AID ALLOCATION, COMPOSITION AND EFFECTS

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"... if you pour yourself out for the hungry and satisfy the desire of the afflicted,
then shall your light rise in the darkness and your gloom be as the noonday"
Isaiah 58.10 (ESV)

and

"Desire without knowledge is not good,
and whoever makes haste with his feet misses the way"

Proverbs 19.2 (ESV)

This process of knowledge creation has certainly not felt hasty, but neither has it diminished the desire.

1. INTRODUCTION

Aid is not homogeneous in goal, composition or effect. This is a common theme throughout the thesis, and while it is a simple assertion it has far-reaching consequences. The idea of a heterogeneous effect has been partially incorporated into the aid-growth literature, and as research on development aid inevitably takes place in the shadow of the controversial research by Burnside and Dollar (1998; 2000, BD hereafter), this is a sensible starting point. That research has cast a long shadow for a number of reasons. First, it offered a simple analysis that 'aid works in a good policy environment'. This is both intuitively plausible, and gives clear guidance as to its implications. Second, the research was given prominence in an important World Bank (1998) report. Third, while many research ideas become popular in their own right, this has also affected policy: there is an "unusually clear link running from a growth regression in an economic study to a policy outcome." (Easterly, 2003).

Perhaps in part because of its impact, the BD research has been widely criticised. The most direct attack on the BD results is also the simplest. Easterly, et al. (2004) retain the methodology, specification and country coverage of BD, but extend the sample from 1970–93 to 1970–97, and include previously excluded observations. This simple extension results in the aforementioned result disappearing (although it is replicated in the 1970–93 period). Burnside and Dollar (2004) have engaged with this criticism, and a number of papers corroborate their general conclusion (Collier and Dehn, 2001; Collier and Dollar, 2002, 2004; Collier and Hoeffler, 2004). However, a large number of papers directly attack the BD results and methodology (these include, but are not limited to, Dalgaard and Hansen, 2001; Easterly, 2003; Guillaumont and Chauvet, 2001; Hansen and Tarp, 2000; Hansen and Tarp, 2001; Lensink and White, 2001; Roodman, 2007).

While there have been many critiques and debates focused directly upon BD, the literature on aid effectiveness (narrowly defined as the effect of aid on growth) has splintered. Various stories have been told, each proposing a variable that determines the effect of aid on growth. The growth effect of aid has been found to be conditional on: the amount of aid (i.e. it has diminishing returns, Hansen and Tarp, 2001; Lensink and White, 2001); the economic environment (Guillaumont and Chauvet, 2001, who find aid helps to mitigate the negative effects of certain adverse economic circumstances); export price shocks (Collier and Dehn, 2001); civil war *and* good policies (Collier and Hoeffler, 2004); volatility (Lensink and

Morrissey, 2000); investment (Gomanee *et al.*, 2005a), and whether or not the recipient is in the tropics (Dalgaard *et al.*, 2004).

A number of surveys and general discussions of the literature are available (Doucouliagos and Paldam, 2008; Doucouliagos and Paldam, 2009; Doucouliagos and Paldam, 2010; Hansen and Tarp, 2000; Roodman, 2007). However, a striking point is that the papers are so copious and profuse as to allow different summaries of the literature to arrive at opposite conclusions, each citing a large evidence base:

> *"When we review the literature we find that the overwhelming majority of recent empirical studies do conclude that aid increases economic growth, and below we cite more than 30 studies that find that growth would be lower in the absence of aid." (Addison et al., 2005)*

> *"There would appear to be one inescapable conclusion from the preceding data… the vast majority of the literature finds that aid is effective in promoting growth" (McGillivray, 2004b)*

> *"There appear to be almost no findings in the contemporary literature that a) find a significant effect of aid on growth, b) are robust, and c) are free of the methodological problems described here." (Roodman, 2008)*

It is worth noting that the distinct conclusions found within the aid-growth literature are perhaps unavoidable. In order to accurately estimate the effect of aid on growth, the econometrician needs to successfully control for other factors. However, much of the profession is sceptical regarding our ability to do this (Rodrik, 2006). Indeed, there are also a large number of technical econometric complications as well (Durlauf *et al.*, 2005; Roodman, 2008).This difficulty means a variety of specifications and approaches will be used, and so a diverse set of results is to be expected. Unfortunately in cross-country regressions on the aid-growth link, Leamer's (1983) comment is apt: "...hardly anyone takes anyone else's data analysis seriously."

One effect of the sheer size of the aid-growth literature has been the promotion of the growth effect of aid as the salient criterion on which aid should be judged. This comes in part from a less than fully realised understanding of the heterogeneity of aid's goal, composition and effect. Clemens *et al.* (2004) argue that the heterogeneous goals of aid mean we should expect heterogeneous effects. Indeed, as they argue, if economic growth is not the objective

of a large proportion of aid, it is unsurprising if it does not meet that objective. However, the persistent focus and attention on the aid-growth link, coupled with contrasting conclusions, has fuelled the belief that aid is inherently and severely flawed, as the number of critics in the public sphere illustrates (Bolton, 2008; Calderisi, 2006; Glennie, 2008; Moyo, 2009). However, if growth is not the objective, aid may still 'work' when measured by a different outcome. Alternatively, acknowledging the heterogeneous effects of aid means a more nuanced view can be taken, as different types of aid given for different reasons have different effects. Truly incorporating this would mean allowing for a growth effect of aid to be more complicated than purely mediated by one factor.

The unfortunate focus on establishing a growth effect of aid through cross-country regressions has meant that other questions regarding aid have been less widely discussed. However, a focus on the effects of aid is both understandable and fitting. The recurring critique is that aid is not as effective as it should be: Easterly (2006, pp.154) states that $2.3 trillion has been spent on aid over five decades with little measurable effect. As mentioned, without a counterfactual it is difficult to truly understand the effect of aid. It is clear, however, that while "foreign aid has at times been a spectacular success... [it] has also been, at times, an unmitigated failure" (World Bank, 1998, pp.1) The recognition that aid has heterogeneous effects immediately implies that aid can become more effective as the causes of this differential impact are understood, and such knowledge utilised. The desire to maximise utility is at the heart of economics, and so a desire to increase aid effectiveness is not remarkable. However, the fact that the situations of many of the world's citizens remain so dire after so much aid explains the intensity of this focus.

So, what is to be done? While the question of aid effectiveness remains important, the econometric research in the field has reached something of an impasse. Indeed, the point of diminishing returns of further research into the growth effect of aid has surely been reached. Rather than add to the voluminous literature on aid effectiveness, we take a different approach, agreeing with Rajan and Subramanian (2005, pp.22-23) that "...it is perhaps more fruitful to move beyond the inconclusive debate of whether aid is effective, and focus on specific ways it can be made to work better, by better understanding the reasons that might impair or enhance its effectiveness." This thesis engages with more specific questions, with the ultimate aim of improving our understanding of the broader ones. For example, a recurring theme in the thesis is governance. While attempts to

establish the effect of aid on growth face technical problems, focusing on more narrowly defined research questions enables us to examine specific channels of effect from aid to governance, and from governance to growth. Thus while this is not 'aid effectiveness research', it should make a valuable contribution to our understanding of the effectiveness of aid.

If governance is undermined by aid, this would represent a major concern over the effects of aid. A commonly cited channel for this detrimental effect is the link between aid and tax, a topic dealt with in the **second chapter**. Specifically, the concern is that aid results in the state becoming too independent from its own citizens, as the taxation link is weakened due to the increase in external finance (Moore, 2007). The recent historical evidence shows that the link between taxation and governance is strong (Ross, 2004), and so any evidence that aid breaks this link, and so undermines good governance, is an important matter. Knack (2001) reports that aid has indeed damaged governance on average, but without modelling the channel endogeneity is a problematic complication. However, governance is not the only concern when considering the relationship between aid and tax. If there is found to be a negative tax revenue effect of aid, donors may find that they are financing tax cuts rather than investment. If this is the case, aid may be non-additional but rather crowd-out development expenditure.

I focus on the specific link between aid and tax, and the assertion that different types of aid have different tax effects. This claim comes from Gupta *et al.* (2004), who state that aid loans have a positive relationship with tax revenues whereas for aid grants that relationship is negative. If this link is established, it implies a clear policy recommendation for donors to minimise a possible source of negative governance effects. However, aid loans have a problematic history, as they have helped produce heavily indebted countries, and have in some cases meant aid meant a net negative revenue transfer. Establishing the strength of evidence of a differential tax revenue effect is therefore crucial.

The broader debate over the relative merits of aid grants and aid loans became both controversial and newsworthy in 2001 when there was a disagreement between donor nations over the amount that the IDA (the World Bank's soft loan financing arm) should disburse in the form of aid grants (World Bank, 2001). The second chapter deals with both the general arguments surrounding the two modalities, and the specific question of their tax revenue effect.

The **third chapter** is an extended literature review of various types of research that have examined aid allocation. It is important to understand aid allocation because it sheds light upon the goal with which aid is given, and informs expectations as to its possible effects. By summarising four approaches, the chapter gives a broader view of aid allocation. First, it discusses literature that proposes an ideal allocation, as informed by some allocation principle. Second are the tools used by donors to calculate appropriate allocations. These shed light on the relative importance of different factors, as proposed by donors themselves. However, the third literature discussed uses actual allocations to infer the relative importance of these factors. These revealed preferences inform discussions of the goals of aid. Fourth is the disparate descriptive literature that describes various aspects of how donors give aid. Appendix B includes a longer discussion of this literature, including the various 'donor errors' that they try to capture.

The **fourth chapter** is a contribution to the positive aid allocation literature. It examines aid allocation practice for seven donors over a 25 year period. The 4P specification is used, as the factors that influence donors are assigned amongst poverty, population, policy and proximity. The previous specification in general use was originally conceptualised as a dichotomy between recipient need and donor interest (the RN-DI model). The chapter differs by allowing donor interest to be replaced by a broader notion of proximity. This means that donors are not necessarily understood to be selfish for giving aid to countries that are geographically closer. Proximity is taken in a broad sense, meaning geographical, commercial, historical, cultural and linguistic proximity are all included. Population is by now a common augment to this specification, but policy has increasingly become important because of the Burnside and Dollar (1998, 2000) research.

The chapter makes a salient contribution to a specific debate within the literature regarding aid selectivity. A number of papers argue that policy has been an increasingly important factor in aid allocation (Berthélemy and Tichit, 2004; Dollar and Levin, 2006) whereas others report that aid selectivity has been low and static (Easterly, 2007; Hout, 2007a, b; Nunnenkamp and Thiele, 2006). The chapter contributes to the debate in a number of important ways. First, we estimate individually by donor, rather than pooling all donors. While pooling all donors may seem an appropriate action, it can lead to erroneous conclusions. For example, if all major donors are insensitive to policy, but numerous smaller donors have become marginally more policy sensitive in recent years, a pooled (non-

weighted) regression could lead to a conclusion of increased policy selectivity. In this situation, it is not that aid has become more sensitive to policy, but rather that aid donors (in a non-weighted average) have become more sensitive to policy.

The fourth chapter finds little evidence that policy selectivity has been practised. While it is beyond the scope of this research to fully explain why this might be the case, the Samaritan's Dilemma offers an interesting insight. The Samaritan's Dilemma was presented by Buchanan (1977) as, put simply, 'helping someone may undermine their incentives to work'. In the aid context 'work' or 'effort' are generally captured by governance. Svensson (2000) used this moral hazard problem framework to examine the incentives a donor faces, especially in the context of conditionality. To summarise, the donor and recipient first agree a contract of reform and aid. In the second period the recipient then reneges on the promise, but the donor does not punish the recipient as it does not want to punish the poor within that country. It may be that in the long term punishing the recipient would be the most effective action, but donors are constrained by their current incentives. This inability to withhold aid (which I term *Samaritan impulse*) in the second period explains neatly why conditionality has failed to be used (Collier, 1997). However, there is no reason why moving the punishment of the recipient to the first period should solve the Samaritan's Dilemma. This, while not fully explored, hints at why the fourth chapter fails to find evidence for increased policy selectivity.

The **fifth chapter** deals with something that may 'solve' the Samaritan's Dilemma: pragmatic selectivity. While the Samaritan impulse deals with the inability to alter the amount of aid in response to different levels of governance, pragmatic selectivity introduces the composition of aid as a second policy lever. Through policy documents it is identified that while the volume of aid may be chosen to satisfy the Samaritan impulse, implicitly the type of aid is chosen so as to satisfy the desire for efficiency. The theoretical literature that deals with modality choice is reviewed, before an original but simple model is introduced. The empirical section tests both the extent (if any) of pragmatic selectivity, but also the factors that determine the composition of aid more generally. The results confirm that the relative efficiency of modalities is important, and also that this effect is larger than that of preference alignment.

The **sixth chapter** finishes, with a brief conclusion drawing together the research.

2. THE TAX REVENUE EFFECTS OF AID GRANTS AND LOANS

In the conventional aid effectiveness literature, aid has been judged by its ability to increase growth rates in the recipient country. Those expecting a robust and unambiguously positive effect from aid have been disappointed (Roodman, 2007), which has led to more attention on aid itself. Specifically, it is now widely recognised that aid is not homogenous in goal: "economic growth is not the sole objective of foreign aid, and in some cases it is not the objective at all." (Clemens *et al.*, 2004, p.1) At the heart of this shift is the acceptance that different types of aid do not just differ in aim but also in effect. It is then important what aid finances, implying a need to address fungibility, especially if from productive to non-productive spending, and broader fiscal effects. Consumption spending is thought to be an 'unproductive' use of aid, and aid that finances investment is considered as more likely to have a positive impact on growth (Gomanee *et al.*, 2005a). However, to the extent that consumption spending finances the provision of public goods, aid used in this way may contribute to increasing welfare, growth and reducing poverty (Gomanee *et al.*, 2005b; Mosley *et al.*, 2004). In short this is an issue of fungibility, as aid can be redirected from the proposed aim of increasing investment to either increasing consumption or substituting for tax revenue. In this case total revenue and expenditure increases by less than the aid hence the potential benefits from aid are not fully realised (McGillivray and Morrissey, 2001).

Thus donors wish to know what they are funding, be it tax cuts, consumption or investment. However, the link between domestic tax and foreign aid is not merely an issue of fungibility; the importance of the relationship between foreign aid and domestic revenue can also be approached from a political economy perspective. Bräutigam and Knack (2004) find that high aid receipts are associated with deteriorating governance. Moss *et al.* (2006) focus on a possible channel for this, arguing that if aid substitutes for tax revenue the citizen-government relationship can be undermined. Thus while the short-term concern for the donor may simply be whether they are funding investment or tax cuts, this aid-tax link has broader and longer-lasting implications.

Recently, an argument that the aid-tax revenue relationship is conditional upon the type of aid has surfaced. The idea is that if aid is given in loan form, it does not affect tax revenue collection. However if aid is given in the form of grants, there is an adverse effect upon tax

revenue collection which may in turn diminish any positive effect of aid. Existing evidence on this point is in two parts: cross-country and single-country. The cross-country evidence is based upon cross-country econometrics, where a tax revenue equation is augmented with the two aid variables. This evidence is, so far, supportive of the idea (Gupta *et al.*, 2004; Odedokun, 2003). The single-country evidence however, does not support the conclusion that aid reduces tax effort (Morrissey *et al.*, 2007; Osei *et al.*, 2005). This paper contributes to this literature by adding to the cross-country evidence for the effect of aid on the tax/GDP ratio for a large set of developing countries over the period 1970-2005.

Section 2 reviews the debate regarding loans and grants. Section 3 outlines the current cross-country research and econometric methodology. Section 4 reports the results and further evidence. Section 5 discusses, and Section 6 concludes.

2.2 GRANTS VERSUS LOANS

This chapter contributes to the wider debate regarding whether aid should be given in the form of grants ('free' money with no obligation to repay) or loans (an obligation to repay, albeit on concessional terms). The most common argument, dating back to Schmidt (1964), is that loans are used more 'effectively' than grants. This argument relates to the fiscal behaviour of the recipient governments – because they have to be repaid, loans encourage better fiscal management and greater tax effort than grants while, additionally, the latter may substitute for (discourage) domestic revenue efforts (Gupta *et al.*, 2004). In financial terms the difference relates to the degree of concessionality of the transfer relative to market terms – for aid the important feature is that the grant element or concessionality is at least 25% although it tends to be far higher, especially for the poorest countries (Morrissey and White, 1996). Cordella and Ulku (2007) argue that higher concessionality is more likely to be growth promoting in poor countries with heavy debt obligations because it adds less to the debt burden and so does not increase the likelihood of default. If grants replace loans, debt burdens are reduced and the impact on growth is potentially positive. As grants are fully concessional they should be favoured for indebted poor countries.

The report of the International Financial Institution Advisory Commission (IFIAC, 2000) recommended that the World Bank should replace its loan programme, specifically IDA, with a grant programme aimed at poverty alleviation, and the United States administration supported this on the basis of three arguments (Sanford, 2002). First, increased grants would

not add to heavy debt burdens in poor countries. Second, loans are inappropriate for financing social sector programs, such as education, as the benefits are realised over a long time period and do not generate financial returns to service a loan (Salazar, 2002). Third, as grants do not burden recipients with the need for repayment they allow greater political scope for donors to require recipient commitment to 'development goals', i.e. to impose conditions (Sanford, 2002).[1] There are dissenting voices. Some think that the move towards grants has already gone too far (Nunnenkamp *et al.*, 2005). Britain was vocal in its opposition to the US IDA grants proposal (DFID, 2001), arguing that the current system made effective use of the limited resources available.

None of the arguments on financing, promoting conditionality or managing debt burdens are persuasive in choosing between concessional loans and grants. As aid loans are highly concessional, there is no strong argument against being fully concessional and providing grants. The choice may well come down to the one empirically testable proposition: the effect on tax revenue. If Gupta *et al.* (2004) are correct that loans encourage but grants discourage tax effort, then there is an argument against grants. If they are incorrect (and the case-study evidence is correct), the balance of arguments favours grants for the poorest countries. The paper addresses this directly.

2.3 METHODOLOGY

To answer this question, we must engage with the literature on the determinants of tax revenue. Unfortunately, cross-country studies of the determinants of the tax/GDP ratio (Tanzi, 1992) rarely include aid among the explanatory variables. Teera and Hudson (2004) find the coefficient on aid to be insignificant in their estimates of tax performance in developing countries. Gupta *et al.* (2004) find that aid has a negative effect on tax effort, but that this varies depending on the type of aid: loans are positively correlated with tax revenue whereas grants are negatively correlated with tax revenue, and infer that grants induce lower tax effort. A core part of this chapter reinvestigates this relationship distinguishing between loans and grants, arguing that the effect of grants has become positive since the mid-1980s. This is the period since adjustment lending became the modus operandi of aid,

[1] *However, there is no reason a priori to imagine that grants would be a more efficient means for donors to influence policy in a recipient country. Loans may provide a better incentive for donors to monitor recipients. While grants can be used selectively to 'reward' those countries that are either already aligned with donor preferences or proved malleable in the past – 'no results, no funds expended, and no funds diverted to offshore bank accounts, vanity projects or private jets' (Lerrick and Meltzer, 2002, p.1) – concessional loans can be deployed in the same way.*

adding to the evidence that conditional lending tends to be associated with more positive effects of aid (see also Mosley *et al.*, 2004).

To our knowledge, only two previous studies have specifically examined the effect of the composition of aid on tax revenue *across* countries. Gupta *et al.* (2004), with data from 107 low and middle-income countries pooled over the period 1970-2000, and Odedokun (2003), with data for 72 low-income countries pooled over the period 1970-99, use cross-country regression analysis to investigate the response of recipient government revenue effort to aid. Specifically, they test whether the tax/GDP ratio differs in response to aid grants and loans. Both find evidence to support the hypothesis that aid grants reduce recipient government tax effort (as represented by the tax/GDP ratio), and that concessional loans increase recipient tax effort. As they carry out a more sophisticated econometric analysis, Gupta *et al.* (2004) is the focus of our discussion and motivates our analysis.

The empirical approach of Gupta *et al.* (2004) follows Lotz and Morss (1967) to model the tax to GDP ratio as determined by variables chosen to proxy for the tax base structure of the economy. The approach is similar to the standard treatment in the literature on the determinants of the tax/GDP ratio (Tanzi, 1992; Teera and Hudson, 2004); additional explanatory variables are often included, although Gupta (2007)[1] shows that few variables are robustly significant. Coefficients from a cross-country regression are interpreted as the 'average' effective tax rate for each tax base, which can be used to construct an index of 'taxable capacity' for each country which, when compared to actual tax ratios, provides an indicator of tax effort (Stotsky and WoldeMariam, 1997). Gupta *et al.* (2004) augment a 'typical' regression equation for tax effort by adding both grants (G) and net concessional loans (L) as a percentage of GDP. Their baseline regression equation takes the form:

$$ln\left(\frac{Tax}{GDP}\right)_{i,t} = \beta_0 + \beta_1 Agr_{i,t} + \beta_2 Ind_{i,t} + \beta_3 Trade_{i,t} + \beta_4 Income_{i,t} + \beta_5 G_{i,t} + \beta_6 L_{i,t} + \varepsilon_{i,t} \quad (1)$$

They control for four determinants of taxable capacity. Agricultural (*Agr*) and Industrial (*Ind*) value added as a percentage of GDP are included as the sector composition of value added is viewed as a key determinant of the tax base. A large agricultural sector reduces taxable capacity ($\beta_1 < 0$) as agriculture is largely a subsistence activity in most low-income

[1] *The reader should note that Gupta et al. (2004) and Gupta (2007) are not only different papers, but are written by different authors.*

countries, which is difficult to tax directly (Emran and Stiglitz, 2005). A large industrial sector is easier to monitor and tax ($\beta_2 > 0$). Openness (*Trade*, the sum of exports and imports as a percentage of GDP), is included as trade taxes are relatively easy to collect ($\beta_3 > 0$) and are a major share of tax revenue in low-income countries (Ghura, 1998; Greenaway and Milner, 1991). GDP per capita (*Income*) is included as a proxy for the level of economic development ($\beta_4 > 0$), to capture increased tax buoyancy (the responsiveness of revenue to income growth) and collection efficiency. However, empirical evidence on the relationship between the tax/GDP ratio and per capita GDP, controlling for economic structure, is inconclusive (Tanzi, 1992). As the dependent variable is non-negative and skewed, the log transformation is used to provide a normal distribution. Nonlinearities in the relationship between aid and the tax ratio are allowed for by the inclusion of squared aid variables.

Gupta *et al.* (2004) estimate equation (1) using alternative techniques and specifications (including other explanatory variables), and the results are found to be quite robust. The coefficient on Agriculture is negative and significant while the coefficients on Industry and Trade are positive and significant. The coefficient on GDP per capita, however, is consistently negative and significant, contrary to expectations.[1] The coefficient on Loans is positive and that on Grants is negative, including when lagged values are used, and both are almost always significant. These results are interpreted as implying that 'a doubling of grants from an average of 4 per cent of GDP to 8 per cent of GDP could decrease revenues by just 0.4 percentage point of GDP' (Gupta *et al.*, 2004, p.402). We make two changes to the specification, first by treating imports (M) and exports (X) separately as the revenue implications should differ (they are not taxed at the same rate and, in general, under trade reforms since the 1980s export taxes were largely eliminated before import taxes were reduced). Second, we allow for diminishing returns to the income variable by including its square. The resulting specification follows:

$$ln\left(\frac{Tax}{GDP}\right)_{i,t} = \beta_0 + \beta_1 Agr_{i,t} + \beta_2 Ind_{i,t} + \beta_3 M_{i,t} + \beta_4 X_{i,t} + \beta_5 Income_{i,t} + \beta_6 Income_{i,t}^2 + \beta_7 G_{i,t} + \beta_8 G_{i,t}^2 + \beta_9 L_{i,t} + \beta_{10} L_{i,t}^2 + \varepsilon_{i,t} \quad (2)$$

A concern with this specification is potential endogeneity. Endogeneity bias arises when an

[1] *Teera and Hudson (2004) also find the coefficient on per capita income to be negative (when significant), and interestingly also find a negative coefficient on the share of manufacturing for developing countries in their sample.*

independent variable (aid in this case) is correlated with the error term, i.e. the variable is endogenous (or not orthogonal to the error term). Endogeneity refers to an inherent identification problem: either because there is a potential bi-directional contemporaneous relationship between the dependent variable and the independent variable of interest, and/or because an omitted third variable may co-determine both, the true effect of the independent variable on the dependent variable is not identified. To ensure identification one needs a predetermined variable, an instrument that is not correlated with the error term. In some cases, an appropriately lagged aid term may be predetermined and hence a valid instrument (correlated with the endogenous regressor but not with the error term); otherwise, one looks for other predetermined variables.

The instrumental variables (IV) approach is a standard method of dealing with endogeneity: one identifies a set of variables that are correlated with the dependent and specific independent variables but are not correlated with the error term. The IV approach is problematic, especially for the current context. Although the aid allocation literature has identified a number of potential instruments that are correlated with aid, these are likely to be weak instruments if tax/GDP exhibits 'heterogeneous responses' to the aid variables, i.e. the effect of aid on tax/GDP across countries is not uniform. The aim is to estimate the average response; if the sample contains some countries where tax/GDP responds to aid and others where it does not, the IV method would estimate ('impose') the coefficient on aid for the former group of countries on the entire sample. Furthermore, IVs generally exhibit high standard errors and low significance, so when using fixed effects estimation there is too little variance to identify the coefficients (i.e. IV does not solve the identification problem). However, the fixed effects estimator is an appropriate method for (2) given that unobserved country-specific characteristics are likely to help explain cross-country differences in tax/GDP ratios.

Two further problems with the IV approach arise specifically for the specification in (2). First, there are two aid terms; while the same instrument set can be used for each, either loans or grants or both will not be well identified (the instrument set is likely to be weak). Second, the inclusion of quadratic aid terms implies that these must be instrumented (which imposes demands on the limited number of available instruments) or excluded. These concerns imply that the standard IV method is not appropriate. Nevertheless, we report IV results in the appendix (Table A 1): these results are quite similar to those reported below, and it

transpires that, using annual data, aid lagged twice is a statistically valid instrument (in addition to total global aid disbursed and a colony variable). The results support the use of lagged aid below; whilst aid may take a number of years to influence tax revenue, and thus lagged aid may be correlated with the error terms (and not a valid instrument), suitably lagged aid serves the purpose of an instrument and captures the dynamic effect of aid.

Specification (2) is quite parsimonious, and there may be a problem of omitted variables. Gupta (2007) examined some 20 potential determinants of tax/GDP (in a sample of up to 105 countries); few other than those included as controls in (2) were robustly significant. The two exceptions are debt share and the share of taxes from income, profits and capital gains, both of which were mostly (but not always) significant. The former is likely to be endogenous; ceteris paribus, countries with high debt are likely to have higher taxes while countries with a tax shortfall are likely to increase debt. Data on the latter are not available for most of our sample. The analysis of Gupta (2007) suggests that (2) includes the variables commonly found to be significant (and unobserved country-specific variables are allowed for in the estimation).

2.4 RESULTS

We here estimate (2) for a larger sample of countries over a longer time period (details on the data sources and definitions are provided in Appendix A). Although the extended dataset covers 107 countries over the period 1970-2005, there are many missing observations so the number of countries used in each regression is smaller (a maximum of 82). Table 2.1 reports the results of estimating (2) using fixed (FE) effects estimators for the full sample; the first set of results include current aid, the second set lagged aid (as a control for potential endogeneity). The FE estimator has the advantage of allowing for unobserved country specific characteristics that do not change over time. If the unobserved effects are correlated with the other explanatory variables, failing to take them into account will bias the results.[1]

The results in Table 2.1 are very similar to Gupta *et al.* (2004); all coefficients have the same sign and are of similar magnitude and significance. Including additional years in the sample

[1] *It may be the case that any unobserved effects are uncorrelated with all explanatory variables in any time period, in which case random effects (RE) will produce more efficient estimators. If there is a correlation, the RE estimators will be inconsistent. It transpires that FE is preferred for our estimation, although RE provides very similar results and are provided in Appendix A, Table A 2.*

(for the same countries) does not alter the results. The distinction between imports and exports appears to be vindicated as the coefficients are significant but with opposite signs, and even to off-set one another (although in the sample the value of *Imports* tends to be greater than *Exports*).[1] It is not evident why the coefficient on exports should be negative (we consider reasons later after seeing if the results persist under alternative estimates). The coefficient on *Agriculture* is negative and significant while the coefficient on Industry is positive and significant. For the aid variables, L*oans* are positively related, and G*rants* negatively related to the tax to GDP ratio and both are significant. The coefficient on *Income* is significant but negative; although this is not what is expected it is a common finding in the literature. However, when we augment (2) to include income-squared, the coefficient on income becomes insignificant whereas that on income-squared is negative and significant (other variables are qualitatively unaffected). This suggests a non-linear relationship between income and tax/GDP such that beyond some income threshold 'tax effort' declines ceteris paribus.

To control for the possibility of endogeneity bias, that donors give more aid in the form of grants to fiscally constrained countries (or that an unobserved variable influences both aid composition and tax effort), we use aid variables lagged one year, on the assumption that the current tax/GDP ratio will not influence the magnitude or composition of previous year aid flows (as noted above, this assumption may not hold, but the use of lagged aid may still be appropriate). The results using lagged aid variables are given in the final two columns of Table 2.1.

[1] *This shows that the two should not be combined. Gupta* et al. *(2004) most probably found a positive significant effect because imports are higher in the sample.*

Table 2.1 Determinants of Tax/GDP Ratios (FE), 1970-2005

Variable	Current Aid		Lagged Aid	
Loans	1.25***	1.31***	0.69**	0.82**
	(3.57)	(3.88)	(1.99)	(2.44)
Loans 2	-0.0028*	-0.0028*	-0.0012	-0.0016
	(1.78)	(1.84)	(0.59)	(0.81)
Grants	-1.18***	-1.00***	-0.76***	-0.62***
	(5.42)	(4.73)	(3.48)	(2.92)
Grants2	0.00033	0.00029	-0.000014	-0.000075
	(1.49)	(1.35)	(0.05)	(0.29)
Agriculture	-1.16***	-0.86***	-1.17***	-0.85***
	(8.59)	(6.37)	(8.54)	(6.26)
Industry	0.51***	0.78***	0.55***	0.85***
	(3.48)	(5.38)	(3.73)	(5.79)
GDP	-0.0054***	0.0008	-0.005***	0.0014
	(8.82)	(0.92)	(8.46)	(1.51)
GDP2		-0.0000004***		-0.0000004***
		(9.45)		(10.04)
Imports	0.55***	0.49***	0.50***	0.44***
	(6.10)	(5.67)	(5.59)	(5.11)
Exports	-0.51***	-0.54***	-0.45***	-0.48***
	(5.17)	(5.55)	(4.52)	(5.00)
N	1554	1554	1558	1558
Countries	82	82	81	81
F-Stat	30.4	37.9	28.0	37.0
Overall R2	0.34	0.40	0.33	0.38

*Notes: The dependent variable is the log of the Tax/GDP ratio multiplied by 100 to assist interpretation. All independent variables are measured as a percentage of GDP, except GDP (per capita). Numbers in parentheses are t-statistics; ***, ** and * indicate significance at 1%, 5% and 10% levels respectively. N is the total number of observations and the number of Countries is also reported. Fixed Effects are shown; results using Random Effects are similar and can be found in the appendix, Table A 2.*

As the data cover a 35 year period, the next step of the research is to ascertain whether there have been any changes over the period. There is no reason *a priori* why we would expect a consistent relationship over a 35 year period, especially when there have been large changes to both tax and aid. Specifically, aid volumes have grown substantially since 1970, and there has been a general transition from trade-based to income-based taxes (e.g. Attila *et al.*, 2009). Table 2.2 reports the results of estimating (2) for the twenty-year period 1970-1990, and Table 2.3 does the same but for the period 1985-2005. While many variables have similar relationships, the difference is clear for the aid variables, as well as exports and

agriculture. The change from significant negative coefficients to significant positive coefficients for grants suggests that the relationship with revenue may have changed dramatically within the period 1970-2005. We now investigate this possibility more thoroughly.

Table 2.2 Determinants of Tax/GDP Ratios (FE), 1970-1990

Variable	Current Aid		Lagged Aid	
Loans	0.23	0.63	-1.48***	-1.16**
	(0.39)	(1.10)	(2.73)	(2.23)
Loans 2	0.051***	0.040***	0.12***	0.11***
	(3.19)	(2.59)	(5.90)	(5.79)
Grants	-1.99***	-1.66***	-1.28***	-0.98***
	(6.33)	(5.46)	(4.06)	(3.24)
Grants2	0.0035	0.0028	-0.00055	-0.0020
	(1.52)	(1.29)	(0.17)	(0.64)
Agriculture	-1.15***	-0.57***	-1.10***	-0.51**
	(5.56)	(2.71)	(5.34)	(2.48)
Industry	0.54**	0.90***	0.72***	1.07***
	(2.27)	(3.90)	(3.09)	(4.74)
GDP	-0.0078***	0.0033**	-0.0074***	0.0041***
	(8.26)	(2.12)	(7.81)	(2.70)
GDP2		-0.0000004***		-0.0000004***
		(8.82)		(9.35)
Imports	0.40***	0.48***	0.38***	0.46***
	(3.51)	(4.35)	(3.46)	(4.33)
Exports	-0.20	-0.20	-0.15	-0.16
	(1.30)	(1.34)	(1.01)	(1.09)
N	981	981	977	977
Countries	75	75	73	73
F-Stat	22.3	29.6	23.5	32.0
Overall R2	0.39	0.44	0.36	0.38

Notes: As for Table 2.1.

Table 2.3 Determinants of Tax/GDP Ratios (FE), 1985-2005

Variable	Current Aid		Lagged Aid	
Loans	1.93***	1.92***	1.69***	1.70***
	(5.05)	(5.06)	(3.90)	(3.95)
Loans 2	-0.0044***	-0.0042***	-0.0031	-0.0031
	(2.86)	(2.71)	(1.40)	(1.42)
Grants	0.22	0.30	0.80***	0.88***
	(0.77)	(1.08)	(2.80)	(3.08)
Grants2	0.00042**	0.00038*	0.00013	0.0001
	(2.02)	(1.83)	(0.52)	(0.40)
Agriculture	-0.41*	-0.24	-0.35	-0.19
	(1.79)	(1.03)	(1.52)	(0.83)
Industry	0.54***	0.62***	0.59***	0.70***
	(2.85)	(3.27)	(3.17)	(3.69)
GDP	-0.0022**	0.0041*	-0.0019**	0.0037**
	(2.42)	(1.92)	(2.11)	(2.02)
GDP2		-0.0000006***		-0.0000005***
		(3.24)		(3.46)
Imports	0.53***	0.51***	0.46***	0.42***
	(4.20)	(4.01)	(3.72)	(3.38)
Exports	-0.66***	-0.65***	-0.57***	-0.56***
	(5.35)	(5.28)	(4.60)	(4.54)
N	885	885	898	898
Countries	77	77	78	78
F-Stat	10.2	10.3	11.0	11.2
Overall R2	0.0063	0.042	0.0027	0.034

Notes: As for Table 2.1.

One possibility is that this apparent relationship change is the product of a more general time effect. To investigate this, we performed three tests (results found in Appendix A, Table A 3). First, we estimated the entire sample with the same specification including annual time dummies for each year; this provided no evidence of a general time effect. Second, we instead included a time trend variable, which was insignificant. Third, we included a 1985-2005 (intercept) dummy, but it was insignificant. In each of the tests the other coefficient estimates were largely unchanged. There is no evidence that the apparent shift in relationship between grants and tax revenue is simply a general time effect.

The next robustness check was to construct a panel of sub-period averages (eight four-year periods, to have balanced panels we start in 1974 and go through to 2005) and estimate (2) using this panel. Such sub-period averages offer certain advantages, notably by smoothing

out year-on-year variations in the data, especially for the aid variables that are quite volatile (Bulír and Hamann, 2008). Period averages also capture something of the dynamic relationship between aid inflows and the tax ratio (it is now previous period rather than previous year aid that can impact on tax effort) and reduces possible autocorrelation. The results in Table 2.3 provide further evidence that grants have not had a negative impact on tax revenues, except in the earliest periods. We report only fixed effects, as the (admittedly weak) Hausman test constantly indicates that it is preferred (as previously, RE results are similar). The validity of FE depends on whether differences in tax rates between countries can be explained by the controls and unobserved country-specific factors. The unexplained (log) difference between countries should not vary too much over time; otherwise using fixed effects is problematic. This seems a reasonable assumption, given the persistent regional heterogeneity in tax rates across the world. On the other hand, the use of sub-period averages is likely to decrease the significance of variables as it reduces the number of observations. This may be why the coefficient on *Industry* is insignificant in Table 2.3. All other variables have the expected sign and are mostly significant. Grants are insignificant and while negative in the whole sample, are positive in the last twenty four years (6 time periods). This supports the earlier suggestion that grants effect on tax revenue has changed.

Table 2.4 Determinants of Tax/GDP Ratios, Period-Averaged Panel (FE)

Variable	1974-2005		1982-2005	
	Current Aid	Lagged Aid	Current Aid	Lagged Aid
Loans	1.16	1.27	1.99**	2.18***
	(1.56)	(1.62)	(2.51)	(2.67)
Grants	-0.79**	-0.60	0.79*	0.68
	(1.97)	(1.42)	(1.71)	(1.47)
Agriculture	-0.83***	-0.84***	-0.64**	-0.59**
	(3.35)	(3.32)	(2.13)	(1.99)
Industry	0.52*	0.45	0.34	0.31
	(1.86)	(1.54)	(1.14)	(1.01)
GDP	0.0038**	0.0034*	0.00089	0.00064
	(2.26)	(1.92)	(0.43)	(0.31)
GDP2	-0.0000006***	-0.0000005***	-0.0000003**	-0.00000021*
	(6.36)	(5.88)	(2.05)	(1.79)
Imports	0.42**	0.39**	0.18	0.19
	(2.29)	(2.11)	(0.90)	(0.98)
Exports	-0.56***	-0.50**	-0.29	-0.30
	(2.81)	(2.46)	(1.42)	(1.44)
N	493	488	373	372
Countries	95	95	92	92
F-Stat	12.5	11.6	4.26	4.36
Overall R2	0.43	0.43	0.11	0.094

Notes: As for Table 2.1; panel comprises averaged data for eight four-year periods. Aid squared terms have been omitted (if included they are all insignificant). Lagged Aid here refers to the period before, i.e. the four year period preceding the current four year period.

TESTING FOR THE BREAK

By augmenting the standard regression with composite terms for the standard variables interacted with a 'post 1984' dummy, we can use a Wald statistic to test the significance of the composite variable. A constant effect of a variable over time would result in the new composite variable possessing a coefficient of zero. If this coefficient is not equal to zero, there is evidence that a change in the relationship occurred. It is only when the Wald statistic is calculated on the composite variable that we have a true test of whether the break is statistically significant. The results are shown in Table 2.5.

Table 2.5 Testing for Breaks in 1984

	Coefficient Variable	Composite Variable
Loans (L)	0.035	1.23*
	(0.05)	(1.45)
L^2	0.158*	-0.16**
	(1.71)	(-1.74)
Grants (G)	-1.521***	0.99***
	(-3.9)	(2.26)
G^2	0.035*	-0.035**
	(1.84)	(-1.83)
Agriculture	-1.00***	-0.094
	(-7.13)	(-1.11)
Industry	1.085***	-0.30***
	(7.04)	(-3.3)
Income	-0.001***	0.008***
	(-13.87)	(11.37)
Imports	0.41***	0.023
	(4.18)	(0.24)
Exports	-0.326***	-0.16
	(-2.79)	(-1.49)
Obs (N)	F-Stat	Overall R^2
1554	25.24	0.37

*Notes: As for Table 2.1. Selected Wald statistics are as follows: All Variables 17.09***, Grants 5.12***, G2 5.12***, L and L2 1.61, Industry 10.86***, Income 129.3*** and All Others 1.49.The Wald statistic is a test of added explanatory power with a chi squared distribution. It can be defined as* $W = (\hat{\theta}_1 - \hat{\theta}_2)'(\hat{V}_1 - \hat{V}_2)^{-1}(\hat{\theta}_1 - \hat{\theta}_2)$ *with the subscripts 1 and 2 denoting the set of variables. ***, ** and * denote significance at the 10%, 5% and 1% level respectively.*

There is clear evidence for a break after 1984 for some relationships between dependent and independent variables. Specifically, the relationships of tax/GDP with grants, industry and per capita income change: grants become less negative (remember, in 1985-2005 a variable is subjected to both coefficients), the diminishing/increasing returns disappear, industry becomes less positive and income per capita becomes positive. All other effects are insignificant. Very similar results are found if other years around 1984 are chosen. Two of these results appear particularly relevant: since the mid-80s the coefficient on GDP per capita has been positive (suggesting that tax collection efficiency increases with income)

whereas grants have had less or no adverse effect on tax revenue (the results here do not show a positive effect of grants).

Medium Term Aid Effects

Aid is given to countries specifically because they have a deficiency in some sense (income, welfare). If this contemporaneous negative correlation (in this case, with tax revenue) is not controlled for, the coefficient on aid will be downwardly biased. Lagging aid by one period is unlikely to solve this fully, given the persistence in aid levels and the likelihood that tax/GDP changes very slowly. This is more problematic for grants than loans: current and lagged grants have a correlation of some 90% (for loans it is 72%). Only when the contemporary effect is controlled for will the medium-term effect become apparent.

To try and isolate the medium term effect we augment the standard regression with a constructed 'previous aid' term, by taking an average of aid given 5-10 years previously. We can test if serial correlation is a problem, given the relatively high correlation of grants over time, following the method of Wooldridge (Drukker, 2003a; Wooldridge, 2002, pp. 274-276). In the presence of serial correlation, estimating using differenced data is recommended although it tends to have low explanatory power, as country specific effects are deleted and the cross-country variation of tax revenue in differenced form is small. This means high standard errors are to be expected, and much of the nuance of panel data is lost.

Table 2.6 presents results using fixed effects estimation and differenced data. The Wooldridge statistic repeatedly shows that there is serial correlation, so the differenced results are also reported, both for the whole sample and 1985-2005. The Wooldridge statistic is essentially a test of whether the time varying error term conforms to the properties one would expect if the data were not serially correlated. The results show the expected pattern for coefficients on imports, exports, industry and agriculture. Loans are positive with 'diminishing returns' (the squared term is negative) both contemporaneously and longer term. The longer term effect is both larger and more significant. Contemporaneous grants are negative but insignificant, but over the medium term grants found to be positive and significant. The Wooldridge test statistic shows (highly significantly) that there is a problem of serial correlation, which implies the differenced equation is needed. Of those results, by far the most significant determinant is per capita income, with a negative sign. This contradicts theoretical assumptions that tax revenue (collection

efficiency) increases as income rises, through both demand for government services and the ability of citizens to pay more tax. Most of the other variables are not significant. Industry is an exception, but only when the whole period is used.

Table 2.6 'Medium' and 'Short Term' Estimates

		1985-2005 FE	1970-2005 Differenced	1985-2005 Differenced
Contemporaneous	*Loans*	1.89***	1.21***	1.84***
		(5.02)	(3.44)	(4.85)
	Loans²	-0.004***	-0.003	-0.004**
		(2.87)	(1.61)	(2.42)
	Grants	-0.03	-1.10***	0.05
		(0.11)	(4.91)	(0.17)
	Grants²	0.0003	0.0003	0.0003
		(1.42)	(1.14)	(1.5)
Historical	*Loans*	5.18***	0.35	1.84***
		(4.25)	(0.83)	(3.61)
	Loans²	-0.24***	†	†
		(3.44)		
	Grants	2.15***	-0.12	0.48
		(3.08)	(0.45)	(1.52)
	Grants²	-0.07***	†	†
		(3.08)		
Contemporaneous	*Agriculture*	-0.19	-1.06***	-0.31
		(0.82)	(7.8)	(1.36)
	Industry	0.59***	0.66***	0.69***
		(3.14)	(4.41)	(3.66)
	GDP	-0.0007	-0.44***	-0.15
		(0.79)	(6.55)	(1.64)
	Imports	0.43***	0.49***	0.43***
		(3.38)	(5.38)	(3.30)
	Exports	-0.54***	-0.49***	-0.56***
		(4.33)	(4.92)	(4.46)
	Wooldridge test	16.62		
	N	873	1468	873
	F-stat	93	97.13	92.36
	Overall R^2	0.16	0.85	0.90

Notes: As for Table 2.1; 'diff' refers to estimating with the first difference of variables. The Wooldridge statistic is essentially a Wald test of whether β is equal to -0.5 in the regression $u_t = \beta u_{t-1} + \varepsilon$. The null hypothesis is then that there is no first order autocorrelation. The critical value for the 1% significance is 7.01. Historical here is an average of aid given between 5 and 15 years before the year being estimated. † Indicates that variables have been omitted: they were insignificant and added no explanatory power (a joint test of their significance gave a chi-squared [χ^2] statistic of 0.1).

The other significant factors are grants and loans, over the medium term. When lagged terms are used with FE for the full sample (the second column), coefficients on loans and grants are positive and significant, suggesting that aid does help countries to increase their tax revenue. It may be that the aid is associated with conditions including measures to

increase tax revenue, which could be interpreted as a positive impact of conditionality. The coefficient on loans is quite robust in sign and significance across specifications, but that on grants is not (and is insignificant in most differenced specifications, as these have high standard errors). In general the differenced results are weak, at least for the aid variables. However, the medium-run estimates suggest that aid is positively associated with tax revenue, whether loans or grants.

DIFFERENCES IN COUNTRY GROUPS

A final possibility tested is that the relationship may be different comparing low and middle income countries; Teera and Hudson (2004, p.796) report that low-income countries have a significantly lower tax/GDP ratio and tax effort score than middle income countries, although there are few significant differences between the coefficient estimates for different groups of countries. To examine if there are differences between countries according to their income level we split the sample in low-income (GDP per capita of no more than $450 in 1970) and middle-income countries. The results are reported in Table 2.7.

Table 2.7 Splitting the Sample by Income Level, Lagged Aid (FE)

Variable	Middle Income		Low Income	
	1970-2005	1985-2005	1970-2005	1985-2005
Loans	0.31	-0.25	-0.15	2.69***
	(0.76)	(0.49)	(0.21)	(3.05)
Loans²	-0.0004	0.002	0.007	0.005
	(0.17)	(0.74)	(0.86)	(0.58)
Grants	-0.95***	1.82***	-0.74*	-0.07
	(3.45)	(4.40)	(1.83)	(0.11)
Grants²	-0.0004	-0.001***	0.0005	0.001
	(1.16)	(3.92)	(0.61)	(1.40)
Agriculture	-1.91***	-1.40***	-0.44*	0.92**
	(10.79)	(4.74)	(1.78)	(2.02)
Industry	0.11	0.07	1.08***	0.54
	(0.62)	(0.34)	(3.15)	(1.14)
GDP	-0.63***	-0.23**	0.08	0.71
	(10.12)	(2.54)	(0.07)	(0.42)
Imports	0.34***	0.52***	0. 78***	0.60*
	(3.32)	(3.45)	(4.24)	(1.75)
Exports	-0.34***	-0.77***	-0.51*	0.18
	(3.08)	(5.21)	(1.72)	(0.46)
N	1171	659	387	178
Countries	71	67	22	20
F-Stat	25.81	8.86	10.23	6.20
Overall R^2	0.27	0.03	0.33	0.01

Notes: As for Table 2.1.

There do appear to be differences in coefficient estimates for different income sub-groups, especially for the later (1985-2005) period. However, we should acknowledge that the low-income sample is quite small and the model does not have much explanatory power for the later period (for this reason, we did not follow the more detailed analysis of income sub-samples). The difference for the aid variables is pronounced: loans are insignificant but grants are significant (negative over the whole period but positive in the later period) for middle-income countries; in contrast, grants are negative (weakly significant) over the whole period but insignificant for 1985-2005 while loans are significant (and positive) only in the

later period for low-income countries. The positive effect of grants in the later period seems to be due to middle-income countries whereas the positive impact of loans appears to be due to low-income countries (but note that the coefficient on grants is insignificant).

The negative coefficient on income seems to be due to middle-income countries as the coefficient is insignificant for low-income countries, although clearly there is not much variation from which to identify any effect. For middle-income countries the share of agriculture is consistently negative whereas industry is insignificant; for low-income countries, the signs are as expected over the whole period, but for 1985-2005 agriculture is positive, suggesting improved ability to tax the sector, and industry insignificant.

2.5 DISCUSSION

The poorest countries tend to receive more aid, the aid they receive is more likely to be in the form of grants, and they also tend to have lower tax/GDP ratios. Indeed, it is the poorest countries that face the greatest difficulty in increasing tax revenue (Keen and Simone, 2004; Teera and Hudson, 2004). If, on average over time, donors give more aid in the form of grants to recipients with relatively lower tax ratios (because this is correlated with aid 'needs'), one would observe a negative contemporaneous relationship between grants and the tax ratio. This is what we tend to observe. To the extent that tax ratios change slowly over time relative to aid, this negative relationship would also be observed using aid lagged one year (i.e. a one year lag may not adequately account for endogeneity). However, if the composition of aid has a systematic causal (or behavioural) impact on the tax ratio, period lagged aid variables should have a significant effect on the tax ratio. This does appear to be the case, i.e. there is evidence that over the medium term or since the mid-80s, grants and loans both appear to encourage tax effort (the results differ for income groups).

The tax revenue equation (excluding aid variables) is a revenue performance equation and is essentially a contemporaneous relationship: current tax/GDP ratios are explained by current values of the economic structure (tax base) variables. The annual panels are appropriate to estimate this; as it is estimated across countries over time, it captures the relationship that holds on average. The results for the structural variables are mostly consistent in alternative specifications. Countries with a relatively large agricultural sector tend to have lower tax revenue (as that sector's income is relatively low and difficult to tax). Countries with relatively large manufacturing sectors have higher tax revenue, because this may proxy for

the size of the formal private sector, which has higher incomes and is easier to tax; the negative coefficient found by Teera and Hudson (2004) does suggest that this may be a poor proxy however. Imports are positively associated with revenue, because developing countries (especially the poorest) tend to rely on import taxes. Exports are negatively associated with revenue, perhaps because most countries reduced or eliminated export taxes in the 1980s.[1] The coefficient on per capita income was generally negative (but may have become positive since the mid 80s), suggesting that poorer countries are collecting as much tax as feasible, given the tax base, whereas richer developing countries are not (see also Table 2.7).

Given the values of the economic (tax base) variables, countries with 'good' revenue performance would have positive residuals and countries with 'bad' performance would have negative residuals. When aid variables are introduced and found to be significant, they are explaining or correlated with some of the residual (the variation in performance). The negative coefficient on grants suggests that such aid is associated with weak performance (a negative residual), while the positive coefficient on loans suggests that such aid is associated with strong performance. The specification and estimation does not permit the inference that aid causes such revenue performance. The correlation found is consistent with other interpretations. In particular, is it recipient behaviour (tax effort) or donor behaviour (aid allocation and/or conditionality)?

In the context of recipient behaviour, it may be that the tax/GDP ratio is difficult to alter, as argued above, implying that aid is not actually a determinant. Morrissey *et al.* (2007), in an analysis for Kenya that distinguishes loans and grants, find no evidence for an adverse effect of grants or a positive effect of loans on tax effort. In fact, the overall results suggest that Kenya has limited ability to alter tax revenue (an example of the more general argument in Keen and Simone, 2004) or to affect the level of grants. Loans, because the government can choose whether or not to seek them, appear to be the more discretionary component of aid. However, while grants appear to contribute to growth by financing public spending in a non-distortionary (relative to taxes) manner, loans appear negatively associated with growth. One inference is that this is because loans are sought to finance unanticipated deficits, and it is these deficits rather than adverse effects of aid that reduce growth (income).

[1] *In the 1970s when there were export taxes, higher exports would have been associated with higher revenue. As export taxes were reduced during the 1980s there may have been a reduction in tax revenue that was correlated with exports, and after the early 1990s revenue would be lower than suggested by high exports.*

Donors may be aware that the tax/GDP ratio is difficult to alter. It may be the case that on average, over time, donors give a greater proportion of aid in the form of grants to countries that are fiscally constrained. This will be the case if donors provide aid to finance the 'fiscal gap' (or at least recognize that aid finances the gap) and do not wish to burden low-income countries with unsustainable levels of debt (hence provide grants). This would yield the negative relationship observed: "shortfalls in aid tend to coincide with shortfalls in domestic revenue [and] countries that suffer from revenue volatility also exhibit higher volatility in aid receipts, perhaps because both revenue and aid fluctuations are driven by domestic policy instability" (Bulír and Hamann, 2008 p.83).

It may also be that aid inflows are associated with negative effects on the economy's tax base, and that countries that are more aid dependent receive a greater proportion of aid in the form of grants. Aid can affect tax bases, and indeed it is likely that the policy reforms associated with aid conditions will affect the tax base. For example, trade policy reform is a major component of conditional lending – the conditions attached to aid (but not necessarily the level of aid) could affect both the tax base (the volume of trade) and effective tax rate (policies included reducing tariffs and eliminating export taxes). Aizenman and Jinjarak (2009) show that reforms such as trade liberalization, financial liberalization and macroeconomic stabilization (reducing seignorage) erode the revenue from 'easy to collect' taxes such as tariffs (which tend to be most important for poorer countries). Poor countries have difficulty replacing the lost revenue through 'hard to collect' taxes, such as VAT or income taxes, which need significant investment in tax collection and resources for monitoring and enforcement. Thus, periods of economic policy reform in developing countries tend to be associated with reductions in the tax/GDP ratio, especially for the poorest countries (see also Baunsgaard and Keen, 2009), but they also tend to be associated with aid episodes.

In this way, aid conditionality may actually generate a negative association between aid/GDP and tax/GDP ratios in the short-run. Such complex effects are not captured in the estimation we have considered, and are beyond the scope of this paper (especially as such effects will be heterogeneous across countries). However, if conditions related to 'tax base policies' such as trade liberalization are more pronounced in low-income countries that are more likely to receive grants, there may be some tendency for grants to be provided to compensate for conditions that reduce tax revenue in the short term. On the other hand,

some of the policy conditions will have the aim of increasing incomes (the tax base) and tax collection efficiency, and perhaps even increasing tax rates (such as consumption taxes). These effects may only be observed over the medium term, and our results suggest that such conditions have (on average) been effective.

Our most significant finding is the evidence for a break in the mid-1980s, after which grants appear to have a positive impact on tax revenue (and there is some evidence that income per capita also had a positive impact in the later period). It is unlikely to be coincidental that the mid-1980s is when World Bank adjustment was in 'full swing' – although in many countries the programmes began in the early 1980s, it would take a few years for effects to be observed, and by the late 1980s most developing countries had recently had adjustment programmes. This was a period associated with increasing aid, temporary declines in trade tax revenue, and various policy reforms. Osei *et al.* (2005) demonstrate that this was associated with improved fiscal management and tax revenue in Ghana, and Attila *et al.* (2009) report early evidence that foreign aid was responsible for quickening this tax transition across many countries. Our results concur that this positive effect was widespread.

The policy reforms associated with conditional aid include many that should contribute to increasing tax revenues – specific tax and fiscal reforms, especially regarding administration and collection efficiency, and general reforms contributing to increasing incomes (i.e. expanding the tax base). The policies may also increase the incentives for governments to increase tax/GDP. In an environment of falling trade tax revenue, the marginal cost of increasing tax revenue will be high (in both monetary and political terms). Countries with higher aid receipts may be better able to invest in new systems of collection, or even finance projects that increase popular support enough that they may undertake unpopular tax reforms. Aid, in either form, also brings with it the advice and perspective of donors. Therefore higher aid may mean recipients are more likely to have absorbed repeated donor messages regarding the importance of building a strong tax base.

More generally, higher income increases both the ability and desire to collect taxes (Tanzi, 1992), so one would expect to find a positive coefficient (we find this only after 1985). Possible explanations for the general negative relationship between income per capita and tax revenue is that low taxes are good for growth (the countries with lower tax burdens become those with higher incomes) or that a wealthy elite may suppress taxes in order to

minimise their own taxation burden. This is an issue worthy of further research, but is not a direct concern of our analysis.

2.6 CONCLUSION

This chapter investigates the recipient tax revenue response to the composition of aid, i.e. if loans and grants have a significantly different effect on the tax/GDP ratio. The core of the paper examines the relationship between the composition of aid and tax revenue using two different panel data sets (annual and sub-period averages) for a large sample of developing countries over 1970-2005. Using annual data, even with lagged aid, suggests that loans are positively related and grants negatively (but weakly) related to the tax to GDP ratio. In the sub-period panel the relationship between aid and the tax ratio is not robust (and often insignificant). However, when we focus on the 1985-2005 period the results are very different for grants, which are found to have a positive effect on tax revenue (at least for middle-income countries, it appears insignificant for low-income countries). We excluded the possibility that it is due to a general time effect, using three separate tests. In further support of this we also find that the effect of grants is positive over the medium term (for the differenced data we can conclude that the evidence for a negative effect of grants on tax revenue is not robust). We tested for breaks for all variables and found that grants are not the only variable to have changed relationship with tax revenue over the period. Per capita income (also turning positive) and industry (becoming insignificant) also have a statistically significant break around 1985. There is evidence that the nature of these breaks was quite different for low-income as compared to middle-income countries, although the samples here are smaller. As the mid-80s represents the time when conditional lending was becoming widespread, this may be evidence of successful conditionality, at least by fiscal criteria.

Of the two effects found in the paper (the series break and the different short and medium term effects of grants) the latter seems to dominate the former. However, they hold similar implications for the donor community. The evidence here does not negate other evidence regarding the possible advantages of loans. It does however shed considerable doubt on any negative fiscal impact of grants, and in this sense supports advocates of a shift towards increasing the share of grants in aid. One interpretation of our results, for the period since the mid-80s, is that in middle-income countries with a better fiscal system (higher revenue and better management) grants are positively associated with tax revenue, whereas in low-

income countries with weaker systems and limited ability to increase tax revenue, grants (which are a relatively important source of revenue) have no adverse effect on tax revenue.

If one accepts that the effect of aid composition on tax revenue is, at most, weak, then the effect on debt may be a more important concern (Cohen *et al.*, 2007) and debt concerns have influenced the choice of loans/grants mix since at least 2005 (IDA, 2005). One problem is defensive lending, where new loans are pushed with the aim of paying off the old loan given that development banks face internal pressure to continue lending (Bulow and Rogoff, 2005). There is general agreement that loans should not be given to countries that cannot afford to repay, or where a debt overhang would reach unsustainable levels; unless countries have demonstrated their ability to achieve growth (hence service debt), grants are preferable to loans (Radelet, 2005). In this context, the role of IDA is to provide concessional loans for countries that do not have access to loans at market rates.

The revenue response to aid remains an important issue. Governments in developing countries face a formidable challenge creating an effective and efficient tax system (Tanzi and Zee, 2000). If it can be demonstrated that aid flows systematically reduce government revenue efforts, then this can be viewed as reducing the effectiveness of aid. Gupta *et al.* (2004) argue that this is the case for aid in the form of grants but we question this: the evidence for a negative coefficient on grants in the tax ratio equation is not robust and where it exists is consistent with alternative explanations. When we account for the effect in the medium term or the post-1985 period it transpires to be positive or insignificant.

We noted in the introduction that much of the debate between grants and loans revolves around their effect on tax effort and/or growth. We have concentrated on the effect on tax effort, and argued that the evidence for grants discouraging tax effort is at best weak, and the effect transpires to be positive or absent (insignificant) on closer examination (at least in the recent period). There are many reasons why it is very difficult for poor countries to increase tax/GDP ratios so an observed negative relationship between grants and tax effort is unlikely to be causal. In fact, there is some evidence that grants may be more conducive to growth than loans, because they do not add to indebtedness and therefore support stable fiscal policy. Cordella and Ulku (2007) find that more concessional aid has a greater impact on growth on poorer countries with weak policies and high debt. For the most vulnerable countries, this suggests that at the margin grants are preferable to loans. For the poorest countries most in need of aid the case for giving this in the form of grants remains strong.

3. LITERATURE REVIEW: AID ALLOCATION

The study of aid allocation is almost as old as aid donors themselves. Aid represents a major source of finance for many developing countries, and the most tangible instrument of development policy by developed nations. For these reasons alone, it is a topic worthy of study; however there are also more recent motives for doing so. Within the last decade, cross-country studies have tried and failed to find robust evidence that foreign aid has had a positive effect on its recipients' economic growth (e.g. Rajan and Subramanian, 2008; Roodman, 2007)[1]. This is both potentially affected by, and affects, aid allocation policy. The first relationship is simple. If aid is not given with the intention of increasing economic growth, then perhaps the weakness of aid is not inherent, merely a function of current allocation principles: it may be that aid has been effective in its aim (e.g. increasing the welfare of the donor country) but unsuccessful when measured against a different criterion (e.g. increasing the economic growth of the recipient). In this circumstance it would be difficult to make a judgment upon the potential of aid to increase growth, as only the effects of current allocation practice can be observed. The second relationship is related to the general idea that a donor's allocation behaviour is a function of its beliefs regarding the potential effects of aid. For example, if a donor stops believing aid to be growth promoting, it is likely to pay more attention to non-development aims when allocating its aid. If, on the other hand, aid is believed to be growth promoting in good policy environments, donors may increase the importance of poverty and policy in allocation.

Consequently, understanding the allocation of aid underpins the analysis of aid effectiveness. Many commentators deplore current practice and argue that there is a better way of allocating aid, a more efficient use of scarce resources. There are two parts of any argument to improve aid allocation. The first focuses on defining or describing *better* aid allocation, the second focuses on current practice. The first can be seen as a normative approach, in that it deals with defining desirable principles and allocations. The second can be seen as a positive approach, seeking to understand what currently happens. The literature can be divided along these lines, but within these two streams there are of course

[1] *Some studies do, however, find a positive effect, see Doucouliagos and Paldam (2009) for a comprehensive review.*

further distinctions to be made, most often by the technique employed. Sections 2 and 3 both deal with research on how aid *should* be allocated, but they employ distinct approaches. The former starts with a desirable principle in aid allocation (e.g. poverty focus) in order to comment on actual allocations. The latter uses practical tools used by donors to give rule-of-thumb allocation volumes. We then move to the question of how aid *is* allocated: the positive literature. Section 4 outlines papers that employ regression techniques to examine which factors determine the volume of aid allocated among different recipients. Section 5 stands apart by focusing on the wider critique of aid allocation practices afforded by the descriptive literature. Section 6 concludes.

3.2 NORMATIVE LITERATURE: WHO SHOULD GET AID?

The normative approach typically begins by proposing an optimal allocation (maximising an objective function), which is then translated into a 'real world' allocation according to certain constraints and assumptions. This ideal allocation is very sensitive to the assumptions made. Here, the brief discussion is restricted to the ideas behind the allocations (see McGillivray, 2004a, for a more technical review). In a recent prominent example, Collier and Dollar (2002) derive a 'poverty-efficient' allocation of aid, as stated below.

Maximise Poverty Reduction $\sum_{j=1}^{n} G_j \alpha_j h_j N_j$

Subject to $\bar{A} = \sum_{j=1}^{n} A_j y_j N_j \ , \quad A_j \geq 0$

Where G is income growth, α the elasticity of poverty reduction (with respect to income), h is a poverty measure, N is population, A is aid, y per capita income and the subscript *j* indexes (potential) recipient countries. From this, the first-order condition is:

$$G_{a,j} = \frac{\lambda y_j}{\alpha_j h_j}$$

Where $G_{a,j}$ is the marginal impact of aid on growth, and λ is the shadow value of aid (the marginal poverty reduction of an additional dollar of aid). A cross-country growth regression in the style of Burnside and Dollar (2000) is then used to estimate some of the parameters.

The postulated aid volume (given by the estimated aid effectiveness) increases with policy, poverty and poverty elasticity, and decreases with per capita income. Lensink and White (2000) criticise the work on three main grounds. First is the assumption that aid can only

affect poverty through its effect on economic growth. Most obviously, a poverty headcount is related to the level of income in a country, and its distribution. Thus if aid changes this distribution of income the assumption does not hold. They also explicitly exclude the possibility that aid has an effect on poverty through changes in policy (Collier and Dollar, 2002, p1476), which is a contentious assumption. For example, Mosley, Hudson and Verschoor (2004) report evidence that aid does exhibit poverty leverage, by increasing pro-poor expenditure – influencing both policy and distribution. Second is the assumption that the effect of aid on economic growth is conditional on policy. It has been well documented that this assumption comes from the regressions in Burnside and Dollar (1998, 2000), whose results have been almost impossible to replicate (Easterly *et al.*, 2004; Roodman, 2007). McGillivray (2004a, p.282) argues these econometric estimates are used to justify a relatively low weight for poverty relative to policy. Third is the assumption that there are diminishing returns to aid. Lensink and White (2000, pp. 404-405) catalogue the problems with this assumption, which must hold otherwise the model cannot be calculated. Most problematic in this instance are the use of an estimated coefficient from a regression based upon a different sample to the other coefficients, and the very low implied turning point for diminishing returns. Anderson and Waddington (2007) used various different estimates of the diminishing returns, and their work inadvertently illustrates the degree of disparity in implied allocations when using different estimates of the strength of diminishing returns.

Llavador and Roemer (2001) approach the question from a very different perspective by seeking to equalise *opportunities* of recipient countries for growth, in contrast to Collier and Dollar (2002) who equalise actual aid-funded economic growth. Their model divides the effects on the growth rate into three: conditions, effort and aid. They argue that aid should be an increasing function of effort and decreasing function of conditions. This is motivated by the equal opportunity rationale – that aid should equalise the *possibility* of growth by compensating for adverse conditions but not low effort. The model is a formalisation of an idea within aid allocation discussion for many years, for example Maizels and Nissanke (1984, p879) argued "aid is given to compensate for shortfalls in domestic resources". In order to operationalise the model, and provide an example of optimal allocation behaviour, variables are assigned to country conditions and country effort in a cross-country growth regression. This decision is crucial, as it means deciding the sign with which a variable should influence aid allocation. The variables found in Burnside and Dollar (1998) are chosen to represent conditions, except for the policy index (budget surplus, inflation and openness)

which is chosen to represent effort. The demarcation between effort and conditions is guided by that which can be controlled by a country, and that which cannot. Using this test, the variables of initial income, ethnolinguistic fractionalisation, assassinations and regional dummies are justifiable as pre-determined or out of the direct control of the government. However, the variables measuring institutional quality[1] and the money supply are directly influenced by the government. In fact, it is not clear that these variables are less controlled by the government than inflation and the budget surplus, which are included as effort variables. For example, Llavador and Roemer (2001) deem the money supply as effort and inflation as a condition, however as the former affects the latter the distinction seems illogical. Yet, it is argued that allocation should punish low levels of effort, and compensate for low levels of conditions (unfavourable circumstances).

Cogneau and Naudet (2007) argue that Llavador and Roemer (2001) does not accurately represent the theory of equality of opportunity, as the model favours better past macro-economic performance, growth and conditions. This resulted in a recommendation for the richest countries to get 72% of all aid, and poorest only 4%. They also criticise the view of Collier and Dollar (2002) that aid effectiveness is solely a function of policy, and thus should drive aid allocation, without allowing for aid effectiveness to also be influenced by structural conditions. Cogneau and Naudet (2007) set out their own application of the equality of opportunity theory, using CPIA as the effort variable and allowing conditions to affect effort. When growth prospects are included, proposed allocation resemble actual allocation more closely. This result is similar to Wood (2008), who extends the Collier and Dollar (2002) approach to include a time horizon. Donors, under this assumption, care about poverty reduction without aid, and then seek to maximise poverty reduction by augmenting that with aid induced poverty reduction. This innovation makes proposed aid allocation resemble actual allocations more closely. The formal presentation of an equal opportunity approach is elegant and reasonable. However, the operationalisation is hampered by the lack of clear distinction between which variables should represent conditions and which should represent effort, a distinction made even more troublesome by the connections between variables.

[1] *ICRG- the International Country Risk Guide, presented in Knack and Keefer (1995).*

3.3 Financial Programming Rules And Performance Based Allocation (PBA) Formulas

Similar to normative studies in method, if not aim, are the financial programming rules that were popular in the 1960s. The most cited example is the two-gap model of Chenery and Strout (1966). The two-gap model builds upon the Harrold-Domar framework where growth is a function of investment, which is in turn a function of the savings rate. This is the 'first gap': the difference between actual savings and the savings required to support investment for a specified economic growth rate. The 'second gap' refers to a shortfall in export earnings required to finance needed capital good imports. Chenery and Strout (1966 pp.728-729) use this to suggest certain performance criteria to employ when allocating aid, including the investment rate, marginal savings rate, marginal tax rate, balance of payments and the incremental capital-output ratio. Easterly (1999) argues that these ideas still influence discussion of aid allocation policy, using examples from the policy documents of multilateral agencies. Cline and Sargen (1975) built upon the work of Adelman and Morris (1968) and suggested a simple allocation rule where aid is allocated on the basis of income per capita and population, and then adjusted to take into account a country's 'performance'. These rules were generally designed to be illustrative in nature, and give some indication regarding the absorptive capacity of a country. PBAs (Performance Based Allocation formulas) now perform a similar task, by producing an indicative figure of the amount of aid that is proportional to the need and population of a country, adjusted for performance. They are also highly informative regarding the donor's relative weights for different factors, even if they are only guidelines.

The IDA (International Development Association, the World Bank arm that focuses on poorest countries) has been using PBAs since 1977, and other donors are increasingly following suit. Their formulation has been heavily influenced not only by financial programming rules, but also by positive and normative approaches discussed elsewhere in this chapter. Other agencies known to use PBA formulas are mainly multilateral (including the African, Asian and Inter-American Development Banks) but do include bilateral agencies (e.g. DFID, UK and the Millennium Challenge Corporation, USA). The associated policy papers and discussions reveal how policymakers have decided to reconcile competing points of view, arriving at models that guide and shape their actual behaviour. Take for example

DFID's Dyer formula (Dyer *et al.*, 2003), which forms the basis of allocations to low income countries:

$$Allocation\ Score = \left(\frac{CPIA}{GNI\ pc}\right) \cdot Population^{0.6}$$

A country's allocation score as a percent of the sum of all countries allocation scores then forms the percentage of aid budget the country could receive. No econometric methodology previously used in the explanatory stream could have revealed this as the true formula, given its multiplicative nature. To compound the difficulties, only around 30% of DFID aid was allocated this way, given its previous commitments. This means there have been multiple data generating processes inherent in any allocation. It is interesting to note just how simple the above formula is; indeed this was one of its aims.

The Development Banks of Africa, Asia and the Caribbean use formulas that are multiplicative in nature, and combine needs and performance factors. Need in each case is a product of population and per capita income, but each formula weights these differently by using different exponents. The SDF (Caribbean Development Bank) also includes here a vulnerability index. The performance factors are then generally a variation on three factors: CPIA, a Governance index and portfolio performance where the performance index is created using various subjective indices of government performance. The Inter-American Development Bank differs by using an additive formula, which is 40% based upon Needs, and 60% upon Performance. It is allocated on the basis of the amount of population, inverse per capita income, debt, and policy when measured as a percentage of the sum of all eligible countries.

The Millennium Challenge Corporation (USA) uses a very different process. Countries are first split into two income groups, and then data collected on each country within three different categories (ruling justly, investing in people and encouraging economic freedom) from many different independent sources. If a country performs above the average for their income group in each of the three spheres it is eligible for funds. This splitting of countries into groups is common in PBAs, as it was in financial programming rules. For the latter, the justification was related to ideas regarding the stages of growth, whereas the current reasoning is that performance should be understood relatively.

CRITIQUE OF PBAS

It is perhaps too far to argue that PBAs are the inevitable result of the positive and normative literatures. However, it is without doubt that PBAs have been heavily influenced by the literature both on aid effectiveness and aid allocation. Indeed, DFID (2005, p.4) states that PBAs are based on three pieces of evidence:

- Aid positively affects poverty through growth
- The size of the impact depends upon the policy environment
- Aid exhibits diminishing returns

The source of these three points is of course the much maligned work of Burnside and Dollar (1998, 2000). Merely extending the original dataset by one time period (and including data that was previously missing) results in ambiguity regarding the impact of aid (Easterly *et al.*, 2004). Many recent papers demonstrate the fragility of the above conclusions (Rajan and Subramanian, 2008; Roodman, 2007). Moreover, Roodman (2008) concludes that the average effect of aid is likely to be so small as to be statistically undetectable. This new found humility regarding cross-country regressions is matched by broader studies of policy and growth. The error of an increased reliance upon 'policy' in aid allocation is then the underlying assumption that we already understand which factors cause growth and/or poverty reduction, when many economists are pessimistic regarding this (Rodrik, 2006). PBAs solidify and formalise the contemporary understanding of growth factors, internalising this weakness. A further problem of using PBAs is that they make use of subjective indices of policy such as the CPIA, which is argued to be Granger-caused by growth (Dalgaard *et al.*, 2004).

Another weakness in PBAs is not related to their existence, but rather their implementation. They have inherited the weakness of explanatory research and focus upon how much money to give, ignoring how it should be given. Thus PBAs do not allow for different policy environments to be met with different types of aid, merely different amounts. Furthermore PBAs, like programming rules before them, are intended to guide the amount of aid given, and be adjusted pragmatically. Many factors couldn't be included easily in a PBA, such as a donor's relative comparative advantage in a sector, recent changes in conditions or new leadership.

3.4 POSITIVE LITERATURE: WHO *DOES* GET AID?

This section provides an overview of the positive literature. McGillivray and White (1993) comprehensively survey the early literature, Neumayer (2003b) surveys the newer research and the next chapter offers a more focused and technical discussion of key papers. This section is designed to give an overview so it can be understood within the context of the various approaches used. The common approach in explanatory work is to model various factors which are thought to influence allocation decisions and then test the model using regression analysis. As the techniques have developed, this approach has gained in popularity, sophistication and use. McKinley and Little (1979) posited a dichotomy between donor interest and recipient need, and Dudley and Montmarquette (1976) argued that a hybrid model was the most accurate reflection of reality. Now, it is generally accepted that a trichotomy is more appropriate: need, policy and proximity. *Need* refers to any factor that motivates the donor's altruistic nature. In estimation, monetary measures are prevalent but infant mortality data have also been used. *Policy* refers to any factor that influences the ability to turn aid into growth or poverty reduction (similar to Collier and Dollar, 2002). *Proximity* reflects 'donor interest', and typically includes measures relating to strategic, cultural, historical or commercial ties between donor and recipient.

Having decided on which variables to represent these three factors there remain three main complications, each with a number of possible solutions. First, the data is left hand censored, as donors first decide who to give aid to, and then how much (i.e. some potential recipients receive no aid, meaning a cluster of aid commitment data at 0). The possible econometric techniques include Tobit, Heckman, and two-step (i.e. the Cragg model). However, Alesina and Weder (2002) argue that the number of zeros is relatively small, thus despite the truncated nature of the data OLS will be considerably biased. This is likely to vary considerably by donor, and the common resolution is to choose one method, and use at least one other as a robustness check. For a more technical discussion of the various estimators, see chapter four.

The second complication is due to the well known population bias in aid allocations. That is to say that if, ceteris paribus, a country doubled in population its aid receipts would increase but not double. Allowing for this in estimation has tended to include population, logged and/or its square, on the right hand side. Furthermore, some researchers choose to omit or truncate large populations like China and India so as to circumvent the problem. Berthelemy

(2006, p.182) argues that, if per capita is used, the population bias is adequately dealt with by simply including population as a regressor. However, this debate has been taking place for some time (McGillivray and Oczkowski, 1992; Neumayer, 2003b) and the most suitable approach is still under discussion. The third complication is due to the fact that different recipients are in essence competitors for the fixed budget of a given donor. This has often been allowed for by using the recipients' percentage of total donor budget as the dependent variable, as opposed to nominal or per capita commitments.

Table 3.1 summarises some of the key papers in the recent literature. The focus is on relatively recent research that looks at more than one bilateral donor, with a selection that represents the major authors and approaches. The papers chosen show the variety in deciding how to solve the three complications aforementioned. Within these six studies there are five different dependent variables, five different estimators and a plethora of different independent variables employed. Given the disparate approaches taken, there is a surprising degree of agreement over donor behaviour. Most donors are found sensitive to recipient need and their own interest. Japan, the United States and France are often found to be more selfish in terms of the importance of proximity and less selfless in terms of the importance of need. Scandinavian donors and the Netherlands are sometimes joined by the United Kingdom when identifying selfless donors. There does not seem to be the same consensus over policy variables, perhaps due to the lack of agreement over which policy variables to use, and issues of multicollinearity. Papers that allow for a quadratic relationship for aid with population and/or need generally find the decision justified.

Table 3.1 Selection of Existing Literature

Authors	Estimation and *Dependent Variable*	Need and *Population*	Policy	Proximity	Conclusions
Berthélemy and Tichit (2004)	Tobit, Random Effects, 1980-1999, *Aid Commitments per Capita*	GDP pc (GDP pc)^2 *Pop* *(Pop)^2*	Growth[t-1] FDI/GDP Primary Enrolment Infant Mortality 'Glob Free'[1] Others Aid Commitments	Trade/GDP Colony Dummy USA-Egypt	Increasing policy sensitivity and importance of trade. Aid related to lower per capita income, higher FDI flows, better school enrolment, higher infant mortality, higher trade with donors and former colonies, with diminishing returns to population
Berthélemy (2006)	Two-part or Heckman model, 1980-1999, *Ln (Aid pc)*	Ln(GDP pc) [-1] *Ln(Pop)*	Debt Burden Others Aid Commitments 'Glob free' External conflict Internal conflict	Colony Dummy USA-Egypt USA-Latin America Japan-Asia Exports	Countries deemed Altruistic, Average or Egoistic, on the basis of the size of the coefficient for exports.
Alesina and Weder (2002)	Tobit Averaged, 1970-1995, *Ln(Aid pc)*	Ln(GDP pc) *Ln(Pop)*	Openness Political Rights Democracy Corruption	Colony (Years) UN Friend – Japan or USA Israel Dummy	Most donors do not have any significant relationship with corruption. Scandinavian donors are an exception, giving less aid to corrupt countries.
Alesina and Dollar (2000)	OLS or Tobit, 1970-1994, 5 year averages, *Logged Aid disbursement*	GDP pc GDP pc^2 *Pop* *(Pop)^2*	Openness Democracy Civil Liberties FDI Rule of Law	Colony (years) UN friend – USA/Japan Egypt / Israel Muslim /Roman Catholic	Donor behaviour is biased for the USA, France and Japan by Israel/Egypt, Colonies and UN Voting respectively. Japan and France are more selfish than other donors, and the USA are especially concerned with democracy.
Dollar and Levin (2006)	Pooled Tobit, 1984-2003, *Ln(Aid)*	Ln(GDP pc) *Ln(pop)*	Democracy Rule of Law	Colony Exports Ln(Distance)	Multilaterals becoming more policy sensitive. Colonial history is important for many, as is trade for France, Japan and Portugal.
Neumayer (2003a)	Two Part Model (Probit and OLS), 1985-1997, *Aid as share of Aid budget*	GDP pc *Pop*	Political/Civil Rights Personal Integrity Rights	Colony Exports US military grants Distance	Poverty and Proximity important for most donors. Rights important for most donors at first stage, and for a few in the second.

[1] 'Glob Free' is a function of political freedom and civil liberty data, both from Freedom House.

Alongside the approach of papers in Table 3.1 are papers that seek to answer specific questions, often without a full econometric specification of the different factors involved. Easterly (2007) looks for changes over time and finds the increased poverty sensitivity to have happened after 'the McNamara revolution' of the 1970s, with little change since then. He also 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) Canavire et al. (2006) report correlations and basic regressions from a similar exercise in support of the conclusion that aid is poverty but not policy-sensitive. Specifically poor are Japanese and French aid, with the US not faring particularly well. The poverty focus is found particularly strong for Scandinavian countries, Germany, Holland and the UK. These donor differences are often found, but one group seems to have particularly distinct behaviour. Gates and Hoeffler (2004) explicitly test and confirm the popular notion that Nordic donors (Norway, Denmark, Sweden and Finland) are more influenced by democracy and less influenced by economic ties, compared with other donors.

A common preoccupation is to question whether a specific variable has any influence in allocation decisions. The end of the cold war has been thought to have changed the allocation of aid in a large way, with economic partners becoming more important than geopolitical concerns (Berthélemy and Tichit, 2004). However, Easterly (2007) finds that the cold war changed little, in terms of sensitivity to democracy. Boschini and Olofsgård (2007) concur that the cold war changed relatively little, but find it may explain the decline in aid volumes. Other factors found to be to some extent irrelevant are debt and human rights. Despite the importance placed upon debt in the literature Birdsall et al. (2003) find that the debt burden has no bearing on aid allocation decisions. Neumayer (2003a) tests the significance of human rights, donor by donor. He finds only limited evidence for its significance, and no systematic response to poor human rights.

The geopolitical significance of a given country for another country is difficult to measure accurately. However, one source of quantifiable data on bilateral cooperation are the UN voting records of different countries. It is understandable that bilateral aid may be correlated with UN voting records, indeed Dreher et al. (2008) find evidence that US aid 'buys' UN general assembly votes. They find no evidence of similar relationships with other donors. However, remarkable results are found in the relationship between similar UN voting records and multilateral donors: Andersen et al. (2006) study IDA allocations (the

World Bank arm focused on the poorest countries) and find that countries that vote in similar ways to the US receive more aid from the IDA. They also find aid allocation is affected positively by human rights and population (the latter with diminishing returns), negatively with political freedom and no significant income effects. Interestingly, Neumayer (2005) finds evidence that food aid is free from these types of geopolitical concerns.

3.5 Descriptive Literature: A Wider Critique

Given the disparate nature of the descriptive literature, a full review is provided in Appendix B, which covers in detail both several recent indices and the donor errors which they identify. The reason for the diversity of research within this field stems from a simple fact: the research reviewed in the previous sections all took the volume of aid as the unit of analysis and the descriptive literature diverges from this. By focusing on donor performance as measured by narrow allocation concerns, the literature has perhaps been culpable of allowing donors to continue in poor practices of a different kind. Many voices have criticised the allocation of aid more broadly since its inception, a recent eloquent example is from the Commission for Africa (2005):

> *"The system for allocating aid to African countries remains haphazard, uncoordinated and unfocused. Some donors continue to commit errors that, at best, reduce the effectiveness of aid. At worst, they undermine the long-term development prospects of those that they are supposed to be helping."*

The purpose of this section is to highlight different ways of measuring the performance of aid donors, which necessitates an understanding of what is good and bad practice among donors. I catalogue some of the commonly cited errors in Appendix B, and the most popular measures of them here. The positive and normative literature focuses attention upon how much aid is allocated, and the forces that decide that amount. However, this approach argues a set amount of money can be given well or badly: *how* aid is given matters, not just how much. Studies that focus more on this kind of question have been called *descriptive*, in that they do not seek to explain the totality of donor behaviour; rather they describe one aspect of their behaviour.

Earliest Descriptive works

Descriptive works can at times resemble explanatory ones in methodology: both could employ a regression of aid allocated on poverty for example. It is in their aims that they

differ, as explanatory work models the entire behaviour of a donor, descriptive studies can be more focused on only one aspect. As such they are more pragmatic, and do not claim to know the data generating process. Mosley (1985) attempted to shift towards a more rounded judgement of aid, including how aid was given rather than solely to whom. The resulting aid quality index measured aid donors on four criteria: poor-country focus, proportion to 'low income friendly' sectors, extent of tying, and the grant element. The index was then an average of those four measures as percentages. More widespread was the focus on equity as a guiding principle: aid should be directed by need, with income per capita being the most widely accepted measure. McGillivray's index is the most prominent example of this, which rates donors on their response to per capita income (McGillivray, 1989). Clark (1992) uses the more statistically informed Suit index to measure the global distribution of aid (it is similar to the Gini index).

A good survey of this period is provided by McGillivray and White (1993). It places allocation within the context of four readily quantifiable aspects (similar to Mosley): volume, concessionality, tying and allocation. They propose separating the allocation dimension from the other aspects in order to avoid introducing arbitrariness. Rao (1994) amends the work by McGillivray and White (1993) by correcting population-related problems with the index, and allowing recipient per capita income to be affected by the aid given to that country. This second point is effectively acknowledging that if the poorest country receives all aid, it may no longer be the poorest country. It works on the basis that per capita income is increased on a one to one basis by aid per capita, rather than more optimistic readings of aid effectiveness. Rao (1997) operationalises the index that was previously proposed.

In a similar vein, White and Woestman (1994) use four criteria to assess aid, but widen this by trying to include policy ties as well as procurement ties. Eschewing the more standard aggregate index, they promote the graphical alternative used by Åshuvud (1986). This avoids the obvious difficulties of deciding which weights to use, and is readily interpreted. This may not be the case if there are more than the four axes used in this example. It is interesting to note that some aspects are aggregates in themselves though. Tying includes policy, purpose and procurement conditions. The problem with this is that even if research agrees that the individual points can be aggregated, the evidence base may change. Thus it may be thought one year that it is right to punish donors for attaching purpose conditions, whereas the following year it may be shown beneficial. Therefore it becomes important that an index

reflects the consensus regarding good donor behaviour. As such, errors must be robustly established before they can be included in a rating of donors.

LATEST DESCRIPTIVE WORKS

The CDI (Commitment to Development Index) is noteworthy because of its large impact on policy: it is used explicitly by Dutch and Finnish governments, has influenced Australian, Canadian and Norwegian policy, has angered Japanese officials, and is supported financially by ten bilateral donors (Roodman, 2006). It is a broad index that seeks to rate the effect of donor countries on developing countries in many different areas, and the Index of Donor Performance (IDP) is the most relevant part here. It is essentially a measure of aid/GDP from donor but aid is discounted for poor practices, for example tied aid is discounted by 20%. The driving force behind the IDP is the amount of aid given, as quality does not vary as much as quantity. The biggest changes are that Japan is penalised for its high inflows from debt interest, and the proliferation of Australia and Italy are penalised. It is interesting to note that each multilateral institution is given a ranking, and scores for donor-countries are adjusted to take account of their contributions to these agencies. The index incorporates a high degree of whimsy and decisions are often taken without a theoretical justification. For example, there is no real conceptual framework to explain why all administration costs are discounted, as it could be argued higher administration costs signify a greater level of efficiency and efficiency in disbursal (McGillivray, 2003b). It is certainly clear that aid completely without administration costs seems unrealistic and undesirable.

A number of other indices exist that either create or collate data with a view to making judgements on donor practices, these are discussed in more detail in Appendix B. Easterly and Pfutze (2008) rank donors on a number of different criteria, giving a composite rank that is an average of the disaggregated rank. They penalise donors for the use of 'outdated' modalities as well as fragmentation, low transparency and so forth. The OECD in its Development Co-Operation Report proposed "a dozen measures of a common-sense kind for measuring the contribution that development co-operation is making to development results" (OECD, 2007, p.15) The OECD also maintain a database of 'Paris Declaration Indicators', a number of different variables that seek to measure progress on the harmonisation agenda. The data generated by the Strategic Partnership with Africa is novel in that its ratings are decided by bilateral aid recipients themselves. They include ratings on issues such as timely disbursement and the number of conditions. These insights are

undervalued and not well publicised, but give great insight into the important technical details that help decide the effectiveness of aid.

CRITIQUE OF INDICES

Aggregated indices enjoy one of the benefits of PBAs: they are easy to understand and interpret, and seem to be based upon sound research principles. Their attractive simplicity means that they are more transparent for both recipients and the wider community. However, they are only valid insofar as they are believed. Indices must choose what to incorporate, and making use of one questionable element may invalidate the entire index in the eyes of a donor. Thus there is a tension between accessibility and credibility. Any attempt to measure what is in essence a subjective or abstract concept is of course innately flawed. One major problem is how to code the index, given the ordinal nature of many of the concepts being measured. While one may be able to rate policy as better or worse than another, the relative level of policy is a subjective decision. When a number of different variables are then involved, this problem is only magnified. Furthermore, the relative importance of the different dimensions involved is not clear. Weighting is often left at 1:1 but this is due to insufficient theoretical and empirical research. Given that the objective is to rate donor behaviour, this inaccuracy can undermine the entire exercise.

Indices by nature have to make simple judgments about what to punish and reward in their statistics, a problem compounded by aggregation. However, many donor actions cannot simply be defined as 'good' or 'bad'. For example, Easterly and Pfutze (2008) include food aid and technical assistance as indicators of the use of inefficient aid channels (along with tied aid). This concludes all food aid and technical assistance is 'bad', without sufficient evidence to support this assumption. Administrative costs are also assumed to be 'bad' but non-existent administrative costs are virtually impossible and are likely to be undesirable. The data cannot discriminate between 'good' and 'bad' food aid, and including a simple measure of the amount of food aid provided makes the final index more difficult to defend.

3.6 CONCLUSION

The discussion has included four approaches to aid allocation research. It is important to realise that these approaches are not inevitably rivals or substitutes, but instead ask complementary questions. This relationship is given by their comparative advantages. The normative literature proposes an ideal allocation principle which can be used to challenge

current allocation practice. It can show the distance of current allocation from some idealised principle. The corollary of a simple principle can be especially useful when identifying which recipients gain or lose heavily in the malaise of fragmented donors. These countries are popularly referred to as aid orphans and aid darlings, and the normative approach can be used to identify these countries (see, for example, McGillivray, 2006). The literature on PBAs is more pragmatic and policy-focused. Donors use them to guide allocations, and aim to base these suggestions on recent research. A major advantage is that recipients can predict more accurately which factors are important to a donor. This transparency can help incentivise the variables most important to a donor, as well as increasing the predictability of aid flows. Both of these strands focus on future aid allocation, projecting and suggesting principles to guide future allocations.

The positive and descriptive literatures are both concerned with describing past allocations. They both show that actual allocations are more complicated than pure prescriptions based on a handful of guiding principles. The positive literature has the advantage of modelling these different factors concurrently, enabling different factors to be controlled for. This means different effects that could not be discovered using the descriptive approach can be disentangled and measured. The descriptive literature's advantage is its flexibility. While constrained by data availability, it does not need to posit an entire model to obtain an indication on a given topic. In the aid allocation context, it is unique in that it can broaden its unit of analysis to include more than just the amount of aid given.

4. 25 YEARS OF AID ALLOCATION PRACTICE: COMPARING DONORS AND ERAS

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 generally 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 *et al.*, 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 *et al.*, 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 increased with respect to poverty and policy respectively. We employ different specifications in order to understand if the expected change has indeed occurred.

This chapter 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 is surprisingly atypical. The chapter 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.

The chapter continues in section 2 with a review of the literature. The econometric approach, including the extensive discussion of the various estimators, is in section 3. Results, discussion and conclusion are in sections 4,5 and 6 respectively.

4.2 Literature Review

The preceding chapter included a review of aid allocation literature, encompassing various approaches and aims. This chapter employs the positive approach, and a small review of literature 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 estimator 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 (2010) 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 faring 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 (as FDI and growth are correlated). 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: Hout (2007a, b) 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.

4.3 Econometric Approach

The early dichotomy of recipient need and donor interest gave way to the hybrid model, where both factors (Recipient Need and Donor Interest) had some influence over aid allocation, given by $A_i = F[RN_i, DI_i]$. I estimate the modern counterpart of that by donor, which is the following:

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

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 that is expected to be positive ($\beta_2 > 0$) and discussed in more detail later. 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 recipient that are not need-related, in the mind of a donor. For example, it may be argued that the USA values 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 ($\beta_3 > 0$).

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,

[1] *Author's calculations, based on OECD data, where all commitments are included. The more common statistic that puts multilateral aid at around 30% is only arrived at only when excluding organisations such as the Bill and Melinda Gates Foundation.*

Recipient Need and Donor Interest are mutually exclusive and a donor must choose between increasing their own welfare and that of a recipient. *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 importance 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 (2003c, 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):

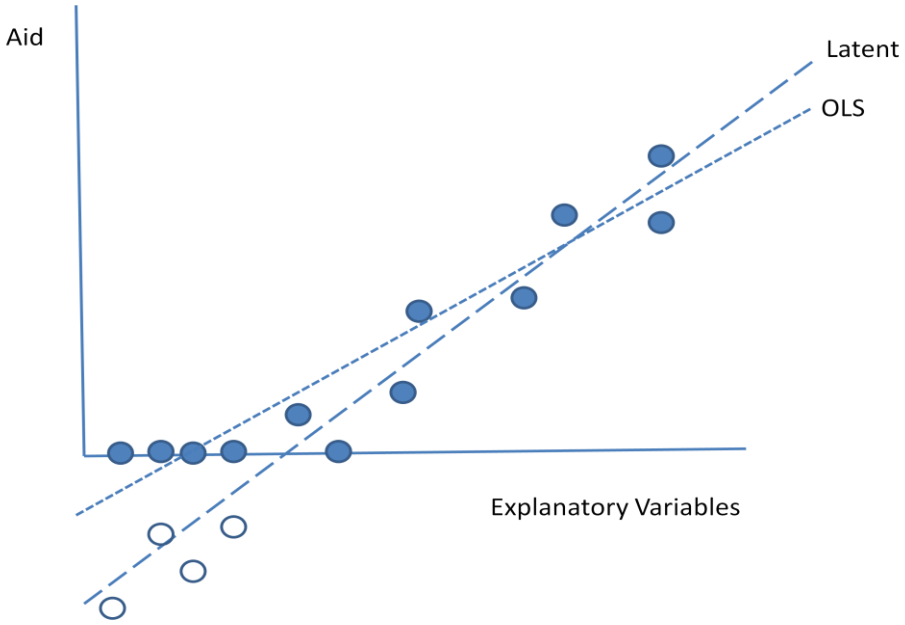
$$H_0: \gamma_1 = \gamma_2 = \cdots = \gamma_x = 0$$

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 4.1). There are various estimators that can be used; 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 (2003c, with application to aid allocation).

Figure 4.1 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 zeros 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 4.1 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 three 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.

$$y^* = \alpha + x'\beta + \varepsilon \tag{1}$$

Where

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

And

$$\varepsilon \sim N[0, \sigma^2] \tag{3}$$

Where y^* is the 'true' or latent variable (as in Figure 4.1). 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 (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 relative size of their effect is the same. A lesser concern in the aid allocation context is the assumption of normality, as the model can easily be extended to include a more appropriate lognormal formulation (Cameron and Trivedi, 2009, p.531).

THE TWO-PART MODEL

This presentation of the Two-Part 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

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

Where

$$\begin{aligned} y_1 &= \begin{cases} 1 & if\ y^* > 0 \\ 0 & if\ y^* \leq 0 \end{cases} \\ y_2 &= \begin{cases} y_2^* & if\ y^* > 0 \\ - & if\ y^* \leq 0 \end{cases} \end{aligned} \tag{5}$$

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 (2003a) 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 (2003a, p.38) cites evidence that the bias leading from breaking this assumption is small (Manning *et al.*, 1987).

HECKMAN

Berthélemy (2006) uses the Heckman estimator (or sample selection model) in the context of aid allocation[1]. 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 does not affect the outcome variable directly" (Cameron 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

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

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, 2003a). I have chosen to use this data for testing as opposed to my own, for one major reason: calculating the Rho statistic was not computationally possible with my data. This is most likely because of the problems with the Heckman estimator discussed elsewhere. In order to maintain consistency, the same data was then used for each test, informing us as to the best estimator for this situation. Qualitatively, the results are similar with my data for those tests which I could perform (i.e. normality and homoskedasticity tests for both Tobit and Two-Part models).

Table 4.1 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

Note: The first test for normality has a chi-square distribution, with two degrees of freedom. The second test is the Breusch-Pagan/Cook-Weisberg test, with the null hypothesis is that error variances are equal. It has a chi-square distribution, with one degree of freedom. The third test is the conditional moment test from Drukker (2002) and the fourth test is from Cameron and Trivedi (2009, p.534-538), and both have a large sample chi squared distribution. The fifth test is a likelihood ratio test of the independence of the two stages, and has a chi-square distribution, with one degree of freedom.

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 and Trivedi, 2009, p.541). The last column reports a test of the independence of the two stages, which involves testing the covariance between errors in two stages. It was calculated by running a Heckman model without an exclusion restriction and testing whether rho is equal to zero (which would indicate independence), 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 annual or period averaged data. Neumayer uses annual 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 towards arbitrary heteroskedasticity and serial correlation" (Neumayer, 2003a, p.50; 2003b) 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[1] (see Drukker, 2003b 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 are not available 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. The panel covers 1982-2006 in five 5-year time periods. A full description of the data can be found in Appendix C.

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

[1] *Tests performed on the level specifications found in Tables 5.4 and 5.5 (Neumayer, 2003b pp.63-64).*

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 $\ln\left(\frac{100*aid_{ij}}{aid_i}\right)$ 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 value of 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* 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

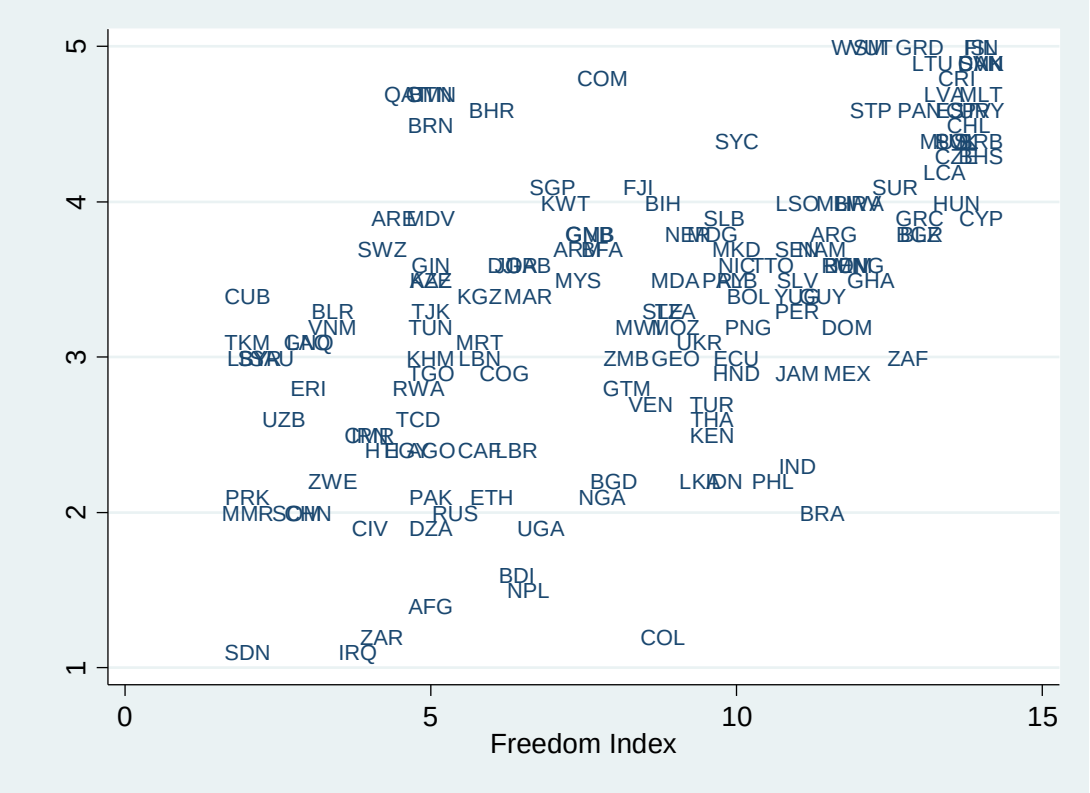
this approach is similar to the approach found in donors' performance based allocation formulas that are available to the public (where income and population are typically included separately, the latter discounted at the rate $x^{0.6}$, see previous chapter for discussion).

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* Political Terror Scale (PTS). The *Freedom Index* is a combined total of political rights and civil liberties indicators, transformed to a scale from 2 (worst) to 14 (best), and taken from Freedom House. The *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. There is little overlap, so 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 4.2.

Figure 4.2 PTS 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 adheres 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 4.3. 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 recipient 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 the Proximity Index, and thus facilitates easier interpretation

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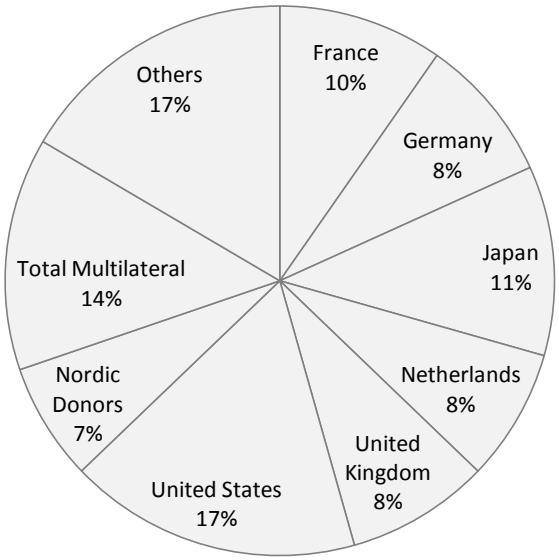
WHICH DONORS?

Figure 4.3 2006 ODA Commitments by Donor, as a % of Total Commitments



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

The literature tends to analyse either average donor behaviour (with deviations from this) or a collection of individual donors. We employ the latter method, focusing throughout on seven donors. The donors found in Figure 4.3 are used, except that multilateral and 'others' are excluded, and Sweden is chosen to represent the Nordic Donors. 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.

4.4 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 4.2 and Table 4.3 report the results for step 1 and 2 respectively, and Table 4.4 the Wald statistics for the four factors.

Table 4.2 1982-2006, Parsimonious Specification, Eligibility Stage

Variables	France	Germany	Japan	Holland	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 4.3 1982-2006, Parsimonious Specification, Level Stage (Standardised Coefficients)

Variables	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 C9. 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 4.4 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 donors 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 *et al.*, 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 *et al.*, 2000). The results show that American and British aid do not have a positive relationship with trade flows, in contrast to other donors.

QUESTION 2: WHAT ARE THE 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 Appendix C (Table C 2 - Table C 8). 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 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 statistics reported below are in essence tests of that assumption.

Table 4.5 Chow Tests for Changed Relationships

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

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

Table 4.5 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 whom 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 4.6 Wald Statistics for Donors with Significant Changes, Using the 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 are shown with P values below, 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 4.6, and further inspection of individual coefficients, we can find the cause of the differences between periods (the effects can be assessed by examining Table C 2 - Table C 8, which report the regressions by period and donor). 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 4.7) 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.

Table 4.7 Retesting the GWOT dummies, USA

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 4.8 reports only the poverty coefficients but they were obtained in a regression using the parsimonious specification over the 25-year time period.

Table 4.8 Income Coefficients: by Step, Period, Stage and Donor

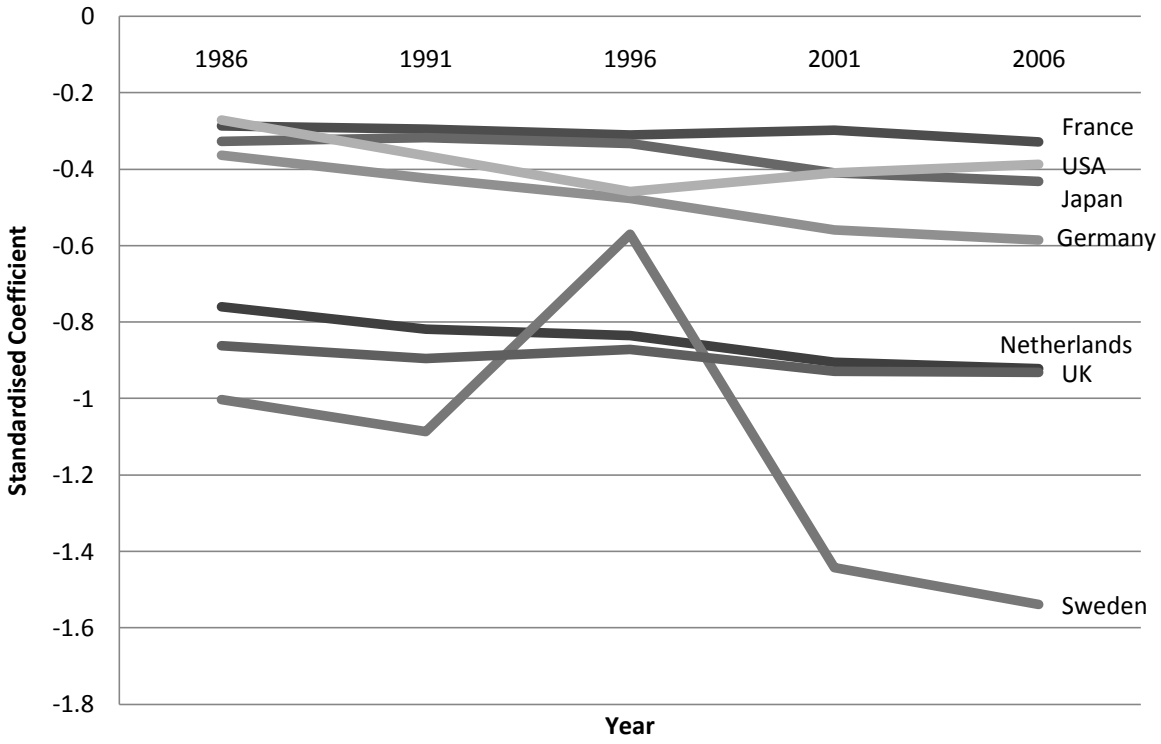
		France	Germany	Japan	Netherlands	Sweden	USA	UK
1st stage	1982-1986	-0.26***	-0.17*	-0.38***	-0.37***	-0.35***	-0.63***	-0.35***
		(3.40)	(2.25)	(5.15)	(5.01)	(4.71)	(8.44)	(4.54)
	1987-1991	-0.29***	-0.19**	-0.37***	-0.43***	-0.37***	-0.63***	-0.36***
		(3.92)	(2.58)	(5.08)	(5.85)	(4.94)	(8.52)	(4.76)
	1992-1996	-0.32***	-0.23**	-0.41***	-0.44***	-0.51***	-0.64***	-0.42***
		(4.29)	(3.18)	(5.64)	(5.98)	(6.54)	(8.80)	(5.55)
	1997-2001	-0.32***	-0.24***	-0.42***	-0.45***	-0.30***	-0.62***	-0.43***
		(4.41)	(3.45)	(5.92)	(6.21)	(4.15)	(8.65)	(5.85)
	2002-2006	-0.32***	-0.24***	-0.42***	-0.52***	-0.30***	-0.60***	-0.44***
		(4.49)	(3.49)	(5.98)	(7.11)	(4.13)	(8.66)	(6.00)
2nd Stage	1982-1986	-0.29*	-0.36**	-0.33**	-0.76***	-1.00***	-0.27*	-0.86***
		(2.39)	(2.74)	(2.61)	(3.97)	(4.53)	(2.35)	(6.02)
	1987-1991	-0.30*	-0.42**	-0.32*	-0.82***	-1.09***	-0.37**	-0.90***
		(2.47)	(3.12)	(2.42)	(4.35)	(4.73)	(2.94)	(6.01)
	1992-1996	-0.31*	-0.48***	-0.33*	-0.83***	-0.57***	-0.46***	-0.87***
		(2.51)	(3.43)	(2.53)	(4.15)	(3.67)	(3.55)	(5.85)
	1997-2001	-0.30*	-0.56***	-0.41**	-0.91***	-1.44***	-0.41**	-0.93***
		(2.39)	(4.03)	(3.11)	(4.59)	(5.11)	(3.08)	(6.33)
	2002-2006	-0.33**	-0.59***	-0.43**	-0.92***	-1.54***	-0.39**	-0.93***
		(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. Controls from the parsimonious specification are included but not reported.

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 4.4. 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 year, 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 4.4 Poverty Sensitivity Coefficients 1982-2006 Level Stage, by Donor



Notes: Coefficients taken from the level stage of Table 4.8, and so a more negative coefficient signifies greater poverty sensitivity.

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[1]. The motivation for suspecting that policy selectivity has increased is the work of Burnside and Dollar (2000) and 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

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

coefficients are reported this scale is somewhat immaterial). This variable has previously been used, but never as extensively (Hout, 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 (this is partly because corruption is distinct from policy per se). 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 4.9 and Table 4.10 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 on corruption, we would expect to find positive and significant coefficients.

Table 4.9 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

Table 4.10 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.

Table 4.9 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 4.10) 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 *et al.*, 2004), some multilateral donors use it as a measure of policy (e.g. IDA). As it is not publicly available for years before 2005, a cross section averaged over the 2001-2006 period is reported here.

Table 4.11 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

Table 4.12 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.

Table 4.11 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 4.12 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 three 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 4.13 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.

4.5 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 here 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 to poverty focus.

Question 2 by contrast revealed 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 *et al.*, 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, 2010), 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 *et al.*, 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 more time to influence allocation policy.

By looking more specifically at poverty sensitivity in question 3 we can reaffirm conclusions already drawn. Figure 4.4 clearly illustrates that the biggest differences in poverty 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, 2010) 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[1]. 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

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

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, 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. However, it is clear 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: increased aid budgets. A 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 (as employed in the next chapter). 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.

4.6 CONCLUSION

This chapter 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)[1].

This chapter 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.

[1] *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.*

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 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.

5. Pull the Other One: Good Governance And The Forgotten Policy Lever in Aid Allocation

The previous chapter can be seen as a test of whether selectivity has been implemented. As the recipient policy indicator was found to have been of marginal concern in allocation decisions for 25 years we can reject the idea that the application of selectivity is widespread, a finding that concurs with those of Hout (2007a, b) Easterly (2007) and Nunnenkamp and Thiele (2006). In isolation this implies that donors are indifferent to recipient policy, which is perhaps misleading. This chapter finds that donors *are* in fact sensitive to policy, although not in the way suggested by the standard conceptualisation of selectivity. It is profitable to briefly consider why selectivity is not practiced more widely. It has been argued that one of the reasons that *conditionality* failed was the inability of donors to punish recipients who had not met conditions as these countries were still poor (Collier, 1997; Easterly, 2001). Selectivity does not negate this characteristic of donors, and so perhaps the practice of donors 'turning a blind eye' to poor governance in part explains how both conditionality and selectivity have failed to be used more widely. The famous aid campaigner Bob Geldof, when campaigning for higher aid, argued that "Something must be done, even if it doesn't work." (Chamberlain, 2005). This statement illustrates the apparent hesitancy of donors to withhold aid in situations where aid is likely to be of little use. We can think about this in terms of the 'warm glow effect', where donors "gain utility from the act of giving" (Andreoni, 1990, p. 473).

Alternatively, this can be explained using the language of the Samaritan's Dilemma. The Samaritan's Dilemma has been used before in this context (Svensson, 2000, discussed later), and it effectively states that a donors concern for the poor stymies their ability to punish low levels of recipient effort. Donors may agree a contract but fail to punish the recipient if it reneges on this contract, as they do not wish to penalise the poor. While this theoretical model is discussed within the context of conditionality, it can give insights into the 'failure' of selectivity (i.e. the failure to implement policy selectivity). It does not appear clear that donors would be more able to punish recipients (by withholding aid) when they have not

agreed a contract. So, the Samaritan's Dilemma may also help explain why selectivity has not been practiced. This chapter introduces the concept of *pragmatic selectivity*, where donors respond to differences in recipient's policy and governance not by altering the amount of aid, but the type. The concept of pragmatic selectivity is so called because it appears to be feasible for donors to implement it, unlike (ex-ante and ex-post) conditionality.

The concept of Pragmatic Selectivity can easily be identified within a number of policy documents and discussions:

> *"…some have argued that in the worst cases - where corruption is rife and governance poor - we should walk away. But we cannot abandon aid just because a country has corrupt leaders … we can earmark aid for a particular programme of work in a sector and account for that money independently through a separate bank account. We do this in the education sector in Kenya, where the financial risk of handing over money to the government is too great."* (Benn, 2006)

> *"Budget support will not always be the most effective way of delivering aid to governments. For example, where government budgets do not prioritise the needs of poor people or where governments are not tackling weaknesses in their public financial management systems or corruption. In these cases we will use other ways of delivering our aid."* (DFID, 2008, p.2)

> *"Even after hundreds of protesters were shot dead by the police after the last elections in 2005, aid to Ethiopia was only repackaged in different forms, not suspended."*
> (The Economist, 2010)

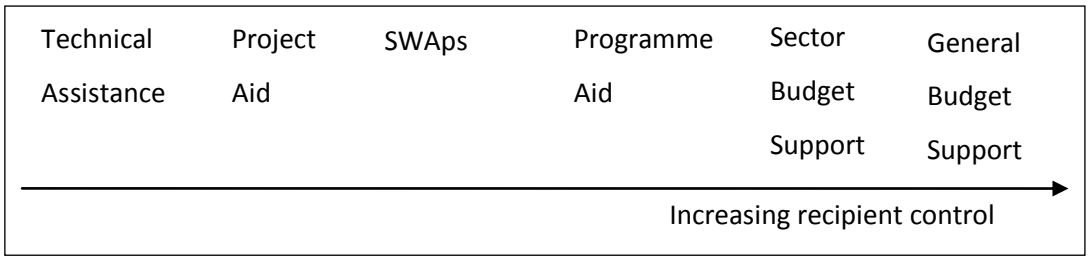
The first quote illustrates immediately the Samaritan's Dilemma. Hillary Benn, the then secretary of state for DFID, dismisses the idea of selectivity because the donor cannot 'abandon' the poor in a country with bad governance. Note that this 'Samaritan impulse' is expressed in the context of selectivity. His proposal is simply to use other means by which to deliver aid. The second quote is the closest explicit statement of pragmatic selectivity in official donor documents that we are aware of. It recognises two factors that should help decide the composition of aid: recipient government preferences for the poor and the efficiency loss of recipient-implemented aid. Both of these points are crucial in the theoretical literature, discussed later. The third quote is a comment on the general

behaviour of donors, again illustrating the 'Samaritan impulse'. The quotes tell us that while aid volumes did not alter after an episode of poor governance, aid composition did. They describe neither conditionality nor selectivity, but a strategy where the policy lever for dealing with low levels of governance is the *type* of aid delivered, specifically the amount of control a recipient is granted. This then implies that the two stage decision (who gets aid and how much) is actually a three-stage decision (adding what type of aid should be given). The chapter continues in section 2 by reviewing the literature. Section 3 presents a simple theoretical model, used to motivate the chapter. Section 4 introduces the empirical approach, and section 5 reports the results. Section 6 discusses and section 7 concludes.

5.2 Literature Review

The theoretical literature models the aid modality choice between two types of aid, generally Project Aid (PA) and General Budget Support (GBS). A brief discussion of various modalities is included first, before moving on to the simplified theoretical conceptualisation. While there are many valid distinctions made between different types of aid, it is best thought of as a spectrum with classification cut-off points being at least partially arbitrary (see Figure 5.1). While a simple dichotomy is often a helpful simplification for theoretical models, a more realistic picture is one of varying degrees of recipient control. Different instruments then offer the recipient different degrees of control. With General Budget Support it is obviously the decision of the recipient how to use that aid money. By contrast, the goal and implementation of project aid is typically not decided by the recipient, but it still offers some control as recipients could anticipate donor spending and decrease their own in a certain sector/region. There are a myriad of other instruments between these two extremes. The majority of measures used within the empirical section are towards the extreme of recipient control, mainly due to data availability.

Figure 5.1: Different Aid Instruments



Many of these different aid modalities, which rise and fall in popularity, were first used because of dissatisfaction with a previous instrument. By understanding the criticisms of the different types of aid, we can better model the aid composition decision, and thus understand and control for other factors. Project aid has been criticised heavily over a long period, with reasons including co-ordination failure, a disregard for recurrent expenditures, high transaction costs and the undermining of local institutions (Ohno and Niiya, 2004 , p. 6). The move towards SWAps in the mid 1990s was in part motivated by a desire to address these weaknesses by clustering projects around a particular sector (Harrold, 1995). The most common contemporary modality with a very high level of recipient control is General Budget Support (GBS). As it is relatively new, well documented and at one extreme of the recipient-control spectrum, it will be used extensively in the empirical section. According to its proponents, GBS solves many of the aforementioned problems with project aid. It is said to strengthen a recipient's own systems (DFID, 2008, p.1) and be more efficient. Indeed the World Bank's budget support in Uganda was found twice as efficient as project support in terms of cost per disbursed USD (Miovic, 2004). An extensive evaluation found the use of GBS improved the overall quality of aid in a country due to increased coherence, harmonisation and alignment (IDD, 2006).

However, GBS is not without its detractors. Killick (2004) and Frantz (2004) both argue that the claim that GBS decreases transaction costs is not sufficiently substantiated. Foster (2000) argues that GBS is in some cases less predictable, and Batley (2005) also reports evidence of timing issues undermining any potential transaction cost benefits. The Netherlands Ministry of Foreign Affairs (2003, p.71) reports that "... for the Netherlands, the decrease in costs (due to pooled funding, harmonisation of procedures and less time needed in direct programme management) is outweighed by the increased time use due to co-ordination, particularly on the sectoral level... Overall, increased intensity of co-ordination has led to an increase of transaction costs for Ugandan partners …" Thus, the differences between aid instruments are highly nuanced and any apparent advantage is not necessarily automatic. Theoretical models, for obvious reasons, simplify this greatly.

From the theoretical literature, we first present the paper by Cordella and Dell'Ariccia (2007) as it has had the biggest effect. The model describes a donor's aid composition choice, between two types of aid: General Budget Support (GBS) and Project Aid (PA). The reader

may be aware of two earlier papers[1] by the same authors. We present the model in Cordella and Dell'Ariccia (2007, hereafter CD) along with criticism and discussion, and include relevant comments on aspects of their other papers. The model contains two agents (donor D and recipient R) and two goods (development good s and non-development good m). The recipient balances the utility derived from these two goods, given by α where $\alpha \in [0,1]$ and a higher α means a more development friendly recipient.

$$U_R = \alpha V(s) + (1-\alpha)V(m)$$

It is implied that the donor's utility is simply $U_D = V(s)$, which makes it clear that this is a principal-agent framework. While Cordella and Dell'Ariccia (2007 p.1264, footnote 10) note that the analysis holds as long donor and recipient preferences over $\{s, m\}$ differ, it is discussed in a situation where the donor is more altruistic than the recipient. Also, if $\alpha = 1$ then there is clearly no issue of divergent preferences. The production function for the development good is given as $s = s(k, e)$ and assumed to be symmetrical. Crucially, k (capital) is observable whereas e is not. It is assumed that the donor can observe the total non-capital expenses $z = e + m$. Implicitly, m is the only input of good m, and so $m = F(m)$.

The budget constraint is given on the recipient side:

$$k + z \leq G + \delta A$$

Where δ is 1 when aid is granted and 0 when it is not; k and z are capital and non-capital expenditures, and G and A are the domestic and external sources of revenue respectively. Using this set up, the paper then deals with three types of aid: unconditional budget support, conditional budget support and project aid. The reservation utility for the recipient is given by the no aid case, where $\delta = 0$. This case is denoted by the superscript *NA*.

$$U^{NA}(\alpha) = \alpha V[s(k^{NA}, e^{NA})] + (1-\alpha)V(G - k^{NA} - e^{NA})$$

We can see that the model assumes that the budget constraint is binding, as $k + e + m = G$ is required to give the last expression. For unconditional budget support (superscript NC),

[1] *The first (Cordella and Dell'Ariccia, 2002) differs from the paper discussed by using a different utility function for the recipient, which is a CES objective function:* $U_R = [\alpha s^{\rho} + (1-\alpha)m^{\rho}]^{1/\rho}$ *Where* $\rho \in [0,1]$. *The analysis continues in much the same way as the version presented here, starting with a situation of full information, moving on to an unobserved e and concluding with unobserved e and α. The second (Cordella and Dell'Ariccia, 2003) combines the theoretical part with an empirical test of its predictions. The theoretical part is almost identical to the model discussed here.*

the problem of the recipient of type α is given by first substituting the budget constraint into the utility function of the recipient, and then using the symmetrical nature of the development good's production function to give the following:

$$k^* = \{\boldsymbol{x}: \alpha V'[s(\boldsymbol{x})]s_x(\boldsymbol{x}) - (1-\alpha)V'(G + \delta A - 2x) = 0\}$$

This finds x such that the marginal utility of the two goods are equal: here $\boldsymbol{x} = (x, x)$, as the production function is symmetrical the recipient has no need to diverge from a situation where $k = e\ (= x)$. With unconditional aid as opposed to the no aid case, the level of s is at least as high as there are additional resources but α has not changed. We can write this as

$$s(k^{NC}, e^{NC}) \geq s(k^{NA}, e^{NA})$$

As long as α is positive the above becomes a strict inequality. However, if $\alpha < 1$ then the additional resources are not entirely spent on the development good, implying conditionality may improve the donor's welfare. Given the assumptions on observability, conditionality can only be on the basis of k and not e. As such, the analysis proceeds with the donor setting a required level of k upon which the aid is conditional. It is worth noting that Bougheas *et al.* (2007) discuss the idea of partial conditionality as a prior action signalling efficiency further, and find that such conditionality may not provide enough information for the donor to ascertain whether the recipient is truly more efficient than another. This casts doubt on the effectiveness of the screening mechanism. Continuing with the CD model, the recipient problem, for any $k > k^{NC}$, is given by:

$$\hat{e}(k) = \arg\max_{e}\{\alpha V[s(k,e)] + (1-\alpha)V(G + A - k - e)\}$$

Where the recipient chooses only e in response to the k that is set by the donor. The problem of the donor is one of setting the required minimum level of k, taking into account the likely response of the recipient:

$$\max_{k} W = s[k, \hat{e}(k)]$$

Subject to $\alpha V\{s[k, \hat{e}(k)]\} + (1-\alpha)V\big(G + A - k - \hat{e}(k)\big) \geq U^{NA}(\alpha)$

This constraint is the individual rationality (IR) constraint of the recipient which simply states that their utility must be greater with conditional budget support than with no aid in order to accept the contract. We can take the no conditionality level of capital (k^{NC}) as a minimum

level of k, and the case where the IR constraint of the recipient is an equality (k^{IR}) as the maximum. The donor has no incentive to set a level lower than k^{NC} and the recipient would not agree to a contract in which it would be worse off than under project aid (given by k^{IR}). Two important points are demonstrated. First, it is shown that the production level of the development good is greater under conditionality. This is important on the donor's side, as they would only prefer conditionality if it increases the production of the development good. Second, conditionality results in an inefficiency in the production, as the level of k will be somewhere between the maximum and minimum: $k^{NC} < k < k^{IR}$ and $\hat{e}(k) < k$. As e is unobserved, the recipient will under provide e for any given level of k, as opposed to the no conditionality case. While the donor buys an overall increase in the production of the development good it loses efficiency, as the recipient revaluates its input decision and alters its production inputs.

The optimal level of conditionality is given by the following:

$$k^c(\alpha) = \min\{k^{IR}; \hat{k}\}$$

with

$$\hat{k} \equiv \arg\max_k s[k, \hat{e}(k)]$$

Here, $\hat{k}$ represents the level of conditionality that the donor would choose if it paid no attention to the recipient's IR constraint. When that constraint is binding, the maximum level of conditionality that the recipient would accept is k^{IR}.

Project aid is introduced as an alternative modality. While budget support is subject to the problem of preference misalignment, project aid is subject to fungibility. The donor's use of project aid is assumed to mean foregoing any influence over the allocation of the recipient government's budget, but gaining complete control over one's own. Project aid is assumed to be an imperfect fit into the recipient's own development strategy. As such, while the production function for the development good remains the same, there is a cost of $(1-\lambda)s(\boldsymbol{A}/2)$ where $\lambda \in [0,1]$ describes the severity of this cost. Alternatively, this could be thought of as an efficiency cost. It is found that for conditional budget support to result in a higher level of the development good than project aid, G and α must be sufficiently high. A larger G means that the donor can influence a larger amount of resources. A larger α means that there is a smaller difference between donor and recipient preferences, and so

conditionality will be met with more favourably. If λ is high (i.e. the efficiency loss of project aid is low) the results are driven by preference alignment. This is the argument found in Morrissey (2006), where the key consideration in the GBS decision is the degree of preference alignment (α) rather than efficiency (λ) or the relative size of the aid transfer (G and A).

The final extension of the model is to consider the case where α is not observable, that is the recipient's type is unknown to the donor (a situation explored more fully in Bougheas *et al.*, 2007) . It is this part of the model which most resembles my own. The rationale for examining this is easily understood by considering a case where a recipient cannot credibly signal its type. If this is the case, the contract design must alter. The distribution of types,$[\underline{\alpha}, \overline{\alpha}]$, is common knowledge, as is G. The following simple case is considered:

$$\underline{\alpha} = \alpha_0 = 0 \qquad \text{with probability} \qquad 1 - p$$

$$\overline{\alpha} = \alpha_1 = 1 \qquad \text{with probability} \qquad p$$

We know that type α_0 recipient will prefer budget support to project aid if $k < A$. Where $k = A$, the α_0 type recipient is indifferent between the two and so for simplicity we say it prefers project aid. Optimal conditionality for type α_1 is given by $\hat{k} = (G + A)/2$. This is clear, as the donor would prefer all resources to be spent upon producing the development good, and so choose $e = k = (G + A)/2$. The ability of the donor to distinguish between the two types of recipient by altering the menu of contracts offered hinges on the relative size of G and A. Where $G \geq A$ the donor can separate the two types, and so expects the following development good output:

$$E(s) = ps\left(\frac{\boldsymbol{G + A}}{2}\right) + (1 - p)\lambda s\left(\frac{\boldsymbol{A}}{2}\right)$$

So, in this case the recipient with aligned preferences chooses to receive budget support from the menu of contracts offered, and the recipient with misaligned preferences chooses project aid. As the model always assumes full commitment, the intuition for this is clear. The required change of behaviour for the α_1 type recipient will be less, and so the recipient prefers budget support as it is a higher transfer (as there is no efficiency loss). For the α_0 type recipient this level of conditionality is a large change of their desired behaviour, and so project aid is preferred (despite the assumed higher transaction costs). For the unlikely case

of $G < A$, there are three options to choose from as to how best separate the recipients, which are not presented here.

There are six criticisms to be made of the model and its assumptions. First, the model states that fungibility is higher for larger projects, and lower for smaller ones. The rationale for this comes from a situation where the aid budget can be larger than the recipient government's own resources, with one donor. In this situation, the recipient would have spent less than the donor spends on a specific area, and so the recipient cannot reallocate, as it can only take away the amount it would have spent on that area. However, this does not seem to be a realistic assumption. As discussed in the literature review, most recipients have a large number of different projects and donors. In this case, it is likely that recipients can only anticipate the larger projects and reallocate their resources in response to the largest aid projects (i.e. the opposite effect to that assumed by CD). Second, while project aid is subject to a cost that can be interpreted as an efficiency cost, GBS is not. This assumes that project aid is at best as efficient as GBS, whereas in some circumstances GBS could be less efficient than project aid. The evidence base for an efficiency gain from GBS is certainly not conclusive (Batley, 2005; Frantz, 2004; Killick, 2004; Netherlands Ministry of Foreign Affairs, 2003). Third, CD assume that a donor always chooses one instrument, rather than a combination of instruments. As shown elsewhere, most donors do use a variety of different types of aid. Fourth, CD do not fully address the insights of the Samaritan's Dilemma, as they always include full commitment. Svensson (2000) shows that donors find it very difficult to adhere to contracts if they require punishing recipients. This means that the model's reliance on full commitment undermines to some extent its attempt to resemble reality. As such, the situation where the donor sets the required level of k, to which the recipient must adhere if it is to receive GBS, is a problematic assumption. In reality a separate technology is available to the recipient: reneging on this level of k. Fifth, if we think of the 'Samaritan impulse', a situation where donors completely withhold aid (giving the reservation utility of the recipient) is unlikely, as donors find it problematic to withhold aid. Thus the reservation utility of the recipient is unlikely to be the no aid case. Sixth, CD assume donors are altruistic, although their model could be presented in a different context.

We now turn to Hefeker (2006)[1], who uses a similar weighting of two groups to CD although in a slightly different set-up. The recipient's utility function is given by:

$$W_R = \alpha u_1 + (1-\alpha)u_2$$

Where group 1 is the poor, group 2 is the rich and α determines the weight placed upon these two groups in the utility function. The utility of each group is given by the shortfall from a target, which equals entire discretionary resources of the government: $u_i = -(g_i - g_i^*)^2$ where $g_i < g_i^*$ and $g_i^* = G + \delta A$. g_i is the level of government spending for group i, and g_i^* is its target. The budget constraint of the recipient is simply $g_1 + g_2 \leq G + \delta A$. Budget support and Project aid are assigned the efficiency loss parameters ε and η respectively. These can be interpreted as losses to corruption or simply as measures of marginal cost, i.e. efficiency parameters. Unfortunately, this feature of the model is not fully developed, and is only used briefly in the last stages of the paper. Under the benchmark case of unconditional budget support, the recipient allocates its budget as it sees fit, giving the shares:

$$g_1^{NC} = \alpha(G + \delta A)$$

$$g_2^{NC} = (1-\alpha)(G + \delta A)$$

Hefeker (2006) starts by assuming that the donor is benevolent, such that its utility function is:

$$W_D = \beta u_1 + (1-\beta)u_2$$

Clearly, this resembles the CD set up if $\beta = 1$. In that model the donor only cares about the poor, whereas this set-up allows for the donor taking into account the rich's welfare as well. There is an additional assumption that $\beta > \frac{1}{2} > \alpha$, i.e. the donor is more pro-poor than the recipient. Project aid is used to introduce the idea of fungibility. In the paper, the author states that recipients can foresee the amount of project aid and adjust their own budget allocation accordingly. Specifically, the production of the development good is $g_1^{PA} = \alpha(G - T) + T$ after fungibility, where T is the amount of aid given as project aid. However, this appears to be an erroneous assertion. If the recipient were to control the entire budget,

[1] *Some of the notation has been changed, so as to facilitate ready comparison to the other papers reviewed in this section.*

it would allocate $g_1^{PA} = \alpha(G+T)$ and $g_2^{PA} = (1-\alpha)(g+T)$. As such the adjustment in resources for group 1 should not be $-\alpha(T)$ but $-(1-\alpha)T$. This does result in the correct final allocations:

$$g_1^{PA} = \alpha(G) - (1-\alpha)T + T$$

$$g_2^{PA} = (1-\alpha)(G) + (1-\alpha)T$$

That notwithstanding, complete fungibility does not seem a realistic assumption, as recipients are unlikely to be able to perfectly anticipate the amount of aid. Furthermore, fully reallocating its budget is likely to be costly and time consuming. The analysis then moves to conditional aid. The donor sets a target level for the proportion of the budget that gets allocated to the poor, $\hat{g}_1$. This target will be higher than under no conditionality $\hat{g}_1 > g_1^{NC}$, otherwise the donor would not use conditionality. The donor then differentiates the amount of aid given on the basis of whether this minimum level of spending on the poor has been achieved, such that

$$A = \begin{cases} \overline{A} \; if \; g_1 \geq \hat{g}_1 \\ \underline{A} \; if \; g_1 < \hat{g}_1 \end{cases}$$

While CD assume the reservation utility is given by no aid, Hefeker makes the situation somewhat more realistic. As he notes, it is unlikely that $\underline{A} = 0$, as donors are subjected to the Samaritan's Dilemma (Svensson 2000) they are reluctant to completely withhold aid, and so are unable to credibly threaten this amount. However, this model still relies upon a change in the amount of aid committed, something that the previous chapter found very little empirical evidence for. Continuing with the analysis, we can write as $\hat{A}$ the level of aid that must be given by the donor in order for them to accept $\hat{g}_1$, where $\hat{g}_1 = \beta(G + \hat{A})$ and β is the donor's weighting of the poor. The critical value of conditional budget support is increasing in $\underline{A}$ and α but decreasing in G and β. These are expected results. $\underline{A}$ is aid received when conditional budget support is rejected, and so gives the fallback utility. α and β are the weighting given to the poor by the recipient and donor respectively. If these are close then the difference between the recipient and donor's desired value of spending on the poor is small. In other words a high α and low β would give a smaller value for $\hat{g}_1 - g_1^{NC}$, meaning the utility loss for the recipient is smaller. An interesting extension of the model is taken from a simple insight – donors are unlikely to be altruistic and bureaucratic incentives may

play a part. This is incorporated in the model by augmenting the donors objective function so it becomes:

$$W_D = \beta u_1 + (1-\beta)u_2 + \gamma(A) + \chi(T)$$

Where $\gamma(A)$ and $\chi(T)$ are the inherent bureaucratic utility derived from disbursing GBS and project aid respectively. Many of the conclusions from this step are similar to those found by CD: for example, they find that budget support is preferred when aid is small relative to the recipient's own resources. However, Hefeker (2006) concludes that if there is not preference alignment, budget aid will be *preferred*. This result comes from the assertion that budget aid is likely to be larger than project aid (if given), something that comes from the assumptions regarding bureaucratic incentives.

Unfortunately, the mathematical derivation of much of the analysis appears to be flawed.[1] While this calls into question many of the results, it does not invalidate the innovative contributions of this paper. The contributions are four-fold. First, it allows for a low donor offer of aid, as opposed to the withholding of aid, to form part of the reserve utility of the recipient. As the previous chapter found that aid allocation is policy sensitive, this seems a more realistic assumption. Second, the paper acknowledges that bureaucratic incentives will play a part in the modality decision of the donor. Third, it models the problem in a new setting – using shortfalls in utility from a target consumption as opposed to a more production-orientated set-up. This is perhaps a more realistic setting, given the likely lobbying by the two groups. Fourth, it allows for the different types of aid to have different efficiency parameters. Frustratingly, this innovation is not explored as their use is limited to a small part of the analysis.

Other than the apparent mathematical mistakes, there are two specific weaknesses of the model. The first is that it does not truly capture an interaction between the two agents, as they are considered separately without truly taking into account the preferences of the

[1] *Further to the apparent mistakes mentioned elsewhere, there is something that this author cannot replicate from the appendix, where the following two equations (original notation maintained) are made equal:* $W_R^{BA} = \alpha(\hat{g}_1 - G - \overline{A})^2 + (1-\alpha)(G + \overline{A} - \hat{g}_1 - G - \overline{A})^2$ *and* $W_R^{NC} = \alpha(g_1^{NC} - G - \underline{A})^2 + (1-\alpha)(G + \underline{A} - g_1^{NC} - G - \underline{A})^2$. *The next step in the paper is to state that* $\alpha(\hat{g}_1 - G)^2 + (1-\alpha)(\overline{A} - \hat{g}_1)^2 = \alpha(g_1^{GOV} - G)^2 + (1-\alpha)(\underline{A} - g_1^{GOV})^2$, *this result is not explained and cannot be reproduced. The working paper version has a different step, but one that is more obviously flawed. Specifically, it goes from the same situation as above* $\alpha(\hat{g}_1 - G)^2 + (1-\alpha)(\overline{A} - \hat{g}_1)^2 = \alpha(g_1^{GOV} - G)^2 + (1-\alpha)(\underline{A} - g_1^{GOV})^2$ *to* $(\hat{g}_1 - G)^2 - (g_1^{GOV} - G)^2 = \lambda[(\underline{A} - g_1^{GOV})^2 - (\overline{A} - \hat{g}_1)^2]$. *However, it then simply takes the root of each side (without taking into account that it should be the root of the total) and concludes that* $\hat{g}_1 - g_1^{GOV} = -\sqrt{\lambda}[(\underline{A} - g_1^{GOV}) + (\overline{A} - \hat{g}_1)]$.

other. The second weakness stems from the use of a shortfall as the utility function. Consider the case of the poor without aid, as opposed to the no conditionality case.

$u_1^{NA} = -(g_1^{NA} - G)^2$

$u_1^{NC} = -(g_1^{NC} - G - A)^2$

We know that the recipient, without conditionality, would choose $g_1^{NC} = \alpha(G + \delta A)$, giving the poor the following shares under no aid $g_1^{NA} = \alpha(G)$ and under aid (without conditionality) $g_1^{NC} = \alpha(G + A)$. We can then state the difference between the two as simply αA, giving $g_1^{NC} = g_1^{NA} + \alpha A$. We also know that $\alpha < 0.5$, which means the αA cannot be as large as A, thus the poor are always worse off with aid than without it. Put mathematically:

$u_1^{NA} > u_1^{NC}$, as

$-(g_1^{NA} - G)^2 > -(g_1^{NA} + \alpha A - G - A)^2$

This is an intuitively simple to understand mistake: the target consumption includes aid, and giving aid to a recipient increases their target consumption by more than the increase in their actual consumption. This methodological weakness means that donors should, if acting in a manner consistent with their stated preferences, never give aid.

Jelovac and Vandeninden (2008, hereafter JV) correct a weakness of the CD paper, by allowing two modalities to co-exist. In order to model this, the marginal utility of the two goods cannot be constant (if they were, one would dominate the other over the whole plane). Thus a Cobb-Douglas form is used for the production function of the development good, and recipient's utility function. Their results are very sensitive to the assumptions made on the efficiency and fungibility losses of project aid. The conclusions differ from CD in two major ways. First, while CD always prefer conditionality, JV find it depends upon the efficiency of the two modalities, preference alignment and the size of the aid budget relative to the donor's own resources. Second, JV find project aid is only preferred when preference alignment and project aid's efficiency loss are low. The approach of JV is an improvement on the CD paper in two main ways. First, recognising the co-existence of various modalities brings the model much closer to reality. Second, the paper emphasises a little more the role of efficiency in the modality choice. However, being so close to the CD paper, it inherits a number of its weaknesses. First, they assume that project aid is at best as efficient as budget

support. The only possible loss to budget support is assumed to be the reallocation of resources stemming from preference misalignment (the crowding out of development expenditure). It is possible however, that project aid is more efficient than budget support in certain cases. Second, JV assume full commitment. While they allow for non-observability of development inputs, complete commitment overstates the likely effect of conditional budget support.

In a similar vein, Azam and Laffont (2003) apply contract theory to the problem, but focus on complete contracts with perfect monitoring, a more extreme position than CD. Hagen (2006a, p.268) is particularly vocal in his criticism of this approach, arguing that contract theory is not appropriate given that in this setting contracts "cannot be enforced in courts and the generally poor record of conditionality demonstrates that such agreements have not been self-enforcing either." The suggested remedy is to use a game theoretical approach, although as the paper does not focus on the choice between modalities it is of little use here. Furthermore, as currently used in this field, game theory implies that the two agents have similar bargaining power, meaning that the suggested modelling framework is not applicable to the aid allocation problem.

Svensson (2000) applied the Samaritan's Dilemma to the aid allocation problem, and while the paper does use different aid modalities it is not the focus. That notwithstanding, the analysis provides one salient insight and so is summarised here. Donors are inequality averse, and so allocate aid to poorer recipients. Recipients anticipate this and so a moral hazard problem is created, as they have less incentive to decrease poverty. In essence, a donor's attempt to punish poor recipient effort is stymied by their own inequality aversion. This analysis is an elegant explanation of 'conditionality failure', that is the failure of donors to *implement* conditionality. It also hints as to why donors do not implement selectivity (ex-post conditionality), as suggested in the preceding chapter. The relevant point for *this* chapter is to underline the flaw of a model that assumes full commitment. However, it would be an overreaction to assume that no commitment is a more realistic assumption. Federico (2004) shows that even under weak commitment, conditionality is still preferred to no conditionality. For the purpose of this chapter, it is sufficient to acknowledge that while full commitment does not exist, donors have succeed in changing the overall spending patterns within a recipient country (Mosley *et al.*, 2004). This means that the composition of recipient spending is not fixed and inflexible but influenced by donors.

Svensson's (2000) suggested remedy focuses on the delegation of aid allocation decisions to international bodies that has less inequality aversion. From a theoretical point of view, Hagen (2006b) questions this recommendation finding that it does not hold when aid efficiency varies across recipients. Furthermore, it is clear that this has not been widely practiced, given the low levels of multilateral commitments. Indeed, there is not enough substantive evidence that multilateral organisations have consistently enforced conditionality to conclude with any confidence that they are not subject to the Samaritan's Dilemma themselves. While it is out of the remit of this chapter to fully explore this idea, it is worth noting that the use of different instruments may offer a way out of this dilemma. In this situation, the amount of aid responds to poverty (fulfilling the 'Samaritan impulse'), and the type of aid to effort/policies (fulfilling the 'efficiency impulse').

Many of the factors within the theoretical literature are echoed by the more policy-orientated literature. While Fritz and Kolstad (2008) do not agree with the high degree of fungibility assumed by some of the theoretical papers, they agree that in certain extreme situations it renders project aid no different in outcome to budget support. The role of preference alignment is acknowledged by DFID (2006 p.24), who state the recipient government's commitment to poverty reduction as one of the three factors in their modality decision (the others being the level of policy and human rights). While there is agreement on the importance of aid dependency and the leverage afforded a donor by the relative size of their aid transfer, the effects expected are contradictory. Fritz and Kolstad (2008) argue that the higher the level of aid dependency, the greater the level of control a donor has with project support. However, IDD et al (2006), argue GBS is "more likely to be an attractive option to countries that are significantly aid dependent." This position is also held by Morrissey (2006). The expected effect depends upon beliefs regarding fungibility and donor influence. In the high aid dependency case, those that argue GBS is the preferred modality focus on the ability of donors to influence the spending composition of donors. Those that argue project aid is preferred in this situation have less confidence in the ability to influence recipients, and focus instead on the assured (but not necessarily additional) spending in the donor's priority areas.

5.3 A Simple Model of Aid Modality Choice

I consider a situation with two agents, a donor (D) and recipient (R) and two possible goods consumed within the recipient country (g_1 and g_2). g_1 can be thought of as all expenditures

that are valued by both the recipient and the donor, and g_2 as all expenditures that are valued by the recipient but not the donor. The recipient's utility is then a weighted sum of these two goods, whereas the donor only values the former. This is referred to as the development good for ease, but note that it is not necessary to assume the 'development good' is actually more efficient than the 'non-development good' in reducing poverty, merely that there exists the possibility of preference misalignment. The distinction between the two goods is simply a demarcation between those which the donor considers a valid use of aid and that which it does not. The utility functions of the recipient and donor are given below:

$$U_r = \alpha g_1 + (1-\alpha) g_2 \quad (1)$$

$$U_d = g_1 + a_r + a_d \quad (2)$$

Where $\alpha \epsilon [0,1]$, and a higher value of α represents a higher degree of preference alignment between the donor and recipient. a_r and a_d represent GBS and Project aid respectively (given the subscript *d* or *r* according to whether it is controlled more by the donor, Project aid, or the recipient, GBS). The theoretical literature commonly relies upon a donor's ability to offer different levels of aid depending upon recipient preferences and commitments. The previous chapter found that this ability to vary aid *levels* in response to policy indicators is not evident. As discussed in the introduction of this chapter, this can be explained as the 'Samaritan impulse' or 'warm glow' effect. This has been incorporated into the utility function of the donor as the sum of aid disbursed, $a_r + a_d$. This models directly the warm glow effect, where donors "gain utility from the act of giving" (Andreoni, 1990, p. 473) as well as from the effect of their giving. Because of this, the amount of aid remains constant throughout the analysis. For simplicity we use the utility function of $U_d = g_1$ in the following equations, but keep the idea that disbursal increases utility by including the assumption that all aid is disbursed.

The context of the model can be explained using the idea of the three step decision: *who* gets aid, *how much* aid they get (both modelled in the previous chapter), and finally *what type* of aid they get. The first two stages are exogenous to this model, and can be thought of as being taken by the donor's head office. The third choice, the type of aid, can be thought of as being taken by the donor's field office. The donor then chooses between the types of aid that it has available (we consider only two for convenience), so as to maximise its

expected welfare. The simple production functions of the two goods and the budget constraints of the recipient and donor are given below.

$$g_1 = e + \theta a_d \quad (3)$$

$$g_2 = m \quad (4)$$

$$G + a_r \geq m + e \quad (5)$$

$$A = a_r + a_d \quad (6)$$

Where m and e are production inputs of the recipient (analogous to expenditure allocation) for the two types of good, a_r and a_d are GBS and project aid respectively, θ is an efficiency parameter for project aid, and G is the recipient's own discretionary budget. Whereas the theoretical literature has generally constrained project aid to be at best as efficient as GBS, here it is only assumed that $\theta > 0$ so that θ can be interpreted as the efficiency of project aid *relative* to GBS. Implicitly, we assume that the efficiency parameter is $\theta = \theta^*/\lambda$, where λ is the efficiency of a_r. To simplify matters (without losing generality), we use the relative efficiency term θ and normalise the parameters such that $\lambda = 1$. We start by assuming all variables are common knowledge, and that recipients are either good ($\alpha = 1$) or bad ($\alpha = 0$) from a donor perspective. The assumption of either an entirely aligned or non-aligned recipient is extreme, and made in order to increase ease of comprehension. It is worth noting two points. First, the model is dealing only with discretionary spending, and so no production of the development good does not equate with no utility for the poor, merely no extra utility above some base level. Second, the later introduction of the probability of misalignment tempers this assumption considerably.

If the recipient is type α_1, then there is no preference misalignment as both agents sole concern is the g_1 good.

$$U_r = U_d = g_1 \quad (7)$$

In this case the recipient would choose $G + a_r = e$, regardless of the donor's actions. That is, the entire recipient's discretionary spending is approved of by the donor. The donor's utility is given by:

$$U_d = e + \theta a_d = G + a_r + \theta a_d \quad (8)$$

Where there is no preference misalignment the sole consideration is the relative efficiency of the two modalities. The donor would seek to maximise its utility, and comparing the marginal efficiency of the two types of aid gives:

$\frac{\partial U_d}{\partial a_r} = 1, \frac{\partial U_d}{\partial a_d} = \theta.$

So, donors would choose to distribute aid either as GBS or as project aid, depending on the efficiency parameters of the two. If $\theta > 1$ the donor would choose project aid, if $\theta = 1$ it would be indifferent, and if $\theta < 1$ it would choose GBS. If the recipient is type α_0, there is complete preference misalignment regarding the discretionary budget. As such, the recipient, regardless of the donor's choice, chooses $G + a_r = m$. Clearly, the donor then chooses project aid (as all resources given to the recipient would be 'misspent'), and so $g_1 = \theta a_d$. We now move to the more interesting situation where there is not complete information.

with probability p, $\alpha = \alpha_1 = 1$

with probability $1 - p$, $\alpha = \alpha_0 = 0.$

Where $p\epsilon[0,1]$, the distribution of which is commonly known. We first find the recipients reserve utility function, given by project aid. If it is the bad type, it does not allocate any resources towards the development good. If it is the good type, it gains utility both from its entire discretionary budget, but also from the donor's project aid.

$U_r^{PA}(\alpha_0) = g_2 = m = G$ (9)

$U_r^{PA}(\alpha_1) = g_1 = e + \theta a_d = G + \theta a_d$ (10)

And the recipient's expected utility is then $E(U_r^{PA}) = G + p\theta a_d$.

Under Project aid, the donor's utility is the total development good it produces, augmented by any produced by the recipient.

$U_d^{PA}(\alpha_0) = \theta a_d.$ (11)

$U_d^{PA}(\alpha_1) = G + \theta a_d.$ (12)

The donor's expected utility is then $E(U_d^{PA}) = pG + \theta a_d$.

If the donor were to give GBS, the donor's utility would be completely determined by the recipient's actions. If they have misaligned preferences, the recipient does not allocate any of its additional discretionary resources to g_1, and so the donor receives no utility. If they have aligned preferences, all resources are spent on the development good (g_1). Regardless of their preferences, the recipient always allocates its available resources as it sees fit, and so always receives the output it desires. This is mathematically stated as:

$$U_d^{GBS}(\alpha_0) = 0 \quad (13)$$

$$U_d^{GBS}(\alpha_1) = e = G + A \quad (14)$$

$$U_r^{GBS}(\alpha_0) = U_r^{GBS}(\alpha_1) = G + A \quad (15)$$

And so

$$E(U_d^{GBS}) = p(G + A) \quad (16)$$

$$E(U_r^{GBS}) = G + A \quad (17)$$

As aforementioned, the reserve utility for the recipient is given by project aid. For GBS to be given instead, both agents need to receive at least as much utility under GBS. The agent's participation constraints are:

$$E(U_r^{GBS}) > E(U_r^{PA}) \quad (18)$$

$$E\left(U_d^{GBS}\right) > E(U_d^{PA}) \quad (19)$$

For the recipient, this means

$$G + A > G + p\theta A$$

i.e. if $1 > \theta \cdot E(p)$then GBS is preferred, which means Project aid is only preferred if the efficiency gained from using project aid (if any) is enough to offset the potential preference misalignment.

The donor will prefer GBS if

$$p(G + a_d) > pG + \theta a_d$$

i.e. if $E(p) > \theta$. This is more easily understood negatively, as depending on whether the expected preference misalignment is smaller than the relative efficiency loss of project aid. This simple framework shows that there are two main factors that influence the choice of

aid modality: the preference alignment (modelled here as the expectation of preferences being aligned) and the efficiency of Project aid relative to GBS. The real value of the model is that it maintains the role of alignment from the existing theoretical literature, and adds to it a more appropriate role for the relative efficiency of the two types of aid. In order to derive a reduced form equation, we can simply state (from the above) the following:

$$\frac{a_r}{A} = F(E[p], \theta)$$

As θ has been understood as the *relative* efficiency parameter, we can extend this slightly. We have been using $\theta = \hat{\theta}/\lambda$ and normalising the parameter using $\lambda = 1$. If we now let this vary, we can say that $E(p) > \hat{\theta}/\lambda$ is the important result that determines the donor's modality choice, where λ is the absolute efficiency of a_r and $\hat{\theta}$ is the absolute efficiency of a_d. This then gives the following

$$\frac{a_r}{A} = F(E[p], \hat{\theta}, \lambda)$$

With the results $F'(E[p]) > 0$, $F'(\hat{\theta}) < 0$ and $F'(\lambda) > 0$. The results are intuitive. In keeping with the previous theoretical literature, we find that the greater the degree of alignment, the more likely GBS will be preferred by the donor. We also confirm that the efficiency of GBS and project aid are crucial, as they determine the effect of aid. We abstract from issues regarding the size of the aid budget relative to the size of the recipient's budget, as we acknowledge that while aid may be a significant source of external finance within a recipient country, the normal situation is for this to be highly disbursed between different donors. Also, the model presented here does not rely on the assumption that donors possess commitment technology. As discussed it is unlikely that donors are credibly able to threaten punishment and so models that rely heavily on the assumption of full commitment are not to be preferred.

5.4 Empirical Approach

The closest existing empirical research that we are aware of is Knack and Eubank (2009). They propose a simple theoretical model with the use of recipient systems thought of as a public good that causes improvements that are enjoyed by all donors (who use government systems). In their framework an individual donor is more likely to use recipient systems if they are more likely to benefit from their improvement (measured by the share of recipient

aid given by a donor), their citizens have a high level of trust in development aid and/or the recipient systems are already of a high level. This simple model is then tested using three dependent variables taken from the OECD (2008) survey *Monitoring the Paris Declaration*, which measure some elements of recipient control. Our analysis differs in that we do not try to explain differences between donors, whereas they aim to account for them by using the level of support for development aid amongst their domestic population and their type (e.g. bilateral, multilateral). Our analysis can include more observations than Knack and Eubank (2009), who are limited to a maximum of 59 donors, 55 recipients and one year, although regression results imply an average of only 13 recipients per donor (as data is only available if a donor gives aid to a recipient). Although they often report a large sample size, this is misleading as a very large proportion of observations are in effect missing (as a given donor may never give any type of aid to a given recipient). Using several datasets affords many more dependent variables, each of which provide new information on the variation and distribution of recipient control. The synthesis of these different perspectives constitutes a more extensive test of the theoretical prediction.

A major impediment to researching this question is data scarcity. Ideally, a measure of the control a recipient exercises over ODA related to their country would be used to see which factors best explain this decision. However, data relating to different aid modalities are scarce, misleading and often incomplete. In fact, they can be misleading precisely because they are incomplete, as any judgement on the general composition of aid is only valid if data on the majority of aid is available (i.e. the aid share given as a certain type of aid is the relevant information, not the absolute amount of aid). Even if data existed on the use of all aid modalities, it would be difficult to combine this into a single sensible measure, as one would need to ascribe a certain level of recipient control to each modality. Here, we use different variables to proxy for recipient control, which complement one another to provide the best evidence available to answer the question at hand. We first discuss the independent variables, as they are largely constant throughout the analysis.

THE INDEPENDENT VARIABLES

We have previously found $\frac{a_r}{A} = F(E[p], \theta, \lambda)$, and so the equation to be estimated, in words, is:

$$\frac{a_r}{A} = \beta_0 + \beta_1 Preferences + \beta_2 Governance + \beta_3 Transaction\ Costs + \beta_4 Controls + \varepsilon$$

With the expectation that $\beta_1 > 0$, $\beta_2 > 0$, and $\beta_3 < 0$. I will now introduce the variables that measure the factors.

Preferences (p): much of the theoretical literature that focuses on a choice between project and programme aid finds the alignment of preferences between donor and recipient to be the key factor. This is distinct from governance or policy, in that it doesn't capture the ability to meet goals, but rather the desirability of those goals from the donor perspective. To capture this we include the percentage of *public spending on education*, with GDP as the denominator. This is close to what has been termed 'pro-public expenditure' (PPE) and should be a good proxy for *e* (the development good's input in the model presented). The assumptions of the theoretical literature are normally of a benevolent donor, who wishes to maximise development outcomes, and as such we can think of donor preferences being constant and predictable. The alignment of recipients to this goal is very difficult for donors to quantify, and we use these data as a signal regarding the commitment to development of a potential recipient. Any missing data (especially in the years after 2006 and before 1998) are replaced with the nearest available data. This closely mirrors the best available data a donor would have, and is therefore a suitable solution. Also included here is the variable *prsp* which is a dummy that takes the value one if the recipient has published a PRSP-related document (this includes progress reports and so forth). This is to capture ownership and the existence of a recipient-led approach, which are likely to increase the efficiency of government-implemented aid-funded activities. The *prsp* variable is a dummy, but may capture alignment more accurately as the process requires a number of prior actions to be taken, over a period of time.

Governance (θ): this is the key motivation of this research. Governance here is conceptualised using a narrow definition of the ability to convert aid inputs into development outcomes. We expect that a donor is less likely to use recipient systems if it incurs a large efficiency loss in doing so, which would be represented here by a positive coefficient on the variables representing governance. There are two main datasets which are relevant here. From the first we can choose from the six Governance Matters variables including *government effectiveness* and *control of corruption*, from 1996 until the present for up to 190 countries. The second is the CPIA, which includes the variables *general public sector quality* and the *quality of the budget*, but only for up to 75 countries over the years 2005-2008. The two sets of variables are correlated, particularly *public sector management*

and *government effectiveness* (0.84). *Government Effectiveness* is chosen as it provides the best reflection of the theoretical conceptualisation of governance (which is the efficiency of government in producing a development good) and coverage.

Transaction Costs (λ): within the policy community transaction costs are a common argument for using newer aid modalities that give the recipient more control. Essentially, they are thought to determine the efficiency of project aid, and so are as good measure of λ. If this is a major motivation, we would expect more control to be granted to recipients that face higher transaction costs. The *number of donors* is included as a measure of how fragmented aid is within a given country, measuring simply the number of donors to disburse aid within that country on the CRS/OECD. The expectation is that recipients with higher fragmentation would have higher transaction costs, and in turn see more efforts by donors to reduce these costs. Ideally this would be a concentration measure, but this is not available. Aid dependency is another potential indicator for higher transaction costs: for a recipient that receives large amounts of aid relative to its GDP, the transaction costs are higher as a percentage of GDP (even when assuming some economies of scale). Aid dependency is also likely to be positively related to new modalities for the simple reason that the more aid there is in a country, the more likely it will be to take different forms. The measure of aid dependency used is *aid as a % of GDP*.

Controls – The income of a recipient (*GNI per capita* PPP in international dollars) is used to measure income. C*eteris paribus*, poorer recipients might be more likely to receive more control as donors will reward good governance relative to a recipient's income level. Controlling for income then allows for the quality of governance to be understood relative to the recipient's income level, something seen in the PBA formulas in the chapter three. The *share of a donor's aid budget* that a recipient represents is included as donors are more likely to use a variety of modalities in recipients that receive more of their aid (a larger programme suggests greater capacity to manage projects). In the Knack and Eubank's (2009) framework, this variable is motivated by the reputational stake a donor has in a country, and the likely ability of the recipient to benefit from any resulting institutional improvement.

Alternative Specification – the preceding description details the main specification, chosen partly to maximise coverage. However, an alternative specification is often employed in the research as a robustness check. This alternative specification replaces two variables (*education spending* and *government effectiveness*) with two variables taken from the CPIA

data. The first variable is called *the equity of public resource use* and is chosen to measure alignment. Some explanation is needed, as the name of the variable is perhaps misleading. The variable measures two things in relation to their effect on the poor: government spending and taxation. The specific indicators (IEG, 2009, p. 79) are:

- "Identification of those (individuals, groups, localities) that are poor, vulnerable, or have unequal access to services and opportunities
- Adoption of national development strategy with explicit interventions to assist groups identified above
- Systematic tracking of composition and incidence of public expenditures and their results feed back into subsequent allocations
- Incidence of major taxes (progressive or regressive) and their alignment with poverty reduction priorities"

As such, this variable is a close representation of alignment as conceptualised by the theoretical literature, where it is the percentage of discretionary resources that are allocated to the poor. The second variable from the CPIA is *public sector management*. It is quite highly correlated with *government effectiveness*, but has a narrower focus. Again, it closely resembles the theoretical literatures as it is the efficiency of the government, rather than a broader notion of governance that includes, for example, democratic values. The alternative specification provides a robustness check of any evidence found using the main specification, this has two main sources. First, it is plain that these different variables are measuring slightly different notions of the factors which they represent. If the evidence from the two specifications concur, there is stronger evidence for the conclusions. If they differ, it implies that one is a truer measure of the important factor for the donor. Second, these variables are not used in the main specification as they are only available for more recent years. This limitation forces us to examine the later years more closely, and highlights whether the aid composition decision has evolved over time.

The Dependent Variables

We use three different datasets, each of which provides a set of dependent variables and a different perspective. First, we use the CRS to give information on the use of GBS. Second, we use AiDA which gives data on program and civil society aid. Third, we use data from DFID on four criteria of recipient control. Because of low data coverage, results relating to this

third dataset are included in Appendix D, rather than in the main body of text. All data is by recipient, donor and year.

GBS from the CRS dataset – this OECD dataset is the most complete single source of disaggregated aid, and we use it to extract information on General Budget Support. As donors only give GBS to a fraction of its aid recipients, we estimate a two-stage model (as discussed in the aid allocation chapter). The first dependent variable is then a dummy, where a one denotes that the recipient has received GBS from the given donor within the last three years. A zero denotes that the recipient has received aid from that donor, but not GBS. Countries that have not received any type of aid are not included in the equation, as the decision as to the amount of aid is seen as exogenous. This time smoothing is used to counteract the volatile reporting of individual aid instruments. The data allows a probit to be estimated by donor, with data from 1996-2007 and up to 88 potential recipients. There is a broad choice of donors and the following six were chosen to give a broad indication of different types of donor, both bilateral and multilateral: Japan, Netherlands, UK, USA, EC and IDA.

AiDA – this dataset is a self-reported dataset that collates donor-published information on their own activities. Each individual packet of aid is broken down into a specific category, from which we focus on Civil Society and General Programme Assistance. These variables are transformed so as to measure the percentage of aid that meets a given criterion, e.g. it is given as civil society aid or as general programme assistance. There are observations for around 110 recipients over the period 2002-2007, for seven donors.

SAMPLE

Throughout the analysis, we estimate by donor; a departure from the literature that is partly motivated by a recognition that donors differ. While Knack and Eubank (2009) seek to explain these differences (using the level of development support within the donor country) we do not. Instead, these donor differences are controlled for completely by running separate regressions. Given the disjointed nature of the data available, the choice of which donors and years to include in the sample is normally dictated by the dataset. Because of this the research should be seen as an initial investigation as to the extent of the implementation of pragmatic selectivity (if any) and more generally the factors that

influence the aid modality decision, rather than an extensive cataloguing of all donors' aid composition decisions.

Data Presentation

As the data used herein are rarely used and the reader may not be aware of the basic attributes, some simple descriptive data plots are provided. Figure 5.2 and Figure 5.3 give basic plots of GBS over time. The first is the fraction of aid recipients that are given GBS. The second is the average percentage GBS/aid for those recipients that receive at least some GBS. While both of these are calculated by donor, they are not labelled so as to assist easy interpretation (by reducing unnecessary information). There are three points that are made apparent by these figures. First, donor heterogeneity is large and the decision to estimate by donor is necessary. Second, there is volatility in the use of this instrument, hence the data smoothing. Third, there may be a slight increase in its use over the period, but there is not as much of an increase as we would expect (given the discussion of the instrument).

Figure 5.2: Fraction of Recipients that are Allocated GBS

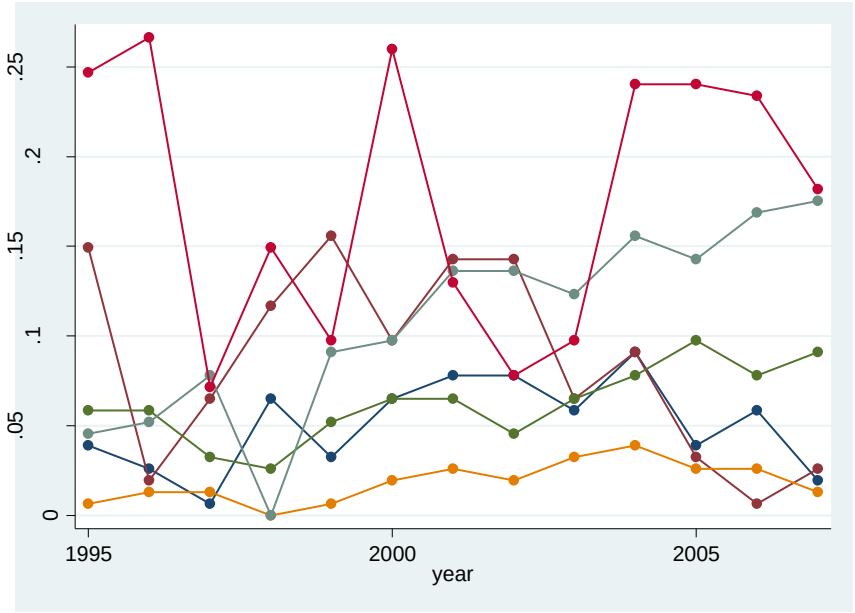
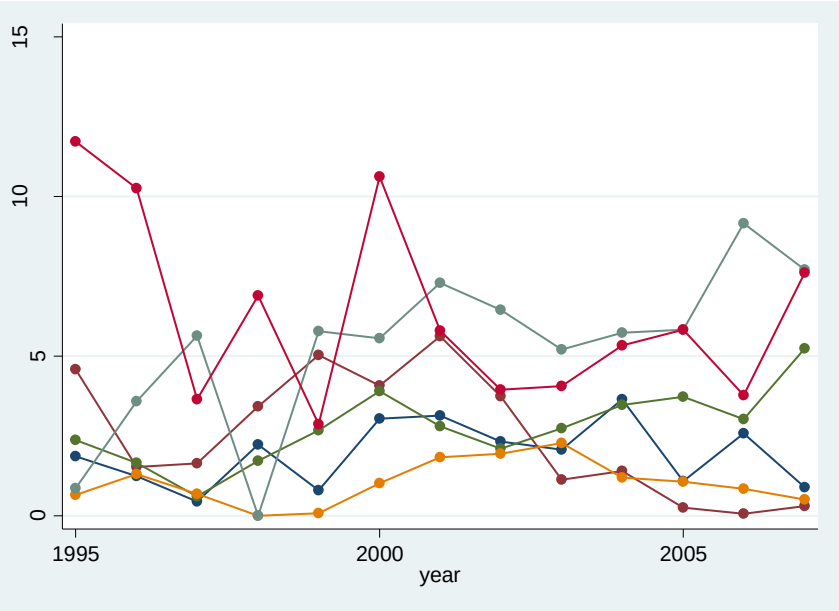
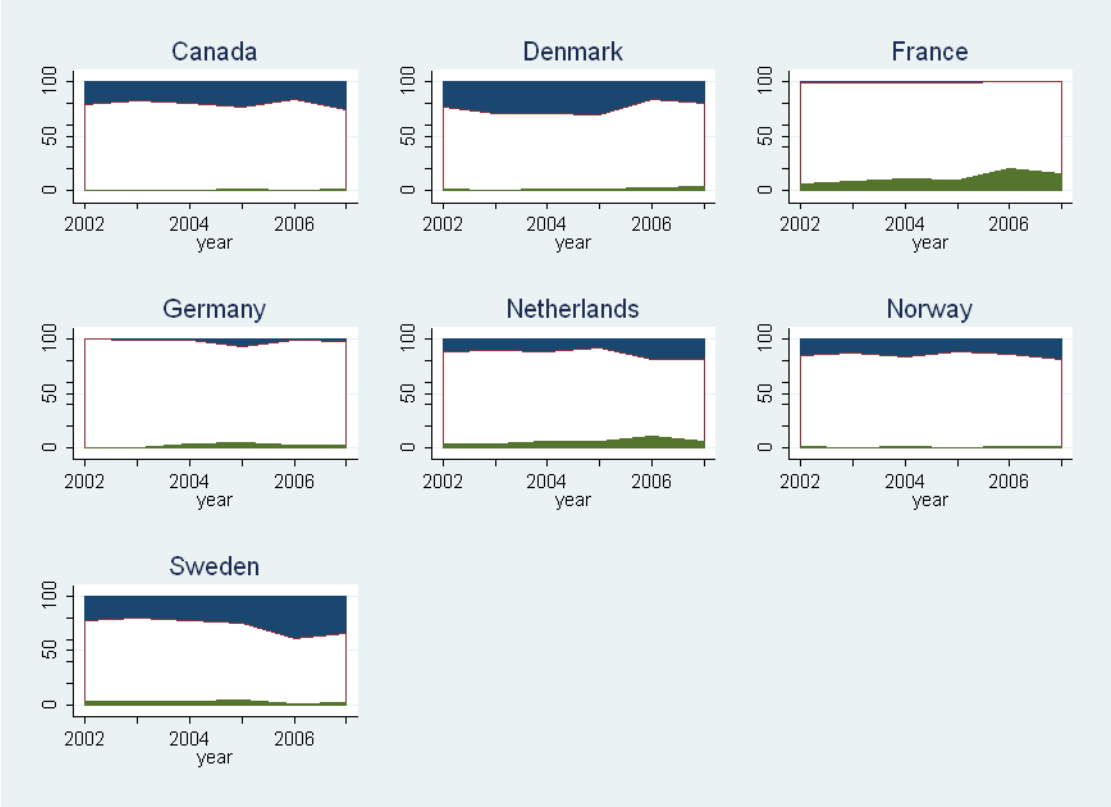


Figure 5.3 Percentage of GBS/Aid



The basic plots of the AiDA data are shown in Figure 5.4. The seven donors for whom reliable data is available are included, over the period for which we estimate. There is obviously great variation between donors. Most obviously, Germany do not give large amounts of aid as either to Civil Society or as General Programme Assistance. France are the only donor who give large amounts (around 20% of their aid) as programme aid, whereas the remaining donors tend to give at least 20% of their aid to civil society organisations. For all donors the six year period is surprisingly consistent, implying no major changes in the use of different instruments.

Figure 5.4 Civil Society and General Programme Aid as % of Aid, 2002-2007



Note: The dark upper area of each graph represents Civil Society/Aid %, the dark lower area of each graph represents Program Aid/Aid %.

5.5 Results

Because of the disparate nature of the results, they are presented briefly in this section, with a longer discussion in the following section taking the results as a whole. The first dependent variable to be used is General Budget Support, as data availability is relatively high, and it represents the extreme of recipient control. A two-step model is used to estimate this, as the problem is similar to that discussed in the aid allocation chapter (clustering at the zero-bound).

Table 5.1 Determinants of GBS recipients, Eligibility Stage 1997-2007

Donor	Japan	Netherlands	UK	USA	EC	IDA
Public spending on education	-0.083**	-0.051	-0.016	-0.15**	-0.033	-0.10***
	(2.16)	(0.81)	(0.35)	(2.01)	(0.85)	(2.65)
PRSP document created	-0.082	0.90***	0.23	0.41	0.78***	1.25***
	(0.49)	(4.01)	(1.46)	(1.33)	(5.25)	(8.63)
Government Effectiveness	0.20	0.53*	0.51	0.58**	0.096	0.26
	(1.06)	(1.69)	(1.50)	(2.20)	(0.56)	(1.51)
Number of Donors	0.041***	0.00051	0.062***	0.030	-0.036***	-0.0022
	(3.40)	(0.03)	(3.18)	(1.26)	(2.93)	(0.22)
Aid/GNI %	0.013	0.016	0.026**	-0.0016	0.029***	0.016**
	(1.57)	(1.50)	(2.25)	(0.13)	(3.26)	(2.30)
GNI per capita (/100)	-0.015***	-0.0050	-0.030**	-0.0013	-0.0073**	-0.0057
	(3.15)	(0.58)	(2.38)	(0.33)	(2.28)	(1.05)
Recipient Share of Donor Aid	0.079*	0.41***	0.12	0.36***	0.39***	0.19***
	(1.71)	(3.87)	(1.46)	(3.18)	(3.44)	(4.63)
Observations	1117	799	955	1042	1058	927
Pseudo R-squared	0.184	0.213	0.316	0.184	0.195	0.300
Mean of Y	0.10	0.12	0.082	0.020	0.21	0.20

*Note: This is the first stage regression, and so is run using a probit with clustered standard errors. T statistics are provided beneath coefficients in brackets, and 10, 5 and 1% significance levels are denoted by ***, ** and * respectively.*

Table 5.1 reports results from a first stage regression over an eleven year period for six major donors. Only recipients that receive some aid from that donor are included, which accounts for the sample size variability. The dependent variable is one if the recipient receives GBS from that donor, and zero otherwise. The first point to notice is actually in the last row, as it reports the fraction of recipients that receive GBS for each donor, e.g. while IDA give at least some GBS for 20% of its aid recipients, the USA does so in only 2% of its recipients. The variability in the utilisation of GBS illustrates the importance of controlling for donor-specific effects, which is done here by estimating them separately. Of the two alignment parameters, public spending on education is always negative and the existence of a PRSP is (almost) always positive (both are significant in three of the six cases). This implies that *PRSP* more fully captures alignment, and that donors have not abandoned the idea that they can influence a recipient's spending patterns. The governance variable is positive for each donor but significant only for the Netherlands and USA. At the eligibility stage then, governance is of real but limited importance. A higher level of aid dependency has a positive relationship with recipient control for each donor, so GBS is more likely to be used in

countries for whom the transaction costs of aid are more prevalent. The results also show that donors are more likely to use GBS if the recipient is poor or they give them a large proportion of their aid budget, as expected.

Table 5.2 Determinants of GBS recipients, Eligibility Stage 2005-2007, Robustness Check

Donor	Japan	Netherlands	UK	USA	EC	IDA
Equality of Public Resource Use	-0.30	-0.077	-0.68	-0.65	-0.43	0.19
	(0.83)	(0.12)	(1.48)	(1.13)	(1.28)	(0.49)
PRSP document created	.	.	-0.81	.	0.95**	0.96***
			(1.21)		(2.24)	(2.63)
Public Sector Management	0.46	2.23***	1.92**	-0.29	0.77*	0.85*
	(0.53)	(3.00)	(2.11)	(0.38)	(1.82)	(1.73)
Number of Donors	0.12***	-0.00085	0.30***	0.22*	-0.044**	0.0040
	(2.73)	(0.02)	(3.83)	(1.94)	(1.96)	(0.19)
Aid/GNI %	-0.017	0.054**	0.046*	0.017	0.055***	0.0015
	(0.65)	(2.19)	(1.67)	(0.99)	(3.25)	(0.13)
GNI per capita (/100)	-0.00021	0.00019	-0.0011	0.000085	0.000022	-0.00021*
	(0.76)	(1.01)	(1.39)	(0.32)	(0.18)	(1.78)
Recipient Share of Donor Aid	0.11	0.64***	0.079*	0.69***	0.70***	0.17*
	(1.22)	(2.68)	(1.67)	(4.00)	(3.25)	(1.92)
Observations	143	115	158	138	183	202
Pseudo R-squared	0.17	0.30	0.47	0.34	0.20	0.239
Mean of Y	0.035	0.30	0.10	0.029	0.40	0.35

*Note: This is the first stage regression, and so is run using a probit with clustered standard errors. T statistics are provided beneath coefficients in brackets, and 10, 5 and 1% significance levels are denoted by ***, ** and * respectively.*

Table 5.2 reports the results from the same regression, but using the alternative specification. This varies a number of things, meaning a similar result would imply robust findings. The first change is obviously in the variables used to measure alignment and governance. The second impact is one of sample coverage – as it restricts the sample to three recent years. Because of this time restriction, we can see that the variable 'PRSP document created' has been dropped for some donors. This is because in later years, for these donors, that variable perfectly predicts the dependent variable. In some cases, in these later years the donor only allocates GBS to recipients that already have a PRSP document in place. As a single variable perfectly predicts the use of GBS, this confirms the importance of alignment. It is reported for the multilateral donors, where it is again found strongly positive and significant. However education spending is found to be irrelevant. These results strongly echo the previous results that *prsp* is a better measure of alignment, as understood by donors. In these results we find that governance is positive and significant

for every donor, apart from Japan and the USA who give low levels of GBS. There are two reasons that might explain why this result differs from earlier findings. First, the variable measuring governance might more closely resemble the important aspect of governance that the donor values. This variable more closely resembles the conceptualisation of governance from the theoretical literature – the technocratic ability of the public sector. Second, it may be that pragmatic selectivity has only come into use in more recent years. A further discussion of this is found below. The coefficients of other variables largely concur with previous findings.

Table 5.3: Determinants of GBS recipients, Eligibility Stage 2005-2007

Donor	Japan	Netherlands	UK	USA	EC	IDA
Public spending on education	-0.20	-0.033	-0.44**	-0.27	-0.048	-0.091
	(1.11)	(0.34)	(2.57)	(1.54)	(0.86)	(1.45)
PRSP document created	-0.29	1.96**	.	-0.51	0.63**	2.22***
	(0.39)	(2.43)	.	(0.74)	(2.02)	(5.93)
Government Effectiveness	-0.40	0.76**	1.30*	1.27**	0.55**	1.20***
	(0.47)	(2.17)	(1.76)	(2.18)	(2.17)	(3.60)
Number of Donors	0.26*	-0.0034	0.30***	0.088	-0.035*	0.012
	(1.86)	(0.09)	(4.01)	(1.43)	(1.89)	(0.47)
Aid/GNI %	-0.0029	0.034**	0.061**	0.028	0.057***	0.0073
	(0.12)	(2.18)	(2.44)	(0.79)	(3.49)	(0.68)
GNI per capita (/100)	-0.00015	0.000077	-0.00076**	-0.00016	-0.000081**	-0.00019*
	(0.85)	(1.13)	(2.01)	(1.12)	(1.99)	(1.90)
Recipient Share of Donor Aid	0.094*	0.51***	0.0067	0.62***	0.43**	0.17*
	(1.73)	(3.56)	(0.20)	(3.11)	(2.47)	(1.92)
Observations	317	208	127	307	298	259
Pseudo R-squared	0.395	0.282	0.434	0.329	0.271	0.379
Mean of Y	0.025	0.18	0.11	0.023	0.27	0.27

*Note: This is the first stage regression, and so is run using a probit with clustered standard errors. T statistics are provided beneath coefficients in brackets, and 10, 5 and 1% significance levels are denoted by ***, ** and * respectively.*

In order to investigate whether the positive relationship between governance and the use of GBS is found because of a change in the sample (a time effect) or a specification change, Table 5.3 reports the results from the first stage regression using the main specification with a restricted time period. These results then differ from those in Table 5.2 in the specification but not the years observed. Because of the greater coverage of the main specification, Table 5.3 does still have more observations. The non-governance coefficients do not vary considerably from previous findings. However, the governance variables are found to be

positive and significant for every donor (apart from Japan, which gives GBS to very few recipients). These results then show that governance is a major factor in deciding which aid recipients of these major donors receive GBS. These three sets of results lead us to conclude that the efficiency of the government in the aid composition decision is important, and has become increasingly important in recent years. *PRSP* is also important, as are some transaction cost concerns, income level and the importance to the donor.

Table 5.4: Determinants of GBS recipients, Level Stage 1997-2007

Donor	Japan	Netherlands	UK	USA	EC	IDA
Public spending on education	0.88	0.35	-1.88	-9.32***	0.12	0.89
	(0.56)	(0.15)	(0.71)	(3.71)	(0.11)	(0.82)
PRSP document created	0.0063	-6.65	5.30	0.46	-8.82*	-1.23
	(0.00)	(0.96)	(0.58)	(0.07)	(1.73)	(0.16)
Government Effectiveness	-2.43	14.0*	21.8**	31.1*	14.1***	-6.86
	(0.25)	(1.87)	(2.30)	(2.09)	(2.70)	(1.07)
Number of Donors	-1.28*	-0.090	-0.78	-1.11	-0.28	-2.14***
	(2.00)	(0.11)	(0.70)	(1.66)	(0.80)	(4.52)
Aid/GNI %	0.42	0.55	-0.066	-0.32	0.17	0.53***
	(1.21)	(1.08)	(0.18)	(0.36)	(0.78)	(2.89)
GNI per capita (/100)	-0.0035	0.31	-0.73	0.79***	-0.27**	0.00038
	(0.02)	(1.61)	(1.08)	(3.51)	(2.39)	(0.24)
Recipient Share of Donor Aid	-1.41**	-7.50**	-6.65**	10.00***	5.01	0.56
	(2.46)	(2.33)	(2.58)	(3.44)	(1.33)	(0.35)
Observations	112	98	78	21	221	188
R-squared	0.217	0.188	0.127	0.918	0.088	0.202
Mean of Y	28.1	46.8	38.7	39.4	34.3	49.1

*Note: This is the second stage regression, and so is run using OLS with clustered standard errors. T statistics are provided beneath coefficients in brackets, and 10, 5 and 1% significance levels are denoted by ***, ** and * respectively.*

Table 5.4 reports the second stage regression using the main specification, where the dependent variable is the percentage of aid from a given donor to a recipient that is GBS. The starting place for the discussion of this table is the final column, which shows the donor's average level of GBS use. Because the USA uses GBS so infrequently, the sample size is so small that its results are fragile and mostly not discussed, but included for completeness. A striking point is that the average level of GBS is quite high, when it is decided that it will be used. Unlike at the eligibility stage, alignment is not a major factor for donors when deciding the amount of GBS. Government effectiveness is positive and significant for four donors, with a sizeable coefficient. Government effectiveness' range is -2.5 to 2.5, and so a 1 unit increase resulting in a 20% increase in GBS/Aid is a sizeable effect.

Japan and the IDA do not exhibit any positive relationship between governance and GBS/Aid however. Unfortunately, due to sample size it is not informative to run the regression with the alternative specification. However the first stage regressions do suggest that the importance of governance has grown over time. There are no clear cross-donor patterns regarding the relationship between the level of GBS and either the variables representing transaction costs, income or the recipient share of donor aid.

AIDA DATABASE

We now turn to analysis that uses the dependent variables from the AIDA database, as detailed earlier in this chapter. The major advantage of this data is that it gives two dependent variables that are broader in definition than GBS, and are from opposite ends of the recipient control spectrum, allowing us to test from both sides. Unfortunately, data availability severely restricts the number of tests we can perform on the hypothesis. A maximum of seven donors are chosen on the basis of size and data coverage. While these donors do not include the USA, UK or Japan, they do include some of the Nordic+ donors as well as France and Germany. The former group are seen as being more pro-poor than other donors (as discussed in the aid allocation chapter), and as AIDA data is self-reported it is likely that the data is more representative of this pro-poor, pro-transparency group of donors. As such, this cannot be seen as a test of whether pragmatic selectivity is used by all donors, but rather whether it is used by this sub-set. Table 5.5 reports the results from the first regression, where the dependent variable is the percentage of aid that is program aid.

Table 5.5 Program Aid, 2002-2006

	Canada	Denmark	France	Germany	Netherlands	Norway	Sweden
% Education spending/GDP	-0.00086	0.034	0.071	0.045	-0.055*	-0.42***	-0.11**
	(0.01)	(0.57)	(1.49)	(1.21)	(1.76)	(3.77)	(2.20)
PRSP document created	-0.092	0.10	0.47**	-0.50***	0.56***	0.23	0.30*
	(0.34)	(0.41)	(2.37)	(2.74)	(4.26)	(0.98)	(1.85)
Government Effectiveness	0.93***	0.87**	-0.27	0.53***	0.60***	0.55	1.19***
	(2.96)	(2.26)	(1.12)	(2.67)	(3.94)	(1.40)	(5.30)
Number of Donors	0.010	0.064**	-0.0027	0.062***	0.0080	0.27***	0.0036
	(0.59)	(2.14)	(0.19)	(3.74)	(0.89)	(5.77)	(0.21)
Aid/GNI %	0.012	0.0043	0.030***	0.030***	0.019***	0.057***	0.039***
	(0.86)	(0.29)	(2.60)	(3.14)	(2.61)	(3.95)	(4.43)
GNI pc/100	-0.015**	-0.030**	-0.011*	-0.016***	-0.012***	-0.025*	-0.036***
	(2.08)	(2.35)	(1.74)	(3.40)	(3.49)	(1.73)	(4.60)
Observations	970	452	336	726	944	917	817
R-squared	0.092	0.124	0.169	0.164	0.142	0.449	0.289
chi2	15.7	20.4	51.7	74.0	101.3	124.6	144.9
Mean of Y	1.02	1.89	9.82	4.23	6.26	0.76	4.49

*Note: Standard OLS used. T statistics are provided beneath coefficients in brackets, and 10, 5 and 1% significance levels are denoted by ***, ** and * respectively.*

Despite the relative similarity of donors, Table 5.5 reports a large difference in the employment of program aid, from 2% to 17%. On alignment there is no consistent cross-donor picture, but as in previous results, there is a negative coefficient for education spending. While the picture is somewhat muddied by the time trend (as a higher number of recipients have agreed a PRSP in later years), there is evidence that alignment (as measured by the *prsp* dummy) is important for some donors (but not for Germany). Government effectiveness is positive and very significant for five of the six donors, duplicating previous results. Familiar findings are also found for the transaction cost and income variables – donors give recipients more of their aid as program aid if the recipient is poorer, has a higher degree of aid/GNI or a larger number of donors. These results allow us to be more confident of the relationship with the *level* of a high-recipient control type of aid (having been restricted in our analysis on the level of GBS), and the consistent picture is reassuring.

Table 5.6 Program Aid 2005-2007, robustness check

	Canada	Denmark	France	Germany	Neth.	Norway	Sweden
Equality of Public Resource Use	-1.46		-0.91**	1.45***	0.38	-0.18	0.54
	(1.03)		(2.15)	(3.25)	(1.06)	(0.37)	(1.16)
PRSP document created	-0.85		0.33	-0.46	-0.080	0.062	-0.37
	(1.11)		(0.89)	(1.35)	(0.28)	(0.14)	(0.98)
Public Sector Management	5.62**		1.35**	0.72	1.79***	0.85	1.17*
	(2.01)		(2.40)	(1.08)	(3.06)	(0.92)	(1.72)
Number of Donors	0.14		-0.0081	0.12**	0.028	0.36***	-0.0089
	(1.29)		(0.26)	(2.35)	(0.74)	(2.75)	(0.18)
Aid/GNI %	0.18**		0.11**	0.050**	0.047**	0.089***	0.030
	(2.16)		(2.48)	(2.13)	(2.36)	(2.95)	(1.22)
GNI pc/100	0.050		-0.019	-0.0084	0.0048	0.043*	-0.037
	(1.12)		(0.96)	(0.49)	(0.33)	(1.65)	(1.61)
Observations	145	87	86	172	137	158	140
R-squared	0.397		0.293	0.390	0.255	0.352	0.225
chi2	14.5		32.6	54.2	40.2	29.9	18.4
Mean of Y	1.94	4.54	20.5	6.56	12.6	1.86	4.87

*Note: Standard OLS used. T statistics are provided beneath coefficients in brackets, and 10, 5 and 1% significance levels are denoted by ***, ** and * respectively. Denmark was omitted due to insufficient variation, in essence the independent variables predicted the dependent variable too well.*

Table 5.6 reports the results of the same regression, when using the alternate specification. Data availability demands a change of time period, with a vastly reduced number of observations. Also, it becomes impossible to estimate for Denmark. The role of alignment is found to again be somewhat erratic across donors, as for four donors the coefficient for the first variable is insignificant and the remaining two have opposing signs. The role of the PRSP in the later time period is again found to be minimal. All donors exhibit signs of using pragmatic selectivity, with four of the donors' having a significant coefficient for the governance variable, and it is positive for all five. The other variables are as expected, except for the income effect which is largely insignificant. These results show that when the broader definition of program aid is used, the phenomenon of a positive relationship between governance and recipient control of aid holds. Alignment, Transaction costs and poverty are found to be important, but evidence for the latter two is not universal.

Table 5.7 Civil Society 2002-2006, Probit

	Canada	Denmark	France	Germany	Neth.	Norway	Sweden
% Education spending/GDP	-0.027	0.015		0.070**	-0.025	-0.047**	-0.10***
	(1.30)	(0.50)		(2.28)	(1.21)	(2.13)	(4.22)
PRSP document created	0.070	0.30*		-0.25	0.051	-0.077	0.42***
	(0.60)	(1.94)		(1.43)	(0.42)	(0.59)	(2.82)
Government Effectiveness	-0.055	0.35**		-0.64***	0.23**	-0.19*	-0.046
	(0.58)	(2.23)		(4.44)	(2.54)	(1.84)	(0.44)
Number of Donors	0.037***	0.048***		0.074***	0.018***	0.12***	0.0053
	(7.39)	(3.48)		(6.46)	(3.38)	(11.69)	(1.00)
Aid/GNI %	-0.014**	0.014*		-0.018*	0.0027	0.0037	-0.010
	(2.36)	(1.67)		(1.69)	(0.44)	(0.56)	(1.59)
GNI pc/100	0.0045***	-0.0019		0.011***	-0.0023	0.0055***	-0.0016
	(2.59)	(0.59)		(4.95)	(1.45)	(2.62)	(0.85)
Observations	970	452	234	726	944	917	817
Pseudo R-squared	0.055	0.046		0.123	0.027	0.168	0.046
chi2	73.3	28.6		73.4	33.6	193.7	42.9
Mean of Y	17.3	18.9	0.48	1.73	10.9	15.1	25.1

*Note: Standard OLS used. T statistics are provided beneath coefficients in brackets, and 10, 5 and 1% significance levels are denoted by ***, ** and * respectively. France was omitted due to insufficient variation (the mean of Y is 0.48).*

Table 5.7 shows a different test of the hypotheses, as it reports regressions where the dependent variable is the percentage of aid that is given to civil society. We can see that this type of aid is much more prevalent than program aid in the data, but that there is variation by donor. As this is at the extreme of low recipient control, we would expect many of the coefficients to be of the opposite sign to those found previously. The alignment variables are not consistent across-donors. We also find a certain symmetrical assortment of results for the governance variable as two of the six donors estimated are insignificant, significant and positive, and significant and negative respectively. The number of donors is found positive and generally significant, with mixed results for aid/GNI. Program aid (as a percentage of aid) has a positive relationship with income, mirroring earlier results.

Table 5.8 Civil Society 2005-2007, Robustness check

	Canada	Denmark	France	Germany	Neth.	Norway	Sweden
Equality of Public Resource Use	0.24	-0.88**		0.71**	0.46	0.14	0.29
	(0.74)	(2.11)		(2.37)	(1.26)	(0.41)	(0.71)
PRSP document created	-0.51**	0.070		-0.33	-0.68**	-0.10	0.39
	(2.01)	(0.20)		(1.32)	(2.49)	(0.37)	(1.23)
Public Sector Management	-0.16	1.41**		-0.20	-0.15	-0.16	-0.92*
	(0.38)	(2.56)		(0.53)	(0.36)	(0.40)	(1.82)
Number of Donors	0.11***	0.037		0.034	0.17***	0.17***	0.15***
	(4.03)	(0.94)		(1.59)	(4.80)	(5.48)	(4.21)
Aid/GNI %	-0.025	-0.025		-0.039**	0.0061	0.0045	0.0028
	(1.62)	(1.37)		(2.23)	(0.38)	(0.26)	(0.16)
GNI pc/100	0.012	-0.038**		-0.020*	0.0034	-0.000046	0.042***
	(0.95)	(2.18)		(1.71)	(0.27)	(0.00)	(2.62)
Observations	145	87	56	172	137	158	140
Pseudo R-squared	0.214	0.148		0.115	0.216	0.224	0.218
chi2	40.7	16.9		23.1	40.2	42.7	28.0
Mean of Y	20.9	16.4	0.33	3.41	9.40	13.3	25.5

*Note: Standard OLS used. T statistics are provided beneath coefficients in brackets, and 10, 5 and 1% significance levels are denoted by ***, ** and * respectively. France was omitted due to insufficient variation (the mean of Y is 0.33).*

Table 5.8 reports the results from the same regression, but using the alternate specification. The mixed evidence on alignment parameters persist. However, the PRSP coefficients are negative for four donors and significantly so for two of these. This mirrors the evidence that recipients that have agreed a PRSP receive higher amounts of program aid and lower amounts of civil society aid. Coefficients on the governance variable are inconclusive, but negative for all but one donor. This mirroring is consistent with the expected behaviour of pragmatic selectivity. A similar relationship is found for the transaction costs and income variables.

5.6 Discussion

The theoretical literature has tended to focus on the importance of preference (mis)alignment in the aid composition decision. This may in part be to the ease to which preference alignment lends itself to being modelled using the principal agent framework. While we acknowledge and confirm the role of alignment, we have presented a simple model that brings out more fully the role of relative efficiency. One of the advantages of this model is that it does not rely unduly on assumptions of full commitment and enforceable conditionality. This takes seriously the point made by Hagen (2006a), that any commitments are neither self-enforcing nor subject to external authority. It has been argued that conditionality in the case of non-alignment is ineffectual (White and Morrissey, 1998) and in

the case of high alignment it is clearly superfluous. The related issue of aid fungibility (resulting from preference misalignment) has been called a "red herring" (McGillivray and Morrissey, 2000), which makes the focus on alignment, conditionality and fungibility in the theoretical literature disconcerting. The model presented here, while simple, acknowledges the role of preference alignment, but adds to that a more substantial role for efficiency.

While the literature has generally assumed that no aid is the relevant comparison for any given modality, for a number of reasons we have modelled project aid as the default modality. It is clear from aid proliferation that donors face incentives to maximise the number of countries and sectors in which they work (Knack and Rahman, 2007), which results in a large amount of project aid being disbursed. The bureaucratic incentives that donors themselves face (Easterly, 2002) lead to a mentality that aid should be disbursed, with less of a focus upon the impact. The critique of 'no aid' forming the appropriate reservation utility of the poor can then be expressed using the ideas of 'warm glow', the Samaritan's Dilemma and/or bureaucratic incentives. Each of these arguments point to a situation where withholding aid is not a credible threat for the donor, and so the fact that the model presented earlier does not include this is a real advantage. The bureaucratic incentives mentioned here, and the model itself, confirm that project aid is the most likely default aid modality.

When discussing the role of efficiency, it is obvious that the efficiency of GBS will be dictated by the recipient government. The less obvious role is that of the efficiency of aid that is more under the control of the donor. It is perhaps because the efficiency of this type of aid is opaque that models have tended to underemphasise its role. The empirical section takes various measures that capture the likely cause of transaction costs as determining this type of aid efficiency. It may initially seem that the efficiency of donor-implemented aid does not vary within a donor. However, the use of these measures of transaction costs show how the efficiency of donor-implemented aid may vary across recipients within a single donor.

The econometric results include estimations for a combined total of twelve donors (including bilateral and multilateral), three sets of dependent variables, two estimation techniques, two specifications and various time periods. While the disparate nature of the econometric evidence means there has been an extensive test of the theory, it also leads to a somewhat disjointed presentation of the results. However, synthesising these into a coherent picture is reasonable because of the consistent nature of the results. The results concerning GBS

focused on six large donors that have varied greatly in the use of GBS. Looking at the first stage (deciding GBS recipients) we found that alignment (as measured by PRSP, but not education) is important. This result held for both specifications and time periods, although negative coefficients on education spending became insignificant when only the later period was focused upon. In conjunction with the estimates regarding the level of GBS, they suggest that donors are not discouraged from allocating GBS to recipients who have not demonstrated preference alignment, but that they may only give small amounts of GBS for those recipients.

The results for governance are more uniformly in keeping with the model predictions. The following is found in each of the regressions using GBS: recipients with higher levels of governance are more likely to receive GBS and the GBS they do receive is likely to be substantially higher. Both variables that measure governance (in the narrower and broader senses) are found to be significant for most donors, but the effect is found to be stronger on average in the 2005-2007 period than in the 1997-2007 period. This means donors are increasingly using the rationale of pragmatic selectivity and responding to different governance levels with different aid modalities. The role of transaction costs is mixed. For most donors and estimations, the transaction cost measures are positively correlated with recipients of GBS at the first (eligibility) stage, a relationship that is often found to be significant. The EC is a consistent exception to this, with the number of donors found to have a significant and negative effect on the use of GBS. This may be capturing the role of the EC in focusing on 'aid orphans', which would exhibit the same behaviour as captured here. The EC excluded, donors do show signs of using GBS in countries where donor-implemented aid is likely to be less efficient, in line with the model's predictions.

The second set of dependent variables necessitated a change of donors and time period. Because earlier results indicate (albeit for a different group of donors) that the implementation of pragmatic selectivity has increased over time, we could expect stronger results as data is only available for more recent years. The change of donors to include more donors from the Nordic+ grouping would also confirm this expectation. The evidence for the role of alignment for this group of donors is not unvarying. Germany is the real exception to the earlier pattern of positive coefficients on PRSP, as a negative coefficient is found for PRSP using both specifications. However, the other donors generally conform to previous findings. The strongest evidence for this pattern is the mirroring found in coefficients when

using *civil society* as the dependent variable. Because this is the one variable that measures the amount of aid at the donor-implemented end of the aid control spectrum, findings of coefficients of the opposite sign to other results are good evidence of the predicted relationship.

The results for governance variables conform to the theoretical predictions for almost every donor in each of the four specifications. They are found to be positive (for all but one donor) and mostly significant in regressions with *program aid*. When switching to use *civil society* they are only found to be positive (i.e. not mirroring the result, and at odds with the predictions) in three cases. These results show that even under a stringent test of the prediction (i.e. testing from both sides with different specifications for different donors and time periods) there are consistent signs that pragmatic selectivity is practiced. The role of transaction costs is somewhat more ambiguous, as positive results are found for the number of donors in both regressions based on *program aid* and *civil society*. Coefficients on the level of aid reveal a more consistent picture, with some amount of mirroring.

The DFID results, reported in Appendix D, confirm the importance of alignment, but also the primacy of efficiency over alignment. In each regression and specification, we see a positive relationship between government efficiency and the measure of recipient control of aid. In six of the eight results, it is also significant. The different levels of significance also support the hypothesis as the strongest evidence is found for the dependent variables with the highest recipient control. Coefficients for the *number of donors* also show transaction costs are an important concern, and so relative efficiency appears to be a suitable way of understanding the mechanism at work here. One control is common to each of the regressions: the income level of the recipient. We find consistent evidence that donors are more likely to cede control to poorer recipients. This result is best understood by acknowledging that this result is found assuming ceteris paribus. As such, we can think of donors giving more control to recipients that have efficient governments *relative to their income level*. An alternate explanation is that income is partly capturing the efficiency of donor-implemented aid (i.e. donor-implemented is less efficient in poorer countries). These two arguments are reinforcing – in each case poorer recipients are rewarded for having a *relatively* efficient government.

5.7 CONCLUSION

This chapter has presented both a theoretical model and an empirical investigation of the aid composition decision. Both of these have highlighted the importance of the relative efficiency of different aid modalities. The econometric results have also shown that alignment is important, and that while data is problematic the PRSP variable does capture alignment at least partially. This also suggests donors have not abandoned the idea that they can influence recipients and their spending plans, a point for which there is some, if limited, evidence (McGillivray and Morrissey, 2000; Mosley *et al.*, 2004). The theoretical model presented here is perhaps more appropriate than previous models as the important point is the expected risk that funds will be reallocated, rather than pre-existing spending patterns. Results from the alternate specification, which includes a variable that is very close to previous theoretical conceptualisations of alignment (the degree to which government resources are used in a pro-poor fashion), calls into question the important role alignment was previously afforded as evidence for the role of alignment is mixed, and weaker than that for efficiency. This chapter has argued that the crucial consideration of donors is not preference alignment (although this is found to be important), but relative efficiency, an argument that agrees with the only known empirical research on the subject.

There is only one paper that we are aware of that has previously worked on the aid composition decision (Knack and Eubank, 2009), and so this research takes place in a relative vacuum of empirical work. Our analysis differs from that paper in a number of ways, most importantly by estimating for different donors separately, and our use of three different datasets which greatly expands the available tests. Our results agree with those of Knack and Eubank (2009), that better government efficiency is related to the amount of control ceded to the recipient. While their results focus on the differences between donors, we find evidence of pragmatic selectivity for many of the donors analysed and donor heterogeneity that is more appropriately controlled for by estimating for donors separately. This is something that the data they use does not readily allow for, as the number of observations for each donor is quite small. Estimating by donor allows us to highlight the low uptake of GBS by the USA and Japan, and other interesting cross-donor differences. Another limitation of the SMPD data (used by Knack and Eubank, 2009) is that they can only estimate for one year (2008) which means they cannot examine any changes over time.

The most salient empirical contribution of this chapter lies in the exploitation of various datasets, which affords a more extensive exploration of the aid modality choice. We have examined the largest donors as well as some of those from the Nordic+ group. While we have found heterogeneity, we find evidence of pragmatic selectivity for most donors. Because of the data we have been able to examine a longer time period, and found that evidence of pragmatic selectivity has become stronger in recent years. We have also looked for signs of pragmatic selectivity for various different indicators of recipient control. These have been narrow (GBS), broad (Programme Aid) and at lower end of recipient control (Civil Society). These dependent variables have provided surprisingly consistent evidence of the primacy of efficiency over alignment. This chapter provides the definition of an alternate aid allocation strategy, and discusses this using policy documents, a theoretical model and econometric results. While this research is not exhaustive and severely hampered by data availability, it constitutes the most complete exploration of the aid composition decision of which we are aware.

It is now well established that donors face incentives to disperse aid, a situation variously described using the Samaritan's Dilemma, 'warm glow' and bureaucratic incentives. The previous chapter confirmed that on average different levels of recipient governance have not been met with different levels of aid. The most popularly proposed solution to the Samaritan's Dilemma is to allocate more aid funds through multilateral agencies, something that is not practised. Furthermore, the success of this suggestion is doubtful as multilateral agencies themselves show signs of inequality aversion and lacking commitment technology. This chapter has identified a different solution to the Samaritan's Dilemma: pragmatic selectivity. Pragmatic selectivity satisfies the Samaritan impulse by allowing aid levels to be dictated by poverty (as well as proximity and population), but meets the desire for efficiency by allowing different levels of governance to be met with different types of aid. This chapter does not propose a new strategy; rather it identifies one currently in practice. Indeed, the importance of the aid composition decision is easily identified in donors' policy documents; it has only been a 'forgotten policy lever' among the academic community.

6. CONCLUSION

Development aid has existed in its modern incarnation for around sixty years. Over that time many different ways of giving aid have been tried, and many more suggested. Countless allocation decisions have been made by donors seeking to strike a balance between competing priorities, be they good or ill. The recipients and donors have fallen into common patterns of behaviour. Public interest has fluctuated, donor priorities have changed and aid agencies have multiplied. The effects of aid have been diverse, and new impacts have been identified. But amid this changing landscape there remains a constant question: could this be done better? We cannot observe the counterfactual (what would have happened without aid) but we can observe the task ahead, the most commonly cited goal of aid being poverty reduction. While there has been some recent debate over the level of poverty in Africa (Sala-i-Martin and Pinkovskiy, 2010), it is clear that the task of poverty reduction remains considerable. This thesis has engaged with some of the potential unintended consequences of aid, as well as the reasons behind the allocation of aid. I will now detail eight specific contributions contained herein.

The first contribution made by this thesis is related to the general question of aid and governance. Specifically, the second chapter reported evidence that aid does *not* systematically reduce tax revenue. A concern when giving aid is that it may undermine governance within the recipient country by breaking the citizen-state link established by taxation (Bräutigam and Knack, 2004; Knack, 2001; Moore, 2007; Ross, 2004). The chapter does not deal directly with governance, but focuses instead on one part of the proposed causality chain: foreign aid's effect on domestic tax revenue. The chapter reports evidence that the detrimental effect of foreign aid found in other research is not replicated, other than for aid grants before 1985. The empirical approach also deals with endogeneity problems more adequately than previous research, by allowing for longer term effects between aid and tax. When looking over the longer term, aid has a positive effect on taxation. As such, the potentially detrimental link from aid to governance, at least through the most commonly mentioned channel, is questioned.

The second contribution is to the debate over the relative merits of aid loans and grants. While there are a number of considerations, the differential tax effect of grants and loans found by Gupta et al. (2004) is found to be key. Chapter two reports evidence that this

differential effect is only found in the early years of the sample: since 1985 grants have not had a detrimental effect on tax revenue. The longer-term perspective also reveals grants to have had a positive effect, possibly by controlling for the endogeneity of the allocation decision. This result is an important one as the decision to choose between the two types of aid appears to hinge on such concerns. The potentially harmful future repayment obligations implied by aid loans do not need to be chosen because of fears over governance.

The third contribution of the thesis is to collate the various perspectives and research on the allocation of aid. These include the approach of starting from an ideal allocation principle, the formulas that donors produce to guide their own allocations and econometric investigations of the importance of different factors in actual allocations. Each of the different approaches reveal the inherent tensions between different views of development aid. As the different perspectives are presented alongside each other, the reader can identify some recurring themes: for example, the relative importance placed on need and effort and the difficulty in separating the two. This tension is made apparent in the decisions of Llavador and Roemer (2001) and Cogneau and Naudet's (2007), when they try to decide which variables belong to the category of *conditions* (with which aid allocation should have a negative relationship) and which to *effort* (with which aid allocation should have a positive relationship). The demonstrated difficulty in identifying and separating *effort* and *conditions* hints at a further reason why donors find it difficult to allocate on the basis of policy (discussed below, and in chapter four).

The fourth contribution is the recognition that aid volume is not the only valuable unit of analysis. Much as the goal of aid has been recognised as important (Clemens *et al.*, 2004), its composition is considered here. The composition of aid is examined in detail in chapters two and five, and broader considerations concerning the way aid is given are discussed in chapter three. The excessive focus on aid-growth regressions has emphasised the volume of aid as the salient piece of information, allowing the composition of aid to be relatively unexamined. By focusing on the type of aid and how it is given, the thesis has contributed to the understanding of the relative merits of grant and loans, as well as how aid composition is used by donors to maximise efficiency.

The fifth contribution is the finding of large donor heterogeneity in aid allocation practice. Previous research has estimated 'average donor behaviour' as a non-weighted average of different donors. While this is a valid approach for some questions, it can be misleading if

smaller donors unduly obscure the behaviour of the larger donors that represent the vast majority of aid. Chapter four presented evidence that donor heterogeneity is persistent, and while there have been changes over the 25 year period, they have been relatively small. The persistence of this heterogeneity has implications for the policy community in trying to decrease fragmentation. If donor's preferences are distinct and change only slowly over time, efforts to harmonise their activities will be necessarily limited in their success.

The sixth contribution is to distinguish donor interest from the proximity of a recipient to a donor when analysing aid allocation. Because of the concerns over fragmentation detailed in chapter three, we can recognise that focusing on recipients that are geographically, linguistically or culturally proximate may indeed be *altruistic* behaviour. The false dichotomy between donor interest and recipient need may also be misleading if donors simultaneously pursue multiple objectives, evidence of which is reported in chapter four.

The seventh contribution is that in contrast to findings of its increasing use reported elsewhere (Berthélemy and Tichit, 2004; Claessens *et al.*, 2009; Dollar and Levin, 2006), chapter four finds that policy selectivity has not been practised by seven major donors over the 25 years examined. This result concurs with a number of other papers (Easterly, 2007; Hout, 2007a, b; Nunnenkamp and Thiele, 2006). This informs our expectations of aid's effect. Whereas some donors (especially Sweden) clearly allocate aid on the basis of poverty, others (the USA and France) are much more influenced by other factors. The chapter also reports evidence that ex post conditionality has failed to be implemented.

The eighth contribution is the identification of an alternative allocation strategy: pragmatic selectivity. This allocation strategy is neither proposed nor argued for here, rather it is found in both the policy papers of donors and their actions. While the econometric investigation is necessarily limited by data availability, a rather consistent picture is found: aid composition is influenced by preference alignment and efficiency concerns. In a context of (ex post and ex ante) conditionality failure, pragmatic selectivity is an alternative strategy that meets both the Samaritan and efficiency impulses. While it may be sub-optimal behaviour, the important point is that this behaviour has not been widely recognised and thus insufficiently studied.

While the thesis has made contributions to the literature, it also leaves untouched areas where our current understanding is inadequate and so points to avenues for further

research. First, the most obvious of these is further exploration of pragmatic selectivity. Given that the research question is novel this is unsurprising. While data availability clearly sets the limits in which new empirical research can operate, new datasets are being made available and some of these may allow for more extensive tests to be performed. While chapter five represents an initial investigation of the question, a dataset covering more years, donors and types of aid would enable the strength and breadth of any conclusions to be improved upon.

Second, while chapter four introduced a number of different econometric techniques that have been used within the aid allocation context, a true understanding of how the use of the different estimators might affect the results is not available. Future research could not only add to our understanding on conditions under which the different estimators are consistent, but also help explain the divergent findings when using the different estimators. In the aid allocation context this would be particularly valuable given the variation in the econometric approach (including specification, sample, years, donors, pooling decisions and variables used).

Third, while chapter five discusses the idea that pragmatic selectivity in some sense 'solves' the Samaritan's Dilemma, a theoretical modelling of this seems more appropriate. This would formalise the different explanations as to why conditionality and selectivity are not implemented, but pragmatic selectivity is.

Within the context of large questions, the thesis has identified specific gaps within the literature and focused on these in detail. Paradoxically, some broader points have been made as a result of this focused methodology. For example, the thesis has been motivated by a desire to contribute to the large question regarding the effects of aid. Because of the link between taxation and governance, chapter two focused on the specific question of whether aid grants and loans have differential tax effects. Econometrically, the discovery of a break in the data and a longer-term focus meant both types of aid are found to have a positive effect on tax (since 1985). This result informs our understanding of a commonly discussed channel from aid through taxation to governance, and is optimistic about aid's effect. The improved methodology used in chapter four reveals donors that are heterogeneous, but remarkably consistent within themselves. This has major implications for those wishing to change donor behaviour. It also limits any expectation of the impact of aid, as several large donors allocate aid on the basis of non-development concerns. Chapter

five identifies pragmatic selectivity as a new allocation strategy, utilising new datasets to confirm that donors use this strategy.

Thus while aid is not homogenous in goal, composition or effect, this heterogeneity does not mean that it is so complex as to be inexplicable. To the contrary, this thesis contains answers to specific questions, and thereby provides evidence towards a broader understanding of the effects of aid.

Appendix A

Appendices A, C and D refer to chapters 2, 4 and 5 respectively. They each report a description of the data used first, followed by any additional tables or results. Therefore, this appendix starts by describing the data used in chapter 2. To extent the Gupta *et al.* (2004) data from 1970-2000 up to 2005, we are sometimes forced to use slightly different sources, and utilise these for the entire sample to ensure comparability. Agriculture, Industry, Imports, Exports and GDP per Capita data are from the World Development Indicators of the World Bank, Population (to derive GDP per capita) is from the UN, Aid is from the OECD and Tax Revenue is from the IMF Government Finance Statistics. Details for each variable are provided below.

Net Aid (OECD) - total Official Development Assistance (ODA) including Technical Co-operation but excluding grants, loans and credits for military purposes. Data reported in Current US$ is converted to share of GDP.

Net Loans (OECD) - ODA loans with maturities of over one year and meeting the criteria set out for ODA, extended by governments or official agencies, and for which repayment is required in convertible currencies or in kind. The net data are reported after deduction of amortisation payments and the impact of other measures reducing debt (e.g. forgiveness). Data reported in Current US$ is converted to share of GDP.

Grants (OECD) - ODA transfers, in money or in kind, for which no repayment is required. Data reported in Current US$ is converted to share of GDP.

Tax Revenue as % of GDP (IMF Government Finance Statistics) - Consolidated Central Government. It comprises compulsory, unrequited, non-repayable receipts for public purposes collected by central governments. This is always included in the analysis as logged, multiplied by 100, to ease interpretation.

Income, GDP per Capita (Current US $) (WDI, World Bank) - derived by first converting GDP in national currency to US dollars and then dividing by total population. For ease of interpretation this was then multiplied by 100.

Agricultural Value Added as % of GDP (WDI, World Bank) - measures the output of the agricultural sector less the value of intermediate inputs. Agriculture comprises value added from forestry, hunting, and fishing as well as cultivation of crops and livestock production.

Industrial Value Added as % of GDP (WDI, World Bank) - comprises value added in mining, manufacturing, construction, electricity, water, and gas. Value added is the net out put of a sector after adding up all outputs and subtracting intermediate inputs.

Imports of Goods and Services as % of GDP (WDI, World Bank) - represents the cif value of all goods and other market services procured from the world.

Exports of Goods and Services as % of GDP (WDI, World Bank) - value of all goods and other market services provided to the world.

Table A 1 reports results from instrumental variables approach, where the instruments used are total years as a colony, total global aid given in that year, and twice lagged aid. The results are broadly in line with the pattern of results using other methods. While tests show the instruments to be valid, the problems of using IV in this context are illustrated by the low explanatory power, especially when using fixed effects.

Table A 1: Instrumental Variable Results (using annual data)

Variable	RE	FE	RE	FE
	Whole sample		1985-2005	
Aid	-0.16	-3.70	7.82***	-67.5
	(0.705)	(2.64)	(2.1)	(461.7)
Aid^2	0.0005	0.023	-0.047***	0.33
	(0.005)	(0.0169)	(0.0128)	(2.227)
Agriculture	-1.08***	-1.74**	-3.99***	-0.25
	(0.155)	(0.859)	(0.989)	(11.28)
Industry	0.74***	1.32***	2.75***	9.30
	(0.166)	(0.379)	(0.88)	(57.58)
Income	0.001	0.006*	0.021***	0.056
	(0.001)	(0.003)	(0.007)	(0.40)
$Income^2$	-0.0000004***	-0.0000003***	-0.000002***	-0.0000003
	(0.00000004)	(0.0000001)	(0.0000005)	(0.000006)
Imports	0.31**	0.79*	0.078	25.3
	(0.152)	(0.461)	(0.42)	(172.9)
Exports	0.02	-0.93*	-0.68	-16.12
	(0.134)	(0.512)	(0.474)	(-0.108)
Observations	1619	1619	887	887
Countries	80	80	72	72
R-Squared	0.36	0.05	0.03	0.01

Notes: As Table 2.1; Overall and Adjusted R-Squared refer to RE/FE respectively. The instruments of total aid given that year, a colony dummy and aid twice lagged are used to instrument for Aid and its square. R squared is either Overall or Adjusted, depending upon whether FE or RE are used.

Table A 2: Determinants of Tax/GDP Ratios (RE), 1970-2005

Variable	Current Aid		Lagged Aid	
Loans	1.11***	1.17***	0.58*	0.72**
	(3.17)	(3.42)	(1.67)	(2.13)
Loans 2	-0.0028*	-0.0029*	-0.0013	-0.0017
	(1.76)	(1.83)	(0.62)	(0.85)
Grants	-1.32***	-1.15***	-0.87***	-0.72***
	(6.05)	(5.41)	(3.96)	(3.38)
Grants2	0.00033	0.00029	-0.0000052	-0.000064
	(1.47)	(1.35)	(0.02)	(0.25)
Agriculture	-1.26***	-0.95***	-1.27***	-0.93***
	(9.46)	(7.10)	(9.44)	(6.95)
Industry	0.44***	0.71***	0.47***	0.77***
	(3.05)	(4.97)	(3.19)	(5.32)
GDP	-0.0061***	0.00046	-0.0061***	0.00084
	(10.07)	(0.52)	(9.92)	(0.93)
GDP2		-0.0000004***		-0.0000004***
		(9.86)		(10.27)
Imports	0.58***	0.54***	0.50***	0.44***
	(6.57)	(6.21)	(5.68)	(5.21)
Exports	-0.48***	-0.49***	-0.37***	-0.40***
	(4.80)	(5.08)	(3.76)	(4.22)
N	1554	1554	1558	1558
Countries	82	82	81	81
Chi-Stat	315.9	438.0	289.9	414.8
Overall R2	0.36	0.43	0.35	0.41

Notes: As Table 2.1,except RE is used.

Table A 3: Investigating Time Effects, 1970-2005 FE

Variables	Base	With Individual Year Dummies	With Year (-1969)	With post 1985 Dummy
Loans	1.25***	1.07***	1.29***	1.20***
	(3.57)	(3.07)	(3.69)	(3.43)
Loans 2	-0.0028*	-0.0029*	-0.0029*	-0.0028*
	(1.78)	(1.86)	(1.84)	(1.76)
Grants	-1.18***	-1.10***	-1.13***	-1.21***
	(5.42)	(5.08)	(5.17)	(5.52)
Grants2	0.00033	0.00061***	0.00039*	0.00031
	(1.49)	(2.73)	(1.73)	(1.43)
Agriculture	-1.16***	-1.26***	-1.27***	-1.10***
	(8.59)	(8.22)	(8.28)	(7.44)
Industry	0.51***	0.39***	0.46***	0.54***
	(3.48)	(2.65)	(3.05)	(3.60)
GDP	-0.0054***	-0.0054***	-0.0050***	-0.0056***
	(8.82)	(8.05)	(7.51)	(8.82)
Imports	0.55***	0.51***	0.55***	0.56***
	(6.10)	(5.62)	(6.09)	(6.19)
Exports	-0.51***	-0.41***	-0.48***	-0.53***
	(5.17)	(3.99)	(4.76)	(5.28)
Year			-0.14	
			(1.49)	
Post 1985 dummy				1.53
				(1.04)
Observations	1554	1554	1554	1554
Countries	82	82	82	82
F	30.4	9.33	27.6	27.5
Overall R squared	0.34	0.37	0.35	0.33

Notes: As Table 2.1 except for the final three columns. For the first of these, individual year dummies are included but not reported. The next column includes a variable that is the year minus 1969, i.e. in the first year included (1970) it is 1, in the second 2 and so forth. The final column includes a 'post 1984 dummy', which takes the value 1 if the year is between 1985 and 2005, and 0 otherwise.

APPENDIX B: DESCRIPTIVE LITERATURE

This section provides a more detailed discussion of the descriptive literature that is summarised in the literature review. It consists of two parts. The first looks at different practices that are considered to be donor errors or weaknesses. Their disparate nature illustrates why a full discussion was not included in the main body of the thesis. The second part examines the most important recent indices that use different data to form judgements on donor behaviour.

IDENTIFYING DONOR-ERRORS

This section catalogues various donor practices and problems that have been identified as 'donor errors'. This descriptive approach seeks to assess donor practices, and uses some of the points within this section to do so. They deal with much broader concerns than the positive literature, and their proponents argue that they capture important points that are not captured by any other methodology.

SELECTIVITY

A number of the newest indices include statistics on selectivity, mainly focused on policy and measured by CPIA or the WGI index[1]. For example, Wane (2004) argues that while aid allocation was previously measured by equity in response to need, the current criterion is policy selectivity. However, there is no such consensus regarding what the basis for selection should be. Wane (2004) argues that the WGI indicators are justified, by showing successful World Bank projects are associated with higher WGI scores on average, compared to failed projects. There are a number of problems with this, not least that WGI scores are highly correlated with income: in essence development projects are more successful in countries that need them less. Indeed it is this that motivates some to argue against not only an expansion of selectivity, but its current more-limited form. Institutional improvement has traditionally been thought of as a potential consequence of aid, rather than a prerequisite to it (Moss *et al.*, 2006). For example, political reform in Uganda was influenced by aid, if in a

[1] *The CPIA is a World Bank measure of governance, complied by 'country experts'. The WGI (Worldwide Governance Indicators) were originally known as KKZ. They use a variety of inputs (including the CPIA) to form an overall judgement.*

highly circuitous fashion (Verschoor, 2007). If a selectivity mind-set had been used in that case, reform may not have occurred.

Others criticise the selectivity agenda by taking the opposite view – that selectivity is too narrow. Amprou et al. (2007) and McGillivray (2003a) both argue that research has identified other significant variables that influence aid effectiveness and that these should be included in any measure of selectivity. They mention post conflict situations (Collier and Hoeffler, 2004), negative terms of trades shocks (Collier and Dehn, 2001), structural vulnerability and political instability (Guillaumont and Chauvet, 2001), and democracy (Kosack, 2003; Svensson, 1999). Amprou et al. (2007) also show that any ranking of donors is heavily influenced by the choice of which of these criteria to use. These arguments must be qualified by the aforementioned (deserved) humility of the research community. Unless there is a strong and consistent effect found in the literature it is difficult to argue that measures of donor performance should be based upon these results.

Fragmentation and Transaction Costs

Calls for donor harmonisation have existed for some 40 years: the Pearson Commission (1969, p.228) stated that "the present multiplicity of agencies and their lack of coordination leads to much unnecessary duplication of effort." Indeed, donors themselves understand the need to tackle fragmentation by using comparative advantages to specialise by country and/or sector (OECD, 2007, p.45). This is motivated by the vast anecdotal evidence regarding the consequences of the status quo. The costs are varied and difficult to catalogue or measure, but an overview is given here. It is reasonable to believe that aid projects are subject to returns to scale. As such, a certain fixed cost will be incurred regardless of the project or disbursement size. For smaller disbursals this fixed cost would be a higher proportion of aid, and so diminish the value of the aid. Fragmentation would then diminish the effectiveness of aid overall. Some of these fixed costs are borne not by donors but by recipients: for example developing countries produce 8,000 audit reports for multilateral agencies, some 5,500 are for the World Bank alone (World Bank, 2003, p.207). A consequence of this fragmentation is that donor's accountability is diminished; if donors each give a small percentage of the total aid for any given country their individual effect cannot be easily measured. This may lead to a lack of responsibility for success and failure (see Acharya *et al.*, 2006; Easterly, 2007). This 'low ownership' may also partly explain why conditionality has often failed to influence policy in recipients. With larger shares of aid

money, a given donor would have had greater influence for potential changes, and greater incentive to ensure the effectiveness of its aid (a point made in the theoeretical literature by Bigsten, 2006; Cordella and Dell'Ariccia, 2007).

Also from a theoretical perspective, Torsvik (2005) models donors as deriving welfare from poverty reduction abroad. Thus aid is a public good amongst altruistic donors, and as such is under provided. Coordination could reduce the resulting moral hazard problem. Acharya et al. (2006) catalogue further consequences of fragmentation, amongst which are inefficient use of staff both within aid agencies and recipient governments, increased wastage to do with promoting a given donor and needless competition over projects and staff. Van de Walle (2001, p.165) shows that these pressures are not negligible: "In Niger, for instance, the majority of NGOs appear to be operated by moonlighting civil servants and ex-ministers of cabinet." In Mozambique a cleaner's salary in the international agencies sector was comparable to the national director of the civil service (Fallon and da Silva, 1994). The competition for staff results in a drain from the government, weakening the institution.

Recent attempts to increase donor harmonisation include the Paris and Rome declarations (2003 and 2005 respectively). However, previous attempts to harmonise through multilateral mechanisms did not work well. For example the UNDP was supposed to fulfil this task after World War 2 (Disch, 1999), but multilateral aid flows remain at relatively low levels. SWAps have also been tried, but often found expensive[1]. It is interesting to note that a lack of coordination is not necessarily a result of divergent donor interests (Halonen-Akatwijuka, 2004). Regardless of their cause, the widespread opinion is that they are undesirable: "a maddening problem in foreign aid for all concerned is the huge administrative costs on both recipient and donor sides from the duplication of donor efforts and their failure to coordinate their efforts with each other." (Easterly, 2007, pp.639-640). However, no accurate measure of them exists, which may contribute to the general inertia when it comes to reducing them.

When thinking about measuring transaction costs, it is worth dividing them into tangible and intangible costs (also called administrative and institutional). Tangible costs are those that are easy to measure, such as the number of hours that a civil servant must spend in order to coordinate an aid transfer. Intangible costs are less easily identified, including things like the

[1] *"The experience of Tanzania and Uganda suggests that the move to SWAps – which may reduce costs for the partner country – often seems to increase the costs for the donors." (OECD, 2003, pg.116)*

damage to an institution by coordinating different projects and programmes. It can also include an element of opportunity cost. Amis et al. (2005) highlight the difficulty in quantifying transaction costs, but point to research that suggests institutional costs are more problematic than administrative ones. The latter are to some extent unavoidable and marginal, the former includes the use of parallel systems, the movement of talented staff away from domestic government to donor missions and so forth. They also highlight many of the concerns that the PRSP approach and Paris Declaration were meant to answer (donor-driven priorities and uncoordinated donors respectively).

Evaluation – an 'Underfunded Public Good'?

Evaluation of a specific kind has enjoyed a surge in support in recent times (a prominent example being J-PAL) and some descriptive works include support for evaluation in an index of donor performance. This popularity has in part been based on results from randomised trials, which reflects the great desire within the development community to have proof of impact. As such, evaluations of different interventions are now more commonplace. One consequence of knowing more is to stop poorly performing projects, another is to expand the coverage of successful ones. The problem of foregone benefits of economies of scale (Acharya *et al.*, 2006; Easterly, 2007) is linked to low levels of evaluation (Birdsall, 2004, pp.13-19). Aid agencies would have more incentive to scale-up successful projects if the differences in outcomes between different projects were known to be large. While the methods of randomised trial evaluations have become popular, they do not enjoy a consensus. Deaton (2009) coherently catalogues their advantages and disadvantages. A common concern is that of applicability: a project found successful in one area may not replicate its success in different circumstances. Given the contentious nature of 'evaluation as a public good' argument, efforts to judge donors on the basis of their support for this type of evaluation seem somewhat premature.

'Outdated' Aid Modalities

Easterly (2007) argues tied aid, food aid and technical assistance are outdated and indefensible aid modalities. Easterly and Pfutze (2008, pp.12-18) report some remarkable statistics for these three types: 92% of Italian aid is tied, 44% of Portuguese aid is food aid and Greece gives 64% of its aid in the form of technical assistance. However, the reality is significantly more nuanced than implied. Tied aid is perhaps the best understood. Recognising the body of research which highlights problems with tied aid, the OECD

recommended that aid be untied to the least developed countries (in 2001), which is steadily becoming a broader recommendation. Tying in this sense relates solely to conditions placed on the procurement of goods and services, commonly meaning an aid transfer must be spent within the donor country. A widely cited statistic is that tying aid reduces aid value by some 13-23% (Jepma, 1991, p.50). This loss in value is natural, given that procurement conditions limit the amount of suppliers that are able to provide a good. Furthermore, foreign markets may sell goods and services that are less catered to distant developing country markets, and incur high transport costs. This estimate of diminished value does not include the wider and less quantifiable consequences, which include the likely foregone increase in neighbouring countries' production and trade with the recipient country. Thus tied aid is found significantly less valuable than untied aid.

Food aid and technical assistance are more contentious areas. Easterly and Pfutze (2008) argue that food aid is commonly the 'dumping' of unwanted food by developed nations, which erodes domestic food production and thus exacerbates food shortages in future years. The UN WFP responded to criticism by arguing 80% of its budget was spent within the third world (Roman, 2008). The resulting concern of this criticism is that the data does not distinguish between good and bad practice. The potential severity of the distinction is shown by an estimate published by the OECD that tied food aid costs can be up to 55% higher than alternatives (Clay *et al.*, 2005), thus reducing the value of aid by up to a third. Food aid is undoubtedly a difficult and complex area, and as such including it in a simple aggregation exercise is not appropriate. Technical assistance (TA) is generally spent within a donor country in order to create knowledge and skills that is of benefit to a recipient, which often entails funding research in the west. The concern with this type of aid is that it never reaches the countries it is supposed to help, where it could achieve greater value for money. This is another type of aid that cannot be judged collectively as simply good or bad, and simplistic judgements are not appropriate.

Newer Modalities

Some efforts to codify donor behaviour have also focused on the type of aid given, with specific reference to some of the newer modalities. To justify this there needs to be a clear advantage or disadvantage of a given type, and while many modalities have been designed in order to overcome the deficiencies of other types that does not automatically mean they achieve their goal. The use of Budget Support is typically motivated by a desire to use a

governments own systems and thus increase recipient government efficiency. The potential benefits of Sector Wide Aproaches (SWAps) are that they increase coordination amongst donors and recipients, predictability of funds and domestic accountability, whilst decreasing transaction costs. Parallel systems have been used in order to ensure a basic level of efficiency, but have been criticised for not giving a government's own system the chance to become more efficient. Research relating to the World Bank suggests the more basic problem that these parallel systems are simply no better than the original systems that they try to circumvent (Boyce and Haddad, 2001). An apparent benefit of project aid is that it allows a donor more control over its own expenditure, and some influence over the recipients' budgets. However, given the widespread acknowledgement of fungibility, there is no guarantee that the money is additional. Cordella and Dell'Ariccia (2007, p.117) compare budget support with project aid, from a theoretical perspective. They essentially demarcate the former as allowing a donor some partial power over the whole budget, whereas the latter gives total power over a smaller section of that budget. When defined in this way, the choice of which type of support to use simply becomes reliant upon the relative size of the aid transfer (which determines a donor's power) and the alignment of donor-recipient preferences (which determines their need for power). Whilst many of the wider consequences are not included in the model (e.g. the potential foregone efficiency improvement in the government associated with using their processes), it is still useful to highlight these two factors in a donors decision.

A preference for project aid may in part stem from a donor's desire to fund popular activities, perhaps owing to a desire to justify their aid expenses to their own citizens. As such, expenditures that 'placate' a donor's citizens are likely to be chosen. Anecdotal evidence confirms this situation: donors are more likely to fund a school being built than a teacher's salary. Clearly, both payments are needed to have the desired effect. But the desired effect (say a better educated population) is less observable or immediate than a simple input statistic. Donors are reluctant to forego control of aid budgets, especially in poor governance situations. The popular wisdom is that countries with a better policy record should receive more aid through its own systems and for its own priorities, such as General Budget Support (GBS)[1]. This is a reasonable proposition, as better policy would tend to be highly correlated with the ability of a government to turn a given amount of money into a

[1] *E.g. DFID state that GBS is preferred 'where country circumstances are suitable' (National Audit Office, 2008, pg.4)*

certain outcome. The relative effectiveness of a parallel system is likely to be less affected by a government's own policy standards. As such the decision regarding the shares of project and programme aid should be influenced by government policy. If this is the case, a simple tally of the use of certain systems does not measure good or bad practice.

DISBURSAL, SCHEDULING AND VOLATILITY ISSUES

Even if the problems of the wrong type of aid are overcome, recipients often complain about the wrong date of delivery. Delayed disbursal appears to be both a severe problem for some donors and a common problem for all donors, Leurs (2002, p.36) reports that Senegal think it is their greatest donor-related problem and it is rated by recipients as the fifth problem overall (ibid, p.13). The SPA (Strategic Partnership for Africa) documents promote an ideal calendar which includes when aid should be disbursed, so that it can be included in planning cycles. Progress has been made, but it is a varied picture. This is closely related to aid volatility, which the SPA also examines. Recipients are unlikely to use aid money as well if they cannot plan ahead for its use, and so multi-year commitments and a smaller number of transparent conditions are useful. A gradual and proportionate response to a condition not being met is also seen as key.

During 1993-2005 the mean absolute error in predicting aid disbursals was around 1% of GDP, but disbursals were not consistently more or less than commitments (Celasun and Walliser, 2008, p.570). Some of the discrepancy is lost aid, whereas other parts are merely postponed. Not only is aid found to be less predictable than tax revenue, it compounds other problems as its fluctuations are likely to coincide with those of tax revenue (Bulír and Hamann, 2008; Celasun and Walliser, 2008, p.574). There is evidence that this kind of volatility considerably hurts developing countries, as they are less able to withstand it than developed countries (Pallage and Robe, 2003). Indeed, Lensink and Morrissey (2000) find once the negative effective of aid volatility is controlled for, aid has a positive effect on growth. It is then worrying that recent measures of volatility and pro-cyclical behaviour are either static or increasing (Bulír and Hamann, 2008).

It is worth noting that some volatility can be defended, for example if a policy condition is not met. Bulír and Hamann (2003) find that countries that are 'on-track' with IMF programs receive 75% of commitments, whereas 'off-track' countries receive only 33%. Indeed the Strategic Partnership for Africa (2005) provide some data that explains why disbursements differ. They report that the most common reasons for late disbursal are (in order) failed

policy-conditions, donor-side administration and recipient-side administration. A large amount of funds are not delivered by IFIs because they have more conditions attached to them, which are not met. This may in part explain why multilaterals are considerably less likely to disburse monies in the same year as they are committed.

LATEST DESCRIPTIVE WORKS

I now turn to outlining various newer indices that typically bring together a range of the above factors. This is an extended version of the summary found in the literature review chapter, and while repetition is kept to a minimum some parts are replicated so as to ease comprehension.

CRITIQUE OF COMMITMENT TO DEVELOPMENT INDEX (CDI)

The CDI (Commitment to Development Index) is noteworthy because of its large impact on policy: it is used explicitly by Dutch and Finish governments, has influenced Australian, Canadian and Norwegian policy, has angered Japanese officials, and is supported financially by ten bilateral donors (Roodman, 2006). It is a broad index that seeks to rate the effect of donor countries on developing countries in many different areas. This results in the weakness that donors may be rewarded or punished for factors that are out of their immediate control. For example, the United States is rewarded for receiving large numbers of migrants, but this is influenced by the proximity of other countries, something Australia does not enjoy. Within this wider index is the Index of Donor Performance (IDP) which is more relevant here. It is essentially a measure of aid/GDP from donor, but aid is discounted for poor practices, for example tied aid is discounted by 20%.

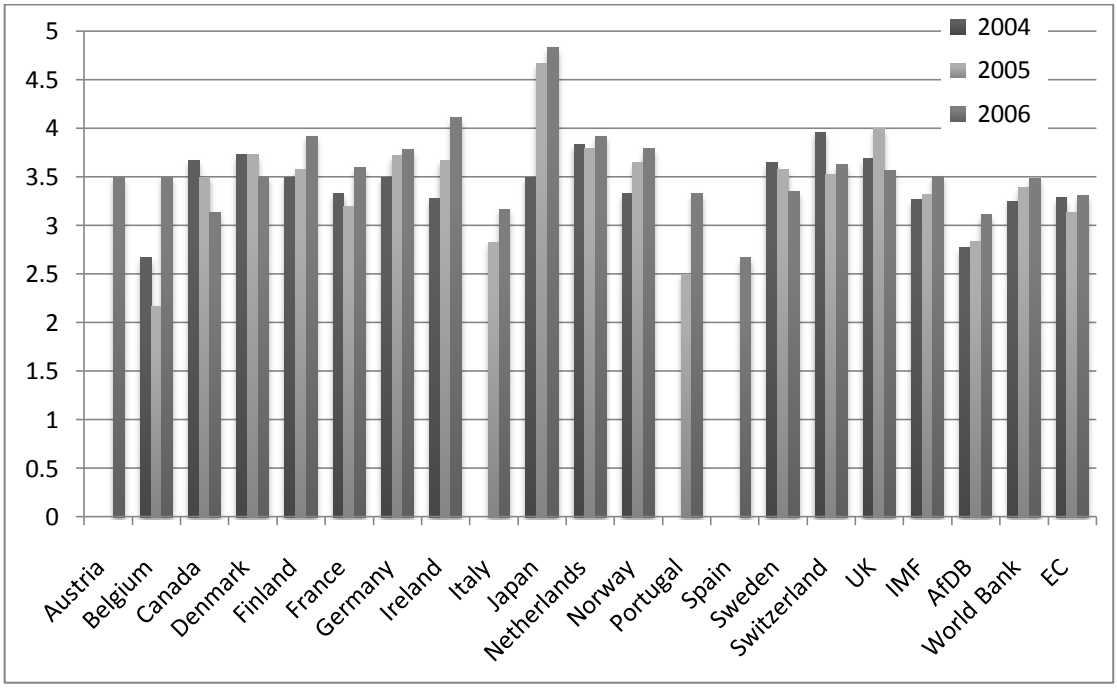
The driving force behind the IDP is the amount of aid given, as quality does not vary as much as quantity. The biggest changes are that Japan is penalised for its high inflows from debt interest and the proliferation of Australia and Italy are penalised. It is interesting to note that each multilateral institution is given a ranking, and scores for donor-countries are adjusted to take account of their contributions to these agencies. The index incorporates a high degree of whimsy and decisions are often taken without a theoretical justification. For example, there is no real conceptual framework to explain why all administration costs are discounted, as it could be argued higher administration costs signify a greater level of efficiency and efficiency in disbursal (McGillivray, 2003b). It is certainly clear that aid completely without administration costs seems unrealistic and undesirable.

The most common problem of aggregation weights seems to be small, as Chowdhury and Squire (2006) examined the CDI and found the weighting was close to the general consensus amongst academics they questioned. However, a large number of problems remain. Selectivity weights used to be independent of the amount of aid given to a recipient (McGillivray, 2003b), and so giving even a miniscule amount of aid to a country with 'bad policy' would result in a worse selectivity rating. In order to incorporate the problems of project proliferation in the index, a discount is used. This fits a form where projects above or below a certain size are discounted. Conservative parameter choices mean these discounts are small, especially for large projects. However, research on the optimal size of aid disbursals does not allow any confidence when deciding upon a figure.

STRATEGIC PARTNERSHIP FOR AFRICA

Next are two attempts to gather data on specific issues. The Strategic Partnership for Africa gathers data on donor behaviour by asking just over a dozen African recipients of general budget support to score donors out of five on six different areas of aid. The six questions relate to alignment, disbursement and transaction costs. The questionnaires are specific to general budget support, but the answers are more broadly applicable. Figure B 1 indicates the average scores of the rated donors by year. Small sample sizes mean, for example, that the 'average' score of Japan is decided solely by Tanzania. The results are published and have on average been improving. The data's main contribution may simply be publicizing and defining bad practice from the recipient's perspective.

Figure B 1 SPA Average Scores By Donor 2004-2006



Source: Strategic Partnership with Africa: Survey of Budget Support, 2006, Volume II – Detailed finding. Available at http://www.spa-psa.org/resources/2007%20Plenary/SPA%20Survey%202006%20Final%20Draft%20Volume%20II.pdf

OECD's Fragmentation Measure

The OECD (2008a) report a measure of fragmentation that differs from others previously used. As reported later in this section, Easterly and Pfutze (2008) use the Herfindahl index, which is a measure of concentration common in the industrial organization literature. The OECD measure is much simpler to calculate and builds upon the OECD concept of country programmable aid (CPA), which is essentially only includes aid which a recipient country government can programme. As shown in the table Table B 1, the concentration measure then counts the number of times a donor is a large donor in a given country, relative to its own size. For example, if a donor gives 1% of global aid, the measure counts the number of times the donor contributes more than 1% of the recipient's aid budget. This is then divided by the number of partners a donor has, and reported as a percentage. Thus IDB has a score of 100% as while it only gives 0.8% of global aid; it always surpasses that figure in each of its recipients (not shown in table). This is in contrast to the Herfindahl index, where it is not feasible for donors to show 'complete' concentration. The advantage of this measure is that it is very simple, and as such gives clear implications for donors wishing to decrease their fragmentation.

Table B 1 OECD Fragmentation Measure for DAC Members 2006, by Donor

	Total CPA – DAC standard definition (USD million)	Donor's share of total CPA	Total no. of partners	No. of partners above average share	Concentration measure (D as % of C)
	A	B	C	D	E
United States	12,967	21.70%	128	34	27%
Japan	8,416	14.10%	135	44	33%
EC	6,219	10.40%	144	82	57%
United Kingdom	3,177	5.30%	93	36	39%
France	2,740	4.60%	123	50	41%
Germany	2,723	4.50%	110	59	54%
Netherlands	1,601	2.70%	93	42	45%
Sweden	1,080	1.80%	91	44	48%
Norway	1,003	1.70%	88	42	48%
Canada	974	1.60%	100	35	35%
Australia	955	1.60%	50	24	48%
Denmark	905	1.50%	71	27	38%
Spain	831	1.40%	81	42	52%
Italy	519	0.90%	76	32	42%
Switzerland	501	0.80%	86	38	44%
Belgium	498	0.80%	83	39	47%
Ireland	347	0.60%	56	23	41%
Finland	241	0.40%	62	27	44%
Austria	158	0.30%	53	27	51%
Portugal	146	0.20%	20	11	55%
Luxembourg	128	0.20%	40	25	63%
New Zealand	122	0.20%	43	25	58%
Greece	119	0.20%	34	23	68%

Note: Gross disbursement average 2005-06 Source: OECD (2006, pg.9)

COLLECTIONS OF DATA

A number of articles and reports exist that do not argue that their sole contribution is new data or original focus. Instead, they collate (but not necessarily aggregate) data on a range of topics to give an overview of donor behaviour. They typically include data on poverty selectivity, aid modalities and perceived indicators of inefficiency. Table B 2 provides a list of the included variables for three collections of data. The list does not include the factor which is represented, but rather the actual variables freely reported.

Table B 2 Collections of Data

Paris Declaration Indicators	OECD Development Co-Operation Reports	Easterly and Pfutze
• Budgetary Aid disbursed as expected • Of technical assistance, how much is coordinated with recipient • Number of Parallel implementation units • Matching of Aid Scheduled and Disbursement • % Aid Untied • % of aid that was programme-based (OECD concept which denotes 'good' aid modalities') • Coordinated donor missions • Coordinated donor analysis	• Total Net ODA receipts • Country Programmable Aid (CPA, meaning the above minus humanitarian work, debt relief, admin, in-donor refugee costs etc.) • Proportion of funds going to the LDCs • Proportion going to 'large countries with good policies and many poor people' • Emergency/humanitarian aid as % of total aid • % of aid that is tied • Paris Declaration indicators	• Fragmentation: Herfindahl index • Selectivity rank (allocations to free, corrupt and poor countries) • Ineffective Channels (food aid, tied aid and TA) • Overheads (mainly staff costs) • Transparency

The Paris Declaration indicators are distinctive due to their clearer focus: they exist to measure progress on the Paris declaration amongst major donors. The indicators of progress together represent ten of the twelve indicators outlined in 2005, and can be found in surveys in 2006 and 2008 (OECD, 2005, 2006, 2008b). Three variables deal with co-ordination, three with aid modalities and two with disbursement. Also from the OECD are the Development Co-Operation Reports, which incorporate the Paris declaration indicators. The Development Co-Operation Report 2003 proposed "a dozen measures of a common-sense kind for measuring the contribution that development co-operation is making to development results" (OECD, 2007, p.15). In a similar fashion to the Paris declaration, the measures are represented by the variables reported above.

Easterly and Pfutze (2008) rank donors on a number of different criteria, giving a composite rank that is an average of the disaggregated rank. The criteria include allocation principles, selectivity, inefficient channels, fragmentation, administration costs and transparency. The ranking system used means Portugal receive no extra punishment in the ranking for providing 44% of its aid as food aid, the punishment is solely based on providing more than 9% as food aid. It is also easier to cut an aid budget from 50% to 40% being food aid than 40% to 30%, thus a quadratic relationship makes sense. Changing the rankings in this way (summing the square of each component) gives the below change:

Table B 3 Altered Ranking of Easterly and Pfutze's (2008) Inefficient Aid Channels Table

	Ranking				Ranking		
	New	Old	Change		New	Old	Change
Ireland	1	3	2	Finland	14	14	0
Luxembourg	2	12	10	Spain	15	23	8
EC	3	9	6	Belgium	16	16	0
Switzerland	4	2	-2	Portugal	17	15	-2
United Kingdom	5	6	1	Canada	18	20	2
Sweden	6	4	-2	Germany	19	17	-2
Denmark	7	10	3	Austria	20	22	2
Japan	8	13	5	Australia	21	25	4
IDB	9	1	-8	United States	22	26	4
Norway	10	4	-6	Italy	23	21	-2
Netherlands	11	11	0	Other UN	=24	=6	-18
New Zealand	12	19	7	UNTA	=24	=6	-18
France	13	18	5	Greece	26	24	-2

The major differences have obvious causes. Luxembourg is no longer punished so severely for giving 4% of its aid as food aid (22 out of 26 countries give less). The UNTAD and other UN are punished more severely for giving all of their aid as technical assistance. Norway and the IDB both drop due to a high reliance upon technical assistance. This simple exercise illustrates the sensitivity to small arbitrary changes of methodology, highlighting a real weakness of this approach. If donors believe the rankings are arbitrary they are less likely to be influenced them, blunting their ability to be used as an agent of change.

APPENDIX C

A description of the data used in chapter 4 is provided first. All Websites accessed June 2009 unless otherwise stated.

Aid – The Aid variable is taken from the OECD DAC Table 3a, and available at http://stats.oecd.org/Index.aspx?DatasetCode=TABLE3A . 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.

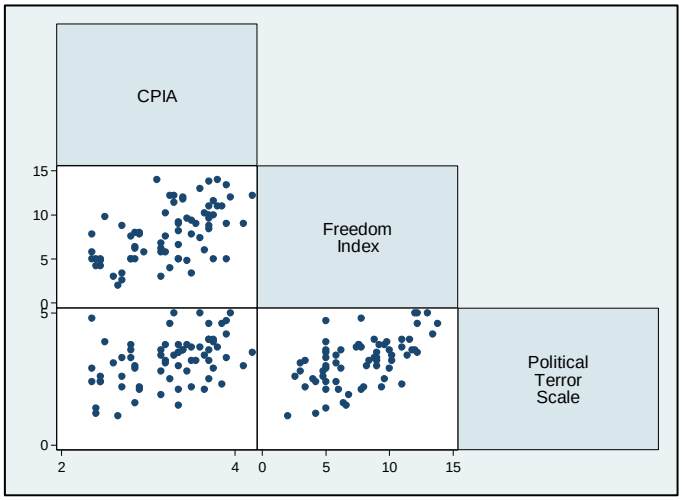
Corruption – The variable is taken from the Worldwide Governance Indicators and is based upon a wide variety of information sources and can be located at info.worldbank.org/governance/wgi/ .

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 C 1 Correlation between Policy Variables



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

Table C 1 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
Netherlands	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

This second half of Appendix C provides additional tables for chapter four, which 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 C 2 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 C 3 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 C 4 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 C 5 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 C 6 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 C 7 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 C 8 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 C 9 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)	
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 is the non-standardised version of Table 4.3.

Appendix D

Dependent variables were transformed (except for the binary variable for GBS) so as be a percentage in the form x/AID, where x is a given criterion or aid instrument. In order to do this the total of aid given from the recipient to the donor was calculated, using the same data source that provided the dependent variable in order to ensure comparability.

DEPENDENT VARIABLES

CRS Dataset – This is the most comprehensive dataset available on General Budget Support. It is made available by the OECD, and can be accessed at http://stats.oecd.org/ .

AiDA Dataset – This dataset was accessed from http://aida.developmentgateway.org/AidaHome. It provides donor-reported information on aid activities. The dependent variables from this dataset are the percentage of aid that is given to the civil society and the percentage that is general programme assistance.

DFID Project database – This is a self-reported dataset from DFID. It provides details of all current DFID activities, with limited information on past years. It is available at http://projects.dfid.gov.uk/home.asp

INDEPENDENT VARIABLES

Prsp – A dummy was created to signify that a PRSP document has been agreed. The list of documents agreed was taken from the IMF (http://www.imf.org/external/np/prsp/prsp.asp)

Education spending – This is the amount of public money spent on education, divided by GDO. This is taken from the World Development Indicators, provided by the World Bank. They can be accessed at http://data.worldbank.org/data-catalog/world-development-indicators.

The equity of public resource use - This is discussed at some length in the body of the text. It is taken from the CPIA, provided by the IMF and available from the World Bank's databank at http://data.worldbank.org/indicator/IQ.CPA.ECON.XQ. The variable ranges from 1 to 6, with a more positive number meaning a more positive situation.

Public sector management - It is taken from the CPIA, provided by the IMF and available from the World Bank's databank at http://data.worldbank.org/indicator/IQ.CPA.ECON.XQ.

The variable ranges from 1 to 6, with a more positive number meaning a more positive situation.

Government Effectiveness – this is taken from the Worldwide Governance Indicators (WGI), and can be accessed at http://info.worldbank.org/governance/wgi/index.asp. The variable ranges from -2.5 to 2.5, with a more positive number meaning a more positive situation.

Number of donors – This was constructed using the CRS/OECD dataset (http://stats.oecd.org/). A dummy was created and took the value one if there was a positive commitment from a donor to a recipient in a given year. The number of donors is then the sum of these, i.e. the number of donors giving aid to a recipient in a given year.

Aid/GNI – This is taken from the World Development Indicators, provided by the World Bank. They can be accessed at http://data.worldbank.org/data-catalog/world-development-indicators.

GNI per capita PPP (in current international dollars is taken from the World Development Indicators, WDI) - accessible at http://data.worldbank.org/data-catalog/world-development-indicators.

Share of a donor's aid budget – This was constructed using the CRS/OECD dataset (http://stats.oecd.org/). The total amount of aid disbursed by a donor in a given year was first calculated, and then used as the denominator to give data in the form 'x% of donor z's aid in year t was allocated to country y'.

DFID RESULTS

DFID – this dataset is provided by DFID, and includes the expenditures on all active DFID projects relating to development. The advantage of this dataset is that it provides rich detail on DFID expenditures. The four variables we extract are based upon dummies, but are transformed to be the percentage of aid to that recipient that meets a given criterion. These criteria are whether the contribution was budget support, to the government sector, a program based approach and/or led by a host country/organisation. Data is available for 56 recipients, and as only active projects are used, the data is aggregated to be in effect over one time period. The dependent variables are highly correlated, with correlations of between 0.60 and 0.84. The variables can be seen as giving different degrees of control, with GBS being the highest, followed by contribution to the government sector, followed by programme approach and then recipient-led. DFID are particularly interesting to study given that they describe what is here termed pragmatic selectivity most vividly, although they never explicitly propose it as an alternative allocation paradigm.

This last set of dependent variables describes the amount of control that DFID relinquish to different recipients. The four variables used each describe a situation toward the end of higher recipient control. Each variable is the percentage of current (in March 2010) aid that meets the given criterion.

Table D 1 reports the results from the DFID dependent variables, with both the main and alternate specifications. The table has the variable indicating most recipient control on the left and the least on the right, although this categorisation is somewhat loose. The two alignment variables do not match closely, but we find one significant effect with the program based approach and equality of public resource use. The PRSP variable is found to be insignificant in every case. Governance is positive in each column and significant in six of the eight. This confirms previous analysis regarding the importance of governance, both in terms of narrowly-defined technocratic ability and the slightly broader sense of government effectiveness. Interestingly, both governance variables report a much stronger relationship with the two higher degrees of recipient control (Budget support and contribution to government sector) than with the broader modalities, matching the broader modalities. The now familiar relationships with aid modality are found for number of donors and income level, but the positive link with aid/GNI is not found.

Table D 1 DFID Control, Current Aid (March 2010), both specifications

	Budget Support		Contribution to the Government Sector		Program Based Approach		Led by Recipient	
% Education spending/GDP	-1.30		0.36		-0.59		0.95	
	(1.06)		(0.22)		(0.39)		(0.47)	
Equality of Public Resource Use		4.90		1.96		22.1*		22.0
		(0.51)		(0.16)		(1.85)		(1.38)
Government Effectiveness	24.4***		30.7***		22.6**		25.6*	
	(3.07)		(2.84)		(2.32)		(1.95)	
Public Sector Management		33.2***		45.0***		12.2		5.36
		(2.73)		(2.90)		(0.80)		(0.26)
PRSP document created	-2.08	-9.91	-2.83	-12.5	-5.84	-12.2	-4.06	-7.26
	(0.27)	(1.27)	(0.27)	(1.25)	(0.62)	(1.25)	(0.32)	(0.56)
Number of Donors	1.34**	0.93*	1.90**	1.02	1.89***	1.19*	2.13**	1.45
	(2.57)	(1.70)	(2.68)	(1.47)	(2.96)	(1.74)	(2.48)	(1.60)
Aid/GNI %	0.53	-0.022	-0.090	-0.79	-0.078	-0.67	-0.24	-0.66
	(1.24)	(0.05)	(0.16)	(1.41)	(0.15)	(1.22)	(0.34)	(0.91)
GNI pc/100	-0.0059***	-0.017***	-0.0083***	-0.025***	-0.0085***	-0.018***	-0.0083**	-0.013
	(2.73)	(3.16)	(2.81)	(3.68)	(3.20)	(2.72)	(2.33)	(1.47)
Observations	54	45	54	45	54	45	54	45
Pseudo R-squared	0.279	0.386	0.215	0.382	0.269	0.322	0.142	0.143
F stat	4.42	5.61	3.42	5.54	4.26	4.48	2.46	2.22
Mean of Y	13.0	15.6	26.6	28.1	23.7	26.6	37.6	39.1

*Note: Standard OLS used. T statistics are provided beneath coefficients in brackets, and 10, 5 and 1% significance levels are denoted by ***, ** and * respectively.*

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