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25 Years of Aid Allocation Practice: Comparing Donors and Eras

by

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Abstract

Focusing on seven bilateral donors over a 25 year period, the paper answers 4 questions related to aid allocation practice. Questions one and two examine allocation differences between donors and time periods. Questions three and four relate to changes in poverty and policy selectivity. To answer these questions a formal approach is used to quantify the effects of four factors that influence aid allocation: poverty, policy, proximity and population. The results reaffirm findings of large donor heterogeneity and the role of non-development influences. However, they dispute recent findings of large or growing policy sensitivity.

JEL Classification: F35, F50,

Keywords: Aid Allocation, Policy Selectivity, Poverty Selectivity, Two-Part Model.

Centre for Research in Economic Development and International Trade, University of Nottingham

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Outline

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1. Introduction

Both aid and its allocation have been much maligned in recent years. Criticism of the former has extended from econometric studies that fail to find a positive effect of aid on growth (see Roodman 2007 for a review), to more popular works that argue reductions in aid would promote development (Calderisi 2006;Glennie 2008;Moyo 2009). An obvious consequence of these critiques is to examine aid allocation itself. If aid is not found to promote development, is this an inherent feature of aid or the consequence of poor allocation practice by donors? Early research on Aid Allocation commonly proposed a dichotomy between donor interest and recipient need, and found the former dominated the later (e.g. McKinley and Little 1979), showing a possible source of distortion. However, there are three major reasons for thinking that aid allocation may have changed since that research was published. First, the end of the Cold War may have freed donors from security concerns to pursue a more development-centred allocation approach. Meernik et al. (1998, p.79) reported early evidence of an increased importance of poverty in allocation decisions in place of security concerns. In that same year it was reported policy-makers were still debating the proper role of aid in the post Cold War era (Schraeder, Hook, and Taylor 1998). Second, influential research (Burnside and Dollar 1998;2000) argued that aid worked in a good policy environment. The associated recommendation was that aid should change to reflect this conclusion, with a move from conditionality to policy selectivity (Collier and Dollar 2002). Third, the terrorist attacks of September the 11th 2001 in the USA saw large increases in American aid budgets and potentially large changes in allocation principles (Moss, Roodman, Standley, and Floor 2005).

In light of this, this research seeks to answer four questions. First, what are the donor differences? This is a static comparison of donor behaviour, to understand which factors guide which donors. In contrast with much of the literature, a formal approach is used to quantify differences between donors. This allows comparisons between donors, and between different factors that are thought to influence allocation practice. Second, what are the changes over time? As aforementioned, there is considerable reason to suspect general changes in allocation practice. Again, we employ a formal technique to identify any significant changes. The third and fourth questions are whether selectivity has

increase with relation to poverty and policy respectively. We employ different specifications in order to understand if the expected change has indeed occurred.

This research is both novel in technique and contribution to current debates. It applies an existing systematic approach to identifying differences in allocation behaviour between donors and over time within the more recent theoretical framework. This allows comparisons to be of a more formal nature. The econometric section includes an evaluation of the different techniques used and a strategy to test between them which are surprisingly atypical. This paper also corrects for the problems of serial correlation and aid volatility which are expected to be commonplace but seldom discussed. The salient contributions to our understanding of aid allocation are three fold. First, we are able to re-examine the effect of the end of the Cold War on allocation. Results suggest that early evidence for increased policy and poverty sensitivity are not easily replicated. Second, we find the evidence of a move from conditionality to selectivity doubtful. Third, the terrorist attacks of September 11th 2001 are shown to have had limited effect on aid allocation practice.

2 Literature Review

This research employs a positive approach and a small review of literature within that stream and directly relevant to the four questions proposed is included here.

Donor Differences

There has been surprisingly little attempt to formally test differences between donors, the common approach instead being a narrative description including some parameter differences. Berthélemy and Tichit (2004, pp.269-270) report the sign and significance level of the coefficients in their model for 18 bilateral donors. They find mixed evidence in support of the importance of recipient need, and find infant mortality to be a better predictor than income for many donors. Policy is significant for most donors, but the USA and Australia exhibit a special preference for democracy whereas France and Belgium both have a negative coefficient estimate. They do not report major differences in their *Donor Interest* variables, but state smaller donors focus regionally. Berthélemy (2006) divided donors into three categories on the basis of the estimated coefficient for the trade-aid relationship. Selfish donors (Australia, France, Italy, Japan and the UK)

have a positive relationship between aid and trade whereas Altruistic donors (Austria, Denmark, Ireland, Netherlands, Norway and Switzerland) have a negative relationship. Trade here is measured as the logged and lagged sum of imports and exports between the donor and recipient as a share of Donor's GDP. Alesina and Dollar (2000) report that for 3 donors their allocation is distorted by a single factor: for the USA it is Israel and Egypt, for France it is colonies and for Japan it is UN voting records. They find France and Japan to be insensitive to Poverty whereas the USA and the Nordic countries give more to poor, democratic and open countries.

The most robust finding when comparing donors is that Nordic donors are distinct. Alesina and Weder (2002) focus on the link between corruption and aid allocation over the period 1975-1995, both in aggregate and by individual donors. Using the Tobit technique for individual donors, they find Nordic donors tend to give less to corrupt recipients, whereas for other donors there is no robust relationship. They postulated that Nordic donors are freed from colonial ties and can thus be more sensitive to other considerations. Gates and Hoeffler (2004) explicitly test and confirm the idea that Nordic donors (Norway, Denmark, Sweden and Finland) are different, finding them to be more influenced by democracy and less influenced by trade, compared with other donors.

Differences Over Time

When examining the differences in donor behaviour over time, the influence of the Cold War (CW, hereafter,) and 9/11 are particularly salient. Meernik et al. (1998, p.79) reported early evidence that the end of the cold war meant a declining importance of security concerns, a large drop in aid transfers in aggregate and an increased importance of poverty in allocation decisions. Boschini and Olofsgård (2007) concur that the CW may explain the decline in aid volumes, but argue that it changed relatively little in allocation practice. Berthélemy and Tichit (2004) argue that the geopolitical concerns of aid allocation during the CW have been replaced not by increased poverty concerns (which they actually find decrease) but by trade relationships. Easterly (2007) finds that the CW changed little in terms of sensitivity to democracy, and Neumayer (2003a) finds it had no effect on the relationship with human rights.

Moss et al. (2005) study the effect of Global War on Terror (GWOT) on US aid allocation using various variables thought to capture *a priori* expectations. They find that essentially the effect of the GWOT was to substantially increase the aid for four countries (Iraq, Afghanistan, Jordan and the Palestinian territories) which was financed mainly by an augmented aid budget but also by reductions for three countries (Israel, Egypt and Bosnia and Herzegovina). Fleck and Kilby (2009) find that the GWOT coincides with increased aid volumes for the USA, and that poverty sensitivity has decreased in this period. Others have tried to capture the effect of the GWOT simply by employing a dummy for the period. This is only a satisfactory modelling solution if the effect of the GWOT was a universal one time-shift in aid allocation transfers for all recipients. While the research implies the role of the GWOT is smaller than might have been expected for the US, this does not seem a suitable solution.

Poverty and Policy Selectivity

The empirical research that argued 'aid works, with good policy' quickly resulted in aid allocation principles (Collier and Dollar 2002) and, it is thought, into policy implementations (Easterly 2003). It would be expected that this would lead to a greater weight for policy in allocation decisions, but also a greater focus on poverty as aid is seen as a possible solution. This move from conditionality to selectivity was being discussed surprisingly early in policy circles (Hout 2007a), but it is unclear whether this move was rhetorical or actual. Hout (2007b) examines the allocations of the Netherlands, USA and World Bank and provides evidence that policy selectivity has not increased within the last few years. Looking at selectivity over a longer time horizon, Easterly (2007) finds increased poverty sensitivity to have happened after 'the McNamara revolution' of the 1970s, with little change since then. Regarding policy, he concludes that "The overall picture is that there is little evidence that donors are learning to be increasingly selective with respect to policies in the recipient countries." (ibid. p.654) Nunnenkamp and Thiele (2006) report correlations and basic regressions from a similar exercise in support of the conclusion that aid is poverty but not policy-sensitive. Specifically policy-insensitive are Japanese and French aid, with the US not fairing particularly well. The poverty focus is found particularly strong for Scandinavian countries, Germany, Holland and the UK.

In contrast to the aforementioned research, are two papers that claim policy selectivity has increased, specifically since the 1980s. Examining 22 bilateral donors over the period 1980-1999, Berthelemy and Tichit (2004, p267) find evidence of increasing selectivity: "… donors give more recognition for good economic policies in the 1990s than in the 1980s." They use aid per capita as the dependent variable, arguing that this helps to examine the small-country bias, and a panel Tobit as the estimator, to capture fluctuations in the donor budget. The conclusion that aid allocation is more selective (in terms of economic policies) is based on the coefficient on lagged economic growth becoming positive and significant in the 1990s, whereas it was negative in the 1980s. FDI is also included in the specification: In the 1980s its coefficient was significant and positive, and in the 1990s it became insignificant and very small. Thus it is highly possible that the results are misleading, due to co-linearity. Leaving this aside, the use of lagged economic growth is highly problematic as a variable to capture 'good economic policies' for the following year. There are other potential explanations for aid 'following' growth that are as compelling as it signifying a concern for economic policy. For example, it may be desirable for a donor to be associated with a country that is growing relatively quickly. Alternatively, a selfish donor may use aid to promote its own exports, by allocating more aid to countries with an expanding market. Then again, it may be that donors imagine that a country with a recent growth record is able to absorb more investment in the form of aid, or conversely that countries that have experienced a negative shock (such as a natural disaster which led to an economic downturn) require higher aid commitments. In short, the evidence does not seem to fully justify the conclusion.

Dollar and Levin (2006) examine a large number of donors, both bilateral and multilateral. They estimate using a Tobit, with logged Aid disbursements as the dependent variable. They use two variables as proxies for policy: the International Country Risk Guide (ICRG) rule of law index and Freedom House's democracy index, and GDP per capita data to inform conclusions on poverty sensitivity. They conclude, on the basis of their statistical analysis, that: "In the past two decades, foreign aid overall has become more selective [in terms of economic governance]" (*ibid.* p.2044) They find this increased selectivity is driven by multilateral agencies, whereas for bilateral donors economic governance has no statistically significant relationship with aid allocation. They

conclude that foreign aid *overall* is more selective in the early 00's than in the late 80's because more donors are significantly selective. However, this picture is highly misleading as in 2006 Multilateral Agencies represented only around 14% of all ODA commitments[1]. Aid donors may have become more selective on average, but has aid? This paper examines the strength of policy selectivity over the last twenty-five years, for the most important donors. By modelling only the most important donors, we hope to ascertain more closely the overall policy selectivity of aid. We thus hope to discover which set of papers reports the most robust result: Easterly (2007) and Nunnenkamp and Thiele (2006) who report static and low policy selectivity, or Dollar and Levin (2006) and Berthélemy and Tichit (2004) who report increasing policy selectivity.

3 Econometric Approach

The early dichotomy of recipient need and donor interest gave way to the hybrid model, where both factors had some influence over aid allocation, given by $A_i = F[RN_i, DI_i]$. Instead of this model, we estimate the following by donor.

$$A_i = \alpha + \beta_1 Poverty_i + \beta_2 Population_i + \beta_3 Policy_i + \beta_4 Proximity_i + \varepsilon_i$$

This is similar to Neumayer (2003), and hereafter referred to as a 4P specification. As in the RN-DI approach, *Poverty* describes the level of need in a potential recipient country. As poverty is thought to be a major motivation for aid, the a priori expectation is for this to be positive ($\beta_1 > 0$). *Population* is another standard variable, and expected to be positive ($\beta_2 > 0$). As discussed, *Policy* is a relatively recent addition to the theory of aid allocation and can be understood in a number of ways. Here it contains two main strands. The first is the ability of a recipient to turn a given amount of aid into a desirable outcome in the mind of the donor. This conceptualisation is similar to the normative proposal of Collier and Dollar (2002). The second aspect includes desirable characteristics of a donor that are not need-related, in the mind of a donor. For example, it may be argued that the USA value democracy inherently and for ideological reasons rather than any effect on poverty reduction. Both parts of policy would be expected to have a positive relationship with aid allocated ($\beta_3 > 0$).

1 Authors Calculations, based on OECD data, where all commitments are included.

Donor Interest was conceived as essentially incompatible with recipient need, a formulation still used (e.g. Berthélemy 2006, who uses the hybrid approach). In the original framework, Recipient Need and Donor Interest are mutually exclusive and a donor must choose between increasing their own welfare and that of recipients. *Proximity* by contrast includes DI but can take many forms including religion, language, culture, history, geography and commerce. This wider understanding means factors that are less obviously in the direct interest of a donor can also be included. For example, it may be *altruistic* for a donor to give to recipients if they share a common language if doing so would decrease transaction costs and increase the value of aid. Indeed, given the critique of aid on the basis of fragmentation, it is perhaps the most sensible way for donors to choose which recipients to focus upon. Neither *Poverty* nor *Policy* change by donor but rather by recipient. If donors weight these factors in similar ways, there is no guidance of how to choose which recipients to focus upon. Instead, *Proximity* may suggest which recipients a donor should focus on, with possible efficiency gains due to lower linguistic or cultural barriers. Whether the motivation for allocations being influenced are good or ill, the expectation is that the relationship will be positive ($\beta_4 > 0$).

There has been surprisingly little work done which formalises the relative import of each factor on a donor's aid allocation. Often the approach has been to ascribe levels of effect to specific variables, rather than the factor which may be represented by a number of variables. One exception to this is the work by McGillivray (2003, pp.8-9) on comparing DI and RN models, which is easily extended from RN-DI to the 4P setting (Hoeffler and Outram 2008). He argues that the fairest test between competing models is simply a joint test of significance on the group of variables representing that factor after a regression of the full model. This test is a test of the following as the null hypothesis, for all of the coefficients that represent the factor (e.g. for Proximity that might include variables for trade, colonial history and shared language):

$$H_0: \gamma_1 = \gamma_2 = \cdots = \gamma_\infty = 0$$

The rationale of the test is to apportion explanatory power to the various competing hypotheses. Each may be significant, but at different levels. It is also informative when a

number of variables represent a single factor, as the test provides information on the factor level, not just the variable level. Having decided the general approach, there remain two main decisions: estimator and specification.

Which Estimator?

Data on aid flows are left-hand censored, i.e. many data points on aid transfers are zero. As such, OLS estimation would be biased, as the data is not normally distributed but instead clustered at the zero bound (see Figure 1). There are various estimators that can be used, which I will discuss these in turn, along with their assumptions and some relevant tests. For a more technical presentation of the estimators, see Cameron and Trivedi (2005, chp.16) or McGillivray (2003, with application to aid allocation).

Figure 1: OLS and Latent predictions of Aid Allocations

OLS

The simplest estimator, used recently in the aid allocation context by Alesina and Dollar (2000), is simply OLS. They use this estimator only when estimating 'average donor performance', and argue that the number of 0s is small enough for the bias to be of little consequence (Alesina and Dollar 2000, p.42). While this may be possible when the percentage of data censored is small, this is unlikely to be the case when estimating by donor. Figure 1 shows the predicted line when estimating using OLS, if all data are

included. The shaded dots show observed aid, whereas the hollow points show aid if it were allowed to be negative. The use of OLS then biases the predictions of beta toward zero, as the observed dependent variable understates the effect of the independent variables.

Censored Models

There are a number of different estimators that have been conceived with censoring in mind. Terminology is inconsistent in the literature, as the main authors use different names for the same estimator. Semantic broadening has further complicated matters, as Tobit is now often used to denote any parametric model that deals with censored data. Here we present 3 models that by that definition are Tobit models, each of which has been used to examine aid allocation behaviour in some form. Tests are presented alongside the estimator, and then the testing strategy is implemented. To briefly summarise, one major difference between the three types of Tobit is their assumption of the relationship between the two stages of the allocation process (eligibility and level). Tobit type 1 assumes each independent variable has the same effect in both the first and second stages. This allows the two stages to be estimated together, as if one were a continuation of the other. The Two-Part Model assumes that the two stages are completely independent, allowing the two stages to be estimated completely separately. The Heckman model normally assumes that there is at least one variable that strongly influences at the selection stage, but not at the level stage, allowing identification and correction of the bias.

Tobit Type 1

The basic (Type 1) Tobit model has a dependent variable that is left-hand censored, with homoskedastic, normally distributed and additive errors.

$$y^* = \alpha + x'\beta + \varepsilon \qquad (1)$$

Where

$$y = \begin{cases} y^* \; if \; y^* > 0 \\ - \; if \; y^* \leq 0 \end{cases} \qquad (2)$$

And

$$\varepsilon \sim N[0, \sigma^2] \qquad (3)$$

Where y^* is the 'true' or latent variable (as in Figure 1). Censored data is denoted by -, in this case at the zero bound, meaning y^* is only observed when positive. The error terms are assumed to be normally distributed. The Tobit (type 1) then in effect estimates the chance of censoring at the same time as estimating the value of y, if not censored. This is commonly estimated using maximum likelihood theory, and has been used extensively in the aid allocation context in both pooled and panel data contexts (Alesina and Dollar 2000;Alesina and Weder 2002;Berthélemy and Tichit 2004;Dollar and Levin 2006). The Tobit, despite its popularity, does suffer from two rather strict assumptions that should at least be tested. The first is that the error terms, as shown in Equation 3, are normally distributed. Specifically problematic is the assumption that the errors are homoskedastic. In the presence of heteroskedasticity, estimates become inconsistent and the model performs poorly in Monte-Carlo tests (Arabmazar and Schmidt 1982;Khan and Powell 2001). Skeels and Vella (1999) derived a test, suggested in Pagan and Vella (1989), which tests this assumption. The alternative conditional moment test is shown, by Drukker (2002), to have essentially no size distortion and reasonable power and is thus the one used in the diagnostic section. The second assumption is that the effects of independent variables are constant for the selection process and the outcome of interest (Smith and Brame 2003). This means, in this context, that not only do the same variables affect both which countries receive aid, and how much, but that the size of their effect is the same. A further concern in the aid allocation context is the assumption of normality, but the model can easily be extended to include a more appropriate lognormal formulation (Cameron, A C and Trivedi 2009, p.531).

The Two-Part Model

This presentation of the Cragg model follows Amemiya (1985, p.387), but it is alternatively described as a Cragg, Type 2 Tobit or (double) hurdle model. It is given by

$$\begin{aligned} y_1^* &= \alpha_1 + \boldsymbol{x_1}'\beta_1 + \varepsilon_1 \\ y_2^* &= \alpha_2 + \boldsymbol{x_2}'\beta_2 + \varepsilon_2 \end{aligned} \qquad (4)$$

Where

$$y_1 = \begin{cases} 1 & if\ y^* > 0 \\ 0 & if\ y^* \leq 0 \end{cases} \qquad (5)$$

$$y_2 = \begin{cases} y_2^* \; if \; y^* > 0 \\ - \quad if \; y^* \leq 0 \end{cases}$$

In contrast to the Tobit (type 1) model, this formulation shows the two stages to be independent, with y_1^* describing the censoring decision, and y_2^* the level decision. The Two-Part Model adds the assumption that $Cov[\varepsilon_1, \varepsilon_2] = 0$, which means the two equations found in (4) can be estimated completely separately. The second stage includes only positive values (i.e. the censored data denoted by – is excluded), and thus results should only be used to make inferences for those countries receiving aid (as only aid recipients are included in the equation). Neumayer (2003b) and Berthélemy (2006) follow Dudley and Montmarquette (1976) in using this estimator. As the two equations are assumed to be independent no exclusion restriction applies, thus circumventing the problem of identifying a regressor that influences only the first equation (which applies in the Heckman case). The validity of the estimator relies on the assumption of independence of the error terms, and can be tested directly. However, Neumayer (2003b, p.38) cites evidence that the bias leading from breaking this assumption is small (Manning, Duan, and Rogers 1987).

Heckman

Berthélemy (2006) uses the Heckman estimator (or sample selection model) in the context of aid allocation[2]. Essentially, the Heckman approach differs by treating the selection bias as a problem of omitted variable bias. It estimates in two stages, the first a selection equation and the second a level equation. Puhani (2000) provides a survey of the Monte Carlo evidence regarding the decision between Heckman and Two-Part models. He concludes that Heckman is particularly inefficient where there is either a large proportion of censoring or correlation between the errors of the two-stages. Also, he points to problems when the regressors of the two stages are correlated, which would result in the inverse Mills ratio being collinear with the other regressors. The model is also criticised for making strong distributional assumptions (Little and Rubin 1987, p.225). More problematic still is the need for an exclusion restriction, meaning there must be some variables that are only included in the first stage of the regression. What is needed is "a variable(s) that can generate nontrivial variation in the selection variable but

2 However it is not clear which, if any, variable is used as an exclusion restriction as the eligibility stage is not reported.

does not affect the outcome variable directly" (Cameron, A C and Trivedi 2009, p.543). While the Heckman *can* be calculated without this restriction, it would be done so using only the nonlinearity of the functional form (Puhani 2000, p.57). In practice, it is often difficult to find a variable that influences the selection without influencing the level. In the aid allocation context, it is likely that any variable that influences whether a country receives aid will also influence how much aid they receive.

Diagnostics

The easiest estimation technique to exclude in this case is the Heckman model. As discussed, Monte Carlo evidence suggests general problems with the technique. However, the main reason for rejecting the model is instead the lack of an obvious exclusion restriction and resulting problems if one cannot be found. Tests of the aforementioned assumptions must then decide between the Tobit type 1 and Two-Part Models. The diagnostics to be calculated are then evidence of independence between the two stages, as well as normality and homoskedasticity tests. Testing the Two-Part model is relatively easy as the same normality and homoskedasticity tests on the second stage can be used as for any standard OLS regression. The Tobit normality and homoskedasticity tests are due to Drukker (2002) and Cameron and Trivedi (2009, p.534-538) respectively. To illustrate the testing strategy, I use data previously used in aid allocation (Neumayer 2003b).

Table 1: Diagnostics To Choose Between Estimators

Estimator	Two-Part Model		Tobit		Between
Test for	Normality	Homoskedasticity	Normality	Homoskedasticity	Rho=0
Test Stat	60.54	1.07	15.86	1155.34	0.31
Prob.	0.00	0.30	0.00	0.00	0.58

In the Tobit case, the results lead to strong rejections of homoskedasticity and normality. For the two-part model, normality is rejected and homoskedasticity accepted. However, in the two-part model neither is a necessary condition for consistency (Cameron, A C and Trivedi 2009, p.541). The test on covariance between errors in different stages was calculated by running a Heckman model without an exclusion restriction, and independence (a key assumption of the Two-Part Model) cannot be rejected. These results point toward the Two-Part model as being the most appropriate.

Period Averaging

A further decision regarding the econometric approach is whether to use yearly data, or period averaged. Neumayer uses yearly data in his estimates and calculates clustered standard errors in the first step, and Huber/White/Sandwich estimator of variance in the second step. He reports that standard errors "are robust to towards arbitrary heteroskedasticity and serial correlation" (Neumayer 2003b, p.50.) It does not appear that these decisions regarding standard errors are in fact adequate to deal with serial correlation, as the Huber/White/Sandwich estimator is designed to deal with heteroskedasticity not serial correlation. The clustering of residuals is a complex topic, but does not by itself guard against bias from serial correlation. Tests for (first order) serial correlation using Neumayer's (2003b) data and specification reject the null hypothesis of no serial correlation for every bilateral donor[3] (see Drukker 2003 for information regarding this test). This is unlikely to be a problem confined to one paper, as much of the research is likely to suffer from this problem. The main strategy employed here to circumvent the problem of serial correlation is to use 5-year period averages. 5 year averages can be thought of as 'snap shots' of the average practice of that period. As annual data is not included, there is less opportunity for persistent independent variables to bias the estimated betas. This approach also diminishes the potential problem of high volatility in aid transfers, and divides the time period neatly into the three 'eras' used.

The Main Specification

It is worth noting throughout the discussion the trade off between data availability and specification accuracy. For example, when examining policy, many variables available do not exist for the first years of the data. It might also be desirable to use information on poverty rather than income, but sufficient data simply do not exist. Throughout the discussion there are similar trade-offs. A parsimonious specification is first chosen, with more information being used in robustness checks or to answer specific questions. This means the time period runs from 1982-2006, in five 5-year time periods. A full description of the data can be found in the appendix.

3 Tests performed on the level specifications found in Tables 5.4 and 5.5 (Neumayer 2003, pp.63-64).

Dependent Variable

The choice of dependent variable is more contentious than might be expected. In the framework used here, a government allocates a proportion of its budget in time *t* to aid. The income of the country, the proportion of that which becomes the government budget and the resultant allocation to the aid agency is all thought exogenous. From this budget a donor first decides between multilateral agencies and bilateral recipients. It then allocates between different recipient countries, and is influenced by four factors: poverty, population, policy and proximity. The paper models only the last step, thus treating all previous steps as exogenous. The dependent variable is then the logged percentage share of a donor's total aid budget commitments in a given year to a given recipient, that is $ln\ (\frac{100*aid_{ij}}{aid_j})$ where the subscript i refers to the donor, and j to the recipient. In this state it is normally distributed, which is desirable for the estimator used. Commitments are used as they more accurately portray the wishes of the donor. Other papers have used aid per capita, aid as a share of GDP and nominal aid as the dependent variable. The first does not reflect the decision that the donors make as closely, as donors commit aid in nominal terms for a specific period of time. The second approach means that the dependent variable is a function of aid commitments but also a recipient's income. This may overstate a poverty bias, as poorer countries will by definition receive 'higher' aid commitments. The third suggested variable is likely to be influenced by fluctuations in a donor's budget. Time dummies would correct for this to some extent, but not completely.

Poverty

Monetary measures of poverty are the most common due to the lack of reliable alternatives. Poverty headcount data simply do not exist on the scale needed, and for this reason logged *GDP per capita* data is used in the parsimonious specification. This is likely to be the data that the donor had access to when making the decision.

Population

Logged *Population* is used to capture what is in essence another indicator of need – the population. By making population a factor in its own right, and separating it from poverty, a donor's decision regarding China and India do not dominate the identification of poverty coefficients. While the population size in India and China presents a problem

for the applied researcher seeking to disentangle the effect of poverty and population upon aid receipts, this solution is more desirable than succumbing to the temptation to exclude them as special cases, which means ignoring the majority of the developing world. Furthermore, as we are modelling the impact of these factors in the mind of the donor, we can be reassured that this approach is similar to the approach found in donors' performance based allocation formulas that are available to the public (where typically income and population are included separately, the latter discounted at the rate $x^{0.6}$).

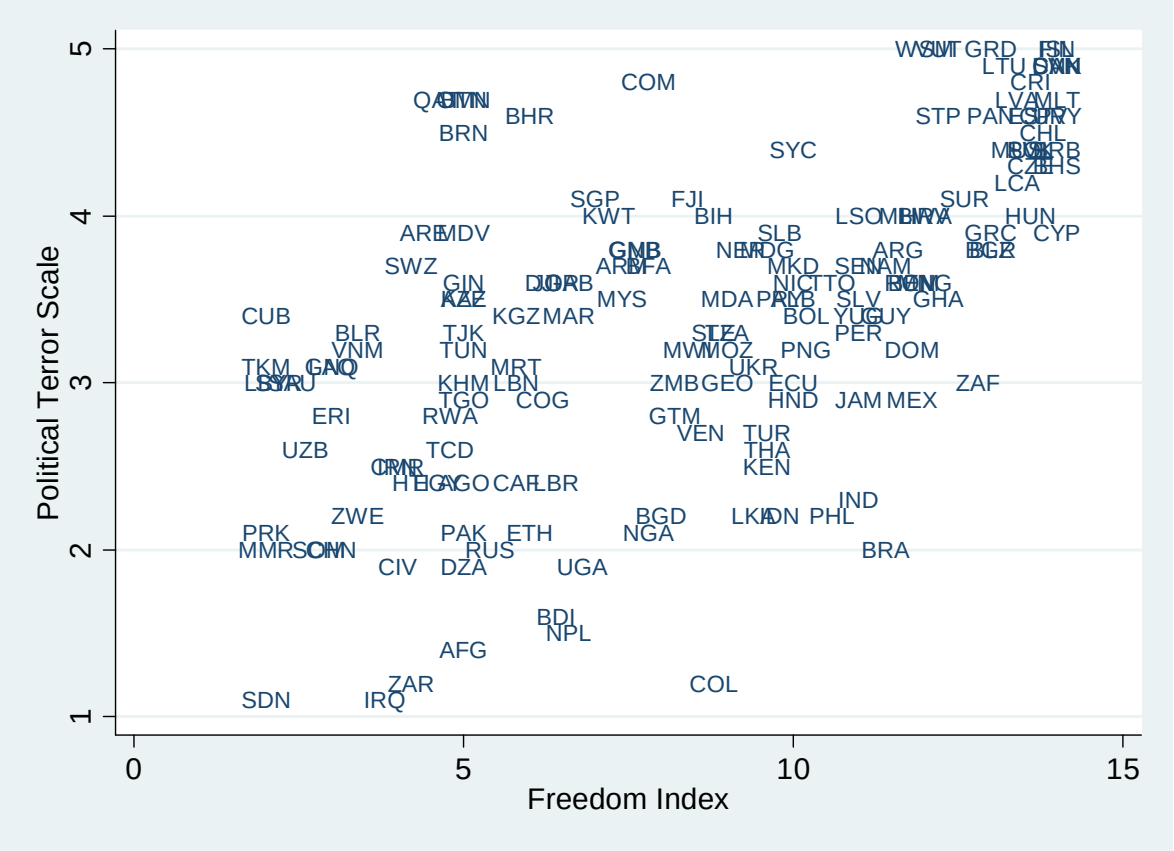
Policy

For the more parsimonious specification, many of the more sophisticated measures cannot be used due to data availability. We follow Neumayer (2003b) in using the *Freedom Index and PTS*. The *Freedom Index* is combined total of political rights and civil liberties, transformed to a scale from 2 (worst) to 14 (best), and taken from Freedom House. The Political Terror Scale (*PTS*) runs from 1 (worst) to 5 (best) and describes the level of terror or absence of the rule of law. The information is ultimately taken from two sources: Amnesty International and the US State department. While the data may or may not capture accurately the policy outcomes or inputs of a recipient, they are likely to capture the level of policy as perceived by donors. Indeed, they have been used by some donors explicitly (e.g. the Millennium Challenge Account). The data also suffer considerably less from missing data than alternate measures over the period examined.

To give a better understanding of the policy data, a few brief examples are presented. There are no signs of multicollinearity between the two variables, but they are positively correlated with a correlation coefficient of 0.51. Both are negatively skewed, with mean scores of 3.4 and 8.6 for PTS and Freedom respectively. In 2006 for the PTS, the nine countries to score four or worse were Afghanistan, Central African Republic, Columbia, Congo (DRC), Iraq, Myanmar, Nepal, Sudan and Sri Lanka. In 2006, the nine worst scores for Freedom were held by Cuba, North Korea, Libya, Myanmar, Saudi Arabia, Sudan, Syria, Turkmenistan and Uzbekistan. While there is obviously some overlap, the distinction is clear in many cases. On a regional level, African countries in 2006 scored higher on the Freedom variable (by 1.5) than the rest of the sample, but had no real difference in PTS. Some countries show low levels of democracy but an absence of

political terror: Qatar, Swaziland and United Arab Emirates. Others show relatively high scores for democracy but low levels for PTS: Brazil, India and the Philippines. However, the overall pattern is of a positive relationship between the two variables, as can be seen in Figure 2.

Figure 2: PTA and Freedom Index, 2006 with ISO Labels



Note: ISO labels are used as markers. Those of interest include: SDN Sudan, COL Columbia, IRQ Iraq, BRA Brazil, CUB Cuba, BRN Brunei, BHR Bahrain and COM Comoros.

Proximity

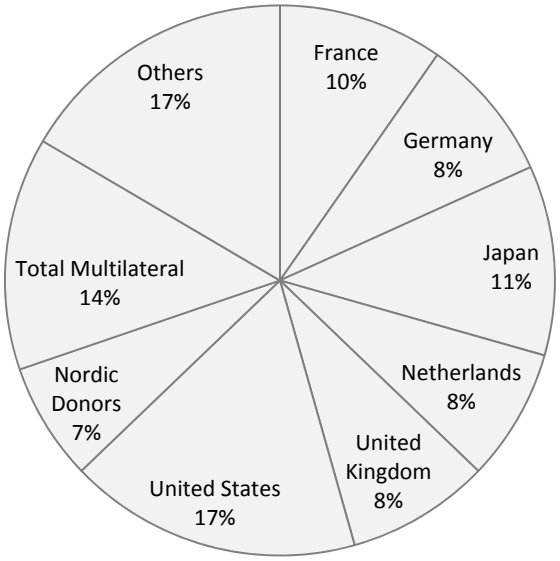
Proximity can be understood in many ways. *Religion*, *Language* and *Colony* variables describe the cultural and historical links between two nations. The religion variable measures the percentage of the recipient's population that adhere to the largest faith in the donor country. For example, for Japan it measures the percentage of recipient population that are Buddhist, and for USA Christian. *Language* is a dummy which takes the value 1 if at least nine percent of the donor and recipient populations speak the same language. This threshold is inherited from the data used (CEPII), but represents the most accurate

and complete dataset available on bilateral common languages. *Colony* is a similar dummy, but with colonial history. To capture trade interests, *Exports* is a variable which is the logged share of donor country exports that a recipient represents. This should capture the importance of the recipient to a donor's export sector. To measure military importance *Arms* is a measure of the total amount of arms exports from the donor to the recipient in that year. This should capture any particularly strategic military relationships, USA-Egypt for example. Another measure to capture military proximity, only available for the USA, is the value of American bilateral military aid transfers (these are not included in ODA).

A *Proximity Index* is constructed for use in answering questions 2-4, where proximity is included as a control rather than a variable of direct interest. While tests show multicollinearity is not a concern in later questions the size of the dataset is more restricted, and the index allows clearer interpretation by aggregating the cultural, religious, historical and military links. The proximity index was constructed by regressing Arms (as a dummy), US military Grants, Religion, Language and Colony and controls in a similar regression to that reported in Table 3. The relevant coefficients were then used as weights in the proximity index, which was scaled so that the 'closest' country received a score of 1, and the 'furthest' country received a score of 0. In this form, the coefficient in the level stage can be interpreted as the difference between the most and least proximate countries. For the second stage, standardised coefficients are used and so interpretation is also clear. Regressions show a negligible loss of information. This index means proximity in questions 2-4 is represented by only two variables: Trade and Proximity Index, and thus facilitates interpretation.

Which Donors?

Figure 3: 2006 ODA Commitments by Donor, as a % of Total Commitments[4]



The literature tends to either analyse either average donor behaviour (with deviations from this) or a collection of individual donors. We employ the latter method[5], focusing throughout on seven donors. The donors found in
Figure 3 are used, except that multilateral and 'others' are excluded, and Sweden is chosen to represent the Nordic Donors.

4 Results

Question 1: What are the Donor Differences?

In order to answer the above question, we estimate the parsimonious specification over the entire time period. Table 2 and 3 report the results for step 1 and 2 respectively, and Table 4 Wald statistics for the four factors.

4 Authors Calculations, based on OECD data, where all commitments are included.

5 This decision is motivated partly by a belief in the heterogeneity of donors which implies some difficulties in aggregating them for current research. Furthermore, 'average donor behaviour' can be a misleading term, as it is an unweighted average. Thus what is happening to donors on average could be quite different to what is happening to aid on average. This later concept will be dominated by the 7 donors chosen, who accounted for over 60% of aid commitments in 2006.

Table 2: 1982-2006, Parsimonious Specification, Eligibility Stage

	France	Germany	Japan	Netherlands	Sweden	USA	UK
Ln(GDP)	-0.44***	-0.36***	-0.42***	-0.46***	-0.30***	-0.64***	-0.47***
	(5.17)	(4.39)	(6.09)	(5.90)	(4.02)	(7.95)	(5.85)
Ln(Population)	-0.14*	-0.096	-0.20***	0.14*	0.29***	-0.12*	0.0046
	(2.06)	(1.43)	(3.64)	(2.44)	(5.06)	(2.01)	(0.080)
Freedom Index	0.039	0.038	0.10***	0.068**	0.12***	0.11***	0.077**
	(1.45)	(1.49)	(4.46)	(2.84)	(4.89)	(4.07)	(3.10)
Political Terror Scale	-0.28**	-0.25*	-0.34***	-0.32***	-0.10	-0.36***	-0.27**
	(2.63)	(2.30)	(3.33)	(3.45)	(1.22)	(3.43)	(2.77)
Religion	0.0064**	0.0077***	-0.00058	0.0069***	0.0030	0.0054*	0.0056**
	(2.99)	(3.39)	(0.12)	(3.65)	(1.77)	(2.42)	(2.74)
Arms	0.0056*	0.0092*		-0.0064	-0.033	0.00028	0.0037
	(2.04)	(2.36)		(1.57)	(1.50)	(0.52)	(1.76)
Exports	0.085	-0.99*	0.68**	-2.28***	-1.23*	0.25	-0.75
	(0.16)	(2.01)	(2.67)	(3.53)	(2.52)	(0.88)	(1.30)
Colony	-0.40	-0.43					0.20
	(1.67)	(0.98)					(0.94)
Language	0.54*			0.24		0.38*	0.39
	(2.04)			(1.75)		(2.24)	(1.71)
US Military Grants						-0.013	
						(1.66)	
Pseudo R-squared	0.153	0.182	0.118	0.293	0.159	0.255	0.201
Observations	532	532	527	570	530	523	531
Non-Recipients	96	90	91	201	296	123	138
Correctly predicted aid recipients	83.60%	85.71%	83.23%	79.71%	66.06%	83.07%	80.74%
Correctly predicted non-recipients	50.00%	54.29%	50.00%	73.29%	71.52%	70.27%	67.57%

Note: This was estimated using a Probit model, without clustered errors. Coefficients are not standardised. 3, 2 and 1 Star(s) denote the 1, 5 and 10 % significance levels respectively.

Table 3: 1982-2006, Parsimonious Specification, Level Stage (Standardised Coefficients)

	France	Germany	Japan	Netherlands	Sweden	USA	UK
Ln(GDP)	-0.055	-0.15**	-0.11*	-0.29***	-0.33***	-0.041	-0.27***
	(1.19)	(2.81)	(2.58)	(4.33)	(3.95)	(0.90)	(5.24)
Ln(Population)	0.35***	0.43***	0.42***	0.28***	0.24*	0.29***	0.44***
	(6.48)	(6.87)	(7.72)	(3.73)	(2.30)	(5.58)	(7.25)
Freedom Index	-0.026	0.057	0.093*	0.13*	-0.034	0.033	0.042
	(0.63)	(1.20)	(2.25)	(2.12)	(0.40)	(0.75)	(0.90)
Political Terror Scale	0.11*	0.0018	0.037	0.058	0.10	-0.084	0.059
	(2.36)	(0.035)	(0.76)	(0.94)	(1.17)	(1.75)	(1.20)
Religion	0.031	-0.047	0.13***	-0.0053	0.078	0.063	0.038
	(0.82)	(1.07)	(3.47)	(0.10)	(1.09)	(1.47)	(0.87)
Arms	-0.050	0.092*	0.058	0.29***	0.023	-0.10*	0.067
	(1.32)	(2.12)	(1.62)	(6.28)	(0.37)	(2.59)	(1.53)
Exports	0.29***	0.20***	0.34***	-0.024	0.016	-0.10*	0.087
	(5.48)	(3.37)	(7.41)	(0.34)	(0.17)	(2.28)	(1.40)
Colony	0.43***	-0.028				0.064	0.45***
	(9.37)	(0.70)				(1.89)	(9.06)
Language	0.19***			-0.11*		0.11**	-0.0011
	(4.48)			(2.30)		(2.78)	(0.021)
US Military Grants						0.67***	
						(17.0)	
Observations	436	442	436	369	234	400	393
Adjusted R-squared	0.554	0.390	0.472	0.257	0.123	0.563	0.509

Note: The second part is estimated using OLS, excluding those recipients that receive no aid. Standardised Coefficients are reported to assist interpretation – where beta reports the standard deviation change in Y resulting from a one standard deviation change in X Non-standardised coefficients can be found in the Appendix, Table A8. Following standard practice, this does not apply to the dummy variables (colony, language, religion) which are instead the standard deviation change in Y resulting from a one unit change in X. 3, 2 and 1 Star(s) denote 1, 5 and 10 % significance levels respectively.

Table 4: Wald Tests

		France	Germany	Japan	Netherlands	Sweden	USA	UK
1st Step	Poverty	6.950***	4.091**	11.55***	17.46***	9.454***	15.95***	11.30***
	Population	1.268	0.579	5.810**	2.678	16.41***	1.645	0.00251
	Policy	4.357	3.037	13.05***	8.406**	15.08***	11.90***	7.582**
	Proximity	6.11	6.669	3.506	27.06***	9.973**	11.84**	9.564*
2nd Step	Poverty	0.116	4.812**	2.374	18.52***	7.734***	0.827	14.16***
	Population	23.17***	26.14***	34.60***	2.011	2.279	24.05***	31.78***
	Policy	1.202	0.142	3.698	1.647	3.198	2.461	2.441
	Proximity	87.03***	8.492*	33.70***	19.14***	2.53	323.6***	63.82***

Note: The Wald Test statistics is shown, with stars denoting the 1, 5 and 10 % significance levels. The Wald statistic has a large-sample Chi-squared distribution.

Here, we can compare aid allocation between donors, having in essence estimated a donor's average allocation behaviour over a 25 year period. The Wald statistics allow us to attribute explanatory power to the competing factors of the 4P framework. Using these statistics, we find that all donors use income as a determinant of aid eligibility (less so for Germany and France), but the same is not true at the levels stage. Amongst the donors reported, we can identify three groups of poverty sensitivity: High (Netherlands, Sweden and the UK), medium (Germany and Japan) and low (USA and France). Looking at population, this is only a significant determinant at the eligibility stage for two donors. For Japan this appears to be in excluding larger countries (most probably a China effect), and for Sweden excluding smaller countries. Sweden selects fewer recipients than other donors, and so this is unsurprising. All donors have positive coefficients for population at the levels stage, and exhibit evidence of a small-country bias.

Four of the seven donors have significant Wald statistics at the eligibility stage for Policy, and none of the seven at the levels stage. Inspection of the coefficients shows that every donor has a positive relationship with the Freedom coefficient and a negative one for Political Terror Scores. Interestingly, for all donors the average PTS score for recipients of aid is higher than for non-recipients. This means that on average donors are more likely to give aid to countries with better human rights, but this result reverses when controlling for other factors. Tests show multicollinearity is not problematic. It may be that donors are less interested in human rights than other factors, and it is simply correlated with poverty or proximity. In the second stage France, Japan and the Netherlands exhibit some signs of Policy sensitivity, but not when tested overall. This implies policy sensitivity has not been a major feature of any donor's allocation principles when averaged over the last 25 years. This is particularly apparent when comparing the size of the Wald statistics with other factors.

Wald statistics show that the Proximity Variables are significant for every donor at some stage, but show large differences between donor in the level of significance and the constituent parts that underlie this significance. Germany and Sweden only have significant Wald statistics at one step, and a relatively low score in the other. For Sweden this manifests itself in a (weakly significant) negative coefficient on trade in the levels

stage and for Germany it is a positive coefficient on the trade variable at the 2nd Step. The Netherlands has a negative coefficient for trade but a positive coefficient for Religion at the first stage. At the levels stage it gives more to countries that purchase its arms and less to recipients with which it shares a language. For the Netherlands, many countries share a language as its own population are often multilingual, i.e. the dummy includes French, German and English speaking countries. The UK and France have much higher Wald statistics for proximity than those donors already discussed. For France almost all proximity coefficients at both stages are positive. The standardised coefficients show the biggest effect is for former colonies at the levels stage, but language and exports are also positive. For the UK, the biggest effect at the levels stage is of former colonies. For both France and the UK being a former colony results in 40% of a standard deviation increase in aid: roughly 0.7% of the aid budget for both donors.

The USA has an almost incomparably high Wald statistic for proximity. At the eligibility stage they are not too dissimilar from other donors. However, at the level stage they have positive and relatively large coefficients for US military grants and language. For the USA the colony dummy is identifying solely on the Philippines, and is thus effectively a dummy for the Philippines (which was a colony of the USA for almost half a century). The coding of the language variable (at least 9% of the recipient-donor pair speaking the same language) means this includes Hispanic America, and is thus positive at both stages. The military variable has the largest coefficient across the standardised coefficients, which suggests American aid is often used to reward or reinforce military relationships. It is likely that this relationship trumps any relationship through arms sales, and thus this later coefficient is found negative.

The USA is different from other donors in their relationship to trade. Most donors show a positive and significant relationship, whereas for the USA it is negative and significant. It was on the basis of this coefficient that Berthélemy (2006) classified some donors as selfish and others as altruistic. However, the literature makes clear that there are several channels though which aid could be used to promote exports (Osei, Morrissey, and Lloyd 2004), and it is difficult to rule out aid being used to promote exports on the basis of a negative coefficient. For example, aid could be given to recipients that currently import a

small amount of goods from the donor country, with the aim of increasing this over time (Lloyd, McGillivray, Morrissey, and Osei 2000). The results show that American and British aid does not have a positive relationship with trade flows, in contrast to other donors.

Question 2: what are changes over time?

To answer the previous question, we looked at each donor in turn over the 25 years. To answer how allocation practice has evolved over time, we look specifically at three periods: Cold War, post Cold War and post 9/11. The post 2001 time period has been studied for American aid, but not for others. It is possible that other donors were affected in the same way, but it also offers some evidence regarding policy selectivity. Separate results by donor, time period and stage are provided in the Appendix (Table A 1- Table A 7). To give some initial measure of the extent to which allocation practice has changed over the period, Chow tests were conducted on the sample. These are calculated by augmenting the previous regression with all variables of the variables interacted with dummies for the CW period, and the period after the September the 11th terrorist attacks. The Chow test is essentially a test of whether there has been a significant change in the underlying relationships, as these new variables contain no new information. If there was a consistent relationship between the dependent and independent variables the coefficients on these new variables would be equal to zero. The below statistics are in essence tests of that assumption.

Table 5: Chow Tests for Changed Relationships

Step	Donor	France	Germany	Japan	Netherlands	Sweden	USA	UK
Cold War		12.99*	19.60***	9.909	13.98*	7.702	10.44	27.12***
	Eligibility	0.0724	0.0065	0.194	0.0515	0.36	0.165	0.0003
		0.956	2.501**	1.021	1.706	2.721***	3.463***	1.091
	Level	0.463	0.0156	0.416	0.106	0.00985	0.00127	0.368
Post 9/11		7.027	5.082	3.243	25.81***	30.28***	8.414	6.285
	Eligibility	0.426	0.65	0.663	0.0005	0.00008	0.298	0.507
		0.712	0.872	0.628	0.66	0.823	2.667**	0.864
	Level	0.662	0.515	0.678	0.682	0.569	0.0104	0.535

Note: Chow statistics have an F distribution and are reported above with the P value below. Results refer to the parsimonious specification. The statistics are relative to the period 1992-2001.

Table 5 shows joint significance tests by step, donor and period. Japan is the only donor that does not show significant differences in overall allocation policy over the twenty-five year period. The USA, Sweden and Netherlands are the only donors for whom the GWOT period is significantly different (and the USA is the sole donor for which it is significant at the level stage). The Cold War period, by contrast, sees changes for every donor apart from Japan. We can further break these differences down into differences using the 4P framework, for those donors that demonstrate significant differences.

Table 6: Wald Statistics for Donors with Significant Changes, Using 4P Framework

Period	CW	CW	CW	CW	GWOT	GWOT	CW	CW	GWOT	CW
Step	1	1	2	1	1	1	2	2	2	1
	France	Germany	Germany	Neth.	Neth.	Sweden	Sweden	USA	USA	UK
Poverty	2.42	2.17	0.22	0.12	0.96	0.002	6.64**	0.16	1.49	0.082
	(0.12)	(0.14)	(0.64)	(0.73)	(0.33)	(0.97)	(0.011)	(0.69)	(0.22)	(0.78)
Population	0.89	0.079	0.21	0.12	1.79	2.17	4.77**	0.15	2.79*	0.33
	(0.35)	(0.78)	(0.65)	(0.73)	(0.18)	(0.14)	(0.03)	(0.70)	(0.096)	(0.57)
Policy	5.84*	5.01*	0.66	2.96	0.79	1.52	0.61	0.88	3.18**	7.42**
	(0.054)	(0.082)	(0.52)	(0.23)	(0.67)	(0.47)	(0.55)	(0.42)	(0.043)	(0.025)
Proximity	0.091	0.38	0.35	1.39	1.91	3.05	3.28**	7.96***	1.26	0.064
	(0.96)	(0.83)	(0.70)	(0.50)	(0.39)	(0.22)	(0.039)	(0.0004)	(0.29)	(0.97)

Note: The Wald Test statistics is shown, with stars denoting the 1, 5 and 10 % significance levels. The Wald statistic has a large-sample Chi-squared distribution. CW denotes the cold war period 1982-1991, GWOT 2002-2006 and Neth., The Netherlands.

Using the breakdown in Table 6, and further inspection of individual coefficients, we can find the cause of the differences between periods. For France, the change is driven by an increasingly negative coefficient on PTS, perhaps as long-term recipients have received worse scores but aid has not decreased. The same can be said for Germany, although that is combined with an increasingly positive coefficient for Freedom. For Germany, the difference of the levels stage for the Cold War period does not have a single factor, but instead a multitude of small changes including *decreasing* poverty sensitivity. Both steps for the Netherlands and Sweden's 1st Step can also be said to be a number of small changes. This is perhaps expected as they are smaller donors that focus more than other donors, and small adjustments in selecting aid recipients may still be identified. Sweden during the Cold War appears different in almost every factor in the levels stage. The coefficient on poverty actually *decreased* with the end of the cold war, whereas policy selectivity appears to have increased (but is still not significant). The coefficient during

the Cold War (for Sweden at the levels stage) on exports was positive, but this has become negative in the latter periods. For the USA, a number of changes occurred, including a decreasing importance of Freedom over time, and a more negative coefficient for PTS. The Proximity index was most positive and significant in the period 1992-2001. This is driven by a significant and positive effect of trade in the first step and significant and negative effect in the second. In the Cold War and GWOT periods the coefficient is insignificant at both levels. The UK has had an increasingly negative coefficient for PTS.

Moss et al. (2005) found three variables were successful in controlling for the effect of the GWOT on US aid allocation. The first was a dummy for four countries that received large aid increases since 2001: Iraq, Afghanistan, Jordan and the Palestinian Territories. The second was a dummy for recipients that saw large drops (which, it is argued, partially financed the aforementioned increases) in aid: Israel, Egypt and Bosnia Herzegovina. The third (which was less successful) was an interaction term for the percentage of Muslim population and a dummy for years after 2001. Retesting these variables for the second stage of US allocation (Table 7) finds only the dummy for those countries that receive less aid is significant, but that this variable is highly significant and represents 30% of a standard deviation fall in aid receipts as well as increased likelihood of censoring.

Table 7: Retesting the GWOT dummies

Step	1st	2nd
Muslim*01	0.0061	-0.0012
	(1.86)	(0.038)
‘Gain’ 01	-	0.013
		(0.42)
‘Loss’ * 01	-	-0.27***
		(4.83)

Note: Standardised Coefficients are reported to assist interpretation – where beta reports the standard deviation change in Y resulting from a one standard deviation change in X. Other variables included, but not reported: these variables are augmenting the parsimonious specification.

Question 3: has poverty selectivity increased?

Augmenting the parsimonious regression equation with income interacted with a period dummy allows us to estimate poverty selectivity by period, while still controlling for the other factors (the normal income variable is, of course, excluded). Table 8 reports only the poverty coefficients but they were obtained in a regression using the parsimonious specification over the 25-year time period.

Table 8: Income Coefficients: by Step, Period, Stage and Donor

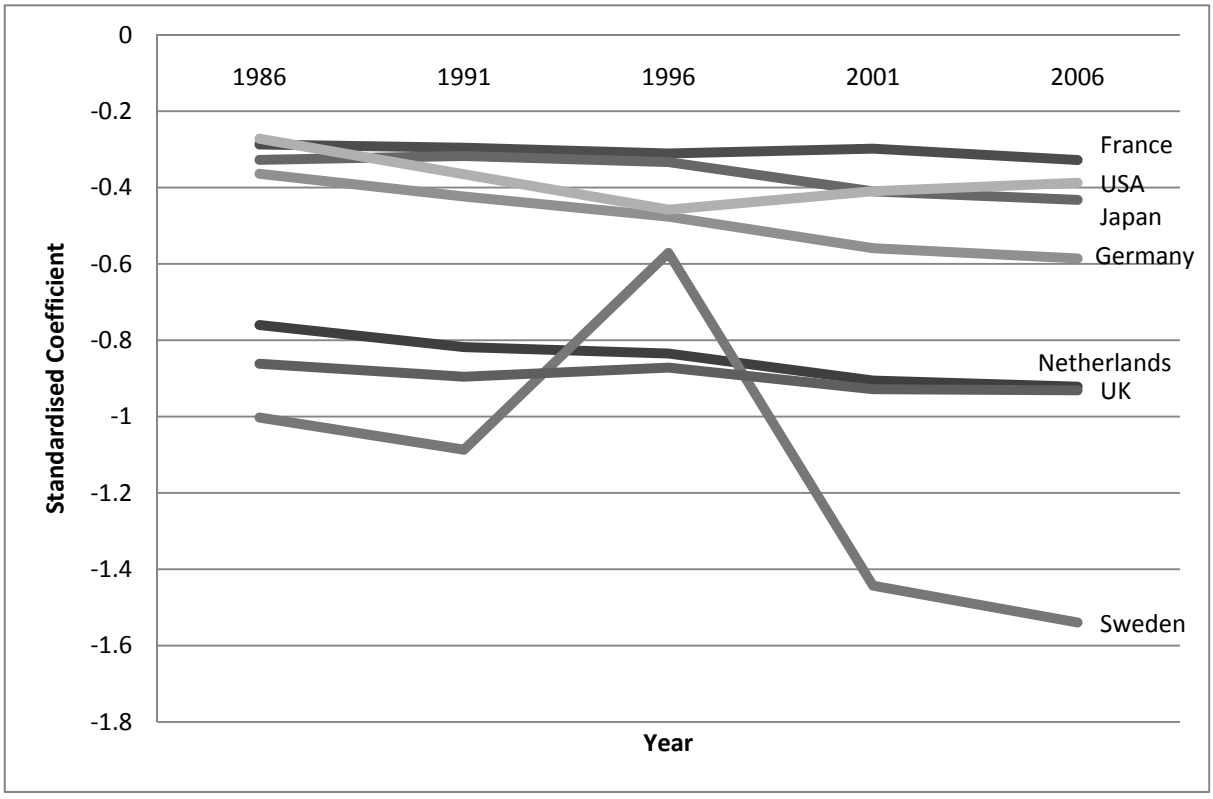
		France	Germany	Japan	Netherlands	Sweden	USA	UK
1st stage	1982-	-0.26***	-0.17*	-0.38***	-0.37***	-0.35***	-0.63***	-0.35***
	1986	(3.40)	(2.25)	(5.15)	(5.01)	(4.71)	(8.44)	(4.54)
	1987-	-0.29***	-0.19**	-0.37***	-0.43***	-0.37***	-0.63***	-0.36***
	1991	(3.92)	(2.58)	(5.08)	(5.85)	(4.94)	(8.52)	(4.76)
	1992-	-0.32***	-0.23**	-0.41***	-0.44***	-0.51***	-0.64***	-0.42***
	1996	(4.29)	(3.18)	(5.64)	(5.98)	(6.54)	(8.80)	(5.55)
	1997-	-0.32***	-0.24***	-0.42***	-0.45***	-0.30***	-0.62***	-0.43***
	2001	(4.41)	(3.45)	(5.92)	(6.21)	(4.15)	(8.65)	(5.85)
	2002-	-0.32***	-0.24***	-0.42***	-0.52***	-0.30***	-0.60***	-0.44***
	2006	(4.49)	(3.49)	(5.98)	(7.11)	(4.13)	(8.66)	(6.00)
2nd Stage	1982-	-0.29*	-0.36**	-0.33**	-0.76***	-1.00***	-0.27*	-0.86***
	1986	(2.39)	(2.74)	(2.61)	(3.97)	(4.53)	(2.35)	(6.02)
	1987-	-0.30*	-0.42**	-0.32*	-0.82***	-1.09***	-0.37**	-0.90***
	1991	(2.47)	(3.12)	(2.42)	(4.35)	(4.73)	(2.94)	(6.01)
	1992-	-0.31*	-0.48***	-0.33*	-0.83***	-0.57***	-0.46***	-0.87***
	1996	(2.51)	(3.43)	(2.53)	(4.15)	(3.67)	(3.55)	(5.85)
	1997-	-0.30*	-0.56***	-0.41**	-0.91***	-1.44***	-0.41**	-0.93***
	2001	(2.39)	(4.03)	(3.11)	(4.59)	(5.11)	(3.08)	(6.33)
	2002-	-0.33**	-0.59***	-0.43**	-0.92***	-1.54***	-0.39**	-0.93***
	2006	(2.66)	(4.25)	(3.28)	(5.34)	(5.40)	(2.90)	(6.41)

Note: 2nd Stage standardised coefficients are reported, with T statistics in parentheses.

The first stage shows evidence of a small increase in the coefficient for some donors. As there is only a single variable representing poverty, the coefficients from stage 2 of the regression can be easily plotted, see Figure 4. This easily allows us to compare donors, and any changes over time relative to differences between donors. France, the USA, Japan and Germany all have coefficients of between -0.2 and -0.6 over the 25 year period. Of these, only the USA has become less poverty-sensitive in recent years – this is possibly an effect of the GWOT. While France has remained fairly static over the period,

Japan and Germany have become more poverty focused. The Netherlands and the UK both started significantly more poverty-sensitive, and have increased this over the period. For both donors, a 1 standard deviation difference in income per capita implies a response in aid budget share of almost one standard deviation. Sweden is even more poverty focused at this second step, but less so at the first step. The big decrease in poverty sensitivity in the 1992-1996 period is reflected by a larger coefficient for poverty in the first stage. We can divide the donors into two: the poverty sensitive donors (Netherlands, UK and Sweden) and the less poverty sensitive donors (France, Germany, Japan and USA). While there is some downward movement of the 25-year period, the largest differences are clearly between different donors, rather than between time periods.

Figure 4: Poverty Sensitivity Coefficients 1982-2006 Level Stage, by Donor



Question 4: has policy selectivity increased?

In order to examine the question of more recent changes to policy selectivity, we can augment the parsimonious specification. This means taking advantage of some of the more sophisticated variables available, at the cost of losing some years of data[6]. The motivation for suspecting that policy selectivity has increased is the work of Burnside and Dollar (2000) and

6 *We lose two time periods: 1982-1986 and 1987-1991. The data is available from the year 1996, and this one observation is used for the 1992-1996 period.*

the apparent move from conditionality toward selectivity. The variable chosen is the corruption variable taken from the Worldwide Governance Indicators (WGI) dataset. This is produced by the World Bank and uses a number of inputs to measure corruption on a scale between -2.5 (most corrupt) and 2.5 (least corrupt, although where standardised coefficients are reported this scale is somewhat immaterial). This variable has previously been used, but never as extensively (Hout, W. 2007b used all of the WGI variables with time variation, but only for three donors;Neumayer 2003b used one observation over the whole time period). Corruption is chosen as it is the most easily measured and widely discussed aspect of policy selectivity, and does not introduce problems of multicollinearity. When corruption is included in the specification as the sole representative of policy (results not reported), it is virtually always insignificant for each donor and step. The only exception is for the 2nd step coefficient for the USA, where it is found to be negative (i.e. more corrupt countries receive more aid). It is possible however that any policy changes are most evident in recent years. For this reason,

Table 9 and

Table 10 show the coefficients for policy variables at the first and second stages, when corruption is interacted with a dummy for different periods. If there was Policy Selectivity, we would expect to find positive and significant coefficients.

**Table 9: 1992-2006 Main Specification Eligibility Stage,
Augmented With Policy Variables Interacted With Time Dummies**

	France	Germany	Japan	Netherlands	Sweden	USA	UK
Ln(GDP)	-0.41***	-0.36***	-0.39***	-0.48***	-0.35***	-0.54***	-0.52***
	(4.00)	(3.58)	(3.83)	(4.69)	(3.67)	(5.41)	(4.87)
Ln(Population)	-0.093	0.066	-0.12	0.25***	0.28***	-0.13	0.10
	(1.32)	(0.91)	(1.61)	(3.76)	(3.98)	(1.95)	(1.48)
Freedom Index	0.093***	0.12***	0.14***	0.11***	0.13***	0.17***	0.15***
	(3.53)	(4.45)	(4.49)	(3.62)	(4.36)	(5.91)	(5.32)
Political Terror Scale	-0.51***	-0.53***	-0.40**	-0.55***	-0.27*	-0.53***	-0.57***
	(3.70)	(3.75)	(2.72)	(4.37)	(2.44)	(3.80)	(4.26)
Control of Corruption	0.076	0.13	0.028	0.020	0.68***	0.057	0.19
	(0.33)	(0.57)	(0.12)	(0.096)	(3.30)	(0.25)	(0.80)
Corruption * 2001	0.74	1.55	-0.25	1.88	-4.47***	-0.92	0.46
	(0.58)	(1.19)	(0.19)	(1.55)	(3.81)	(0.73)	(0.36)
Corruption * 2006	-0.59	-0.36	-1.89	2.76*	-5.25***	-2.54	-0.31
	(0.45)	(0.26)	(1.33)	(2.32)	(4.37)	(1.96)	(0.23)
Exports	-0.37	-1.58**	0.49	-3.46***	-1.65**	0.92*	-1.37
	(0.57)	(3.01)	(1.51)	(4.00)	(2.77)	(2.10)	(1.85)
Proximity Index	0.049	-0.0093	2.46	-0.24	-0.20	-2.07	0.65**
	(0.17)	(0.016)	(0.19)	(1.34)	(0.64)	(1.95)	(2.77)
Observations	379	379	317	398	378	376	379
Pseudo R-squared	0.162	0.211	0.169	0.318	0.237	0.247	0.266

Note: Other variables not reported, the variables shown are augmenting the parsimonious specification.

**Table 10: 1992-2006 Main Specification Level Stage,
Augmented With Policy Variables Interacted With Time Dummies**

	France	Germany	Japan	Netherlands	Sweden	USA	UK
Ln(GDP)	-0.11*	-0.18**	-0.10	-0.27***	-0.34***	-0.075	-0.30***
	(2.00)	(2.94)	(1.65)	(3.45)	(3.61)	(1.60)	(5.04)
Ln(Population)	0.24***	0.41***	0.43***	0.38***	0.37**	0.26***	0.53***
	(3.78)	(5.63)	(5.92)	(4.07)	(3.23)	(4.96)	(7.11)
Freedom Index	0.034	0.057	0.098	0.29***	0.12	0.089	0.14*
	(0.67)	(0.99)	(1.61)	(3.60)	(1.23)	(1.84)	(2.33)
Political Terror Scale	0.034	-0.041	-0.012	-0.077	-0.018	-0.052	0.059
	(0.58)	(0.61)	(0.16)	(0.86)	(0.17)	(0.96)	(0.93)
Control of Corruption	-0.014	0.027	-0.054	-0.14	-0.18	-0.089	-0.12
	(0.19)	(0.33)	(0.65)	(1.41)	(1.05)	(1.41)	(1.59)
Corruption * 2001	0.012	0.075	0.071	0.14	0.20	-0.057	0.061
	(0.23)	(1.22)	(1.11)	(1.86)	(1.54)	(1.18)	(1.08)
Corruption * 2006	-0.0052	0.025	0.027	0.24**	0.29*	-0.070	0.040
	(0.10)	(0.42)	(0.44)	(3.32)	(2.19)	(1.49)	(0.74)
Exports	0.34***	0.29***	0.34***	-0.14	-0.17	-0.14**	0.072
	(5.58)	(4.44)	(5.46)	(1.52)	(1.56)	(3.00)	(1.06)
Proximity Index	0.54***	-0.051	0.051	0.073	0.12	0.73***	0.44***
	(12.2)	(1.08)	(1.03)	(1.14)	(1.58)	(18.6)	(9.33)
Observations	297	294	253	228	174	283	257
Adjusted R-squared	0.555	0.397	0.436	0.201	0.178	0.644	0.525

Note: Standardised Coefficients are reported to assist interpretation – where beta reports the standard deviation change in Y resulting from a one standard deviation change in X. Other variables not reported, these variables are augmenting the parsimonious specification.

Table 9 shows the results for the first step. Only Sweden has a positive coefficient for corruption but this is only for the period 1992-1996, and in later years it is significant and negative. The Netherlands show evidence of an increased coefficient on corruption, but for most donors there is little change. At the levels stage (

Table 10) the Freedom and PTS coefficients are typically insignificant, the exceptions being positive coefficients for the UK and Netherlands. There is scant evidence of an increasing importance of corruption; the Netherlands and Sweden being exceptions to this. In the case of the Netherlands, this is particularly interesting as it is then sensitive to corruption at both steps. There is (insignificant) evidence of a negative relationship between US aid and high levels of corruption, and of this increasing over the period. It should be remembered that the augmented coefficient should be interpreted in conjunction with the standard coefficient, e.g. the coefficient corruption for the Netherlands in 2006 is $-0.14 + 0.24 = 0.10$, so a one standard deviation increase in the control of corruption results in 10% of a standard deviation increase in aid budget share. Also, it is the change from previous practice that is found significant (or, in most cases, insignificant) rather than the practice itself. Overall, the picture is not one of high policy selectivity at either stage for most donors, but rather insignificant coefficients for policy variables.

The results are robust to exclusions of other policy variables, and the use of Corruption Perception Index data instead of the WGI data (not reported). We report one robustness test here; using the Country Policy and Institutional Assessment (CPIA) which is a dataset constructed by World Bank staff to measure economic and social policies. While criticised in some of the academic literature for being too closely related to growth (Dalgaard, Hansen, and Tarp 2004), some multilateral donors use it as a measure of policy. As it was not publicly available before 2005, a cross section averaged over the 2001-2006 period is reported here.

Table 11: Augmented with CPIA: 2001-2006 Cross Section, Eligibility Stage

	France	Germany	Japan	Netherlands	Sweden	USA	UK
Ln(GDP)	-0.064	-0.15	-0.37	0.45	0.027	-0.30	0.049
	(0.15)	(0.35)	(0.68)	(0.84)	(0.052)	(0.70)	(0.10)
Ln(Population)	0.20	0.70	0.39	1.35*	1.00*	0.45	0.81*
	(0.69)	(1.80)	(0.90)	(2.32)	(2.51)	(1.64)	(1.99)
Freedom Index	0.15	0.27*	0.20	0.53*	0.098	0.16	0.54**
	(1.37)	(2.07)	(1.10)	(2.47)	(0.91)	(1.42)	(2.76)
Political Terror Scale	-0.70	-0.37	-0.35	-4.26**	-1.22	0.040	-1.21
	(1.25)	(0.67)	(0.52)	(2.86)	(1.82)	(0.076)	(1.84)
Control of Corruption	0.91	0.83	0.35	5.54**	1.75	0.44	0.96

	(1.01)	(0.96)	(0.26)	(2.85)	(1.83)	(0.56)	(0.91)
CPIA	-0.36	-0.35	-0.081	-1.12	0.017	-0.39	-1.15
	(0.40)	(0.39)	(0.068)	(0.94)	(0.017)	(0.48)	(1.14)
Exports	-1.13	-15.2*	-5.15	-19.1	-11.5*	-2.59	-9.57
	(0.12)	(1.99)	(1.28)	(1.91)	(2.34)	(0.52)	(1.85)
Proximity Index	-0.14	-1.19	-3.98	1.83*	1.54	10.3	1.03
	(0.18)	(0.92)	(0.10)	(2.02)	(1.20)	(0.62)	(1.11)
Observations	65	65	47	62	65	65	65
Pseudo R-squared	0.146	0.323	0.251	0.584	0.505	0.187	0.513

Note: Other variables not reported, these variables are augmenting the parsimonious specification.

Table 12: Augmented with CPIA: 2001-2006 Cross Section, Levels Stage

	France	Germany	Japan	Netherlands	Sweden	USA	UK
Ln(GDP)	-0.14	0.12	0.21*	-0.21	-0.21	0.051	-0.061
	(1.03)	(0.86)	(2.06)	(1.40)	(1.27)	(0.43)	(0.54)
Ln(Population)	-0.35	0.42	0.72***	0.90**	0.35	0.17	0.31
	(1.43)	(1.59)	(4.11)	(3.45)	(1.17)	(0.86)	(1.41)
Freedom Index	-0.018	0.16	0.27	0.13	-0.089	0.081	0.041
	(0.13)	(1.00)	(1.75)	(0.65)	(0.46)	(0.61)	(0.26)
Political Terror Scale	-0.15	-0.018	-0.019	0.44	0.10	-0.16	-0.13
	(0.81)	(0.095)	(0.12)	(1.76)	(0.46)	(0.93)	(0.81)
Control of Corruption	-0.33	-0.12	0.059	-0.071	-0.29	-0.33*	-0.16
	(1.98)	(0.66)	(0.40)	(0.32)	(1.30)	(2.24)	(1.03)
CPIA	0.31	-0.020	-0.33	0.069	0.53*	0.22	0.082
	(1.82)	(0.11)	(1.96)	(0.29)	(2.26)	(1.47)	(0.51)
Exports	0.66***	0.27	0.20	-0.60**	-0.25	-0.15	0.27
	(4.31)	(1.68)	(1.62)	(3.12)	(1.09)	(1.25)	(1.90)
Proximity Index	0.32*	-0.090	0.076	-0.047	0.076	0.51***	0.37***
	(2.60)	(0.77)	(0.77)	(0.32)	(0.49)	(4.58)	(3.65)
Observations	58	56	43	38	47	57	53
Adjusted R-squared	0.425	0.325	0.616	0.352	0.133	0.526	0.581

Note: Standardised Coefficients are reported to assist interpretation – where beta reports the standard deviation change in Y resulting from a one standard deviation change in X. Other variables not reported, these variables are augmenting the parsimonious specification.

Table 11 shows the policy variables for the first stage of the regression. The CPIA variable is not significant for any donor. It does however mean that the Freedom Index coefficient become positive, this is most likely due to the sample size restrictions of including the CPIA.

Table 12 reports the 2nd Step, where only Sweden has a significant relationship with the CPIA. Interestingly, the corruption coefficient for the USA becomes significant and negative upon the inclusion of the CPIA. This implies the US is less concerned with corruption than other dimensions of policy selectivity.

Robustness Checks

A common variable in the literature not included here is that of geographical distance between the donor and recipient. It was not included in the preceding estimations for two main reasons. First, while the variable is commonly justified by a desire to capture a focus on countries that are relatively close, the variable is often found to be positive. This leads to problems of interpretation, as it is not immediately clear why donors would focus on distant recipients, controlling for other factors. Second, the estimation of a geographical distance variable is highly sensitive to which countries are included in the sample. For example, the choice of whether to include eastern-European countries will severely influence the estimation of a European donor's actions. Third, it is hoped that the proximity index is able to capture the majority of the proximity effects. Bearing these caveats in mind, we present below the first and second steps for the 7 donors over the 25 year period. As qualitative differences in the majority of the other variables were neither expected nor found, we only report the three variables of interest.

Table 13: Step 1 and 2, Including Geographical Distance

Step 1	*France*	*Germany*	*Japan*	*Netherlands*	*Sweden*	*USA*	*UK*
Exports	0.47	-1.10*	0.90***	-2.57***	-1.41**	0.42	-0.83
	(1.02)	(2.50)	(3.47)	(3.99)	(2.98)	(1.64)	(1.73)
Proximity Index	0.18	-0.12	15.5	-0.43**	-0.16	-0.80	0.61***
	(0.81)	(0.28)	(1.43)	(3.15)	(0.65)	(1.11)	(3.38)
Distance	0.096***	0.086***	0.092***	0.0053	0.026	-0.0052	0.039*
	(4.82)	(4.33)	(3.83)	(0.26)	(1.32)	(0.26)	(2.12)
Step 2	*France*	*Germany*	*Japan*	*Netherlands*	*Sweden*	*USA*	*UK*
Exports	0.25***	0.28***	0.24***	-0.015	0.049	-0.14***	0.14**
	(5.90)	(5.64)	(5.26)	(0.24)	(0.57)	(3.54)	(2.85)
Proximity Index	0.54***	-0.013	0.040	0.074	0.12	0.68***	0.41***
	(16.3)	(0.33)	(1.15)	(1.58)	(1.82)	(20.5)	(10.3)
Distance	-0.10**	-0.057	-0.29***	0.070	0.075	-0.11***	0.033
	(3.12)	(1.45)	(7.32)	(1.44)	(1.11)	(3.36)	(0.93)

Note: the above coefficients were the result of the standard parsimonious regression; only variables of direct relevance are included here. The first step was estimated by probit, the second step by OLS. The second step is reported using standardised coefficients.

In the first step, distance is positive for six of the seven donors, implying donors are less likely to allocate aid to countries that are closer. Using France as an example, the effect of the proximity index moving from 0 to 1 (i.e. from the least proximate to the most proximate country) is the same as being closer by 2000 km, as one unit of distance is 1000 km. In the second step, distance is found to have a significant and negative effect for three donors, and is insignificant for the remaining four. For Japan the effect is large and highly significant, and the proximity index becomes insignificant. It is understandable that distance is more significant for Japan as its proximity index includes neither common language nor colonial history. While it is significant for the USA and France, it is much less significant than the proximity index.

5 Discussion

We have presented evidence that differences between donors are large and persistent. While much of the recent discussion and debate regarding the differences in allocation policy has focused on changes over time, this should not obscure the larger differences between donors. In answering question 1, abundant evidence was found of these differences. In terms of poverty sensitivity three clear groups were identified: high (Netherlands, Sweden and the UK), medium (Germany and Japan) and low (USA and France). The two smallest donors (Sweden and Netherlands) appear to have a much larger small-country bias than other donors. Policy does not appear to be a major determinant for any donor, when averaged over the last 25-year period. The different weights attached to Proximity are so large as to be almost incomparable. The results agree with previous research that argued Nordic donors (represented by Sweden) are different (Gates and Hoeffler 2004), but also provide evidence that a more suitable distinction can be made. The ‘Nordic+’ group of donors (Norway, Sweden, Finland, UK, Ireland, the Netherlands and Denmark) that are included here are shown to indeed be like-minded with regard poverty focus.

Question 2 by contrast found only modest evidence for changes over time. The differences found were often far from substantive, and of a more evolutionary nature. Berthélemy and Tichit (2004) found the CW to have caused a shift from geopolitics to trade relationships as a key factor. The only donor that fits that pattern in our results is the

USA, for whom trade is only significant (with the opposite sign at the levels stage to that postulated by Berthélemy and Tichit, 2004) between the CW and GWOT periods. For the UK, by contrast, we find the Trade coefficient to be insignificant in the post-cold war period. We also find little evidence in favour of the idea that the end of the CW meant an increased importance of poverty (Meernik, Krueger, and Poe 1998), but instead evidence that it resulted in little substantial change (Boschini and Olofsgard 2007;Easterly 2007). We find the GWOT to have had relatively little effect in aid practice, even for the USA. While other research presents evidence of a changed size of the overall aid budget for the USA (Fleck and Kilby 2009), this would not be detected in our analysis as the size of the budget is treated as exogenous. Our analysis instead focuses on the change in how that money is allocated, and this confirms findings that the anticipated large-scale change in allocation practice (Buzan 2006;Woods 2005) did not truly materialise (Moss, Roodman, Standley, and Floor 2005). For other donors, the GWOT period was not expected to be different primarily because of the GWOT, but instead the idea of selectivity may have had time to influence allocation policy.

By looking more specifically at poverty sensitivity in question 3 we can reaffirm conclusions already drawn. Figure 4 illustrates clearly that the biggest differences in policy sensitivity are between donors, not time periods. The picture is complicated slightly by the two stage process, as some donors are more affected by poverty at the first step (such as the USA and the Netherlands). Nevertheless, this more focused section restates the large difference between the most poverty-sensitive donors (Netherlands, Sweden and the UK) and the others. The same figure also shows no shift in the poverty coefficient around the CW period ending, which some research suggests likely. Instead, it shows a modest increase in the weight given to poverty. There also appears to be no great effect due to the proposed move from conditionality to selectivity in the mid 1990s, which would imply shifts of a bigger magnitude rather than incremental moves. The slight recent decline in US poverty sensitivity found elsewhere (Fleck and Kilby 2009) is replicated, but for other donors this has increased slowly.

Question 4 seeks to directly address the debate regarding whether policy sensitivity is low and fairly static (Easterly 2007;Nunnenkamp and Thiele 2006) or significant and

increasing (Berthélemy and Tichit 2004;Dollar and Levin 2006). Much of the disagreement appears to be the inevitable result of distinct methodologies, specifically in defining which variable or variables represent policy. Easterly (2007) uses a number of variables, looking over time at Openness, Inflation, Democracy and Corruption[7]. His general conclusions are of low selectivity with little movement, with some donor heterogeneity amongst the five donors used. Nunnenkamp and Thiele (2006) use a mixture of WGI and CPIA data, and conclude from both approaches that there is little evidence of policy selectivity. By contrast, Berthélemy and Tichit (2004) conclude that policy selectivity has increased. As discussed, this is substantiated by the coefficient on lagged economic growth and has various problems of collinearity and rival interpretation. Dollar and Levin (2006) use ICRG data on the Rule of Law and Freedom House data on democracy as their policy variables and find that aid selectivity increases over time *when averaging by donor*. For bilateral aid (which is much larger than multilateral aid) they find no statistically significant result. Our econometric approach differs from these four papers by using the Two Part model, controlling for serial correlation and aid volatility, expanding the time period, presenting econometric tests between the various estimators, linking the specification to the 4P framework and formally comparing the explanatory power of these factors. When using the WGI corruption variable the overall picture is one of low policy selectivity with few changes over time. There is limited evidence for recently increased policy selectivity for the Netherlands and Sweden at the level stage, and at the eligibility stage for the Netherlands. When using the CPIA only Sweden has a significant coefficient in either step. It is to be expected that Nordic donors differ (Gates and Hoeffler 2004), however, it has previously been found that the Netherlands were not remarkable in their policy-focus (Hout, W 2007a, p.166). This conclusion was based upon a regression for the period 1999-2002, and we only find coefficients on policy to be positive in the period 2002-2006. This then has been a recent move. Donors often commit aid for many years at a time, and thus a policy shift may be undetectable for a number of years. It is clear however, that most donors do not place much emphasis on policy.

The chosen methodology does not include the size of the aid budget, and therefore cannot identify perhaps the biggest effect of both the CW and GWOT: higher aid budget. A

7 Democracy is from the Polity IV dataset and Corruption from the ICRG, a private company that provides data for researchers and businesses.

further ‘blind spot’ in the methodology may obscure another change in aid allocation over the period: It is possible that donor selectivity does in fact exist but is not detected using this methodology. Having chosen to remain within the positive allocation tradition, the unit of analysis is the amount of aid (budget share) given by a donor to a recipient. There have been some signs that policy selectivity could alter not the level but the type of aid on the basis of recipient policy (Cordella and Dell'Ariccia 2007). A different methodology would need to be employed to find if that is true. However, the methodology chosen remains highly beneficial to the literature in a number of ways. Relating to the given examples, the econometric results presented challenge earlier findings. It has previously been found that the end of the CW changed allocation practice significantly. Also, it has been suggested that the GWOT would have a similarly large effect on allocation practice. The results presented question both ideas.

6 Conclusion

This paper has examined aid allocation by donor and time period, and sought to answer a number of salient questions. The formal framework used to analyse and compare different factors permits these comparisons across donor and year to be more credible. It divides allocation motives into the four competing factors of the 4P framework (Poverty, Population, Policy and Proximity) allowing the relative weights of the factors to be estimated. An increased weight for one factor would often mean a decreased weight for another, and the revealed preferences can then be examined by factor, donor and time period. The major feature of aid allocation found by this approach is not a change in poverty or policy sensitivity, but substantial and entrenched donor heterogeneity. Sweden, the Netherlands and the UK are considerably more poverty sensitive than other donors. It has been argued that Sweden is free from colonial ties and therefore is able to attach more weight to the poverty factor (Alesina and Weder 2002), and we indeed find proximity to be insignificant for Sweden. However, this argument cannot be made for the UK and the Netherlands, for whom both proximity and poverty factors are significant. The USA and France do not appear to balance the two factors either, with proximity being many times more important. By comparison, Japan and Germany attach less weight to proximity and more to poverty, although the relative difference is marginal. These results imply that for

some donors there is a sizeable tension between factors (Germany, Japan, the Netherlands and the UK), but for others there is not (Sweden, the USA and France)[8].

This research contributes to the debate regarding whether aid selectivity on the basis of good policy and low income levels have been adopted in recent years. Expectations of large shifts in selectivity due to the end of the CW, Selectivity or the GWOT have largely been confounded. With regards poverty, the overall picture is one of slowly increasing sensitivity at the level stage. With regards policy, small increases for some donors in recent years must be understood in the context of broad insensitivity to policy. There are many possible reasons for this. The main impact of the CW and GWOT appear to be changes in the *volume* of aid, rather than its allocation. The rationale for selectivity was predicated upon specific econometric results, which have been found dubious. Thus it is possible that donor-confidence in selectivity diminished along with that of the econometric results. A further reason is that change amongst aid agencies is likely to be slow, because they are often large institutions, but also due to implementation. Multi-year commitments mean that only a percentage of aid allocation decisions will be made in that year. This means data on commitments will see a smoother and slower transition in allocation principles than would otherwise be the case.

This research underlines the large differences between donors. In some cases this is due to the competing priorities, in others there is no competition and one factor dominates the allocation. These distinct donor preferences are the most likely cause of donor fragmentation. The results do suggest that some donors share preferences, and efforts to reduce fragmentation could be more successful if it focused upon those donor groups (e.g. among the Nordic+ donors). This fragmentation in itself is likely to increase transaction costs and thus lower aid effectiveness (Bourguignon and Sundberg 2007;Knack and Rahman 2007). Furthermore, research has also shown a link between allocation practice and aid effectiveness (Headey 2008;Reddy and Minoiu 2006). A vicious circle is perhaps at work, as if donors continue to weight proximity highly in allocation decisions (and this in turn reduces aid effectiveness), it may continue to be difficult to identify a positive effect from aid (Roodman 2008). This may cause donors to believe aid has limited

8 This is one reason why the approach of Berthélemy (2006) in grouping donors merely by their trade coefficient seems inadequate – some donors show signs of concurrently pursuing more than one goal.

growth-promotion effects, leading to a higher weight for proximity than (potentially) growth-promoting factors. However, this research does not enable statements upon the effects of policy selectivity; instead it confirms that aid selectivity has not been practiced over 25 years of aid allocation and thus its effects are most likely unknown. The lesson taken from the history of poverty sensitivity is salient here: donor change happens slowly. The two large events that were external to donors (the conclusion of the Cold War and the Global War on Terror commencing) as well as the policy discussion that was to some extent internal (policy selectivity) have changed aid allocation only incrementally. Comparisons by donor and era clearly demonstrate that even over 25 years, there is much greater resemblance within a donor's allocation than within a single time period.

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Data Sources and Descriptions

All Websites accessed June 2009 unless otherwise stated.

Aid – The Aid variable is taken from the OECD DAC Table 3a. It is the total ODA commitment of a donor to a specific recipient, divided by the total commitments by the donor to all recipients (bilateral and multilateral) in that year, multiplied by one hundred and then logged.

Ln(GDP) - GDP per capita data was taken from the World Development Indicators (WDI) and is in constant 2000 US$. This was then logged so as to be normally distributed.

Ln(Population) - Population data is from the World Development Indicators 2008, the annual World Bank publication. This was then logged so as to be normally distributed.

Freedom Index- This index was taken from Freedom House (www.freedomhouse.org), which is based upon surveys of various experts of each country. The original Index constitutes of two scores, civil liberties and political rights, each scaled from 1-7. The new scale then combined and scaled from 2-14 (as in Neumayer 2003b) with the higher number being the best score, to facilitate interpretation. While the two scores measure slightly separate concepts, for this purpose they can be taken together to measure the extent to which democratic values are in action.

Political Terror Scale - PTS data is taken from Gibney, M., Cornett, L., & Wood, R., (2008) Political Terror Scale 1976-2007 from www.politicalterrorscale.org. This codifies US State Department and Amnesty International reports to create a measure of 1-5 (which is rescaled so that higher scores are better outcomes) that focuses on politically-motivated violence and imprisonment.

Religion - the variables Protestant/Catholic, Buddhist and Muslim are the percentage of adherents to the respective religions of the recipient population. The data is taken from Neumayer (2003b).

Arms - Figures are SIPRI Trend Indicator Values (TIVs) expressed in US$ m. at constant (1990) prices. They are the value of arms purchased by a recipient from a donor. Data are provided by the think tank SIPRI (www.sipri.org).

Exports – Data is the value of exports from a donor to a recipient, as a share of total exports of that donor. This was then multiplied by 100 and logged so as to be normally distributed. The Data is 'Trade in value by partner countries' from OECD.source **(**www.sourceoecd.org**),** apart from for the Netherlands, which is from the IMF Directions of Trade Statistics, as it has large amounts of data missing in the OECD database.

Colony – The group of variables take the value 1 if the recipient was ever a colony of the donor, and zero otherwise. It is taken from the CEPII distance dataset (often used in the trade literature) and be accessed from www.cepii.fr/anglaisgraph/bdd/distances .

Language is a selection of variables that take the value 1 if at least 9% of the recipient and donor's populations share a common language, and zero otherwise. It is taken from the CEPII distance dataset (often used in the trade literature) and be accessed from www.cepii.fr/anglaisgraph/bdd/distances .

US Military Grants – Only available for the USA, the data is taken from Neumayer (2003b), and is the value of military aid from the USA by recipients.

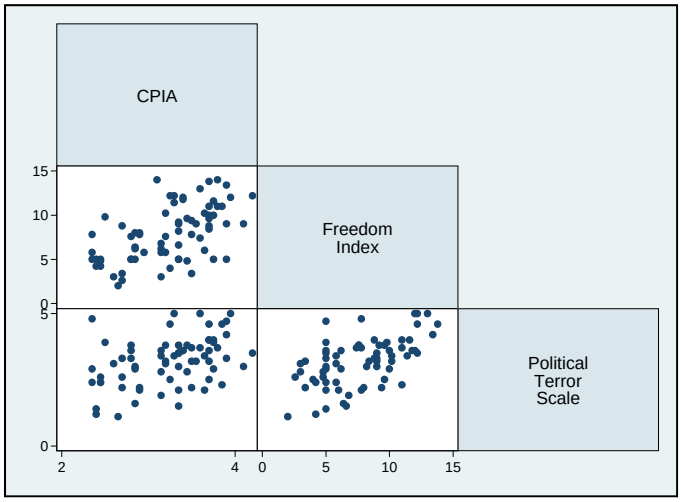
Corruption – The variable is taken from the Worldwide Governance Indicators and is based upon a wide variety of information sources and can be located at info.worldbank.org/governance/wgi/ .

CPI - The Corruption Perception Index is used as a robustness check, and is taken from Transparency International (www.transparency.org).

CPIA- The Country Policy and Institutional Assessment is published by the World Bank for 16 criteria, and can be accessed at : http://go.worldbank.org/7NMQ1P0W10

Distance - is a variable that measures the weighted distance between the 'centre' of two countries, here coded such that one unit represents 1000 km. It is taken from CEPII: http://www.cepii.fr/anglaisgraph/bdd/distances.htm

Figure 5: Correlation between Policy Variables



Note: This Figure only includes data points if available for all three policy variables.

Descriptive Statistics

	Variable	Observations	Mean	Stn. Dev.	Min	Max
General	LN(GDP)	752	8.22	1.16	5.15	11.1
	LN(Population)	880	15.09	2.13	9.9	21
	Freedom	793	7.88	3.78	2	14
	PTS	733	3.42	1.04	1	5
	Protestant/Catholic	930	43.97	39.78	0	99.9
	Buddhist	930	3.37	14.64	0	92
France	Arms	660	9.48	39.11	0	606.4
	Imports	804	0.1	0.17	0	1.2
	Colony	890	0.18	0.38	0	1
	Language	890	0.15	0.36	0	1
Germany	Arms	660	8	36.06	0	401.6
	Imports	806	0.09	0.18	0	1.5
	Colony	890	0.05	0.22	0	1
Japan	Arms	660	0.05	0.9	0	21
	Imports	794	0.13	0.31	0	2.6
Italy	Arms	660	3.3	13.12	0	140.6
	Imports	790	0.11	0.2	0	1.3
	Colony	890	0.01	0.11	0	1
UK	Arms	660	8.5	36.17	0	504.4
	Imports	799	0.09	0.18	0	1.2
	Colony	890	0.34	0.47	0	1
	Language	890	0.32	0.47	0	1
USA	Arms	660	38.3	145.01	0	1259
	Imports	782	0.13	0.29	0	2.7
	Colony	890	0.03	0.17	0	1
	Language	890	0.43	0.49	0	1
	US Military	825	2.2	9.4	0	114.2
Neth.	Arms	660	2.42	18.96	0	346
	Imports	815	0.07	0.15	0	1
	Language	890	0.42	0.49	0	1
Sweden	Arms	660	1.42	11.29	0	220
	Imports	785	0.08	0.16	0	1.1

Appendix

Additional Tables are included by donor, period and step. They are estimated in similar ways to the econometric results shown in the body of the paper, e.g. the first step models use the Probit estimator, and the second OLS. Time period 1 refers to the Cold War period (1982-1991), 2 the post-Cold War period (1992-2001) and 3 the GWOT period (2002-2006). In these regressions each period is estimated separately, meaning the results come from much lower sample sizes than the main regressions. Table A8 gives the non-standardised betas for the equivalent of table 3.

Table A 1 France: By Step and Time Period

Step	1st	1st	1st	2nd	2nd	2nd
Time Period	1	2	3	1	2	3
Ln(GDP)	-0.13	-0.40***	-0.40*	-0.088	-0.090	-0.13
	(1.01)	(3.38)	(2.46)	(1.28)	(1.60)	(1.34)
Ln(Population)	-0.058	-0.18*	0.041	0.38***	0.29***	0.22
	(0.62)	(2.13)	(0.35)	(4.45)	(4.35)	(1.66)
Freedom Index	0.049	0.12***	0.032	-0.074	0.00066	0.073
	(1.35)	(3.97)	(0.74)	(1.31)	(0.013)	(0.80)
Political Terror Scale	-0.048	-0.55***	-0.31	0.12	0.082	-0.019
	(0.30)	(3.76)	(1.18)	(1.80)	(1.38)	(0.15)
Exports	0.050	0.31	-1.17	0.19**	0.35***	0.31**
	(0.063)	(0.37)	(1.33)	(2.69)	(5.86)	(2.83)
Proximity Index	0.16	0.22	-0.19	0.57***	0.62***	0.42***
	(0.43)	(0.63)	(0.42)	(10.4)	(13.7)	(4.72)
Observations	220	270	137	188	212	106
Pseudo R-squared	0.018	0.164	0.183	0.583	0.670	0.378

Table A 2 Germany: By Step and Time Period

Step	1st	1st	1st	2nd	2nd	2nd
T	1	2	3	1	2	3
Ln(GDP)	-0.019	-0.28**	-0.38*	-0.21*	-0.17*	-0.14
	(0.13)	(2.59)	(2.45)	(2.56)	(2.47)	(1.45)
Ln(Population)	-0.0014	-0.037	0.19	0.42***	0.38***	0.46***
	(0.014)	(0.45)	(1.49)	(3.97)	(4.58)	(3.41)
Freedom Index	0.055	0.11***	0.098*	0.15*	0.064	0.077
	(1.38)	(3.46)	(2.19)	(2.39)	(0.97)	(0.81)
Political Terror Scale	-0.046	-0.52***	-0.29	-0.089	-0.0048	0.0050
	(0.28)	(3.58)	(1.06)	(1.19)	(0.063)	(0.038)
Exports	-1.96*	-1.41*	-1.68*	0.22*	0.40***	0.20
	(2.13)	(2.07)	(2.18)	(2.58)	(5.30)	(1.82)
Proximity Index	0.99	0.45	-0.48	-0.032	-0.018	-0.11
	(1.21)	(0.62)	(0.55)	(0.56)	(0.33)	(1.39)
Observations	220	270	137	192	211	105
Pseudo R-squared	0.085	0.175	0.251	0.433	0.454	0.329

Table A 3 Netherlands: By Step and Time Period

Step	1st	1st	1st	2nd	2nd	2nd
T	1	2	3	1	2	3
Ln(GDP)	-0.55***	-0.48***	-0.29	-0.28**	-0.26**	-0.34*
	(4.22)	(4.22)	(1.93)	(2.76)	(2.88)	(2.63)
Ln(Population)	0.066	0.11	0.31*	0.25	0.36***	0.70***
	(0.75)	(1.56)	(2.44)	(1.95)	(3.58)	(3.51)
Freedom Index	0.16***	0.11***	0.12*	0.11	0.32***	0.22
	(4.16)	(3.35)	(2.32)	(1.23)	(3.53)	(1.57)
Political Terror Scale	-0.20	-0.41**	-0.65*	-0.087	-0.12	0.29
	(1.31)	(3.18)	(2.39)	(0.97)	(1.13)	(1.55)
Exports	-4.43**	-2.60**	-3.43*	0.18	-0.079	-0.39*
	(2.98)	(2.64)	(2.38)	(1.67)	(0.80)	(2.37)
Proximity Index	-0.45	-0.40	0.061	0.12	0.087	0.054
	(1.71)	(1.88)	(0.21)	(1.59)	(1.21)	(0.45)
Observations	227	283	141	161	183	64
Pseudo R-squared	0.321	0.285	0.347	0.284	0.192	0.256

Table A 4 Japan: By Step and Time Period

Step	1st	1st	1st	2nd	2nd	2nd
T	1	2	3	1	2	3
Ln(GDP)	-0.33*	-0.37***	-0.56***	-0.20**	-0.098	-0.085
	(2.50)	(3.56)	(3.29)	(2.70)	(1.51)	(0.86)
Ln(Population)	-0.29**	-0.17*	-0.13	0.35***	0.38***	0.54***
	(2.75)	(2.12)	(0.93)	(3.91)	(4.53)	(4.01)
Freedom Index	0.096*	0.14***	0.096	0.16*	0.11	0.070
	(2.36)	(3.83)	(1.89)	(2.53)	(1.58)	(0.67)
Political Terror Scale	-0.15	-0.46**	-0.45	-0.018	0.0036	0.029
	(0.93)	(2.92)	(1.52)	(0.26)	(0.044)	(0.21)
Exports	1.03	0.59	0.39	0.40***	0.43***	0.17
	(1.75)	(1.64)	(0.73)	(5.60)	(6.10)	(1.55)
Proximity Index	4.87	6.41	-2.65	0.078	0.088	-0.019
	(0.46)	(0.39)	(0.12)	(1.33)	(1.57)	(0.21)
Observations	195	223	113	168	179	89
Pseudo R-squared	0.095	0.145	0.219	0.501	0.491	0.341

Table A 5 Sweden: By Step and Time Period

Step	1st	1st	1st	2nd	2nd	2nd
T	1	2	3	1	2	3
Ln(GDP)	-0.25	-0.32**	-0.34*	-0.57***	-0.36**	-0.34*
	(1.95)	(3.02)	(2.21)	(3.93)	(2.84)	(2.57)
Ln(Population)	0.24**	0.25**	0.45***	-0.088	0.35*	0.36
	(2.85)	(3.18)	(3.38)	(0.45)	(2.28)	(1.78)
Freedom Index	0.10**	0.14***	0.10*	0.019	0.21	0.11
	(2.96)	(3.90)	(2.10)	(0.14)	(1.60)	(0.81)
Political Terror Scale	-0.0085	-0.23*	-0.40	-0.064	-0.046	0.081
	(0.065)	(1.99)	(1.49)	(0.48)	(0.33)	(0.48)
Exports	-0.74	-1.44*	-2.44*	0.29	-0.063	-0.17
	(0.72)	(2.07)	(2.56)	(1.81)	(0.45)	(0.98)
Proximity Index	0.44	-0.48	0.45	0.14	0.12	0.080
	(1.09)	(1.31)	(0.80)	(1.30)	(1.16)	(0.72)
Observations	218	268	136	82	96	81
Pseudo R-squared	0.126	0.169	0.339	0.258	0.162	0.113

Table A 6 UK: By Step and Time Period

Step	1st	1st	1st	2nd	2nd	2nd
T	1	2	3	1	2	3
Ln(GDP)	-0.40**	-0.35**	-0.65***	-0.24**	-0.40***	-0.24*
	(3.03)	(3.11)	(3.61)	(2.85)	(5.77)	(2.48)
Ln(Population)	-0.020	0.047	0.14	0.38***	0.52***	0.53***
	(0.23)	(0.62)	(1.00)	(3.36)	(6.22)	(3.53)
Freedom Index	0.15***	0.12***	0.18***	0.0075	0.15*	0.054
	(3.73)	(3.90)	(3.39)	(0.10)	(2.25)	(0.53)
Political Terror Scale	-0.038	-0.51***	-0.68*	0.025	0.091	-0.046
	(0.25)	(3.81)	(2.26)	(0.30)	(1.25)	(0.35)
Exports	-1.15	-1.04	-1.66	0.22*	0.098	0.029
	(1.42)	(1.24)	(1.35)	(2.53)	(1.30)	(0.26)
Proximity Index	0.42	0.50	1.42**	0.35***	0.45***	0.46***
	(1.25)	(1.96)	(3.03)	(5.04)	(8.15)	(5.30)
Observations	220	269	137	177	185	89
Pseudo R-squared	0.198	0.190	0.376	0.414	0.549	0.473

Table A 7 USA: By Step and Time Period

Step	1st	1st	1st	2nd	2nd	2nd
T	1	2	3	1	2	3
Ln(GDP)	-0.65***	-0.64***	-0.56***	-0.10	-0.087	-0.19*
	(5.26)	(5.49)	(3.65)	(1.32)	(1.64)	(2.50)
Ln(Population)	-0.14	-0.18*	-0.089	0.23*	0.32***	0.089
	(1.62)	(2.37)	(0.80)	(2.55)	(5.22)	(0.86)
Freedom Index	0.14***	0.21***	0.082	0.11	0.014	0.090
	(3.79)	(5.86)	(1.79)	(1.59)	(0.25)	(1.11)
Political Terror Scale	-0.24	-0.58***	-0.47	-0.11	-0.021	-0.30**
	(1.61)	(4.00)	(1.67)	(1.46)	(0.34)	(2.74)
Exports	-0.13	1.04*	0.85	-0.069	-0.18***	-0.073
	(0.31)	(2.09)	(1.17)	(0.90)	(3.35)	(0.89)
Proximity Index	0.57	-1.93	-2.38	0.62***	0.79***	0.56***
	(0.50)	(1.72)	(0.83)	(9.88)	(17.9)	(7.80)
Observations	215	268	136	157	198	106
Pseudo R-squared	0.195	0.257	0.211	0.538	0.683	0.568

Table A 8: 2nd Step with Non-Standardised Betas

	France	Germany	Japan	Netherlands	Sweden	USA	UK
Ln(GDP)	-0.018	-0.051**	-0.055*	-0.12***	-0.14***	-0.015	-0.11***
	(1.19)	(2.81)	(2.58)	(4.33)	(3.95)	(0.90)	(5.24)
Ln(Population)	0.073***	0.092***	0.13***	0.068***	0.059*	0.063***	0.11***
	(6.48)	(6.87)	(7.72)	(3.73)	(2.30)	(5.58)	(7.25)
Freedom Index	-0.0029	0.0066	0.015*	0.016*	-0.0043	0.0038	0.0056
	(0.63)	(1.20)	(2.25)	(2.12)	(0.40)	(0.75)	(0.90)
Political Terror Scale	0.040*	0.00072	0.021	0.025	0.043	-0.032	0.027
	(2.36)	(0.035)	(0.76)	(0.94)	(1.17)	(1.75)	(1.20)
Religion	0.00028	-0.00046	0.0040***	-0.000057	0.00079	0.00060	0.00042
	(0.82)	(1.07)	(3.47)	(0.10)	(1.09)	(1.47)	(0.87)
Arms	-0.00053	0.00092*	0.028	0.010***	0.0037	-0.00034*	0.00091
	(1.32)	(2.12)	(1.62)	(6.28)	(0.37)	(2.59)	(1.53)
Exports	0.58***	0.46***	0.53***	-0.11	0.039	-0.12*	0.24
	(5.48)	(3.37)	(7.41)	(0.34)	(0.17)	(2.28)	(1.40)
Colony	0.37***	-0.052				0.21	0.41***
	(9.37)	(0.70)				(1.89)	(9.06)
Language	0.17***			-0.094*		0.081**	-0.00098
	(4.48)			(2.30)		(2.78)	(0.021)
US Military Grants						0.038***	
						(17.0)	
Constant	-1.05***	-0.83**	-1.53***	-0.010	0.28	-0.65*	-0.93**
	(3.88)	(2.62)	(4.03)	(0.023)	(0.49)	(2.51)	(2.59)
Observations	436	442	436	369	234	400	393
Adjusted R-squared	0.554	0.390	0.472	0.257	0.123	0.563	0.509

Note: This table is the non-standardised version of table 3.