{*}, that depends on various country-specific and time-specific characteristics, including foreign assistance, through the following relationship:

$$Y_{it}^{*} = \alpha_i + \beta\, AID_{it} + \sum_k \gamma_k \boldsymbol{X}_{it} + v_{it} \qquad \text{(A.1)}$$

where AID_{it} denotes the measure of aid utilized as received by country i at time t, $\boldsymbol{X}_{it}$ includes a set of k observable characteristics of country i and $v_{it} \sim N(0, \sigma_v^2)$ is an i.i.d. disturbance term. Assuming that the time-invariant term, α_i, can be split into a constant part, α, and a random, country-specific part, μ_i, with $\mu_i \sim N(0, \sigma_\mu^2)$, so that $\alpha_i = \alpha + \mu_i$, then (A.1) can be written as:

$$Y_{it}^{*} = \alpha + \beta\, AID_{it} + \sum_k \gamma_k \boldsymbol{X}_{it} + \varepsilon_{it} \qquad \text{(A.2)}$$

where $\varepsilon_{it} = \mu_i + v_{it}$ with $\varepsilon_{it} \sim N(0, \sigma_\mu^2 + \sigma_v^2)$ and the random country-specific part, μ_i, is uncorrelated with the explanatory variables, i.e. $Corr(\mu_i, \boldsymbol{X}_{it}) = Corr(\mu_i, AID_{it}) = 0$ for all t. Now, if foreign assistance that country i receives at time t, AID_{it}, is affected by unobservable or omitted factors captured by the idiosyncratic effects, ε_{it}, then AID_{it} will be correlated with the error term. To account for the potential impact of aid endogeneity, we use here an Instrumental Variables methodology by following the control function approach suggested by Petrin and Train (2003). This approach decomposes the endogenous

[1] Petrin A. and K. Train (2003): 'Omitted product attributes in discrete choice models', *NBER Working Paper 9452*.

regressor, namely AID_{it}, as a function of a set of exogenous instruments, $g(w_{it})$ where w_{it} is the instrument set, and omitted characteristics, u_{it}, as follows:

$$AID_{it} = g(w_{it}) + u_{it} \tag{A.3}$$

In our case $Corr(AID_{it}, \varepsilon_{it}) \neq 0 \Rightarrow Corr(u_{it}, \varepsilon_{it}) \neq 0$ since $Corr(w_{it}, \varepsilon_{it}) = 0$, thus implying that the disturbance terms in the equations of aid and regime are correlated. In fact, the direction of the correlation is not obvious. The error terms will be negatively correlated if donors responded to negative democratization shocks by providing more assistance. On the opposite case, countries making progress towards democratization may receive favorable treatment from donors, thus triggering a positive correlation between u_{it} and ε_{it}. Given that $Corr(u_{it}, \varepsilon_{it}) \neq 0$, ε_{it} can be decomposed into a mean conditional on u_{it} given by $f(u_{it})$ called the 'control function' since it controls for the part of the original error term, ε_{it}, that is correlated with foreign aid, AID_{it}, and a deviation from the mean, ξ_{it}, which is orthogonal to AID_{it}. Following Petrin and Train (2003) and Villas-Boas and Winer (1999), we will assume that the control function is linear in the residuals of the form $f(u_{it}) = \lambda u_{it}$.[2] Thus, we can then rewrite the regime function as:

$$Y_{it}^{*} = \alpha + \beta\, AID_{it} + \sum_{k} \gamma_k \boldsymbol{X}_{it} + \lambda u_{it} + \xi_{it} \tag{A.4}$$

where $\xi_{it} \sim N(0, \sigma_{\xi}^{2})$ and i.i.d. The conditional probability of a country i being democratic can then be written as:

$$\Pr(Y_{it} = 1 \mid AID_{it}, \boldsymbol{X}_{it}, u_{it}) = G\left(\alpha + \beta AID_{it} + \sum_{k} \gamma_{\kappa} \boldsymbol{X}_{it} + \lambda u_{it} \right) \tag{A.5}$$

where G is the logistic function taking values between zero and one. The control function approach adopted here requires a two-stage estimation process. In the first stage, foreign aid is regressed on a set of instruments and the exogenous explanatory variables of the regime equation, $\boldsymbol{X}_{it}$, whereas second-stage estimation involves a random-effects logit of Y_{it} on AID_{it}, the predicted residuals of the first stage, $\hat{u}_{it}$, and $\boldsymbol{X}_{it}$. This two-stage Maximum Likelihood procedure yields consistent and efficient estimates

[2] Villas-Boas J. and R. Winer (1999): 'Endogeneity in brand choice models', *Management Science*, 45, 1324–1338.

compared to other estimation techniques (Maddala, 1983, p. 122-123).[3] The random effects logit sacrifices less degrees of freedom as it requires substantially fewer parameters to be estimated than a typical fixed-effects logit. Thus, it is more efficient when the number of cross sections exceeds the number of time units as in our case. Fixed-effects estimation also has the practical effect of precluding investigation of explanatory variables that change very slowly or not at all. This constraint leaves out democracy determinants such as geographic position, ethnic or religious identity or colonial origin. On the contrary, random effects estimation controls for individual time-series and cross-sectional error components in the panel and it is strongly recommended in the context of the present empirical analysis, where several time-invariant democracy factors should be taken into account. These factors also capture a great proportion of the country-specific variation of democracy rates, thus mitigating the usefulness of a fixed-effects estimation scheme. We can then test for the endogeneity of AID_{it} by applying a Wald test on λ. Rivers and Vuong (1988) have shown that the two-step estimator is consistent and that the usual t statistic on the residuals is a valid test of the null hypothesis that the independent variable of interest, AID_{it}, is exogenous.[4]

[3] Maddala G.S. (1983): *Limited-Dependent and Qualitative Variables in Econometrics*, Cambridge: Cambridge University Press.
[4] Rivers D. and Q.H. Vuong (1988): "Limited Information Estimators and Exogeneity Tests for Simultaneous Probit Models", *Journal of Econometrics* 39(3), 347-66.

Table A1. Summary Statistics (Time Period: 1967-2002)

	Dataset 1: Full Sample								Dataset 1.1. Initially Non-democratic countries							
	a. No restriction (Dataset 1.a)				b. Outliers-free Sample (Dataset 1.b)				No restriction				c. Outliers-free Sample (Dataset 1.1.c)			
Continuous Variables	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
AID (%)	5.9	8.3	-0.5	95.0	5.5	6.5	-0.5	35.4	7.0	9.0	-0.5	95.0	6.8	7.5	-0.5	44.0
US AID (%)	0.7	1.4	-0.2	25.7	0.5	0.8	-0.2	7.1	0.8	1.5	-0.2	25.7	0.6	0.8	-0.2	7.1
Multilateral AID (%)	2.0	3.4	-0.6	45.7	1.9	2.9	-0.6	22.2	2.4	3.6	-0.6	45.7	2.3	3.1	-0.6	22.2
Trade Openness (%)	65.6	39.0	6.3	251.1	58.5	33.0	6.3	198.8	65.9	36.6	6.3	251.1	60.5	32.2	6.3	198.8
Literacy Rate (%)	75.1	22.3	11.7	99.8	74.1	23.0	11.7	99.8	72.0	22.7	11.7	99.8	71.5	23.4	11.7	99.8
Income inequality (%)	46.5	4.7	31.4	59.1	46.5	4.1	34.7	58.9	47.0	4.5	33.2	59.1	46.7	4.3	33.2	58.9
Military Expenditures (%)	3.3	3.6	0.0	20.3	2.3	1.5	0.0	7.0	3.4	3.8	0.6	20.3	2.4	1.7	0.6	7.0
Latitude (0,1)	0.2	0.1	0.0	0.4	0.2	0.1	0.0	0.4	0.2	0.1	0.0	0.4	0.2	0.1	0.0	0.5
Initial Population (log)	6.7	0.7	5.1	8.7	6.8	0.7	5.1	8.7	6.7	0.7	5.3	8.0	6.8	0.6	5.4	8.0
Initial GDP (log) $ per capita	2.9	0.5	2.0	4.7	2.8	0.5	2.0	3.8	2.8	0.5	2.0	4.7	2.7	0.4	2.0	3.8

Dummy variables (=1)	***Obs***	***%***	***Obs***	***%***	***Obs***	***%***	***Obs***	***%***
Regime (Dep. Variable)	629	34.38	536	34.93	337	23.15	314	24.6
Oil Exporting activity	229	12.50	107	7.02	195	13.42	74	5.87
Muslim Dummy	571	31.22	415	27.06	515	35.34	396	31.0
Catholic Dummy	630	34.44	534	34.80	412	28.28	350	27.4
Ethnolinguistic Fractional.	1170	60.58	1058	68.90	921	63.15	882	69.1
Sub-Sahara	367	20.08	332	21.66	354	24.31	320	25.1
Central America	204	11.18	172	11.25	118	8.15	86	6.81
Franc Zone	195	10.69	195	12.75	195	13.42	195	15.3
Egypt	27	1.47	0	0.00	26	1.84	0	0.00
Initial Regime	371	20.30	308	20.10	1460	100.0	1277	100.
No of countries (N)	**64**		**57**		**51**		**47**	
No of observations (Obs)	**1832**		**1537**		**1460**		**1277**	

TABLE A2. The impact of aid as % of GDP (*AID*) on the political regime (1967-2002): Robustness tests (outliers-free estimations)

	Initially non-democratic		Time-trend added		Constraint on the executives added		British Colonization dummy added		Polity 2 index	
Variables	**(1a)**	**(1b)**	**(2a)**	**(2b)**	**(3a)**	**(3b)**	**(4a)**	**(4b)**	**(5a)**	**(5b)**
AID		-0.35**		-0.35***		-0.53***		-0.27*		-0.30***
		(0.17)		(0.15)		(0.19)		(0.15)		(0.11)
Initial GDP	-3.93***		-3.25***		-2.31***		-3.28***		-3.58***	
	(0.57)		(0.43)		(0.46)		(0.50)		(0.46)	
Initial Population	-6.46***		-4.05***		-3.52***		-5.06***		-3.47***	
	(0.47)		(0.26)		(0.25)		(0.43)		(0.22)	
Sub-Saharan Africa	3.24***		3.80***		4.11***		3.15***		4.29***	
	(0.54)		(0.47)		(0.50)		(0.57)		(0.46)	
Egypt	Dropped		Dropped		Dropped		Dropped		Dropped	
Central America	-4.85***		-3.32***		-2.78***		-3.94***		-3.10***	
	(0.60)		(0.42)		(0.43)		(0.70)		(0.41)	
Franc Zone	-3.03***		-0.62		-0.68		-1.99***		-0.02	
	(0.73)		(0.54)		(0.57)		(0.70)		(0.55)	
Literacy Rate	-0.10***	-0.13***	-0.08***	-0.07***	-0.09***	-0.09***	-0.08***	-0.05*	-0.07***	0.00
	(0.01)	(0.04)	(0.01)	(0.01)	(0.01)	(0.03)	(0.01)	(0.03)	(0.01)	(0.02)
Trade Openness	-0.01	-0.01	0.00	0.00	0.01	0.00	-0.01	0.00	0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Military Expenditures	-0.66***	-0.58	-0.36***	-0.48	-0.22*	-0.15	-0.49***	-0.55	-0.25***	-0.13
	(0.12)	(0.43)	(0.10)	(0.35)	(0.12)	(0.38)	(0.11)	(0.34)	(0.10)	(0.25)
Oil Exporting activity	-1.20***	-4.39	0.16	-1.83	-0.30	-2.02	1.63***	-1.85	-0.38***	-0.37
	(0.32)	(3.07)	(0.32)	(1.95)	(0.32)	(1.97)	(0.42)	(2.26)	(0.12)	(0.45)
Income inequality	0.19***	-0.01	0.26***	-0.01	0.32***	0.09	0.28***	-0.01	0.29***	0.08
	(0.05)	(0.08)	(0.04)	(0.07)	(0.04)	(0.09)	(0.44)	(0.07)	(0.04)	(0.06)
Muslim Dummy	3.02***	-2.84*	2.07***	-2.70**	1.66***	-3.29***	1.80***	-1.53	0.89**	-1.56***
	(0.43)	(1.67)	(0.32)	(1.24)	(0.36)	(1.30)	(0.44)	(1.34)	(0.15)	(0.40)
Catholic Dummy	4.23***	6.16***	3.00***	2.29*	1.93***	1.68	2.49***	3.41**	1.29***	-0.74*
	(0.48)	(2.01)	(0.35)	(1.26)	(0.39)	(1.34)	(0.48)	(1.45)	(0.16)	(0.41)
Ethnolinguistic Fractionalization	3.42***	3.93**	1.58***	1.06	1.81***	1.00	1.85***	1.49	0.91***	-0.18
	(0.47)	(1.81)	(0.35)	(1.30)	(0.38)	(1.30)	(0.38)	(1.35)	(0.18)	(0.45)

TABLE A2. (continued)

Latitude	0.80*** (0.27)	1.73** (0.77)	0.43** (0.20)	0.50 (0.63)	0.32 (0.22)	0.17 (0.66)	0.34 (0.24)	0.74 (0.62)	0.83*** (0.22)	-0.24 (0.44)
Post-Cold War Period	0.84 (0.60)	1.43*** (0.51)			0.07 (0.47)	1.19* (0.66)	0.48 (0.48)	1.19*** (0.40)	1.31*** (0.28)	3.04*** (0.33)
Linear time trend			0.12*** (0.02)	0.29*** (0.03)						
Constraint on the Executive at Independence					-2.10*** (0.54)	0.10 (0.80)				
Prior colonization by the British							-1.81*** (0.64)	3.42* (2.04)		
Initial Political Regime			-1.25*** (0.20)	5.20*** (1.31)	-0.98*** (0.22)	4.57*** (1.34)	-1.55*** (0.25)	4.22*** (1.33)	-2.05*** (0.24)	3.35*** (0.78)
Predicted residuals		0.36** (0.17)		0.41*** (0.16)		0.60*** (0.20)		0.33** (0.15)		0.30*** (0.12)
No of countries (observations)	47 (1278)		57 (1534)		52 (1392)		53 (1453)		53 (1449)	
F-statistic of excluded instruments	94.32		126.22		92.28		104.31		127.44	
Sargan-Hansen X^2 statistic (Prob)		6.50 (0.17)		3.65 (0.45)		4.38 (0.36)		3.65 (0.46)		8.21 (0.08)
Second-Stage X^2 (Prob)		157.04 (0.00)		201.98 (0.00)		173.56 (0.00)		195.26 (0.00)		217.39 (0.00)
Rho - LR test (Prob)		0.83 (0.00)		0.76 (0.00)		0.44 (0.00)		0.74 (0.00)		0.56 (0.00)
R^2 / Pseudo-R^2	0.53	0.35	0.57	0.37	0.58	0.39	0.56	0.38	0.58	0.35
% Correctly Predicted		83.1%		82.27%		83.05%		81.07%		78.54%
Average AID		6.77		5.46		5.59		5.73		5.53
Predicted Probability $\hat{P}(Y_{it}=1)$		0.13		0.26		0.22		0.22		0.40
Percentage Change in $\hat{P}(Y_{it}=1)$		-13.8%		-18.46%		-26.81%		-17.73%		-14.25%
Marginal Effect of average AID		-0.018*** (0.003)		-0.048*** (0.005)		-0.059*** (0.006)		-0.039*** (0.005)		-0.057*** (0.006)

Notes:
Specifications (1a)-(1b) correspond to Dataset 1.1.c. Specifications (5a)-(5b) are Hadi-outliers-free estimations and correspond to Dataset 1.a minus: Bahrain, Belize, Egypt, Iran, Israel, Jordan, Kuwait, Malawi, Malta, Mauritania, Namibia, Oman, Saudi Arabia, and Turkey (*50 countries in total*).

TABLE A3. The impact of aid as % of GDP (*AID*) on the political regime: Barro's (1999) specification

Variables	**(1a)**	**(1b)**	**(2a)**	**(2b)**
AID		-0.51** (0.21)		-0.38* (0.22)
Sub-Saharan Africa	2.62*** (0.91)		2.11** (0.90)	
Egypt	Dropped		Dropped	
Central America	-1.06* (0.55)		-0.63 (0.55)	
Franc Zone	2.43** (1.09)		2.76** (0.90)	
5-year lag of dependent variable	-0.24 (0.39)	3.01*** (0.44)	-0.09 (0.44)	2.77*** (0.45)
10-year lag of dependent variable			-0.31 (0.48)	0.87* (0.46)
Log(GDP)	-6.63*** (0.95)	-4.37** (1.87)	-6.93*** (0.97)	-3.35* (1.99)
Years of primary schooling	-0.20 (0.22)	0.32* (0.18)	-0.21 (0.22)	0.24 (0.18)
Gap between male and female primary schooling	-1.02** (0.41)	-1.05*** (0.38)	-0.70 (0.46)	-0.83** (0.37)
Urbanization rate	0.03 (0.02)	0.02 (0.02)	0.04** (0.02)	0.02 (0.02)
Log(population)	-3.09*** (0.45)	-1.15 (0.73)	-2.85*** (0.45)	-0.67 (0.74)
Oil country dummy	0.07 (0.58)	0.23 (0.76)	-0.12 (0.58)	0.03 (0.78)
Predicted residuals		0.61*** (0.22)		0.47** (0.24)
No of countries (observations)	61(323)	61(323)	61(313)	61(313)
F-statistic of excluded instruments	21.40		18.94	
Sargan-Hansen X^2 statistic (Prob)		10.05 (0.07)		10.45 (0.06)
Second-Stage X^2 (Prob)		91.16 (0.00)		89.72 (0.00)
R^2 / Pseudo-R^2	0.59	0.33	0.59	0.33
% Correctly Predicted		84.52%		84.87%
Average AID		4.84		4.66
Predicted Probability $\hat{P}(Y_{it}=1)$		0.275		0.304
Percentage Change in $\hat{P}(Y_{it}=1)$		-36.3%		-26.32%
Marginal Effect of average AID		-0.101*** (0.038)		-0.080* (0.046)