

Aid and Regulation

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Abstract: In recent decades, one of the objectives of international development assistance has been to encourage developing country governments to reorient their economies from highly regulated and centrally controlled to deregulated and market-based. However, poor economic performance on its own might well necessitate such a shift. Does aid from donors accelerate this process by providing additional incentives and critical resources (finance and advice)? Or do donor funds slow the retreat of the state by lessening financial crises and indirectly promoting state control (e.g., through state-run development projects)? This paper contributes to the empirical analysis of this question by examining the link between aid flows and regulatory burden. Using an instrumental variables method on panel data from 71 aid receiving countries from 1970 to 1995, estimation results support the first position. Donor funds favor more heavily regulated economies and successfully promoted deregulation. This apparent example of successful conditionality points to the importance of a more disaggregate analysis of the interaction of aid and policy in developing countries.

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I. Introduction

In recent decades, one of the central objectives of international aid agencies has been to encourage developing country governments to reorient their economies from highly regulated and centrally controlled to deregulated and market-based. However, poor economic performance on its own might well necessitate such a shift. Does aid from donors accelerate this process by providing additional incentives and critical resources (finance and advice)? Or do donor funds slow the retreat of the state by lessening financial crises and indirectly promoting state control (e.g., through state-run development projects)? This paper contributes to the empirical analysis of this question through an econometric study of aid flows and regulatory burden.

The key issue in estimating this relationship is a potential endogeneity problem. While aid may result in an improved regulatory environment (if donors are successful in this objective), the allocation of aid itself may be conditional on regulation. As the questions above make clear, we do not know *a priori* if aid reduces or sustains regulation. Nor do we know if donors target highly regulated economies (consistent with traditional conditionality) or favor already deregulated ones (selectivity). Whichever direction these causal links take, estimation of the impact of regulation on aid allocation and of aid on the level of regulation must address endogeneity.

This paper uses instrumental variables methods to estimate separate aid and regulation equations using panel data from 71 aid receiving countries from 1970 to 1995. The measure of regulation is a component of the Fraser Institute's "Economic Freedom in the World" series. The estimated aid allocation equation reveals that, *ceteris paribus*, more aid flows to heavily regulated economies, perhaps reflecting the use of conditionality. The estimated regulation equation suggests such conditionality has been effective: aid consistently appears to lower

regulation. The apparent contradiction between these findings and recent harsh assessments of conditionality more broadly may be explained by omitted variable bias in the latter. In analyses that examine only a single, broad measure of policy, the response to one component may mask the response to another component. Overall, the results of this paper point to the importance of a more disaggregate analysis of the interaction of aid and policy in developing countries.

Section II reviews the relevant previous research, focusing on empirical studies of aid, regulation and related topics and identifying potential instruments for regulation and aid. Section III presents the data and model to be estimated. Section IV discusses results for different specifications, samples, and estimation techniques. Section V concludes.

II. Previous Research

The recent literature on foreign aid often questions whether aid achieves its stated goals. While donor agencies such as the World Bank and USAID advocate policy reform through structural adjustment packages, a substantial body of research finds these methods ineffective for a number of reasons. Donors appear reluctant to enforce conditions attached to loans and grants (e.g, Mosley et al., 1995). One explanation is the so-called Samaritan problem. For a sufficiently altruistic donor, giving aid is a dominant strategy. Given this knowledge, donor threats to make aid conditional on policy reform are not credible and the recipient government's reform decision is unaffected (Svensson, 2000B). A less sympathetic critique suggests aid may be self-serving rather altruistic. Bureaucratic budget mechanisms, "defensive lending," and promotion of donor economic and strategic interests may create an imperative to dispense aid (Fleck and Kilby, 2001; Mosley et al., 1995; Svensson, 2003; Tendler, 1975).

An even harsher challenge maintains that aid funds work against reform (e.g., Boockmann and Dreher, 2003). A country's current policies reflect the outcome of a political

process that balances competing interests. Radical policy change (e.g., economic reform) is unlikely until the status quo no longer is feasible because of currency or debt crisis. Foreign aid provides the state with additional resources—hard currency, new loans, debt relief—and potentially staves-off the day of reckoning. According to this line of reasoning, if donors do not hold fast to their reform conditions, aid will delay policy change and, as Casella and Eichengreen (1996) point out, this can make the net effect of aid negative. Even if donors do require some changes, these may be less extreme than the measures the recipient government would have been forced to adopt without the cushion of aid. Taking this argument one step further, a potential aid recipient may strategically avoid reform to attract aid. If more aid flows to countries where development is stalled, a government that benefits more from aid than from growth has an incentive to impede reform to attract more aid (Collier, 1997).

Arguments in favor of adjustment assistance focus on economic dynamics as well as political aspects of the reform process. Such arguments are the traditional justifications given by international institutions such as the World Bank and IMF. External funding may shield the country as a whole from negative short-run effects of policy reform, smooth the flow of resources from contracting sectors to expanding sectors (through training, transitional assistance, etc.) or serve as the up-front investment needed to privatize state-owned enterprises.

Macroeconomic stabilization provided by aid may be a prerequisite for the private investment (domestic and foreign) needed to fuel expansion in areas of comparative advantage, the route by which growth is restarted. Aid may provide technical assistance for appropriate deregulation. An alternative political economy perspective argues that distributing benefits (such as aid funds) may “buy support.” Such benefits can include a generous severance package to quell unrest among former public sector employees, compensation for powerful business elites now exposed

to competition, postponing tax compliance efforts, etc. (Alesina and Rodrik, 1994; Blanchard, 1996; Fleck, 2000; Rodrik, 1996). The key is that more resources in the control of committed reformers promotes reform. Because the country is in crisis, it does not have access to international credit markets and must rely on aid.

These debates are the launching point for the extensive literature on aid effectiveness. Research on aid effectiveness at the World Bank and elsewhere culminated in the publication of *Assessing Aid* (World Bank, 1998) and a subsequent article in the *American Economic Review* entitled “Aid, Policies and Growth” (Burnside and Dollar, 2000—henceforth BD). Earlier attempts to assess the impact of aid on economic growth were plagued by questions of the direction of causation since aid allocation itself may depend on growth. A potential solution to this problem came in Boone (1996). He notes that the extensive literature on aid allocation finds three motives for giving aid: humanitarian, commercial, and geopolitical. Since aid allocated for humanitarian reasons may depend on growth, Boone constructs instruments using variables reflecting only the geopolitical and commercial motives. Using this approach, Boone finds aid ineffective. BD take the analysis one step further, exploring the interaction of aid and policy.¹ They find that aid is effective in “good policy” environments but ineffective otherwise. BD also report that: 1) aid has generally not been targeted at “good policy” countries; 2) aid does not change policy (using an aggregate policy index); and 3) aid is associated with increased government consumption due to fungibility. *Assessing Aid* as well as several other articles from the same World Bank research group (e.g., Collier, 1997; Collier and Dollar, 2001) advocate a policy of aid selectivity, i.e., *ex post* conditionality whereby substantial aid is given to a country

¹Ironically, BD find no significant endogeneity problem—aid allocation does not depend on growth. This suggests that the earlier literature on aid effectiveness may be worth revisiting.

only after a number of years of reform. This precludes the possibility of feigned reforms and reversals and ostensibly has incentive effects for other countries since the rewards of reform are greater. This selective approach to aid distribution has spread rapidly with increasing use of performance-based allocation at the World Bank and other donor organizations (Clemens et al., 2004, 8). President Bush's initiative to increase U.S. foreign aid by 50% through the Millennium Challenge Account (MCA) is the most direct implementation of this concept.²

Not surprisingly, this attempt to justify reducing the role of need in aid allocation has sparked a strong response in the academic community. While researchers have taken a variety of approaches (see, for example, the August 2001 issue of *The Journal of Development Studies*), most compelling are empirical critiques which demonstrate that the BD results are not robust across different specifications, time periods or country samples (Clemens et al., 2004; Dalgaard et al., 2004; Easterly, 2003; Easterly et al., 2004; Hansen and Tarp, 2000, 2001). The central theme of this research is that the empirical foundations of aid selectivity are far too fragile to justify such a major policy shift.

The question of whether aid facilitates specific types of reform is beginning to be explored empirically.³ Remmer (2004) looks at government size as measured by the ratio of

²At least on paper. The administrative structure of the implementing agency (the Millennium Challenge Corporation) holds out the possibility that the MCA will divert resources and control from USAID to the State Department and the Treasury. See Brainard et al. (2003) for a discussion of the agency structure and Lucas and Radelet (2004) for an analysis of the first year's selection process.

³As Banerjee and Rondinelli (2003, 1528) note: "Although the literature on the effects of foreign aid on economic growth and development is large and growing, analysis of its impacts on economic reforms in general, and on specific reform policies such as privatization, is still nascent."

government expenditures to GDP to see how it relates to aid. Particularly since the debt crisis of the 1980s and the rise of the “Washington Consensus,” reducing the size of government has been a frequent donor refrain (Williamson, 1990, 2003). The argument that aid may increase rather than decrease the size of government is much the same as presented above: donors only weakly demand fiscal discipline while they actually provide the government with the means of expansion. Taking a public choice perspective, Remmer states “the notion that augmenting the resources of a government from outside may reduce or have no systematic effect on government size is intrinsically implausible. The reason is that the political costs and benefits of expenditures financed by external resources and those funded by domestic taxation or revenue generation differ significantly” (Remmer, 2004, 80). The assumption that the latter are higher is equivalent to the argument that aid conditions have no teeth. Remmer also echoes the strategic non-reformer view, suggesting that aid dependent countries may expect to leverage fiscal deficits into more future aid. Remmer uses panel data (1970-1999 for 120 nations) on government spending as a share of GDP, tax effort and aid (alternatively as a share of GNI, government spending, and imports) to estimate error-correction models of government spending and tax effort.⁴ Estimation results show that aid is associated with increased public spending (apparently consumption not investment) and decreased tax effort. These results are consistent with BD and Devarajan et al. (2001).

Examining another stated goal of aid, Banerjee and Rondinelli (2003) estimate the impact of aid on the timing, pace and intensity of privatization in a group of 35 developing countries.⁵

⁴Controlling for openness, demographics, GDP per capita, debt service ratio, and government revenue as a share of GDP.

⁵Timing is when the first privatization takes place; pace is the number of privatizations in a given

Banerjee and Rondinelli also note the two roles that aid can play in privatization: donors can force aid dependent governments to undertake privatization but aid funds may also directly or indirectly subsidize hemorrhaging state-owned enterprises and thereby postpone privatization. The authors test for aid endogeneity (e.g., more aid allocated to countries that privatize) but fail to reject exogeneity. The estimation results are difficult to interpret for a number of reasons: 1) the negative binomial specification for pace requires the improbable assumption that each privatization is an independent event; 2) results vary widely across specifications and measures of aid⁶; and 3) pace and intensity are not measured relative to the size of the economy. The authors take several lessons away from this empirical exercise. In general, the role of aid in privatization is limited primarily to technical assistance supporting an ongoing process. For countries with “superior governance structures,” aid may play a more important part (Banerjee and Rondinelli, 2003, 1546). Aid, however, does not appear to have a systematic influence on the decision to begin the privatization process.

The impact of aid on corruption has also been studied using aggregate cross-country data.

Alesina and Weder (2002) find weak evidence of a “voracity effect” (Lane and Tornell, 1996)—increases in corruption contemporaneously associated with increases in aid even when including lagged values of each variable. In an attempt to deal better with endogeneity (corruption or its effects causing aid flows), Tavares (2003) instruments aid using measures of

year; and intensity is the value (in constant dollars) of privatizations in a year.

⁶The authors use the phrase “no systematic impact” to describe results that differ by specification and measure of aid but go on to discuss individual specifications or measures of aid that do have statistically significant results.

cultural and geographical proximity between donors and recipients. The results appear to show that aid reduces corruption since countries with high values of the instrumented aid are less corrupt according to the both the ICRG measure and the Business International index. While this two stage approach is undoubtedly the right method, it is not clear that the instruments selected are exogenous to corruption. The recent (and hotly debated) literature on colonization, institutions and growth links a similar set of indicators to institutional quality.⁷ Thus, Tavares' instrumented aid may proxy for institutional quality which itself is linked to lower levels of corruption.

As Svensson (2000A) points out, corruption and regulation are tightly linked. Regulation that restricts competition generates rents; competition for these rents results in corruption. Indeed, politicians and government officials may design regulations specifically to create rents and the ensuing corruption. Svensson's main focus is on corruption and aid but, because of this link between regulation and corruption, he estimates an equation for regulation and aid as a robustness check.⁸ To allow for aid endogeneity, Svensson uses a 2SLS method with log of population as the instrument for aid. The variables included in this regression are an index of ethnolinguistic fractionalization (*ethnic*), windfalls as measured by aid plus terms of trade shocks (*aid+tt*), primary product exports as a share of GDP (*sxp*), log of initial real per capita GDP

⁷See for example Acemoglu et al. (2001, 2002), Diamond (1997), Easterly and Levine (2003), Fleck and Hanssen (2003), Rodrik et al. (2002), Sachs (2003).

⁸Svensson uses the Fraser Institute's "Freedom from Government Regulation" (*fgr*) measure as the dependent variable in this estimation (Column 2b in Table 2). It appears that *fgr* is inverted to make it consistent with the other proxies for rent-seeking activities so that higher values indicate more regulation.

(*lgdp*), regional dummies and time dummies.⁹ The specification also includes interactions of *ethnic* with both *aid+tt* and *sxp*.¹⁰ Ethnolinguistic fractionalization is intended to capture the extent of divisions within a society which, in Svensson's model, lead to more regulation. Below I discuss the significant results.

lgdp enters negatively and significantly, indicating that higher income levels are linked to less regulation.¹¹ To gauge the effects of ethnic fractionalization and “windfalls” (aid plus terms of trade shocks), we need to look at both the direct effect and the interaction term. More regulation is linked to higher ethnic fractionalization only for large windfalls—about three times the mean value. On the other hand, regulation increases with aid for all values of ethnic fractionalization.¹² Thus, windfalls increase regulatory burden, more so in divided societies.

⁹*ethnic* reflects values in 1960. It “measures the probability that two randomly selected people in a country belong to different ethnolinguistic groups.” Terms of trade shocks (*tt*) are measured by “the average growth rate of dollar export prices times initial share of exports in GDP minus the average growth rate of import prices times initial share of imports to GDP,” a variable from the World Development Indicators (Svensson, 2000A, 458).

¹⁰The interaction term (*ethnic*(aid+tt)*) is instrumented with *ethnic* interacted with time and regional dummies (Svensson 452, fn 10).

¹¹Because Svensson uses initial income level at the start of the period, changes in regulation over the period are not driving this result. However, it may still be the case that regulation prior to the start of the period influences the initial GDP and subsequent regulation (because of persistence).

¹²As I interpret the results. Reported results for regression 2b contain 2 apparent errors—negative coefficient estimates with positive t-statistics (*(aid+tt)*ethnic* and *centam*). The inverting of *fgr* mentioned above might explain the sign error typos. Given the other estimates in adjacent equations, the signs of the standard errors and Svensson's discussion, it is clear the negative signs are simply typos.

One would expect to get similar results using aid separately.

Harms and Lutz (2003) examine how the link between aid and private foreign investment—FDI broadly defined to include portfolio investment—is influenced by the institutional environment (i.e., aspects of governance) in the recipient country. Their central result is that aid is linked to higher levels of private foreign investment in economies with a *high* regulatory burden. This appears to run counter to the central theme in BD and *Assessing Aid* that aid is only effective in “good policy” environments (e.g., low regulatory burden). Harms and Lutz’ measure of regulatory burden is taken from Kaufmann et al. (1999); a single measure is used across the decade examined on the assumption that the regulatory environment changes only slowly. Using the same instruments as BD, Harms and Lutz also cannot reject the hypothesis that aid is exogenous in their investment equation.

A number of interesting lessons and possibilities are raised by this literature. First, donors may consider the level of regulation in aid allocation. Likewise, aid may influence the level of regulation. As stated above, donors may use *ex post* conditionality (selectivity) giving aid preferentially to countries with “good policies,” in this case, low levels of regulation. Alternatively, donors may give more funds to highly regulated economies, either because the ill effects of excess regulation mean that those economies also need more aid or because donors hope to “buy reform” and reduce regulation through *ex ante* conditionality (traditional structural adjustment). Although empirical evidence of endogeneity is mixed, we must examine these issues.

Looking across the literature, a number of good instruments for regulation and aid present themselves. BD estimate an aid allocation equation (Table 8) which includes variables intended to reflect donors’ economic and strategic motives for aid allocation. Some of these

prove insignificant including membership in the Franc Zone (so called CFA countries) and arms imports as a share of overall imports.¹³ The first is intended to measure links to France while the second may reflect alliances with donors such as the United States and its close allies; both are expected to result in more aid but prove insignificant. However, even if these are unrelated to aid allocation, there is reason to suppose they could be related to the level of regulation. Particularly during the late 1980s and early 1990s, CFA countries experienced severe macroeconomic problems because of exchange rate misalignment. Governments in CFA countries may attempt to counteract resulting imbalances via regulation. A high value for arms imports as a share of total imports suggests a repressive regime, one likely associated with widespread corruption and regulation. The mechanism for this may follow Svensson's description of a highly divided society (following a similar story as ethnolinguistic fractionalization) or may simply be the result of the approach of a repressive regime.¹⁴ In short, BD show these are unrelated to aid allocation while Svensson's work suggests they (and *ethnic*) may be linked to kleptocracy and hence to corruption and regulatory burden.

Svensson also provides an instrument for aid, namely population. Although we measure aid as a share of GDP, research on aid allocation has long shown a bias against large countries. A number of explanations have been offered for this pattern. A minimum efficient scale for aid operations due to fixed administrative costs may result in very small countries getting

¹³Regarding arms imports, they state: "To capture strategic interests we also use a measure of arms imports relative to total imports lagged one period. This variable helps explain the allocation of aid to middle-income countries, but has only minor relevance in the low-income country data set" (BD, 26-7).

¹⁴Interestingly, these variables are included in BD's aid allocation equation because they view them as exogenous to growth.

disproportionate levels of aid. At the other end of the spectrum, donors significantly under fund large recipient countries, apparently so that their aid programs (and hence influence) reach a larger number of countries. In the case of international financial institutions, such a strategy limits the amount of leverage large recipient countries have over them.

III. Data and Model

I draw mainly on two existing data sets to examine the links between aid and regulation. The primary measure of regulation comes from the Fraser Institute and is an aggregate measure of “Freedom from Government Regulation” (**FGR**).¹⁵ The variable can range from 0 (low freedom from government regulation = high regulation) to 10 (high freedom from government regulation = low regulation) and is available for 1970, 1975, 1980, 1985, 1990, 1995, 2000, and 2001 though not all years are available for all countries. This is one of five component indices averaged together to generate the Fraser Institute’s “Economic Freedom of the World” index. **FGR** is itself an aggregate of fifteen sub-indices, five each related to credit market regulation, labor market regulation, and business regulation.¹⁶

Other variables come directly from the data set posted by Easterly, Levine, and Roodman (2003—henceforth ELR). These include the measure of aid (**AID**) used by ELR, an extension of the Effective Development Assistance measure (EDA) developed by Chang, Fernandez-Arias and Serven (1998) and used by BD. While the standard measure of aid (Official Development

¹⁵Gwartney et al. (2003). This is the same measure used by Svensson (2000A). In the Fraser Institute’s documentation, this rating is called “Regulation of Credit, Labor, and Business.”

¹⁶Other available measures of regulation (e.g., the Wall Street Journal/Heritage Foundation regulation variable and an index developed by Kaufmann et al. (2003)) provide only very short time series.

Assistance or ODA) lumps concessional loans together with grants if the loan's grant element exceeds 25%, EDA converts these loans to their grant equivalent and thus provides a better measure of long term resource flows.¹⁷ However, EDA and ODA are highly correlated and research results thus far do not depend on which measure is used (e.g., BD). Aid is measured as a percentage of GDP to reflect the relative importance of aid to the recipient government. I use the natural log of period averages.¹⁸

The remaining variables mirror those used by BD and ELR; again, the data come directly from ELR (2003). **POLICY** is an aggregate measure of policy constructed by BD and extended by ELR.¹⁹ Several regional and country dummy variables are included: Sub Saharan Africa (**SSA**), East Asia (**EASIA**), **EGYPT**, and Central America (**CENTAM**). I refer to these collectively as **REGIONS**. In addition, I include the log per capita GDP (in constant dollars) at

¹⁷The threshold for ODA is 35% in the case of mixed credits, e.g., where concessional finance funds are packaged with a commercial contract. While there is some debate about the relative merits of EDA in analyzing growth, it does seem a reasonable measure when considering issues of conditionality.

¹⁸Past research has explored using aid as a share of the government budget or other refinements. These alternate approaches by-and-large yield the same results as aid/GDP (e.g., Remmer, 2004). Typically, the distribution of residuals is closer to a normal distribution if we take logs. In the aid models reported below, we cannot reject the assumption of normality. For example, using specification Aid 6 (Table 2, column 3) yields the following p-values for various tests of normality: Skewness test: 0.214; Kurtosis test: 0.139; Joint Skewness/Kurtosis test: 0.153; Shapiro-Wilk W test: 0.511; Shapiro-Francia W' test: 0.248. Taking logs drops 13 observations with non-positive aid values, values that are difficult to interpret in any case.

¹⁹This does not directly include a measure of regulation though one of its sources is ICRG data; other variables from this database are used in constructing indices such as Kaufmann et al. (2003)

the start of each period (**GDP_CAP**) and the log of population (**POP**). Finally, three variables previously discussed are an indicator for CFA countries (**CFA**), imports of military equipment as a share of total imports lagged by one period (**ARMS_1**), and an index of ethnolinguistic fractionalization in 1960 (**ETHNIC**). All of these variables are drawn from ELR. All equations include unreported time dummies for each period except the first.

It may be appropriate to include **POLICY** along with **FGR** (or other measures of regulation) in an aid equation since the latter might proxy for the former. The country and regional dummy variables were proposed by BD to reflect donor economic and strategic interests. **GDP_CAP** may reflect recipient need and hence play an important role in aid allocation.²⁰ See above for discussion of the roles of **POP**, **CFA**, **ARMS_1**, and **ETHNIC**.

The basic specifications proposed above are:

$$\mathbf{AID}_{it} = \alpha_0 + \alpha_1 \mathbf{FGR}_{it} + \alpha_2 \mathbf{POLICY}_{it} + \alpha_3 \mathbf{GDP_CAP}_{it} + \alpha_4 \mathbf{POP}_{it} + \alpha_5 \mathbf{REGIONS}_i + \varepsilon_{it} \quad (1)$$

$$\mathbf{FGR}_{it} = \beta_0 + \beta_1 \mathbf{AID}_{it} + \beta_2 \mathbf{ARMS_1}_{it} + \beta_3 \mathbf{ETHNIC}_i + \beta_4 \mathbf{CFA}_i + \beta_5 \mathbf{GDP_CAP}_{it} + \beta_6 \mathbf{REGIONS}_i + \zeta_{it} \quad (2)$$

Endogeneity is an issue if α_1 and β_1 are non-zero. Note that, contrary to BD, I include **CFA** and **ARMS_1** not in aid equation (1) but in regulation equation (2). I have proposed different interpretations of these variables and capitalized on their insignificance in the aid equation (both here and in BD and ELR). Because of the potential for simultaneity, I include estimation of (1) and (2) using instrumental variables methods.²¹

²⁰The aid allocation literature is actually split on the interpretation of this variable with some authors claiming that it reflects need and others suggesting it may proxy for economic importance, e.g., as a market for donor exports.

²¹To confirm the role of **CFA**, I implement an overidentification test (e.g., Wooldridge 2003, 508). I use **ARMS_1** as an instrument for **FGR** in the aid equation (Aid 6, Table 2) and test for the

The expected signs of the estimated coefficients are clear in some cases but not in others. For both **FGR** and **POLICY**, we may observe positive, zero or negative signs in the aid equation.²² If donors target countries with highly regulated economies either because these countries “need more help” or because donors aspire to promote policy reform through traditional conditionality, then $\alpha_1 < 0$. Conversely, if selectivity dominates, $\alpha_1 > 0$. Finally, if donors either ignore regulation issues when allocating aid or pursue different strategies in different settings, we may find $\alpha_1 = 0$.²³ We can tell similar stories for **POLICY**. While these two variables are correlated ($\rho = .42$), they do measure different characteristics of recipient countries and it is possible that donors take different approaches for each. Finally, note that **POLICY** may be endogenous. BD reject the endogeneity of **POLICY** (and, indeed, this is the basis for the selectivity approach to aid allocation); I take the simplified approach of estimating the aid equation with and without **POLICY**.²⁴

Although there are competing interpretations, one might reasonably expect **GDP_CAP** to reflect recipient need in the aid equation and hence enter negatively as it does in BD. A bias

exogeneity of **CFA**. The auxiliary regression yields $nR^2 = 1.16$ as compared to $\chi^2_{1,.05} = 3.84$ (p-value = .28) and we cannot reject the null hypothesis that **CFA** is exogenous.

²²For both **FGR** and **POLICY**, higher values indicate “better” policy, i.e., less regulation in the case of **FGR** and a higher index of openness, lower inflation, and better fiscal policy in the case of **POLICY**.

²³This includes some donors pursuing conditionality while others pursue selectivity. The analysis in this paper looks at aggregate donor behavior; individual donors may follow a variety of strategies.

²⁴Note, however, that I test and reject the exogeneity of **POLICY** as an instrument for **AID** in the regulation equation.

against large countries would be reflected in a negative coefficient on **POP** (recall we are looking at aid/GDP, not the absolute amount of aid) as in BD and Alesina and Dollar (2000).

The various **REGIONS** might be expected to receive more aid as they are ostensibly of strategic and economic importance to major donors. That said, there are in fact many possible interpretations for what these dummy variables reflect.

Turning to the regulation equation, the above literature review suggests **AID** may enter positively, negatively or not at all. If conditionality succeeds or aid provides critical resources (technical assistance or transitional support), we expect $\beta_1 > 0$. If aid does not promote deregulation in aggregate (it fails or deregulation was never a serious objective of donors), $\beta_1 = 0$. Finally, $\beta_1 < 0$ if aid resources facilitate continued regulation or even actively promote the expansion of the state through development planning.

Svensson (2000A) presents an argument for **ETHNIC** entering negatively; by extension, **ARMS_1** and **CFA** should also enter negatively. Other interpretations of these variables are possible but also suggest a link with more rather than less regulation (i.e., lower values of **FGR**). **GDP_CAP** is likely to enter positively though the direction of causation is controversial. Much of the research on the indices produced by the Fraser Institute has focused on how these various aspects of “Economic Freedom” may promote growth and hence be linked indirectly to higher incomes. However, when considering income itself, it is also plausible that “Economic Freedom” is a normal good, the consumption of which rises with income (Hanson 2003). As in Svensson (2000A), **GDP_CAP** is the initial value at the start of the period so that it could not be influenced directly by current regulation (**FGR**).²⁵ However, because the level of regulation is

²⁵This statement is a bit too strong since expectations about regulation during the period could certainly influence income.

relatively persistent over time ($\rho(\mathbf{FGR}, \mathbf{FGR_1}) = .76$), I explore including **FGR_1** as an explanatory variable.²⁶ Finally, the various **REGIONS** variables may capture broadly different patterns of regulation across regions not already captured by the other explanatory variables.

The sample covers 71 countries over five year periods from 1970 to 1995 although not all periods are available for all countries. The total number of observations is 306, an average of 4 periods per country but going as low as 1 and as high as 6. When I include **POLICY** in the aid equation, the sample falls to 257 observations on 66 countries due to missing observations for **POLICY**. When I include the previous period's regulation (**FGR_1**) as an explanatory variable in the regulation equation, the sample falls to 236 observations on 70 countries. Finally, because the influence and identification of outliers has been a controversial issue (see ELR), I examine several methods to exclude outliers. With aid in log form, the method used by BD and ELR (Hadi, 1994) finds no potential outliers. DFITS, Cook's Distance, and Welsch Distance methods do find potential outliers; the group identified by DFITS contains the others.²⁷ Dropping these leaves 239 observations (66 countries) in the aid equation which includes **POLICY** and 222 observations (69 countries) in the regulation equation which includes **FGR_1**.

The appendix provides details on the data used. Table A1 lists countries and coverage periods. Basic descriptive statistics are in Table A2 and simple correlations in Table A3. All three tables cover the overall sample except where noted.

²⁶**FGR_1** is **FGR** lagged by one period (5 years). The short length of the time series (maximum 6 periods, median 4 periods) prevents a meaningful exploration of the time series characteristics of the data even with relatively strong assumptions across countries in the panel (e.g., Levin et al., 2002).

²⁷For a good summary of these methods, see Cook and Uchida (2003).

IV. Estimation Results

Table 1 presents OLS estimation results for two specifications of each equation. The first column (Aid 1) excludes **POLICY**. The second column (Aid 2) also excludes **POLICY** but uses the same, smaller sample as column 3. The third column (Aid 3) includes **POLICY**. The second set of columns follow a similar pattern for the regulation equation. Regulation 1 excludes **FGR_1**, Regulation 2 excludes **FGR_1** but uses the same, smaller sample as the next column. Regulation 3 includes **FGR_1**. This approach demonstrates which changes are due to specification and which to sample. I use this approach throughout because of the issues raised in ELR. I use heteroskedasticity-robust standard errors (allowing for country clusters) to construct the reported t-statistics.

[Table 1 about here]

Results of all three OLS aid allocation estimations are consistent. “Freedom from government regulation” (**FGR**) is insignificant at all conventional confidence levels.²⁸ **GDP_CAP** enters negatively and is significantly less than -1, in line with the “need” interpretation of GDP per capita.²⁹ **POP** is negative and significant, mirroring previous evidence

²⁸If we do not take the log of aid, **FGR** is negative and significant even in the OLS specifications. However, these specifications fail normality tests.

²⁹Recall that **AID**, **GDP_CAP**, and **POP** are all in logs. Define aid =level of aid to country, GDP =level of GDP, and pop =level of population. Then $AID = \ln(aid/GDP)$, $GDP_CAP = \ln(GDP/pop)$ and $POP = \ln(pop)$. Ignoring other variables in equation (1), $AID = \alpha_3 GDP_CAP + \alpha_4 POP$ can be rewritten as $\ln(aid) = (1 + \alpha_3)\ln(GDP) + (-\alpha_3 + \alpha_4)\ln(pop)$. Thus, the elasticity of aid with respect to $GDP = 1 + \alpha_3$ and,

of a bias against large countries. Where significant, the regional variables are positive as we would expect if they capture strategic significance as posited in BD. Reducing the sample from 306 observations in Aid 1 to 257 observations in Aid 2 results in only small changes in the estimated coefficients. Aid 3 introduces **POLICY** as an additional explanatory variable. Consistent with BD and Alesina and Dollar (2000), this variable is insignificant, suggesting that aid is not consistently allocated to “good policy” countries. The introduction of **POLICY** has a minimal impact on other coefficient estimates.

In the regulation equations, results for the central variables are again consistent across the three estimations in terms of sign and significance although the inclusion of the lagged dependent variable has a sizeable impact on magnitude. **AID** enters positively and significantly; *ceteris paribus*, more aid is associated with less regulation. This suggests that aid may promote deregulation either through conditionality or providing critical resources. **ARMS_1** enters negatively and significantly with a higher share of arms imports linked to more regulation. This result is consistent and strong across all specifications estimated (both reported and unreported).³⁰ **ETHNIC** enters positively but is insignificant, again a result which holds across all specifications. This is something of a puzzle given Svensson’s theoretical and empirical results.³¹ **CFA** enters negatively and significantly as expected: CFA countries face a

ceteris paribus, aid allocation follows recipient need if $\alpha_3 < -1$. One-sided tests of $H_0: \alpha_3 = -1$ v. $H_1: \alpha_3 < -1$ reject null hypothesis in favor of recipient need in all specifications run.

³⁰Note that the Fraser ratings have a separate category for trade restrictions so this is not simply driven by trade policies which restrict overall imports and thus raise the share of arms imports.

³¹The estimated coefficient remains positive if **ARMS_1** is omitted. The simple correlation between **FGR** and **ETHNIC** is negative in the regression sample ($\rho = -0.1335$, $n=306$).

significantly higher regulatory burden even compared to other Sub-Saharan African countries. **GDP_CAP** enters positively; higher levels of income are associated with significantly higher “Freedom from Regulation” scores. Finally, *ceteris paribus*, there is still regional variation in levels of regulation.

Regulation 2 differs from Regulation 1 in two respects. First, the sample is reduced by 70 observations to match that used in Regulation 3 (which includes the lagged dependent variable). Second, the reduced sample means no observations for the first time period so the number of period dummies drops from 5 to 4. As the Regulation 2 results show, the estimation is not sensitive to the reduced sample. Regulation 3 includes **FGR_1** to allow for persistence over time which the coefficient on **GDP_CAP** might otherwise reflect.³² As one would expect, introducing **FGR_1** substantially reduces the magnitudes of the key coefficient estimates, by 56% for **AID**, 61% for **ARMS_1**, 36% for **CFA**, and 62% for **GDP_CAP**. However, as noted above, the sign and significance of these variables does not change.

Table 2 addresses endogeneity issues via instrumental variables.³³ Because **FGR** enters the aid equation negatively and **AID** enters the regulation equation positively, there is the possibility of a bias toward zero in both coefficient estimates when ignoring endogeneity.

³²Svensson (2000A) includes the same measure of GDP per capita in his regulation equation without **FGR_1**. However, his specification is IV with random country effects (454).

³³Instruments for **FGR** in the aid equation are **ARMS_1**, **ETHNIC**, and **CFA**. An overidentification test (Wooldridge 2003, 508) for **ETHNIC** and **CFA** yields $nR^2=2.50$ as compared to $\chi^2_{2,.05}=5.99$ ($p=.29$) for Aid 4 and $nR^2=4.98$ ($p=.08$) for Aid 6. The only instrument used for **AID** in the regulation equation is **POP**; the overidentification test for **POLICY** rejects exogenous ($nR^2=19.17$, $\chi^2_{1,.05}=3.84$, $p=.00001$ for Regulation 4; $nR^2=16.96$, $p=.00004$ for Regulation 6).

Comparing estimates across tables 4 and 5 for comparable samples and specifications, the pattern is consistent with this. The estimated coefficients in the IV specifications are further from zero than those from OLS, in some cases by a considerable margin.

[Table 2 about here]

The estimated coefficient for instrumented **FGR** in Aid 4 (column 1) is substantially larger and now statistically significant. While OLS may conflate the impact of aid with donor allocation, the instrumental variables method allows us to identify where regulation is high for exogenous reasons. The negative, significant coefficient on instrumented **FGR** implies that countries where regulation is high for exogenous reasons receive more aid. The IV method has little impact on the other variables in the equation; **GDP_CAP** and **POP** enter negatively and significantly with coefficient estimates of approximately the same magnitude.

Turning to Aid 5 and 6 (columns 2 and 3), there are few changes. Reducing the sample to those countries and periods where policy data are available (Aid 5) has very little impact. Including **POLICY** (Aid 6) increases the impact of regulation on aid somewhat but has little impact on coefficients. The estimated coefficient for **POLICY** is substantially larger than without instrumenting (Aid 3) and now statistically significant. This implies that preferential aid allocation to countries with “good policy,” in contrast to the findings of BD as well as Alesina and Dollar (2000).

The instrumental variables estimates for the regulation equation (Regulation 4 through 6) are quite similar to the OLS estimates in Table 1. With or without instrumenting, **AID** is positive and significant in all three specifications though with a much smaller estimated

coefficient in the specification including the lagged dependent variable (Regulation 3 or 6).

Point estimates for the **AID** coefficient are somewhat larger when instrumented (from 28% to 65% larger) though naturally the standard errors are also larger. These results appear to be clear evidence that countries where aid is high for exogenous reasons have lower levels of regulation. Estimates for other variables are virtually unchanged by instrumenting.

The results presented thus far provide strong support for the hypothesis that donors target high regulation economies and that aid is effective in reducing regulation. Donor targeting is apparent once we control for the endogeneity of regulation (i.e., in IV estimations); aid's ability to reduce regulation stands whether we instrument or not. Furthermore, the results are robust to dropping a substantial fraction of the observations, 15% of the countries/years missing **POLICY** data in the Aid equation and 20% (the first time period) in the regulation equation.

However, a note of caution is appropriate. Past research using cross-country data has shown that results can be sensitive to sample (e.g., BD as compared to ELR). Although the method described by Hadi (1994)—seemingly the standard in cross-country aid regressions—does not identify any potential outliers, DFITS, Cook's Distance, and Welsch Distance methods do with the group identified by DFITS containing the others. The last columns of Table 2 (Aid 7 and Regulation 7) give results under the toughest standard, dropping points identified by DFITS. The most striking change is that the estimated coefficients for **FGR** in the aid equation and **AID** in the regulation equation shrink dramatically in absolute value and are no longer statistically significant. While this is a harsh test—selecting the most extreme of four methods and successively whittling the sample down from the original 306 observations to 239 for Aid and 222 for Regulation—it does indicate results that are somewhat sensitive to sample.³⁴

³⁴Starting with Aid 4 as the base, the sample without DFITS outliers is 286 and **FGR** is again

With this caveat noted, a quick scan of Tables 1 and 2 underscores that the results are relatively robust. For the period examined, the IV aid specifications point toward aid conditionality rather than selectivity as more aid flows to more heavily regulated economies. This is not surprising as support for selectivity is only now spreading in policy circles. Other results are more surprising. Once we account for the endogeneity of regulation, aid appears to reward “good policy”—even in Regulation 7 using our stringent method to exclude outliers. With the exception of Regulation 7, all regulation results find aid effective in reducing the level of regulation. Finally, the degree of ethnic fractionalization (**ETHNIC**) is positive but insignificant in every specification, unlike in Svensson (2000A).³⁵

V. Conclusion

As the debate over governance and aid unfolds, empirical research is moving from more general issues and measures of governance to the examination of specific attributes. The degree of government intervention in the economy via regulation is undoubtedly one of the most direct

insignificant. However, starting with Regulation 4, the sample without DFITS outliers is 292 but **AID** is significant. The coefficient estimate is in line with that in the larger sample (0.216) as is the t-statistic (2.65).

³⁵Estimation methods do not account for differences with Svensson (2000A). Re-estimating Regulation 4 and 6 from Table 2 with random effects and instruments yields results in line with those reported above including positive, insignificant coefficients on **ETHNIC** and positive, significant coefficients on **AID**. Svensson’s negative link between regulation and ethnolinguistic fractionalization is captured by the interaction of *ethnic* with *aid+tt* when the latter is very high, an issue not explored in this paper. However, Svensson finds that regulation increases with aid for all values of *ethnic*, in contrast to results reported here.

and important aspects of governance as it pertains to economic performance (World Bank, 2004). Deregulation is a central component of the policy reforms advocated and supported by the major international development agencies. A better understanding of the multiple links between aid and regulation is critical to the successful implementation of such reforms.

There are many reasons to think the links between aid and regulation are complex and bidirectional. Although there is no shortage of theories in the literature about the links between aid, regulation, corruption, and policy reform more generally, sorting out the actual relationship is fundamentally an empirical question. This paper attacks the problem using cross-country panel data on aid and regulation for 71 countries from 1970 to 1995.

The results presented in this paper provide strong evidence that donors allocate more aid to countries with high regulatory burden and that aid reduces the level of regulation. This finding of apparently effective conditionality contrasts with much of the research on structural adjustment in general which finds lax enforcement of conditions and little improvement in recipient behavior (e.g., Mosley et al., 1995). It may be that the leverage of conditionality on aid and the resources to provide support for deregulation have been more effective than for other aspects of structural adjustment.

Another related result is surprising in light of past research. While an index of policy is not a significant determinant of aid allocation in BD or Alesina and Dollar (2000), the same index is robust, positive, and significant in the aid allocation equation in this paper once we instrument for regulation. This works in the opposite direction from regulation—more “freedom from government regulation” means less aid but better policies mean more aid—although the two variables are closely connected ($\rho(\mathbf{FGR}, \mathbf{POLICY}) = .40$). If we estimate the aid equation without regulation, the results conform to BD—policy is not a significant factor in aid allocation.

This suggests an interesting possibility. The typical “non result” may be driven by incorrectly aggregating two aspects of policy. If regulation conditionality works, donors should use it and hence allocate more funds to highly regulated countries. If other policy conditionality does not work, donors should be selective and allocate more funds to countries that have already improved these aspects of policy. Putting the two elements together, we get aggregate aid allocation that does not appear responsive to policy and aggregate policy which does not appear very responsive to aid.

At a minimum, this discussion underscores the broader importance of understanding the links between aid and regulation. The intellectual case for more intensive selectivity—the current direction of World Bank lending and the core of President Bush’s \$5 billion MCA initiative—fundamentally is built on claims that aid has not been responsive to policy and policy has not been responsive to aid. Clearly, a more disaggregate analysis is appropriate.

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Table 1: OLS Estimation

<i>AID</i>	Aid 1		Aid 2		Aid 3	
	Coef	t-stat	Coef	t-stat	Coef	t-stat
FGR	-0.051	-0.32	-0.001	-0.00	-0.057	-0.33
POLICY					0.168	1.66
GDP_CAP	-1.990	-8.82 **	-2.065	-8.78 **	-2.081	-8.79 **
POP	-0.743	-7.08 **	-0.747	-6.11 **	-0.719	-5.85 **
SSA	-0.575	-1.49	-0.623	-1.49	-0.574	-1.37
EASIA	0.311	1.15	0.348	1.21	0.139	0.41
EGYPT	1.882	7.78 **	1.919	7.66 **	1.847	6.93 **
CENTAM	-0.079	-0.24	-0.049	-0.13	0.023	0.06
<i>R-squared</i>	0.722		0.715		0.720	
<i>N</i>	306		257		257	
<i>Period Dummies</i>	5		5		5	
<i>Method</i>	OLS		OLS		OLS	

<i>FGR</i>	Regulation 1		Regulation 2		Regulation 3	
	Coef	t-stat	Coef	t-stat	Coef	t-stat
AID	0.156	2.79 **	0.147	2.54 **	0.064	2.42 **
FGR_1					0.654	9.99 **
ARMS_1	-4.777	-6.42 **	-4.680	-6.26 **	-1.836	-4.22 **
ETHNIC	0.203	0.60	0.141	0.42	0.042	0.33
CFA	-0.396	-1.80 *	-0.490	-2.28 **	-0.312	-4.15 **
GDP_CAP	0.626	4.76 **	0.630	4.85 **	0.241	3.77 **
SSA	0.124	0.51	0.136	0.57	-0.041	-0.48
EASIA	0.133	0.44	0.170	0.58	0.179	2.12 **
EGYPT	-0.033	-0.17	-0.095	-0.49	0.156	1.88 *
CENTAM	0.638	2.67 **	0.717	3.27 **	0.252	1.71 *
<i>R-squared</i>	0.374		0.436		0.714	
<i>N</i>	306		236		236	
<i>Period Dummies</i>	5		4		4	
<i>Method</i>	OLS		OLS		OLS	

Robust t-statistics using country clusters.

* 10% significance level

** 5% significance level

Table 2: Instrumental Variables Estimation

<i>AID</i>	Aid 4		Aid 5		Aid 6		Aid 7	
	Coef	t-stat	Coef	t-stat	Coef	t-stat	Coef	t-stat
FGR	-0.798	-3.13 **	-0.748	-2.55 **	-0.876	-2.85 **	-0.131	-0.49
POLICY					0.447	2.56 **	0.215	1.72 *
GDP_CAP	-1.741	-6.98 **	-1.829	-6.75 **	-1.880	-6.66 **	-1.987	-11.57 **
POP	-0.861	-5.47 **	-0.879	-4.75 **	-0.800	-4.73 **	-0.622	-6.74 **
SSA	-0.500	-1.06	-0.511	-1.01	-0.384	-0.76	-0.340	-1.26
EASIA	0.678	1.81 *	0.699	1.78 *	0.135	0.27	0.033	0.13
EGYPT	1.901	5.60 **	1.892	5.28 **	1.702	4.39 **	1.707	7.80 **
CENTAM	0.405	1.27	0.260	0.75	0.442	1.36	0.225	0.97
<i>R-squared</i>	0.603		0.601		0.599		0.813	
<i>N</i>	306		257		257		239	
<i>Period Dummies</i>	5		5		5		5	
<i>Method</i>	IV		IV		IV		IV	

<i>FGR</i>	Regulation 4		Regulation 5		Regulation 6		Regulation 7	
	Coef	t-stat	Coef	t-stat	Coef	t-stat	Coef	t-stat
AID	0.235	2.82 **	0.242	2.86 **	0.082	2.17 **	0.018	0.59
FGR_1					0.648	9.72 **	0.750	18.44 **
ARMS_1	-5.015	-5.98 **	-4.980	-5.69 **	-1.915	-4.07 **	-1.386	-4.64 **
ETHNIC	0.304	0.91	0.268	0.81	0.066	0.51	0.058	0.50
CFA	-0.460	-2.01 **	-0.562	-2.44 **	-0.327	-4.03 **	-0.322	-5.03 **
GDP_CAP	0.740	4.07 **	0.760	4.35 **	0.268	3.38 **	0.160	3.14 **
SSA	0.069	0.30	0.052	0.24	-0.055	-0.64	-0.031	-0.38
EASIA	0.134	0.45	0.173	0.60	0.179	2.12 **	0.202	3.23 **
EGYPT	-0.081	-0.45	0.038	0.21	0.145	1.75 *	0.227	3.53 **
CENTAM	0.572	2.36 **	0.636	2.90 **	0.242	1.65	0.184	3.15 **
<i>R-squared</i>	0.364		0.421		0.713		0.822	
<i>N</i>	306		236		236		222	
<i>Period Dummies</i>	5		4		4		4	
<i>Method</i>	IV		IV		IV		IV	

Robust t-statistics using country clusters.

* 10% significance level

** 5% significance level

Appendix

Table A1: Sample Coverage

Country	Coverage			Periods	Country	Coverage			Periods
Argentina	1970	1985	4		Malta	1980	1995	4	
Burundi	1975	1995	5		Myanmar	1970	1995	6	
Benin	1980	1995	4		Mauritius	1975	1995	5	
Bolivia	1980	1995	3		Malawi	1975	1995	5	
Brazil	1970	1995	4		Malaysia	1970	1990	5	
Barbados	1980	1990	3		Niger	1975	1995	5	
Botswana	1980	1995	4		Nigeria	1975	1995	5	
Central African Rep.	1985	1995	3		Nicaragua	1985	1995	3	
Chile	1975	1995	5		Nepal	1980	1995	4	
Cote d'Ivoire	1980	1995	4		Pakistan	1970	1995	6	
Cameroon	1980	1995	4		Panama	1975	1995	5	
Congo, Rep.	1980	1995	4		Peru	1980	1995	4	
Colombia	1980	1995	4		Philippines	1975	1995	5	
Costa Rica	1975	1995	5		Papua New Guinea	1980	1995	4	
Dominican Republic	1980	1995	4		Paraguay	1990	1995	2	
Algeria	1990	1995	2		Rwanda	1975	1995	5	
Ecuador	1980	1995	4		Senegal	1980	1995	4	
Egypt, Arab Rep.	1980	1995	4		Singapore	1970	1975	2	
Gabon	1980	1995	4		Sierra Leone	1975	1995	5	
Ghana	1975	1995	5		El Salvador	1985	1995	3	
Guatemala	1970	1995	6		Syrian Arab Republic	1970	1995	6	
Guyana	1995	1995	1		Chad	1985	1995	3	
Honduras	1980	1995	4		Togo	1980	1995	4	
Haiti	1990	1995	2		Thailand	1970	1995	6	
Indonesia	1970	1995	6		Trinidad and Tobago	1980	1995	4	
India	1970	1995	6		Tunisia	1970	1995	6	
Iran, Islamic Rep.	1980	1995	4		Turkey	1975	1995	5	
Jamaica	1980	1995	4		Tanzania	1970	1995	6	
Jordan	1970	1995	6		Uganda	1975	1995	5	
Kenya	1970	1995	6		Uruguay	1980	1995	4	
Korea, Rep.	1975	1990	4		Venezuela	1970	1985	4	
Sri Lanka	1980	1995	4		South Africa	1995	1995	1	
Morocco	1980	1995	4		Congo, Dem. Rep.	1970	1995	6	
Madagascar	1970	1995	5		Zambia	1975	1995	5	
Mexico	1975	1995	4		Zimbabwe	1980	1995	4	
Mali	1975	1995	5						

Table A2: Summary Statistics

	Mean	Median	Std. Dev.	Min	Max	N	Units/Scale
AID	-0.583	-0.143	1.868	-6.502	2.545	306	log % of GDP
FGR	5.199	5.236	0.979	2.585	7.278	306	0 to 10
FGR_1	5.186	5.225	0.984	2.585	7.278	236	0 to 10
POLICY	1.520	1.273	1.068	-4.115	3.391	257	index
GDP_CAP	7.429	7.488	0.808	5.598	9.339	306	log of 1985 \$
POP	16.170	16.046	1.459	12.423	20.659	306	log
ARMS_1	0.046	0.017	0.090	0	0.870	306	% of imports
ETHNIC	0.489	0.580	0.300	0	0.930	306	0 to 1
CFA	0.144	0	0.351	0	1	306	indicator
SSA	0.395	0	0.490	0	1	306	indicator
EASIA	0.092	0	0.289	0	1	306	indicator
EGYPT	0.013	0	0.114	0	1	306	indicator
CENTAM	0.085	0	0.279	0	1	306	indicator

Table A3: Simple Correlations

	AID	FGR	GDP_CAP	POP	ARMS_1	ETHNIC	CFA	SSA	EASIA	EGYPT	CENTAM	
AID	1											
FGR	-0.13	1										
GDP_CAP	-0.66	0.33	1									
POP	-0.36	-0.29	-0.18	1								
ARMS_1	0.14	-0.41	-0.02	0.11	1							
ETHNIC	0.16	-0.13	-0.44	0.26	-0.08	1						
CFA	0.33	-0.14	-0.27	-0.23	-0.07	0.35	1					
SSA	0.48	-0.15	-0.60	-0.21	-0.12	0.49	0.51	1				
EASIA	-0.21	0.08	0.12	0.29	-0.09	0.11	-0.13	-0.26	1			
EGYPT	0.06	-0.04	0.01	0.12	0.08	-0.17	-0.05	-0.09	-0.04	1		
CENTAM	0.03	0.24	0.12	-0.21	0.00	-0.21	-0.12	-0.25	-0.10	-0.04	1	
	AID	FGR	POLICY	GDP_CAP	POP	ARMS_1	ETHNIC	CFA	SSA	EASIA	EGYPT	CENTAM
AID	1											
FGR	-0.07	1										
POLICY	-0.04	0.41	1									
GDP_CAP	-0.66	0.29	0.30	1								
POP	-0.36	-0.31	-0.15	-0.17	1							
ARMS_1	0.15	-0.43	-0.22	-0.05	0.10	1						
ETHNIC	0.09	-0.07	-0.11	-0.36	0.28	-0.09	1					
CFA	0.27	-0.11	-0.02	-0.22	-0.21	-0.07	0.32	1				
SSA	0.43	-0.06	-0.20	-0.55	-0.24	-0.13	0.42	0.46	1			
EASIA	-0.19	0.07	0.34	0.12	0.28	-0.10	0.14	-0.12	-0.25	1		
EGYPT	0.07	-0.05	0.01	0.00	0.12	0.09	-0.19	-0.04	-0.09	-0.04	1	
CENTAM	0.08	0.16	-0.02	0.07	-0.20	0.02	-0.20	-0.10	-0.22	-0.10	-0.04	1
	AID	FGR	FGR_1	GDP_CAP	POP	ARMS_1	ETHNIC	CFA	SSA	EASIA	EGYPT	CENTAM
AID	1											
FGR	-0.18	1										
FGR_1	-0.12	0.79	1									
GDP_CAP	-0.67	0.38	0.30	1								
POP	-0.34	-0.29	-0.34	-0.21	1							
ARMS_1	0.17	-0.44	-0.40	-0.05	0.09	1						
ETHNIC	0.17	-0.18	-0.11	-0.46	0.28	-0.08	1					
CFA	0.33	-0.19	-0.08	-0.28	-0.22	-0.07	0.33	1				
SSA	0.52	-0.21	-0.09	-0.62	-0.20	-0.10	0.50	0.50	1			
EASIA	-0.23	0.11	0.04	0.14	0.30	-0.11	0.12	-0.13	-0.26	1		
EGYPT	0.05	-0.03	-0.06	0.01	0.12	0.09	-0.17	-0.05	-0.09	-0.04	1	
CENTAM	0.03	0.25	0.23	0.11	-0.21	0.01	-0.21	-0.12	-0.25	-0.10	-0.03	1