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Does Aid Help Improve Economic Institutions?[1]

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Abstract

Aid is expected to promote better living standards by raising investment and growth. But aid may also affect institutions directly. In theory, these effects may or may not work in the same direction as those on investment. This paper examines the effect of aid on economic institutions and finds that aid has neither a positive nor a negative impact on existing measures of economic institutions. These results are found using pooled data for non-overlapping five-year periods, confirmed by pooled annual regressions for a large panel of countries and by pure cross-section regressions. We explicitly allow for time invariant effects that are country specific and find our results to be robust to model specifications, estimation methods and different data sets.

JEL Classification: O11; O19; O16

Keywords: Aid; Economic Institutions; Dynamic Panel

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Introduction

Institutions are shaped by history. Whatever other factors may affect their form, institutions have inertia and "robustness".

R.D. Putnam, (1993), pg.8

How effective is aid in promoting better growth in poor countries? Over the last decade, new data and techniques have been used to examine the relationship between aid and growth, the urgency of the issue varying presumably with the tightness of aid budgets. Conflicting results have been reported in these different studies. Some, notably, Clements et al (2004) and Dalgaard, Hansen and Tarp (2004) among recent papers, find a positive or conditional relationship between aid and growth. Papers attempting to nuance the relationship between aid and growth such as Burnside and Dollar (2000) and World Bank (1998) find that aid promotes growth in good policy environments. However, Hansen and Tarp (2001), Easterly (2003), Easterly, Levine and Roodman (2004) and Roodman (2004) find that these results are not robust to different data sets and specifications. Rajan and Subramanian (2005) reviewing the literature find no robust relationship between aid and growth.

In these papers, the main hypothesis is that aid should raise growth by providing funds for investment. As Rajan and Subramanian (2005) indicate, even assuming that all aid is invested (and using plausible values for both the share of capital in income and the output capital ratio for the average developing country), estimations show that a 1% increase in the ratio of aid to GDP should at most raise long run growth by 0.16 percent. If half of aid is wasted or consumed this coefficient of aid's impact on growth should be close to 0.1. This is not a large amount. They also note that the coefficient on aid should be close to its impact on investment. Barro and Martin (2004, Chapter 12) find a coefficient on the investment to GDP growth rate in the order of .03%; thus aid should not be expected to have a large impact on growth through this channel. They conclude that to justify higher coefficients found in the literature, one would have to assume that aid has a substantial impact on total factor productivity growth- for example because it promotes

higher human capital, better institutions or better policies. This paper examines the effect of aid on economic institutions and finds no robust evidence of either a positive or negative relationship between aid and economic institutions.

Studies of the relationship between aid and growth find that much of aid does not translate one for one into higher public investment and the effect varies tremendously between countries (World Bank, 1998). The typical aid dollar finances 29 cents of public investment. Even more noteworthy, studies have found that the share of GDP devoted to public capital spending does not have a relationship to economic growth. This is because how spending translates into public capital depends on the efficiency of government expenditures and the relationship between budgeted and realized expenditures. For example, infrastructure services have been shown to be important for growth (Serven and Calderon, 2004) but it is harder to show that infrastructure expenditures are important (Hulten, 1996 and World Bank, 1998).

Even if aid does not affect investment and thus growth directly, aid could affect growth (indirectly) through its impact on policies or institutions (or even skills/human capital). Some of these changes could be productivity enhancing (or reducing) and could add value even if funding public capital does not (see World Bank, 1998). For example, donors often attempt to strengthen legal systems and property rights. There are countless projects funded that aim to reform the judiciary and numerous laws adopted as a result of donor intervention, for example: bankruptcy, accounting, disclosure, company, collateral, and land registration laws. It has been argued that many countries underwent rapid economic liberalization (or deregulation, which is equivalent to a particular form of institutional reform) because of pressures from the donor community. This "Washington Consensus", though geared towards what was believed to be an improvement in policies and institutions was often blamed for much of the vicissitudes of Russia and Latin America in the 1990s. Explanations for the less than positive results have varied between an underestimation of the time needed to implement institutional change or the too rapid pace of

liberalization (and accompanying change in institutions. In the case of the socialist countries this might be expected to show up as less regulation).[2]

If aid worsened some institutions but supported public expenditures on investment, the net effect on growth could be either positive or negative. Similarly, the impact would be ambiguous if it improved some institutions but worsened others. If aid succeeded in raising critical skills then the net effect of all three on growth could be nil, positive or negative. Or the impact of aid may vary between the short and long runs.

Papers examining the impact of aid on certain aspects of institutional development in a country have become popular. The potentially negative impact of aid on institutional development is discussed in several papers, old and new. Aid can encourage rent seeking and corruption, alleviate pressures to reform domestic institutions (such as taxation), keep undemocratic or "bad" governments in power by providing them with resources for their cronies, and generally weaken accountability. Alesina and Weder (2002) and Knack (2000) find that foreign aid inflows lead to increases in corruption as agents vie for aid resources. Brautigam and Knack (2004) contend that high levels of aid create moral hazard for donors and recipients, exacerbate collective action problems and weaken local pressures for reform and for accountability. In addition, the unpredictability and volatility of aid can have perverse effects on fiscal policy (Celasun and Walliser, 2005) and donor fragmentation can reduce bureaucratic quality as different donors put pressure on staff in government bureaucracies (Knack and Rahman, 2004) to satisfy donors' own needs (for example needs for financial reporting). Djankov et al (2005) find that foreign aid has a negative impact on democracy. They explain these results by saying foreign aid could lead politicians in power to engage in rent-seeking activities in order to appropriate these resources and to exclude other groups from the political

[2] It is also believed that changes in formal laws and regulations (economic institutions) did not take into account existing informal systems and other initial conditions (such as income and skill levels) and so tended to undermine the transactions they were supposed to strengthen.

process. Thus political institutions are damaged because they become less representative and less democratic. Some work on economic institutions Knack (2000) indicates that even measures of economic institutions performed poorly as a result of aid. Svennsson (2000) finds that aid is associated with more regulation and that more regulation is associated with more corruption.

But just as aid can have a negative effect on political institutions and corruption through the rent seeking process, it may also have a positive effect on certain economic institutions. These effects would be generated because (a) resources are needed to set up institutions and aid provides these resources, (b) the skill building, knowledge dissemination (which provides relevant information on economic outcomes to stakeholders, including voters and leaders within existing political systems)[3], policy advice and conditionality associated with aid aims to improve institutions and (c) aid enhances incentives to undertake reforms (not necessarily investment). Thus it is equally possible for aid to influence economic institutions positively. Presumably the larger the flows of aid to a country, the higher in absolute terms is the flow of these other factors to a country so that aid flows serve as proxy for the other dimensions of aid. In reality, there may be countries where the two dimensions of aid are substitutes: countries receiving less money may receive relatively more effective policy advice (so that countries with higher aid flows may show less institutional development because the non-money factor is absent or unimportant or because the negative effect of money overrides any positive effect of policy advice)[4]. We assume that countries receiving higher aid flows also receive more policy advice and conditionality aimed at strengthening institutions. Finally, as mentioned earlier, aid could have positive effects on some types of institutions but have negative effects on others so that the overall effect may be zero.

Kilby (2004) finds that donor funds favor more heavily regulated economies and promote deregulation. Remner (2004) finds aid to be associated with increased public spending but reduced tax effort. Bannerjee and Rondinelli (2003) find that aid does not have a systematic

[3] If aid is used to disseminate policy impacts, aid may change leaders' incentives within political institutions.
[4] In practice we are not able to disentangle these separate effects in our regressions.

influence on the decision to begin privatization, that the impact of aid on privatization is limited to technical assistance and that in countries with "better" governance, aid may be more important or effective in the privatization process. Tavares (2003) finds that aid reduces corruption.

Other potential impacts of aid on factors that affect growth are also possible. The aid literature has examined the negative effects of real exchange rate appreciation following high aid inflows on trade and growth. A recent example of these types of papers is that by Rajan and Subramanian (2005) who find that aid inflows have systematic adverse effects on a country's competitiveness, measured as a decline in the share of labor intensive and tradable goods industries in the manufacturing sector. In order to understand how aid affects development it is important to examine aid's impact on a broader set of factors.

In this paper, we revisit the issue of aid and institutions, focusing particularly on the "economic" institutions measures. Historical development and the cross country growth literature have both demonstrated that different types of political institutions and conditions have supported growth. More specifically, having more or less democracy does not predict growth outcomes. But economic institutions matter. (Acemoglu et al. (2002, 2004, 2005)) and they are not linked in a one to one predetermined relationship with political institutions. Figures 1-4 show the correlation between a measure of democracy from polity4 and alternative measures of economic institutions from ICRG and the Fraser Institute. Correlations between different measures of economic and political institutions are not large over decades (though they are usually significant) as shown in Tables 1and 2 below and tables A1 and A2 in the appendix.

Table 1: Correlations between democracy and measures of economic institutions

	1984			1997		
	bq	**corr**	**polity4**	**bq**	**corr**	**polity4**
bq	1			1		
corr	0.8276	1		0.6971	1	
polity4	0.6069	0.5634	1	0.4555	0.5783	1

Table 2: Correlations between democracy and measures of economic institutions

	Average 1984-1997		
	avg_bq	**avg_corr**	**avg_polity4**
avg_bq	1		
avg_corr	0.829	1	
avg_polity4	0.5942	0.6002	1

*Notes for Tables 1 and 2:
bq= bureaucratic quality, corr= corruption, from ICRG; polity4 (from polity IV index, normalized 0-1)= democracy measure, avg= average.

Figure 1: Change in World Institutions: corruption and democracy, 1984-1997

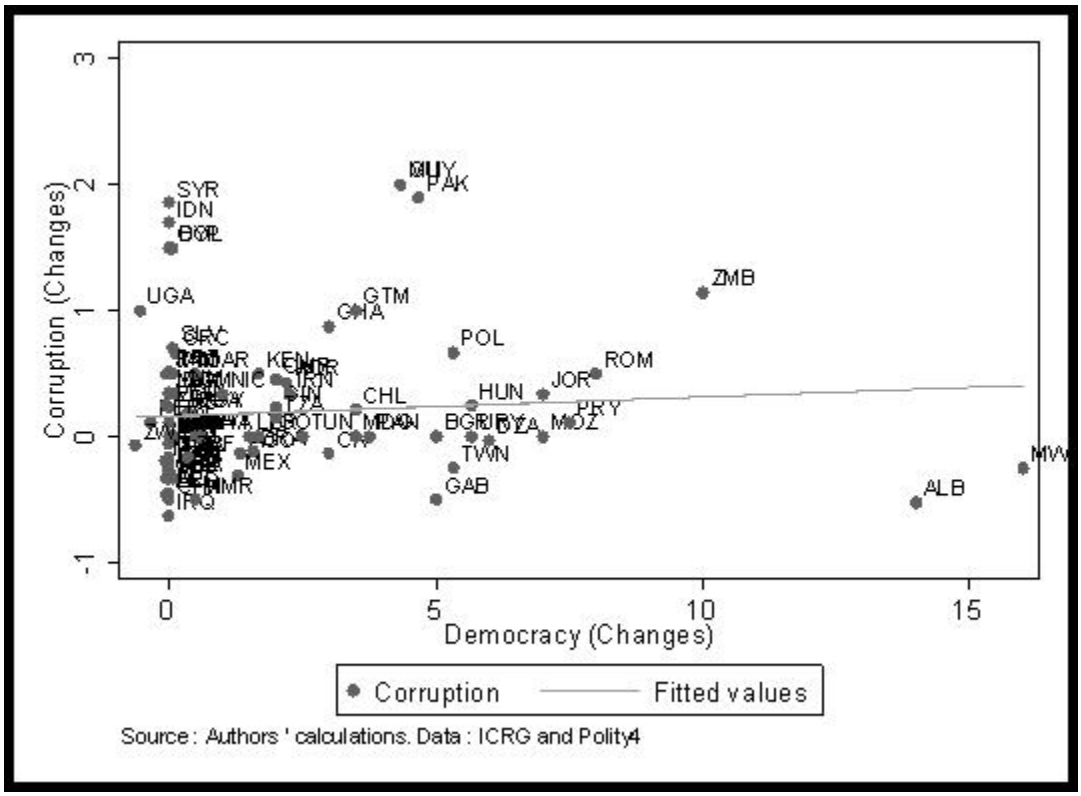


Figure 2: Change in World Institutions: bureaucratic quality and democracy, 1984-1997

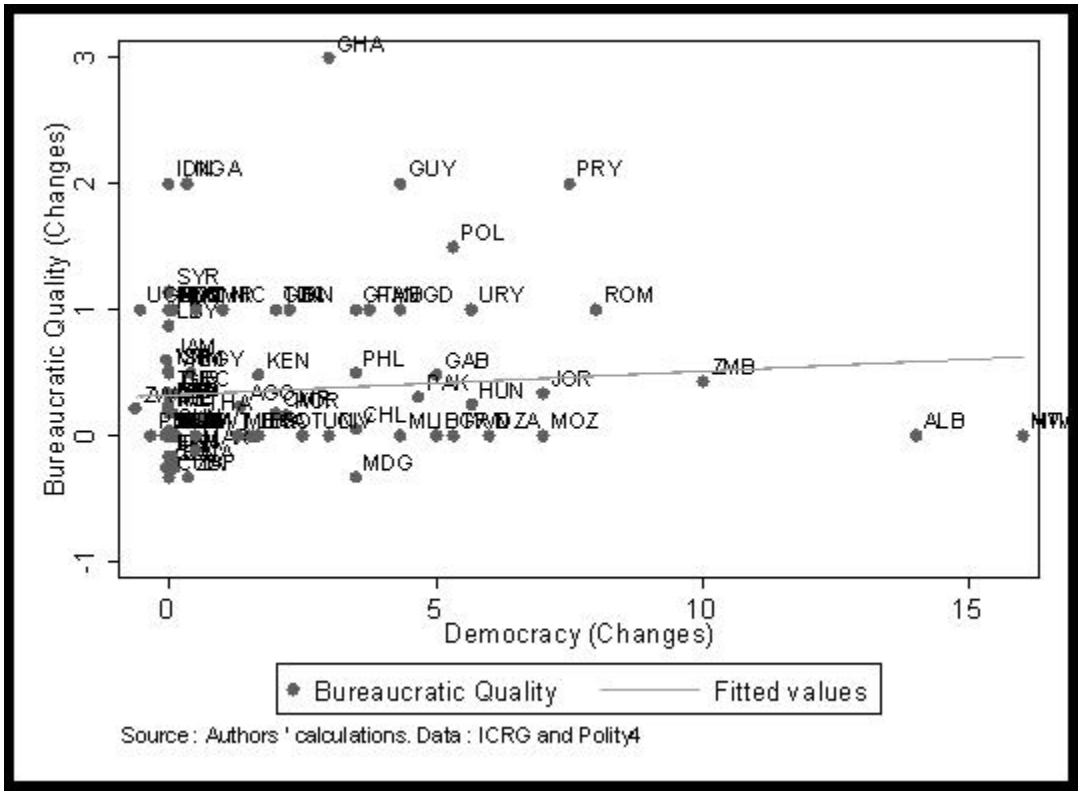


Figure 3: Change in World Institutions: property rights and democracy, 1970-2000

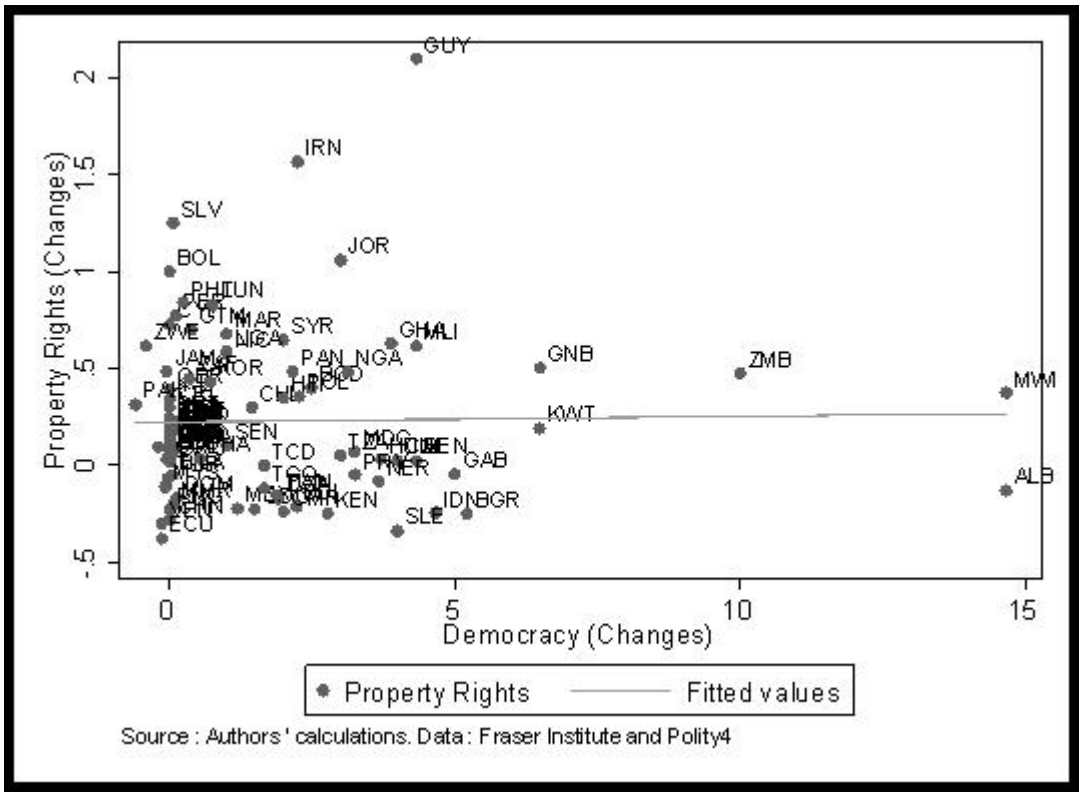
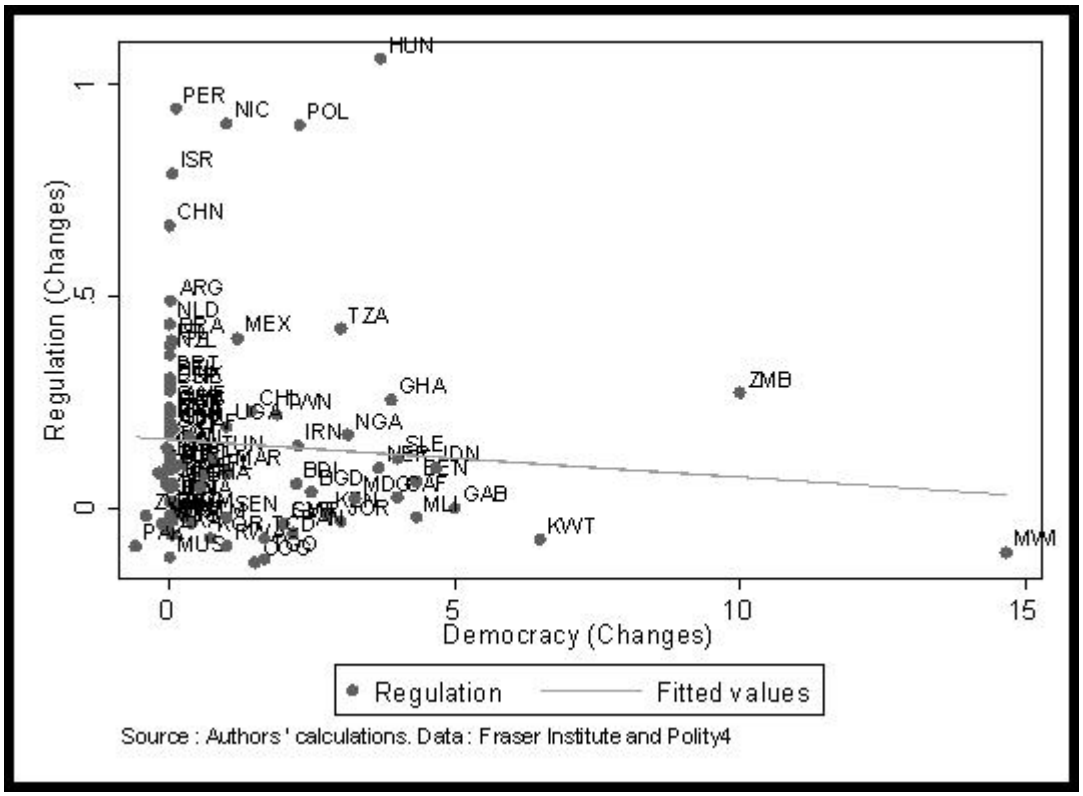


Figure 4: Change in World Institutions, regulation and democracy, 1970-2000



The main conclusion of this paper is that aid does not have an impact on different measures of economic institutions once we explicitly control for country effects that are time invariant and are country specific. We conclude that estimations showing that aid either negatively or positively affects institutions omit historical determinants of institutions that are important in shaping today's institutions. Once we account for these factors, we find no robust effect of aid on institutions. We do both cross section and dynamic panel data (GMM) analysis to ask the question whether aid has any effect on a variety of measures of economic institutions. Our results are robust to different definitions of aid (for example we distinguish between technical assistance and other forms of lending), to a number of specifications and to different datasets.

Furthermore, we find that the results in the literature that find a negative relationship between aid and democracy (a measure of political institutions) and a positive relation between aid and corruption are not robust to specification or estimation methods and vanish once we explicitly consider country fixed-effects. These results are not shown here.[5]

Data on Aid and Economic Institutions

For the panel regressions in this paper we use indicators of economic institutions from two main datasets for the panel regressions-the ICRG and Fraser Institute datasets. As a robustness check we also use an indicator to measure institutional development more narrowly. This measure is linked to institutional development in the financial sector. We focus on a subset of indicators that represent our notion of economic institutions – those that support economic transactions but not through the political process. Indicators of democracy, checks and balances within government and political stability (or civil wars) fall in the latter category. For the ICRG dataset, available for 1984-2002 we use measures of bureaucratic quality and corruption as indicators of "economic" institutions (in contrast to government stability, internal conflicts,

[5] But are available upon request.

ethnic tensions, democracy and accountability which do not fall in this group). The ICRG data are available on an annual basis for 113 countries.[6]

In the annual data we selected five indicators from the ICRG data set, (see details in the data appendix table). The variables we considered are bureaucratic quality, corruption, law and order, expropriation risk, or repudiation risk.[7]

The other data we use are five yearly data from the Fraser Institute available for the period 1970-2000. Two indicators are of interest: freedom from government regulation and legal structure/security of property rights. The first variable ranges from 0 (low freedom=high regulation) to 10 (high freedom=low regulation). This indicator is an aggregate of 15 sub-indices and is called "Regulation of Credit, Labor, and Business. This index reflects a mix between what is commonly accepted as desirable institutional outcomes and simply, less regulation. For example, under most conditions it is desirable that administrative procedures are not an important obstacle to setting up a business, that the percentage of deposits held in private banks is high (as opposed to deposits in public banks), that businesses are generally free to set their own prices and that interest rate controls and regulations do not lead to negative rates. But the indicator also includes a measure of whether hiring and firing practices are determined by private contract (as opposed to some government regulations). It is not clear how much regulation would be considered "good" in all contexts. For those who would contend that the indicator picks up mostly "less regulation", it would help test the hypothesis that donors successfully promoted the Washington Consensus model of "liberalization, privatization and competition". Alternatively, it would be a test of whether donors supported better institutions.

The second indicator measures aspects of: a) judicial independence, b) impartial courts, c) protection of intellectual property rights, d) military interference in rule of law and the political process, and e) the integrity of the legal system.

[6] Note that other authors, e.g. Knack (2000and 2004) also use the ICRG data.
[7] In the empirical section we do not present all the regressions due to space constraints but these are all available upon request.

Finally, we use another measure of institutional quality that is sector specific. It may be argued that most of the existing measures of institutional quality are too broad (and subjective) to be good indicators of the effects of aid on institutional quality. One way to narrow the analysis is to consider institutional quality in a particular sector or a set of institutions geared towards promoting a particular transaction- we choose the financial sector. Following Clague et al. (1999), we think that measures of financial assets kept in the banking system, namely M2 minus currency in circulation, are a good proxy for institutional development in the financial sector. The reasoning is that individuals would be very unlikely to hold money in banks if the legal and regulatory framework did not sufficiently lower the risk of bank failure or expropriation so as to make bank deposits desirable. Donors have put a lot of aid funds and policy advice into financial system development, specifically bank privatisation and banking sector regulation and supervision.

Three types of aid flows are used. The first is Effective Development Assistance over real GDP (AID), as in Chang et al., 1998. The second is Net Overseas Development Assistance over real GDP (ODA) and includes technical assistance, as in Alesina and Weder (2002) and technical assistance completed as the difference between ODA-AID. This variable is available for 1975-1995 from Chang et al (1998). Missing values are extrapolated based on a regression of EDA on Net ODA. The standard measure of aid (Official Development Assistance, ODA) lumps concessional loans together with grants if the loan's grant element exceeds 25%, while Effective Development Assistance converts these loans to their grant equivalent and thus provides a better measure of long term resource flows.

Model Specification and Estimation Strategy

Cross-Section Estimation

In this section, as in Knack (2001), Knack (2004), Brautigam and Knack (2004) and Kilby (2005) we consider the effect of aid on economic institutions in a pure cross-section of

countries. Since the main source of variation is given by cross-country differences we are forced to omit country effects (fixed effects). The equations we estimate are of the following form:

$$\Delta y_i = \alpha + \delta y_{0i} + \beta X_i + \delta A_i + \varepsilon_i \qquad (1)$$

Where y_i represents the change in the quality of economic institutions, and y_{0i} is a variable representing the initial level of institutional quality. This specification is used since initial institutional effectiveness is believed to determine subsequent development (institutions are persistent) and to catch a regression toward the mean effect. It is also the specification used in a number of recent papers (such as those mentioned above). The variable A_i represents three different types of aid flows: overall development assistance (oda), development assistance net of technical assistance (aid) and technical assistance (ta). For the cross country regressions, our main specification assesses the effect of aid flows in the specifications shown in (1), though our main results remain unchanged if the model is specified in levels or in *pure changes*, that is, without the initial level of institutions.[8] For the panel data, GMM analysis, we focus on a dynamic equation in levels, though we also consider the model in changes. In the levels equations, we capture the effect of past aid flows on institutions through lagged institutions on the right hand side.

In all but the most basic specifications we control for the initial level of GDP or concurrent level of GDP. Real GDP computed as LGDP above, is in 1985 dollars. Other conditioning variables used in the robustness checks differ across specifications and are added drawing from the theory of what determines institutional quality as well as the empirical literature on institutions. These variables are legal origin, to account for past historical/political influences on institutions, latitude (to account for geographical factors), settler mortality rates a la Acemoglu et al. (2002), ethnic fractionalisation, and trade openness to account for the effect of trade and competition on institutions. Following Mulligan and Shleifer (2003) who find that

[8] We do not think the regression in levels (with aid flows) or in pseudo-changes (with initial institutions) to be the most appropriate specification but have estimated these equations in order to reproduce those in recent papers.

more heavily populated states in the US are more heavily regulated, we believe that population has a direct effect on institutions. Institutions being costly to set up, more populated countries would tend to be more heavily regulated or have more complex institutions.[9]

We run both ordinary least squares (OLS) and instrumental variables (IV) regressions to instrument for aid. We used two sets of instruments. The first instrument set considers only lags of the log of gnp per capita and the log of population. For instance, to insure exogeneity we consider lags 7 and 11 (depending on the data used), the first, to instrument for the regressions using the ICRG data and the second for those using Fraser data and the CIM indicator. Although not reported, we checked for the first stage F-statistics of the excluded instruments (in order to see if the instruments are weakly correlated with the endogenous regressors, (see Stock Wrigth and Yogo [2004]). All the first stage regressions report an F-test greater than 10.

The second instrument set is larger and it is composed of the first instrument set and a series of dummies for ex-colonies, and for Sub-Saharan Africa, Europe, Central America, and Egypt, as in Rajan and Subramanian (2005). In all the IV regressions, since we have more instruments than endogenous regressors, we can test for the validity of the instruments using the Hausman tests. In all the specifications we do not reject the null hypothesis of instrument validity. Although ten lags seem long enough, it is clearly possible that countries' economic/ institutional situation of ten years ago could affect the level of economic institutions today. It is hard to find truly exogenous variations in aid particularly since donors do not give aid randomly.[10]

Tables 3 and 4, report summary statistics.

[9] Regressions not presented here are available from the authors upon request.

10 Given that the specification we consider includes the initial level of institutions, we believe that considering only aid as endogenous can lead to severe inconsistency in the estimation of the effect of aid on institutions in the cross section estimates. The initial level of institutions is itself correlated with the unobservable country effect and the initial level of institutions is itself endogenous.

Table 3: Descriptive Statistics – Panel

Variable	Available	*N*	Obs	Mean	Std. Dev.	Min	Max
reg	5 years	70-94	680	5.44	1.15	2.5	8.8
prop	"	"	648	5.46	1.91	1.1	9.6
icrge	"	"	602	4.58	1.96	0	10
cim	"	110	844	0.77	0.15	0	1
rule_law	Annual	90-100	1497	3.33	1.69	0	6
bur_qual	"	"	1497	3.28	1.64	0	6
ethn_tens	"	"	1497	3.79	1.61	0	6
repud	"	"	1457	6.29	2.28	1	10
exp_risk	"	"	1457	6.89	2.23	1.5	10
corr	"	"	1500	3.41	1.55	0	6
oda	"	"	763	1.67	2.54	-0.02	16.29
loda	"	"	572	-0.19	1.81	-9.67	2.79
aid	"	"	763	0.91	1.77	-3.8	12.67
laid	"	"	462	-0.69	1.98	-14.09	2.54
gdpg	"	"	770	1.81	3.51	-13	32.29
lgdp	"	"	773	7.94	1.04	5.83	10.75
popg	"	"	838	1.84	1.47	-5.86	16.62
lpop	"	"	840	15.79	1.7	11.59	20.91
syr	"	"	663	1.27	1.12	0.01	5.09

Table 4: Descriptive Statistics – Cross-Section

Variable	Available	*N*	Obs	Mean	Std. Dev.	Min	Max
reg8500			104	0.64	0.95	-0.8	3.6
linireg85			104	5.31	1.15	2.5	7.3
prop8500			108	0.79	1.28	-2	4.4
liniprop85			108	5.11	1.8	1.7	8.3
bur_qu9095			113	0.27	0.71	-2	2
bur_qu90			113	3.26	1.6	1	6
corr9095			113	0.24	1	-3	4
corr90			113	3.38	1.52	0	6
exprisk9095			113	1.98	1.8	-3.6	6
exprisk90			113	6.75	2.18	2	10
repud9095			113	1.54	1.64	-2	6.4
repud90			113	6.07	2.34	1.6	10
eth9095			113	0.96	1.24	-1.5	4.8
eth90			113	3.5	1.64	0	6
cim8500			117	0.04	0.08	-0.26	0.51
linicim85			118	0.79	0.13	0.34	0.98
lgnp9095			100	0.1	0.37	-1.48	0.9
laid			88	0.91	2.09	-6.36	4.51
lpop84			97	16.25	1.49	12.29	20.76
lgnp84			85	7.38	1.32	4.79	9.65
lpop8500			120	0.26	0.18	-0.05	0.81
lgdp8500			110	0.1	0.32	-0.61	1.08
lpop75			120	15.6	1.71	11.7	20.52
lgdp75			109	7.76	0.97	5.83	10.58
latitude			176	18.51	24.09	-36.89	63.89
legal_origin			176	2.63	1.81	1	9
eng_legor			176	0.31	0.46	0	1
french_legor			176	0.38	0.49	0	1
germ_legor			176	0.03	0.18	0	1
scand_legor			176	0.03	0.17	0	1
social			176	0.17	0.38	0	1
islam			176	0.05	0.22	0	1
centam			121	0.06	0.23	0	1
easia			121	0.07	0.26	0	1
egypt			121	0.01	0.09	0	1
ssa			121	0.22	0.42	0	1
logmort0			64	4.65	1.25	2.15	7.99

Estimation Results:

Table 5 reports the cross section results for 3 variables from the ICRG data set. The first column presents the results of the OLS estimation for the base specification for aid (using the first instrument set)[11] with GDP and population. Subsequent columns show the IV estimation instrumenting for aid and the third column shows the model with a fuller specification that includes most of the above-mentioned variables. The model in pure changes is not shown but the results are similar. The regressions in Table 4 show that even the OLS regressions do not indicate any significant relationship between aid and institutional measures.

Table 6 shows similar estimations for the two indicators from the Fraser data set. In this table, column 4 shows the estimation in pure changes (differencing all variables). Table 6 shows the only dependent variables among the several tested that have a consistent relationship with aid-the variable from the Fraser data set measuring the extent of regulation. These results indicate that aid is quite robustly associated with more regulation. High values of this indicator suggest that the economy is over regulated. Aid seems to have consistently raised regulation; an alternative way to interpret this result is that aid has not been a force for market liberalisation.[12] Table 7 shows the results for our financial sector specific institutional variable or contract intensive money. Table 6 indicates that some specifications using this variable show a negative relationship between aid and institutions.

The OLS cross section results in Table 5, 6, and 7 indicate that it is possible to get a significant relationship between aid flows and institutions. But in all cases except one, these results are not robust across alternative specifications. In other words, any positive or negative relationship between aid and institutions is not robust to the addition of control variables or to using IV techniques, with the exception of the indicator "regulation".

[11] Similar results are obtained using oda and ta.
[12] Or that the "Washington Consensus" view is substantiated.

As in Acemoglu et al. (2005) we consider the role of history to be crucial in shaping institutions today; this means that we consider the cross-sectional results estimation done in previous works to be driven by the omission of country effects that are correlated with the initial level of institutions. We observe a large and significant effect of the past level of institutions on the change of institutions and on the level itself in all the specifications, estimation techniques, and considering different indicators of institutions.

Table 5. Regressions with Rule of Law, Bureaucracy, Corruption

	Rule of Law			Bureaucracy			Corruption		
	Model 1 (OLS)	**Model 2 (IV Aid)**	**Model 3 (IV Aid)**	**Model 1 (OLS)**	**Model 2 (IV Aid)**	**Model 3 (IV Aid)**	**Model 1 (OLS)**	**Model 2 (IV Aid)**	**Model 3 (IV Aid)**
aid	-0.08	-0.13	-0.06	-0.08	-0.05	-0.03	-0.06	-0.03	0.02
(log)	-0.64	-0.07	-0.06	-0.05	-0.07	-0.05	-0.06	-0.06	-0.05
initial institution	-.67***	-.72***	-.67***	-.40***	-.36***	-.40***	-.52***	-.51***	-.54***
(in year 1990)	-0.086	-0.08	-0.07	-0.07	-0.06	-0.07	-0.08	-0.08	-0.08
lpopulation	-1.12	-3.31	0.61	0.37	-2.7	.15*	-0.86	-3.93*	-0.44
('90~'95)	-2.61	-2.68	-2.29	-1.9	-2	-1.85	-1.82	-2.13	-1.72
lgnp	.63*	.69**	1.11***	.45*	0.38	.54**	0.29	0.3	.63**
('90~'95)	-0.32	-0.31	-0.26	-0.25	-0.31	-0.27	-0.26	-0.29	-0.28
English Legal Origin			-0.24			-.43*			-.65**
			-0.28			-0.26			-0.31
French Legal Origin			-.7**			-.79***			-.83**
			-0.32			-0.26			-0.32
Latitude			.01**			0.003			0.008
			-0.006			-0.003			-0.004
Constant	3.16***	3.47***	3.21***	1.36***	1.57***	1.92***	1.96***	2.17***	
	-0.38	-0.35	-7.43	-0.26	-0.29	-0.35	-0.36	-0.35	
# obs	79	62	62	79	62	62	79	62	62
R^2	0.4	0.81	0.85	0.33	0.48	0.6	0.43	0.52	0.62

Notes:

1) dependent variable is indicator of institutional quality.

2) instrument 1 for aid (lgnp and lpop in 1984) is used. Instrument 2 not shown but basic story similar.

3) robust standard error is reported in the brackets below the coefficient

4) ***designates significance at .01 level, ** at .05 level, * at .10 level.

Table 6. Regressions with Regulation and Property Rights

	Regulation				Property Rights			
	Model 1 (OLS)	**Model 2 (IV Aid)**	**Model 3 (IV Aid)**	**Model 4 Changes (IV)**	**Model 1 (OLS)**	**Model 2 (IV Aid)**	**Model 3 (IV Aid)**	**Model 4 Changes (IV)**
Aid	-.14***	-.22***	-.22***	-.33***	.03*	-.09**	-.06*	0
(log)	-0.04	-0.06	-0.06	-0.09	-0.08	-0.11	-0.11	-0.13
Initial Institution	-.47***	-.47***	-.49***		-.60***	-.63***	-.61***	
(in year 1985)	-0.08	-0.08	-0.09		-0.1	-0.1	-0.1	
lpopulation	-1.07	-0.76	-0.8	-0.95	-0.99	0.998	-0.85	-0.9
('85~'00)	-0.72	-0.76	-0.77	-1.11	-1.15	-1.3	-1.3	-1.65
lgnp	-.25**	-0.39	-.45*	-1.11***	.93**	0.7	0.61	0.22
('85~'00)	-0.25	-0.26	-0.26	-0.38	-0.41	-0.45	-0.47	-0.54
English Legal Origin			0.3				0.53	
			-0.3				-0.43	
French Legal Origin			.02*				0.11	
			-0.28				-0.44	
Latitude			0.001				0.01	
			-0.004				-0.01	
Constant	3.85***	4.09***	4.06***	2.19***	3.49***	4.12***	3.52**	1.04***
	-0.49	-0.59	-0.58	-0.49	-0.66	-0.72	-0.82	-0.56
# obs	75	74	74	74	78	76		76
R^2 (uncentered)	0.61	0.59	0.61	0.27	0.5	0.49		0.25

Notes:

1) Dependent variable is change in the institutional indicator, 1985~2000. For column 4, the variables are all specified in changes.

2) instrument 1: log population in 1975, log gnp in 1975

3) robust standard error is reported in the brackets below the coefficient

4) ***designates significance at .01 level, ** at .05 level, * at .10 level.

Table 7. Regressions with CIM and aid

	Model 1 (pure change,OLS)	Model 2 (pure changes,IV1 Aid)	Model 3 (pure changes,IV2 Aid)	Model 4 (IV1 Aid)	Model 5 (IV2 Aid)
Aid	0.0009	-0.003	-0.002	-.012*	-.01*
(log)	-0.0037	-0.005	-0.005	-0.006	-0.006
Initial CIM				-.29**	-.29**
(in year 1985)				-0.14	(-1.97)
Population	-0.059	-0.06	-0.06	-.07*	-.07*
('85~'00)	-0.043	-0.049	(-1.33)	-0.04	-0.04
GDP	.039***	.038**	.039**	.04**	.04**
('85~'00)	-0.013	-2.37	-2.43	-0.017	-0.017
Constant	.063***	0.08	.08***	.35**	.34**
	-0.019	-0.028	-0.027	-0.14	-0.14
# obs	94	87	87	87	87
R^2 (uncentered)	0.27	0.26	0.27	0.36	0.36

Notes:

1) Dependent variable CIM is Contract-Intensive Money, 1985~2000.

2) instrument 1: log population in 1975, log gnp in 1975
instrument 2: log population in 1975, log gnp in 1975, dummy for Central America, East Asia, Europe and Sub-Saharan Africa

3) robust standard error is reported in the brackets below the coefficient

4) ***designates significance at .01 level, ** at .05 level, * at .10 level.

Dynamic Panel GMM Estimation

Cross-section methods are simple and easy to interpret, but have weaknesses. Relationships may be artificially created by unobserved heterogeneity that is caused by a number of factors such as history, mortality rates, geography, or endowments of resources (country effects that are not time varying-in the case of economic institutions). The use of panel data can overcome this problem. We can look at relationships over time (so we can add *within* country variation to the *across* country variation), to see whether increases in aid are followed by changes in economic institutions, and to distinguish between short-run and long-run effects. The main estimated equation is the following one:

$$\Delta y_{i,t} = (\alpha - 1) y_{i,t-1} + \beta' X_{i,t} + \gamma t + \eta_i + \varepsilon_{i,t} \qquad (2)$$

Or alternatively,

$$y_{i,t} = \alpha y_{i,t-1} + \beta' X_{i,t} + \gamma t + \eta_i + \varepsilon_{i,t} \qquad (3)$$

Where y_it represents the level of the different institutional indicators for country (i) at time (t), y_it-1 is its own lag. t, is a time trend attempting to catch a common co-movement between the regressors and the dependent variable, in the estimates we consider both a linear trend and a set of time dummies but we report results only with time dummies. η_i are the country specific effects and $\varepsilon_{i,t}$ are the i.i.d stochastic disturbances. X_it are the regressors. Among them we consider the lagged level of the natural log of aid flows.

The following section provides details on the variables considered and a discussion of the results. In this section we describe the methodological aspects of estimating (dynamic) models with panel data. There are several econometrics methods which can be used to estimate equations like (2), or equivalently (3) where the number of countries (N) is larger than the number of time periods for which data are available (T). Because of the assumptions on the

presence of country specific effects η_i, and on the error terms, i.i.d. $\varepsilon_{i,t}$; the Ordinary Least Squares (OLS) and the Instrumental Variables estimators[13] of α, *and* β in the level equation (2) and (3) are biased and inconsistent. This is so because the explanatory variable $y_{i,t-1}$ is by definition positively correlated with the error term $\eta_i + \varepsilon_{i,t}$ due to the country fixed effect. This correlation does not vanish as the number of countries in the sample gets larger, nor as the number of time periods increases. As discussed in Bond et al. (2001) and in Bond (2002), the OLS levels estimator of the autoregressive coefficient (which suffers from omitted variable bias) is considered that upper bound for the true estimates. The Within Groups estimator eliminates these sources of inconsistency by transforming the equations so as to eliminate η_i [14].

$$\Delta y_{i,t} = \alpha \Delta y_{i,t-1} + \beta' \Delta X_{i,t} + \gamma \Delta t + \Delta \varepsilon_{i,t} \qquad (4)$$

The original observations are expressed in first differences, as in equation (4), and OLS estimates performed. Even if the country effect η_i is removed by the first difference transformation, for panels where the number of time periods is small relative to the number of countries the first difference transformation induces a "non-negligible" correlation between the transformed lagged dependent variable and the transformed error term. The above correlation can be shown to be negative and does not vanish as the number of countries gets larger[15].

As discussed in Bond et al. (2001) and in Bond (2002), we consider the Within Groups estimator of the autoregressive coefficient as a lower bound for our estimates. Because of the

[13] We refer to estimation that considers aid as the only endogenous regressor and only instruments for aid (as in cross section specifications).
[14] When T=2 the Within Group estimator and the First Difference estimators delivers the same data point estimates. If T>2 the two estimators are different. In the estimation we consider Fixed Effect Estimates, see Wooldridge (2003).
[15] However, the correlation induced by the transformation vanishes, and the Within Groups estimator is a consistent estimator when T is large.

opposite nature of the bias of the two above estimators, we would expect better estimators-that is the true parameters- to lie between the OLS and the Within Groups estimates, or at least to not be significantly higher than the former or significantly lower than the latter.

Several consistent estimators potentially bounded between the OLS and the Within Groups estimators are proposed by the econometrics literature but have not been commonly used to disentangle the effect of aid on institutions. We focus here only on two of them namely the "Differenced Estimator" proposed and developed by Holtz-Eakin, Newy and Rosen (1988) and Arellano and Bond (1991) and the "System Estimator" developed by Arellano and Bover (1995) and further developed in Blundell and Bond (1997). The main advantage of considering these Generalized Method of Moments estimators is that both GMM estimators allow us to control for potential endogeneity of all explanatory variables, country fixed effects and temporary measurement error. Aware of the severe bias problems and instrument validity issues in the case of a persistent (time-variant) measurement error, we leave the discussion on the performance of both the Difference and the System estimators to Hauk et al. (2004). By following Bond et al. (2001), in the robustness check of the results we assume that the measurement error is transient and there is a permanent additive measurement error that is considered time-invariant and thus absorbed in the country effect. To allow for the mentioned problem we repeated the estimates using as instruments the lagged levels starting from *(t-3)* instead of *(t-2)* and the lagged difference starting from *(t-2)* instead of *(t-1)*.[16]

GMM estimators control for endogeneity by using "internal instruments", that is, instruments based on lagged values of the explanatory variables. These methods do not allow us to control for full endogeneity but for a weak version of it. More precisely, we assume that the explanatory variables are only "weakly exogenous", which means that they can be affected by

[16] Results are the same and not reported but available on request.

current and past realizations of institutions but must be uncorrelated with future unanticipated shocks to institutions (the error term). Thus, the above correlation assumption implies that future unpredictable innovations in institutions do not affect current foreign aid. This assumption is commonly shared and accepted in the growth literature (the first application is Levine et al. (2000), for details see Bond et al. (2005)) and for the first time discussed in the aid and institutions literature.

To consistently estimate equations (2) and (3), where potentially all the regressors, $X_{i,t}$, can be endogenous, and to account for errors in the differenced equation being correlated with the lagged dependent variable we need some additional assumptions. First we assume that the error term $\varepsilon_{i,t}$ is i.i.d, and second that the explanatory variables $X_{i,t}$ are weakly exogenous such that in the GMM estimation, as suggested by Arellano and Bond (1991), we can exploit the following orthogonality conditions:

$$E[y_{i,t-s}(\varepsilon_{i,t} - \varepsilon_{i,t-1})] = 0 \; s \geq 2 \; , 2; \; t = 3; \; ::::: ; \; T \qquad (5)$$

$$E[X_{i,t-s}(\varepsilon_{i,t} - \varepsilon_{i,t-1})] = 0 \; s \geq 2 \; , 2; \; t = 3; \; ::::: ; \; T \qquad (6)$$

The GMM estimator based on the above orthogonality conditions is commonly defined as the "Differenced Estimator". The two orthogonality conditions only satisfy one of the two requirements needed by the GMM estimators. As showed by Alonso-Borrego and Arellano (1996) and by Blundell and Bond (1997), when the explanatory variables are persistent over time and this is the case when we deal with variables like institutions which have an autoregressive coefficient that is always greater than 0.7 - lagged levels of these variables are weak instruments for the regression equation in first differences. In these cases severe problems of identification

can lead to bias and could result in a poorly performing differenced estimator, see Arellano et al. (2002), Wright (2005), Bond et al. (2005). [17]

To reduce the weak instruments bias[18] of the Difference-Estimator, Arellano and Bover (1995) and Blundell and Bond (1997) propose a different GMM estimator: the System Estimator. The System Estimator, using some mild stationarity assumptions, relies on a *system* of equations, namely the regression in differences, (eq. (4)), added to the regression in levels, (eq. (3)). The system estimator exploits the orthogonality condition of equations (5) and (6) for the equation in differences. For the equation in levels, lagged *differences* of the corresponding variables are used as instruments. Under mean stationary assumptions, these are shown to be appropriate instruments; that is the additional conditions required are that there be no correlation between the *differences* of the variables and the country specific effects:

$$E[(y_{i,t+p} - y_{i,t+q})\eta_i] = 0 \,;\, E[(X_{i,t+p} - X_{i,t+q})\eta_i] = 0 \qquad \forall\ p,q \tag{7}$$

The additional moment conditions for the second part of the system, the regressions in levels are:

$$E[(y_{i,t-s} - y_{i,t-s-1})(\eta_i + \varepsilon_{i,t})] = 0 \quad s = 1, \tag{8}$$

and

$$E[(X_{i,t-s} - X_{i,t-s-1})(\eta_i + \varepsilon_{i,t})] = 0 \ \ s = 1 \tag{9}$$

The extra orthogonality conditions help in gaining identification in the case where the dependent variable (institutions) comes from a very persistent series. For instance, in the extreme case of a unit root the difference, $(y_{i,t-s} - y_{i,t-s-1})$ would be a pure innovation and so the best instrument for an AR (1) process. For all the estimates we present a test for the extra assumptions

17 Monte Carlo experiments show that the weakness of the instruments can increase the small-sample bias of the GMM estimates and make inference unreliable, Staiger and Stock (1997), Stock et al. (2002).

[18] It is worth noting that the bias properties under weak instruments, in this case the presence of a highly persistent series, are well known only for the autoregressive coefficient.

required by the System-Estimator. As in Huang and Temple (2005), we compare the model that considers the assumptions of eq. (5) and eq. (6), and the model that adds the assumptions of eq. (7) and eq. (8). The test consists in estimating both the unrestricted and the restricted models using the two different sets of moments conditions, and comparing their (two-step) Sargan/Hansen statistics using an incremental Sargan test of the form:

$$D_{RU} = n(J(\tilde{\gamma}) - J(\hat{\gamma})) \sim \chi_r^2 \qquad (10)$$

Where $J(\tilde{\gamma})$ is the minimized GMM criterion for the restricted model, and $J(\hat{\gamma})$ for the unrestricted model, *n* is the number of observations and *r* is the number of restrictions. Bond et al. (2001), and Bond and Windmeijer (2005) discuss the statistical properties of these tests.

To test the validity of our preferred specifications we will consider two sets of commonly adopted tests. The first is a Sargan/Hansen test of over-identifying restrictions and the second is the Arellano and Bond autocorrelation test. The latter examines the hypothesis that the error term $\varepsilon_{i,t}$ is not serially correlated in the equation in levels. We choose the model's lag structure and the instrumentation strategy by looking at the best combination of results from these tests. To avoid model "over fitting" that is, having more instruments than countries- throughout the paper our reduced instrument set uses no lags longer than *(t-4)*. As discussed in Bowsher (2002), using fewer moment conditions improves the power of the Sargan/Hausman test.

Estimation Results

This section presents the results of our panel data analysis in which we estimate regressions in the form of equation (3). We consider a dynamic specification for institutions and two different information sets. The first one, the basic information set, considers GDP and the lag of institutions as the only control variables to explain the level of institutions. The second one, the full information set, considers different control variables depending on data availability.

For instance variables representing human capital are not available on annual basis and thus are not included in the full information set in the annual regressions.

The main findings can be summarized as follows. Increases in the flow of aid are not followed by improvements or declines in the quality of economic institutions. For the whole sample, and sub samples used in the robustness check, such as considering Sub-Saharan countries only, or East Asian countries, this result is robust across different estimation methods, and to variation in the choice of moment conditions. Not surprisingly the annual data results are less precise due to the instrumentation strategy but in all the cases we do not reject the restriction of b1+b2=0 representing a 2 year effect of the flow of aid on institutions (see Table 8). [19]

For all the specifications we consider OLS, and Within Groups (WG) estimates, and two versions of the "Differenced" GMM estimator. All models include a full set of time dummies. The first version of the Dif-GMM (1) is the one-step version of the GMM estimator with White-Huber heteroskedasticity-robust standard errors, while the Dif-GMM (2) is the two step version of the GMM estimator. The Dif-GMM (2) estimator always uses the small sample correction for standard errors of the two step GMM estimates, (see Windmeijer (2005)). We also consider a two-step version of the System estimator, with standard errors robust to heteroskedasticity and autocorrelation and Windmeijer (2005) corrected. For all the different GMM estimators, for the sake of consistency of the results, we consider the same set of instruments following the selection rules discussed in the previous section.

Table 9 reports the first set of results on the legal structure/property rights variable from the Fraser Institute. As the table shows, there are reasons to believe that this model and the selection of the instrument is properly specified and that the first lag of the dependent variable is

[19] Though the coefficients are individually significant and of opposite sign during the first and second years, for the annual data we believe that this reflects noise in the annual data rather than a systematic effect of aid on institutions that changes sign over the years. In any case, the 2 year effect is zero.

sufficient to account for the persistency of institutions. All the specification tests in both the System and the Difference estimators pass the desired thresholds. Moreover, the incremental Sargan test does not reject the null hypothesis; the extra moment conditions are valid. It is worth noting that the autoregressive parameters estimated by both Dif-GMM and the Sys-GMM are both positive and statistically significant and do lie in the interval defined by the OLS and the WG estimates. This confirms the validity of our instruments and it also hints that, for this specification, the instruments are not weak. Looking at all the tables together, we note that, across all the estimation methods, there is strong evidence that an increase in (the flow) of aid does not affect the level of institutions.

Table 8: Aid and Institutions: Basic Specification, Annual Data, GMM

Dep. Var: $I_{i,t}$	*Corruption*	*Bur. Qual.*	*Repudiation*	*Rule of Law*	*Exp Risk*
$I_{i,t-1}$	0.893**	0.876**	0.908**	0.906***	0.92**
	(0.041)	*(0.043)*	*(0.038)*	*(0.037)*	*(0.034)*
$\ln(AID_{i,t-1})$	0.024	0.016	0.097*	0.084**	0.125**
	(0.032)	*(0.037)*	*(0.050)*	*(0.036)*	*(0.051)*
$\ln(AID_{i,t-2})$	-0.04	0.047**	-0.041	-0.059**	-0.074*
	(0.030)	*(0.018)*	*(0.045)*	*(0.026)*	*(0.043)*
$\ln(GNPPC_{i,t-1})$	0.03	0.097**	0.224**	0.117**	0.055
	(0.041)	*(0.038)*	*(0.071)*	*(0.042)*	*(0.045)*
Unit Root					
$\alpha = 1$	*[0.010]*	*[0.004]*	*[0.018]*	*[0.012]*	*[0.021]*
Long Run Effects (1)	-0.153	0.524	0.61	0.265	0.636
$\frac{\beta_1+\beta_2}{1-\alpha}=0$	*[0.616]*	*[0.153]*	*[0.154]*	*[0.398]*	*[0.207]*
Long Run Effects (2)					
$\beta_1+\beta_2=0$	*[0.585]*	*[0.143]*	*[0.171]*	*[0.400]*	*[0.146]*
Sargan/Hansen Test	*[0.213]*	*[0.646]*	*[0.765]*	*[0.316]*	*[0.258]*
m(1)	*[0.001]*	*[0.000]*	*[0.000]*	*[0.000]*	*[0.000]*
m(2)	*[0.034]*	*[0.241]*	*[0.153]*	*[0.015]*	*[0.163]*
No. Observations	877	877	877	877	877

Notes:
Reported here are the results using two step system GMM estimation. Robust standard error are reported in "()"
The dependent variables are the annual level of Expatriation Risk,Bureaucratic Quality, Repudiation, Epropriation Risk, Corruption (ICRG). Robust standard error are reported in *()* parenthesis.
The value reported for the GMM estimations are computed considering a reduced instrument set. We use lags *t-2* up to *t-3* of the dependent variable and lag *t-2* only of Aid, and the log(gnppc) in both the difference and the system estimations, while the first lag differences of Institutions, Aid and the log of gnppc are added (under the mean stationarity assumption) in the system estimation.
Long Run Effects (1): is the test (b1+b2)/(1-a)=0 and Long Run Effects(2): is the test (b1+b2)=0.
Long Run Effects (1) is a non linear test of restrictions where the standard errors are computed using the delta method, while Long Run Effects (2) is just a test for the numerator and is a linear test of restrictions.
Estimated coefficient and robust standard errors for the time dummies are omitted but available on request.
The values reported for the Sargan/Hansen test are the p-values for the null hypothesis of instrument validity
The values reported for m(1) m(2) are the p-values for the Arellan-Bond test for AR(1) and AR(2) disturbances in the first differences equations. Under H0: it is tested if the residuals are not first (second) order serially correlated."*[ ]"* report the p-values of the tests.
Data are for 12 years, between 1984-1995, as used in Alesina et al. (2002).

Table 9: Aid and Institutions: Legal Structure, Basic Specification, five-year Data.

Dep. Var: $_{i,t}$	*W.G*	*OLS levels*	*Dif-GMM(2)*	*SYS-GMM*
$I_{i,t-1}$	0.173**	0.598**	0.258**	0.318**
	(0.059)	*(0.041)*	*(0.075)*	*(0.074)*
$\ln(AID_{i,t-1})$	0.182	0.093**	0.163	0.016
	(0.095)	*(0.041)*	*(0.245)*	*(0.111)*
$\ln(GDPPPC_{i,t-1})$	0.505	0.454**	-0.206	0.532**
	(0.359)	*(0.083)*	*(0.850)*	*(0.246)*
Unit Root				
$\alpha = 1$	*[0.0000]*	*[0.0000]*	*[0.805]*	*[0.009]*
Sargan/Hansen Test	-	-	*[0.157]*	*[0.104]*
Incremental Sargan	-	-	-	*[0.190]*
m(1)	-	-	*[0.003]*	*[0.001]*
m(2)	-	-	*[0.236]*	*[0.241]*
No. Observations	331	331	237	331

Notes:
The dependent variable is the five year level of Legal Structure (FRG). "W.G" is the Within Groups estimation. "Dif-GMM(2)" is the two step step difference GMM estimation.
"SYS-GMM" is the two step system GMM estimation. Robust standard error while not robust for the Within Groups estimator. More precisely, in columns (v) and (vi) are computed *Windmeijer's* corrected standard errors, while Huber-White sandwich corrected estimates in column (iv).
Column (iii) reports Praise-Winsten AR(1) regressions corrected, and for heteroskedasticity a la' White.
The value reported for the GMM estimations are computed considering a reduced instrument set. We use lags *t-2* up to *t-4 f*or all the estimators and for all the instruments. Estimated coefficient and robust standard errors for the time dummies are omitted but available on request.
The values reported for the Sargan/Hansen test are the p-values fot the null hypothesis of instrument validity.
The value reported for the Incrental Sargan test is the p-value for the joint tests the extra moments used for the system estimator. Under Ho: we test the validity of the extra moments.
The values reported for m(1) m(2) are the p-values for the Arellan-Bond test for AR(1) and AR(2) disturbances in the first differences equations.
Under H0: it is tested if the residuals are not first (second) order serially correlated.
"[ ]" report the p-values of the tests. Data are for 5 years, between 1970-2000.
** Significant at 5 %, * Significant at 10%

Table 10: Aid and Institutions (GMM system): Basic Specification, five-year Data

Dep. Var: $I_{i,t}$	*Property*	*Regulation*	*C.I.M*
$I_{i,t-1}$	0.318**	0.684**	0.768**
	(0.074)	*(0.079)*	*(0.122)*
$\ln(AID_{i,t-1})$	0.016	-0.014	0.0008
	(0.111)	*(0.048)*	*(0.004)*
$\ln(GDPPPC_{i,t-1})$	0.532**	0.07	0.026**
	(0.246)	*(0.098)*	*(0.013)*
Sargan/Hansen Test	*[0.104]*	*[0.434]*	*[0.860]*
Incremental Sargan	*[0.190]*	*[0.784]*	*[0.875]*
m(1)	*[0.001]*	*[0.001]*	*[0.003]*
m(2)	*[0.241]*	*[0.694]*	*[0.642]*
No. Observations	331	332	438

Notes:
The dependent variable is the five year level of Property, Regulation and CIM, respectively. "SYS-GMM" is the two step system GMM estimation. Robust standard, error are reported in "()" which is computed *Windmeijer's* corrected standard errors. The value reported for the GMM estimations are computed considering a reduced instrument set. We use lags *t-2* up to *t-4 for all the estimators and for al the instru-*ments.
Estimated coefficient and robust standard errors for the time dummies are omitted but available on request. The values reported for the Sargan/Hansen test are the p-values fot the null hypothesis of instrument validity. The value reported for the Incremental Sargan Test are the p-values to test the moments used for the system estimator. Under Ho: we test the validity of the extra moments.
The values reported for m(1) m(2) are the p-values for the Arellan-Bond test for AR(1) and AR(2) disturbances in the first differences equations. Under H0: it is tested if the residuals are not first (second) oreder serially correlated."*[ ]"* report the p-values of the tests. Data are for 5 years, between 1970-2000.
** Significant at 5 %, * Significant at 10%

Only in the pooled OLS estimation is there an effect of aid and it is positive. Previous work based on pure cross-section data, the Kilby (2004) panel data and the Djankov et al. (2005) estimations also find the OLS estimators to be significant. The effects of economic development on economic institutions are positive and statistically significant at 5%. This result confirms that countries' legal structure is affected by the level of economic development. In all the specifications we observe that the autoregressive coefficient of institutions is positive and statistically significant. Given the discussion of the previous section we interpret only the estimates of the Differenced and the System estimator. Institutions are persistent and the probability of having higher level of institutions today is monotonically increasing in the level of past year's value of institutions.

Table 10 reports the summary of the System Estimation for our legal structure/property rights variable, regulation and contract intensive money. As expected the estimated coefficients for the lagged dependent variable are always positive and statistically significant at 5%. The coefficient on the level of (the natural log) GDP per capita is positive and significant for legal structure and contract intensive money at 5% while it is not for Regulation. Although, we have considered a very parsimonious specification given the few theoretical models and possible explanations for the evolution of economic institutions, all the tests for specification and instruments validity suggest that the selected model is well specified. For instance, for all three indicators the Sargan/Hansen tests do not reject the null of instrument validity and the Arellano-Bond test for autocorrelation rejects the null of first order serial correlation for the AR(1) type, but not for the AR(2) type. All the incremental Sargan tests suggest that the extra assumptions used in the "System-GMM" estimation are valid. Finally, the unit root tests suggest that although institutions are very persistent they do not have a unit root.

All the estimation details for regulation and contract intensive money are not reported but available on request. For both indicators we followed the same rules to select the best specification and the number of instruments as discussed for legal structure. Table 8 reports the estimation results for the five ICRG indicators. We report the "System" estimates only though others are available on request[20]. The table reports tests for the presence of a unit root and for long run effects (or rather the effect over two years). For all the specification and for all the indicators we find no evidence of a joint effect of aid. In contrast to Alesina and Weder (2002), once we consider the "System" estimator we sometimes find that both the first and the second lag of aid are statistically significant but with opposite sign (except for bureaucratic quality which is positively associated with more aid). We also considered the specification with contemporaneous aid and the first lag of aid and observe the same change in signs). In contrast to previous results, we also test the joint (2 year) effect of aid on economic institutions we confirm the zero effect as in the five year data.

Conclusion

This paper examines the effect of aid on institutions, specifically institutions that affect economic transactions between private agents or private agents and the state. It excludes institutions that regulate political relationships between the state and citizens. Thus our focus is not on institutions of democracy or those that determine political stability or voice and accountability (though these may affect economic outcomes). In the very long run, one may argue that the nature of economic and political institutions is linked. In the shorter run, as the paper discusses, we do not see convergence. A priori, there is no reason why there should be a 1-1 relationship between the precise design of political institutions and the effectiveness of

[20] For the annual data we follow the same procedures to select the model and to assess that the estimates are indeed bounded between the within group estimator (fixed effect) and the pooled OLS.

economic institutions (which also vary in design). Moreover, a continuous process of what might be termed “shocks” for want of a better word, such as changes in external trade, natural events, technology, or health, may provide windows of opportunity for changing certain institutions (for example economic institutions) before they have an impact on other types of institutions.

So we ask to what extent aid can affect economic institutions. This is an important question since donors attempt to raise growth in recipient countries not just by providing capital for investment but by advising on policies and institutions designed to raise the productivity of investment. We find no empirical evidence to suggest that aid has a robust and significant impact on institutional quality using several measures of institutions. The only exception to this was aid’s impact on regulation as defined by one indicator from the Fraser Institute (aid is seen to favor more regulation).

These results suggest that in general aid cannot be said to have affected institutional quality. One caveat is that the data could be refined much further. For example, the influence of aid targeted to a particular sector could be assessed with measures of institutional quality in that sector. For this, more work on clearing up the aid data is needed and better estimates of institutional quality would help. We tried to assess the impact of aid in a particular sector, the financial sector, but found no effect.

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Data Appendix:

Variable	Code	Data source	Note
Initial GDP per capita	LGDP	Summers and Heston, 1991, updated using GDPG	Natural logarithm of GDP/capita for first year of period; constant 1985 dollars
Institutional quality 1	Prop, Reg	Fraser Institute: http://www.freetheworld.com/2005/2005Dataset.xls	Disaggregate indicators, available on the web.
Institutional quality 2	ICRG	PRS Group's IRIS III data set (see Knack and Keefer, 1995)	Revised version of variable. Computed as the av-erage of the three compo-nents still reported
Institutional quality 3	CIM	Contract Intesive Money, IFC data, 2005	M2=M1+quasimoney=34+35 Currency comes from line 14a of International Financial Statistics, "currency outside deposit money banks." *M* 2 is defined by IFS as the sum of money and quasi-money, or the sum of lines 14a (currency outside banks), 24 (demand deposits), 15 (time deposits
Sub-Saharan Af-rica	SSA	World Bank, 2003	Codes nations in the south-ern Sahara as sub-Saharan
East Asia	EASIA	Dummy for China, Indonesia, South Korea, Malaysia, Philippines, and Thailand, following Burnside and Dollar	
Effective Devel-opment Assis-tance/ real GDP	AID	Chang et al., 1998; DAC, 2002; IMF, 2003; World Bank, 2003; Summer and Heston, 1991	Available values for 1975–95 from Chang et al. Miss-ing values extrapolated based on regression of EDA on Net ODA. Con-verted to 1985 dollars with World Import Unit Value index from IMF, series 75. GDP computed like LGDP above
Net Overseas De-velopment Assistance/real GDP	ODA	DAC, 2002; IMF, 2003; World Bank, 2003; Summer and Heston,	Like AID exception using ODA from DAC Real GDP expressed in PPP
Population	LPOP	World Bank, 2003	Natural logarithm
Mean years of secondary schooling among those over 25	SYR		Barro and Lee, 2000

Tables and Figures:

Table A1: Correlations between economic and political indices, 1970 and 2000

1970

	prop	reg	polity4	przdemaug	fhpolrigau
prop	1				
reg	0.2424	1			
	0.1606				
polity4	0.5156	0.569	1		
	0.0002	0.0001			
przdemaug	0.4296	0.4506	0.868	1	
	0.0023	0.0027	0		
fhpolrigaug	0.6278	0.6219	0.9017	0.8446	1
	0	0	0	0	

2000

	prop	reg	polity4	przdemaug	fhpolrigau
prop	1				
reg	0.6644	1			
	0				
polity4	0.377	0.4439	1		
	0	0			
przdemaug	.	.	.		
	1	1	1		
fhpolrigaug	0.5431	0.5662	0.9004	.	1
	0	0	0	1	

Table A2: Correlations between economic and political indicators, Average 1970-2000

Average 1970-2000

	prop	reg	polity4	przdem	fhpolrig
avg_prop	1				
avg_reg	0.4469	1			
	0				
polity4	0.604	0.4346	1		
	0	0			
przdem	0.5272	0.3697	0.8938	1	
	0	0	0		
fhpolrig	0.6712	0.5212	0.9565	0.8812	1
	0	0	0	0	

Notes: Table A1 and A2
Sources: prop rights index (from Fraser, see appendix), regulation (Fraser).

Polity4 (see above)
Fhpolrig: gmented Freedom House political rights index (w/Bollen data) (normalized 0-1)