Essays on the Allocation, Effectiveness and Coordination of Development Aid

Dissertation

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Overview:

An Empirical Analysis in Five Chapters

My dissertation contributes to three main topics in the literature on development aid: aid allocation, aid effectiveness and aid coordination. A vast literature exists on each of these topics: more than 200 papers have dealt with the allocation of official development assistance (ODA), more than 100 studies have looked at the effect of official aid on economic growth, and dozens of papers exist on aid fragmentation and donor coordination across aid recipient countries. The aim and motivation for my dissertation has been to explore new research paths in the field of development aid, trying to answer important questions which have yet to be sufficiently tackled in the literature: Chapter I assesses the allocation of aid by non-governmental organizations (NGOs), a topic seriously under-researched mainly due to the lack of appropriate data. Chapter II analyzes aid allocation decisions within an aid recipient country, a research question which has almost not been looked at to date. Chapter III moves away from the custom to look at the aggregate effect of aid on economic growth and asks a "smaller" question, i.e., has foreign aid specifically meant to fight sexually transmitted diseases been effective in alleviating HIV/AIDS epidemics in developing countries? In Chapter IV, we investigate the effectiveness of a new form of conditionality, i.e., making aid commitments conditional on past performance. Finally, Chapter V looks at the coordination of donors within an aid recipient country, in contrast to the existing cross-country studies on this subject. In the following, I motivate the specific research question of each chapter, briefly outline the different approaches employed and present the main findings.

Official bilateral and multilateral donors have extensively been criticized by following commercial and political motives rather than allocating aid funds according to the needs of the developing world (e.g., Alesina and Dollar 2000, Dreher et al. 2009a, 2009b, Kuziemko and Werker 2006). In light of this, proponents of NGOs have argued that the aid allocation of NGOs is superior to that of official donors. However, this has scarcely been subject to empirical scrutiny. The few studies examining this question barely find any evidence supporting the superiority of NGOs' aid allocation (Dreher et al. 2007, Dreher et al. 2012,

Koch et al. 2009, Nancy and Yontcheva 2006, Nunnenkamp and Öhler 2011, Nunnenkamp et al. 2009). Concerns have also been raised about the increasing funding of NGOs by official financiers. In particular, the autonomy of NGOs' allocation decisions has been questioned in this regard (Edwards and Hulme 1996).

In Chapter I, Axel Dreher, Peter Nunnenkamp, Johannes Weisser and I address these concerns and analyze whether a high financial dependence of NGOs on official financiers alters their aid allocation patterns. Most importantly, we examine whether a high financial dependence leads NGOs' aid allocation to align more with that of the official financier. We draw on an exceptionally large and detailed database on Swiss NGO aid allowing us to analyze the aid allocation of more than 300 organizations across low- and middle-income countries within a panel Tobit framework. Our results indicate that the allocation of NGO aid is in line with that of ODA to a large extent. This particularly applies to officially refinanced NGOs. Moreover, NGOs tend to locate themselves where their peers are active. However, the poverty orientation of NGOs and their incentives to engage in difficult environments are independent of the degree of official refinancing.

As already mentioned above, donors are often confronted with the critique that their aid allocation decisions are economically and politically motivated rather than need-based. A typical response to this criticism is that the literature on aid allocation mainly consists of cross-country studies which fail to capture the poverty focus of donors within recipient countries. The spatial distribution of wealth across regions within developing countries is typically very unequal. Furthermore, the quality of local institutions tends to vary substantially within countries. Thus, even in countries with an average income per capita which is above subsistence levels, donors can legitimize their engagement in these countries by focusing on deprived regions.

In Chapter II, Peter Nunnenkamp, Maximiliano Sosa Andrés and I help closing this gap in the empirical research on aid allocation by employing Poisson estimations on the determinants of the World Bank's choice of aid project locations at the district level in India. We combine the project-related information offered by AidData in collaboration with the World Bank, with exceptionally rich data reflecting economic, institutional and political conditions available for 620 districts in 28 Indian states and seven Union territories. The case of India is particularly relevant to assess the within-country allocation of aid. The country is characterized by striking regional disparities with respect to poverty, governance and stability, which should have affected the location of aid projects. Our main objective is to assess

whether the World Bank adhered to its own insights – according to which aid tends to be more effective in poor environments with better governance (World Bank 1998) – when distributing its projects within India. According to our results, the evidence of needs-based location choices is very weak, even though World Bank activities tend to be concentrated in relatively remote districts. Institutional conditions matter insofar as project locations are clustered in districts belonging to states with greater openness to trade.

Apart from the criticism that aid is largely economically and politically motivated, aid has often been criticized for not being effective in fostering economic growth. A large proportion of the literature points in this direction (e.g., Rajan and Subramanian 2010).[1] However, various problems related to aid-growth regressions in the literature exist. The most common and important issues are serial correlation in the errors, endogeneity bias, multicollinearity related to the inclusion of aid, aid squared and interactions of aid with other variables in the same regression, and complex combinations of the three (Roodman 2008). Furthermore, aid is far from being homogeneous, consisting of a wide range of assistance, ranging from emergency aid to aid for good governance. It seems likely that some forms of aid are effective in achieving their development goals, whilst others are not. Following this reasoning, some of the more recent literature has moved away from the question about the general aid-growth nexus, instead focusing on questions about the effectiveness of aid in specific sectors. For instance, Dreher et al. (2008) analyzed the impact of aid for education on school enrollment rates, while Williamson (2008) looked at the effect of aid for health on health outcomes.

In Chapter III, Peter Nunnenkamp and I contribute to this literature by evaluating whether foreign aid has been effective in alleviating HIV/AIDS epidemics, which figures prominently among the Millennium Development Goals. Employing a difference-in-difference-in-differences (DDD) approach, we identify the treatment effect of ODA specifically meant to fight sexually transmitted diseases on HIV/AIDS-related outcome variables, i.e., new infections and AIDS-related deaths. The essential idea underlying the DDD approach is to combine before-after comparisons and with-without comparisons. This approach appears to be the most appropriate to assess the recent steep increase in ODA directed against HIV/AIDS, and helps mitigate important limitations that plague both types of comparisons when employed in isolation. Our empirical findings indicate that ODA has not prevented new infections. However, the results regarding the medical care of infected people

[1] Nevertheless, there are also studies coming to more positive conclusions, among them most recently Clemens et al. (2012).

are mixed: evidence on significant treatment effects on AIDS-related deaths exists for the major bilateral source of ODA, the United States, in contrast to ODA from multilateral organizations.

A possible means to foster economic growth and reduce poverty in recipient countries is the so-called conditionality attached to aid. Collier (1997: 56), for instance, states that aid might be "remarkably effective if it induces governments to adopt growth-inducing and poverty-reducing policies." However, traditional conditionality has largely failed to induce policy reforms in recipient countries; ex ante threats by donors that they will not disburse committed aid if the recipient does not fulfill reform promises are hardly credible. In light of this, some scholars have argued for a redesigning of conditionality (e.g., Mosley et al. 2004, Svensson 2003); rather than making aid conditional on reform promises, aid should be allocated based on retrospective performance criteria. In practice, performance-based aid has been increasingly introduced in the health sector (e.g., Eichler and Glassman 2008). However, the most important aid scheme implementing 'new' conditionality has been the Millennium Challenge Corporation (MCC). The Millennium Challenge Corporation, established by the Bush administration in 2004, has been deliberately shaped in such a way as to grant aid as ex post rewards for proven achievements.

In Chapter IV, Axel Dreher, Peter Nunnenkamp and I focus on the effects of the MCC's conditionality on corruption in potential recipient countries of MCC aid. The international aid community has been aware that corruption is a major bottleneck for the effectiveness of aid, and development prospects in general, since the second half of the 1990s (Easterly 2007). Furthermore, corruption features most prominently among the MCC's eligibility criteria. We employ a DDD approach to assess whether the treatment groups fought corruption more effectively than the control groups after the Bush administration announced the creation of the MCC and its performance-based aid allocation approach in 2002. We consider different variants of defining the treatment group as well as different time periods during which incentive effects could have materialized. We find evidence of strong anticipation effects immediately after the announcement of the formation of the MCC, while increasing uncertainty about the timing and amount of MCC aid appears to have weakened the incentive to fight corruption over time.

Another problem impairing aid effectiveness appears to be the fragmentation of aid and the lack of donor coordination (e.g., Easterly 2007, Knack and Rahman 2007, Morss 1984). In

fact, cross-country studies reveal increasing aid fragmentation over the last few decades and a clear lack of coordination among official donors (e.g., Aldasoro et al. 2010, Klasen and Davies 2011), leading to the well-known problems of high transaction costs (e.g., Anderson 2011), administrative burden (e.g., Roodman 2006), public administrations in recipient countries being deprived of their best staff (Knack and Rahman 2007), and blurred responsibilities among donors leading to a tragedy of the commons, moral hazard and free rider problems (Dreher and Michaelowa 2010). However, duplication of aid efforts mainly occurs at the regional and sectoral level within recipient countries.[2] In fact, the Accra Agenda for Action in 2008 has highlighted the importance of within-country division of labor. At the same time, rigorous quantitative analyses of within-country aid coordination are largely missing.

In Chapter V, I evaluate the coordination behavior of donors in the regional-sectoral space within a recipient country, i.e., Cambodia. More precisely, I consider simulatenously the regional *and* sectoral dimension of projects and assess whether donors take active projects by other donors into account when deciding whether to start a project in a given region and sector. The exceptionally comprehensive database on aid projects in Cambodia allows me to consider the whole pool of donors active in the country, including China as an important emerging donor as well as numerous international and Cambodian NGOs. Despite becoming important development actors in the last few decades, donor countries outside the Development Assistance Committee (DAC) of the OECD and NGOs have largely been neglected in donor coordination studies. My results indicate a modest degree of donor coordination within Cambodia, even after the 2005 Paris Declaration. In particular, the coordination efforts among bilateral donors seem rather limited, suggesting that their political and economic interests prevent closer coordination. With respect to the behavior of NGOs, I find them to be mainly active in the same regions and sectors as official donors, creating coordination problems between the two groups of donors. In addition, NGOs appear to cluster in the regional-sectoral space although there seems to be some sort of coordination among them.

[2] Lawson (2010) provides some examples: oversupply of insecticide-treated bed nets in one region, while the people in another region receive none; or geological surveys for a road or water project in a specific region conducted by more than one donor.

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Chapter I:

Financial Dependence and Aid Allocation by Swiss NGOs: A Panel Tobit Analysis

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I.1 Introduction

Foreign aid granted by non-governmental organizations (NGOs) based in OECD donor countries is subject to considerable controversy. Traditional "articles of faith" (Tendler 1982) credit NGOs for being closer to the poor by circumventing governments in the recipient countries and dealing directly with local target groups (Riddell, Bebbington and Peck 1995). Moreover, the World Bank (1998) posited that government-to-government transfers do not work when governance is particularly deficient in the recipient country, and argued that NGOs have a comparative advantage of working in difficult environments. However, some critics suspected in the 1990s already that NGOs might be less autonomous than widely believed. According to Edwards and Hulme (1996, 970), the relations of NGOs with state agencies are "too close for comfort" – with NGOs often becoming "the implementer of the policy agendas" of governments. In particular, the view has come under attack that NGOs have a stronger focus on the poor than state agencies.[1]

Indeed, recent findings indicate that NGOs tend to imitate the allocation of official development assistance (ODA) rather than trying to excel and using their comparative advantages in reaching the poor and working in difficult local environments (see Section 2). Yet empirical evidence on the behavior of NGOs continues to be scarce, notably concerning the question of whether official financial support to NGOs undermines the autonomy of NGOs in allocating aid. Dependence on official financiers may weaken the incentive of NGOs to address entrenched forms of poverty and go where official donors are hardly present. At the same time, government agencies might support those NGOs that allocate aid in line with ODA in the first place. To the best of our knowledge, the present study is the first to assess the link between financial dependence and the allocation of NGO aid systematically.

We draw on an exceptionally large and detailed database on Swiss NGO aid allowing us to evaluate previously untested hypotheses. First of all, we use NGO-specific data on aid allocation across low- and middle-income countries by more than 300 organizations. Second, we distinguish between self-financed and officially co-financed NGO aid; for NGOs relying on official financiers we are thus able to assess whether the allocation of co-financed funds differs from the allocation of own resources. Third, and most importantly, we classify all NGOs according to their financing structure. This renders it possible to assess the much disputed issue of whether the degree of financial dependence is correlated with the allocation of NGO aid.

The structure of the paper is as follows. In Section 2, we refer to the principal-agent model of Fruttero and Gauri (2005) from which we derive several hypotheses concerning the effects of financial

[1] See the references given in Edwards and Hulme (1996); more recent examples include Amin, Rai and Topa (2003). Bebbington (2005, 937) notes that earlier "celebrations meant that inevitably disillusion would follow, and indeed it did."

dependence on the allocation behavior of NGOs. The database on Swiss NGO aid is described in Section 3, which also presents the panel Tobit approach. Empirical results are shown in Section 4. We find that the allocation of Swiss NGO aid is correlated with the allocation of aid by NGO peers as well as the allocation of ODA. Furthermore, officially financed NGOs show a greater tendency to allocate their funds in line with the allocation of ODA. However, the degree of financial dependence is not associated with the poverty orientation of NGO aid and the incentives of NGOs to engage in easier environments.

I.2 Analytical Background and Hypotheses

In contrast to the extensive literature on the allocation of ODA, empirical studies analyzing the allocation of NGO aid are still rare – despite its considerable importance in quantitative terms.[2] Dreher, Mölders and Nunnenkamp (2010) on Sweden, Nunnenkamp, Weingarth and Weisser (2009) on Switzerland, and Nunnenkamp and Öhler (2011) on Germany perform Tobit estimations in which either NGO aid or ODA is the dependent variable. The comparison of the corresponding marginal effects of various explanatory variables, including indicators on the recipients' need for aid and the donors' (political and economic) self-interest, casts into doubt the still widely held belief that the targeting of NGO aid is generally more needs-based than that of ODA. Nancy and Yontcheva (2006) as well as Koch et al. (2009) take a different approach by adding ODA to the list of explanatory variables of NGO aid. In this way, it is tested whether the allocation of NGO aid is correlated with the allocation of ODA. This turns out to be the case for the sample of 61 NGOs from various donor countries in Koch et al. (2009). By contrast, the allocation of aid by European NGOs appears to be unaffected by ODA from the EU, according to Nancy and Yontcheva.

While we build on the approach of the latter two contributions, we offer an important extension by focusing on the role of official financing for the allocation behavior of NGOs. Similar to Edwards and Hulme (1996), Fisher (1997, 451) argues that "while the moniker 'nongovernment organization' suggests autonomy from government organizations, NGOs are often intimately connected with their home governments." However, the critics' attempts to demystify NGO aid have largely in common with the articles of faith of NGO proponents that they have rarely been subjected to empirical scrutiny. This applies especially to the conjecture that financial dependence of NGOs on government funding works against better targeted NGO aid. The principal-agent model of Fruttero and Gauri (2005) offers

[2] For instance, self-financed aid by Swiss NGOs accounted for 32 percent of Switzerland's total bilateral aid to developing countries in 2006 (DCC 2008, 12-13). See Werker and Ahmed (2008) for an analysis explaining the increased presence of NGOs in the last few decades.

important insights on how official co-financing may weaken the incentives of NGOs to excel and compromise their charitable motivations. Several testable hypotheses can be derived from this model.

Fruttero and Gauri (2005) show that the dependence of NGOs (the agents) on external funding (from official financiers as principals) tends to drive a wedge between charitable objectives such as poverty alleviation in the recipient countries and organizational imperatives related to future NGO operations and sustained funding. This happens even if principals and agents share altruistic aid motivations. Principals have incomplete information on NGO projects, while future funding of agents depends on perceived success or failure of current projects. NGOs having to demonstrate success are inclined to avoid locations where "the risk of a failure is so high that it could jeopardize the flow of funding from donors" (Fruttero and Gauri 2005, 761).

Risk aversion could shape NGOs' aid allocation in several respects. First, it weakens their incentive to operate in difficult environments where the probability of failure is particularly high. NGOs facing fiercer competition for funding may rather allocate aid strategically to where success is easier to achieve (see also Bebbington 2004). Second, the poverty orientation of NGO aid may weaken if official financiers insist on immediate results; this is because visible results are easier to present when aid projects address less entrenched forms of poverty. NGOs may thus shift attention away from the neediest recipients. Third, there might be an incentive to locate where other donors are engaged as well. Conformity of location choices is supposed to render it more difficult for principals to assess the performance of individual agents, and may thus help prevent financial sanctions.

Fruttero and Gauri (2005) evaluate the location choices of NGOs empirically at the sub-national level within one particular recipient country, Bangladesh. They find support for several propositions derived from the principal-agent model of officially funded NGO aid. Most importantly, strategic funding considerations appear to have de-linked location choices from indicators of need in local communities. Furthermore, NGOs tended to prefer locations where official service providers were engaged as well. In other words, NGOs hardly specialized by making use of their perceived comparative advantage of working in difficult environments, but rather minimized risk by duplicating efforts of other donors.

As for aid allocation across recipient countries, however, the scarcity of data on the re-financing of NGOs has so far prevented systematic testing of the hypothesis that dependence on official financiers distorts the allocation of NGO aid. Previous studies such as Nancy and Yontcheva (2006) and Koch et al. (2009) failed to differentiate between more and less financially dependent NGOs.[3] Both

[3] The sample of Nancy and Yontcheva (2006) does not include NGOs that are financially independent from official EU financing. The sample of Koch et al. (2009) comprises five (endowed) foundations which do not receive public funds and,

studies have also in common that self-financed NGO aid is not treated separately from officially co-financed aid, even though the allocation of these aid categories may differ from each other. These major shortcomings can be overcome by drawing on the exceptionally detailed database on Swiss NGO aid, described in the next section. These data will then be used to address the proposition of Fruttero and Gauri (2005, 773) that "an NGO might have to undertake pragmatic actions (that is, actions that increase the probability of survival, but that would not be undertaken were the NGO independent of external funding)." Specifically, it will be tested for a large panel of NGOs and recipient countries whether financial dependence is associated with parallel behavior of NGOs with official aid agencies as well as among NGO peers. It will also be tested whether financial dependence is associated with a weaker poverty orientation of NGO aid and weaker incentives for NGOs to work in difficult environments.

I.3 Data and Method

Swiss NGO Aid

The Swiss Agency for Development and Cooperation (French acronym DCC) reports exceptionally detailed data on Swiss NGO aid (DCC [a]). Of particular importance for our analysis, DCC differentiates between major aid channels involving Swiss NGOs. The first channel, labeled "NGO aid proper" in the following, concerns self-financed NGO aid; i.e., Swiss NGOs draw exclusively on their own revenues (notably private donations) to finance aid projects falling into this category. The data on NGO aid proper are based on annual surveys conducted by DCC. Questionnaires were sent to about 500 NGOs; for the years under consideration here (2002-2005), DCC lists 408 NGOs that took part in the survey and reported aid projects abroad, though not necessarily in each year. As stated in the DCC statistics, coverage of NGO aid proper may still be incomplete. DCC may be unaware of some small NGOs and fail to contact these, or NGOs not interested in official co-financing may not feel obliged to provide the required survey information. Nevertheless, the risk of serious underreporting appears to be small, notably compared to most other donor countries lacking comprehensive and officially conducted surveys on NGO aid. The activities of various NGOs contacted by DCC for the annual survey are purely focused on Switzerland.[4] This implies that questionnaires that were not returned do not necessarily result in underreporting of NGO aid. Furthermore, NGOs that joined the DCC's database

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thus, do not have to care about the preferences of official financiers. The results reported by Koch et al. are hardly affected when the five foundations are excluded from the overall sample of 61 NGOs.

[4] In about 12 percent of the returned questionnaires during 2002-2005, the NGOs explicitly stated that they were not operating any aid projects abroad.

only recently are typically fairly small compared to NGOs included in the database since 2002 at least (see also below on the sample underlying our estimations).[5]

The second channel, so-called "contributions," relates to officially co-financed NGO aid. Co-financing can take different forms: So-called program block grants (covering a set of various projects and supporting NGO budgets over 3-4 years) as well as project-specific contributions (of about 30-50 percent) to the overall costs of project proposals designed by NGOs.[6] The third aid channel involves NGOs only as implementing agencies for projects designed and fully financed by official agencies. These so-called "mandates" represent a specific mode of ODA delivery, rather than NGO aid in a strict sense; official agencies award contracts to NGOs typically by means of public tender. In contrast to NGO aid proper and contributions, DCC does not report country-specific ODA delivery through mandates;[7] nor can it be figured out how strongly specific NGOs are involved in ODA delivery through mandates. This data limitation does not pose serious problems for the present analysis. To the contrary, one might even argue that we would bias our results against finding evidence for well targeted NGO aid if mandates were included in assessing the dependence on official financiers. ODA delivery through mandates implies by definition that NGOs allocate (this type of) aid according to the preferences of official donors. This is different from contributions where NGOs decide to apply for official co-financing of *self-designed* projects and programs. Not counting those funds as NGO aid that the official financier explicitly requires to be used for pre-defined projects in a given country thus ensures a "fairer" assessment of the allocation of NGO aid.

We use the data on NGO aid proper and contributions in two complementary ways to analyze whether the allocation behavior of financially dependent Swiss NGOs is systematically different from the allocation of NGOs receiving no official co-financing.[8] First, we use an overall sample of 307

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[5] For example, the average amount of NGO aid proper amounted to 2.6 US$ million in 2005 for all NGOs included in the database since 2002. By contrast, the corresponding average was 270,000 US$ for those NGOs that entered the database in 2005.

[6] The data situation does not allow differentiating between program block grants and project-specific contributions at the level of specific recipient countries or NGOs. Broad indications are that project-specific contributions have declined in importance relative to program block grants (DCC [b], 2004; and e-mail communication with DCC staff).

[7] Some information on the overall importance of ODA delivery through mandates is available from the DCC's Annual Reports (DCC [b]). For instance, official aid agencies concluded mandates in the order of US$ 228 million with Swiss NGOs in 2003-2004, compared to co-financing contributions in the order of US$ 96 million (DCC [b], 2004, Table 11). DCC ([b], 2007, Table 2) reports a similar relation between mandates and co-financing in most recent years.

[8] In the Swiss context, an independent evaluation of DCC's cooperation with NGOs voiced concerns in line with Edwards and Hulme (1996) and Fisher (1997). Accordingly, DCC's "extensive funding" may bode not well for NGOs' actual independence and autonomy (DCC 2004, 13).

Swiss NGOs, including NGOs without any official co-financing.[9] The mean aid disbursed to a country by these NGOs is US$ 6,361 and the maximum is almost US$ 17 million. Note that the median amount of aid is zero, which indicates that most NGOs give aid to few countries only. Specifically, there are 2,059 country-NGO pairs with positive disbursements of aid, out of the total 38,682 observations. The maximum number of countries an NGO gives aid to is 63 (Caritas Switzerland), with an average of 6.7 countries and 2 being the median. 13 countries out of the low- and middle-income countries do not receive aid by any NGO in our sample.[10] Focusing on only those countries that do receive positive amounts of aid by a specific NGO, the average amount of aid is substantially larger compared to the overall sample, US$ 119,499, with a median of US$ 19,751.

In the basic specification for the overall sample of 307 Swiss NGOs, we consider aid from NGO *i* to country *j* as the dependent variable, representing the sum of NGO aid proper and contributions. We also construct the share of contributions in each NGO's aid budget as our measure of dependence on official financiers, by relating the sum of all (country-specific) contributions received by NGO *i* to the total aid budget of NGO *i* (contributions plus NGO aid proper to all recipient countries). The share of contributions is then interacted with several explanatory variables (see below) in order to test whether the impact of these variables on NGO aid depends on the degree of financial dependence.

Second, we focus on the sub-sample of 40 NGOs that actually received official co-financing in 2002-2005. In the estimations performed for this sub-sample, NGO aid proper and contributions enter alternatively as dependent variables. In this way, we can evaluate for those NGOs granting both types of aid whether the correlation between official financing and the allocation of contributions is stronger than the correlation with the allocation of NGO aid proper.

Here as well as in the regressions reported below we use four-year averages of NGO aid for the period 2002-2005.[11] Given the volatility of annual aid flows (Bulir and Hamann 2003; Gupta, Pattillo and Wagh 2006), it is advisable to smooth the aid data. For instance, averaging over several years reduces the impact of business cycles in the donor country. Private donations and, thus, NGO aid proper tend to be negatively affected during recessions. In the case of Switzerland, NGO aid proper

[9] Note that we do not consider the smallest quartile of all 408 NGOs listed by DCC [a]. The excluded quartile accounts for only about 0.1 percent of total Swiss NGO aid. While this reduction of the NGO sample is quantitatively irrelevant, it prevents the failure of the Maximum Likelihood estimations to converge to an optimum.

[10] The 13 countries are Djibouti, Dominica, Grenada, Guyana, Kiribati, Oman, Solomon Islands, St. Kitts and Nevis, St. Lucia, St. Vincent and the Grenadines, Tonga, Vanuatu, and Yemen. Eight of these do also not receive ODA from Switzerland. Our results are not affected if these countries are excluded from the analysis.

[11] For earlier years, it appears impossible to consistently relate (project-specific) contributions to the particular NGO receiving official co-financing.

declined by almost seven percent in 2002 when GDP growth had dwindled to 0.3 percent (from 3.6 percent in 2000). We convert aid flows from Swiss Francs (CHF) into US Dollars, using annual average exchange rates.

Excluding mandates for the reasons stated above, self-financed aid is clearly the dominant form of NGO aid. The annual average of NGO aid proper in 2002-2005 (US$ 276 million) exceeded the annual average of contributions (US$ 46 million) almost six-fold (DCC [a]). While most NGOs in our sample did not receive any contributions, the 40 NGOs with contributions are quantitatively important. They tend to be much larger than NGOs without contributions and accounted for half of total NGO aid in 2002-2005 (Table I.1). The degree of financial dependence varies widely across NGOs with contributions. The share of contributions is below ten percent for seven of the 40 NGOs, whereas it exceeds 70 percent for another seven NGOs. Within the sub-sample, the share of contributions is negatively correlated with NGO size in terms of (self-financed plus officially co-financed) NGO aid, but the correlation coefficient of -0.26 is statistically significant at the ten percent level only. At the same time, the correlation of 0.63 between self-financed NGO aid and contributions is statistically significant at the one percent level.

Explanatory Variables

In line with the previous literature on aid allocation, we include a standard set of possible determinants of NGO aid. First of all, the logged per-capita GDP (purchasing power parity adjusted constant 2000 international US$) of recipient countries provides an indicator of need which has repeatedly been shown to shape the distribution of aid (Berthélemy and Tichit 2004; Berthélemy 2006; Nunnenkamp and Thiele 2006; Dollar and Levin 2006). We expect the marginal effects of per-capita GDP on aid to be significantly negative. Second, we use "Control of Corruption" as presented by Kaufmann, Kraay and Mastruzzi (2007) to measure institutional development, with higher index values indicating less corruption.[12] The effect on NGO aid is ambiguous *a priori*. The argument that NGOs have a comparative advantage to work in difficult environments implies a negative correlation between NGO aid and the control of corruption. As noted in Section 2, however, NGOs may rather prefer environments where success is easier to achieve. Third, we control for (logged) population of recipient countries, which is required as the dependent variable is not in per-capita terms. Fourth, we account for natural disasters, which often motivate emergency aid to recipient countries; the severity of disasters is

[12] As detailed below, we use alternative measures of institutional development in several robustness tests.

proxied by the logged number of people affected.[13] Fifth, we set a dummy variable equal to one for so-called fragile states; fragile states may confront donors with a particularly difficult environment, though one in which aid may provide an effective means of post-conflict resolution (Collier and Hoeffler 2004).

In addition to these variables, we follow Nancy and Yontcheva (2006) as well as Koch et al. (2009) by accounting for ODA as a factor that may affect the allocation of NGO aid.[14] If NGOs mimic the allocation of ODA or official support is granted predominantly to those NGOs with engagements in countries which the official financier prefers, we would expect a significantly positive coefficient on ODA. In contrast to Nancy and Yontcheva (2006) and Koch et al. (2009), we account for possible over-specification related to the inclusion of ODA as explanatory variable. The allocation of ODA has often been shown to depend on the variables just mentioned, in addition to the political and trade-related self-interest official donors may have.[15] Therefore, we first regress Swiss ODA on these aid determinants (see Appendix I.4). The generalized residuals from this Tobit regression,[16] comprising the additional information on ODA that is not explained by the five determinants listed in the previous paragraph, then enter as explanatory variable into the model on NGO aid.[17] When presenting our results we focus on that part of ODA left unexplained by the variables included in our model, but the statistical significance of ODA does not depend on this choice (see below).

Moreover, as noted above, we interact (the residual of) ODA with the share of contributions. This implies that the share of contributions by itself has to be included in the list of explanatory variables. If financially more dependent NGOs show a greater tendency to allocate their funds in line

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[13] Gassebner, Keck and Teh (2010) show that while natural disasters destroy a country's export capacities, their impact on imports depend on the level of democracy. While autocracies have lower levels of imports in the aftermath of disasters, democracies increase their imports, e.g., via increased aid flows.

[14] More precisely, we consider Swiss public aid minus contributions as the latter are included in the former.

[15] Recent studies include Berthelémy (2006), Nunnenkamp and Thiele (2006), Dollar and Levin (2006), Kuziemko and Werker (2006) and Dreher, Sturm and Vreeland (2009a; 2009b).

[16] The generalized residuals are defined as $\frac{1}{\sigma^2}[z_j(y_j - x_j'\beta) - (1-z_j)\sigma\lambda_j]$ with $z_j = 1$ if ODA>0 and $z_j = 0$ if ODA=0, $\lambda_j = \frac{\phi_j(x_j'\beta/\sigma)}{1-\Phi_j(x_j'\beta/\sigma)}$ with ϕ_j indicating the standard normal density function, Φ_j the cumulative standard normal distribution and σ being the standard deviation (Greene 2003, 771). The generalized residuals from the Tobit regression are uncorrelated with the five explanatory variables. Using OLS rather than Tobit to calculate the residuals does not change the results.

[17] In other words, we assume that any variation in the other five explanatory variables influences NGO aid directly and not via ODA.

with ODA, the coefficient on the interaction term should be positive and significant. We also control for a particular NGO's budget, as larger NGOs tend to grant higher amounts of aid to a particular recipient country, all else equal. Finally, we also take account of the possibility that omitted variables affect NGO aid and ODA at the same time by making use of a suitable instrument, as detailed below.

In summary, we specify $NGOaid_{ij}$ (aid from NGO *i* to recipient country *j*) as a function of the following variables:

$NGO\ aid_{ij}$ = f(*Per-capita* GDP_j, *Control of* $corruption_j$, $Population_j$, *People affected by* $disaster_j$, *Fragile* $state_j$, *NGO* $budget_i$, *Residual* ODA_j, *Share of* $contributions_i$, *Residual* ODA_j**Share of* $contributions_i$)

We extend this basic specification in several ways. For instance, we account for the possibility that financial dependence might also affect the impact of country characteristics (per-capita GDP, control of corruption, and whether a country is classified as fragile state) on the allocation of NGO aid. Hence, we also interact the share of contributions in an NGO's aid budget with these characteristics. Furthermore, we assess the incentive of NGOs to allocate aid to where other NGOs are active as well. In other words, the hypothesized conformity of location choices is tested with respect to both official financiers and NGO peers.

Appendix I.1 provides detailed definitions and sources for all variables. Appendices I.2 and I.3 present descriptive statistics and the bivariate correlation matrix, respectively.

Method

A distinguishing feature of our data is that the dependent variable has many zero observations. The clustering of zero observations is due to the fact that most NGOs, especially small NGOs, engage in a limited number of recipient countries; e.g., they may focus on a particular region. As Neumayer (2002) points out, there are basically two options for dealing with the bounded nature of the dependent variable, based on different assumptions. The first option is based on the assumption that donors decide – in the first step – whether to allocate aid to a country at all, while – in the second step – they decide on the amount of aid to be given once recipients are selected. For the first step of this model, Logit (or Probit) is the adequate technique of estimation. Ideally, the second step should take account of information derived from the first step. Employing ordinary least squares estimation (OLS) to the sample of selected countries and including the inverse Mills ratio derived from the first step to account

for selection is the way forward here. The resulting Heckman selection model requires exclusion restrictions on the allocation equation.[18]

In general, it is difficult to find variables which could be argued to be important for selection exclusively. Therefore, Neumayer (2002) suggests OLS as an alternative method of estimation, ignoring the selection bias that tends to result from not considering the inverse Mills ratio. The bias associated with OLS might be moderate when the sample contains a limited number of zero observations. However, the number of zeros in our sample amounts to almost 95 percent. Neither a Heckman selection model nor OLS are thus appealing in our context.

The second option is based on the assumption that the same set of variables determines both whether a country is selected as aid recipient and how much aid is being allocated to that country. Tobit would then be the preferred method. While we mainly use Tobit for the regressions below, we test for the robustness of our results focusing on the selection decision (and employing Logit).[19] We adopt a random effects Tobit approach with Swiss NGOs and aid recipient countries representing the two dimensions of our data:

$$\begin{aligned} y_{ij} &= \max(0, x_{ij}\beta + v_i + u_{ij}) \\ u_{ij} \mid x_i, v_i &\sim Normal(0, \sigma_u^2) \qquad j = 1,...,J \qquad (1) \\ v_i \mid x_i &\sim Normal(0, \sigma_v^2) \end{aligned}$$

where y_{ij} stands for aid from NGO i to recipient country j and x_{ij} refers to the determinants of NGO aid; v_i are the random effects, while u_{ij} is an i.i.d. error term. While a fixed effects Tobit approach is generally biased, we test for the robustness of our results by estimating a Tobit model with country fixed effects (i.e., with dummy variables for each recipient country) below. Since the number of recipient countries is smaller than the number of NGOs an argument could be made that such a specification is consistent (though biased in a small sample).[20]

Note that the coefficient β cannot be interpreted directly in the context of the nonlinear Tobit model. Instead, we are interested in the marginal effects of the explanatory variables on either $P(y_{ij}>0 \mid x_{ij})$, $E(y_{ij} \mid x_{ij}, y_{ij}>0)$ or $E(y_{ij} \mid x_{ij})$. We calculate them below at the mean of the respective covariates.

Given that our model also includes interaction terms, we face an additional complication: Interpreting the interaction effect in nonlinear models (such as Tobit) is not analogous to linear models. As Ai and Norton (2003, 123) point out, "the magnitude of the interaction effect in nonlinear models

[18] Alternatively, the model would have to be identified solely based on the non-linearity inherent in the Probit selection equation.

[19] For a more detailed discussion of methodological issues related to the aid allocation literature, see Neumayer (2002; 2003), Berthélemy (2006) and Kilby (2011).

[20] We thank a referee for pointing this out.

does not equal the marginal effect of the interaction term." It can even be "of opposite sign." Moreover, a simple t-test on the coefficient of the interaction term is not appropriate to test for the significance of the interaction. Rather, we have to calculate the cross derivative in order to test for the significance of the interaction effect (e.g., at the mean of all independent variables). Omitting subscripts, for the marginal effect on $E(y_{ij} \mid x_{ij})$ we obtain:

$$\frac{\partial E(y \mid x)}{\partial x_1 \partial x_2} = \beta_{12}\Phi(\frac{x\beta}{\sigma}) + (\beta_1 + \beta_{12}\bar{x}_2)\phi(\frac{x\beta}{\sigma})\frac{(\beta_2 + \beta_{12}\bar{x}_1)}{\sigma} \quad (2)$$

with ϕ indicating the standard normal density function, Φ the cumulative standard normal distribution, σ being the standard deviation, and $\beta_1, \beta_2, \beta_{12}$ being the coefficients of the two variables forming the interaction and the interaction term, respectively. The x's are the corresponding variables indexed accordingly, while the over bar indicates the mean value at which we calculate the interaction effect. Note that in contrast to linear models, the significance of the interaction effect depends on all variables included in the model.[21]

The marginal effects have to be calculated by building the first derivative of $P(y_{ij}>0 \mid x_{ij})$, $E(y_{ij} \mid x_{ij}, y_{ij}>0)$ or $E(y_{ij} \mid x_{ij})$, respectively. These partial derivatives amount to:

$$\frac{\partial \mathrm{P}(\mathrm{y} > 0 \mid \mathrm{x})}{\partial x_1} = (\frac{\beta_1 + \beta_{12}\bar{x}_2}{\sigma})\phi(\frac{x\beta}{\sigma}), \quad (3)$$

$$\frac{\partial \mathrm{E}(\mathrm{y} \mid \mathrm{x}, \mathrm{y} > 0)}{\partial x_1} = (\beta_1 + \beta_{12}\bar{x}_2)\left\{1 - \lambda(\frac{x\beta}{\sigma})\left[\frac{x\beta}{\sigma} + \lambda(\frac{x\beta}{\sigma})\right]\right\}, \quad (4)$$

$$\frac{\partial \mathrm{E}(\mathrm{y} \mid \mathrm{x})}{\partial x_1} = (\beta_1 + \beta_{12}\bar{x}_2)\Phi(\frac{x\beta}{\sigma}), \quad (5)$$

with λ being the ratio between ϕ and Φ.

Finally, the dependent variables are skewed so that we logged them, following standard practice in large parts of the aid allocation literature. The sample of recipients comprises 126 low- and middle-income countries, and excludes countries with per-capita GDP exceeding US$ 13,000.

I.4 Results

Overall NGO Sample

The interpretation of our results is largely restricted to the overall marginal effects, i.e., the marginal effects of the explanatory variables on $E(y_{ij}|x_{ij})$. The marginal effects of the interaction and the interacted variables (and their corresponding standard errors) are calculated following (2) and (5) above. While we do not show tables reporting the marginal effects according to (3) and (4), we discuss

[21] We calculate the marginal effects using the nlcom command of Stata, version 11.

them in the text for our variables of main interest. Tables I.2 and I.3 report the results for the overall sample of 307 Swiss NGOs. While Table I.2 shows the coefficients of the respective variables, Table I.3 reports the corresponding overall marginal effects.

The basic specification in column (1) is restricted to the standard determinants of aid, in order to be able to compare our results with earlier work. Recall that the present results are based on a panel analysis of NGO-specific aid, whereas previous studies typically consider aggregate NGO aid. Nevertheless, the findings on the standard aid determinants are very similar. Our measure of need for aid – per-capita GDP – turns out to be negative and significant at the one percent level (i.e., as expected, higher-income countries get less aid). Also at the one percent level of significance, the positive coefficient on population signals that larger countries receive more NGO aid. The control of corruption index and the dummy for fragile states are not significant at conventional levels as in Nunnenkamp et al. (2009), indicating that Swiss NGOs do not grant more aid to countries with difficult environments – even though the World Bank (1998) suggests that NGOs may have a comparative advantage to work there. Finally, NGOs grant more aid to recipient countries hit by (more serious) disasters, at the one percent level of significance.

In column (2) we add ODA to the basic specification. As can be seen, NGO aid rises with ODA, at the one percent level of significance. However, the results also show that per-capita GDP becomes insignificant when ODA is included, nicely illustrating why we prefer to purge ODA of its likely determinants and to use residual ODA rather than ODA.

Before turning to the impact of residual ODA, we test for its potential endogeneity with respect to NGO aid. Arguably, even though we purged the original ODA variable from the influence of those variables we control for in the regression, omitted variables might drive both ODA and NGO aid. To formally test for endogeneity, we make use of an instrument that has become standard in the recent political economy literature on aid: a country's voting behavior in the United Nations General Assembly. The empirical literature on political influences shows that developing countries get more aid and better terms from official donors when they have closer political ties with the donor, as measured by their voting behavior in the General Assembly (Thacker 1999; Alesina and Dollar 2000; Vreeland 2005; Barro and Lee 2005; Dreher and Jensen 2007; Kilby 2009; Bjørnskov 2010). Relying on data from Voeten (2004), we code votes in agreement with Switzerland as 1, votes in disagreement as 0, and absences and abstentions as 0.5. We then divide by the total number of votes in a particular year to derive a measure of voting coincidence between zero and one. While related to the amount of Swiss ODA a country receives, there should not be a direct impact of political considerations on NGO aid.

Using the Smith-Blundell procedure to test for endogeneity,[22] we find that – controlling for the other variables in our model – residual ODA is not endogenous to NGO aid (see Appendix I.5).

Turning to the results with residual ODA included, the standard determinants of NGO aid are hardly affected as compared to column (1) when augmenting the specification by our variables of principal interest in columns (3) and (4). In particular, per-capita GDP is significant at the one percent level again. We now also include the NGO's overall budget, which enters with the expected positive coefficient, significant at the one percent level. Swiss ODA and its interaction with the NGO's dependence on official financiers are not independent from the allocation of NGO aid. According to the marginal effects shown in Table I.3, residual ODA is significant at the one percent level at the mean of the independent variables when included individually (column 3) and has the expected positive coefficient. This result holds when calculating the marginal effects according to equations (3) and (4) above (not shown in the table). Calculated at the minimum and, respectively, maximum of residual ODA the overall marginal effect remains significant at the one percent level. Quantitatively, our results imply that an increase in residual ODA by 1 percent increases NGO aid by 0.094 percent, according to column (3). The corresponding increase is 0.02 percent at the minimum value and 0.2 percent at the maximum level of residual ODA.

With the interaction term included (column 4), and calculating the marginal effects in line with equations (2) and (5) above, NGO aid still increases with residual ODA, at the one percent level of significance. The results also show that NGO aid decreases with higher financial dependence, at the five percent level of significance, while the interaction effect between residual ODA and the share of contributions in NGO aid is positive (and also significant at the five percent level). Our results thus corroborate the finding of Koch et al. (2009) that NGOs tend to conform to official donors when deciding on the cross-country allocation of aid. The new insight here is that the degree to which the allocation of NGO aid resembles the allocation of ODA increases considerably with stronger dependence on official financiers. In Figure I.1 we show the marginal effect and significance (with 90 percent confidence intervals) of the interaction depending on the expected value of NGO aid. The impact of the interaction of residual ODA and the share of contributions on the amount of NGO aid is not significant at conventional levels at low values of NGO aid only. For 78 percent of all observations, the marginal effect is significant at the ten percent level at least. The figure also shows that the impact becomes quantitatively more important with rising values of (expected) NGO aid.

Turning to the marginal effect of residual ODA, NGO aid increases by 0.062 percent at the mean of the explanatory variables with an increase of residual ODA by 1 percent. Note that the

[22] See, e.g., Wooldridge (2002, 531).

marginal effects of residual ODA are again equally significant at the one percent level when calculated in line with equations (3) and (4).

The remainder of Table I.3 offers various extensions and tests for robustness. In column (5), we consider the possibility that the similarity of the NGO aid allocation with the allocation of ODA depends not only on the *relative* dependence on official financiers but also on the absolute amount of contributions. Replacing the share of contributions in NGO aid, our preferred measure of financial dependence, by the absolute amount of contributions leaves our results unaffected. Comparing column (5) with the corresponding column (4), the sign as well as the significance level of the interaction effect remain the same.

Next, we account for additional interactions of the share of contributions in NGO aid with other explanatory variables. Note that we keep the interaction with residual ODA in columns (6)-(10) as previous results suggest that dropping this interaction would result in omitted variable bias. We add the interaction of the share of contributions with the dummy for fragile states, the control of corruption index, and per-capita GDP, respectively. The first two interactions are meant to capture the effect financial dependence may have on the NGOs' incentive to operate in easier environments. However, the marginal effects of the interactions are not significant at conventional levels (calculated with equation (2) above). In other words, we do not find evidence supporting the hypothesis that financially dependent NGOs avoid difficult environments in order to secure future official co-financing by demonstrating visible success stories. This result is corroborated when considering alternative measures of institutional development. The marginal effects of the interactions with the share of contributions are all insignificant at conventional levels when the control of corruption index from Kaufmann et al. (2007) is replaced by (i) Law and Order from the International Country Risk Guide (ICRG), (ii) Corruption from the ICRG, (iii) the POLITY IV index of democracy, and (iv) the POLITY IV subindex on executive constraints (both from Marshall and Jaggers 2009).

This is not really surprising once it is taken into account that even NGOs without any official co-financing did not allocate aid according to their perceived comparative advantage of working in difficult environments. The marginal effects on our two standard institutional indicators *per se* continue to be insignificant in columns (6) and (7).[23] As for the alternative institutional indicators, we even find that NGOs are generally more likely to engage in countries with easier environments. The only exception is the law and order index, which carries a negative coefficient suggesting that NGOs are more engaged where this dimension of institutional conditions is relatively weak.

[23] Note that these marginal effects have again been calculated with Stata's nlcom command.

It also remains in columns (6) and (7) that the allocation of NGO aid is correlated with the allocation of ODA. Likewise, previous findings prove to be robust when adding the interaction with per-capita GDP in column (8). At the same time, the insignificant interaction effect suggests that the degree of financial dependence does not affect the poverty orientation of NGO aid.[24]

Finally, we take into account that NGOs may not only behave in conformity with their official financiers but also with NGO peers. In column (9) of Tables I.2 and I.3, we augment the estimation equation by including the number of other Swiss NGOs being active in a recipient country. Taking the *number* of other NGOs as a measure of herding follows directly from the principal-agent model of Fruttero and Gauri (2005) from which we derived our hypotheses on NGO behavior. Fruttero and Gauri argue that the principal's ability to monitor and "determine whom to blame and whom to congratulate for development outcomes" (page 761) is inversely related to the number of agents active in a particular location. Conversely, it is easier for a particular NGO to hide in a larger crowd of peers. Easterly (2002, 245) argues along similar lines and posits that it is "the joint product of the many agents [that] makes it hard to evaluate the efforts of any one agent. … Hence, there is safety in numbers in the foreign aid business."[25]

Similar to ODA before, we account for possible over-specification related to the inclusion of the number of other NGOs as explanatory variable. Therefore, we consider the residuals from a first-stage OLS regression with the number of NGOs as the dependent variable (see Appendix I.6).[26]

According to the results, and in line with the principal-agent model of Fruttero and Gauri (2005), the significant and positive marginal effect of the residual number of other NGOs present indicates that NGOs grant more aid to where their peers are engaged as well, controlled for the usual determinants of location choice. Arguably, conformity of location choices tends to render it more difficult for principals to assess and sanction individual agents, increasing the incentive to go where others already are. But the results in column (10) of Table I.3 reveal that the incentive to hide in the NGO crowd does not depend on the relative importance of official financing. In quantitative terms, an

[24] Note, however, that the insignificant interaction with per-capita GDP may be due to the extremely strong correlation between the share of contributions and the interaction term.

[25] See below for a robustness test with the *amount* of aid from other NGOs as an alternative proxy of herding among NGOs.

[26] In a previous version of the paper we checked for potential endogeneity of the number of other NGOs using the degree of linguistic fractionalization in the recipient country as an instrument. Switzerland is a highly fragmented country, with four official languages. We argued that a linguistically fractionalized recipient country will attract a larger number of Swiss NGOs. Most obviously perhaps, recipient countries where at least part of the population is French speaking can be expected to attract more NGOs from French speaking Swiss cantons. Given that the theoretical justification for this instrument is comparably weak, we no longer use it here. It may be noted, however, that when we use it – and control for the other determinants of NGO aid – we do not find that the number of NGOs is endogenous to NGO aid.

increase in the residual number of NGOs present in a particular country by one increases NGO aid by 0.003 percent. This result holds when using the number of NGOs, rather than the residual, at the one percent level of significance. Note also that all major results on other determinants of NGO aid remain.

Alternative Methods and Specifications

In Table I.4 we test for the robustness of our main results with respect to the method of estimation. In columns (1) and (2) we focus on the selection of recipient countries rather than the amount of aid as dependent variable. As can be seen from the random effects Logit model reported in column (1), the results are very similar in terms of the direction of the coefficients and their statistical significance. When we calculate the marginal effect of an increase in residual ODA by one percent, we find that the probability to be selected as recipient of NGO aid increases by 0.01 percent, at the mean of the variables. Conditional on official co-financing, the probability to receive aid by a particular NGO increases by 0.01 when the NGO does not receive any official funds (with the share of contributions equal to zero) and 0.016 when the NGO receives equal amounts of private and official funds (with the share of contributions equal to 0.5).[27]

In column (2) we estimate a conditional NGO fixed effects Logit model rather than a random effects Logit model and cluster standard errors at the country level. With the exception of disasters, our control variables are no longer significant at conventional levels. The share of contributions and NGO budgets do not vary within NGOs. They thus have to be omitted and their effects are captured by the fixed NGO effects. Importantly, the coefficients of the interaction terms are almost identical compared to the random effects specification in column (1). Even though their standard errors are slightly higher, this makes us confident about the robustness of our results.[28]

In column (3) we estimate a Tobit model including country fixed effects, with standard errors again clustered at the country level. With the exception of the budget of the NGOs, the share of contributions, and the interaction of the share with residual ODA as well as with the residual number of NGOs, all variables do not vary at the country level and are therefore collinear with the country dummies. They thus have to be omitted from the regression. In order to calculate the marginal effect,

[27] It is now interesting to see whether the marginal effect of residual ODA in the Tobit regressions is largely driven by its effect on selection. We therefore calculated the marginal effects according to equations (3) and (4) above. Based on the specification of column (10) in Table II.2 we find that residual ODA and the interaction with the share of contributions are relevant for both selection and the amount of aid conditional on being selected in the first place. Specifically, the marginal effect of an increase in ODA by one percent at the mean of the explanatory variables amount to 0.02 percent in the selection equation and 0.23 percent for the amount of aid. The detailed results are available on request.

[28] Again, the coefficients and t-values of the interaction terms cannot directly be interpreted (Ai and Norton 2003).

we would need the coefficient of residual ODA and that of its interaction with the share of contributions. Given that the level of ODA is captured by the fixed effects we cannot properly calculate its marginal effects when the country dummies are included (and residual ODA is dropped as a consequence). The same holds for the residual number of other NGOs present in a country. As can be seen, the coefficients and standard errors are very similar to those reported in column (4) of Table I.2. Even though we cannot calculate the marginal effects for residual ODA and the residual number of NGOs for the reasons stated above, this gives us further confidence in the robustness of our results.[29]

In column (4), we measure the engagement of other NGOs in a particular country by the *amount* of their aid, rather than their number. This measure may also capture herding of NGOs, e.g., when smaller NGOs engage where large and prominent peers are active already. In contrast to the number of NGOs, however, the amount of aid mainly reflects the clustering of NGO activity at the country level, which may be high even if just a few large NGOs are active. Barr and Fafchamps (2006) argue that the marginal benefit from the engagement of an additional NGO falls with the amount of aid a country receives from other NGOs. NGOs exclusively interested in the welfare gains of recipient countries would thus engage where NGO aid is less clustered so far. However, their empirical analysis of location choice by (local) NGOs within Uganda points to "excessive geographical clustering." Similarly, we find clustering among Swiss NGOs across recipient countries of NGO aid. The results of column (4) are much in line with those shown previously.[30]

Officially Co-financed NGOs

We now turn to the sub-sample of 40 Swiss NGOs that actually received official co-financing, though to widely different degree (see Section 3). The specification of the Tobit models is as before except that we do not report all of the previous extensions. We replicate the estimations for total aid granted by the sub-sample of officially co-financed NGOs in columns (1)-(5) of Table I.5.[31] More interestingly, however, we perform separate estimations by distinguishing between the two types of NGO aid, i.e., self-financed NGO aid proper in columns (6)-(10) and official contributions in columns (11)-(15).[32]

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[29] When we replicate this model with OLS (clustering the standard errors at the country level) our results are also qualitatively unchanged.

[30] Note that the marginal effect of the interaction between total NGO aid and the share of contributions is not significant at conventional levels.

[31] To reduce clutter we only report the overall marginal effects at the mean here; the coefficients of all explanatory variables are available on request.

[32] When we estimate fixed effects Tobit models by including dummy variables for each NGO instead, the coefficients of the variables of interest and their standard errors are almost identical. These additional results are available on request.

In several respects, the results for total NGO aid in Table I.5 are fairly similar to those reported before for the overall sample of 307 Swiss NGOs. Once again, the effects of population, the NGOs' budgets, and the severity of disasters are positive and highly significant. Poorer countries still get more aid, even though the level of significance is lower than in Table I.3. The insignificance of the control of corruption index and the dummy for fragile states reveals that the institutional environment prevailing in the recipient countries did not shape the allocation of aid by the sub-sample of NGOs, in line with the findings for the overall sample. There is also a close resemblance of findings in that NGOs generally tend to conform to the allocation of ODA and go where other NGOs are present. In contrast to the full sample, it appears that the conformity with the state no longer strengthens with stronger dependence on public co-financing. The interaction of the share of contributions with residual ODA turns insignificant in column (3). However, when calculating the effect of the interaction for each individual observation rather than at the mean, the effect is still significant, at the ten percent level at least, for almost 40 percent of the observations. The interaction with the residual number of NGOs turns out to be negative and significant at the ten percent level in column (5) of Table I.5.

The effect of the interaction between the share of contributions and ODA may weaken in the reduced sample as the "dependency syndrome" (DCC 2004, 59) results at least partly from accepting public co-financing at all, and not only from co-financing contributing a large share to the NGO's overall budget. At the same time, the interaction effects may work in opposite directions for the two types of aid in the sub-sample of NGOs. This possibility is addressed next by raising the question of whether officially co-financed NGOs allocate self-financed aid differently from the contributions of official financiers.

Indeed, the allocation of NGO aid proper differs in several respects from the allocation of contributions.[33] The evidence is mixed on whether NGOs are more inclined to allocate contributions to countries offering an easier environment. In conflict with this proposition, the poverty orientation of contributions does not appear to be consistently weaker than that of NGO aid proper. On the other hand, the effect of the control of corruption index is significant and positive in columns (12)-(15) for contributions, while the two institutional indicators remain insignificant for NGO aid proper. Recalling that higher values of the control of corruption index reflect more advanced institutions, it appears that the allocation of contributions is biased towards easier (institutional) environments.[34] It remains open to

[33] By contrast, the results for NGO aid proper are very close to those for total NGO aid. This is not surprising recalling that NGO aid proper accounts for the bulk of total NGO aid even in the sub-sample of officially co-financed NGOs. Note also that total NGO aid and NGO aid proper are highly correlated, while the correlation with contributions is relatively weak (Appendix 3).

[34] This does not hold for the dummy of fragile states, however.

debate, however, if this is due to NGOs attempting to secure future financing by demonstrating success stories. The same bias could result from official financiers using the co-financing of NGOs as a means to channel aid to well-governed recipient countries. In this way, aid agencies such as DCC may have circumvented mission statements that require them to engage primarily in less benign environments in order to actively fight problems of corruption.

Regarding the allocation of contributions, we find strong evidence that NGO aid goes to where ODA also is. Calculating the elasticity of contributions with respect to residual ODA reveals an elasticity of 0.18 percent at the mean of the variables (column 12). In addition, the similarity of the allocation of NGO aid with that of ODA and that of other NGOs increases with the relative importance of official financing. According to columns (13) and (15) of Table I.5, the interaction of the share of contributions with both residual ODA and, respectively, the residual number of NGOs turns out to be significantly positive at the five percent level, calculated at the mean of the other variables, with a marginal effect of 0.242 for the interaction term with residual ODA in column (13). Figure I.2 shows the marginal effects of the interaction corresponding to the model shown in column (13) – with 90 percent confidence intervals – depending on the expected value of the contributions. For almost 80 percent of the observations, the marginal effect is significant at the ten percent level at least. The figure also shows that the quantitative impact does not systematically depend on the (expected) values of contributions.

Herding behavior can also be observed when it comes to the allocation of NGO aid proper. The overall marginal effects of residual ODA as well as the residual number of NGOs are significantly positive at the one percent level. In contrast to contributions, however, the degree to which NGOs depend on official refinancing has no say on the strength of the correlation with the allocation of ODA, according to column (8) of Table I.5. The interaction of the share of contributions with the residual number of NGOs turns out to be negative, at the five percent level of significance, for NGO aid proper (column 10).

In particular the latter finding suggests that the allocation of NGO aid tends to be affected not only by their dependence on official financiers but also by the competition for private donations. Note that NGOs with less official refinancing are under fiercer pressure to raise a sufficient amount of private donations. Arguably, private donations respond to visible success stories – i.e., obviously successful NGO projects – in essentially the same way as does official refinancing, especially when the allocation of NGO aid proper is concerned. In other words, principal-agent problems are not confined to official financiers. This would explain that the effects of the interaction between the residual number of NGOs and the share of contributions work in opposite directions for the two types of NGO aid. Yet

both types of aid would still have in common that herding of NGOs tends to reduce the risk of future financing.

I.5 Summary and Conclusion

While NGO aid has gained considerable importance in quantitative terms, it is increasingly disputed whether the allocation of NGO aid is superior to that of ODA. Principal-agent models suggest that NGOs have incentives to follow official donors and NGO peers, rather than trying to excel and swim against the tide. However, empirical studies systematically evaluating the allocation of NGO aid are still rare – in contrast to the extensive literature on the allocation of ODA.

To help closing this gap we draw on the exceptionally rich, though largely ignored, data on aid granted by Swiss NGOs. We contribute in three important ways to the nascent literature on NGO aid. First, we perform panel Tobit estimations covering more than 300 NGOs and essentially all aid recipient countries. Second, we distinguish between self-financed NGO aid and officially co-financed NGO aid. Third, and most importantly, we classify all NGOs according to their financial dependence on government support. This allows us to address the unresolved issue of whether dependence on official financiers tends to erode the distinctive characteristics that the allocation of NGO aid may have compared to the allocation of ODA, and to induce herding among NGO peers.

We find that the allocation of NGO aid is generally much in line with the allocation of ODA. At the same time, the allocation of NGO aid is more similar to that of ODA for NGOs receiving official co-financing. The finding that aid from financially dependent NGOs and ODA tend to be channeled to the same recipients is robust to changes in the specification of the estimation equation for the full sample. It is almost impossible, however, to isolate two possible explanations for the parallel behavior of NGOs and official agencies. On the one hand, the government may put pressure on the NGOs receiving financial support to change their allocation in line with the government's preferences. On the other hand, the government may select those NGOs with similar preferences in the first place. In either case, our results provide little evidence that the allocation of NGO aid is superior to the allocation of ODA.[35] We also find support for the view that NGOs tend to locate where their peers are active, probably because conformity of location choices renders it more difficult for official financiers to assess and sanction individual NGOs.

In contrast to what one might suspect, financial dependence did not impair the poverty orientation of NGO aid. Neither did we find evidence that financially dependent NGOs have weaker incentives to engage in difficult environments. This seems to be largely because Swiss NGOs were

[35] We thank a reviewer for pointing this out.

generally reluctant to go where ODA is widely supposed to fail due to particularly weak institutions and deficient governance. There are some indications, however, that the allocation of one particular type of NGO aid, namely officially co-financed aid, is biased towards recipient countries with less corruption. Finally, it is this type of NGO aid for which a higher degree of financial dependence is associated with a considerably stronger similarity between the allocation of NGO aid and that of ODA.

Taken together, our findings caution against the view that aid would be better targeted to the needy and deserving if only NGOs had more resources at their disposal. In particular, providing NGOs with more public resources by officially co-financing NGO aid may make little difference to raising ODA directly. The Swiss example suggests that NGOs are unlikely to swim against the tide especially when accepting official co-financing. NGOs might better do without public financial support if they really want to distinguish themselves from other donors. This is not to ignore, however, that the fierce competition for private donations may also give rise to principal-agent problems.

Of course, Switzerland is too small as a donor to come up with general verdicts on the allocation of NGO aid. This applies all the more so to the effectiveness of NGO aid in promoting the economic and social development of recipient countries, compared to the effectiveness of ODA. It would thus be desirable to perform similar case studies on NGO aid for other donor countries, especially quantitatively more important donors such as the United States, France, Germany or the United Kingdom. Other possible extensions of the present analysis include, e.g., assessing the location choices of NGOs at a finer geographical level *within* recipient countries and adding a time dimension to the analysis. It remains to be seen whether NGOs help overcome serious data constraints and support future research along these lines by opening their books on aid allocation and the structure of financing.

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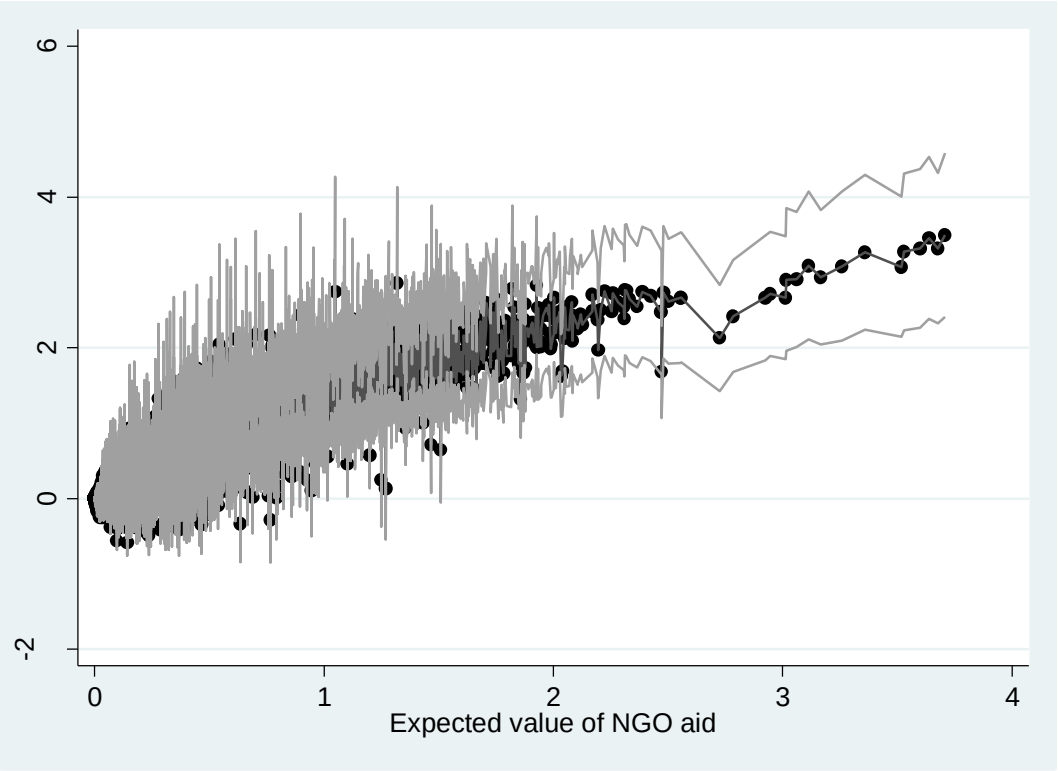
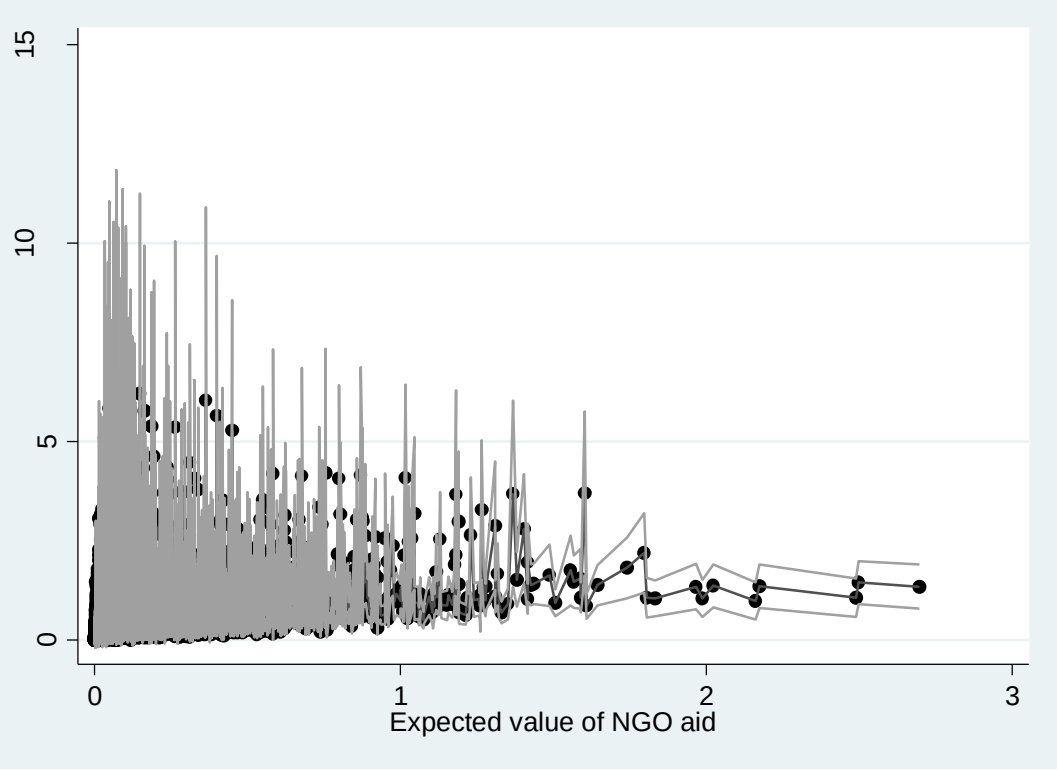
Figure I.1: Marginal effects of the interaction *ODAresid*Share*



Notes: Shows the effect of the interaction between *ODAresid* and *Share* (column 4 of Table I.3). Each dot represents the marginal effect for one observation. Also shown is the 90 percent confidence interval for each marginal effect.

Figure I.2: Marginal effects of the interaction *ODAresid*Share*, Contributions



Notes: Shows the effect of the interaction between *ODAresid* and *Share* (column 13 of Table I.4). Each dot represents the marginal effect for one observation. Also shown is the 90 percent confidence interval for each marginal effect.

Table I.1: Swiss NGO aid, sample characteristics

	Officially co-financed NGOs		Other NGOs (Share = 0)
	NGO aid	Share of contributions	NGO aid
Median	1166	0.234	62
Mean	3068	0.355	462
Std. dev.	4893	0.281	1712
Number NGOs	40		267
Share in total NGO aid (%)	49.9		50.1

Note: NGO aid is the sum of self-financed and officially co-financed NGO aid (in 1000 US$). The share of contributions is the relation of contributions to NGO aid.

Source: DCC [a].

Table I.2: Total sample of Swiss NGOs: panel Tobit results, coefficients, total NGO aid

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Per-capita GDP	-0.469***	-0.059	-0.542***	-0.537***	-0.534***	-0.537***	-0.536***	-0.524***	-0.412***	-0.413***
	(0.093)	(0.099)	(0.094)	(0.093)	(0.093)	(0.093)	(0.093)	(0.097)	(0.090)	(0.090)
Population	0.959***	0.631***	1.119***	1.113***	1.105***	1.113***	1.113***	1.113***	0.920***	0.918***
	(0.051)	(0.056)	(0.056)	(0.055)	(0.055)	(0.055)	(0.055)	(0.055)	(0.052)	(0.052)
People affected by disaster	0.143***	0.152***	0.141***	0.139***	0.139***	0.139***	0.139***	0.139***	0.116***	0.116***
	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.021)	(0.021)
Control of corruption index	-0.144	0.228	0.009	-0.001	0.001	-0.001	-0.057	-0.001	-0.238	-0.227
	(0.163)	(0.164)	(0.162)	(0.160)	(0.160)	(0.160)	(0.169)	(0.160)	(0.160)	(0.159)
Fragile state, dummy	0.136	0.337	0.126	0.136	0.127	0.087	0.137	0.136	0.148	0.150
	(0.205)	(0.205)	(0.204)	(0.202)	(0.202)	(0.214)	(0.202)	(0.202)	(0.197)	(0.197)
ODA		0.463***								
		(0.037)								
ODA, residual			2.534***	2.183***	2.139***	2.183***	2.185***	2.180***	1.704***	1.703***
			(0.206)	(0.213)	(0.224)	(0.213)	(0.213)	(0.213)	(0.198)	(0.198)
NGO budget				1.431***	1.392***	1.431***	1.431***	1.431***	1.429***	1.431***
				(0.079)	(0.089)	(0.079)	(0.079)	(0.079)	(0.079)	(0.078)
Share of contributions				-2.334**		-2.470***	-1.917*	-0.551	-2.329***	-2.963***
				(0.906)		(0.929)	(0.981)	(3.987)	(0.903)	(0.942)
Residual ODA* Share				6.045***		6.093***	5.898***	6.125***	5.641***	5.693***
				(1.299)		(1.307)	(1.297)	(1.315)	(1.254)	(1.273)
Sum of contributions					-0.016					
					(0.073)					
Residual ODA* Sum					0.250***					
					(0.068)					
Share * Fragile state						0.820				

						(1.159)				
Share * Control of corruption							0.898			
							(0.832)			
Share * Per-capita GDP								-0.229		
								(0.500)		
Number of NGOs, residual									0.105***	0.100***
									(0.005)	(0.005)
Residual Number of NGOs * Share										0.090***
										(0.031)
Constant	-23.562***	-24.629***	-25.535***	-32.127***	-31.915***	-32.125***	-32.161***	-32.224***	-29.715***	-29.646***
	(1.118)	(1.141)	(1.161)	(1.254)	(1.262)	(1.254)	(1.255)	(1.273)	(1.209)	(1.207)
Observations	38682	38682	38682	38682	38682	38682	38682	38682	38682	38682
Number of NGOs	307	307	307	307	307	307	307	307	307	307

Note: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table I.3: Total sample of Swiss NGOs: panel Tobit results, overall marginal effects, total NGO aid

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Per-capita GDP	-0.019***	-0.002	-0.020***	-0.014***	-0.014***	-0.014***	-0.014***	-0.014***	-0.010***	-0.010***
	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)
Population	0.039***	0.024***	0.042***	0.028***	0.028***	0.028***	0.028***	0.028***	0.022***	0.022***
	(0.003)	(0.003)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
People affected by disaster	0.006***	0.006***	0.005***	0.004***	0.004***	0.004***	0.004***	0.004***	0.003***	0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Control of corruption index	-0.006	0.009	0.000	-0.000	0.000	-0.000	-0.000	-0.000	-0.006	-0.005
	(0.007)	(0.006)	(0.006)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Fragile state, dummy	0.006	0.013	0.005	0.004	0.003	0.003	0.004	0.003	0.004	0.004
	(0.009)	(0.008)	(0.008)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
ODA		0.017***								
		(0.002)								
ODA, residual			0.094***	0.062***	0.060***	0.062***	0.062***	0.062***	0.047***	0.047***
			(0.010)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)
NGO budget				0.036***	0.036***	0.036***	0.036***	0.036***	0.034***	0.034***
				(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Share of contributions				-0.059**		-0.060**	-0.058**	-0.060**	-0.056**	-0.071***
				(0.023)		(0.024)	(0.023)	(0.024)	(0.022)	(0.024)
Residual ODA* Share				0.092**		0.093**	0.090**	0.093**	0.088***	0.076**
				(0.038)		(0.038)	(0.038)	(0.038)	(0.034)	(0.034)
Sum of contributions					-0.000					
					(0.002)					
Residual ODA* Sum					0.006**					
					(0.003)					
Share * Fragile state						0.018				

						(0.030)				
Share * Control of orruption							0.023			
							(0.022)			
Share * Per-capita GDP								0.008		
								(0.013)		
Number of NGOs, residual									0.003***	0.003***
									(0.000)	(0.000)
Residual Number of NGOs * Share										-0.001
										(0.001)
Observations	38682	38682	38682	38682	38682	38682	38682	38682	38682	38682
Number of NGOs	307	307	307	307	307	307	307	307	307	307

Note: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table I.4: Total sample of Swiss NGOs: test for robustness, coefficients, total NGO aid

	(1) RE Logit	(2) Conditional Logit	(3) Country FE Tobit	(4) RE Tobit
Per-capita GDP	-0.171***	-0.171		-0.591***
	(0.037)	(0.724)		(0.091)
Population	0.360***	0.360		1.573***
	(0.021)	(0.369)		(0.070)
People affected by disaster	0.052***	0.052*		0.079***
	(0.009)	(0.028)		(0.022)
Control of corruption index	-0.095	-0.098		-0.063
	(0.068)	(0.620)		(0.155)
Fragile state, dummy	0.066	0.066		0.105
	(0.082)	(0.572)		(0.199)
ODA, residual	0.586***	0.638*		2.948***
	(0.036)	(0.381)		(0.227)
NGO budget	0.636***		1.584***	1.431***
	(0.083)		(0.001)	(0.078)
Share of contributions	-1.310***		-4.117***	-2.608***
	(0.451)		(0.086)	(0.917)
Residual ODA* Share	2.398***	2.397**	5.898***	5.857***
	(0.561)	(0.935)	(0.150)	(1.264)
Number of NGOs, residual	0.040***	0.040***		
	(0.002)	(0.004)		
Residual Number of NGOs * Share	0.025**	0.022	0.073***	
	(0.013)	(0.014)	(0.005)	
Total NGO aid, residual				2.872***
				(0.174)
Total NGO aid * Share				1.693*
				(0.986)
Constant	-11.686***		-47.233***	-38.784***
	(0.465)		(0.005)	(1.410)
Observations	38682	38682	38682	38682
Number of NGOs	307	307	307	307

Note: Standard errors in parentheses (clustered by country in columns (2) and (3));
*** p<0.01, ** p<0.05, * p<0.1.

Table I.5: Sub-sample of officially co-financed NGOs: panel Tobit results, overall marginal effects

	Total NGO aid					NGO aid proper				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Per-capita GDP	-0.042*	-0.046**	-0.027**	-0.019*	-0.019*	-0.041*	-0.044**	-0.026**	-0.018*	-0.018*
	(0.023)	(0.021)	(0.012)	(0.011)	(0.010)	(0.021)	(0.020)	(0.011)	(0.010)	(0.010)
Population	0.100***	0.111***	0.068***	0.053***	0.052***	0.095***	0.105***	0.063***	0.050***	0.049***
	(0.020)	(0.022)	(0.009)	(0.008)	(0.008)	(0.019)	(0.021)	(0.008)	(0.007)	(0.007)
People affected by	0.021***	0.019***	0.011***	0.009***	0.009***	0.019***	0.017***	0.010***	0.008***	0.008***
disaster	(0.006)	(0.006)	(0.003)	(0.003)	(0.003)	(0.006)	(0.005)	(0.003)	(0.002)	(0.002)
Control of corruption index	0.022	0.037	0.021	0.016	0.016	0.020	0.034	0.019	0.014	0.014
	(0.037)	(0.033)	(0.019)	(0.018)	(0.017)	(0.035)	(0.031)	(0.018)	(0.017)	(0.016)
Fragile state, dummy	0.043	0.044	0.030	0.029	0.029	0.039	0.039	0.027	0.026	0.025
	(0.052)	(0.046)	(0.028)	(0.026)	(0.026)	(0.048)	(0.043)	(0.026)	(0.024)	(0.024)
ODA, residual		0.378***	0.267***	0.219***	0.213***		0.353***	0.245***	0.200***	0.196***
		(0.078)	(0.044)	(0.038)	(0.038)		(0.074)	(0.041)	(0.035)	(0.035)
NGO budget			0.100***	0.094***	0.092***			0.093***	0.087***	0.086***
			(0.014)	(0.013)	(0.013)			(0.013)	(0.012)	(0.012)
Share of contributions			-0.164**	-0.158**	-0.176***			-0.168***	-0.163***	-0.177***
			(0.066)	(0.063)	(0.063)			(0.062)	(0.059)	(0.060)
Residual ODA* Share			0.046	0.022	-0.015			-0.001	-0.021	-0.048
			(0.134)	(0.118)	(0.117)			(0.124)	(0.108)	(0.108)
Number of NGOs, residual				0.007***	0.007***				0.006***	0.006***
				(0.001)	(0.001)				(0.001)	(0.001)
Residual Number of NGOs * Share					-0.006*					-0.006**
					(0.003)					(0.003)
Observations	5040	5040	5040	5040	5040	5040	5040	5040	5040	5040
Number of NGOs	40	40	40	40	40	40	40	40	40	40

Table I.5 (continued)

	Contributions				
	(11)	(12)	(13)	(14)	(15)
Per-capita GDP	-0.030**	-0.028**	-0.016**	-0.012**	-0.012**
	(0.015)	(0.012)	(0.006)	(0.005)	(0.005)
Population	0.028***	0.033***	0.019***	0.009***	0.009***
	(0.008)	(0.008)	(0.004)	(0.003)	(0.003)
People affected by disaster	0.013***	0.010***	0.005***	0.004***	0.004***
	(0.004)	(0.003)	(0.002)	(0.001)	(0.001)
Control of corruption index	0.037	0.036*	0.020*	0.018**	0.018**
	(0.025)	(0.019)	(0.010)	(0.009)	(0.009)
Fragile state, dummy	0.018	0.020	0.014	0.013	0.013
	(0.034)	(0.026)	(0.016)	(0.014)	(0.014)
ODA, residual		0.177***	0.107***	0.071***	0.070**
		(0.039)	(0.039)	(0.027)	(0.027)
NGO budget			0.034***	0.030***	0.030***
			(0.005)	(0.004)	(0.004)
Share of contributions			0.045*	0.039*	0.035
			(0.026)	(0.022)	(0.023)
Residual ODA* Share			0.242**	0.183**	0.174**
			(0.089)	(0.071)	(0.071)
Number of NGOs, residual				0.003***	0.003**
				(0.000)	(0.001)
Residual Number of NGOs * Share					0.003**
					(0.002)
Observations	5040	5040	5040	5040	5040
Number of NGOs	40	40	40	40	40

Note: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Appendix I.1: Variable description and sources

Variable	Description	Source
NGO aid	Sum of (self-financed) NGO aid proper and (officially co-financed) contributions; natural logs of 1 + the original values; average for 2002-2005.	DCC, various issues; see also: DCC online statistics, http://www.deza.ch/en/Home/documentation (accessed: October 2008)
NGO aid proper	Self-financed NGO aid; natural logs of 1 + the original values; average for 2002-2005.	DCC, various issues; DCC online statistics
NGO budget	NGO aid proper plus contributions to all recipient countries; natural logs of 1 + the original values; average for 2002-2005.	DCC, various issues; DCC online statistics
Contributions	Official co-financing of projects and programs of Swiss NGOs; natural logs of 1 + the original values; average for 2002-2005.	DCC, various issues; DCC online statistics
Share of contributions	Sum of contributions over all recipient countries, relative to total NGO aid.	DCC, various issues; DCC online statistics
Sum of contributions	Sum of contributions to a specific NGO's budget over all recipient countries.	DCC, various issues; DCC online statistics
ODA	Swiss public aid minus contributions; natural logs of 1 + the original values; average for 2002-2005.	DCC, various issues; DCC online statistics
ODA, residual	Generalized residuals of a Tobit regression of Swiss ODA (excluding contributions) on GDP, Population, Corruption, Fragile.	
Number of NGOs	Number of other NGOs engaged in a country.	DCC, various issues; DCC online statistics
Total NGO aid	NGO aid by other NGOs engaged in a country.	DCC, various issues; DCC online statistics
Per-capita GDP	Per-capita GDP at constant 2000 US$, PPP adjusted; natural logs; average for 1997-2001.	World Bank, World Development Indicators 2006
Population	Population in natural logs; average for 1997-2001.	World Bank, World Development Indicators 2006
People affected by disaster	Number of people affected by disasters; natural logs; average for 1997-2001.	International Disaster Database, http://www.em-dat.net/ (accessed: November 2008)
Control of corruption index	Index ranging from -2.5 to 2.5 with higher values indicating less corruption; average for 1996-2000.	Kaufmann, Kraay and Mastruzzi (2007)
Fragile state, dummy	Dummy = 1 for countries with CPIA of 3.0 or below in 2005.	World Bank's Country and Institutional Assessment (CPIA)
UN voting	Voting coincidence between Switzerland and aid recipient countries in the United Nations General Assembly; average for 2002-2005.	Voeten (2004)

Note that NGO aid, NGO aid proper, contributions and the number of (other) NGOs are specific for each NGO *i* and recipient country *j*.

Appendix I.2: Descriptive statistics, total sample of Swiss NGOs

	Obs.	Mean	Std. Dev.	Min	Max
NGO aid (in 1,000 US$)	38682	6.36	106.15	0	16798.09
NGO aid proper (in 1,000 US$)	38682	5.54	102.10	0	16798.09
Contributions (in 1,000 US$)	38682	0.82	16.58	0	806.13
Per-capita GDP (constant 2000 US$)	126	4167.11	3192.90	491.63	16807.28
Population (number)	126	38000000	143000000	42705	1250000000
People affected by disaster	126	8.98	4.33	0	18.19
Control of corruption index	126	-0.41	0.51	-1.68	1.34
Fragile state, dummy	126	0.13	0.34	0	1
UN voting	126	0.70	0.07	0.51	0.88
ODA (in 1,000 US$)	126	4141.33	5538.44	0	24271.64
ODA, residual	126	0	0.41	-1.46	0.94
NGO budget (in 1,000 US$)	307	801.46	2520.35	8.72	21561.47
Share of contributions	307	0.05	0.17	0	0.91
Sum of contributions (in 1,000 US$)	307	107.95	564.56	0	6748.94
Number of NGOs	126	17	15.88	0	83
Number of NGOs, residual	126	0	11.19	-22.30	39.39
Total NGO aid (in 1,000 US$)	126	1948.59	2936.07	0	19082.75
Total NGO aid,_residual	126	0	0.60	-2.94	1.71

Appendix I.3: Correlations between dependent and independent variables, total sample of Swiss NGOs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)	(25)
(1) NGO aid	1.00																								
(2) NGO aid proper	1.00	1.00																							
(3) Contributions	0.46	0.42	1.00																						
(4) per-capita GDP	-0.05	-0.05	-0.02	1.00																					
(5) Population	0.14	0.14	0.04	-0.23	1.00																				
(6) People affected by disaster	0.11	0.11	0.04	-0.28	0.55	1.00																			
(7) Control of corruption index	-0.03	-0.03	-0.01	0.53	-0.16	-0.11	1.00																		
(8) Fragile state, dummy	0.00	0.00	0.00	-0.34	-0.07	-0.05	-0.28	1.00																	
(9) UN voting	0.00	0.00	-0.01	0.34	0.20	0.04	0.26	-0.28	1.00																
(10) ODA	0.12	0.12	0.04	-0.42	0.67	0.40	-0.29	0.03	0.18	1.00															
(11) ODA, residual	0.03	0.03	0.02	0.00	0.00	0.00	0.00	0.00	0.25	0.68	1.00														
(12) NGO budget	0.26	0.26	0.13	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	1.00													
(13) Share of contributions	0.04	0.03	0.12	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.23	1.00												
(14) Sum of contributions	0.17	0.16	0.22	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.49	0.73	1.00											
(15) Number of NGOs	0.20	0.20	0.08	-0.26	0.66	0.53	-0.16	-0.02	0.03	0.59	0.15	0.00	0.00	0.00	1.00										
(16) Number of NGOs, residual	0.13	0.13	0.06	0.00	0.00	0.00	0.00	0.00	-0.13	0.02	0.00	0.00	0.00	0.00	0.70	1.00									
(17) Total NGO aid	0.14	0.14	0.05	-0.34	0.74	0.49	-0.22	0.03	0.11	0.77	0.35	0.00	0.00	0.00	0.74	0.25	1.00								
(18) Total NGO aid, residual	0.04	0.04	0.02	0.00	0.00	0.00	0.00	0.00	-0.06	-0.01	0.00	0.00	0.00	0.00	0.28	0.40	0.54	1.00							
(19) Residual ODA * Share	0.03	0.03	0.06	0.00	0.00	0.00	0.00	0.00	0.07	0.19	0.29	0.00	0.00	0.00	0.04	0.00	0.10	0.00	1.00						
(20) Residual ODA * Sum	0.06	0.06	0.07	0.00	0.00	0.00	0.00	0.00	0.09	0.24	0.36	0.00	0.00	0.00	0.05	0.00	0.12	0.00	0.76	1.00					
(21) Share * Fragile state	0.01	0.01	0.04	-0.09	-0.02	-0.01	-0.07	0.27	-0.08	0.01	0.00	0.08	0.35	0.26	-0.01	0.00	0.01	0.00	0.00	0.00	1.00				
(22) Share * Control of corruption	-0.03	-0.03	-0.08	0.12	-0.04	-0.03	0.23	-0.06	0.06	-0.07	0.00	-0.14	-0.60	-0.44	-0.04	0.00	-0.05	0.00	0.00	0.00	-0.42	1.00			
(23) Share * Per-capita GDP	0.04	0.03	0.12	0.03	-0.01	-0.01	0.02	-0.01	0.01	-0.01	0.00	0.23	0.99	0.73	-0.01	0.00	-0.01	0.00	0.00	0.00	0.32	-0.55	1.00		
(24) Number of NGOs *Share	0.10	0.08	0.20	-0.05	0.13	0.11	-0.03	0.00	0.01	0.12	0.03	0.16	0.72	0.52	0.20	0.14	0.15	0.06	0.10	0.08	0.24	-0.52	0.69	1.00	
(25) Total NGO aid * Share	0.03	0.02	0.04	0.00	0.00	0.00	0.00	0.00	-0.02	0.00	0.00	0.00	0.00	0.00	0.08	0.11	0.16	0.29	0.00	0.00	0.00	0.00	0.00	0.20	1.00

Appendix I.4: First-stage regression, Tobit estimations with public aid (ODA) as the dependent variable

	Coefficients	Overall marginal effects
	(1)	(2)
Per-capita GDP	-1.077***	-1.071***
	(0.320)	(0.318)
Population	1.112***	1.105***
	(0.137)	(0.136)
People affected by disaster	-0.026	-0.026
	(0.062)	(0.062)
Control of corruption index	-0.503	-0.500
	(0.503)	(0.499)
Fragile state, dummy	-0.465	-0.461
	(0.694)	(0.688)
Constant	-2.649	
	(3.478)	
Observations	126	126

Note: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Appendix I.5: Test for endogeneity of ODA, UN voting as the instrumental variable

Column (1): first-stage Tobit estimation with ODA as the dependent variable

Column (2): second-stage panel Tobit estimation with residuals of 1st stage as additional regressor

	(1)	(2)
Per-capita GDP	-1.573***	-0.248
	(0.017)	(0.160)
Population	1.020***	0.765***
	(0.007)	(0.117)
People affected by disaster	-0.041***	0.141***
	(0.003)	(0.022)
Control of corruption index	-0.533***	0.180
	(0.026)	(0.169)
Fragile state, dummy	-0.376***	0.205
	(0.036)	(0.221)
UN Voting	10.764***	
	(0.178)	
ODA		0.324***
		(0.109)
First-stage residuals		0.539
		(0.503)
Constant	-4.610***	-24.218***
	(0.191)	(1.142)
Observations	38375	38375

Note: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Appendix I.6: First-stage regression, OLS with # NGOs as the dependent variable, coefficients

	(1) Total sample	(2) Subsample
Per-capita GDP	-1.253***	-1.253***
	(0.085)	(0.234)
Population	4.205***	4.195***
	(0.035)	(0.097)
People affected by disaster	0.803***	0.800***
	(0.016)	(0.045)
Control of corruption index	-0.428***	-0.431
	(0.133)	(0.367)
Fragile state, dummy	-0.098	-0.103
	(0.182)	(0.502)
ODA, residual	5.654***	5.600***
	(0.145)	(0.622)
NGO budget	-0.030	-0.047
	(0.033)	(0.105)
Share of contributions	0.058	0.056
	(0.375)	(0.648)
Residual ODA * Share	-0.059	0.035
	(0.889)	(1.381)
Constant	-46.024***	-45.736***
	(0.926)	(2.667)
Observations	38682	5040

Note: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Chapter II:

Need, Merit, and Politics in Multilateral Aid Allocation: A District-Level Analysis of World Bank Projects in India

Joint work with Peter Nunnenkamp[a] and Maximiliano Sosa Andrés[b]

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In 1990 more than nine out of ten of the world's poorest … lived in poor countries. Now, three quarters live in middle-income states such as China, India and Brazil. This is a problem for the World Bank because it mostly still lends to poor countries, not poor people.

The Economist, April 21st, 2012

II.1 Introduction

Donors have often been criticized for insufficient targeting of foreign aid. In particular, donor selectivity in terms of favouring needy and deserving recipients appears to be weak according to earlier studies such as Alesina and Dollar (2000) and Alesina and Weder (2002).[1] The typical defence line of donors is that essentially all empirical studies assess aid allocation across countries. The highly aggregate level of recipient countries may disguise that poverty affects large segments of the population within countries whose average income level is well above subsistence levels. Furthermore, the quality of governance may differ within countries so that some local administrations may put aid to productive use, whereas other local administrations do not merit aid.

The geography of foreign aid within recipient countries is largely unexplored territory.[2] Donors typically do not reveal the precise location of their aid projects. However, AidData in collaboration with the World Bank provides a project- and location-specific database covering on-going projects in various recipient countries.[3] We combine the project-related information offered by AidData with the exceptionally rich data reflecting economic, institutional and political conditions available for 620 districts in 28 Indian states and seven

[1] Several recent studies stress the differences between donors. While some donors allocate aid altruistically according to recipients' need and merit, other donors behave egoistically and use aid as a means to promote their own commercial and political interest (e.g., Berthélemy 2006; Dollar and Levin 2006; Thiele et al. 2007; Claessens et al. 2009). Fleck and Kilby (2010) find that the United States placed less emphasis on need for core aid recipients during the War on Terror.

[2] Zhang (2004) provides an exception by assessing the allocation of World Bank projects across Chinese provinces. Specifically, Zhang presents OLS regression results based on about 30 provincial observations for the amount of World Bank loans and five possible determinants, including the population and per-capita income of provinces.

[3] The link to the database is as follows: http://open.aiddata.org/content/index/geocoding (accessed: February 2012).

Union territories.[4] The case of India is particularly relevant to assess the within-country allocation of aid. The vast country is characterized by striking regional disparities which should have affected the location of aid projects. India is currently classified as a lower-middle income country by the World Bank and has traditionally been among the major recipients of foreign aid. As a so-called blend country, India is eligible for highly concessional funding by the International Development Agency (IDA) as well as for IBRD loans.[5] In 2006-2010, IDA directed almost 14 per cent of its overall aid commitments to India, the highest share among all recipient countries. Conversely, IDA ranked second (behind Japan) among India's donors of official development assistance (ODA), contributing 30 per cent to ODA commitments from all sources in 2006-2010. IBRD loans to India, which are subsumed under other official flows (rather than ODA), amounted to US$12 billion during the same period, making the country the third largest IBRD borrower behind Mexico and Brazil.[6]

By analysing the location of World Bank projects within India we extend the aid allocation literature in several ways. First of all, we account for need and merit at the regional level to overcome the limitations of cross-country studies. Wide income gaps prevail within India. Average per-capita income in Bihar was just one fifth of average per-capita income in Maharashtra in 2005/06 and the following years. At the same time, average per-capita income in the three richest districts of Maharashtra was about three times as high as average per-capita income in the three poorest districts of the same state. Likewise, there are striking regional differences with regard to governance and stability. Kerala experienced close to 200 riots per million inhabitants in 2006, compared to essentially zero in Punjab.[7] The frequency of riots within Karnataka varied by a factor of four between the districts of Dakshin (South) Kannada and Uttara (North) Kannada. Hence, our first objective is to assess whether the World Bank adhered to its own insights – according to which aid tends to be more effective in poor environments with better governance (World Bank 1998) – when distributing its projects within India.

Another contribution is that we take spatial considerations into account. We assess whether World Bank projects tend to cluster regionally, either through previous projects encouraging further projects in the same district or through spatial effects, i.e., neighbouring

[4] There are data gaps in several respects, however. Consequently, we miss districts in most Union territories and some small states; see below for details. Note also that we do not cover some 30 districts that were created only recently.
[5] See: http://data.worldbank.org/about/country-classifications (accessed: February 2012).
[6] For details, see: http://stats.oecd.org/index.aspx?DataSetCode=CRS1 (accessed: February 2012).
[7] See the dataset on crime in India of the Center for Systemic Peace available at: http://www.systemicpeace.org/inscr/inscr.htm (accessed: February 2012). More recent data on riots are not available from this source.

project locations affecting one another. Spatial effects may be expected particularly if poverty and governance conditions are similar in neighbouring districts. At the same time, regional bodies within India may compete for World Bank projects.

We also aim at evaluating the impact of political factors at the state and district level on the location of World Bank projects. Arguably, the distribution of World Bank assistance within India "has been strongly conditioned by states' political clout with the central government, owing to their ruling parties' ties to the central coalition" (Kirk 2005: 287). On the other hand, the World Bank may have circumvented the meddling of the federal government by its "strategy of 'focus states' lending in India, targeting state-level governments for wide-ranging policy reforms meant to promote economic growth and poverty reduction" (ibid). Political factors may bias the distribution of projects not only at the state level. Furthermore, Chief Ministers of states may direct projects primarily towards their own constituencies and home districts. For instance, the Chief Minister of Uttar Pradesh in 2007-2012, Kumari Mayawati, distanced herself from her predecessor by declaring: "I am not Mr. Mulayam Singh Yadav, who had diverted all the funds to develop his home area only."[8]

In addition to political interference in India, we assess at least tentatively whether the allocation of projects is affected by commercial interests of the World Bank's major shareholders. This could be the case if projects were mainly directed to locations preferred by foreign investors and traders. Several studies suggest that multilateral institutions are vulnerable to pressure of major shareholders. Dreher et al. (2009b: 742) find that loans from the International Monetary Fund are "a mechanism by which the major shareholders of the Fund can win favour with voting members of the [United Nations] Security Council." Likewise, World Bank projects have been directed by major shareholders and funnelled to politically important developing countries (Dreher et al. 2009a). According to Fleck and Kilby (2006), the United States influences World Bank lending in pursuit of US commercial (and strategic) interests.[9] Pressure of shareholders may also affect the within-country allocation of World Bank projects, notably when specific regions are of particular commercial interest.

To preview our major results, the evidence of needs-based location choices across Indian districts by the World Bank is very weak, even though projects tend to concentrate in relatively remote districts. Institutional conditions matter insofar as project locations cluster in districts belonging to states with greater openness to trade. We do not find any evidence that

[8] See: http://bspindia.org/kumari-mayawati.php (accessed: February 2012).
[9] Kilby (2009) finds that US pressure has undermined the World Bank's conditionality in structural adjustment programs with countries that are politically friendly with the United States.

location choices are affected by political patronage at the state or district level. However, the World Bank prefers districts where foreign direct investors may benefit from projects related to infrastructure.

II.2 Data and Method

The geocoded database of World Bank projects by AidData on which we draw for the subsequent analysis lists 86 projects in India that were approved in 2006 or later and were still in operation in September 2011. Taken together these projects involve World Bank commitments in India in the order of US$ 21 billion during the period 2006-2011. Some projects are small and limited to few locations within a single Indian state. For instance, the World Bank committed US$ 13.6 million in 2009 for a project in energy and mining that was located in two districts of Karnataka. At the other extreme, one project in transportation involves overall commitments of US$ 1.5 billion, spreading over some 200 locations in seven states. Appendix II.1 shows the sectoral breakdown of overall World Bank commitments. The focus is clearly on infrastructure, notably transportation and energy (including mining), while social services such as education and health play a minor role.

The database does not provide the regional breakdown of the overall amount of project-related World Bank commitments. However, the entries in the database typically specify the districts in which (part of) a project is located.[10] Appendix II.2 shows how these entries are distributed across Indian states. Not surprisingly, the larger states typically rank high in terms of the absolute number of project locations in 2006-2011. By contrast, some small Union territories (Lakshadweep, Andaman and Nicobar, Daman and Diu) stand out when comparing project locations per million of inhabitants. Importantly, the simple correlation between project locations per million of inhabitants and the average per-capita income at the state level is essentially zero (not shown), casting into doubt that the location of World Bank projects is strongly poverty-related.

Against this backdrop, we opt for Poisson Pseudo Maximum Likelihood (PPML) estimations to assess the determinants of the number of World Bank's project locations at the district level. Poisson regression models are generally appropriate when the dependent

[10] The database has 1638 entries for projects approved since 2006. No specific districts are given for 258 of these entries. For most of these 258 entries, relating to 31 projects, the location given in the database refers to the so-called first order administrative division (ADM1, i.e., the state in India), but the location within the ADM1 is unknown. In some cases, the location is an entire state or lies between populated areas, along rivers or borders, etc. We also omit a single project location in Delhi.

variable takes non-negative values and is skewed.[11] The expected number of project locations is given by:

$$E(y_i|x_i) = e^{x_i'\beta}$$

As described below our independent variables include both district- and state-level variables. To avoid underestimating the standard errors of state-level variables we cluster them by state.

In the basic Poisson model, the dependent variable is defined as the number of project locations within one particular Indian district for all World Bank projects approved during the period 2006-2011. The sample of World Bank projects tends to be tilted increasingly towards longer-term projects the further one goes back in time by including projects approved in earlier years. This is why we focus on recent years, i.e., projects approved in 2006-2011.[12] The cross-section approach appears appropriate to avoid an excess of zeros. Nevertheless, we perform two sets of panel data analyses in subsequent steps in order to test for the robustness of our results. First, we slice the project data by the year of approval. The dependent variable is then defined as the number of project locations for World Bank projects approved in year *t* (*t* = 2006, …, 2011). Second, we consider the number of project-specific locations within a district; i.e., we replace the time dimension by the finer project dimension. Note that the number of zero observations increases substantially when defining the dependent variable along the project dimension.

In both the cross section analysis and the panel data analysis we perform two sets of estimations. The first set includes district- and state-level determinants of location choices.[13] The second set includes district-level determinants with state fixed effects to fully control for unobserved heterogeneity at the state level. District-level indicators of need and merit are as follows. GDP per-capita at the district level in 2005/06 represents our indicator of need. This information is available from the Planning Commission for 24 states.[14] Merit is typically captured by the quality of institutions and governance in aid allocation studies (e.g. Berthélemy 2006; Dollar and Levine 2006). While information on institutional conditions is

[11] Note also that the PPML estimator allows for over- and under-dispersion, i.e., the conditional variance of the dependent variable does not have to be equal to the conditional mean (see e.g., Santos Silva and Tenreyro 2006).
[12] All the same, we perform a robustness test below by considering all World Bank projects approved since 2001.
[13] See Appendix II.3 for a detailed definition of all variables and data sources; Appendix II.4 provides summary statistics.
[14] See: http://planningcommission.nic.in/plans/stateplan/index.php?state=ssphdbody.htm (accessed February 2012). This source does not cover six Union territories, Gujarat, and some small states such as Goa, Nagaland, and Tripura. Therefore, we also experimented with an alternative indicator of need drawn from the Indian Census by interpolating data of 2001 and 2011 on district-level literacy rates. However, literacy proved to be irrelevant throughout at conventional levels of significance so that we do not report detailed results. The poor results for literacy rates are probably because social services such as education and health play a minor role in the World Bank's project portfolio in India (see Appendix II.1).

limited for relatively small regional units such as Indian districts, we consider the frequency of riots and social unrest (per 100,000 inhabitants) as a proxy for the quality of institutions at the district level. This information is drawn from Marshall and Marshall (2008).

To account for possible political patronage at the district level, we identified the political constituencies of the Chief Ministers of state governments. Political patronage at the district level would imply that the chances of districts receiving World Bank projects are higher when the Chief Minister has her constituency there. Hence, we set a dummy variable equal to one for districts where the Chief Minister had her constituency.[15]

We also account for the number of FDI projects in Indian districts, approved in the post-reform period 1991-2005. One would expect World Bank projects to locate in districts with (more) FDI projects if aid activities of the World Bank in India serve the commercial interests of its major shareholders, notably by improving local infrastructure that, in turn, supports the profitability of FDI.[16] In addition, we control for several factors at the district level. First of all, larger districts (in terms of population) are expected to attract a larger number of project locations. Second, we assess whether World Bank projects are concentrated in remote districts. This could provide an indirect indication of needs-based aid allocation, assuming that districts further away from economic centres are characterized by more serious bottlenecks to economic and social development. Alternatively, the World Bank may be reluctant to engage in remote areas where the visibility of projects is more limited. Planting its flag in less remote and, thus, easier locations may also be tempting for the World Bank in order to impress its shareholders with successful project implementation. Remoteness is measured as the minimum travel time, based on a cost distance analysis, from a district to any of the ten most important economic centres in the country.[17] Third, we include a dummy variable set equal to one for districts affected by the Asian tsunami in December 2004.[18] We assume that the impact of the tsunami in western and southern districts of India on the location of World Bank projects is minimized by restricting the analysis to projects approved since 2006. Nonetheless, we control for tsunami effects as the approval of related projects could have been delayed. Fourth, we control for the number of projects that a specific district

[15] More precisely, the variable may vary between zero and one to account for the possibility that the Chief Minister has her constituency in the district for only part of the period of observation (with the proportion on months used as weights).

[16] See Zhang 2004 for a similar reasoning on World Bank activity in China.

[17] Delhi, Mumbai, Ahmedabad, Bangalore, Chennai, Hyderabad, Kolkata, Patna, Bhubaneswar and Guwahati. We thank Henry Edward Jewell and Hyoung Gun Wang of the World Bank's Finance, Economics and Urban Department who developed this measure, as well as Uwe Deichmann who helped us access these data.

[18] The relevant information is taken from maps revealing the affected districts; see: http://www.mapsofindia.com/maps/tsunami-in-india/tsunami-affected-area-india.html.

received in previous years (2001-2005).[19] A positive coefficient of this variable may point to clustering of World Bank projects at the district level.

In extended specifications, we account for the spatial lag of the dependent variable. Rather than considering only neighbours with a common border, we also account for project locations in districts without a common border. Technically speaking, we apply a row-standardized inverse distance matrix based on distances between Indian districts as our weighting matrix.[20] However, Poisson estimations augmented by spatial lags may be biased due to the endogeneity of spatial lags which tend to be determined simultaneously with project locations in the district under consideration. Therefore, we also present spatial autoregressive models with spatial autoregressive disturbances (SARAR models) estimated by general spatial two-stage least squares (GS2SLS) proposed by Kelejian and Prucha (1998):

$$y = \gamma W y + X\beta + u$$
$$u = \rho W u + \varepsilon$$

where Wy is known as the spatial lag with W being the row-standardized inverse distance matrix, X the exogenous regressors, u an disturbance term which may depend on a weighted average of other disturbances and ε an independent but heteroskedastically distributed error term. The method uses WX and W^2X as instruments.[21]

While the list of variables at the state level covers similar aspects, we expect deeper insights in two major respects. Compared to the district level, better information is available at the state level with respect to institutional and economic policy conditions. We consider the degree of economic freedom across states, based on an aggregated score rating the size of government, legal structure and property rights, access to sound money, freedom to trade internationally and labour and business regulations.[22] Economic policy conditions are also captured by openness to trade at the state level.[23] This measure relates to the World Bank's view on aid effectiveness, according to which openness to trade represents an essential element of good policy conditions for aid to be effective (e.g., Burnside and Dollar 2000).

Finally, as concerns possible political patronage at the state level, we introduce two dummy variables. The first variable is set equal to one for districts located in states where the

[19] These projects, too, were still active as of September 2011.
[20] We use the average distance between districts in neighbouring states as the cut-off distance, i.e., we neglect districts which are further than 489 kilometres away from the district under consideration and set the respective weights to zero.
[21] We use Drukker et al.'s (2011) spreg command in Stata for the estimations. See the same paper for more details on the method. Note that the model cannot account for the count nature of our dependent variable and for standard errors to be clustered by state.
[22] The score is based on the Fraser Institute's *Economic Freedom of the World* ranking. See Debroy et.al. (2011).
[23] More precisely, we use export data for so-called export-oriented units; for details, see: http://www.eouindia.gov.in/fact_figure09.htm#005.

Chief Minister belongs to the same political party as the Prime Minister at the federal level. Similarly, the second variable takes a value of one if the Chief Minister belongs to a party in coalition at the federal level. In this way, we account for the possibility that the chances of districts receiving World Bank projects may increase when the same party is in power at the state and federal levels. As noted before, the political clout of state authorities at the federal level has been suspected to matter for World Bank decisions (Kirk 2005).[24]

II.3 Results

Cross section results

The Poisson estimations reported in Table II.1 employ the number of project locations at the district level as the dependent variable. We consider all World Bank projects that were approved since 2006 and still in operation by September 2011. Recall from above that one particular project may spread over several locations within a district, for instance, in the case of large projects covering roads and other infrastructure in several smaller administrative units. As independent variables we enter both district- and state-level factors that could have an impact on location choices for the World Bank projects.[25] The basic specifications in columns 1 and 2 include population as well as indicators of need (*log per capita GDP*, *tsunami*) and merit (*riots*) at the district level, institutional conditions (*trade openness* or *economic freedom*) at the state level, and our proxies for possible political patronage at the level of districts (*Chief Minister constituency*) and states (*same party* and *coalition*). In columns 3-6 of Table II.1, we extend the list of independent variables at both the district and state level.

The baseline estimations suggest that location choices were not affected by need and merit at the district level. The only exception is the significantly positive tsunami dummy in column 2, indicating a higher number of project locations in districts affected by the tsunami in December 2004. However, the tsunami dummy is no longer significant in the subsequent estimations. Population is the only variable at the district level that proves to be consistently significant, at the one per cent level, implying that larger districts are typically characterized by more project locations.

[24] Constitutional controls over state borrowing position the centre as the legal borrower that is ultimately responsible for repaying the World Bank's dollar-denominated loans. Consequently, the World Bank needs the federal government's "explicit cooperation" (Kirk 2005: 289) for its state-focused lending. Similar to the Chief Minister's constituency at the district level, we use a weighting scheme based on the proportion of months during the period of observation for the two indicators of political patronage at the state level. Accordingly, these two indicators may also vary between zero and one.

[25] As discussed in more detail below, we replace state-level determinants by state-fixed effects in the next step of our analysis.

The insignificance of the frequency of riots at the district level does not necessarily mean that merit does not play any role for the location of World Bank projects. Rather, column 2 provides some evidence that World Bank projects tend to locate in districts belonging to Indian states with a better institutional and economic environment. The number of locations significantly increases, at the ten per cent level, for districts in states with greater openness to trade. Economic freedom turns out to be insignificant, however.[26] At the same time, we do not find any evidence suggesting that the location of World Bank projects is affected by political patronage. The insignificant coefficients of our proxies for political affinity of state governments with the federal government in Delhi speak against patronage in favour of political allies at the state level. It rather appears that the World Bank succeeded in limiting meddling by the federal government and dealt with state governments directly, e.g., in the context of its strategy of 'focus states' lending (Kirk 2005). Likewise, districts did not benefit, in terms of more project locations, from the Chief Minister having her political constituency there. This surprising finding may be attributed to the fact that the dependent variable covers six years of project approvals, a period during which Chief Ministers changed several times in some Indian states. It is also possible, however, that political patronage plays a minor role at the district level as various large World Bank projects spread across several districts almost by construction.

The results reported so far are fairly robust when extending the specification. The most notable exception refers to GDP per capita at the district level. While the coefficient of GDP per capita continues to be statistically insignificant in columns 3-5, it turns out to be significantly negative, at the ten per cent level, in the fully specified model in column 6. We return to the question of the needs-based allocation of World Bank projects further below.

The extensions in columns 3-5 mainly concern aspects of remoteness and agglomeration at the district level. The remoteness of districts, measured by (logged) minimum travel time from districts to the closest economic centre in India, may reflect greater regional need for World Bank projects (see Section 2). It could thus be taken as an indication of needs-based aid allocation if the number of project locations rises with distance. On the other hand, one may suspect that World Bank projects locate mainly where foreign direct investors have an interest in improved infrastructure, i.e., where the number of FDI projects is high. In that case location choices might be driven by the demands of foreign investors, rather than regional needs. The remoteness of districts always fails to pass conventional levels of significance, whereas the number of FDI projects proves to be significant and positive in the

[26] Recall that indicators of economic freedom and openness to trade are not available at the district level.

fully specified model in column 6. This indicates that World Bank support, in terms of the number of project locations, responds favourably to existing clusters of FDI projects.[27]

The fully specified model accounts for GDP per capita, population and the frequency of riots and unrest at both the district and state level, in addition to previous extensions. The results reported in column 6 indicate that the location of World Bank projects is influenced by conditions at both levels, though not necessarily by the same factors and in the same direction.[28] For instance, projects tend to cluster in larger districts, but districts are typically penalized if they are located in larger states. This indicates a "small state bias" similar to the small country bias found in the cross-country aid allocation literature (e.g., Berthélemy and Tichit 2004). Need as reflected in GDP per capita does not play any role for location choices at the state level, while we find some weak evidence of a needs-based allocation at the district level. By contrast, it appears that merit shapes location choices at the state level: greater openness to trade and a lower frequency of riots at the state level are associated with a larger number of project locations. However, the frequency of riots enters insignificant at the district level. It appears reasonable that merit is assessed at the state level, rather than the district level, considering that state authorities tend to have a larger say on institutional conditions.[29]

We modify our estimation approach in Table II.2 by focussing on the determinants at the district level and controlling for heterogeneity across states through fixed effects. State fixed effects control for unobserved heterogeneity beyond the state-level variables in Table II.1. The modification reveals a significant impact of the distance between districts and major economic centres. This indicates that the World Bank favours relatively remote districts with presumably greater need. On the other hand, GDP per capita proves to be insignificant in all four specifications with state fixed effects, providing no evidence for a needs-based allocation based on this criterion. Other results are hardly affected. Most importantly, there is still no significant evidence for political patronage at the district level. Existing FDI clusters continue to have a significantly positive impact on location choices (except in column 2).

Compared to Tables II.1 and II.2, we extend our period of observation in Table II.3 by including all World Bank projects approved since 2001.[30] In this way we assess whether previous results are affected by changes in the sample of World Bank projects that were still

[27] We also add the number of project locations in districts that relate to earlier World Bank projects (approved in 2001-2005). However, this variable proves to be insignificant. See also the discussion on Table II.3 below.
[28] As before, however, political patronage does not affect location choices at either the district level or the state level.
[29] Kochhar *et al.* (2006) argue that state-level policies and institutions increasingly mattered in the aftermath of the reform program of the early 1990s.
[30] Redefining the dependent variable in this way implies that clustering at the district level, proxied in Tables II.1 and II.2 by the number of project locations approved in 2001-2005, can no longer be observed.

in operation in September 2011. Looking further back and including projects with earlier approval dates implies that the sample increasingly consists of projects of longer duration.[31] All the same, most of our results are hardly affected by the inclusion of projects with approval dates in 2001-2005.

It is only in one respect that the results reported in Table II.3 differ considerably from those in Tables II.1 and II.2. The tsunami dummy is now statistically significant at the ten per cent level or higher in all estimations with both district- and state-level variables included (columns 1-5). The same applies for the estimations with state fixed effects (columns 6-8). The stronger evidence for location choices being influenced by the tsunami is not surprising. It indicates that the World Bank reacted promptly by approving projects and locating them where emergency relief was most urgently required.

Apart from emergency relief, the evidence for a needs-based allocation of World Bank support continues to be weak in Table II.3. Again, lower GDP per capita at the district level is associated with more project locations only in the fully specified model with both district- and state-level variables (column 5). In the estimations with state fixed effects, it is only the significantly positive coefficient of (logged) distance that may provide some indirect evidence for a needs-based allocation. Compared to Table II.1, the role of institutional and policy conditions at the state level proves to be stronger for the larger sample of World Bank projects. Typically, openness to trade enters highly significant and positive, suggesting that districts in states with better conditions are preferred project locations. Table II.3 corroborates that World Bank projects tend to concentrate in districts with clusters of FDI, while political patronage does not appear to matter.

In the final step of our cross section analysis, we account for the spatial lag of the dependent variable.[32] As noted before, large World Bank projects, notably in the field of physical infrastructure, often cover various locations. We expect that a district's chances to attract (parts of) projects improve with the number of project locations in neighbouring districts. In columns 1, 3 and 5 of Table II.4, we simply augment the corresponding Poisson estimations in Tables II.1 and II.2 by spatial lags as defined above. In addition, we present SARAR models estimated by GS2SLS in columns 2, 4 and 6 in order to account for the endogeneity of spatial lags.

Spatial lags turn out to be significantly positive as expected, at the ten per cent level or higher, with just one exception in column 1. All the same, most previous findings carry over

[31] The results may also be affected if our independent variables have less predictive power for earlier location choices. This is unlikely to be the case for variables that do not vary, or just slightly, over time.
[32] In contrast to Table II.3, we return to the sample of World Bank projects approved in 2006-2011.

to Table II.4. In particular, the variables at the district level closely resemble the corresponding results in Tables II.1 and II.2. An exception is the frequency of riots which enters negative and significant at the five per cent level in the SARAR model in column 2. It should also be noted that all three SARAR models reveal significant effects of previous location choices in 2001-2005 on the number of project locations approved since 2006, at the five per cent level or higher. Hence, it appears that districts have better chances to benefit from subsequent project approvals if they already attracted (parts of) earlier projects. As concerns the variables at the state level, openness to trade is no longer significant when accounting for the endogeneity of the spatial lag in the SARAR models in column 2 and 4.[33] Nevertheless, we still find evidence that institutional conditions matter at the state level. Economic freedom in column 2 and the frequency of riots at the state level in column 4 (and III) show significant effects.

Estimations with panel data

In the cross section estimations reported so far, we considered the sum of project locations in a particular district for all World Bank projects approved since 2006 (or, alternatively, since 2001). This aggregate perspective appears to be most appropriate for keeping zero observations within reasonable limits. Nevertheless, Santos Silva and Tenreyro (2011) show that the PPML estimator also performs well in the presence of a very large proportion of zeros in the sample.[34]

We slice the project data in two alternative ways in the following. In the first approach, our dependent variable is defined as the number of locations for World Bank projects approved in each single year, rather than during the period 2006-2011 as a whole. In the second approach, our dependent variable is defined as the number of locations for each single project. In both approaches, we also refine some of the independent variables. In the first approach, we would ideally employ annual data throughout. This is not possible, however, as information is often missing for recent years. Hence, we maintain the assumption underlying the cross section models that location choices throughout the period of observation depend on initial conditions in 2005/06 in terms of GDP per capita (at the district level), the frequency of riots and the number of FDI projects. By contrast, we use time variant information for other variables, including population, the political variables *Chief Minister*

[33] Moreover, the dummy variable *Coalition* enters significantly negative in column 2, which is not particularly intuitive.

[34] The zero-inflated Poisson model, on the other hand, is not appropriate in this context as we do not have two distinct processes generating the zeros and non-zeros.

constituency, *same party* and *coalition*, and the indicators for the institutional and policy environment at the state level.[35] In the second approach, we further refine the definition of the political variables, i.e., they refer to the Chief Ministers in power at the exact approval date of the respective project.

Table II.5 presents the results of the first panel approach with annual observations of the dependent variable.[36] We report estimations with and without spatial lags. Columns 2, 4 and 6 clearly indicate the importance of spatial lags for location choices.[37] However, the inclusion of spatial lags has typically limited effects on our variables of major interest. Table II.5 corroborates several of our previous findings. First, the number of project locations increases with the size (in terms of population) of districts, while districts in larger states are again penalized. Second, the number of project locations is higher in districts hosting more FDI projects. This finding supports the view that the World Bank responds favourably to demands, e.g., for better infrastructure, by foreign investors and local authorities. All the same, the estimations with fixed state effects attest that the World Bank is more strongly engaged in remote districts.

Third, we again find no evidence for political patronage at either the state or the district level. Fourth, Table II.5 also corroborates that our proxy of merit at the district level, *riots*, has no significant impact on the location of World Bank projects. In contrast to the cross section analysis, however, the panel data estimations no longer provide any evidence that merit matters at the state level.

Fifth, the panel data estimations for the fully specified models with district- and state-level variables (columns 3 and 4) suggest that districts with lower GDP per capita receive a larger number of project locations. Nonetheless, the evidence for a needs-based allocation of World Bank support continues to be weak when defining the dependent variable on an annual basis. Note that GDP per capita at the state level now enters significantly positive, at the one per cent level, when added to the variables at the state level in columns 3 and 4. This indicates that districts are penalized if they are located in poor Indian states.

Finally, Table II.6 presents the results for the second approach with project-related observations of the dependent variable. Once again, we present estimations with and without spatial lags. Note that all estimations in Table II.6 include project fixed effects to account for

[35] See Appendix II.3 for those variables that had to be interpolated.

[36] Compared to previous tables, we reduce the number of specification and focus on the fully specified estimations for the sake of brevity.

[37] Note that SARAR models cannot be applied in this context as the used command spreg is not suitable for panel data. This implies that we do not account for the endogeneity of spatial lags here and in Table II.6 below.

unobserved project heterogeneity.[38] Nevertheless, the results are similar to those in Table II.5. A major difference is that the spatial lag proves to be insignificant in the estimation with state fixed effects. It appears that location choices in neighbouring districts with respect to the same project do not affect the number of locations in the district under consideration once state fixed effects have been taken into account.[39]

II.4 Summary and Conclusion

Aid allocation studies typically stop short of assessing the targeting of foreign aid according to the need and merit within recipient countries. Recent efforts by AidData, in collaboration with the World Bank, help explore the geography of on-going World Bank projects. We combine the location-specific information for projects in India with the exceptionally rich data reflecting economic, institutional and political conditions in Indian states and districts to assess the allocation of aid within one of the major recipient countries of World Bank support.

We perform Poisson estimations across Indian districts, supplemented by panel data specifications. We also augment the models by spatial lags and account for their endogeneity by using spatial econometric techniques (SARAR models estimated by GS2SLS). Our major results can be summarized as follows. First of all, the evidence of needs-based location choices across Indian districts by the World Bank is very weak, even though activities tend to concentrate in relatively remote districts. Second, spatial lags prove to be significant and positive, indicating that the chances of districts to participate in aid projects improve when the World Bank is active in neighbouring districts. Third, institutional conditions matter insofar as project locations cluster in districts belonging to states with greater openness to trade and fewer riots. However, there is no longer any evidence for a merit-based aid allocation in the panel data analyses. Fourth, we do not find any evidence that location choices are affected by political patronage at the state or district level. On the other hand, the World Bank prefers districts where foreign direct investors may benefit from projects related to infrastructure.

These findings could be relevant for the authorities in the recipient country as well as the World Bank. As concerns the former, the importance of spatial lags suggests that remote areas are well advised to join forces when trying to attract aid projects. Local authorities at lower administrative levels may realize that their own influence is limited when it comes to merit-based aid allocation. This could necessitate cooperation with higher levels of the

[38] For the last column, we had to run the GLM poisson command in Stata, which relies on the iterated re-weighted least squares algorithm, in order to achieve convergence of the likelihood function.

[39] Furthermore, *Chief Minister constituency* proves to be significantly negative in two out of three estimations with spatial lags. This result is clearly surprising and counterintuitive.

political administration to improve the institutional environment and governance that donors may require to engage locally. However, the present case study is clearly insufficient to conclude that cooperation along these lines between different administrative levels would be more effective in attracting foreign aid projects than political patronage along party lines and by favouring political constituencies.

As concerns the World Bank, the initial quote from *The Economist* suggests that it will become increasingly important to target poor people. Location choices within recipient countries could be an essential element in this regard. However, we find little evidence that the World Bank favours locations with greater need for aid. This invites the conclusion that the World Bank should adhere to its own insights more strictly than the Indian case suggests it does. In particular, it appears that local needs for aid have to be assessed more systematically in order to render World Bank aid more effective in fighting poverty at the local level. In this context, the Indian case is encouraging as it appears possible for the World Bank to deal directly with state governments and, thereby, avoid political patronage and meddling by the central government.

Finally, the implications of our analysis extend beyond the World Bank as a leading multilateral donor. Bilateral donors face the same challenge of targeting aid not only to poor countries with appropriate governance for aid to be effective, but also to locations within recipient countries where the need for aid is most obvious and local conditions are conducive to a productive use of external support. Progress in mapping the geography of aid projects within recipient countries could provide an important step towards meeting this challenge. It would allow for comparisons between bilateral and multilateral donors in future research. Deeper insights into the allocation of aid may be gained in particular if location-specific information on aid projects as well as the economic, institutional and political covariates covered an increasing number of donors and recipient countries.

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Table II.1 - PPML estimations, number of locations at the district level for all projects approved in 2006-2011, district- and state-level determinants

	(1)	(2)	(3)	(4)	(5)	(6)
Log per capita GDP	0.151	0.258	0.0887	0.0788	0.110	-0.613*
	(0.485)	(0.455)	(0.507)	(0.515)	(0.525)	(0.313)
Log population	0.491***	0.446***	0.455***	0.476***	0.489***	0.948***
	(0.148)	(0.118)	(0.146)	(0.156)	(0.141)	(0.124)
Riots	-0.00822	-0.0262	-0.0153	-0.0149	-0.0140	-0.000118
	(0.0295)	(0.0274)	(0.0288)	(0.0294)	(0.0286)	(0.00996)
Tsunami	0.392	0.582*	0.396	0.412	0.297	0.386
	(0.320)	(0.326)	(0.334)	(0.330)	(0.400)	(0.348)
Chief Minister constituency	0.190	0.177	0.169	0.116	0.113	-0.0604
	(0.246)	(0.249)	(0.255)	(0.228)	(0.219)	(0.165)
FDI projects			0.000141	0.000154	0.000168	0.000450***
			(0.000233)	(0.000209)	(0.000208)	(0.000120)
Log distance				0.0111	0.0115	0.131
				(0.0718)	(0.0731)	(0.0823)
Project locations 2001-2005					0.131	0.0173
					(0.0807)	(0.0560)
Same party	-0.245	-0.309	-0.233	-0.235	-0.251	-0.0924
	(0.474)	(0.427)	(0.469)	(0.467)	(0.467)	(0.485)
Coalition	-0.392	-0.457	-0.592	-0.646	-0.724	-0.261
	(0.791)	(0.776)	(0.818)	(0.837)	(0.840)	(0.564)
Economic freedom	3.256		2.937	2.991	2.909	1.429
	(2.473)		(2.428)	(2.496)	(2.450)	(1.603)
Trade openness		6.107*	5.034*	5.409*	3.994	8.201***
		(3.200)	(2.879)	(2.809)	(3.058)	(3.043)
Log per capita GDP (state)						0.632
						(0.491)
Log population (state)						-0.805***
						(0.210)
Riots (state)						-0.0749**
						(0.0341)
Constant	-8.734**	-7.916*	-7.515	-7.892	-8.371	-0.587
	(4.297)	(4.269)	(4.779)	(5.308)	(5.401)	(7.451)
Observations	472	509	472	468	468	468
State Fixed Effects	N0	N0	N0	N0	NO	NO
Pseudo R-squared	0.100	0.123	0.110	0.112	0.117	0.215

Robust standard errors clustered by state in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table II.2 - PPML estimations, number of locations at the district level for all projects approved in 2006-2011, district-level determinants and state fixed effects

	(1)	(2)	(3)	(4)
Log per capita GDP	-0.288	-0.323	-0.299	-0.285
	(0.307)	(0.298)	(0.273)	(0.290)
Log population	0.770***	0.747***	0.818***	0.810***
	(0.106)	(0.0969)	(0.0998)	(0.0989)
Riots	-0.0142	-0.0148	-0.00526	-0.00547
	(0.0114)	(0.0113)	(0.0106)	(0.0106)
Tsunami	0.201	0.188	0.158	0.0955
	(0.258)	(0.269)	(0.189)	(0.245)
Chief Minister constituency	-0.0459	-0.111	-0.00602	-0.000583
	(0.173)	(0.200)	(0.152)	(0.148)
FDI projects		0.000194	0.000423***	0.000436***
		(0.000145)	(0.000111)	(0.000109)
Log distance			0.209***	0.209***
			(0.0529)	(0.0505)
Project locations 2001-2005				0.0588
				(0.0416)
Constant	-8.116***	-7.407***	-11.31***	-11.31***
	(2.679)	(2.669)	(2.468)	(2.506)
Observations	509	509	499	499
State Fixed Effects	YES	YES	YES	YES
Pseudo R-squared	0.343	0.344	0.345	0.346

Robust standard errors clustered by state in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table II.3 - PPML estimations, number of locations at the district level for all projects approved in 2001-2011

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log per capita GDP	0.121	0.190	0.0432	0.0364	-0.568**	-0.302	-0.328	-0.301
	(0.427)	(0.404)	(0.443)	(0.453)	(0.266)	(0.262)	(0.255)	(0.233)
Log population	0.390***	0.383***	0.347**	0.365**	0.846***	0.700***	0.681***	0.751***
	(0.143)	(0.119)	(0.146)	(0.157)	(0.120)	(0.107)	(0.102)	(0.106)
Riots	-0.00696	-0.0247	-0.0165	-0.0160	-0.00240	-0.0157	-0.0161	-0.00743
	(0.0281)	(0.0256)	(0.0267)	(0.0274)	(0.0105)	(0.0108)	(0.0107)	(0.0107)
Tsunami	0.490*	0.655**	0.497*	0.508*	0.482**	0.314*	0.303*	0.273**
	(0.260)	(0.273)	(0.268)	(0.263)	(0.241)	(0.177)	(0.184)	(0.107)
Chief Minister constituency	0.157	0.136	0.145	0.111	-0.0352	-0.0536	-0.109	0.0169
	(0.231)	(0.234)	(0.243)	(0.217)	(0.154)	(0.169)	(0.199)	(0.143)
FDI projects			0.000128	0.000163	0.000450***		0.000149	0.000411***
			(0.000213)	(0.000187)	(0.000124)		(0.000147)	(0.000107)
Log distance				0.0269	0.168**			0.226***
				(0.0634)	(0.0786)			(0.0657)
Same party	-0.160	-0.225	-0.136	-0.130	0.147			
	(0.441)	(0.390)	(0.429)	(0.426)	(0.392)			
Coalition	-0.156	-0.302	-0.423	-0.451	0.0486			
	(0.745)	(0.701)	(0.751)	(0.763)	(0.460)			
Economic freedom	2.888		2.456	2.470	0.825			
	(2.417)		(2.343)	(2.402)	(1.504)			
Trade openness		7.620***	6.853***	7.147***	11.30***			
		(2.910)	(2.575)	(2.541)	(2.617)			
Log per capita GDP (state)					0.356			
					(0.423)			
Log population (state)					-0.878***			
					(0.177)			
Riots (state)					-0.0781**			
					(0.0329)			
Constant	-6.800*	-6.335*	-5.320	-5.868	4.185	-6.883***	-6.344***	-10.47***
	(3.783)	(3.770)	(4.186)	(4.664)	(6.364)	(2.396)	(2.403)	(2.359)
Observations	472	509	472	468	468	509	509	499
State Fixed Effects	N0	N0	N0	N0	N0	YES	YES	YES
Pseudo R-squared	0.0996	0.136	0.119	0.120	0.225	0.327	0.328	0.329

Robust standard errors clustered by state in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table II.4 - PPML and SARAR by GS2SLS estimations with spatial lags (based on inverse distances), number of locations at the district level for all projects approved in 2006-2011

	(1)	(2)	(3)	(4)	(5)	(6)
	poisson	SARAR	poisson	SARAR	poisson	SARAR
Spatial lag (Wy)	0.167	3.451***	0.196*	2.906***	0.202***	1.560**
	(0.143)	(0.679)	(0.101)	(0.713)	(0.0297)	(0.759)
Log per capita GDP	0.0890	-0.330	-0.645**	-0.900*	-0.327	-0.544
	(0.525)	(0.507)	(0.324)	(0.531)	(0.294)	(0.535)
Log population	0.482***	1.417***	0.924***	1.949***	0.790***	1.897***
	(0.147)	(0.225)	(0.118)	(0.251)	(0.0928)	(0.235)
Riots	-0.0155	-0.0581**	0.00147	-0.00599	-0.00327	-0.0123
	(0.0277)	(0.0237)	(0.00995)	(0.0300)	(0.0102)	(0.0249)
Tsunami	0.334	-0.300	0.427	0.109	0.102	0.512
	(0.408)	(0.810)	(0.375)	(0.793)	(0.255)	(0.731)
Chief Minister constituency	0.136	-0.329	-0.0321	-0.443	-0.00802	-0.0457
	(0.218)	(0.739)	(0.167)	(0.692)	(0.153)	(0.486)
FDI projects	0.000178	0.00197*	0.000465***	0.00223**	0.000450***	0.00222**
	(0.000211)	(0.00107)	(0.000117)	(0.000985)	(0.000113)	(0.000980)
Log distance	0.0181	0.363	0.138*	0.447**	0.211***	0.573***
	(0.0714)	(0.221)	(0.0802)	(0.212)	(0.0505)	(0.197)
Project locations 2001-2005	0.131	0.695***	0.0227	0.448**	0.0629	0.385**
	(0.0818)	(0.175)	(0.0608)	(0.184)	(0.0393)	(0.195)
Same party	-0.220	-0.185	-0.131	0.408		
	(0.470)	(0.450)	(0.466)	(0.624)		
Coalition	-0.824	-1.404**	-0.407	-0.383		
	(0.818)	(0.689)	(0.556)	(0.878)		
Economic freedom	3.229	4.114*	1.582	0.159		
	(2.449)	(2.200)	(1.551)	(2.377)		
Trade openness	5.099*	-5.521	8.874***	4.837		
	(2.870)	(6.413)	(2.835)	(6.409)		
Log per capita GDP (state)			0.754	-0.272		
			(0.470)	(0.781)		
Log population (state)			-0.757***	-1.549***		
			(0.206)	(0.319)		
Riots (state)			-0.0812***	-0.209***		
			(0.0308)	(0.0681)		
Constant	-8.371	-29.23***	-2.224	1.463	-10.68***	-26.16
	(5.509)	(7.723)	(7.280)	1.463	(2.619)	(0)
Wu		1.771***		1.882		0.895
		(0.594)		(1.502)		(1.549)
Observations	458	458	458	458	488	488
State Fixed Effects	NO	NO	NO	NO	YES	YES

Robust standard errors in parentheses; clustered by state in uneven columns

*** p<0.01, ** p<0.05, * p<0.1

Table II.5 - PPML estimations, number of locations at the district level for all projects by year from 2006 to 2011

	(1)	(2)	(3)	(4)	(5)	(6)
Spatial lag (Wy)		3.039***		3.059***		3.532**
		(0.671)		(0.694)		(1.414)
Log per capita GDP	-0.0465	-0.103	-0.630**	-0.617**	-0.289	-0.323
	(0.423)	(0.376)	(0.293)	(0.279)	(0.288)	(0.268)
Log population	0.422**	0.378**	0.728***	0.595***	0.781***	0.756***
	(0.173)	(0.172)	(0.136)	(0.128)	(0.109)	(0.112)
Riots	0.00939	-0.00287	-0.00850	0.00561	-0.00446	0.00826
	(0.0259)	(0.0249)	(0.0114)	(0.0104)	(0.0107)	(0.0120)
Tsunami	0.0851	0.166	0.102	0.204	0.0843	0.207
	(0.430)	(0.333)	(0.434)	(0.364)	(0.252)	(0.231)
Chief Minister constituency	0.205	-0.0484	0.0357	-0.124	0.0541	-0.0739
	(0.216)	(0.245)	(0.197)	(0.227)	(0.158)	(0.212)
FDI projects	0.000214	0.000270	0.000359**	0.000351***	0.000308*	0.000322**
	(0.000224)	(0.000185)	(0.000179)	(0.000128)	(0.000185)	(0.000146)
Log distance	0.0495	0.103	0.0802	0.115	0.212***	0.274***
	(0.0841)	(0.0832)	(0.0816)	(0.0816)	(0.0612)	(0.0617)
Project locations 2001-2005	0.166*	0.176**	0.164*	0.158*	0.0713*	0.0801*
	(0.0957)	(0.0890)	(0.0890)	(0.0906)	(0.0425)	(0.0480)
Same party	0.0923	0.157	-0.124	-0.112		
	(0.262)	(0.228)	(0.289)	(0.247)		
Coalition	0.184	-0.259	0.0882	-0.370		
	(0.431)	(0.398)	(0.396)	(0.379)		
Economic freedom	2.973	2.075	1.970	0.568		
	(2.178)	(1.947)	(1.701)	(1.665)		
Trade openness	-2.690	0.196	-8.994	-4.826		
	(5.854)	(4.740)	(7.115)	(6.098)		
Log per capita GDP (state)			1.125***	1.190***		
			(0.339)	(0.359)		
Log population (state)			-0.323***	-0.199*		
			(0.104)	(0.112)		
Riots (state)			-0.0117	-0.0518		
			(0.0426)	(0.0369)		
Constant	-8.904*	-8.172*	-13.56***	-14.58***	-13.20***	-13.18***
	(4.785)	(4.401)	(4.590)	(4.339)	(2.783)	(2.869)
Observations	2,688	2,634	2,688	2,634	2,853	2,793
State Fixed Effects	NO	NO	NO	NO	YES	YES
Time Fixed Effects	YES	YES	YES	YES	YES	YES
Pseudo R-squared	0.138	0.199	0.165	0.220	0.226	0.276

Robust standard errors clustered by state in parentheses

*** p<0.01, ** p<0.05, * p<0.1

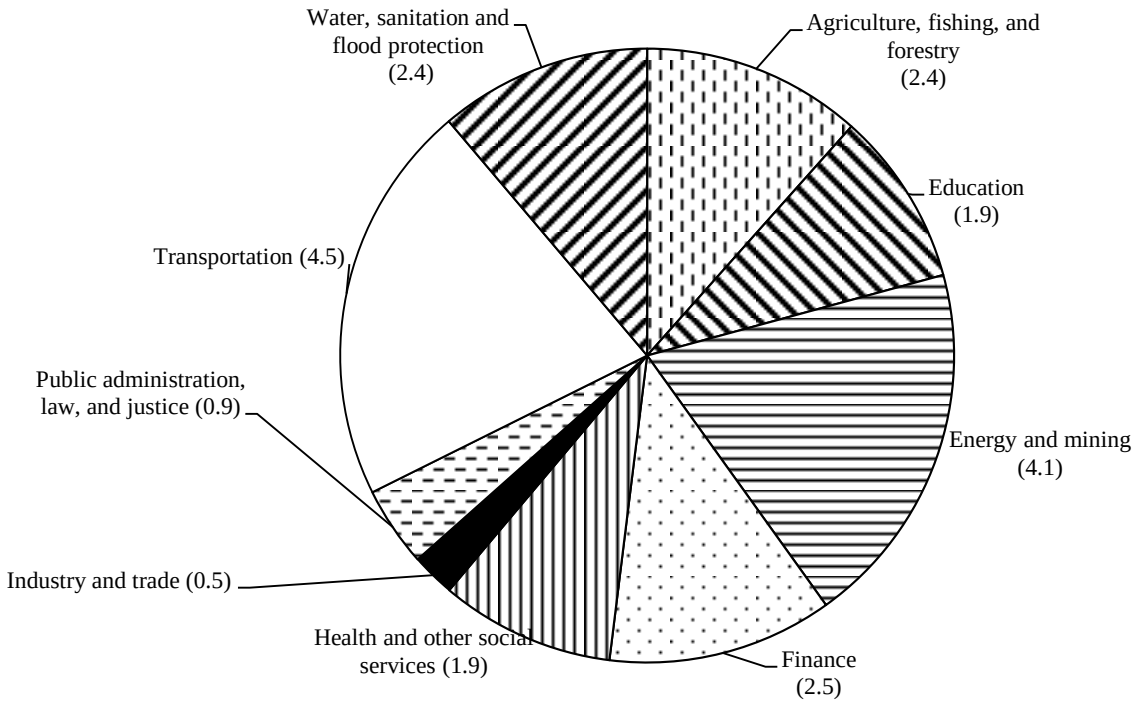
Table II.6 - PPML estimations, number of project-specific locations at the district level (for each project approved in 2006-2011)

	(1)	(2)	(3)	(4)	(5)	(6)
Spatial lag (Wy)		0.985***		0.981***		1.543
		(0.161)		(0.152)		(1.223)
Log per capita GDP	-0.0685	-0.163	-0.624**	-0.692***	-0.282	-0.389
	(0.419)	(0.362)	(0.286)	(0.260)	(0.291)	(0.245)
Log population	0.448**	0.398**	0.748***	0.556***	0.794***	0.807***
	(0.174)	(0.186)	(0.133)	(0.134)	(0.106)	(0.157)
Riots	0.0115	-0.000193	-0.00831	0.00455	-0.00477	0.00414
	(0.0249)	(0.0247)	(0.0116)	(0.0114)	(0.0104)	(0.0333)
Tsunami	0.106	0.301	0.116	0.333	0.0816	0.336
	(0.431)	(0.307)	(0.435)	(0.358)	(0.252)	(0.241)
Chief Minister constituency	0.258	-2.563**	0.125	-2.364**	0.125	-1.046
	(0.217)	(1.288)	(0.204)	(1.131)	(0.160)	(1.025)
FDI projects	0.000381**	0.000517***	0.000519***	0.000567***	0.000468***	0.000526***
	(0.000191)	(0.000195)	(0.000137)	(0.000118)	(0.000114)	(0.000118)
Log distance	0.0504	0.0356	0.0836	0.0153	0.223***	0.220**
	(0.0867)	(0.0939)	(0.0825)	(0.0849)	(0.0535)	(0.0869)
Project locations 2001-2005	0.191**	0.184**	0.184**	0.162*	0.0604	0.0849*
	(0.0838)	(0.0902)	(0.0851)	(0.0951)	(0.0412)	(0.0475)
Same party	0.0270	0.229	-0.175	-0.0835		
	(0.280)	(0.274)	(0.321)	(0.302)		
Coalition	-0.198	-0.389	-0.234	-0.489**		
	(0.287)	(0.247)	(0.255)	(0.237)		
Economic freedom	3.589*	2.531	2.635	0.802		
	(2.104)	(1.832)	(1.706)	(1.535)		
Trade openness	-1.043	2.434	-7.081	-2.817		
	(5.948)	(5.827)	(7.454)	(7.280)		
Log per capita GDP (state)			1.054***	1.344***		
			(0.347)	(0.417)		
Log population (state)			-0.313***	-0.123		
			(0.103)	(0.118)		
Riots (state)			-0.00439	-0.0466		
			(0.0417)	(0.0363)		
Constant	-9.061*	-7.636*	-13.28***	-15.38***	-13.25***	-12.95***
	(4.859)	(4.251)	(5.079)	(4.804)	(2.586)	(4.306)
Observations	25,409	24,875	25,409	24,875	27,084	26,495
State Fixed Effects	NO	NO	NO	NO	YES	YES
Project Fixed Effects	YES	YES	YES	YES	YES	YES
Pseudo R-squared	0.155	0.231	0.168	0.242	0.205	

Robust standard error clustered by states in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Appendix II.1: Sector-specific World Bank commitments in India, 2006-2011 (US$ billion)



Source: AidData (http://open.aiddata.org/content/index/geocoding)

Appendix II.2: Distribution of World Bank projects across Indian states, 2006-2011

State/ Union territory	Project locations in a particular state, 2006-2011	
	Absolute number	Per million of inhabitants
Tamil Nadu	205	3.15
Andhra Pradesh	135	1.67
Punjab	135	5.18
Bihar	120	**1.32**
Maharashtra	117	1.12
Karnataka	111	1.97
Orissa	105	**2.7**
Jharkhand	78	**2.66**
Rajasthan	78	**1.25**
Madhya Pradesh	77	**1.16**
Uttar Pradesh	67	**0.37**
Uttarakhand	52	5.64
Himachal Pradesh	50	7.74
West Bengal	49	**0.57**
Haryana	43	1.84
Gujarat	39	0.71
Chhattisgarh	32	**1.42**
Meghalaya	19	**7.69**
Kerala	14	0.42
Andaman and Nicobar	5	11.9
Jammu and Kashmir	4	**0.37**
Arunchal Pradesh	2	1.71
Daman and Diu	2	9.09
Delhi	2	0.12
Goa	1	0.67
Lakshadweep	1	14.29
Mizoram	1	**1.05**
Pondicherry	1	0.91
Assam	0	**0**
Chandigarh	0	0
Dadra and Nagar Haveli	0	0
Manipur	0	**0**
Nagaland	0	0
Sikkim	0	0
Tripura	0	**0**

Note: Ranked according to absolute numbers in the first column; bold figures in last column for states and Union territories whose average per-capita income in 2005/06 was below the average for all-India; data on per-capita income are missing for Daman and Diu, Dadra and Nagar Haveli, and Lakshadweep.

Source: AidData (http://open.aiddata.org/content/index/geocoding); Planning Commission (http://planningcommission.nic.in/data/datatable/index.php?data=datatab).

Appendix II.3: Definition of variables and data sources

Variable	Definition	Source
Project locations 2006-2011	Total number of locations of World Bank aid projects at district level, approved in 2006-2011 and still active as of September 2011	AidData http://open.aiddata.org/content/index/geocoding
Log per capita GDP	Log of Gross Domestic Product per capita at district level	Planning Commission – Government of India http://planningcommission.nic.in/plans/stateplan/index.php?state=ssphdbody.htm
Log population	Log of total population at district level (interpolated in the panel data analyses)	India Census (2001 and 2011). http://www.censusindia.gov.in/2011-provresults/census2011_PPT_paper1.html
Riots	Number or riots at district level by 100,000 inhabitants	Marshall and Marshall (2008)
Tsunami	Dummy variable, set equal to one if district was affected by the 2004 tsunami	http://www.mapsofindia.com/maps/tsunami-in-india/tsunami-affected-area-india.html.
Chief Minister constituency	Dummy variable set equal to one if the Chief Minister of the state had her constituency in district i at period t, weighted by months	Own collection based on internet search
FDI projects	Total number of foreign direct investment projects at district level in the 1991-2005 period	Unpublished data from the Indian Ministry of Commerce and Industry
Log distance	Log of minimum travel time based on a cost distance analysis of a district to any of the ten most important economic centres in the country	Jewell, Edward and Hyoung Gun Wang - World Bank's Finance, Economics and Urban Department.
Project locations 2001-2005	Total number of locations of World Bank aid projects at district level, approved in 2001-2005 and still active as of September 2011	AidData
Same party	Variable set equal to one if the same party is ruling at the state and federal level at period t, and 0 otherwise, weighted by months	Own collection based on internet search
Coalition	Variable set equal to one if the party ruling at the state level is in coalition at the federal level at period t, and 0 otherwise, weighted by months	Own collection based on internet search
Economic freedom	Score varying from zero to ten rating the size of government, legal structure and property rights, access to sound money, freedom to trade internationally and labor and business regulations at state level (interpolated in the panel data analyses)	Debroy et. al. (2011)
Trade openness	Total exports at state level divided by Gross Domestic Product (interpolated in the panel data analyses)	Export Promotion Council for EOUs and SEZs http://www.eouindia.gov.in/fact_figure09.htm#005
Log per capita GDP (state)	Log of Gross Domestic Product per capita at state level (interpolated in the panel data analyses)	Reserve Bank of India http://www.rbi.org.in/scripts/AnnualPublications.aspx?head=Handbook%20of%20Statistics%20on%20Indian%20Economy
Log population (state)	Log of total population at state level	Reserve Bank of India
Riots (state)	Number of riots at state level by 100,000 inhabitants	Marshall and Marshall (2008)

Note: All links accessed in May 2012.

Appendix II.4: Descriptive Statistics

Variable	Observations (districts)	Observations (states)	Mean	Std. Dev.	Minimum	Maximum
Project locations 2006-2011	620	N/A	2.2	2.9	0	19
Log per capita GDP	521	N/A	9.9	0.6	6.2	12.2
Log population	584	N/A	14.1	1.0	10.4	16.1
Riots	580	N/A	4.9	5.1	0.0	30.9
Tsunami	620	N/A	0.1	0.2	0	1
Chief Minister constituency	613	N/A	0.0	0.2	0	1
FDI projects	620	N/A	26.5	173.3	0	2984
Log distance	586	N/A	12.9	0.7	7.1	14.1
Project locations 2001-2005	620	N/A	0.3	0.7	0	5
Same party	613	29	0.3	0.4	0	1
Coalition	613	29	0.2	0.3	0	1
Economic freedom	548	20	0.4	0.1	0.3	0.6
Trade openness	615	30	0.0	0.0	0.0	0.2
Log per capita GDP (state)	616	31	10.0	0.4	9.0	11.4
Log population (state)	620	34	17.4	1.4	11.0	19.0
Riots (state)	620	34	5.2	4.0	0.0	19.2

Chapter III:

Throwing Foreign Aid at HIV/AIDS in Developing Countries: Missing the Target?

Joint work with Peter Nunnenkamp[a]

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III.1 Introduction

Five years after starting operations in 2002, the Global Fund to Fight AIDS, Tuberculosis and Malaria boasted of having saved 1.8 million lives as the result of financially supported programs.[1] By the end of 2009, the number of lives the Global Fund claims to have saved had reached almost five million.[2] The public-private partnership which is funded by various governments, multilateral institutions and private foundations is less explicit in explaining how it arrives at these impressive numbers.[3] Yet the specific claims of the Global Fund are in striking contrast to the general verdict of Easterly (2006) that foreign aid has done "so much ill and so little good." They also contradict Allen's (2004: 1123) earlier conclusion that HIV/AIDS policies have been "seriously inadequate."

The Global Fund is the most important multilateral donor engaged in the fight against HIV/AIDS, contributing about one-fifth of total official development assistance (ODA) disbursed in 2008 to prevent and treat sexually transmitted diseases – notably HIV/AIDS.[4] The United States stands out among the bilateral donors. The Bush administration launched the President's Emergency Plan for AIDS Relief (PEPFAR) in 2003. PEPFAR originally committed US$ 15 billion over five years to contain the HIV/AIDS pandemic. In 2006-2008 the United States directed 14 percent of its overall ODA commitments to HIV/AIDS.

Notwithstanding steep increases in ODA for preventing and treating sexually transmitted diseases, UNAIDS (the Joint United Nations Programme on HIV/AIDS) estimates that 33.4 million people were living with HIV in 2008.[5] This number was still increasing even though the number of new infections had slightly declined to an estimated 2.7 million since the peak in the mid-1990s. New infections continued to exceed the number of adult and child deaths due to AIDS of about two million in 2008. Country-specific evidence on the effects of aid on HIV/AIDS outcomes is also ambiguous. According to Morfit (2011), the massive

[1] See: http://www.theglobalfund.org/documents/publications/brochures/whoweare/TGFBrochure_FundingInAction.pdf ; accessed: July 2010.

[2] See: http://www.theglobalfund.org/documents/replenishment/2010/Progress_Report_Summary_2010_en.pdf; accessed: July 2010.

[3] In its 2007 Results Report, the Global Fund states: "Based on the reported ARV [antiretroviral treatment] figures each year, we computed scenarios with and without treatment to see the difference as 'lives saved'. Annual survival rates were assumed in line with the mortality assumptions in UNAIDS estimation models" (Global Fund 2007: 76, Box 24). In addition, a so-called resource input model is used to estimate lives saved due to averted HIV infections. See also Global Fund (2009: 5-63).

[4] See section 3 for details on HIV/AIDS-related ODA.

[5] See: http://data.unaids.org/pub/FactSheet/2009/20091124_FS_global_en.pdf, accessed: July 2010.

influx of ODA had little effect on HIV prevalence and incidence in Malawi. Lieberman (2007) mentions Botswana and Uganda where ODA may have been more effective given the local governments' demonstrated commitment to tackle HIV/AIDS problems. The Republic of Africa represented the most notorious counterexample until recently, where ODA was unlikely to have much effect because of official denial of HIV/AIDS problems at the highest political level.

The combination of donor generosity and persistent human suffering calls for an assessment of bold claims about the effectiveness of HIV/AIDS-related ODA. We argue in Section 2 that specific ODA items such as aid against HIV/AIDS may be effective in achieving specific objectives. Analyzing the effects of sector-specific aid is particularly relevant considering the highly ambiguous findings on the effectiveness of aggregate ODA in promoting economic growth or alleviating poverty in the recipient countries. We propose a difference-in-difference-in-differences (DDD) approach to identify the treatment effect of ODA specifically meant to fight sexually transmitted diseases on HIV/AIDS-related outcome variables. As explained in more detail in Section 4, the essential idea underlying the DDD approach is to combine before-after comparisons and with-without comparisons. This approach appears to be most appropriate to assess the recent steep increase in ODA directed against HIV/AIDS and helps mitigate important limitations that plague both types of comparisons when employed in isolation. We report our empirical results in Section 5. While ODA has not reduced the number of new HIV infections, we find that ODA from the United States has contributed effectively to the medical care of infected people.

III.2 Assessing Aid Effectiveness: An Alternative Approach

The controversy continues on whether or not aggregate ODA has been effective in promoting economic growth and alleviating poverty. Even surveys on the nexus between ODA and growth come to opposite conclusions (McGillivray et al. 2006; Doucouliagos and Paldam 2009). A more focused view on aid effectiveness may offer less ambiguous insights. Donors typically stress the multidimensionality of their objectives, which suggests assessing the impact of specific ODA items on narrower outcome variables than economic growth. Yet previous examples of this more modest approach also failed to provide clear-cut evidence in favor or against effective aid. For instance, Dreher et al. (2008) find that aid for education significantly increased school enrollment, while Michaelowa and Weber (2007) report inconclusive results. Mishra and Newhouse (2009) show aid for health to be effective in reducing infant mortality, whereas Williamson (2008) reaches the opposite conclusion. It is

equally debated whether aid helps promote democracy and better governance (e.g., Finkel et al. 2007; Busse and Gröning 2009; Kalyvitis and Vlachaki 2010). Öhler et al. (2010) employ a DDD approach, as we will do in the following, revealing that an innovative US aid scheme, the Millennium Challenge Corporation, was successful in fighting corruption in recipient countries.

There are very few studies assessing the links between specific ODA items and HIV/AIDS-related variables such as the prevalence of HIV, new HIV infections and the number of deaths due to AIDS. Lieberman (2007) finds that ethnic fractionalization in recipient countries has a negative influence on the policy responses to HIV/AIDS epidemics, including the responses of foreign donors. This study addresses the allocation of ODA, rather than its effectiveness. Burns (2010) laments a dearth of funding and conceptual flaws that undermine the effectiveness of Japan's HIV/AIDS programs in Asian recipient countries. Peiffer and Boussalis (2010) consider antiretroviral (ARV) treatment coverage rates and some other intermediate outcomes (tuberculosis treatment of HIV infected persons and HIV education in schools) in one particular year, as reported in UNAIDS Country Progress Reports in 2008, as dependent variables. According to their cross-country regressions, an increase in HIV/AIDS-related ODA by one dollar per capita of the recipient countries' population would increase the odds of complete coverage with antiretroviral treatment by 3-5 percent.

We are aware of just one study whose approach is similar to ours. Bendavid and Bhattacharya (2009) analyze the effects of PEPFAR on HIV/AIDS-related outcome variables in sub-Sahara Africa. The authors perform separate regressions with annual data for the periods 1997-2002 and 2004-2007, i.e., before and after PEPFAR started operations. Year dummies are interacted with a dummy variable that is set equal to one for the twelve PEPFAR focus countries in sub-Sahara Africa to assess the effectiveness of this program. The study of Bendavid and Bhattacharya is restricted by focusing on just one, though major, aid scheme and just one, though major, recipient region. It pays only scant attention to the fact that other major donors such as the Global Fund scaled up operations in line with PEPFAR. Moreover, their results may be biased as their estimation strategy does not systematically control for variables possibly affecting the link between ODA and HIV/AIDS outcomes.

The scarcity of empirical evaluations of the effectiveness of ODA in containing HIV/AIDS is surprising. Human suffering has been as severe and widespread, notably in sub-Sahara Africa, for the fight against the pandemic to be listed among the so-called Millennium Development Goals (MDGs). MDG 6 ("Combat HIV/AIDS, malaria and other diseases") lists

specific targets requesting the international community to "have halted by 2015 and begun to reverse the spread of HIV/AIDS" (Target 6.A) and to "achieve, by 2010, universal access to treatment for HIV/AIDS for all those who need it" (Target 6.B).[6]

In addition to its social relevance, analyses of HIV/AIDS-related ODA may help close important gaps in the literature on aid effectiveness. New insights may be gained for several reasons. First of all, aid targeted at HIV/AIDS may be more effective than other aid items as health-related interventions have increasingly been based on retrospective performance appraisals. Indeed, the health sector, including the fight against HIV/AIDS by bilateral donors such as the United States and multilateral organizations such as the Global Fund, can be regarded as the test case where donors pioneered the concept of performance-based aid.[7] Eichler and Glassman (2008: 21) conclude that the shift in focus of ODA for health to outputs and outcomes as the most relevant allocation criteria "has the potential to overcome common limitations associated with input-based financing." Similarly, Öhler et al. (2010) argue that ODA may become more effective once traditional forms of conditionality are replaced by retrospective performance appraisals, combined with sufficiently high ex-post aid rewards.

Health-related interventions are not only distinct insofar innovative ODA approaches have been tried predominantly in this sector. At the same time, ODA against HIV/AIDS may be peculiar in that its effectiveness is not undermined by selfish donor motives. In other sectors, ODA has repeatedly been shown to be allocated according to the donors' commercial and political interest (e.g. Alesina and Dollar 2000). Donor interest may also play a role in ODA targeted at HIV/AIDS. While some altruistic donors are committed to alleviate the suffering in the afflicted countries, other donors may be mainly concerned about security repercussions of the pandemic at home.[8] In contrast to commercial and political self-interest, however, HIV/AIDS-related donor interest is unlikely to distort the needs- and merit-based allocation of ODA. In other words, recipient and donor interests appear to be better aligned for ODA against HIV/AIDS. Indeed, Boussalis and Peiffer (2010: 3) conclude from their analysis of the determinants of bilateral ODA allocations: "In contrast to the findings of many studies on ODA flows which suggest that political factors are more important than need, we find that HIV/AIDS assistance is substantially influenced by the level of recipient need."

[6] The third target ("Have halted by 2015 and begun to reverse the incidence of malaria and other major diseases") is not addressed in the context of the present study.

[7] See Eichler and Glassman (2008) as well as Oomman et al. (2010) for a detailed presentation and discussion.

[8] For example, the United States has considered the HIV/AIDS pandemic to be a national security issue since the late 1990s; see Allen (2004) and, for a detailed discussion of US foreign policy in the context of HIV/AIDS, Fidler (2004).

Another reason why ODA directed at HIV/AIDS may be distinct is that "the global AIDS pandemic was experienced as something of a shock" (Lieberman 2007: 1427), in contrast to the chronic problems that recipients and donors got used to since decades. According to Morfit (2011: 64), "while AIDS has dominated the global consciousness, the same cannot be said of … other issues." The so-called "AIDS exceptionalism" implies both considerably higher attention and more resources. Higher attention could render ODA more effective, e.g., if alert donors as well as the public better scrutinize the outcomes in terms of prevention and care. All the same, this effect may be offset to the extent that substantially increased resources weaken the incentives to use them productively. For instance, Morfit (2011) observed in Malawi that HIV prevalence and incidence were little affected by a massive influx of ODA.

III.3 Data and Stylized Facts

We assess the effects of ODA on two HIV/AIDS outcome variables: the number of AIDS-related deaths of adults and children, and the number of new HIV infections. We focus on the former measure as UNAIDS provides point estimates for this variable covering a large number of countries and the period 1990-2007.[9] The number of AIDS-related deaths should decline if ODA is effective in providing better treatment of HIV infected people.[10] Likewise, the number of new infections should decline if ODA is effective in preventing the spread of the pandemic. Regrettably, country-specific estimates of the number of new HIV infections are not directly available from UNAIDS. We proxy the number of new infections in *t* by calculating the difference in the number of people living with HIV in *t* and *t-1*, and adding the number of AIDS-related deaths in *t*.[11] The number of new infections proxied in this way

[9] See Appendix III.1 for the definition of variables and details on the sources used. Note that the HIV/AIDS outcomes relate to the national level of developing countries. By contrast, the theoretical analysis of Sonntag (2010) focuses on specific HIV/AIDS interventions such as developing a vaccine from the perspective of effectively financing international public goods. For some countries and years, we take UNAIDS' lower or upper bound estimates, the average of the two (if both are available), or an estimate given by UNAIDS as "smaller than x" in order not to lose observations. We assess whether the results are sensitive to this procedure by excluding all observations for which a precise point estimate is lacking from UNAIDS in a robustness test (see below).

[10] Data on potentially superior measures of effective treatment, notably the number of disability adjusted life years (DALYs) gained, are not available for a sufficiently large panel of countries and years. For a discussion of measurement, notably on DALYs, see World Bank (1999), Gaffeo (2003), and Global Fund (2009).

[11] See also World Bank (1999: 54-56) on the relationship between HIV incidence (= new infections), HIV prevalence, and AIDS mortality.

proved to be extremely volatile. Therefore, we take two-year averages at the beginning and the end of each period.[12]

In contrast to AIDS-related deaths and new infections, the effects of ODA would be ambiguous by construction if we chose the number of people living with HIV as the dependent variable. On the one hand, this number rises to the extent that ODA helps infected people to live longer. On the other hand, the number declines to the extent that ODA helps prevent new infections. Therefore, we refrain from presenting estimations with the number of people living with HIV (or HIV prevalence) as the dependent variable.[13] Neither do we employ some other HIV/AIDS-related measures that have been used in previous studies. The most widely used measure is the coverage of ARV treatment (e.g., Nattrass 2006; Lieberman 2007; Peiffer and Boussalis 2010). Compared to the number of AIDS-related deaths, coverage of ARV treatment is an intermediate policy variable. As noted by Peiffer and Boussalis (2010), the available data on ARV treatment and other policy-related HIV measures are incomplete and their reliability may be limited.[14] In the present context it is more important, however, that data on ARV treatment and other policy-related measures are not available over a sufficiently long time interval.[15] In other words, it is only by using UNAIDS data on AIDS-related deaths as well as new infections that we are able to focus on aid effects over time, in contrast to previous studies which are mostly purely cross-sectional. The dependent variables used in the present study are available for a large number of countries since the 1990s.

The data on ODA are drawn from the OECD's Creditor Reporting System (CRS).[16] The CRS reports HIV/AIDS-related ODA mainly under purpose code 13040, i.e., "all

[12] For more details see Section 4.

[13] An anonymous referee strongly advised us to drop previous estimations with the number of people living with HIV. See also Lieberman (2007) as well as Peiffer and Boussalis (2010) for the ambiguity of this measure.

[14] According to UNAIDS (http://unstats.un.org/unsd/mdg/Metadata.aspx?IndicatorId=0&SeriesId=765), "a particular source of uncertainty is that some country-reported data do not distinguish between people who have ever started antiretroviral therapy and those who are still receiving it."

[15] This also applies to the AIDS Program Effort Index employed by Lieberman (2007) and HIV education in school employed by Peiffer and Boussalis (2010).

[16] These data are available online since 1995. While data availability tends to constrain our period of observation, this does not appear to be problematic in the present context (see also below). We are also unlikely to lose relevant information by restricting the analysis to bilateral and multilateral donors reporting to the CRS. This is even though a new database, AidData (http://www.aiddata.org/home/index), covers some more donors that are not members of the OECD's Development Assistance Committee (DAC). The non-DAC donors typically have rather small overall aid budgets and they devoted a marginal share of their budgets to the fight

activities related to sexually transmitted diseases and HIV/AIDS control, e.g., information, education and communication; testing; prevention; treatment and care" (http://www.oecd.org/dac/stats/crs/hivaids).[17] Another purpose code (16064) was created in 2005 to separately identify special ODA programs for social mitigation of HIV/AIDS. Examples include: supporting vulnerable groups and children orphaned by HIV/AIDS and the human rights of HIV/AIDS affected people. Overall ODA reported under purpose code 16064 was just a small fraction of ODA reported under purpose code 13040 (three percent in 2005-2007). Yet we employ the sum of both purpose codes in our estimations performed below.

Principally, disbursements of ODA should be preferred over commitments when assessing the effectiveness of ODA. Commitment data may be inflated by donor promises that are not kept at all or only with delay. However, data on ODA disbursements for specific purposes such as the fight against HIV/AIDS are available from the CRS database since 2002 only. Hence, we use commitments which are in constant prices of 2008. ODA is defined in per-capita terms of the recipient countries' population.

As noted by Morfit (2011), it was only in the 1990s that the international attention focused on HIV/AIDS. Indeed, ODA directed at HIV/AIDS was marginal until the late 1990s (Figure III.1) so that the lack of earlier data is unlikely to involve a significant loss of relevant information. ODA from all donors increased to US$ 1.5 billion (in 2008 prices) until 2002. The fight against HIV/AIDS received much greater emphasis from multilateral and bilateral donors around that time. In June 2001, heads of state and government issued the Declaration of Commitment on HIV and AIDS at the special session of the United Nations General Assembly on HIV/AIDS. This UN session is considered "a major milestone in the AIDS response" helping "to guide and secure action, commitment, support and resources for the AIDS response."[18] The Global Fund became operational in 2002, and PEPFAR was launched in 2003 as "the largest effort by any nation to combat a single disease".[19] ODA soared to US$ 7.5 billion in 2008. Figure III.1 also reveals that the United States is the most important donor by far, accounting for almost half of ODA from all donors over the whole period of 1995-2008.[20] The Global Fund stands out among multilateral donors and contributed 22 percent to overall ODA since starting operations.

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against HIV/AIDS (details are available on request). We thank an anonymous referee for alerting us to this data issue.

[17] For a detailed description of aid activities related to HIV/AIDS as reported by the CRS, see OECD (2007).

[18] See: http://www.unaids.org/en/AboutUNAIDS/Goals/UNGASS/default.asp.

[19] See: http://www.pepfar.gov/about/index.htm.

[20] The US share was even 55 percent in 2006-2008.

ODA against HIV/AIDS is concentrated on a few major recipient countries. The 20 largest recipients absorbed three quarters of overall ODA in 2003-2007 (Table III.1). This share was similarly high in the first sub-period of 1998-2002. Moreover, the lists of the largest recipients in absolute terms overlap considerably in the two sub-periods.[21] In per-capita terms, six recipients received more than ten US$ of ODA to fight HIV/AIDS in 2003-2007. The steep increase in ODA directed at HIV/AIDS is clearly reflected in that per-capita transfers of four US$ were sufficient for Cape Verde to rank first in 1998-2002, while Cambodia ranks 18th with the same amount in 2003-2007. Figure III.2 underscores the point made by Boussalis and Peiffer (2010) that ODA directed at HIV/AIDS is largely driven by recipient need. The median of per-capita ODA is below 0.5 US$ for countries with low HIV prevalence, compared to 5.7 US$ for countries with high HIV prevalence.

While the data on HIV/AIDS and ODA are available for essentially all developing countries, our overall sample comprises only those developing countries for which the rate of HIV prevalence exceeded one percent of the adult population in 2003. In other words, we exclude all developing countries in which the HIV/AIDS epidemic cannot be considered "generalized."[22] This is in order to avoid our results to be biased in favor of finding ODA to be effective. As shown in Figure III.2, countries without pressing HIV/AIDS problems are unlikely to receive higher HIV/AIDS-related ODA, while HIV prevalence could only rise from practically zero. On the other hand, a higher threshold than one percent reduces the number of remaining observations considerably. Moreover, the World Bank (1999: 280) argued that donors should pay particular attention to countries with "nascent" epidemics where prevention is most cost-effective; this suggests that setting the threshold too high would miss relevant observations. The 47 sample countries included in the baseline estimations are listed in Appendix III.3.[23]

III.4 Method

We perform difference-in-difference-in-differences (DDD) estimations to assess the effects of ODA on the number of AIDS-related deaths and HIV infections. While other estimation approaches (e.g., fixed-effects estimations) may offer valid alternatives, we rely on a DDD

[21] Six recipients appear on just one list; Botswana, Namibia, Haiti, DR Congo, Cote d'Ivoire and Thailand joined the top-20 in 2003-2007.

[22] Note that the epidemic is often considered "generalized" if HIV prevalence exceeds one percent in antenatal clinics (e.g., Bendavid and Bhattacharya 2009). For reasons of data availability the one percent threshold applied here relates to HIV prevalence among all adults.

[23] In Section 5 we present several robustness tests with reduced samples.

approach since we are particularly interested in analyzing whether the steep increase in ODA directed at HIV/AIDS in a limited number of recipient countries in recent years had an impact on the evolution of the HIV/AIDS epidemics in those countries. This approach appears to be most appropriate recalling Lieberman's (2007) notion of the AIDS pandemic having been perceived as a shock which led to the creation of new specialized multilateral institutions, notably the Global Fund, and new national programs such as PEPFAR. Considering the development of ODA shown in Figure III.1, it seems most natural to compare the five-year period 2003-2007 with the previous five-year period (1998-2002) when the aid amounts were still relatively moderate. The dividing line is set at 2002/03 as ODA commitments by all donors directed at HIV/AIDS more than doubled from US$ 1.47 billion in 2002 to US$ 3.06 billion in 2003.

The distinguishing feature of a DDD approach is to have a treatment group and a control group. This distinction is based on the stylized facts shown in Section 3. Even though countries with high HIV prevalence clearly received more ODA than countries with low HIV prevalence, there are major differences in ODA directed at HIV/AIDS between recipient countries with similar epidemics. Such differences have also been stressed by the Global Fund (2009). In other words, donors "treated" a limited number of afflicted countries with substantially increased ODA. This enables us to split the overall sample into the two groups. We allocated those countries for which the increase in per-capita ODA from all (bilateral and multilateral) donors was relatively high (i.e. above the median) to the treatment group, while those countries for which the increase in ODA was relatively low (plus two countries for which ODA declined) to the control group.[24]

The DDD approach combines before-after comparisons and with-without comparisons. This helps mitigate important limitations that plague both types of comparisons when employed in isolation. The simple before-after approach would compare HIV/AIDS outcomes in developing countries prior and subsequent to a general shift in donor emphasis. Clearly, the implicit assumption that no other omitted variable might have affected the HIV/AIDS outcome variables over time is unlikely to hold. The simple with-without alternative of comparing HIV/AIDS outcomes between countries receiving high amounts of

[24] In the treatment group the average increase in ODA amounts to 8.75 US$ per capita. In the control group the average increase is just 0.97 US$. As described in Section 5 below, we check whether our main results are sensitive to choosing the median of the increase in per-capita ODA as the dividing line between the treatment and controls groups. We also present several robustness tests with reduced samples.

ODA and those receiving low or no ODA would ignore that outcomes might have developed differently in the groups due to factors unrelated to ODA.

By applying the DDD estimator, we remove any fixed country effects (first differences) and any fixed time trends (second differences).[25] Formally, the DDD estimator for our baseline specification is as follows:

$$DDD = ((HIV^{T}_{2007} - HIV^{T}_{2003}) - (HIV^{C}_{2007} - HIV^{C}_{2003}))$$
$$- ((HIV^{T}_{2002} - HIV^{T}_{1998}) - (HIV^{C}_{2002} - HIV^{C}_{1998}))$$

with *HIV* being the level of either the number of new HIV infections or the number of AIDS-related deaths in treatment group *T* and control group *C*, respectively, in the years indicated.[26] The estimator corresponds to the coefficient of the interaction term between a dummy for the treatment group and a dummy for the period 2003-2007 in the basic regression specification with the change in the number of new HIV infections or the change in the number of AIDS-related deaths as the dependent variable and without control variables. This specification is then extended in several steps. First, we add the level of the dependent variable at the beginning of the first and second periods (i.e., 1998 and 2003).[27] In this way we take into account that changes in the outcome variables may depend on their levels at the beginning of the periods of observation. Ignoring this factor may bias the effects of ODA.

Second, we include various other control variables (at the beginning of the first and second period) that may affect the changes in the outcome variables.[28] We include the countries' population as both dependent variables are defined in absolute numbers. The countries' GDP per capita may affect the dependent variables as higher average incomes provide better opportunities for costly treatment and prevention programs.[29] The chances to fight HIV/AIDS might also be relatively favorable in countries with better control of corruption. By contrast, local conditions appear to be particularly unfavorable in countries

[25] See also Johnson and Zajonc (2006).

[26] For new HIV infections we use the two-year averages 1998/99, 2001/02, 2003/04 and 2006/07 because of the extreme volatility of this variable.

[27] For new HIV infections we again use the two-year averages 1998/99 and 2003/04.

[28] See Appendix III.1 for definitions and sources, and Appendix III.2 for summary statistics.

[29] However, the relations between per-capita income and HIV/AIDS-related outcomes are quite complex. While higher incomes are associated with better treatment, HIV was found to be "unique among widely prevalent infectious diseases in striking rich people in the same proportion, or larger proportions, than it strikes the poor" (World Bank 1999: 207). The World Bank study shows that HIV/AIDS is not necessarily more widespread in poorer regions, especially in the early stages of the epidemic. On the other hand, poor households are clearly less able to cope with the consequences and afford adequate treatment.

suffering from civil war.[30] Public health expenditure (as a share of GDP) may reflect the local government's commitment to tackle health problems, including HIV/AIDS. An extended list of control variables enters some robustness tests specified in Section 5 below.

Finally, we interact the level of the dependent variable as well as the other control variables with the dummy variable for the second period 2003-2007. The identifying assumption of our DDD estimator is that, in the absence of the treatment, the difference in the dependent variable between the two periods would have been the same, on average, in the treatment and control group. The plausibility of this assumption is debatable if the treatment and control group differ from each other in certain aspects that might be associated with the dynamics of the outcome variable (Abadie 2005). In our case, the two groups differ particularly with respect to the level of the outcome variables (see Appendix III.2). Concerning AIDS-related deaths, a relatively high initial level of the outcome variable is strongly associated with a relatively large increase in the outcome variable in the first period, while this correlation weakens considerably in the second period.[31] Ignoring these dynamics in the outcome variable would violate the identifying assumption and bias the results with respect to the treatment effect. The treatment and control groups also differ with respect to other control variables, though to a lesser extent. Again, the interaction of the control variables with the dummy variable for the second period accounts for different dynamics in the two groups.

III.5 Results

Baseline results

Table III.2 reports our baseline results with the change in the number of new HIV infections (columns 1-6) and the change in the number of AIDS-related deaths (columns 7-12) as dependent variables. We also considered the rate of HIV prevalence and the number of people living with HIV as dependent variables. For the reasons stated in Section 4 these estimations are not shown, however. We proceed in several steps to evaluate the treatment effect of ODA. In columns (1) and (7) of Table III.2, we present the basic DDD estimations without any additional controls. In the next step, we add the number of new infections in 1998/99 and

[30] Civil war conditions are proxied by setting a dummy variable equal to one if a major internal armed conflict (at least 1,000 battle-related deaths in one year) occurred in the recipient country during the respective period (1998-2002, 2003-2007).

[31] In the first period, the correlation between the initial level and the change in *AIDS deaths* amounts to 0.64. In the second period, the correlation declines to 0.12.

2003/04 (column 2) and, respectively, the number of AIDS-related deaths in 1998 and 2003 (column 8). In columns (3) and (9), we enter the full list of control variables introduced above, in order to account for the effects of these variables on the changes in the outcome variable. In columns (4) and (10), we interact the level of the dependent variable with the dummy variable for the second period. We interact all control variables with the second period dummy in columns (5) and (11). Finally, we replicate the last step for a slightly reduced sample in order to allow for a clearer break between the treatment and control group (see below).

The results of the basic DDD estimations differ considerably between the two outcome variables. In column (1) with the change in the number of new infections as the dependent variable, the negative coefficient of *Treatment * 2nd period* fails to pass conventional significance levels. In other words, there is no convincing evidence that ODA has been effective in causing a more favorable dynamic in the treatment group with respect to the change in the number of new infections. The lack of convincing evidence suggests that aid has not contributed to effective prevention against HIV/AIDS. It should be recalled, however, that these estimations are based on estimated numbers of new infections; the volatility of this measure, though reduced by taking two-year averages, cautions against strong conclusions.

By contrast, the basic DDD estimation reported in column (7) of Table III.2 reveals a significant treatment effect of ODA on the change in the number of AIDS-related deaths. Note that the dynamic of AIDS-related deaths is relatively unfavorable in the treatment group when considering the first period 1998-2002, compared to the control group.[32] However, the relatively unfavorable dynamic in the treatment group disappears in the second period 2003-2007. In other words, no significant difference between the treatment and the control group can be observed anymore.[33] Taken together the treatment effect of ODA is significant at the ten percent level. According to this estimate, the stronger increase in ODA in the treatment group, compared to the control group, led to 16,665 fewer AIDS-related deaths, on average, in a country of the treatment group in the second period.[34]

The different results for the two outcome variables are hardly affected when controlling for their levels at the beginning of the first and the second period in columns (2)

[32] The marginal effect is significant at the five percent level.

[33] Throughout this section, the marginal effects and the significance levels of variables included in interaction terms are calculated with the "margins" command of Stata 11.0.

[34] The difference in the increase in ODA between the treatment and the control group amounts to 7.78 US$ per capita, on average.

and (8). This is even though the changes in the outcome variables strongly depend on initial levels. Higher initial levels are associated with a smaller increase (or a stronger decline) in the number of new infections and, respectively, with a higher increase (or smaller decline) in AIDS-related deaths, both at the one percent level of significance. Yet the treatment effect of ODA remains as before – insignificant with respect to the number of new infections, but significant (now at the five percent level) with respect to AIDS-related deaths. The major results also hold when adding the levels of a broader set of control variables at the beginning of the first and the second period. In fact, most of the additional control variables do not affect the changes in the outcome variables in columns (3) and (9) of Table III.2. The only exception is the positive impact of a larger population on the change in the number of infections and deaths (larger increase or smaller decline).

The interaction of the level of the dependent variable with the dummy variable for the second period proves to be relevant in column (10) with AIDS-related deaths as the dependent variable, in contrast to column (4) with the number of new infections. The results reported in column (10) indicate that the positive effect of the level on the change in the number of AIDS-related deaths weakens considerably in the second period.[35] Given that the level is, on average, considerably larger in the treatment group than in the control group, we can infer that the treatment effect of ODA would be overestimated if we ignored these dynamics. This bias is fairly pronounced in column (10) where the treatment effect of ODA with respect to AIDS-related deaths is no longer significant. In other words, we no longer find ODA to be effective in reducing the number of AIDS-related deaths once it is taken into account that the treatment and control groups differ in the level of the number of deaths and that this difference is associated with the dynamics of this outcome variable.

Apart from the levels of the dependent variables, the relevance of most other control variables continues to be weak when taking account of the dynamics of their levels between the first and second period. This applies particularly to the results in column (5). Importantly, the treatment effect of ODA on the number of new infections remains insignificant. In column (11) larger countries show an unfavorable dynamic in the first period, but this effect disappears in the second period. Considering that the countries in the control group are, on average, larger than the countries in the treatment group, we can infer that the treatment effect of ODA on AIDS-related deaths would be underestimated if we did not consider these dynamics. In fact, the treatment effect is, in absolute terms, larger in column (11) than in column (10). However, it still remains insignificant.

[35] The effect of the level proves to be even insignificant in the second period.

So far we split the sample into the treatment and control group by taking the median of the change in ODA per capita as the dividing line. As noted above, the average increase in ODA differs considerably between the two groups when applying this procedure. However, the median may represent an arbitrary dividing line as ODA per capita is only modestly above the median for some countries in the treatment group, and only modestly below the median for some countries in the control group.[36] Therefore, we re-estimated the specification in columns (5) and (11) for a reduced sample by excluding ten sample countries with an increase in ODA relatively close to the median. As shown in Figure III.3, this results in a clear break in ODA per capita from $1.49 for Benin in the control group to $3.16 for Cambodia in the treatment group.[37] By widening the gap between the groups in this way, we test whether the previously reported results are sensitive to the choice of the median as the dividing line. This does not appear to be the case according to the findings shown in columns (6) and (12) of Table III.2. Compared to columns (5) and (11), the results are hardly affected. A minor exception is that the interaction of the level of AIDS-related deaths with the dummy variable for the second period loses its significance in column (12). Importantly, both treatment effects are insignificant once again. Excluding more countries in the middle of the distribution (and, thus, widening the gap even more) does not alter the results.[38]

ODA from all donors: robustness tests

In Table III.3 we report the results of four robustness tests. As before, the classification of the treatment and control groups is based on the increase in ODA per capita from all donors, as reported under purpose codes 13040 and 16064 in the CRS database. We return to using the median as the dividing line between the treatment and control groups, as in columns (1) – (5) and (7) – (11) of Table III.2, in order to maintain a larger number of observations. Once again we assess the treatment effect of ODA with regard to the number of new HIV infections and the number of AIDS-related deaths. The two periods under consideration, 1998-2002 and 2003-2007, are also the same as before.

The first robustness test reported in columns (1) and (5) excludes 13 countries from the treatment group that have HIV prevalence rates greater than the maximum level of HIV

[36] We are grateful to an anonymous referee for alerting us to this issue.

[37] Now, the average increase in ODA amounts to 11 US$ per capita in the treatment group. In the control group it corresponds to 0.73 US$ only.

[38] These results are available on request.

prevalence rates in the control group, i.e., 5.8 percent (in 2003).[39] Reducing the treatment group in this way serves to remove the considerable difference in the level of the outcome variables compared to the control group.[40] In other words, it provides an alternative to including the interactions of the level of dependent variables with the dummy variable for the second period. Importantly, this robustness test corroborates the previous finding that ODA has failed to reduce either the number of new infections or the number of AIDS-related deaths in the treatment group, compared to the control group, once we control for differences in the level of the dependent variables.

Our major result also holds when restricting the estimations to sample countries located in sub-Sahara Africa (columns 2 and 6). This is not surprising as the 11 sample countries located in other regions spread across the whole spectrum as concerns the increase in ODA, even though their HIV prevalence rate was relatively low and varied only modestly (Appendix III.3). The exclusion of observations for which an exact point estimate of the outcome variables was not available from UNAIDS does not change our results either (columns 3 and 7).

Finally, we report the results from an extended specification in columns (4) and (8).[41] Two control variables, the number of people affected by drought and the scarcity of food (proxied by food consumption per capita), are primarily meant to account for an adverse local environment; the dynamics of both variables may affect changes in AIDS-related deaths. Two further variables are included to capture the willingness of politicians to engage in the fight against HIV/AIDS. Lieberman (2007) argues that ethnic fractionalization weakens the political engagement; Peiffer and Boussalis (2010) expect the same from cultural traditionalism (proxied by female participation in the formal workforce). We enter ODA directed to healthcare as well as water and sanitation systems as foreign support in these fields may also help fight HIV/AIDS. The last variable added in the extended specification, the density of roads, reflects the quality of local infrastructure and is meant to capture the degree to which ODA can be absorbed effectively in the recipient country. However, as can be seen in Table III.3, these additional control variables prove to be insignificant with just two

[39] See Appendix III.4 for the countries in the respective treatment and control groups.

[40] Now, the level is even somewhat higher in the control group than in the treatment group: The number of AIDS-related deaths amounts to 17,430 in the treatment group and 18,050 in the control group, on average.

[41] We would like to thank an anonymous referee for suggesting the additional control variables introduced here.

exceptions in column (4).[42] At the same time, the treatment effects of ODA against HIV/AIDS are still insignificant.

Differences between major donors

So far we have separated the treatment group from the control group on the basis of the increase in total HIV/AIDS-related ODA per capita received by a developing country from all sources. Subsequently we take into account that the effectiveness of ODA may differ between major sources. In particular, ODA from multilateral donors such as the Global Fund to Fight AIDS, Tuberculosis and Malaria is widely perceived to be superior to ODA from selfish bilateral donors, notably major DAC countries such as the United States.[43] The empirical evidence supporting this view is inconclusive, however (Ehrenfeld 2004). Ram (2003) even finds that positive economic growth effects of bilateral ODA are largely offset by negative growth effects of multilateral ODA.

Against this backdrop we replicate the estimations with changes in AIDS-related deaths as the dependent variable, reported in columns (7)-(11) of Table III.2, by refining the treatment group.[44] In addition to belonging to the upper half of the sample in terms of the increase in ODA from all sources, we restrict the treatment group to either those recipient countries for which HIV/AIDS-related ODA comes mainly from bilateral sources (all DAC countries) or those for which ODA comes mainly from (all) multilateral sources. The results for the treatment group with the DAC countries as the major source of ODA are reported in columns (1)-(5) in Table III.4, and those for the treatment group with multilateral organizations as the major source in columns (6)-(10).

Table III.4 reveals that the results for the control variables, including the levels of the dependent variables, are largely as before in Table III.2. This implies that the effects that most of the control variables exert on the change in the number of AIDS-related deaths do not differ considerably between the estimations with DAC countries as the major source of ODA

[42] While ethnic fractionalization is associated with a higher number of new HIV infections in the first period, the positive impact of female labor participation in this period in column (4) is rather counterintuitive. Both factors are no longer relevant in the second period.

[43] For instance, *The Economist* has argued repeatedly that multilateral organizations "reach the poor more accurately" (March 14th, 2002), whereas "bilateral aid is of dubious quality" (June 2nd, 2005) because of strategic and commercial self-interest of donor countries such as the United States.

[44] We also replicated the estimations with changes in the number of new HIV infections as the dependent variable. These estimations did not offer additional insights and are not reported. The results are available on request.

and those with multilateral organizations as the major source. An exception is the dynamics of the output variable due to a country's population, which can be observed in the case of bilateral aid only. At the same time, the results clearly show that the treatment effects presented before for ODA from all sources are driven by ODA effects in those countries receiving ODA mainly from DAC countries. The treatment effect proves to be insignificant throughout columns (6)-(10) with multilateral organizations as the major source of ODA. This finding sharply contradicts the widely held belief that multilateral ODA is more effective in promoting economic and social development, including by fighting HIV/AIDS.

By contrast, the treatment effect is significantly negative in columns (1)-(3) with bilateral donors as the major source of ODA. According to these estimates, the stronger increase in ODA in the treatment group, compared to the control group, leads to 25,000-29,000 fewer AIDS-related deaths, on average, in a country of the treatment group in the second period.[45] The impact is thus more pronounced than that reported in columns (7)-(9) of Table III.2. As before in Table III.2, the treatment effect loses its significance when controlling for the dynamics in the outcome variable by the interaction of its level at the beginning of the two periods with the dummy variable for the second period (column 4). In other words, the treatment effect is overestimated once again if we do not control for these dynamics. However, when controlling for the dynamics due to the countries' population in the full specification (column 5), the treatment effect is only insignificant at the margin. It also increases in size, compared to column (4).[46]

Importantly, the results in Table III.4 point to striking differences when accounting for the source of ODA. It should be noted in this context that, in most countries with a particularly large increase in ODA, the funds come mainly from bilateral sources (see Appendix III.4). This implies that the difference in the increase of ODA between the treatment and the control group is more pronounced when bilateral donors represent the major source, compared to multilateral donors being the major source. The differences found in the effectiveness of bilateral and multilateral ODA are probably related to this pattern. In particular, bilateral donors are more likely to make a difference as they tend to focus on a few

[45] Now, the average increase in ODA amounts to 11.04 US$ per capita in the treatment group. Thus, the difference in the increase in ODA between the treatment and the control group amounts to 10.07 US$ per capita, on average.

[46] Note that, as in Table III.2, the treatment effect is biased downwards if we do not account for the dynamics due to the countries' population. Interestingly, the coefficients of the level of the outcome variable and of its interaction with the second period lose almost their significance when controlling for the dynamics due to the countries' population.

recipient countries.[47] The World Bank (1999) observed more than a decade ago already that the allocation of HIV/AIDS-related ODA across recipient countries differed between bilateral and multilateral donors: Bilateral donors appeared to be particularly concerned about HIV/AIDS in recipient countries where the epidemic was most severe – either because they were altruistically responding to serious suffering in these countries, or because they viewed "their self-interest as jeopardized most acutely by countries where there are large numbers of infected people" (World Bank 1999: 250). The World Bank posited that, compared to bilateral donors, multilateral organizations were less focused, spreading ODA more widely including to countries with nascent epidemics and minor HIV/AIDS problems.

This issue is investigated further by concentrating on two dominant donors, the United States among DAC countries and the Global Fund among multilateral organizations. Both donors accounted for about 70 percent of bilateral and, respectively, multilateral ODA flows reported under purpose codes 13040 and 16064 to all recipient countries during the period 2003-2007. At the same time, the United States and the Global Fund represent examples of the different allocation behavior noted above. About 72 percent of country-specific ODA reported by the United States under the above purpose codes was concentrated in just ten recipient countries in 2003-2007; the corresponding share of the top-10 recipients of ODA from the Global Fund accounted for just 46 percent of the Fund's overall commitments (CRS database).

The results shown in Table III.5 indicate that the US approach was quite successful in containing the number of AIDS-related deaths.[48] Similar to the previous procedure, two criteria apply for countries in the treatment group: an increase in ODA from all sources above the median of the overall sample, and either the United States (columns 1-10) or the Global Fund (columns 11-15) representing the major donor. The results for the Global Fund mirror those for all multilateral organizations as the major source of ODA for countries in the treatment group: the treatment effect remains insignificant throughout at conventional levels. This is not surprising as the treatment group comprises almost the same set of countries independent of whether all multilateral organizations or the Global Fund is regarded as the major donor (Appendix III.4). By contrast, the results with the United States as the major donor show a significant treatment effect not only in the specifications without the

[47] This becomes apparent when looking at the countries in the control group where HIV/AIDS-related ODA comes mainly from multilateral sources.

[48] Again, we do not report the results with the number of new HIV infections as the dependent variable. US ODA did not appear to be superior to ODA from the Global Fund in this regard.

interactions of the control variables with the second period dummy (as before for all DAC countries), but even in the full specification in column (5). Moreover, the quantitative impact increases to almost 30,000 fewer AIDS-related deaths. Note that the level of the outcome variable and its interaction with the second period dummy are no longer significant when controlling for the dynamics due to the countries' population. The results indicate that the treatment effect is underestimated if no dynamics of any kind are taken into account (as in columns 1-3).

The quantitative impact increases further to about 35,000 fewer deaths when replicating the full specification with an additional requirement for countries to be included in the treatment group. In columns (6)-(10) the United States must not only be the major donor, but the recipient countries must also be on the list of PEPFAR's so-called focus countries. In all other respects, the results in columns (6)-(10) differ only marginally from those in columns (1)-(5) of Table 5. This is plausible considering that the overlap between recipient countries where the United States is the major donor and the focus countries of PEPFAR is almost perfect, with Cambodia representing the only exception (Appendix III.4).

The significant treatment effect of ODA that we find in PEPFAR's focus countries is in line with the earlier findings of Bendavid and Bhattacharya (2009). According to Bendavid and Bhattacharya, a significant difference exists between PEPFAR's focus countries and these authors' control group of other sub-Saharan African countries with respect to the number of deaths due to AIDS in the period 2004-2007. More precisely, they show the annual change in the number of AIDS-related deaths to be about ten percent lower in the focus countries.[49] Recalling that the analysis of Bendavid and Bhattacharya is restricted to PEPFAR's activities in sub-Sahara Africa, the comparability with our results is limited. However, our more comprehensive analysis of all bilateral and multilateral donors clearly underscores that PEPFAR "is unique in its distinctive approaches and disproportionate funding of a few countries" (Bendavid and Bhattacharya 2009: 688).

Finally, we show in Table III.6 that the treatment effects of ODA reported in this sub-section are robust to the exclusion of observations for which UNAIDS does not provide exact point estimates of the number of AIDS-related deaths.[50] Once again, multilateral ODA as well

[49] By contrast, Bendavid and Bhattacharya (2009) do not find significant differences with respect to the number of people living with HIV and HIV prevalence rates.

[50] We report only the preferred full specification with the interactions included.

as ODA from the Global Fund proves to be ineffective.[51] Also as before, the treatment effect is significant at the five percent level and quantitatively most pronounced when the treatment group is restricted to PEPFAR's focus countries. The concentration on a few needy recipient countries appears to have helped ODA effectiveness. This is even though PEPFAR was widely criticized for earmarking a part of its funds for abstinence-only programs and refusing to cooperate with partner organizations offering counseling on abortion (e.g., Burns 2010: 160).

III.6 Summary and conclusion

We contribute to the nascent literature on the effectiveness of foreign official development assistance (ODA) that focuses on particular items of ODA meant to achieve specific objectives. The fight against HIV/AIDS epidemics figures prominently among the Millennium Development Goals agreed by the international community in 2000. Donor countries and multilateral organizations have mobilized steeply increasing resources in recent years to halt and reverse the spread of HIV/AIDS. Donors should know about the effectiveness of their ODA independent of whether they are altruistically committed to alleviate the suffering in the afflicted countries, or mainly concerned about security repercussions of the pandemic at home.

We employ a difference-in-difference-in-differences (DDD) approach to identify the treatment effect of ODA specifically targeted at sexually transmitted diseases on HIV/AIDS-related outcome variables. Controlling for various factors that may affect the dynamics of the number of new HIV infections and AIDS-related deaths, our empirical findings clearly indicate that any generalized verdicts on the effectiveness of ODA are unwarranted. The treatment effect of ODA varies considerably between different outcome variables and critically depends on the source of ODA.

Optimally, ODA would help prevent new HIV infections as well as provide better care for the infected. Our results indicate that ODA-financed prevention has been insufficient to reduce the number of new HIV infections. By contrast, we find evidence of significant treatment effects on AIDS-related deaths for the major bilateral source of ODA, the United States. However, the treatment effect proved to be insignificant when multilateral organizations represented the major source of ODA. In particular, our findings are in sharp conflict with claims of the most important organization in this field – the Global Fund to Fight

[51] Note that the treatment group is identical for the estimations with all multilateral donors and the Global Fund reported in Table III.6.

AIDS, Tuberculosis and Malaria – that its performance-based support has saved almost five million lives by the end of 2009.

A recent US assistance program, the President's Emergency Plan for AIDS Relief (PEPFAR), appears to be particularly effective in reducing the number of AIDS-related deaths. The treatment effect is significant across a number of different specifications, including when controlling for various factors affecting the dynamics of the outcome variable. At the same time, the quantitative impact is considerable with about 35,000 fewer deaths in a recipient country receiving an above-median increase in ODA mainly from PEPFAR in 2003-2007. This may be surprising recalling the harsh critique leveled against PEPFAR for earmarking a part of its funds for abstinence-only programs.

One may suspect that the striking difference between ODA from the United States and the Global Fund is mainly because the former donor commands over a larger aid budget to fight HIV/AIDS effectively.[52] Indeed, the overall amount of bilateral US ODA granted to all our sample countries in 2003-2007 (annual average of $1.4 billion in constant 2008 prices) exceeded the corresponding figure for the Global Fund ($1 billion). Fully comparable data are not available for PEPFAR, the new and major US scheme in this field. Nevertheless, the difference in overall bilateral ODA by PEPFAR and the Global Fund – both of which were not yet in operation until 2002 – is most likely far too small to explain why only PEPFAR had significant effects on the number of AIDS-related deaths.

At the same time, we would caution against concluding that multilateral aid from the Global Fund is inherently less effective than bilateral aid from major donor countries such as the United States. It rather appears that some particular features of PEPFAR rendered aid more effective – i.e., features that other donors, including multilateral institutions, might copy.

Among those features, the concentration of PEPFAR's financial support in a few recipient countries figures prominently. Selectivity with regard to the number of countries in which PEPFAR has engaged has the effect that annual average US aid in 2003-2007 to each country in the treatment group ($77 million) was almost three times as large as the corresponding figure for the Global Fund. The combination of performance-based aid allocation with relatively high financial "rewards" for considerable national efforts to tackle development problems may have strengthened the incentives of local governments and, thus, helped ODA effectiveness. Öhler et al. (2010) come to similar conclusions with regard to

[52] We would like to thank an anonymous referee for alerting us to this possibility.

innovative approaches of aid allocation by the United States and its effects on better control of corruption in the recipient countries.

Relating aid to the number of people living with HIV in the treatment groups reveals another difference between the two major donors. Surprisingly perhaps, the United States spent less per person infected with HIV in 2003-2007 ($360) than the Global Fund ($420). This indicates that the United States engaged in relatively large countries with high HIV prevalence. The focus on countries with relatively severe epidemics may render it easier politically to raise financial resources for fighting HIV/AIDS and strengthen public support in the donor country. The donor's self-interest in helping contain relatively severe epidemics appears to have worked against the so-called aid fatigue. However, the experience of raising more aid funds through a better alignment of recipient need and donor interest may prove hard to be transferred to other areas of development cooperation.

Finally, it cannot be ruled out that factors other than different allocation rules are underlying the inferiority of ODA from other sources. For instance, the focus of specific programs may also play an important role. Medical care for infected people may have effects that are easier to detect in the shorter run, compared to efforts to prevent HIV infections. This could have worked against the Global Fund and in favor of US schemes such as PEPFAR. Reports by the two donors suggest that the Global Fund devoted less than 40 percent of its resources to HIV treatment and care, compared to 70 percent in the case of PEPFAR.[53] The striking differences in the effectiveness of ODA from different sources clearly deserve more attention in future research.

[53] For the Global Fund, see e.g.: http://www.theglobalfund.org/documents/replenishment/2010/Global_Fund_2010_Innovation_and_Impact_en.pdf (page 25); for PEPFAR, see: http://www.avert.org/pepfar.htm.

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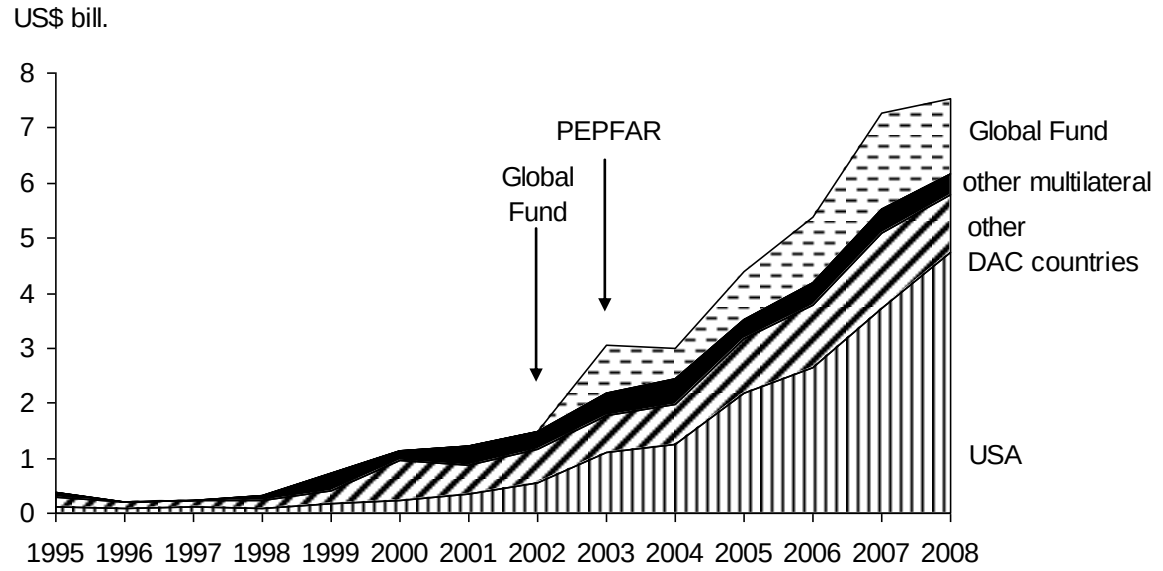
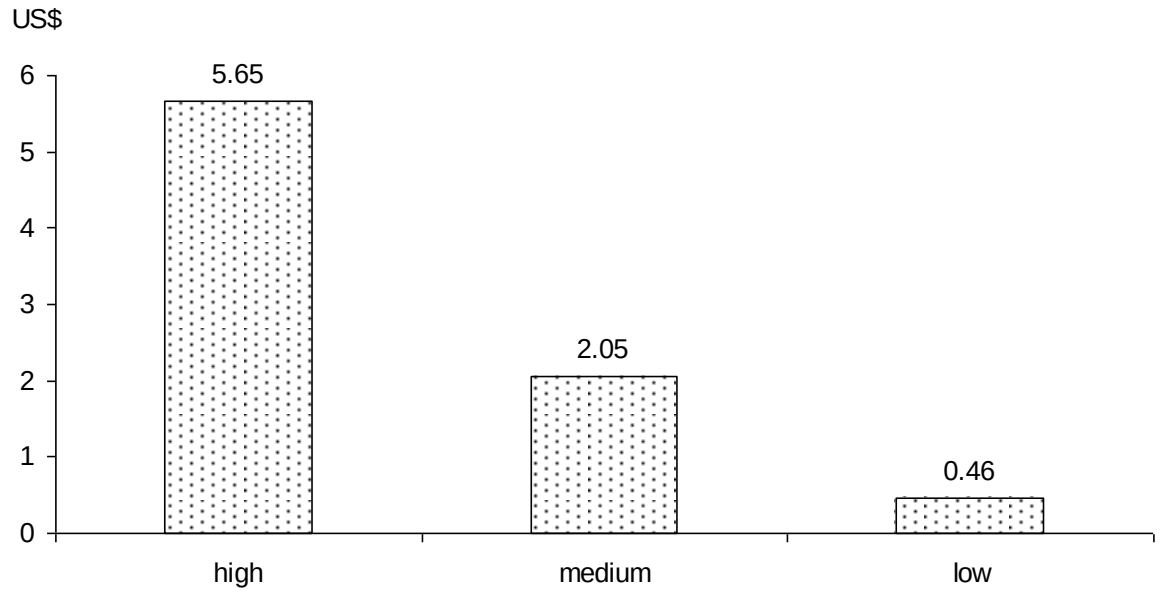
Figure III.1: Aid commitments by bilateral and multilateral donors to fight HIV/AIDS, 1995-2008 (US$ billion in constant prices of 2008)



Notes: Sum of DAC codes 13040 and 16064; data for 2009 are still incomplete.

Source: http://stats.oecd.org/Index.aspx?DatasetCode=CRSNEW

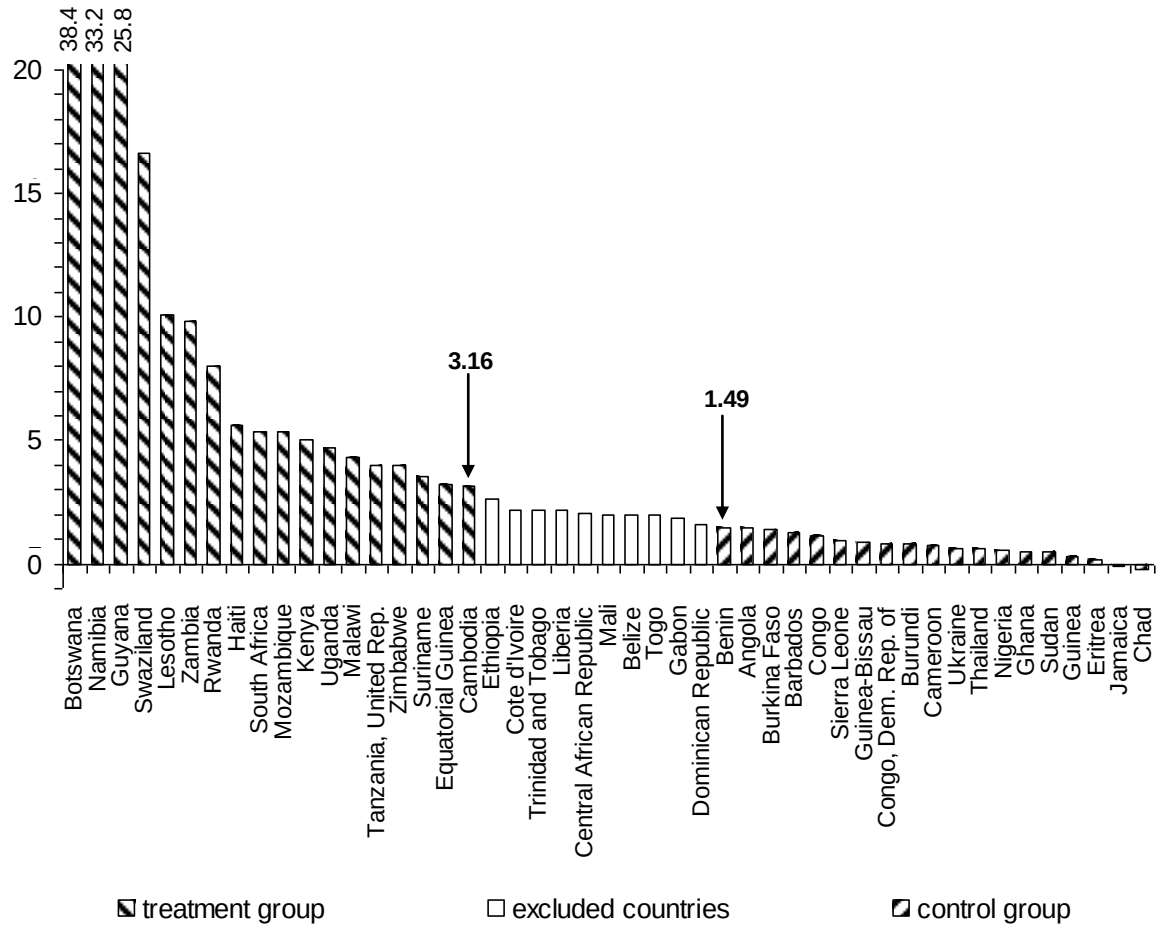
Figure III.2: Median of per-capita aid against HIV/AIDS (in 2003-2007) for sub-samples of countries with high, medium and low HIV prevalence (in 2003)



Notes: high = 24 countries with prevalence ≥ 2.5 percent; medium = 24 countries with prevalence of 1-2.4 percent; low = 55 countries with prevalence < 1 percent.

Source: http://stats.oecd.org/Index.aspx?DatasetCode=CRSNEW; UNAIDS

Figure III.3: Treatment versus control group: Increase in aid per capita, 2003-2007 compared to 1998-2002



Source: http://stats.oecd.org/Index.aspx?DatasetCode=CRSNEW

Table III.1 – Aid against HIV/AIDS: Top-20 Recipients, 1998-2002 and 2003-2007

	1998-2002					2003-2007				
	US$ per capita		US$ million			US$ per capita		US$ million		
1	Cape Verde	4.0	India	77.8	[11.6]	Botswana	40.3	Rep. South Africa	273.2	[8.3]
2	Papua New G.	3.5	Nigeria	76.0	[23.0]	Namibia	36.1	Ethiopia	224.2	[15.0]
3	Jamaica	2.9	Kenya	36.6	[28.5]	Guyana	26.9	Kenya	224.1	[21.8]
4	Namibia	2.9	Uganda	34.6	[33.6]	Swaziland	18.5	India	209.5	[28.1]
5	Grenada	2.5	Tanzania	28.9	[37.9]	Zambia	12.1	Tanzania	190.4	[33.9]
6	Zambia	2.3	China	24.4	[41.6]	Lesotho	11.0	Uganda	176.8	[39.2]
7	Eritrea	2.0	Zambia	24.2	[45.2]	Rwanda	9.1	Nigeria	168.5	[44.3]
8	Malawi	2.0	Malawi	23.3	[48.7]	Mozambique	6.6	Zambia	142.8	[48.6]
9	Botswana	1.9	Mozambique	22.9	[52.1]	Malawi	6.3	Mozambique	138.9	[52.8]
10	Swaziland	1.9	Ghana	21.5	[55.3]	Haiti	6.3	China	95.8	[55.7]
11	Gambia	1.8	Ethiopia	21.4	[58.5]	Kenya	6.2	Malawi	86.0	[58.3]
12	Zimbabwe	1.5	Papua New G.	19.0	[61.3]	Uganda	6.1	Rwanda	81.9	[60.8]
13	Uganda	1.4	Zimbabwe	18.7	[64.1]	Rep. South Africa	5.8	Botswana	75.2	[63.1]
14	Mozambique	1.2	Viet Nam	18.6	[66.9]	Zimbabwe	5.5	Namibia	73.6	[65.3]
15	Kenya	1.2	Rep. South Africa	18.4	[69.7]	Djibouti	5.3	Zimbabwe	68.4	[67.4]
16	Guyana	1.1	Indonesia	14.0	[71.8]	Tanzania	4.8	Haiti	59.6	[69.2]
17	Ghana	1.1	Cambodia	11.6	[73.5]	Papua New G.	4.5	Cambodia	56.1	[70.9]
18	Rwanda	1.0	Burkina Faso	9.8	[75.0]	Cambodia	4.0	Congo. DR	53.7	[72.5]
19	Senegal	1.0	Senegal	9.5	[76.4]	Equ. Guinea	3.7	Cote d'Ivoire	47.3	[73.9]
20	Cambodia	0.9	Rwanda	8.4	[77.6]	Suriname	3.6	Thailand	42.4	[75.2]

Notes: annual averages; ODA is in constant prices of 2008; the two sub-periods are defined as used in the subsequent estimations; in brackets: accumulated shares in percent of total aid against HIV/AIDS allocated to specific countries.

Source: http://stats.oecd.org/Index.aspx?DatasetCode=CRSNEW

Table III.2: ODA effects on the number of people living with HIV and AIDS-related deaths: Baseline results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	New infections						AIDS deaths					
2nd period	-621	-610	-1,765	-1,141	8,551	2,290	-6,700	-7,811	-9,356	-2,532	512	-299
	(6,386)	(4,720)	(4,263)	(4,826)	(9,862)	(11,106)	(5,943)	(5,653)	(6,019)	(5,353)	(12,451)	(15,534)
Treatment	-3,315	5,124	10,973**	5,552	10,274**	17,961***	12,285**	8,839	8,159	1,753	2,683	3,138
	(6,455)	(4,868)	(4,405)	(4,943)	(4,758)	(5,976)	(6,007)	(5,800)	(6,234)	(5,601)	(6,396)	(9,080)
Treatment * 2nd period	-405	-2,493	-2,791	-3,475	-1,435	-8,948	-16,665*	-18,994**	-16,303*	-8,130	-11,572	-16,553
	(9,128)	(6,751)	(5,902)	(6,988)	(6,708)	(8,509)	(8,496)	(8,098)	(8,357)	(7,895)	(8,942)	(12,809)
New infections		-0.159***	-0.212***	-0.167***	-0.209***	-0.218***						
		(0.018)	(0.019)	(0.023)	(0.025)	(0.027)						
New infections * 2nd period				0.022	-0.009	0.018						
				(0.038)	(0.041)	(0.045)						
AIDS deaths								0.155***	0.094	0.473***	0.335***	0.337**
								(0.047)	(0.062)	(0.088)	(0.121)	(0.145)
AIDS deaths * 2nd period										-0.419***	-0.245*	-0.230
										(0.101)	(0.138)	(0.166)
Log population			0.00042***		0.00044***	0.00051***			0.00022*		0.00038**	0.00040*
			(0.00008)		(0.00012)	(0.00014)			(0.00012)		(0.00017)	(0.00021)
Log population * 2nd period					-0.00004	-0.00020					-0.00047**	-0.00050*
					(0.00016)	(0.00019)					(0.00022)	(0.00029)
Log GDP per capita			-0.932		-0.955	-3.649			1.199		1.124	5.503
			(1.137)		(1.928)	(3.494)			(1.606)		(2.425)	(4.847)
Log GDP per capita * 2nd period					-0.145	1.316					0.451	-2.023
					(2.428)	(4.166)					(3.048)	(5.775)
Control of corruption			-2,663		-6,416	-4,379			4,953		5,114	4,522
			(3,120)		(4,481)	(5,212)			(4,316)		(5,498)	(7,100)
Control of corruption * 2nd period					7,553	3,646					-1,042	-468
					(6,474)	(7,162)					(7,960)	(9,758)
Public health expenditure			-1,489		101	-2,204			1,339		1,320	-134
			(1,416)		(2,310)	(2,787)			(2,001)		(2,962)	(3,908)
Public health exp. * 2nd period					-2,546	-80.213					-811	1,166
					(2,975)	(3,476)					(3,776)	(4,852)
Civil war			-2,957		-4,382	-6,982			-4,262		-10,103	-10,749
			(4,696)		(6,208)	(6,751)			(6,633)		(7,894)	(9,525)
Civil war * 2nd period					2,561	7,052					10,204	6,979
					(9,929)	(11,213)					(12,475)	(15,638)
Constant	-148	3,690	-78.672	3,885	-5,706	415	6,254	4,575	2,912	1,121	-996	-1,214
	(4,515)	(3,366)	(5,278.181)	(3,396)	(7,433)	(8,580)	(4,202)	(4,023)	(7,477)	(3,793)	(9,466)	(12,076)
Observations	94	94	90	94	90	70	94	94	90	94	90	70
Number of countries	47	47	45	47	45	35	47	47	45	47	45	35
R-squared	0.007	0.463	0.655	0.465	0.664	0.725	0.158	0.250	0.331	0.372	0.489	0.510

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table III.3: ODA effects on the number of people living with HIV and AIDS-related deaths: Robustness tests

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	New infections				AIDS deaths			
2nd period	7,711	9,490	19,448	15,370	-5,110	-2,238	2,778	7,066
	(6,032)	(11,455)	(12,665)	(34,875)	(5,632)	(13,828)	(17,362)	(48,550)
Treatment	-1,841	14,008**	9,888*	9,905*	-371	1,100	4,474	3,381
	(2,869)	(5,939)	(5,709)	(5,063)	(2,767)	(7,881)	(8,113)	(7,289)
Treatment * 2nd period	7,069*	-2,852	-1,753	-4,565	-4,258	-15,596	-13,247	-12,611
	(4,050)	(8,466)	(7,969)	(7,398)	(3,769)	(11,022)	(11,211)	(10,516)
New infections	-0.183***	-0.213***	-0.216***	-0.215***				
	(0.056)	(0.030)	(0.027)	(0.025)				
New infections * 2nd period		-0.020	-0.007	-0.031				
		(0.049)	(0.045)	(0.047)				
AIDS deaths					0.089	0.336**	0.282*	0.380**
					(0.116)	(0.137)	(0.152)	(0.147)
AIDS deaths * 2nd period						-0.248	-0.145	-0.286
						(0.156)	(0.171)	(0.173)
Log population	0.00034***	0.00045***	0.00048***	0.00050***	0.00040***	0.00045**	0.00042**	0.00032
	(0.00010)	(0.00015)	(0.00013)	(0.00013)	(0.00010)	(0.00018)	(0.00020)	(0.00020)
Log population * 2nd period	0.00006	0.00002	-0.00005	-0.00003	-0.00054***	-0.00043*	-0.00055**	-0.00040
	(0.00007)	(0.00020)	(0.00018)	(0.00017)	(0.00008)	(0.00025)	(0.00026)	(0.00026)
Log GDP per capita	0.496	-2.052	-1.169	2.036	0.190	5.717	3.515	-0.874
	(1.101)	(3.151)	(2.894)	(2.816)	(1.027)	(3.618)	(3.725)	(3.827)
Log GDP per capita * 2nd period	-0.324	0.075	-0.884	-0.081	0.231	-1.360	-1.799	2.204
	(1.386)	(3.982)	(3.956)	(3.806)	(1.292)	(4.608)	(5.127)	(5.165)
Control of corruption	-1,320	-7,850	-11,231*	-5,751	-1,184	7,698	5,904	7,639
	(2,675)	(5,301)	(6,211)	(5,039)	(2,495)	(6,081)	(8,144)	(6,719)
Control of corruption * 2nd period	3,675	10,111	12,729	6,802	-2,142	-149	-1,004	-4,623
	(3,953)	(7,896)	(8,600)	(7,587)	(3,690)	(9,104)	(11,232)	(10,171)
Public health expenditure	945	-23	3,132	1,506	-1,338	-583	1,161	1,030
	(1,839)	(2,964)	(2,907)	(2,565)	(1,716)	(3,629)	(4,132)	(3,716)
Public health exp. * 2nd period	-4,097	-2,360	-5,108	-3,575	1,441	1,070	-1,652	-1,814
	(2,568)	(3,688)	(3,578)	(3,210)	(2,416)	(4,465)	(4,957)	(4,548)
Civil war	1,382	-6,691	-587	-7,798	-7,961**	-11,421	-7,795	-7,721
	(3,629)	(7,298)	(6,844)	(6,562)	(3,325)	(8,841)	(9,367)	(8,888)
Civil war * 2nd period	-194	2,948	-1,220	5,682	11,906**	12,526	7,177	8,122
	(5,847)	(12,265)	(10,610)	(10,331)	(5,584)	(14,641)	(14,171)	(13,992)
Ethnic fractionalization				19,492*				-2,275
				(11,350)				(16,389)
Ethnic fractionalization * 2nd period				-4,477				-4,625
				(17,044)				(23,933)
Food consumption				-12.670				7.763
				(8.986)				(12.040)
Food consumption * 2nd period				5.575				1.081
				(12.259)				(16.611)
Female labor participation				346**				43
				(166)				(233)
Female labor participation * 2nd period				-327				-341
				(242)				(331)
ODA p.c. for health / water & sanitation				2,007				-2,154
				(2,752)				(3,732)
ODA p.c. * 2nd period				1,990				5,011
				(3,996)				(5,489)
Road density				4,321				-7,789
				(8,405)				(11,490)
Road density * 2nd period				-3,018				5,475
				(11,720)				(15,961)
Log people affected by drought				-71				-671
				(455)				(622)
Log people affected * 2nd period				736				862
				(743)				(1,035)
Constant	-5,143	-6,165	-16,449	-19,305	2,048	3,474	-1,436	-8,195
	(4,434)	(8,567)	(10,152)	(24,043)	(4,147)	(10,497)	(14,205)	(33,730)
Observations	64	70	72	88	64	70	72	88
Number of countries	32	35	36	44	32	35	36	44
R-squared	0.405	0.703	0.721	0.744	0.720	0.577	0.504	0.539

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

columns (1) and (5): additional control variables included;

columns (2) and (6): countries with HIV prevalence > 5.8 excluded from the treatment group;

columns (3) and (7): only countries in sub-Sahara Africa;

columns (4) and (8): excl. observations for which UNAIDS does not provide clear point estimates.

Table III.4: ODA effects on AIDS-related deaths: DAC countries versus multilateral organizations as major donors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Treatment group: DAC countries major donor					Treatment group: Multilateral major donor				
2nd period	-6,700	-7,969	-10,004	-3,287	1,735	-6,700**	-7,646**	-7,529**	745	-1,551
	(6,391)	(6,043)	(6,446)	(5,952)	(15,433)	(3,068)	(2,937)	(3,202)	(2,109)	(5,652)
Treatment	18,667**	12,262*	12,064	1,851	6,215	2,357	2,289	3,842	2,031	1,023
	(7,446)	(7,314)	(8,068)	(7,784)	(9,007)	(4,154)	(3,950)	(4,174)	(2,560)	(3,114)
Treatment * 2nd period	-25,193**	-29,245***	-25,269**	-13,697	-21,543	-3,400	-3,767	-414	-2,969	-2,367
	(10,530)	(10,018)	(10,190)	(10,865)	(13,056)	(5,874)	(5,587)	(5,788)	(3,622)	(4,382)
AIDS deaths		0.177***	0.107	0.463***	0.289*		0.132***	-0.184	0.633***	0.601***
		(0.056)	(0.075)	(0.111)	(0.152)		(0.048)	(0.129)	(0.062)	(0.203)
AIDS deaths * 2nd period				-0.374***	-0.160				-0.664***	-0.481**
				(0.126)	(0.175)				(0.072)	(0.237)
Log population			0.00024*		0.00046**			0.00033**		0.00002
			(0.00014)		(0.00020)			(0.00013)		(0.00015)
Log population * 2nd period					-0.00056**					-0.00021
					(0.00028)					(0.00021)
Log GDP per capita			1.667		2.710			-0.736		-0.508
			(2.672)		(3.476)			(0.889)		(0.999)
Log GDP per capita * 2nd period					-1.687					0.676
					(5.195)					(1.260)
Control of corruption			5,883		6,191			-1,090		-1,005
			(5,459)		(6,854)			(2,809)		(2,596)
Control of corruption * 2nd period					-687					-2401
					(10,537)					(3,910)
Public health expenditure			1,264		849			309		-119
			(2,928)		(3,707)			(1,450)		(1,813)
Public health exp. * 2nd period					534					-977
					(5,788)					(2,200)
Civil war			-6,798		-8,646			-4,080		-6,625*
			(7,522)		(9,032)			(4,212)		(3,652)
Civil war * 2nd period					5,144					8,802
					(15,111)					(5,827)
Constant	6,254	4,337	3,397	1,220	-2,155	6,254***	4,825**	3,363	-617	930
	(4,519)	(4,307)	(8,809)	(4,223)	(10,855)	(2,169)	(2,127)	(4,397)	(1,499)	(4,409)
Observations	76	76	72	76	72	66	66	62	66	62
Number of countries	38	38	36	38	36	33	33	31	33	31
R-squared	0.191	0.290	0.388	0.369	0.507	0.125	0.222	0.346	0.679	0.739

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table III.5: ODA effects on AIDS-related deaths: United States and Global Fund as major donor

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	Treatment group: US major donor					Treatment group: US major donor + PEPFAR					Treatment group: Global Fund major donor				
2nd period	-6,700	-8,140	-10,351	-3,762	-8,630	-6,700	-8,148	-10,423	-3,873	-10,653	-6,700**	-7,648**	-7,525**	720	-620
	(6,366)	(5,935)	(6,408)	(5,936)	(15,955)	(6,422)	(5,996)	(6,495)	(6,030)	(16,336)	(3,101)	(2,970)	(3,249)	(2,147)	(6,046)
Treatment	16,738**	10,435	9,882	1,853	7,436	18,321**	11,441	10,523	2,327	8,806	3,371	3,124	4,051	2,187	172
	(7,595)	(7,295)	(8,223)	(7,727)	(9,361)	(7,865)	(7,603)	(8,825)	(8,197)	(10,552)	(4,386)	(4,172)	(4,478)	(2,720)	(3,740)
Treatment * 2nd period	-21,492**	-25,632**	-22,730**	-12,967	-29,879**	-22,917**	-27,480**	-24,454**	-14,036	-35,049**	-4,600	-5,124	-1,428	-3,234	-1,971
	(10,741)	(10,060)	(10,378)	(10,793)	(13,440)	(11,123)	(10,447)	(10,814)	(11,434)	(14,933)	(6,203)	(5,902)	(6,125)	(3,851)	(4,910)
AIDS deaths		0.200***	0.144	0.473***	0.257		0.201***	0.149	0.468***	0.239		0.132***	-0.181	0.631***	0.615***
		(0.058)	(0.088)	(0.118)	(0.171)		(0.060)	(0.093)	(0.122)	(0.184)		(0.049)	(0.131)	(0.064)	(0.209)
AIDS deaths * 2nd period				-0.351**	0.013				-0.343**	0.061				-0.662***	-0.500**
				(0.134)	(0.200)				(0.138)	(0.214)				(0.074)	(0.242)
Log population			0.00019		0.00049**			0.00018		0.00050**			0.00033**		0.00001
			(0.00015)		(0.00021)			(0.00160)		(0.00022)			(0.00013)		(0.00016)
Log population * 2nd period					-0.00083***					-0.00088***					-0.00020
					(0.00030)					(0.00031)					(0.00021)
Log GDP per capita			1.719		3.162			1.772		3.314			-1.020		-0.963
			(2.820)		(3.594)			(2.884)		(3.667)			(1.177)		(1.475)
Log GDP per capita * 2nd period					-3.980					-4.536					0.648
					(5.246)					(5.341)					(1.784)
Control of corruption			4,358		6,531			4,209		6,309			-1,128		-1,550
			(5,625)		(6,707)			(5,707)		(6,816)			(2,935)		(2,929)
Control of corruption * 2nd period					-8,568					-8,558					-2,227
					(10,742)					(10,877)					(4,196)
Public health expenditure			1,361		-200			1,303		-695			404		473
			(3,577)		(4,623)			(3,779)		(4,967)			(1,540)		(2,308)
Public health exp. * 2nd period					6,544					8,187					-1,430
					(6,662)					(7,047)					(2,637)
Civil war			-7,886		-8,475			-8,085		-8,878			-4,165		-7,033*
			(7,529)		(8,763)			(7,673)		(8,957)			(4,339)		(3,825)
Civil war * 2nd period					1,397					2,358					8,786
					(14,726)					(14,948)					(6,011)
Constant	6,254	4,077	2,893	1,114	-683	6,254	4,065	2,941	1,166	-25	6,254***	4,821**	3,508	-602	114
	(4,502)	(4,234)	(9,226)	(4,217)	(11,282)	(4,541)	(4,279)	(9,446)	(4,286)	(11,615)	(2,193)	(2,151)	(4,484)	(1,526)	(4,866)
Observations	74	74	70	74	70	72	72	68	72	68	64	64	60	64	60
Number of countries	37	37	35	37	35	36	36	34	36	34	32	32	30	32	30
R-squared	0.156	0.281	0.369	0.347	0.518	0.163	0.285	0.371	0.346	0.525	0.133	0.229	0.351	0.678	0.743

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table III.6: ODA effects on AIDS-related deaths: Robustness tests for major donors

	(1)	(2)	(3)	(4)
	DAC countries	Multilateral (Global Fund)	US	PEPFAR
2nd period	6,377	14,925	-6,779	-8,326
	(19,952)	(9,629)	(20,592)	(21,078)
Treatment	5,964	-2,736	6,865	7,843
	(10,702)	(4,359)	(10,556)	(11,929)
Treatment * 2nd period	-19,854	-849	-29,319*	-33,963**
	(14,988)	(5,780)	(14,910)	(16,645)
AIDS deaths	0.237	0.632***	0.209	0.197
	(0.187)	(0.219)	(0.198)	(0.211)
AIDS deaths * 2nd period	-0.068	-0.519*	0.134	0.174
	(0.210)	(0.275)	(0.228)	(0.243)
Log population	0.00051**	0.00003	0.00053**	0.00054**
	(0.00024)	(0.00016)	(0.00024)	(0.00025)
Log population * 2nd period	-0.00064*	-0.00022	-0.00096***	-0.00101***
	(0.00032)	(0.00025)	(0.00034)	(0.00350)
Log GDP per capita	3.094	-3.114*	3.519	3.599
	(4.274)	(1.776)	(4.274)	(4.363)
Log GDP per capita * 2nd period	-0.818	2.174	-4.341	-4.752
	(6.201)	(2.274)	(6.164)	(6.290)
Control of corruption	6,593	-12,992**	7,419	7,227
	(9,672)	(5,468)	(9,550)	(9,756)
Control of corruption * 2nd period	-712	6,027	-11,282	-11,264
	(13,081)	(7,162)	(13,264)	(13,515)
Public health expenditure	2,116	6,792**	895	574
	(4,733)	(3,279)	(5,884)	(6,218)
Public health exp. * 2nd period	-3,509	-7,466**	4,519	5,827
	(7,072)	(3,616)	(8,167)	(8,559)
Civil war	-6,073	-7,261*	-5,869	-6,189
	(10,541)	(4,189)	(10,047)	(10,349)
Civil war * 2nd period	1,729	7,633	-4,159	-3,264
	(16,829)	(6,819)	(16,174)	(16,535)
Constant	-3,766	-17,381**	-1,596	-1,190
	(15,932)	(8,459)	(16,582)	(16,985)
Observations	60	48	58	56
Number of countries	30	24	29	28
R-squared	0.517	0.812	0.546	0.552

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Appendix III.1: Definition of variables and sources

Variable	Definition	Source
People living with HIV	Estimated number of people living with HIV by country, 1998-2007	UNAIDS (http://www.unaids.org/en/KnowledgeCentre/HIVData/GlobalReport/2008/2008_Global_report.asp)
AIDS deaths	Number of AIDS deaths in adults and children by country, 1998-2007	UNAIDS
New HIV infections	Number of new HIV infections (in t), defined as the difference in the number of people living with HIV in t and t-1, plus the number of AIDS-related deaths in t, 1998-2007	UNAIDS
HIV prevalence rate	Adult (15-49) HIV prevalence, percent by country, 1998-2007	UNAIDS
HIV/AIDS-related ODA	Official development assistance, commitments, US$ per capita of the recipient country's population, constant prices of 2008, 1998-2007, reported under purpose codes 13040 and 16064	OECD, CRS (http://stats.oecd.org/Index.aspx?DatasetCode=CRSNEW)
Population	Population of the recipient country at the beginning of each period (1998, 2003)	World Development Indicators (WDI), http://databank.worldbank.org/ddp/home.do (accessed: July 2010)
GDP per capita	GDP per capita of the recipient country at the beginning of each period (1998, 2003), US$, constant prices of 2000	World Development Indicators (WDI)
Control of corruption	Control of corruption of the recipient country at the beginning of each period (1998, 2003)	World Bank's Worldwide Governance Indicators (WGI), http://info.worldbank.org/governance/wgi/index.asp
Public health expenditure	Public health expenditure as a share of GDP of the recipient country at the beginning of each period (1998, 2003), in percent	Institute for Health Metrics and Evaluation (IHME), http://www.healthmetricsandevaluation.org/resources/datasets/2010/public_financing_health.html, variable: GHE-S/GDP, WHO
Civil war	Dummy variable set equal to one if a major internal armed conflict (at least 1,000 battle-related deaths in one year) occurred in the recipient country during the respective period (1998-2002, 2003-2007)	UCDP/PRIO Armed Conflict Dataset v.4-2009, http://www.pcr.uu.se/research/UCDP/data_and_publications/datasets.htm
Ethnic fractionalization	Degree of the ethnolinguistic fractionalization in a country, ranging from 0 to 1	Alesina et al. (2003)
Food consumption	Dietary energy consumption per person; the amount of food, in kcal per day, for each individual in the total population, 2000-2002 and 2005-2007	Food and Agriculture Organization of the United Nations, http://www.fao.org/economic/ess/food-security-statistics/en/ (13/09/2010)
Female labor participation	Labor participation rate, female (% of female population ages 15+), at the beginning of each period (1998, 2003)	World Development Indicators (WDI)
ODA per capita for health and water/ sanitation	Official development assistance, commitments, US$ per capita of the recipient country's population, constant prices of 2008, 1998-2007, reported under sector codes 120 (Health) and 140 (Water Supply & Sanitation); average over each period (1998-2002; 2003-2007)	OECD, CRS
Road density	Km of road per square km of land area, average over each period (1998-2002; 2003-2007)	World Development Indicators (WDI)
People affected by drought	Number of people affected by drought; average over each period (1998-2002; 2003-2007)	EM-DAT: The OFDA/CRED International Disaster Database, http://www.emdat.be (accessed: December 2010)

Appendix III.2: Descriptive statistics (year 2003)

	Control group					Treatment group				
	Obs.	Mean	Std. Dev.	Min	Max	Obs.	Mean	Std. Dev.	Min	Max
People living with HIV (level)	24	251,013	486,786	2,200	2,400,000	23	740,244	1,138,644	4,600	5,300,000
AIDS deaths (level)	24	18,050	33,607	200	160,000	23	55,404	69,160	200	270,000
New HIV infections (level)	24	24,158	43,739	250	215,000	23	64,024	106,409	800	490,000
HIV prevalence rate (level)	24	2.3	1.4	1.1	5.9	23	9.7	8.6	1.2	26.6
Population	24	20,100,000	30,200,000	251,955	134,000,000	23	14,700,000	17,800,000	487,301	70,900,000
GDP per capita	23	1,006	1,285	83	4,020	23	1,364	1,961	124	8014.387
Control of corruption	24	-0.65	0.58	-1.51	1.22	23	-0.63	0.65	-1.74	1.07
Public health expenditure	23	1.68	0.94	0.00	4.56	23	2.35	1.61	0.55	7.29
Civil war	24	0.08	0.28	0	1	23	0.09	0.29	0	1
Ethnic fractionalization	24	0.68	0.19	0.14	0.87	23	0.58	0.26	0.06	0.93
Food consumption	24	2,403	435	1,590	3,230	22	2,257	296	1,850	2,990
Female labor participation	24	58.8	14.3	29.9	90.1	23	61.5	16.5	36.3	87.7
ODA p.c. for health / water & sanitat	24	5.89	4.99	0.30	17.37	23	8.60	7.65	0.27	29.92
Road density	24	0.38	0.81	0.01	3.72	23	0.22	0.33	0.03	1.62
People affected by drought	24	26,933	95,458	0	430,000	23	454,204	873,244	0	3,040,000

Appendix III.3: HIV prevalence and ODA in sample countries (treatment and control group)

Country	HIV prevalence rate 2003	ODA pc 1998 -2002 (mean)	ODA pc 2003 - 2007 (mean)	Difference	Treatment group
Botswana	25.9	1.85	40.25	38.40	1
Namibia	15.2	2.90	36.09	33.20	1
Guyana	2.5	1.11	26.86	25.75	1
Swaziland	26.6	1.85	18.48	16.62	1
Lesotho	23.7	0.88	10.97	10.09	1
Zambia	15.2	2.30	12.13	9.82	1
Rwanda	3.7	1.03	9.06	8.03	1
Haiti	2.2	0.68	6.28	5.60	1
South Africa	17.9	0.41	5.80	5.38	1
Mozambique	11.5	1.23	6.56	5.33	1
Kenya	7.0	1.15	6.16	5.00	1
Uganda	6.9	1.39	6.09	4.70	1
Malawi	12.8	1.95	6.28	4.33	1
Tanzania, United Rep. of	6.7	0.83	4.83	3.99	1
Zimbabwe	22.7	1.50	5.48	3.98	1
Suriname	1.7	0.07	3.59	3.52	1
Equatorial Guinea	3.7	0.49	3.71	3.22	1
Cambodia	1.2	0.89	4.05	3.16	1
Ethiopia	2.2	0.32	2.93	2.61	1
Cote d'Ivoire	5.3	0.21	2.42	2.20	1
Trinidad and Tobago	1.4	0.22	2.40	2.18	1
Liberia	1.5	0.04	2.20	2.16	1
Central African Republic	6.4	0.57	2.61	2.04	1
Mali	1.5	0.35	2.36	2.01	0
Belize	2.1	0.15	2.14	1.99	0
Togo	3.5	0.08	2.04	1.97	0
Gabon	5.9	0.14	1.99	1.85	0
Dominican Republic	1.2	0.44	2.02	1.58	0
Benin	1.3	0.74	2.23	1.49	0
Angola	1.9	0.14	1.59	1.45	0
Burkina Faso	1.9	0.82	2.19	1.37	0
Barbados	1.2	0.00	1.25	1.25	0
Congo	4.0	0.41	1.56	1.16	0
Sierra Leone	1.5	0.30	1.26	0.96	0
Guinea-Bissau	1.9	0.29	1.19	0.90	0
Congo, Dem. Rep. of the	1.4	0.08	0.92	0.84	0
Burundi	2.9	0.87	1.68	0.80	0
Cameroon	5.7	0.40	1.14	0.74	0
Ukraine	1.1	0.00	0.64	0.64	0
Thailand	1.5	0.06	0.64	0.59	0
Nigeria	3.2	0.60	1.18	0.58	0
Ghana	2.2	1.08	1.59	0.51	0
Sudan	1.4	0.01	0.51	0.50	0
Guinea	1.4	0.71	0.98	0.27	0
Eritrea	1.2	2.02	2.20	0.18	0
Jamaica	1.5	2.92	2.81	-0.11	0
Chad	3.5	0.63	0.43	-0.21	0

Ranked according to difference in ODA, (2003-2007) minus (1998-2002)

Appendix III.4: Countries in the respective treatment and control groups

	Difference in ODA, (2003-2007) minus (1998-2002)	Table 1: All columns except Column (6), (12)	Table 2: Column (1), (5)	Table 2: Column (2), (6)	Table 2: Column (3), (7)	Table 2: Column (4), (8)	Table 3: DAC countries	Table 3: Multilateral	Table 4: US	Table 4: PEPFAR	Table 4: Global Fund
Treatment group											
Botswana	38.4	X	X	X		X	X	X		X	X	
Namibia	33.2	X	X	X		X	X	X		X	X	
Guyana	25.8	X	X	X	X			X		X	X	
Swaziland	16.6	X	X	X		X	X		X			X
Lesotho	10.1	X	X	X		X	X		X			X
Zambia	9.8	X	X	X		X	X	X		X	X	
Rwanda	8.0	X	X	X	X	X	X	X		X	X	
Haiti	5.6	X	X	X	X		X	X		X	X	
South Africa	5.4	X	X	X		X	X	X		X	X	
Mozambique	5.3	X	X	X		X	X	X		X	X	
Kenya	5.0	X	X	X		X		X		X	X	
Uganda	4.7	X	X	X		X	X	X		X	X	
Malawi	4.3	X	X	X		X	X		X			X
Tanzania, United Rep. of	4.0	X	X	X		X	X	X		X	X	
Zimbabwe	4.0	X	X	X		X	X	X				
Suriname	3.5	X	X	X	X				X			X
(Equatorial Guinea)	3.2	X	X	X	X	X			X			X
Cambodia	3.2	X	X	X	X		X	X		X		
Ethiopia	2.6	X		X	X	X	X		X			X
Cote d'Ivoire	2.2	X		X	X	X	X	X		X	X	
Trinidad and Tobago	2.2	X		X	X				X			
Liberia	2.2	X		X	X	X	X		X			X
Central African Republic	2.0	X		X		X	X		X			X
Control group												
Mali	2.0	X		X	X	X	X	X	X	X	X	X
Belize	2.0	X		X	X			X	X	X	X	X
Togo	2.0	X		X	X	X	X	X	X	X	X	X
Gabon	1.9	X		X		X	X	X	X	X	X	X
Dominican Republic	1.6	X		X	X		X	X	X	X	X	X
Benin	1.5	X	X	X	X	X	X	X	X	X	X	X
Angola	1.4	X	X	X	X	X	X	X	X	X	X	X
Burkina Faso	1.4	X	X	X	X	X	X	X	X	X	X	X
(Barbados)	1.3	X	X	X	X			X	X	X	X	X
Congo	1.2	X	X	X	X	X	X	X	X	X	X	X
Sierra Leone	1.0	X	X	X	X	X	X	X	X	X	X	X
Guinea-Bissau	0.9	X	X	X	X	X		X	X	X	X	X
Congo, Dem. Rep. of the	0.8	X	X	X	X	X		X	X	X	X	X
Burundi	0.8	X	X	X	X	X	X	X	X	X	X	X
Cameroon	0.7	X	X	X	X	X	X	X	X	X	X	X
(Ukraine)	0.6	X	X	X	X			X	X	X	X	X
Thailand	0.6	X	X	X	X		X	X	X	X	X	X
Nigeria	0.6	X	X	X	X	X	X	X	X	X	X	X
Ghana	0.5	X	X	X	X	X	X	X	X	X	X	X
Sudan	0.5	X	X	X	X		X	X	X	X	X	X
Guinea	0.3	X	X	X	X	X	X	X	X	X	X	X
Eritrea	0.2	X	X	X	X	X		X	X	X	X	X
Jamaica	-0.1	X	X	X	X			X	X	X	X	X
Chad	-0.2	X	X	X	X	X	X	X	X	X	X	X

For countries in brackets control variables have missing values.

Chapter IV:

Does Conditionality Work? A Test for an Innovative US Aid Scheme

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IV.1 Introduction

Performance-based aid has received increasing attention in the international development community recently, particularly in the health sector (e.g., Eichler and Glassman 2008). However, empirical assessments of its effectiveness are still largely lacking. We attempt to fill this gap by investigating the so-called MCC Effect.[1] The Millennium Challenge Corporation, established by the Bush administration in 2004, has been deliberately shaped in such a way as to grant aid as *ex post* rewards for proven achievements.

The move towards performance-based aid reflects increasing doubts in the donor community that traditional forms of conditional aid have been effective. *Ex ante* conditionality appears to have failed in international development cooperation, in particular to the extent that donors had used foreign aid as a means to "buy" policy reform in recipient countries (Collier 1997).[2] A similar verdict appears to apply to less intrusive motives of conditionality such as ensuring loan recovery. International financial institutions, notably the World Bank and the International Monetary Fund (IMF), have achieved little by attaching (a typically large number of) conditions to adjustment loans. Dreher (2009: 256) concludes from a comprehensive survey of IMF-related research that "there is no empirical evidence showing that conditions enhance ownership or make program success more likely."

By contrast, conditionality is widely perceived to have been "very effective" (Sedelmeier 2008: 806) in the European Union's enlargement strategy. The desirability of EU membership appears to have prompted candidate countries to adhere to a host of conditions contained in the so-called *Acqui Communautaire*. The EU concept is based on *ex post* selectivity according to which performance standards have to be met prior to accession and the associated financial rewards. It is open to question whether the EU experience offers relevant lessons for international development cooperation. All the same, the review of the literature in Section 2 leads us to hypothesize that the effectiveness of conditionality depends more on the underlying concept, rather than specific treatment groups reacting differently to similar incentives.

We focus on the effects of MCC aid on corruption in candidate countries. The international aid community has been aware that corruption is a major bottleneck to the effectiveness of aid, and development prospects in general, since the second half of the 1990s

[1] See http://www.mcc.gov/mcc/panda/activities/mcceffect/index.shtml.

[2] Note that the literature uses the terms *ex ante* and *ex post* conditionality in different ways. In the Public Choice literature, *ex ante* refers to the time before a country turns to international institutions, notably the IMF, for financial assistance (e.g., Vaubel 1991, Meltzer 2006). Typical *ex ante* conditions suggested in the literature include responsible fiscal and monetary policies and sound financial systems. *Ex post* conditionality refers to conditions negotiated after a country turned to the IMF. Examples are reductions in the government's deficit or in the rate of monetary expansion.

(Easterly 2007).[3] Corruption features most prominently among MCC's eligibility criteria. We employ a difference-in-difference-in-differences (DDD) approach to assess whether the treatment groups fought corruption more effectively than the control groups after the Bush administration announced the creation of the MCC and its performance-based aid allocation approach (Section 4). We consider different treatment group definitions, as well as different time periods during which incentive effects could have materialized. We find evidence of strong anticipation effects immediately after the announcement of the MCC, while increasing uncertainty about the timing and amount of MCC aid appear to weaken the incentive to fight corruption over time (Section 5). Section 6 summarizes the MCC experience and offers policy conclusions on how conditionality can be designed in such a way that it becomes more effective.

IV.2 The Debate on Conditionality

There are different reasons why international financial institutions and bilateral donors attach conditions to adjustment loans and development aid. Most obviously, temporary balance-of-payments support in the form of loans from the International Monetary Fund (IMF) has to be repaid by the borrowing country, and conditionality appears to be a means for the IMF to monitor the borrowing country's adjustment efforts and, thereby, ensure timely repayment. In the IMF's own words, "the intended purpose of conditionality is as a mechanism to help bring together a combination of financing and policies as a solution to economic difficulties: It is needed to provide assurances to both the authorities and the Fund that both parts of the package are provided together" (IMF 2001: 12).

Drazen (2002) argues that conditionality implies the presence of conflicts of interest. Conflicts of interest may prevail between the IMF and the borrowing country, or within the country. Drazen stresses that reform-minded local authorities may be interested to implement an adjustment program, but IMF conditionality may still be needed to overcome internal opposition. In other words, conditionality does not necessarily mean that the IMF imposes policies on local authorities unwilling to reform. However, the incentives of local authorities to actually implement policy changes tend to weaken once the adjustment loans have been received, even if they agreed to policy adjustments in earlier negotiations with the IMF.

The IMF typically provides its financial support in successive tranches contingent on progress in achieving policy adjustments in order to mitigate time inconsistency problems

[3] Mosley et al. (2004) conclude that corruption, along with inequality and the composition of public expenditure, has a particularly strong association with the poverty-reducing impact of aid.

(IMF 2001: 11). However, this does not prevent recipient countries from reneging on their promises. According to Killick (2006) and Vreeland (2006, 2007), noncompliance is not rigorously punished by the IMF. The frequency of waivers from program obligations indicates that the IMF is not credibly committed to disbursing successive tranches only if policy conditions are actually implemented by the borrowing countries. There is even evidence suggesting that the number of waivers has increased over time (IMF 2002). At the same time, the increasing number of conditions in the 1980s and 1990s appears to have eroded compliance and "ownership" of reforms by IMF clients (Bird 2009; Vreeland 2006). Both the IMF's Independent Evaluation Office (IEO 2007) and Arpac et al. (2008) find little to suggest that compliance has improved since the IMF attempted to streamline its conditionality.[4]

Conditionality – and its failure – could also be due to conflicts of interest between international financial institutions and major member countries. On the one hand, the funding of these institutions may be easier to sell to national constituencies in major member countries if borrowers are supported only when principally agreeing to strict conditions. On the other hand, the lax enforcement of traditional conditionality can also be attributed to political pressure from major member countries. For instance, Stone (2002, 2004) shows that the punishment for noncompliance with IMF conditions is significantly weaker for countries that are considered to be important to the United States.[5]

Compared to adjustment loans by the IMF, donors of official development aid are less likely to use conditionality as a means to ensure the repayment of transitory financial support. Aid reported by the OECD's Development Assistance Committee consisted mainly of outright grants, while repayable loans contributed just 20 percent to total aid in 2005-2009.[6] Furthermore, aid transfers are motivated by different donor objectives, some of which are unrelated to policy reform in the recipient countries. For instance, conditionality is unlikely to be an issue if donors pursue selfish aims such as export promotion through aid. The same applies to emergency relief which accounted for about eight percent of total aid by all donors in 2005-2009.[7] All the same, Collier (1997) posits that policy reform is an important criterion on which aid might reasonably be judged: Aid might be "remarkably effective if it induces

[4] Bird (2009) acknowledges, however, that the global financial crisis of 2008/09 might result in a systematic overhaul of IMF conditionality.

[5] Likewise, Kilby (2009) shows that the United States has prevented the World Bank from strictly imposing structural adjustment conditionality on countries which are friendly with the United States. Stone (2008) and Dreher et al. (2009) report similar results for IMF conditionality. While studies on the political determinants of conditionality largely focus on the IMF and the World Bank, recent research confirms the importance of political considerations for US aid (Kuziemko and Werker 2006, Anwar and Michaelowa 2006), and aid by the Asian Development Bank (Kilby 2006, 2010).

[6] See http://www.oecd.org/dataoecd/50/17/5037721.htm (last accessed January 13, 2011).

[7] This includes humanitarian aid and food aid as reported by the OECD's Development Assistance Committee.

governments to adopt growth-inducing and poverty-reducing policies. This is indeed the core of what conditionality is supposedly about – aid buys reform. Unfortunately, it does no such thing" (Collier 1997: 56).

The example of Kenya has repeatedly been used to demonstrate the failure of traditional conditionality in international development cooperation (Collier 1997; Svensson 2003). Kenya has outmaneuvered even major donors such as the World Bank. The country agreed to agricultural reform on four occasions in 15 years but backtracked each time after having received the aid money. *Ex ante* threats by donors that they will not disburse committed aid if the recipient does not fulfill reform promises are hardly credible. Time inconsistency problems loom large considering the incentives for aid agencies and specific country desks to spend overall budgets and fully exhaust country quotas. Indeed, Svensson (2003: 383) finds "no link between a country's reform effort, or fulfillment of 'conditionality', and the disbursement rate [of aid funds]." According to Heckelman and Knack (2008), higher aid even slowed policy reform over the 1980-2000 period.[8]

Focusing on corruption, as we do in the following, Alesina and Weder (2002: 1136) do not find "any even weak evidence" that efficient and honest governments have been rewarded by more bilateral or multilateral aid during the 1975-1994 period. In 1997, the German government responded to a parliamentary inquiry that "no development cooperation contracts were annulled due to proof of corruption" (as quoted by Cremer 2008: 122). This is in line with the results in Isopi and Mattesini (2010), who find that Germany gives more aid to more corrupt countries. The same holds for Finland, France, Japan, and the Netherlands, while Canada, Norway and the United Kingdom, give less aid to more corrupt recipients. According to Easterly (2007), little has changed after former World Bank President Wolfensohn highlighted corruption as a major impediment to development and effective aid in 1996.

A vicious circle of aid inducing more corruption and further eroding governance may follow from the failure of traditional conditionality. According to Alesina and Weder (2002), increases in aid tend to be associated with an increase in corruption. Svensson (2000) presents similar results for ethnically diverse recipient countries in which social groups compete over common-pool resources, whereas Tavares (2003) finds aid to be associated with less corruption. Knack (2001) provides cross-country evidence that higher aid levels erode the quality of governance more generally.

Taken together, these rather bleak findings have fuelled the debate on how to redesign aid conditionality. Collier (1997) as well as Collier et al. (1997) observed a move towards

[8] Dreher and Rupprecht (2007) find that IMF programs also reduce reform efforts.

"short-leash" conditionality in the 1990s; i.e., donors disbursed program aid in several tranches depending on progress in reform implementation. Collier and co-authors argued, however, that donors taking these first steps towards performance-based aid allocation still attempted to buy reforms from recipients that were frequently unwilling to sustain reforms. These authors called for a fundamental change in donor behavior to reward reform-minded recipient countries and enhance the effectiveness of aid: Aid allocation would be based on retrospective performance appraisals, rather than being conditional on reform promises.

Svensson (2003) presents a theoretical model for rewarding good performance by granting more aid *ex post*. Accordingly, time inconsistency problems could be overcome if disbursement decisions were centralized, rather than left to country desks that are mainly interested in spending funds allocated to them. Instead of committing fixed amounts of aid on a country-by-country basis, the donor would link "the allocation and disbursement decision by committing the aggregate amount to a group of countries, but where the actual amount disbursed to each individual country depends on its relative performance" (Svensson 2003: 384). The opportunity costs of disbursing aid would increase, thereby strengthening donors' incentives to direct aid to where favorable conditions for its effective use exist. Furthermore, competition among recipient countries might result in an overall improvement in the conditions for effective use of aid.

As detailed below, the so-called MCC Effect is based on the reasoning that the reform-mindedness of recipient countries will generally increase once aid is disbursed according to the relative performance of competing countries. However, whether rewarding relatively good performance ex post will result in stronger ownership of reforms by aid recipients remains the subject of controversy and debate. According to Mosley et al. (2004), selectivity may provide incentives to improve policies prior to receiving aid, but recipients would still have the option to reverse reforms after having been selected by donors. Conflicting hypotheses on whether selectivity promotes sustained reform efforts have hardly been subjected to systematic empirical tests in the aid-related literature.[9]

The plausibility of conflicting hypotheses may be assessed at least tentatively by referring to the extensive literature on the role of preconditions that must be fulfilled by candidate countries for EU membership. Clearly, the situation of EU accession countries

[9] One strand of this literature focuses on discussing the intricate practical problems of designing incentive-compatible contracts between donors and specific recipient countries. For instance, Adam and Gunning (2002) examine the role and impact of performance indicators in the case of Uganda, which has served as a model for redesigning traditional relationships between donors and recipients of aid. Another strand of the literature discusses important elements of specific pioneering schemes of performance-based aid, notably in the health sector, such as the Global Alliance for Vaccines and Immunisation (GAVI) and the Global Fund for AIDS, Tuberculosis and Malaria (GFATM) (e.g., Eichler and Glassman 2008).

differs considerably in several dimensions from the situation of aid recipients in the developing world. The conditions imposed in the context of EU accession, ranging from minority rights and judicial reforms to social security systems and corporate governance, extend far beyond the typical coverage of conditions in aid and lending contracts that developing countries enter with donor countries and international financial institutions. At the same time, financial transfers in the EU context are not only much higher than those to developing countries; they are also rules-based in the sense that net-payers – i.e., the richer EU members – cannot reduce or discontinue the transfers at their own discretion, unlike aid donors who are notorious for granting aid as it fits the current domestic budget situation and political environment. As a consequence, EU transfers are reliable and predictable, whereas aid flows are volatile and hard to predict.[10]

Keeping these important qualifications in mind, the EU approach of granting financial rewards only at the end of a fairly long process of accession invites several hypotheses concerning the effects of performance-based aid. First, the literature on the EU's enlargement strategy suggests that the incentive effects of conditionality are likely to depend on initial conditions in the treatment group of prospective EU member countries or, respectively, aid recipient countries. Unfavorable initial conditions imply higher costs of compliance.[11] Unfavorable initial conditions may weaken the incentive effects of conditionality, especially if selection occurs according to relative performance, as is the case under MCC rules (see below). The reform efforts of candidate countries that lie closer to the current threshold would raise the stakes for all candidates so that those with unfavorable initial conditions might abstain from reform if they consider the reward to be out of reach.

Second, for given costs of compliance the incentive effects can be expected to depend on the expected size of the reward and the likelihood that it will actually materialize before too long.[12] In the EU context, this point has been made with respect to the Eastern enlargement as well as with respect to current candidate countries.[13] Especially Turkey's incentives to fulfill EU conditions are weakened considerably by the distance and lacking

[10] We thank an anonymous referee for having alerted us to these differences.
[11] For example, high domestic costs of compliance with EU conditions in the western Balkans and Turkey render the prospect of membership less compelling (Epstein and Sedelmeier 2008: 796).
[12] A similar argument has been made in different contexts. One example is the reforms that took place in China after it joined the WTO (Bigsten 2006). Bräutigam (2000) and Azam et al. (1999) attribute part of the success stories of development in Botswana, Korea, and Taiwan to the presence of a single dominant donor offering sufficiently high rewards. Similarly, Knack and Rahman (2007) attribute part of the Marshall Plan's success in Europe to the presence of a single large donor.
[13] According to Grabbe (2001: 1025), the EU's influence on governance in Central and Eastern Europe has been diluted by "an uncertain linkage between fulfilling particular tasks and receiving particular benefits."

credibility of prospective membership. As we will argue below, uncertainty about prospective rewards may also have weakened the MCC Effect.

Third, one might suspect that the materialization of the reward puts the sustainability of reforms at risk. Studies addressing this hypothesis in the EU context reveal ambiguous findings.[14] In the light of our focus on corruption, it is most interesting to note that Pridham (2008), *inter alia*, assesses the fight against corruption in Latvia and Slovakia during the first three years of EU membership. This study concludes that "there is no common pattern whereby conditionality loses momentum and becomes unscrambled." It remains open to question whether this finding carries over to aid contracts between donor and recipient countries. As indicated before, phenomena such as status-quo bias and social learning – offered by Pridham (2008) as possible explanations for sustained reforms in new EU countries – may play a minor role in relatively "casual" donor-recipient relations.

IV.3 The MCC's Approach and Conditions

The earlier discussion on the failure of traditional forms of conditionality provides one major pillar of the MCC concept. The second pillar is given by Burnside and Dollar's (2000; 2004) highly influential analysis which suggests that aid promotes growth only in an environment characterized by "good" policies and institutions. Following Burnside and Dollar, it has been subject to controversial debate whether certain conditions have to be met by recipients for aid to be effective, and exactly which of these conditions might be most relevant. For instance, Easterly et al. (2004), Rajan and Subramanian (2008), and Doucouliagos and Paldam (2010) show that the interactions of aid with various conditioning variables in growth models are generally fragile, sensitive to small changes in the data set or in the model specification, and dependent on author affiliation and ideology.[15]

However, not even strong critics of Burnside and Dollar dismiss the relevance of selectivity. Easterly (2007: 645) acknowledges that "the idea that aid money directed to governments would be more productive if those governments had pro-development policies and institutions is very intuitive."[16] Moreover, as noted before, Easterly (2007: 653) stresses the aid community's awareness of "corruption as a factor influencing the effectiveness of aid

[14] Blavoukos and Pagoulatos (2008) observe that fiscal adjustment in Italy, Greece and Portugal deteriorated after they had joined the European Monetary Union. However, the summary of several empirical contributions presented by Epstein and Sedelmeier (2008: 795) concludes that EU accession conditions proved "more enduring than predicted."

[15] Bjørnskov (2010) even finds that foreign aid is *more* harmful when given to democracies, as it leads to a more skewed income distribution.

[16] Easterly (2007) also notes that the so-called Pearson Commission already suggested linking aid to performance in its 1969 report on reforming international development cooperation.

and development prospects in general." Similarly, Mosley et al. (2004) share the view that aid could be more effective if it were reallocated to less corrupt recipient countries.

While the failure of traditional conditionality and the discussion on the need for an appropriate local environment for aid to be effective may have shaped the aid allocation procedures of various multilateral development agencies and bilateral donors, the MCC clearly stands out so that the effects of the new donor approach should be most visible in the allocation of MCC aid. Instead of merely adjusting or extending the mandate of an established aid agency such as USAID, the Bush administration explicitly established the MCC as a new aid agency in order to prevent institutional legacies from undermining innovative performance-based allocation rules (Radelet 2003; Rieffel and Fox 2008). Given the unresolved debate on the incentive effects of ex post conditionality and sparse empirical evidence, it would be most useful to know whether and to what extent MCC-type conditionality affects the policies of the potential beneficiaries. The MCC (2008: 1) itself is fairly confident of having improved the incentives for reform in potential recipient countries:

> *The MCC Effect is the positive impact that MCC is having on developing countries beyond its direct investments. To date, the most significant impact has been the incentive created for countries to adopt legal, policy, regulatory, and institutional reforms related to the MCC eligibility criteria. Eligibility for MCC funding can lead to international recognition and increased private sector investment, which has encouraged many countries to implement significant political, social, and economic reforms with tangible results on the ground. In areas as diverse as women's rights, anti-corruption and governance, and business registration, countries are taking it upon themselves to re-evaluate their laws, policies, regulations, and ways of 'doing business'.*

The MCC signed so-called compacts – the multi-year aid programs concluded between the MCC and eligible countries – with 20 strictly selected recipient countries until early 2010 (Appendix IV.2). In various cases, the MCC offers remarkable financial rewards. It is not only in small recipient countries such as Cape Verde, Vanuatu and Lesotho that MCC aid played an important role. MCC's aid obligations agreed to in compacts exceeded 20 percent of total aid commitments of all donors during the five previous years in eight out of 20 countries with compacts, and 10 percent in another six countries (Figure IV.1).

Furthermore, Dreher et al. (2010) find that other donors followed the MCC by granting more aid to countries with compacts, pointing to additional indirect MCC effects. MCC aid is also relevant relative to the recipient countries' GDP in the year of signing compacts. On average, this ratio amounted to 6 percent in the group of 20 countries, ranging from less than 1 percent in Morocco to 23 percent in Lesotho (Figure IV.2). The hypothesis that the MCC creates incentives to improve policies and institutions is thus plausible for most recipient countries.

The MCC's eligibility criteria leave little doubt about the strictness of selectivity in granting aid to needy and well performing recipients only. Eligibility is restricted to relatively poor countries.[17] The "hurdles approach" (Radelet 2003: 24) requires potential recipient countries to score higher than the median on at least half of the eligibility criteria (in each of three broad categories) across peers in the same income category, with control of corruption being the only mandatory prerequisite (see also below).[18] The MCC groups the 16 indicators, all taken from independent sources, into three categories: Ruling Justly, Investing in People, and Encouraging Economic Freedom. Ruling Justly comprises the Civil Liberties and Political Rights indicators from Freedom House, and four indicators from the World Bank's Governance Indicators (Kaufmann et al. 2009), including the control of corruption.[19] Economic Freedom consists of indicators on regulatory quality, a country's credit rating, inflation, as well as fiscal, regulatory, and trade policies.[20] Investing in People refers to public expenditures on health and primary education, immunization rates, and primary education completion rates.[21]

It is open to question, however, whether the MCC's selective aid allocation has indeed the desired impact on the incentives of potential recipient countries to improve their policies and institutions. To our knowledge, the only independent and systematic study on this issue provides preliminary evidence supporting positive MCC effects (Johnson and Zajonc 2006). Specifically, Johnson and Zajonc find that candidate countries improved their indicators by 25

[17] The threshold for per-capita income was set at US$ 1,415 when the MCC began operations in 2004. Eligibility was extended in 2006 to lower-middle income countries with a per-capita income of up to US$ 3,255. The present analysis applies the original threshold. Countries subject to legal provisions that prohibit them from receiving United States economic assistance are excluded. See Appendix IV.1 for the list of eligible countries.

[18] Specific eligibility criteria have been slightly modified over time, with a few indicators being replaced or added. We use the indicators as shown in the MCC's scorebook of 2004 (http://www.mcc.gov/mcc/bm.doc/score_fy04_all.pdf).

[19] Note, however, that improvements on one indicator might make improvements on others less rather than more likely. According to recent research, e.g., an improvement in institutional quality can well increase the degree of corruption, given the potentially perverse effects of institutions on the unofficial economy (Bjørnskov 2011).

[20] Note that, while performance on most indicators must be superior relative to a specified group of countries, consumer price inflation is an absolute criterion and must be below 20 percent.

[21] While selection is, in principle, based on these objective indicators, the MCC's board has some discretion in deciding on selection at the margin, e.g., when data are missing, substantial deviations on specific indicators occur, or time trends are visible.

percent more than non-candidate countries after the MCC had been announced. It is interesting to note, however, that this study does not find support in justification of MCC claims about having induced better control of corruption, even though this criterion figures most prominently among the eligibility criteria. As indicated above, a country must score above the median with regard to the control of corruption, regardless of how well it performs on all other eligibility criteria.

IV.4 Data and Method

The significance of the control of corruption among the MCC's eligibility criteria and widespread support that it commands within the aid community provide the reasons for us to focus on the effects of MCC conditionality on corruption in potential recipient countries. In accordance with MCC convention, we use the index of Kaufmann et al. (2009) on the control of corruption. The index is constructed by Kaufmann et al. using an unobserved components model, based on a large number of different surveys of perceived corruption from various independent organizations, and measures perceptions of corruption, defined as the abuse of public power for private gains.

Composite corruption indexes such as the index of Kaufmann et al. aggregate and synthesize information from various third-party data sources (for details, see, e.g., UNDP 2008). In the case of Kaufmann et al., the underlying data are partly objective and partly subjective. The index is also hybrid in that input-based (*de jure* on institutions and rules) as well as output-based (*de facto* on impact) indicators are included. It focuses on corruption in the public sector but also uses information on corruption in the private sector. Kaufmann et al. construct the index in such a way that it follows a normal distribution with a mean of zero across all countries and a standard deviation of one; index values range from -2.5 to +2.5 with higher values indicating better control of corruption.

According to the summary statistics presented in Table IV.1, most of our sample countries clearly rank below the mean for all 208 countries (in 2008) included in Kaufmann et al. (2009). This applies to both sub-samples, i.e., the treatment and control groups used for the baseline estimations reported in Table IV.2 in Section 5.a below.[22] However, countries in the control group score over a much wider range of the index than countries in the treatment group. While the best-placed country in the treatment group consistently ranks below zero

[22] The treatment group consists of countries with control of corruption in the second quartile, while countries with control of corruption in the 1st, 3rd or 4th quartile are in the control group. See below for details.

throughout the period of observation, the distribution is less lopsided for the countries in the control group.

Critics stress the limitations of composite corruption indexes (e.g., Arndt and Oman 2006; Knack 2007). These include problems related to the aggregation of indicators from multiple sources (e.g., insufficient transparency and the correlation of errors among sources).[23] Furthermore, it is debated whether index values are comparable over time. Arndt and Oman (2006: 61) argue that the scale of the index constructed by Kaufmann et al. is "largely arbitrary" so that it is difficult to monitor the control of corruption over time. Kaufmann et al. (2009: 15) admit that their "aggregate estimates convey no information about trends in global averages of governance, but they are of course informative about changes in individual countries' relative positions over time." Note that the relative perspective for individual countries is exactly how the MCC makes use of the index.

The comparability of index values may also suffer from changes in the composition of the index (Arndt and Oman 2006; Knack 2007). The sources used in constructing composite indexes do not only vary from country to country; they may also change over time for one particular country. Kaufmann et al. (2009: 21) address this point by assessing the extent to which changes in the composite index were driven by new data sources. They find that "compositional effects are not large" during the period of observation underlying our subsequent analysis (1998-2008). We test for the robustness of our results to changes in the composition of the index on control of corruption further below.

Inherent limitations notwithstanding, composite corruption indexes have been widely used because of their availability across a large number of countries. At the same time, drawing on a large number of sources has the advantage of limiting measurement error of individual indicators and sources (UNDP 2008: 21).[24] More specifically, even critics such as Arndt and Oman (2006: 49) acknowledge that Kaufmann et al. present "probably the most carefully constructed" set of governance indicators. Compared to alternative indexes such as Transparency International's Corruption Perceptions Index (CPI), Kaufmann et al. use more sources and cover a larger number of countries (see also Knack 2007; Rohwer 2009).[25]

[23] As for transparency in constructing composite indexes, Knack (2007: 263) notes that "replication of the indexes by independent analysts would be costly…. Some of the sources are available only to paying subscribers or members, and some are not publicly available at all." Recently, however, the underlying data have been made available by the World Bank (http://info.worldbank.org/governance/wgi/sources.htm, last accessed January 15, 2011).

[24] Kaufmann et al. (2009: 16) stress that their aggregate indicators, by combining information from various sources, have greater precision than any individual underlying data source.

[25] For 1998, the first year of our period of observation, Kaufmann et al. provide index values for almost 200 countries, compared to just 85 countries in the case of the CPI of Transparency International. The sample available from the International Country Risk Guide (ICRG) is also considerably smaller (note also that ICRG

Relying on the index of Kaufmann et al., we ask whether the substantial increases in US aid provide sufficient incentives for candidate countries to fight corruption more effectively. President Bush announced in 2002 that the MCC would command over US$ 5 billion annually and that MCC aid would be "above and beyond existing aid." Hence, recalling the discussion on sufficiently large rewards in Section 2, MCC effects on corruption could differ considerably from the earlier findings of Alesina and Weder (2002), according to which continuous and quantitatively small disbursements of aid tend to be associated with *more* corruption.

Our focus on corruption alone implies that our approach is rather modest in comparison with that of Johnson and Zajonc (2006) who consider all eligibility criteria, even though we do take into account that the incentives for candidate countries to fight corruption may depend on how they score on other eligibility criteria (see below). On the other hand, we extend the analysis of Johnson and Zajonc in several ways. Most crucially, we overcome the drawback that the MCC was still in its infancy when the earlier study was presented; it was probably too early to draw conclusions with data only extending into the first year (2004) of MCC operations.[26] The considerably longer period of observation available in the present analysis also allows us to test several of the specific hypotheses raised in Section 2 above. First, we can address the disputed issue of whether reforms (i.e., more effective control of corruption) are likely to be sustained once the reward (i.e., MCC aid) has been granted. Second, we can assess whether the incentive to reform weakens once the reward becomes less compelling, either because the MCC's overall budget fell short of initial announcements or because the actual disbursement of aid was delayed in the context of compacts.[27] Finally, by controlling for initial conditions in the candidate countries, we evaluate whether higher costs of compliance with MCC conditions undermine the MCC Effect.

In order to address these questions empirically, we rely on the difference-in-difference-in-differences (DDD) approach. Our identification strategy rests on the observation that there can be no incentive effect prior to 2002 when the MCC was announced. Moreover, we limit our sample of MCC candidates to countries with per-capita GDP equal to or less than US$ 1,415, as countries above this threshold were ineligible for MCC aid when operations started in 2004.

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data are included in the index of Kaufmann et al.). Consequently, it proved not feasible to perform robustness tests by replacing the control of corruption as reported by Kaufmann et al. by the corruption indexes reported by Transparency International and ICRG.

[26] Apart from that, Johnson and Zajonc (2006) use a different definition of treatment and control groups. More precisely, they discriminate between treatment and control groups using GDP per capita. Hence, they need to assume that corruption dynamics are invariant across income levels.

[27] See, for example, Rieffel and Fox (2008) on budget cuts and delayed disbursements of MCC aid.

A simple approach to test for the potential effect of the MCC would be to use the before-after approach which involves comparing the level of perceived corruption in eligible countries before and after 2002. Clearly, the strong assumption that no other omitted variable might have changed corruption after 2002 is unlikely to hold. The alternative with-without approach would simply entail comparing the changes in corruption in countries with incentives to qualify for MCC aid and countries without, subsequent to the announcement of the MCC. Again, this would require a strong assumption that is unlikely to hold, namely that no other factors affect eligible and non-eligible countries systematically in the period of observation.

Combining the before-after approach and the with-without approach has considerable merit for alleviating the problem of drawing correct inferences regarding the MCC Effect (Johnson and Zajonc 2006). By applying the difference-in-difference-in-differences estimator to the levels of perceived corruption, identification is based on the change in the perceived corruption differentials between the treatment group and the control group that occurred between the periods before and after the announcement of the MCC. Formally, the DDD estimator for our base specification amounts to:

$$\begin{aligned} DDD = &((Corr^{T}_{2004} - Corr^{T}_{2002}) - (Corr^{T}_{2002} - Corr^{T}_{2000})) \\ &- ((Corr^{C}_{2004} - Corr^{C}_{2002}) - (Corr^{C}_{2002} - Corr^{C}_{2000})) \end{aligned} \quad (1)$$

with *Corr* being the level of perceived corruption in treatment group *T* and control group *C*, respectively, in the years indicated. For a start, we choose the periods 2004-2002 and 2002-2000 as the MCC was announced in 2002 and became operational in 2004. However, we consider alternative periods below in order to test the hypotheses introduced before.

The standard errors and t-statistics come from a regression where the change in corruption in the different periods is specified as the dependent variable. Dummies for the treatment group and the second period (2004-2002) and an interaction term between the two dummies are included as independent variables. The coefficient on the interaction term corresponds to the DDD estimate. Formally, the regression is as follows:

$$Change\ in\ corruption = \alpha + \beta\ Treat + \gamma\ 2nd\ Period + \delta\ (Treat * 2nd\ Period) + \epsilon \quad (2)$$

Most importantly, we have to decide how to allocate MCC candidate countries to the treatment and control groups. Our baseline estimation assumes, in line with the reasoning in

Section 2, that only countries that do not lie too far below the median with respect to the control of corruption indicator have an incentive to improve on that indicator in order to become eligible for MCC aid. Countries below, but relatively close to the median (second quartile) thus qualify for our treatment group. The underlying argument is that the costs of compliance are relatively low for this quartile, while the prospect of being rewarded by MCC aid is relatively favorable. All other countries qualify for the control group in our baseline estimation, either because their position far below the median (first quartile) implies high costs of compliance or because their position above the median (third and fourth quartiles) does not preclude them from being eligible for MCC aid.[28] In additional estimations, we redefine the treatment and control groups to account for more complex incentive effects. We introduce these alternatives in the next section, following the presentation of baseline results.

IV.5 Results

Baseline results

The upper panel of Table IV.2 presents the difference (D) in the levels of the control of corruption index over the two-year periods in our sample, averaged for the treatment and control groups, respectively. We include countries in the treatment group if their level of corruption is in the second quartile, and in the control group otherwise. This split places 14 countries in the treatment group and 48 countries in the control group.[29] Countries' positions on other performance indicators are disregarded in Table IV.2, but are introduced further below.

The difference (D) results reported in panel (1) show that, prior to the announcement of the MCC (2002-2000), control of corruption decreased in the treatment group (i.e., perceived corruption became more pervasive), while it increased in the control group. This pattern is reversed in the 2004-2002 period, with an increase in control of corruption in the treatment group and a decrease in the control group.

The second panel in Table IV.2 reports the DDD results for equation (1) above. The DDD is positive and significant at the one percent level, implying that the countries in the treatment group did indeed react to the incentives offered by getting access to the MCC. In other words, it appears that the MCC had positive incentive effects even before becoming

[28] The allocation of the MCC candidate countries to the treatment and control groups is based on the control of corruption indicator of the year 2002 (reported in the scorebook of 2004).

[29] See Appendix IV.3 for the countries included in the treatment and control groups according to the different variants of our DDD analysis.

operational, probably because prospective candidate countries anticipated that the proposal by President Bush would reward reform efforts with considerable amounts of additional aid.[30]

The third panel of Table IV.2 slightly modifies equation (1), changing the periods employed for comparison:

$$DDD = ((Corr^{T}_{2006} - Corr^{T}_{2004}) - (Corr^{T}_{2002} - Corr^{T}_{2000})) - ((Corr^{C}_{2006} - Corr^{C}_{2004}) - (Corr^{C}_{2002} - Corr^{C}_{2000})) \quad (3)$$

Instead of capturing anticipation effects, we now consider the first two years of MCC operations. The DDD is significantly positive at the five percent level, but substantially smaller in size. This weakening of the MCC Effect may be somewhat surprising. It becomes fairly plausible, however, once the "rough start" of the MCC is taken into account (Rieffel and Fox 2008: 6 and 7). It took longer than expected for President Bush to sign the MCC-related legislation, and funding proposals met with "resistance from the Congress immediately." The first compact (with Madagascar) only came into effect in July 2005. Consequently, the prospect that reform efforts would be rewarded with additional US aid might have been diluted.

Comparing the 2008-2006 period with the 2002-2000 period (panel 4), the DDD is significant at the ten percent level only and its size is reduced further. Arguably, ongoing MCC operations led to increasing uncertainty about the timeliness and the amount of expected aid rewards. The number of signed compacts remained fairly small. It became increasingly obvious that the actual MCC budget would persistently fall short of the originally proposed additional US aid of US$ 5 billion per annum.[31] At the same time, the MCC "has been extraordinarily slow in disbursing the sizeable amount of funding appropriated to it, raising questions about the efficacy of this new model of performance and ownership-based aid giving" (Lancaster 2008: 8). Sustained reform efforts were further eroded by rumors that the MCC might not survive as a distinct aid agency under the Obama administration.[32] In total, our results imply that the effect was strongest directly after the announcement of the MCC and decreased in the following years of operations.[33]

[30] Note that the Bush administration announced the 16 performance indicators already at the end of 2002.

[31] The budget volume for 2010 amounts to only US$ 1.1 billion (http://www.mcc.gov/mcc/press/releases/release-uscongressapproves-121309.shtml).

[32] For instance, Rieffel and Fox (2008: 11) speculate that the MCC "is still 'small potatoes'" that may be moved into "a beefed up USAID."

[33] One might worry that mean reversion may drive the results. That is because treatment and control group are not balanced and symmetric: The countries in the treatment group all start below the median while two thirds of the countries in the control group start above the median. We gauge the relevance of this issue by estimating an

In the remaining panels of Table IV.2, we compare four-year periods in order to assess whether the findings reported so far are sensitive to the choice of two-year periods for the DDD calculations. Panel (5) separates the years 2008-2004 and 2004-2000. By taking 2004 as cut-off, this specification disregards any anticipation effects and focuses on possible incentive effects during the full period of MCC operations for which data are available. The DDD is not significant at conventional levels. This is no longer surprising when recalling that we found strong anticipation effects before, while enthusiasm among candidate countries about prospective rewards appears to have cooled progressively in the course of MCC operations.

The strongly positive incentive effects found in panel (6) fit perfectly into this pattern. By comparing the years 2006-2002 and 2002-1998 we capture anticipation effects as well as the effects during the first phase of MCC operations. Recent years are excluded during which the sustainability of reform efforts has come under increasing threat as a result of the uncertainty of rewards. The quantitative difference given in panel (6) is relevant. It is, e.g., similar to the difference in 2007 between Uganda (which is in our treatment group) and Nigeria (which is not). Of all the countries for which the control of corruption indicator is calculated, only 20 improved their indicator by at least this difference over the 2002-2006 period.

As the treatment group comprises only 14 countries, a visible presentation may be useful to identify potentially influential observations. Figure IV.3 shows the change in control of corruption of each candidate country prior to the announcement of the MCC (2002-1998) and after the announcement of the MCC (2006-2002). Countries between the two vertical lines correspond to the treatment group (second quartile). The other countries constitute the control group. Looking at the treatment group, the Solomon Islands (SLB) seems to be influential, with a decrease in control of corruption of 0.74 between 1998 and 2002 and an increase of 1.07 between 2002 and 2006. Excluding this country from the estimation, the DDD decreases to 0.24. Importantly, the effect remains significant, though at the ten percent level only.

The estimations shown in Table IV.2 are based on the composite index values of control of corruption as reported in the 2009 release of Kaufmann et al.'s data. However, we used both an earlier and a more recent release of this database (2008 and 2010, respectively) in order to assess whether the findings reported so far are sensitive to changes in the composition of Kaufmann et al.'s index. This does not appear to be the case. Rather, the

alternative specification: The control of corruption indicator of 2000 was used to define the treatment and control groups with 2000-1998 being the ex ante period and 2002-2000 the ex post period. Since we do not find any effect with this specification, mean reversion is likely not a problem.

results based on the earlier and most recent composition of the index reveal essentially the same pattern as before.[34] In particular, the evidence on the strong anticipation effects is corroborated for all releases of the index. In another (unreported) robustness test of our baseline results reported in Table IV.2, we excluded all countries for which the overall index on control of corruption relied on less than three sources. This applied to seven (mostly small) countries in 1998, the beginning of our period of observation.[35] Recalling the discussion of data issues in Section 4, the precision of the index is probably weakest for these countries. Moreover, compositional changes are most likely to matter when the number of underlying sources increases considerably over time.[36] Yet excluding these countries affects our results only modestly. The size of the DDDs and significance levels are somewhat lower, but the general pattern of effects remains the same.

Refined treatment groups

It has so far been assumed that the incentive effects of the MCC are restricted to candidate countries that do not lie too far below the median score (second quartile) for the control of corruption indicator. The allocation to the treatment and control group was based on the assumption that candidate countries far below the median (first quartile) had no reasonable chance to get their reform efforts rewarded by MCC aid and, thus, would not increase attempts to fight corruption effectively. We now test whether this assumption is valid.

In Table IV.3, we perform estimations with the treatment group comprising candidate countries falling into the first quartile rather than the second quartile, and the control group comprising candidate countries above the median. In this way, we can test directly whether candidate countries with unfavorable initial conditions nevertheless embark on reforms as a response to the announcement of the MCC. Our results clearly contradict such a proposition. The fairly remote chances that candidate countries falling far below the median score will receive MCC aid appear to have eroded the incentives to fight corruption. In most panels of Table IV.3, the DDDs are not significant at conventional levels. The only exception is the comparison between the 2002-1998 and 2006-2002 periods, where the difference is marginally significant. This marginal significance is driven by one influential observation, Georgia. Georgia is at the border to the second quartile, and substantially reduced corruption

[34] It should be noted that Panels (4) and (5) could not be replicated for the earlier release of the database as the data for 2008 were not yet available at that time. For the sake of brevity, the estimations based on the index values released in 2008 and 2010 are not shown in detail. However, the additional tables corresponding to Table IV.2 are available on request.

[35] Afghanistan, Bhutan, Cape Verde, Kiribati, Solomon Islands, Tonga, and Vanuatu.

[36] In the case of Afghanistan, the number of sources increased from just one in 1998 to nine in 2008.

over the relevant periods. When we exclude it from the treatment group, the DDD in Panel (6) of Table IV.3 is no longer significant. Note that the (previously strong) anticipation effects are no longer observed when initial conditions rendered it unlikely to have reform efforts rewarded through MCC aid.

Table IV.4 modifies the analysis of Table IV.2 to make the results strictly comparable with those reported for the first quartile in Table IV.3. The treatment group includes those countries with control of corruption in the second quartile (as in Table IV.2), but the control group is restricted to countries with control of corruption above the median (as for the first quartile in Table IV.3). As can be seen, the results are very similar compared to Table IV.2. In other words, the incentive effects of the MCC differ considerably depending on initial conditions; the effect is strong under favorable conditions and at best weak otherwise.

In the next step, we take into account that candidate countries scoring just slightly above the median (third quartile) may also have an incentive to control corruption more effectively. Even though eligibility for MCC aid is based on 2004 scores, candidates in the third quarter may have to "defend" their position by further reform efforts relative to reformers in the second quartile in order to retain eligibility in the future. Therefore, we enlarge the treatment group in Table IV.5, including all candidates in the second and third quartiles (with the first and fourth quartiles representing the control group). This split places a roughly equal number of countries in the treatment group and the control group.[37]

With the inclusion of Georgia in the control group, none of the DDDs are significant at conventional levels (not shown in table). Given Georgia's position at the border to the second quartile, we also tested for differences including it in the treatment group, as shown in Table IV.5. Overall, the effects are considerably weaker than those in Table IV.2. Nevertheless, there are several similarities. The comparison between the 2006-2002 and 2002-1998 periods in panel (6) again shows the strongest effect, significant at the ten percent level. As before, we find a positive effect – also significant at the ten percent level – when comparing the years 2004-2002 and 2002-2000 in panel (1). On the other three panels, the DDDs are not significant at conventional levels. This underscores the overriding role of anticipation effects in MCC candidate countries; MCC effects were increasingly eroded over the time of actual operations.

[37] Note that the MCC, when calculating the country scores on the different indicators, includes countries that would be considered candidate countries but are subject to legal provisions that prohibit them from receiving US economic assistance. However, these countries are excluded in the present analysis. This explains why the treatment and control groups do not precisely add up to the same number of countries in Table IV.5.

When comparing the significant DDDs in Table IV.5 with those in Table IV.2, we find that they have almost halved. This seems to imply that it is mainly countries below the median that have fought corruption in response to the announcement of the MCC, while countries above the median have not been incentivized to control corruption more effectively in order to defend their favorable positions. It cannot be ruled out that countries in the third quartile were simply myopic and unaware of the risk of losing eligibility once reformers that previously scored below them climbed up in the ranking. More plausibly, however, it may be argued that their reluctance to step up efforts toward controlling corruption was rational. Performance-based selection of eligible countries notwithstanding, it was open to question whether the MCC would be equally strict in suspending eligibility once it had been granted.[38] According to the MCC's Policy on Suspension and Termination, its CEO "*may* make a recommendation to the Board" to suspend eligibility if the country has "engaged in a pattern of actions inconsistent with selection criteria."[39] In a public outreach meeting in early 2007, the (former) CEO of the MCC, John Danilovich, stated:[40]

> *I sent remediation letters to our partner countries that exhibited certain slippages in our criteria in the areas of ruling justly, investing in people and economic freedom. We are constructively and continuously engaging our Compact-eligible countries to help them create and implement a corrective plan of action to address these areas of slippage.*

This suggests that, in contrast to performance-based selection, the credibility of the MCC in suspending eligibility may be no greater than the credibility of traditional aid agencies in imposing sanctions against countries violating previously agreed-upon conditions for aid. This would explain why the MCC Effect weakened considerably once countries had passed the eligibility criterion of scoring above the median with regard to the control of corruption.[41]

[38] Yemen appears to have been the first country (among very few cases up to now) that was suspended from MCC funding in late 2005, "due to policy slippage on a number of MCC's selection indicators" (http://www.mcc.gov/mcc/bm.doc/mcc-workingpaper-corruption.pdf).
[39] See: http://www.mcc.gov/mcc/bm.doc/07-suspensionandterminationpolicy.pdf; emphasis added.
[40] See: http://www.mcc.gov/mcc/bm.doc/transcript-021507-publicoutreach.pdf
[41] Along similar lines, Haughton (2007) argues that the EU's influence tends to be greatest *before* opening accession negotiations, as the threat of being excluded from membership weakens once negotiations have started.

Complex incentive structure

The separation of treatment and control groups has so far been based exclusively on the control of corruption. However, the incentives for candidate countries to fight corruption may also depend on where a country stands with respect to the other eligibility criteria used by the MCC. Deviation from other requirements can be expected to weaken the incentive to fight corruption, even when a country is close to the median score on the control of corruption indicator. The costs of compliance would clearly be higher for countries that have to reform on various fronts in order to become eligible. At the same time, the prospect of being rewarded with MCC aid would be fairly remote. In the following, we take this into account by refining our classification of the treatment and control groups.

In Table IV.6 the treatment group consists of 10 countries that meet two requirements simultaneously. As before in Table IV.2, these countries score in the second quartile with respect to control of corruption. In addition, they score in the second, third or fourth quartile with respect to each of the three broad MCC categories of Ruling Justly, Investing in People and Economic Freedom. The average scores for these broad categories are calculated according to the aforementioned MCC rule that, in order to become eligible, a country has to score higher than the median on at least half the eligibility criteria in each category. A country attempting to obey by this rule at minimal costs of compliance would probably focus on those eligibility criteria in each category where it is already performing close to the median level. Consequently, we average the scores for the three indicators on which the country performs best in the categories Ruling Justly and Economic Freedom. The two best indicators are considered in the category Investing in People.[42] Countries falling into the first quartile in any of these three broad categories are included in the control group. The related costs of compliance tend to be highest for these countries and the prospects of receiving aid rewards are most unfavorable. Consequently, their incentives to fight corruption might be particularly weak.

Accounting for the more complex incentive structure in Table IV.6 leads to results that are surprisingly similar to those reported in Table IV.2 above. Again, the differences (D) in panel (1) show that, prior to the announcement of the MCC, control of corruption decreased in

[42] Recall that the overall number of indicators in the category Investing in People is only four. Note also that inflation is considered for the average score in the category Economic Freedom and that we therefore do not consider the level of inflation as an independent criterion. This will not distort our results. None of the countries included in the treatment group in Table IV.6 has a rate of inflation above the threshold of 20 percent. It therefore seems that inflation is not a critical criterion for selection. In averaging the scores, all scores above the median of 50 are set equal to 50. This is because it has no implications for the costs of compliance whether a country performs considerably, or only slightly better than the median.

the treatment group, while it increased in the control group. This pattern is again reversed in the 2002-2004 period.

The DDD is positive in all sample periods, though (as before) not significant at conventional levels when comparing the years 2008-2004 and 2004-2000 (panel 5). Table IV.6 also confirms that the effect is largest when comparing the years 2006-2002 and 2002-1998 (panel 6), followed by the effect shown in panel (1) for the years 2004-2002 vs. 2002-2000. In other words, the dominance of positive anticipation effects is corroborated when refining the classification of treatment and control groups according to the more complex incentive structure. Likewise, taking account of all eligibility criteria confirms the previous finding that the MCC Effect weakened over time, even though this development is less pronounced than in Table IV.2. Rising uncertainty among candidate countries about the timeliness and amount of aid rewards again appears to have undermined incentive effects.

Robustness tests

Finally, we perform a range of robustness tests. In particular, we assess whether previous results are sensitive to the classification of MCC candidate countries into treatment and control groups. We systematically combine alternative variants of the two dimensions of this classification used before, namely the cut-off with regard to the control of corruption and the cut-off with regard to the three broad categories Ruling Justly, Investing in People, and Economic Freedom. In terms of the three categories, the treatment group either comprises all countries except those in the first quartile (variant 1), or all those above the median (variant 2). In terms of control of corruption, the treatment group comprises countries scoring (a) in the second quartile (as in Tables IV.2 and IV.6), (b) in the second quartile, but with the reduced control group (as in Table IV.4), or (c) in the second and third quartile (as in Table IV.5). In Appendix IV.3, we list the countries falling into the treatment groups under each alternative.

Table IV.7 lists the results starting with variant 1 of the cut-off for the three categories combined with alternative cut-offs for corruption in columns 1b and 1c.[43] Columns 2a to 2c present the results for combinations of variant 2 of the cut-off for the three categories with all three variants of corruption. We consider variant 2 in order to test whether our results are sensitive to the implicit assumption under variant 1 that scoring below the median in any category increases the costs of compliance to the same extent. It is hardly possible to relax

[43] Note that the combination of variant *1* of the three categories and variant *a* of corruption is the same as that presented in Table IV.6 above.

this assumption by accounting for varying costs of compliance for each category as these costs cannot be observed. Instead, we require in variant 2 that countries in the treatment group perform better than the median in all three categories and, thus, do not incur additional costs of compliance. This "restrictive" refinement results in relatively small treatment groups (see Appendix IV.3) whose incentives to fight corruption are not affected by varying costs of compliance across the three categories.

Table IV.7 only reports the DDDs for the five robustness tests, omitting differences and difference-in-differences. As can be seen, the results are similar throughout, corroborating the major findings reported above. First of all, the comparison of the years 2004-2002 and 2002-2000 reveals significantly positive DDDs in all five columns, at the ten percent level in column 1c and the five percent level at least for the remaining specifications. This underscores the prominence of anticipation effects. It also turns out, as before, that the evidence for *sustained* reform efforts is weaker, as indicated by the comparison of the years 2008-2006 and 2002-2000. The DDD for this comparison is not significant at conventional levels in column 1c and significantly positive only at the ten percent level in column 2c. Likewise, the MCC Effect during the first phase of operations appears to be weak in columns 1c and 2c where the treatment groups include candidates scoring in the second and third quartiles with respect to the control of corruption. This resembles the insignificant incentive effects reported in Table IV.5 that used the same cut-off for corruption. However, the MCC Effect during the first phase of operations turns out to be slightly stronger than the anticipation effect in columns 2a and 2b.

Our previous results based on four-year periods for the DDD calculations, instead of two-year periods, are hardly affected by the redefinition of our treatment and control groups in Table IV.7. The DDD is largest throughout, and significant at the five percent level at least, when we compare the years 2006-2002 and 2002-1998 which attempts to capture anticipation effects together with the MCC effects during the first phase of operations. In sharp contrast, the DDDs become substantially smaller in size when comparing the four years of MCC operations with the four years before the MCC commenced operations, with three DDDs being completely insignificant.

Interestingly, the DDDs are generally larger in columns 2a and 2b where the definition of the treatment group applies variant 2 of the cut-off for the three broad categories. As noted above, this definition is relatively rigorous and results in comparatively small treatment groups. It is plausible that larger DDDs result from relegating those countries which fall below the median in at least one category to the control group. These countries have weaker

incentives to fight corruption effectively as the costs of compliance tend to be higher if reforms are required on several fronts.[44] However, applying variant 2 of the cut-off for the three broad categories yields rather ambiguous results (with most DDDs being smaller compared to column 1b) when treatment in terms of corruption includes candidate countries from the third quartile (column 2c). This corroborates the previous argument that the MCC Effect might weaken considerably once countries have been selected as eligible as the threat to suspend eligibility in the case of policy slippage does not appear particularly credible.

IV.6 Summary and Conclusion

Performance-based aid has been proposed as an alternative to the failed traditional approach of donors making aid conditional on reform promises of recipient countries. However, empirical evidence on the effectiveness of ex post rewards of reforms hardly exists. We attempt to fill this gap by investigating the so-called MCC Effect. The Millennium Challenge Corporation was explicitly established as a new US aid agency to pursue an innovative approach to the allocation of aid. Strict selectivity in granting aid exclusively to needy and well performing recipients was expected to strengthen the reform-mindedness of possible recipient countries.

The analysis in this paper has focused on the control of corruption – an aspect of reform that commands widespread support in the aid community and features most prominently in the MCC's aid eligibility criteria. Employing a difference-in-difference-in-differences (DDD) approach, we find that the MCC was successful in promoting better control of corruption. Candidate countries that had reasonably good chances of gaining access to the MCC (as they scored relatively close to the selection criteria thresholds) fought corruption more effectively than other candidate countries. The impact of the MCC on corruption is not only statistically significant, but also of quantitative importance. The results reported in panel 6 of Table IV.2, e.g., imply an effect of the MCC amounting to the difference in corruption (in 2007) between Uganda (which is in our treatment group) and Nigeria (which is not). Of all the countries for which the control of corruption indicator is calculated, only 20 improved their indicator by at least this difference over the 2002-2006 period. This finding suggests that performance-based aid may succeed in promoting reforms, in contrast to aid conditioned on recipients' promises to reform.

However, the MCC experience also reveals that the incentive effects of performance-based aid are rather weak, or even absent, under several circumstances. While we find

[44] Recall, however, that we cannot compare the costs of compliance across the three categories.

surprisingly strong anticipation effects, i.e., candidates fighting corruption even before MCC operations started, the incentive effects weakened over time. Arguably, this was mainly because it became increasingly uncertain whether the timeliness and magnitude of the MCC's aid rewards would sufficiently compensate for the costs of reform efforts. In other words, sustained reform efforts may suffer not only from insufficient willingness on the part of aid recipients, but also from broken promises and delayed aid on the part of donors.

Even if the rewards for reform could be taken for granted, the incentive effects appear to be restricted to a sub-group of candidate countries. According to our results, the MCC failed to give any impetus to the fight against corruption in candidate countries that scored far below the eligibility threshold. Economically, this finding is plainly intuitive: These countries had only remote chances to receive MCC aid as a reward for reforms, while the costs of complying with MCC conditions were comparatively high. The implication is rather troubling, however, as the MCC concept of performance-based aid tends to fail exactly where the need for reforms is most urgent.

Arguably, the MCC concept should be adapted to strengthen the incentive effects for countries where starting conditions are unfavorable. Rather than offering fruits that simply hang too high, the thresholds for eligibility could be designed relative to a country's own performance. A country could be eligible, e.g., if it improved its control of corruption score by a predefined extent over a specific period of time. This modified approach could also take into account that the costs of corruption to a country do not arise from its relative position compared to other countries, but from absolute corruption levels. The modified approach would thus help reduce corruption in those countries suffering from it the most.

Another limitation of the MCC concept relates to those candidate countries that have already passed the eligibility criteria. As long as they are not too far above the threshold, one would expect that, by judging performance relative to the median, the MCC would provide incentives for these candidates to further improve their score on the control of corruption. Our results suggest, however, that candidates having passed the threshold did not intensify reform efforts in an attempt to defend their position against reformers trying to become eligible. More research is necessary in order to identify more clearly the reasons behind this apparently irrational, or at least myopic, behavior.

As it seems, the MCC concept is flawed in its ability to encourage continued reform efforts once a country has passed the gate-keeping stage of becoming eligible. In contrast to strict selectivity in defining eligible countries, the MCC's credibility in suspending eligibility does not appear to be stronger than the credibility of international financial institutions,

notably the IMF, and traditional aid agencies in imposing sanctions against countries that violate previously agreed-upon conditions for adjustment loans and development aid. The conclusion is obvious, but may be hard to enforce: Strict selectivity must be complemented with automatic suspension for performance-based aid to have better chances at succeeding across the board.

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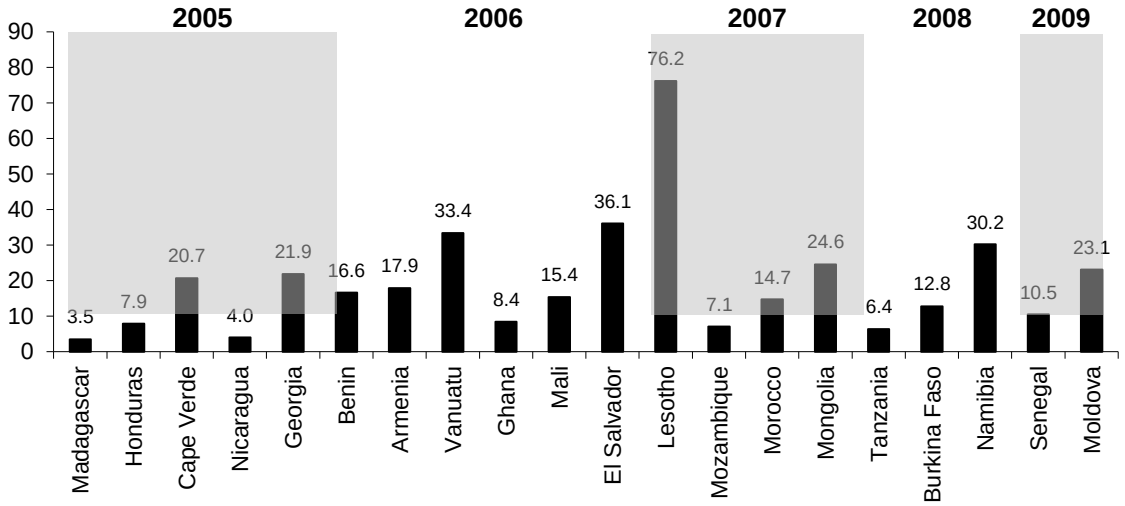
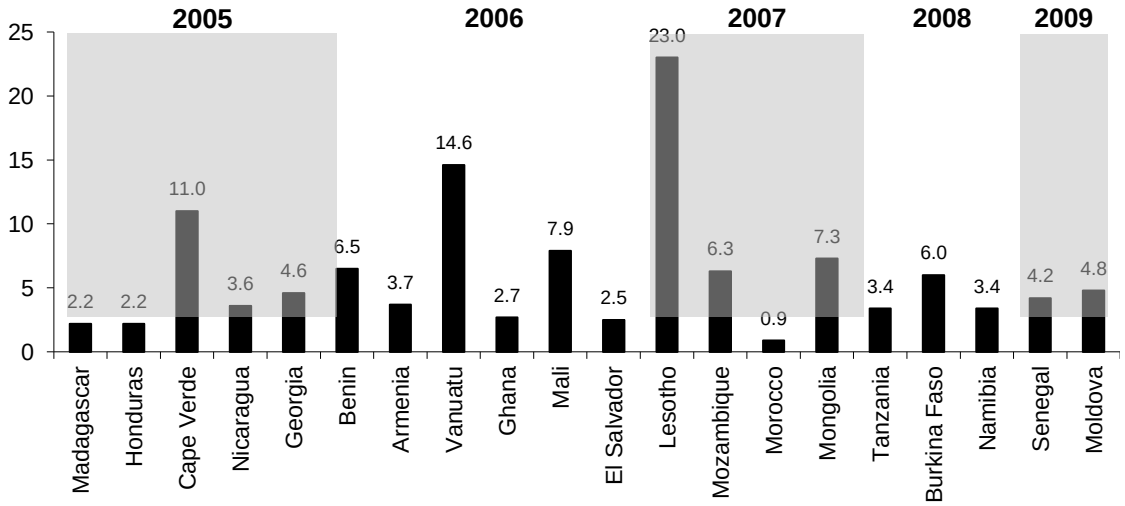
Figure IV.1: MCC compact obligations in percent of total aid commitments of all donors in previous five years



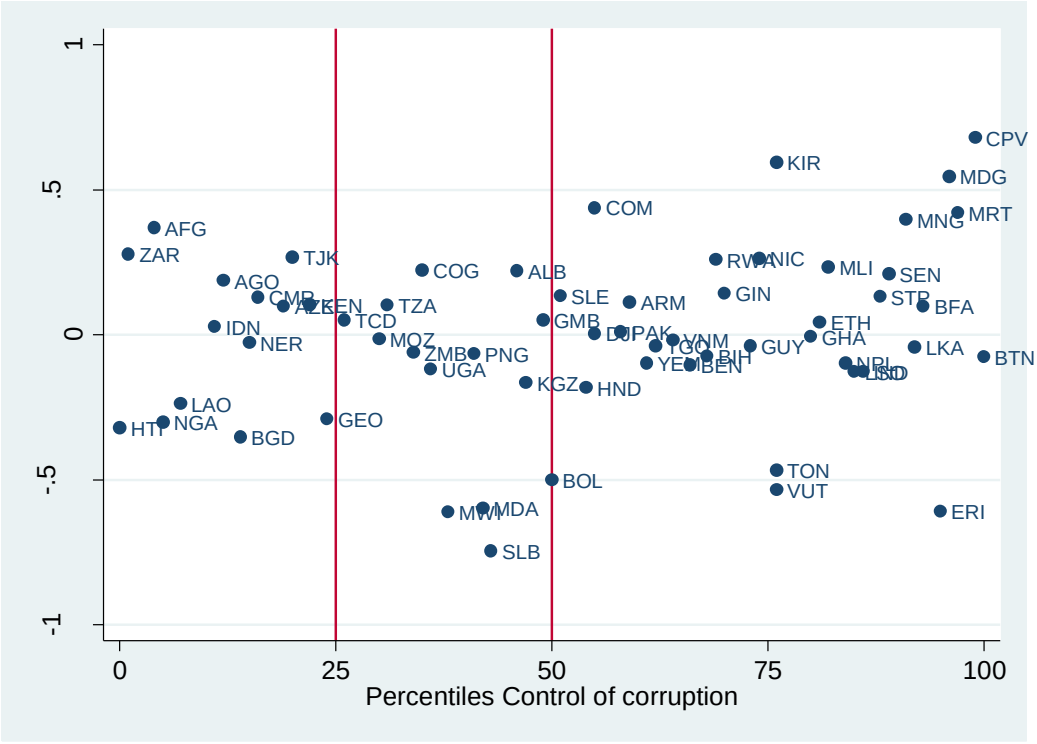
Notes: MCC compact obligations as given in Appendix IV.2; listed according to years when the compacts were signed. Total aid commitments as reported by the OECD in the five previous years.
Sources: Millennium Challenge Corporation (http://www.mcc.gov/mcc/panda/index.shtml); OECD (http://stats.oecd.org/qwids).

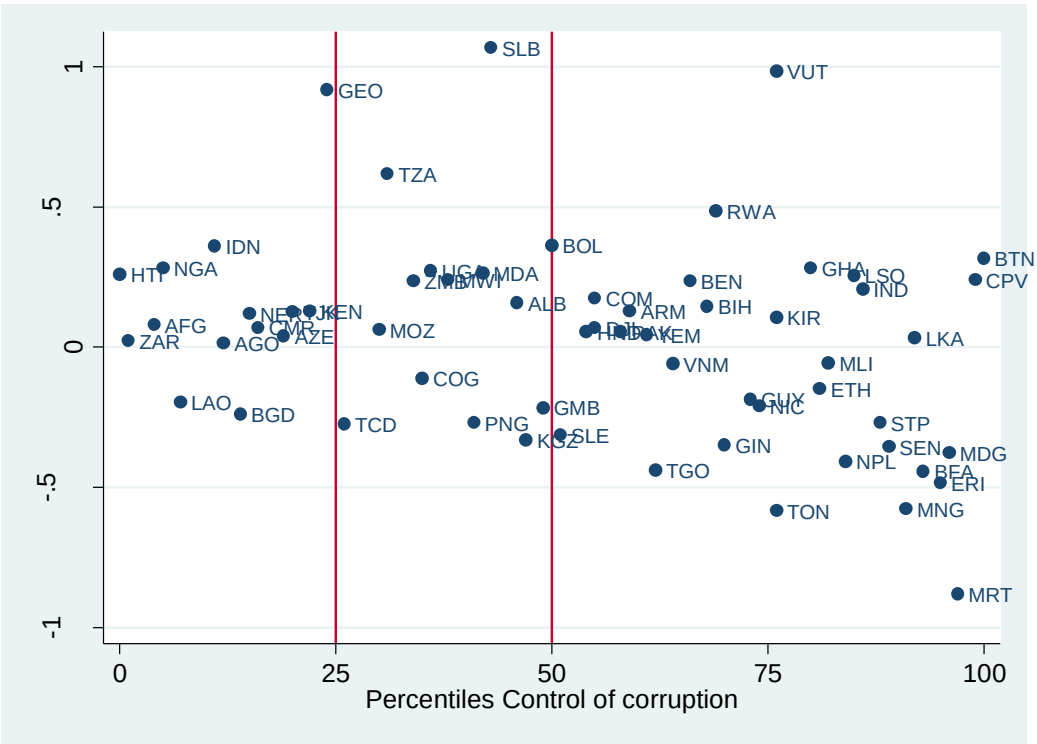
Figure IV.2: MCC compact obligations in percent of recipient countries' GDP



Notes: MCC compact obligations as given in Appendix IV.2; listed according to years when the compacts were signed. GDP in the year of signing compacts (2009 for Moldova).
Sources: Millennium Challenge Corporation (http://www.mcc.gov/mcc/panda/index.shtml); World Bank (WDI)

Figure IV.3: Change in Control of corruption before and after the announcement of MCC





Notes: Shows the change in control of corruption of each candidate country prior to the announcement of the MCC (2002-1998) and after the announcement of the MCC (2006-2002). Countries between the two vertical lines correspond to the treatment group (second quartile). The other countries constitute the control group.

Table IV.1: Summary Statistics

Control of	Control group					Treatment group				
Corruption	Obs.	Mean	Std. Dev.	Min	Max	Obs.	Mean	Std. Dev.	Min	Max
1998	48	-0.66	0.52	-1.91	0.78	14	-0.75	0.29	-1.21	-0.33
2000	48	-0.66	0.53	-1.91	0.67	14	-0.79	0.23	-1.11	-0.37
2002	48	-0.62	0.52	-1.70	0.59	14	-0.91	0.19	-1.35	-0.49
2004	48	-0.65	0.49	-1.51	0.82	14	-0.82	0.18	-1.18	-0.51
2006	48	-0.63	0.51	-1.51	0.91	14	-0.76	0.28	-1.22	-0.28
2008	48	-0.60	0.50	-1.64	0.75	14	-0.75	0.33	-1.45	-0.41

Notes: The treatment group consists of countries with control of corruption in the second quartile, while countries with control of corruption in the 1st, 3rd or 4th quartile are in the control group (as in Table IV.2 below).

Table IV.2: MCC Eligibility and Control of Corruption (2nd quartile)

(1)

	D Treatment			
Period	02-00	04-02	06-04	08-06
Mean	-0.12	0.09	0.06	0.02
SE	0.05	0.08	0.04	0.04
SD	0.20	0.28	0.16	0.14
Countries	14	14	14	14

	D Control			
Period	02-00	04-02	06-04	08-06
Mean	0.04	-0.03	0.02	0.03
SE	0.03	0.03	0.04	0.02
SD	0.20	0.23	0.25	0.15
Countries	48	48	48	48

	Period		DD Treatment	DD Control	DDD	t-value
(2)	2004-2002	Mean	0.21	-0.06	0.27	2.84 ***
	2002-2000	SE	0.08	0.05	0.09	
(3)	2006-2004	Mean	0.18	-0.02	0.20	2.12 **
	2002-2000	SE	0.08	0.04	0.09	
(4)	2008-2006	Mean	0.13	-0.01	0.14	1.87 *
	2002-2000	SE	0.07	0.04	0.08	
(5)	2008-2004	Mean	0.11	0.04	0.07	0.60
	2004-2000	SD	0.10	0.06	0.12	
(6)	2006-2002	Mean	0.31	-0.05	0.36	2.54 **
	2002-1998	SE	0.12	0.07	0.14	

Notes:
Treatment group: corruption 2nd quartile; control group: corruption 1st, 3rd or 4th quartile. Panel 1: difference in the level of corruption for the treatment and control groups (D Treatment and D Control, respectively) in the periods indicated in the head row. Panels 2-6: DD represents the difference between the D's for the two periods indicated in the front column; DDD represents the difference between the corresponding DD's for the treatment and control groups; for details, see equations (1) and (2) in the text. Panels 2-4 compare two-year periods, while panels 5 and 6 compare four-year periods; * (**,***): significant at the ten (five, one) percent level.

Table IV.3: MCC Eligibility and Control of Corruption (1st quartile)

(1)

	D Treatment			
Period	02-00	04-02	06-04	08-06
Mean	0.00	0.05	0.09	0.05
SE	0.05	0.06	0.03	0.04
SD	0.20	0.22	0.13	0.15
Countries	14	14	14	14

	D Control			
Period	02-00	04-02	06-04	08-06
Mean	0.05	-0.06	-0.01	0.02
SE	0.03	0.04	0.05	0.03
SD	0.20	0.23	0.28	0.16
Countries	34	34	34	34

	Period		DD Treatment	DD Control	DDD	t-value
(2)	2004-2002	Mean	0.05	-0.11	0.16	1.65
	2002-2000	SE	0.08	0.05	0.10	
(3)	2006-2004	Mean	0.08	-0.06	0.14	1.41
	2002-2000	SE	0.09	0.05	0.10	
(4)	2008-2006	Mean	0.05	-0.03	0.08	1.01
	2002-2000	SE	0.07	0.04	0.08	
(5)	2008-2004	Mean	0.08	0.02	0.07	0.53
	2004-2000	SD	0.11	0.07	0.13	
(6)	2006-2002	Mean	0.15	-0.13	0.28	1.96 *
	2002-1998	SE	0.12	0.08	0.14	

Notes:

Treatment group: corruption 1st quartile; control group: corruption above the median. Panel 1: difference in the level of corruption for the treatment and control groups (D Treatment and D Control, respectively) in the periods indicated in the head row. Panels 2-6: DD represents the difference between the D's for the two periods indicated in the front column; DDD represents the difference between the corresponding DD's for the treatment and control groups; for details, see equations (1) and (2) in the text. Panels 2-4 compare two-year periods, while panels 5 and 6 compare four-year periods; * (**,***): significant at the ten (five, one) percent level.

Table IV.4: MCC Eligibility and Control of Corruption (2nd quartile)

(1)

	D Treatment			
Period	02-00	04-02	06-04	08-06
Mean	-0.12	0.09	0.06	0.02
SE	0.05	0.08	0.04	0.04
SD	0.20	0.28	0.16	0.14
Countries	14	14	14	14

	D Control			
Period	02-00	04-02	06-04	08-06
Mean	0.05	-0.06	-0.01	0.02
SE	0.03	0.04	0.05	0.03
SD	0.20	0.23	0.28	0.16
Countries	34	34	34	34

	Period		DD Treatment	DD Control	DDD	t-value
(2)	2004-2002	Mean	0.21	-0.11	0.32	3.16 ***
	2002-2000	SE	0.08	0.05	0.10	
(3)	2006-2004	Mean	0.18	-0.06	0.24	2.33 **
	2002-2000	SE	0.09	0.06	0.10	
(4)	2008-2006	Mean	0.13	-0.03	0.17	2.07 **
	2002-2000	SE	0.07	0.04	0.08	
(5)	2008-2004	Mean	0.11	0.02	0.09	0.69
	2004-2000	SD	0.11	0.07	0.13	
(6)	2006-2002	Mean	0.31	-0.13	0.44	2.88 ***
	2002-1998	SE	0.13	0.08	0.15	

Notes:

Treatment group: corruption 2nd quartile; control group: corruption above the median. Panel 1: difference in the level of corruption for the treatment and control groups (D Treatment and D Control, respectively) in the periods indicated in the head row. Panels 2-6: DD represents the difference between the D's for the two periods indicated in the front column; DDD represents the difference between the corresponding DD's for the treatment and control groups; for details, see equations (1) and (2) in the text. Panels 2-4 compare two-year periods, while panels 5 and 6 compare four-year periods; * (**,***): significant at the ten (five, one) percent level.

Table IV.5: MCC Eligibility and Control of Corruption (2nd or 3rd quartile)

(1)

	D Treatment			
Period	02-00	04-02	06-04	08-06
Mean	-0.03	0.05	0.04	0.02
SE	0.04	0.05	0.04	0.03
SD	0.20	0.25	0.19	0.14
Countries	30	30	30	30

	D Control			
Period	02-00	04-02	06-04	08-06
Mean	0.03	-0.05	0.01	0.03
SE	0.04	0.04	0.05	0.03
SD	0.21	0.24	0.27	0.16
Countries	32	32	32	32

	Period		DD Treatment	DD Control	DDD	t-value
(2)	2004-2002	Mean	0.08	-0.08	0.16	1.93 *
	2002-2000	SE	0.06	0.06	0.08	
(3)	2006-2004	Mean	0.07	-0.02	0.09	1.12
	2002-2000	SE	0.06	0.06	0.08	
(4)	2008-2006	Mean	0.05	0.00	0.04	0.67
	2002-2000	SE	0.05	0.05	0.07	
(5)	2008-2004	Mean	0.04	0.06	-0.02	-0.24
	2004-2000	SE	0.07	0.07	0.10	
(6)	2006-2002	Mean	0.15	-0.08	0.23	1.95 *
	2002-1998	SE	0.09	0.08	0.12	

Notes:
Treatment group: corruption 2nd or 3rd quartile; control group: corruption 1st or 4th quartile. Panel 1: difference in the level of corruption for the treatment and control groups (D Treatment and D Control, respectively) in the periods indicated in the head row. Panels 2-6: DD represents the difference between the D's for the two periods indicated in the front column; DDD represents the difference between the corresponding DD's for the treatment and control groups; for details, see equations (1) and (2) in the text. Panels 2-4 compare two-year periods, while panels 5 and 6 compare four-year periods; * (**,***): significant at the ten (five, one) percent level.

Table IV.6: MCC Eligibility and Control of Corruption (complex incentive structure)

(1)

	D Treatment			
Period	02-00	04-02	06-04	08-06
Mean	-0.12	0.06	0.07	0.05
SE	0.07	0.06	0.05	0.04
SD	0.21	0.19	0.17	0.13
Countries	10	10	10	10

	D Control			
Period	02-00	04-02	06-04	08-06
Mean	0.02	-0.01	0.02	0.02
SE	0.03	0.04	0.02	0.02
SD	0.20	0.25	0.24	0.15
Countries	52	52	52	52

	Period		DD Treatment	DD Control	DDD	t-value
(2)	2004-2002	Mean	0.18	-0.04	0.22	1.96 *
	2002-2000	SE	0.10	0.04	0.11	
(3)	2006-2004	Mean	0.19	0.00	0.19	1.82 *
	2002-2000	SE	0.10	0.04	0.11	
(4)	2008-2006	Mean	0.17	0.00	0.17	1.98 **
	2002-2000	SE	0.08	0.04	0.09	
(5)	2008-2004	Mean	0.18	0.03	0.15	1.15
	2004-2000	SD	0.12	0.05	0.13	
(6)	2006-2002	Mean	0.31	-0.02	0.33	2.05 **
	2002-1998	SE	0.15	0.06	0.16	

Notes:

Treatment group: corruption 2nd quartile,the three categories 2nd, 3rd, or 4th quartile; control group: corruption 1st, 3rd or 4th quartile or at least one category 1st quartile. Panel 1: difference in the level of corruption for the treatment and control groups (D Treatment and D Control, respectively) in the periods indicated in the head row. Panels 2-6: DD represents the difference between the D's for the two periods indicated in the front column; DDD represents the difference between the corresponding DD's for the treatment and control groups; for details, see equations (1) and (2) in the text. Panels 2-4 compare two-year periods, while panels 5 and 6 compare four-year periods; * (**,***): significant at the ten (five, one) percent level.

Table IV.7: Tests for Robustness

	Period		(1b) DDD	(1b) t	(1c) DDD	(1c) t	(2a) DDD	(2a) t	(2b) DDD	(2b) t	(2c) DDD	(2c) t
(3)	2004-2002	Mean	0.25	2.28 **	0.16	1.94 *	0.31	2.45 **	0.33	2.65 ***	0.25	2.71 ***
	2002-2000	SE	0.11		0.08		0.13		0.12		0.09	
(4)	2006-2004	Mean	0.22	1.98 **	0.09	1.07	0.33	2.65 ***	0.35	2.81 ***	0.16	1.71 *
	2002-2000	SE	0.11		0.08		0.12		0.12		0.09	
(5)	2008-2006	Mean	0.19	2.16 **	0.11	1.60	0.26	2.58 **	0.27	2.72 ***	0.14	1.80 *
	2002-2000	SE	0.09		0.07		0.10		0.10		0.08	
(6)	2008-2004	Mean	0.16	1.21	0.03	0.33	0.28	1.80 *	0.29	1.92 *	0.04	0.36
	2004-2000	SD	0.14		0.10		0.15		0.15		0.11	
(7)	2006-2002	Mean	0.37	2.31 **	0.27	2.15 **	0.46	2.46 **	0.49	2.70 ***	0.35	2.53 **
	2002-1998	SE	0.16		0.12		0.19		0.18		0.14	

Notes:

(1b) Treatment group: corruption in the 2nd quartile and not in the 1st quartile on any category; control group: corruption above the median or at least one category in 1st quartile.

(1c) Treatment group: 2nd or 3rd quartile of corruption and not in the 1st quartile on any category; control group: corruption 1st or 4th quartile or at least one category in the 1st quartile.

(2a) Treatment group: corruption in the 2nd quartile and above the median on the three categories; control group: corruption 1st, 3rd or 4th quartile or at least one category below median.

(2b) Treatment group: corruption in the 2nd quartile and above the median on the three categories; control group: corruption above the median or at least one category below median.

(2c) Treatment group: 2nd or 3rd quartile of corruption and above the median on the three categorie; control group: corruption 1st or 4th quartile or at least one category below median.

DDD represents the difference between the corresponding DD's for the treatment and control groups; for details, see equations (1) and (2) in the text. Panels 3-5 compare two-year periods, while panels 6 and 7 compare four-year periods; t = t-value; * (**,***): significant at the ten (five, one) percent level.

Appendix IV.1: MCC candidate countries in 2004

Afghanistan	Georgia	Nicaragua
Albania	Ghana	Niger
Angola	Guinea	Nigeria
Armenia	Guyana	Pakistan
Azerbaijan	Haiti	Papua New Guinea
Bangladesh	Honduras	Rwanda
Benin	India	Sao Tome And Principe
Bhutan	Indonesia	Senegal
Bolivia	Kenya	Sierra Leone
Bosnia-Herzegovina	Kiribati	Solomon Islands
Burkina Faso	Kyrgyzstan	Sri Lanka
Cameroon	Laos	Tajikistan
Cape Verde	Lesotho	Tanzania
Chad	Madagascar	Timor-Leste
Comoros	Malawi	Togo
Congo	Mali	Tonga
Congo, Dem. Rep.	Mauritania	Uganda
Djibouti	Moldova	Vanuatu
Eritrea	Mongolia	Vietnam
Ethiopia	Mozambique	Yemen
Gambia	Nepal	Zambia

Note: Countries that would be considered candidate countries but are subject to legal provisions that prohibit them from receiving US economic assistance are excluded. In the empirical analysis, Timor-Leste is excluded because of missings in the control of corruption index.

Appendix IV.2: MCC compacts, 2005-2010

Country	Signed	In force	Obligations ($ million)	Disbursements ($ million, as of Oct. 2009)
Madagascar (LIC)	April 2005	July 2005	109.8	81.5
Honduras (LIC)	June 2005	Sept. 2005	215.0	109.1
Cape Verde (LMIC)	July 2005	Oct. 2005	110.0	61.1
Nicaragua (LIC)	July 2005	May 2006	175.0	75.7
Georgia (LIC)[a]	Sept. 2005	April 2006	295.3	145.9
Benin (LIC)	Feb. 2006	Oct. 2006	307.3	49.0
Armenia (LMIC)	March 2006	Sept. 2006	235.7	41.3
Vanuatu (LIC)	March 2006	April 2006	65.7	38.6
Ghana (LIC)	Aug. 2006	Feb. 2007	547.0	89.9
Mali (LIC)	Nov. 2006	Sept. 2007	460.8	46.4
El Salvador (LMIC)	Nov. 2006	Sept. 2007	460.9	49.3
Lesotho (LIC)	July 2007	Sept. 2008	362.6	17.2
Mozambique (LIC)	July 2007	Sept. 2008	506.9	11.6
Morocco (LMIC)	Aug. 2007	Sept. 2008	697.5	22.2
Mongolia (LIC)	Oct. 2007	Sept. 2008	285.0	7.9
Tanzania (LIC)	Feb. 2008	Sept. 2008	698.0	7.9
Burkina Faso (LIC)	July 2008	July 2009	480.9	0
Namibia (LMIC)	July 2008	Sept. 2009	304.5	2.0
Senegal (LIC)	Sept. 2009	not yet	540.0	0
Moldova (LMIC)	Jan. 2010	not yet	262.0	0

Notes: LIC and LMIC in parentheses stand for low-income country and, respectively, lower-middle income country.
[a] Compact amendment involving an additional amount of $100 million signed in November 2008.

Source: Millennium Challenge Corporation (http://www.mcc.gov/mcc/panda/index.shtml); Center for Global Development (http://www.cgdev.org/section/initiatives/_active/mcamonitor); World Development Indicators (http://databank.worldbank.org/ddp/home.do); OECD, CRS (http://stats.oecd.org/Index.aspx?DatasetCode=CRSNEW); last accessed: March 2010.

Appendix IV.3: Countries in the respective treatment group

Country	Table 2	Table 3	Table 4	Table 5	Table 6	Table 7				
						(1b)	(1c)	(2a)	(2b)	(2c)
Afghanistan			X							
Albania	X	X		X	X	X	X	X	X	X
Angola			X							
Armenia				X			X			X
Azerbaijan			X							
Bangladesh			X							
Benin				X			X			X
Bolivia	X	X		X	X	X	X	X	X	X
Bosnia-Herzegovina				X			X			X
Cameroon			X							
Chad	X	X		X						
Comoros				X						
Congo	X	X		X						
Congo, Dem. Rep.			X							
Djibouti				X			X			
Gambia	X	X		X	X	X	X	X	X	X
Georgia			X	X			X			X
Guinea				X						
Guyana				X			X			X
Haiti			X							
Honduras				X			X			X
Indonesia			X							
Kenya			X							
Kyrgyzstan	X	X		X	X	X	X			
Laos			X							
Malawi	X	X		X	X	X	X	X	X	X
Moldova	X	X		X	X	X	X	X	X	X
Mozambique	X	X		X						
Nicaragua				X			X			X
Niger			X							
Nigeria			X							
Pakistan				X						
Papua New Guinea	X	X		X	X	X	X			
Rwanda				X			X			
Sierra Leone				X						
Solomon Islands	X	X		X						
Tajikistan			X							
Tanzania	X	X		X	X	X	X	X	X	X
Togo				X			X			
Uganda	X	X		X	X	X	X			
Vietnam				X			X			X
Yemen				X			X			
Zambia	X	X		X	X	X	X	X	X	X
Number of countries	14	14	14	30	10	10	22	7	7	15

Chapter V:

Do Aid Donors Coordinate Within Recipient Countries?

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V.1 Introduction

Recent international summits on the effectiveness of aid have put much emphasis on aid fragmentation and donor coordination. The Paris Declaration in March 2005 has outlined harmonization as one of the five principles to make aid more effective; donors committed themselves to more coordination in order to reduce duplication of aid efforts. The subsequent Accra Agenda for Action in 2008 has continued in this direction and emphasized that the division of labor among donors can be achieved "through improved allocation of resources *within* sectors, *within* countries, and across countries" (OECD 2008, §17, italics added).

Aid allocation and donor coordination on a regional and sectoral level within recipient countries have largely been neglected in the academic literature. According to the results of cross-country studies, donors are typically criticized for prioritizing their political and commercial interests and for neglecting the needs of the recipient countries when deciding on how to allocate aid (e.g., Alesina and Dollar 2000). Furthermore, studies on donor coordination conclude that aid allocation on a country level is largely uncoordinated between the different donors (e.g., Klasen and Davies 2011). However, the (more) important question of how aid funds are used within recipient countries is largely neglected. In fact, donors typically argue that studies on aid allocation across recipient countries do not capture their poverty focus within countries; in particular, high income inequality within many countries often implies that poverty affects large segments of the population (Nunnenkamp et al. 2012). More relevant for this study, duplication of aid efforts mainly occurs at the regional and sectoral level within recipient countries.[1] This leads to the well-known problems regarding aid fragmentation and a lack of donor coordination. Among them are high transaction costs (Acharya et al. 2006; Anderson 2011), administrative burden (Kanbur 2006, Lawson 2009, Roodman 2006), public administration being deprived of their best staff (Knack and Rahman 2007), blurred responsibilities among donors leading to a tragedy of the commons, moral hazard, and free rider problems (Dreher and Michaelowa 2010).

Since various projects are sector-specific and limited to particular regions within recipient countries, a disaggregated analysis on a regional and sectoral basis seems necessary to evaluate donor coordination.[2] Empirical studies have largely overlooked this issue so far. The costs of

[1] Lawson (2010) provides some examples: oversupply of insecticide-treated bed nets in one region while the people in another region receive none; or geological surveys for a road or water project in a specific region conducted by more than one donor.

[2] Even in a relatively small country such as Cambodia, only about half of all projects by official donors in 2000-2007 are considered nation-wide projects.

coordination failure could have been overestimated if coordination at a regional-sectoral level actually occurred. On the other hand, "if donors are not only failing to coordinate at a national level, but also sub-nationally, the costs of failed coordination could be even higher than currently estimated" (Powell and Findley 2011: 3). The scarce literature analyzing donor coordination on a more disaggregated level than merely across recipient countries consists of Aldasoro et al. (2010), Nunnenkamp et al. (2013) and Powell and Findley (2011). The latter look at coordination between the World Bank and the African Development Bank across regions within six recipient countries in Sub-Saharan Africa. In contrast, Aldasoro et al. (2010) and Nunnenkamp et al. (2013) employ sector-specific aid data at the recipient country level to assess coordination between 19 major donors.[3] However, these studies likely fail to properly identify duplication of aid efforts. Arguably, the fact that two donors are active in the same region within a recipient country does not imply aid duplication if the two donors are active in different sectors. The same applies in cases where two donors are active in the same sector but in different regions of a recipient country.[4]

The present study contributes to closing these gaps. First, we evaluate the coordination behavior of donors in the regional-sectoral space within a recipient country, i.e., Cambodia. More precisely, we consider at the same time the regional *and* sectoral dimension of projects and assess whether donors take active projects by other donors into account when deciding on whether to start a project in a given region and sector. Second, the exceptionally comprehensive database on aid projects in Cambodia allows us to consider the whole pool of donors active in the country, including China as an important emerging donor as well as numerous international (and Cambodian) non-governmental organizations (NGOs). Donor countries outside the Development Assistance Committee (DAC) of the OECD and NGOs have largely been neglected in donor coordination studies although they have become important development actors in the last few decades. Arguably, the severity of the coordination problem between official donors and NGOs depends on whether the two groups tend to be active in different regions and/or sectors or whether their aid allocation pattern is relatively similar.[5]

[3] Including EU institutions, the International Development Agency (IDA) and 17 major bilateral DAC donors.
[4] Furthermore, in the case of Powell and Findley (2011), the results may be biased because the other donors present in the recipient countries are not taken into account. For instance, a cluster of World Bank and African Development Bank projects in one area and no projects in another area does not necessary imply a lack of coordination between the two donors. Possibly, the neglected area by the World Bank and the African Development Bank is covered by other donors implying that an engagement by the aforementioned donors is not necessary in this area.
[5] Again, studies on this question only exist in a cross-country context (e.g., Dreher et al. 2012b).

Our results indicate a relatively high degree of donor coordination within Cambodia. However, the degree of coordination seems lower in the case of bilateral donors than in that of multilateral donors, suggesting that the bilateral donors' political and economic interests prevent them from coordinating more extensively. With respect to the behavior of NGOs, we find them to be mainly active in the same regions and sectors as official donors, implying coordination problems between the two groups of donors. In addition, NGOs appear to cluster in the regional-sectoral space although they seem to coordinate with each other once similar location choices across regions and sectors are controlled for.

The structure of the paper is as follows: In Section 2 we derive our hypotheses from the related literature. Section 3 gives a descriptive analysis of the aid landscape in Cambodia and introduces the method employed. Empirical results are presented in Section 4, while Section 5 concludes.

V.2 Hypotheses and related literature

Bilateral and multilateral official donors

Aid fragmentation and the lack of donor coordination have been widely recognized as principal problems impairing the effectiveness of official aid (e.g., Easterly 2007, Knack and Rahman 2007, Morss 1984).[6] The literature evaluating donor coordination, however, mainly consists of cross-country studies: Berthélemy (2006a) uses aid by other official donors as an explanatory variable when analyzing the allocation decisions by individual donors. He argues a negative coefficient to imply that donors coordinate with each other. According to Berthélemy (2006a), the coefficient switches sign from significantly positive to significantly negative once donor country fixed effects are accounted for. In contrast, Klasen and Davies (2011) find a significantly positive coefficient by taking the endogeneity of aid by other donors into account.[7] Frot and Santiso (2011) also find evidence for herding among donors when applying a method used in the literature on financial markets. By using a different approach, Mascarenhas and Sandler (2006) come to the clear conclusion that none of analyzed 15 official donors behaved cooperatively when deciding on the allocation of aid. Taken together, the evidence shows that official donors largely fail to coordinate across recipient countries. It is however open to question whether this

[6] The controversy on whether foreign aid has been effective to stimulate economic growth is ongoing (e.g., Rajan and Subramanian 2008, Clements et al. 2012).

[7] They use instruments borrowed from the spatial econometrics literature.

finding carries over to coordination *within* recipient countries. Analyses employing sector-specific aid data at the recipient country level provide first indications that coordination problems also exist within countries (Aldasoro et al. 2010, Nunnenkamp et al. 2013). Furthermore, Powell and Findley (2011) find a lack of coordination between the World Bank and the African Development Bank in four out of the total of six African countries studied.[8] This leads us to hypothesize:

H1: Coordination among official donors is largely lacking within recipient countries.

Taken at face value, the Paris Declaration in March 2005 should have improved donor coordination.[9] Donors agreed to "eliminating duplication of efforts and rationalizing donor activities to make them as cost-effective as possible" (OECD 2005: paragraph 3), acknowledging that aid fragmentation impairs effectiveness while "a pragmatic approach to the division of labour … can reduce transaction costs" (paragraph 33). Nevertheless, a recent cross-country study finds no improvement in aid fragmentation and donor coordination after 2005 (Nunnenkamp et al. 2013). Analyzing within-country aid fragmentation and donor coordination, the OECD (2011)'s own monitoring comes to mixed conclusions: while aid fragmentation largely increased in the 2005-2009 period, progress has been made in the division of labor in the same period.[10] Therefore, we hypothesize:

H2: Official donors started to coordinate more within countries after the Paris Declaration in 2005.

The degree of coordination may differ between bilateral and multilateral donors. It has been widely shown that bilateral donors follow commercial and geo-political motives when deciding

[8] Note, however, that Powell and Findley (2011) employ a rather different approach than the one used in this study. In particular, they argue that the optimal level of geographic clustering of aid projects depends on how concentrated the need is in the recipient countries. As described in more detail in the empirical analysis, we account for varying needs by including regional-sectoral fixed effects.

[9] Note that data limitations for the most recent years prevent us from testing whether the Accra Agenda for Action in 2008 has been effective in improving coordination among donors.

[10] The OECD (2011) report acknowledges that emerging donors and NGOs are not taken into account in their analysis. A case study by AFRODAD (2007) on Kenya also comes to the conclusion that some coordination efforts can be observed in the last few years; some donors have withdrawn from particular sectors and donors have increasingly used so-called SWAps (sector-wide approaches) and contributed to common basket funds although there are still several lead donors in some sectors.

on how to allocate their funds (e.g., Alesina and Dollar 2000, Kuziemko and Werker 2006).[11] For instance, former colonial ties or the War on Terror may urge bilateral donors to keep up their aid efforts.[12] This may prevent them from coordinating with other donors. Although the aid allocation of international organizations such as the World Bank or the IMF also appears to be shaped by the political interests of major stakeholders, most prominently of the US (e.g., Dreher et al. 2009a, 2009b), this seems less pronounced than in the case of bilateral donors. In addition, bilateral donor agencies appear to have more of an interest than multilateral organizations to "plant their flag" in any field where it is highly visible in order to demonstrate their engagement to the tax payers at home and secure future funding (Nunnenkamp et al. 2013).

H3: Multilateral donors coordinate more than bilateral donors.

As mentioned above, a recent study shows that bilateral donors react to aid flows from other donor countries by increasing their own aid funds (Klasen and Davies 2011). This occurs particularly in recipient countries where the donors compete economically (in terms of exports) and politically (in terms of UN votes) with each other (Barthel 2012, Curtone 2012). The direct competition between bilateral donors suggests that they are more reluctant to coordinate with each other than multilateral donors. In addition, the focus of some multilateral organizations is restrained to specific sectors (e.g., the Global Fund to Fight AIDS, Tuberculosis and Malaria), which already implies a certain degree of coordination. Indeed, results of cross-country studies show that herding (i.e., the opposite of coordination) among multilateral donors is not as pronounced as in the case of bilateral donors (Barthel 2012, Frot and Santiso 2011). We expect that this finding spills over to coordination within countries.

H3.1: Multilateral donors coordinate more among each other than bilateral donors do.

NGOs

NGOs are typically not taken into account in studies dealing with aid fragmentation and donor coordination despite being important donors in many recipient countries. The reason is that data

[11] However, some studies stress the heterogeneity among bilateral donors as some of them are regarded as more "altruistic donors" (e.g., Berthélemy 2006b).

[12] See Fleck and Kilby (2010) and Dreher and Fuchs (2011) for the War on Terror's effect on US aid and aid by other bilateral donors.

on NGO aid are scarce. If data on the aid allocation of international NGOs exist, they typically only cover NGOs based in one particular donor country with funds being allocated across recipient countries, with no additional information on how these funds have been used within countries. To assess coordination problems between official donors and NGOs, however, one would need to know whether the two groups tend to be active in different regions and sectors or whether their aid allocation patterns within countries are relatively similar.

The literature points to several reasons why the regional or sectoral focus of NGOs may differ from the focus of official donors. First, NGOs are widely believed to be more poverty oriented than official donors. The latter are often criticized since their projects are widely perceived as failing "to reach down and assist the poor" (Riddell and Robinson 1995: 2). This would imply that NGO activities are concentrated in poorer regions within a particular recipient country and in sectors that are more poverty-related (clean water, basic education, etc.). Second, NGOs are supposed to have a comparative advantage in difficult environments as they can more easily circumvent corrupt local governments and deal with local target groups directly (Riddell et al. 1995). NGOs acting according to their comparative advantage would, therefore, focus on regions with high levels of corruption which are likely to be neglected by official donors. Finally, NGOs are supposed to be more altruistic than official donors, i.e., their aid allocation is less likely to be shaped by commercial or political interests (Nancy and Yontcheva 2006: 3). This could have implications for the choice of both regions and sectors. For instance, NGOs may tend to work more in sectors that are not directly related to commercial interests (e.g., in social services instead of economic infrastructure) or in regions populated by people not representing the political constituency of the ruling recipient government.[13]

H4.a: NGOs are active in other regions and sectors than official donors, thereby alleviating coordination problems between the two groups of donors.

Risk aversion, however, may weaken the incentives of NGOs to work in difficult environments where extreme poverty and high levels of corruption decrease projects' chance of success. NGOs generally need to compete for funds, urging them to allocate aid strategically to where the probability of failure is low (Bebbington 2004). The principal-agent model of Fruttero

[13] Nunnenkamp et al. (2012) analyze whether the political constituencies of state governments influence the allocation of World Bank projects within India. However, they do not find any significant effect.

and Gauri (2005) shows that the dependence of NGOs (the agents) on external funding (from official donors as principals) leads them to abandon their objectives such as poverty alleviation to some extent in favor of organizational imperatives related to future NGO operations and sustained funding. This occurs even if the principals and agents share the same development objectives. The asymmetric information of the principals on NGO projects implies that NGOs are tempted to produce visible results to assure future funding. Hence, NGOs tend to avoid locations where "the risk of a failure is so high that it could jeopardize the flow of funding from donors" (Fruttero and Gauri 2005: 761). In cases where official financiers tend to favor short-term and quantifiable results, NGOs may be especially reluctant to work in regions with entrenched poverty or in sectors where outcomes are difficult to quantify (e.g., empowerment of certain groups) (Edwards and Hulme 1995). This reasoning invites a counterhypothesis to H4.a. In particular, the aid allocation of NGOs which depend heavily on official financing may not be too different from that of official donors. Evidence from cross-country studies suggests a relatively similar aid allocation of bilateral donors and international NGOs (Dreher et al. 2012b, Dreher et al. 2012c, Koch et al. 2009, Nunnenkamp et al. 2009), with Nancy and Yontcheva (2006) being an exception. Dreher et al. (2012b) even find that in the case of NGOs based in Switzerland, the more that they depend on official financing, the more their aid allocation imitates that of official Swiss aid.

H4.b: NGOs tend to be active in the same regions and sectors as official donors, creating coordination problems between the two groups of donors.

In addition to coordination problems between NGOs and official donors, coordination among NGOs is another issue with respect to NGO aid. Barr and Fafchamps (2006) argue that the marginal benefit from the engagement of an additional NGO falls with the amount of aid a location receives from other NGOs. NGOs exclusively interested in maximizing the welfare gains of the recipient country's citizens would thus be expected to focus their efforts in areas not already saturated with NGO aid. However, risk aversion may also work against coordinated aid allocation among NGOs. Fruttero and Gauri (2005, 761) argue that the principal's (the official financier's) ability to monitor and "determine whom to blame and whom to congratulate for development outcomes" is inversely related to the number of agents active in a particular location. Conversely, it is easier for a particular NGO to hide in a larger crowd of peers, where

their own contribution is but a small part of the larger whole. Easterly (2002, 245) argues along similar lines and posits that it is "the joint product of the many agents [that] makes it hard to evaluate the efforts of any one agent. . . . Hence, there is safety in numbers in the foreign aid business." The empirical analysis by Barr and Fafchamps (2006) supports these expectations by showing that the location choices by national NGOs in Uganda are characterized by "excessive geographical clustering."[14]

H5: NGOs tend to allocate their funds in regions and sectors where other (international or national) NGOs are already active.

V.3 Data and Method

The data for our analysis come from the CRDB/CDC database,[15] which has been developed by the Cambodian government as a response to the Paris Declaration in 2005. We consider the 2000-2007 period since a comparison between these data and those of AidData reveals that aid figures from the CRDB/CDC database are incomplete outside this period.[16] Figure V.1 shows the number of projects approved each year by bilateral and multilateral donors and international and national NGOs. It can be seen that the number of projects approved by official donors increased substantially (although in 2007 a decline in the number of projects by multilaterals can be observed), while in the case of the NGOs, the number stayed relatively constant. This, taken together with the fact that the number of official donors approving projects stayed relatively constant over the years (Figure V.2), reveals that aid proliferation by official donors increased in the 2000-2007 period.

Interestingly, the number of official donors approving a project in a given year was approximately the same as the number of international NGOs until 2005. A significant rise in the number of international NGOs only occurred in the last two years, the number exceeding twice that of official donors in 2007. Taken together, Figures V.1 and V.2 reveal that a single official

[14] Another study finds that "[n]orthern NGOs present in Kenya have not harmonized or aligned or even co-ordinated their activities." Furthermore, "[t]he relationship between Northern NGOs and Kenyan CSOs is eclectic. There is a lack of clarity in terms of roles and responsibilities" (Skalkaer Consult 2007: 28).

[15] http://www.cdc-crdb.gov.kh/ (accessed: March 2012).

[16] AidData is the most comprehensive dataset on foreign official aid, which combines different sources (OECD CRS, annual reports for aid by multilateral development banks and non-DAC bileteral donors etc.). Excluding China, which is not considered in the standardized dataset by AidData, the aid amounts of official donors in CRDB/CDC over the 2000-2007 period are slightly higher than the amounts in AidData (2.6% higher to be exact), although the figures vary in the individual years to a greater extent.

donor tends to carry out many more projects than a single NGO. More precisely, the average NGO starts only one project per year, while the average official donor starts six. One may expect that another difference between official donors and NGOs is the size of the projects. However, Table V.1 reveals that this difference is not substantial. The difference instead lies in the variation of the size: official projects vary much more in this regard than NGO projects. Surprisingly, projects by national NGOs are, on average, slightly larger than projects by international NGOs. An explanation might be that official donors rely more on national NGOs as a channel to distribute aid, thereby enlarging the projects of national NGOs. In fact, a comparison of the sources of funds by international and national NGOs shows that the share of official financing is significantly higher for national NGOs compared to international NGOs (53.0% vs. 31.4%). Smaller local NGOs not receiving official funds may abstain from reporting to CRDB/CDC.

As pointed out in Section 2, NGO proponents argue that NGOs are more poverty-oriented than official donors, which may lead them to work in different regions and sectors than the latter. By calculating simple correlations between per capita aid by official donors and NGOs and infant mortality per province, we may get a first glance on the poverty orientation of the two donor groups.[17] For official donors, the correlation is almost zero and not significant at conventional levels (rho = 0.06).[18] For NGOs, on the contrary, the correlation is -0.51 and significant at the five percent level. This means that a person living in a poor region (high infant mortality) receives less NGO aid compared to a person living in a rich region (low infant mortality). This is surprising and a closer look at the data reveals that this finding is largely driven by the concentration of NGO aid in the capital Phnom Penh. The capital receives the highest amount of aid per capita from NGOs, even though it has the lowest infant mortality (see Appendix V.1). Excluding Phnom Penh, the correlation is no longer significant, but still negative (-0.31). All the same, these results indicate that the poverty orientation of NGOs within Cambodia is not more pronounced than that of official donors.

The correlation between per capita aid by official donors and per capita aid by NGOs across regions is positive, although not significant (rho = 0.24). This speaks against different regional allocation decisions by official donors and NGOs which would have required a negative

[17] Per capita aid is calculated as an average over the 2000-2007 period. Data on infant mortality per province are taken from the 2010 Cambodia Demographic and Health Survey (CDHS), http://www.measuredhs.com/pubs/pdf/GF22/GF22.pdf (accessed: March 2012). Note that some provinces are merged in the CDHS. In these cases, we take the combined figures for the respective provinces. Cambodia is divided into 20 provinces and 4 cities (treated as provinces in our empirical analysis).

[18] The significance levels used in this paper are one, five and ten percent.

correlation. Concerning the sectoral allocation of projects, we observe a similar focus by official donors and NGOs; when ranking the sectors based on the number of projects in 2000-2007, the eight most important sectors are the same for both groups of donors.[19] If we calculate aid amounts by sector, the differences between official donors and NGOs are, however, more pronounced. For instance, "Transportation" is the third most important sector for official donors, a sector which is not covered at all by NGOs. Finally, the correlation between sector-specific amounts of official aid and sector-specific amounts of NGO aid across regions is almost zero and not significant (rho = 0.07).

The descriptive statistics have already revealed some interesting patterns. To evaluate the coordination behavior among the different donors more rigorously, we perform logit estimations with the basic specifications:

$$Pr(Project_{dpst} = 1) = F(\beta * Projects\ by\ other\ donors_{pst} + u_{ps} + v_d + y_t)$$
$$Pr(Project_{dpst} = 1) = F(\beta * Log\ aid\ by\ other\ donors_{pst} + u_{ps} + v_d + y_t)$$

where the dependent variable $Project_{dpst}$ is a dummy variable equal to one if donor d starts a project in province p and sector s in year t. $Projects\ by\ other\ donors_{pst}$ or $Log\ Aid\ by\ other\ donors_{pst}$ are the number of projects or the (logged) sum of committed aid funds, respectively, by other donors active in province p and sector s in year t. We control for province-sector fixed effects u_{ps} to account for sector-specific needs which vary across provinces.[20] v_d and y_t are donor and year dummies, respectively, and robust standard errors are clustered by province-sector pairs.

As the main variable of interest we use the number of active projects by other donors. More precisely, we accumulate, for each province-sector-year combination, all projects by other

[19] These sectors are: "Governance & Administration," "Agriculture," "Health," "Education," "Rural Development," "Community and Social Welfare," "Environment and Conservation," and "HIV/AIDS" (sorted by their importance for official donors; see Appendix V.2 for a complete list of sectors). While these sectors account for almost 100 percent of the NGO activities, the projects of official donors are somewhat more diversified, with other sectors accounting for 21 percent of all projects. In particular, a substantial number of projects can be observed in the sectors "Transportation" and "Water and Sanitation" (99 and 80, respectively). However, to make the subsequent analysis comparable between official donors and NGOs, we refrain from including these sectors.

[20] However, we cannot control for changes in sector-specific needs over the period under consideration (2004-2007 as described below). Nonetheless, we can reasonably well assume that we only cover sectors in which needs do not change dramatically within the few years studied. In particular, the analysis does not include emergency and food aid.

donors which are active in the respective year. The variable measures the degree of aid duplication in the regional-sectoral space when donor d is confronted with the decisions whether or where to start a project. Alternatively, we use the sum of the aid funds by other donors (accumulated in the same way as the projects). Possibly, a donor may take into account the amount of aid allocated in the regional-sectoral space by other donors rather than the number of projects. In other words, a (large) donor may still intend to realize a project in a province and sector where only small, although many, projects are in place. In a final specification, we include both variables at the same time. The consideration of projects by other donors may depend on the size of these projects. In other words, we need to control for the total funds spent on these projects when evaluating the attention of donors to duplication of aid efforts.

We exclude nation-wide projects and limit our analysis to the eight sectors with a relevant number of projects by both official donors and NGOs in 2000-2007.[21] Importantly, we limit the time dimension T of the dependent variable to the 2004-2007 period. At the same time, when constructing the explanatory variables (i.e., *projects by other donors* and *log aid by other donors*), we not only consider projects that started between 2004 and 2007 but also projects that started between 2000 and 2003 if they are active in year t (t = 2004,…,2007). Note that the average duration of projects is 3.8 years. By not considering the years 2000-2003 in T, we avoid the possibility that the explanatory variable is biased: In t = 2000,…,2003 we would disregard many relevant projects, approved before the year 2000 and active in the years thereafter.[22]

V.4 Results

Bilateral and multilateral official donors

First, we examine the coordination behavior of bilateral and multilateral donors. NGOs are considered in the next subsection. The results of the baseline specification are presented in column (1) of Table V.2. The variable of interest, *projects by other donors*, turns out to be negative and significant, at the one percent level. This means that the likelihood of a bilateral or multilateral donor starting a project decreases with the number of projects already in place in a certain province, sector and year. Calculating marginal effects, we find that an additional active project by another bilateral or multilateral donor predicts a decrease in the likelihood of starting a

[21] About half of the projects are excluded because they are nation-wide projects. Additionally, about 20 percent of the projects are located in sectors which are not considered in the analysis.
[22] Recall that the CRDB/CDC dataset is incomplete before 2000.

project by 0.5 percent. This corresponds to a decrease of 1.5 percent when the change in the number of other projects is one standard deviation (3.3). Given that the average likelihood of a bilateral or multilateral donor to start a project in a certain province, sector and year is just four percent, the effect turns out to be relatively large. This result indicates a rather high degree of coordination among official donors. Given the existing evidence, made up of mostly cross-country studies, that donor coordination is largely lacking, this is an unexpected result (hypothesis *H1).* Nevertheless, the average number of active projects by other official donors in a certain province, sector and year of 3.5 reveals room for improvement.

In columns (2) and (3), we split the sample into the periods before and after the Paris Declaration, i.e., 2004-2005 and 2006-2007. We include the year 2005 in the pre-period because 2005 has arguably been too early to observe an effect of the Declaration. We find the marginal effects to be almost identical before and after the Declaration.[23] This result does not support hypothesis *H2*. Rather, it is in line with Nunnenkamp et al.'s (2013) finding that the Paris Declaration has not made a difference.

The question whether multilateral donors respond to the projects of other official (bilateral and multilateral) donors to a higher degree than bilateral donors is evaluated in column (4). We interact our variable of interest, *projects by other donors,* with a multilateral donor dummy.[24] Note that interaction effects cannot be inferred from the coefficients in logit estimations (Ai and Norton 2003).[25] Rather, marginal effects for bilateral and multilateral donors, respectively, have to be calculated. The marginal effects in Table V.3 do not support hypothesis *H3* (i.e., multilateral donors generally coordinate more than bilateral donors) as the difference in the marginal effects is not significant.

A more nuanced picture emerges if we look at how bilateral and multilateral donors respond to their respective peers (i.e., other bilateral or multilateral donors, respectively) (marginal effects in Table V.3 based on column (5) of Table V.2). We find that the effects are significantly different at the one percent level. This indicates that multilaterals respond more

[23] The marginal effects are also similar if we exclude 2005 from the pre-period (not shown). However, the marginal effect of the pre-period is not significant anymore (p-value = 0.12), possibly because of the drop in the number of observations.

[24] In order to include the multilateral donor dummy, individual donor dummies need to be excluded here and for the rest of the estimations in this subsection.

[25] According to Greene (2010), the decision of including an interaction term in a regression should depend on whether it improves the goodness of fit. Nevertheless, an interaction effect can also be present in logit models where the interaction term is not significant (Berry et al. 2010). We opt to always keep the interaction terms in the estimations as the loss of efficiency is only marginal with the present number of observations.

strongly to projects of other multilaterals than bilateral donors do to projects of other bilateral donors. In other words, multilateral donors coordinate more among each other than bilateral donors do, confirming hypothesis *H3.1*. However, we also find that bilateral donors respond to projects by multilateral donors to a similar degree as multilaterals do amongst themselves. This is in line with the reasoning in Section 2, i.e., political and economic competition among bilateral donors prevents them from coordinating more extensively with each other. It seems that duplication of aid efforts is less likely to occur in the case of multilateral aid.

Table V.4 presents the alternative specifications. In columns (1)-(5), Table V.2 is replicated with the sum of aid funds instead of the number of projects by other donors as the explanatory variable. The results are qualitatively similar throughout, with two major exceptions: First, we now find multilateral donors generally reacting more strongly to aid funds by other donors than bilateral donors do, independent of whether the aid is bilateral or multilateral (see marginal effects in Table V.5, based on column (4) of Table V.4). Second, the effect of aid funds from bilateral peers turns out to be insignificant for bilateral donors. In other words, we do not find any evidence of coordination among bilateral donors when it comes to the amount of aid allocated within Cambodia. If bilateral aid in specific regions and sectors is high, bilateral donors seem to be reluctant to step back and leave the field to other donor countries. As noted before, the reason may lie in their political or economic interests in (important) regions and sectors where other bilateral donors are also present.

Quantitatively, the marginal effect of the sum of aid funds is smaller than the effect of the number of projects. An increase in aid funds by one standard deviation leads to a decrease in the likelihood of starting a project by only 0.7 percent (based on column (1) of Table V.4). In addition, the pseudo R^2 is higher in the estimations using the number of projects as the explanatory variable. Hence, donors seem to pay attention to the number of projects rather than the amount of aid spent by other donors when deciding how to allocate their aid funds. This is quite plausible considering that projects by other donors are easy to spot, while amounts spent on projects are often unknown to outsiders.

The estimations in columns (6)-(10) include both variables, i.e., number of projects and aid amounts. Controlling for the sum of aid funds by other donors, the marginal effect of the number of projects by other donors is still significant at the one percent level and similar in size as above (see Table V.5, based on column (6) of Table V.4). The other previous findings also carry over to the new specification. Note, however, that the difference between bilateral and

multilateral donors in how they react to their peers' projects is now only significant at the ten percent level, while the difference is still significant at the one percent level when looking at the peers' aid funds (see Table V.5, based on column (10) of Table V.4).

International and national NGOs

We now turn to the results with respect to international and national NGOs active in Cambodia. To evaluate the conflicting hypotheses, i.e., whether NGOs engage in different regions and sectors than official donors (*H4.a*), or whether NGOs tend to be active in the same regions and sectors as official donors (*H4.b*), we include *projects by official donors* as the explanatory variable and no province-sector fixed effects in column (1) of Table V.6.[26] The marginal effect in the respective column of Table V.7 turns out to be positive and significant at the five percent level.[27] This indicates regional-sectoral clustering of NGOs with official donors, implying coordination problems between the two groups of donors. In column (2), we differentiate between projects by bilateral and multilateral donors in the explanatory variables. It turns out that clustering only occurs with multilateral donors. Nevertheless, the insignificant effect of bilateral projects is still in line with *H4.b* as it shows that the regional-sectoral aid allocation of NGOs is not significantly different from that of bilateral donors. In column (3), we differentiate between international and national NGOs. The marginal effects in the respective column of Table V.7 show that both international and national NGOs cluster with multilateral donors, but not with bilateral donors. Nevertheless, neither international nor national NGOs allocate their aid to different regions and sectors than bilateral donors.

In columns (4)-(6) of Table V.6, we augment the model with province-sector fixed effects again. In this way, we account for the fact that sector-specific needs vary across provinces.[28] Accordingly, it might be justifiable that the different donors largely neglect some provinces in sector-specific aid allocation decisions. The marginal effect in column (4) of Table V.7 is not significant at conventional levels implying no coordination of NGOs with official donors. This finding is in contrast to the revealed coordination among official donors. By differentiating between projects by bilateral and multilateral donors, the marginal effects in column (5) of Table

[26] Note that NGO fixed effects are included in columns (1), (2), (4) and (5) of Table V.6, i.e., in the columns where the International NGO dummy is not included.

[27] Although the average likelihood for NGOs to start a project in a certain province, sector and year is very small (0.5 percent), the effect is not particularly large: An additional active project by a bilateral or multilateral donor predicts an increase in the likelihood of starting a project by 0.02 percent.

[28] See: http://www.measuredhs.com/pubs/pdf/FR249/FR249.pdf (accessed: November 2012).

V.7 reveal weak evidence of coordination with bilateral donors, but no coordination with multilateral donors. By looking at the different results for international and national NGOs in column (6), we only find a negative and significant effect in the case of national NGOs with bilateral donors.[29]

Now we turn to the question whether NGOs tend to allocate their funds in regions and sectors where other (international or national) NGOs are already active (columns (1)-(3) of Table V.8 and V.9). To test this, we do not include province-sector fixed effects.[30] The positive and significant marginal effect in column (1) of Table V.9 clearly reveals regional-sectoral clustering among NGOs.[31] By differentiating between international and national NGOs in column (2), we find clustering of international and national NGOs among their respective peers. Furthermore, international NGOs engage predominantly in regions and sectors where national NGOs are already active. Conformity of locations seems to be important for NGOs to hide in the crowd.[32] In contrast, national NGOs do not appear to take the projects by their international counterparts into account when deciding where to start a project. Finally, when including the explanatory variables of both Table V.6 (V.7) and V.8 (V.9), i.e., projects by international and national NGOs as well as projects by bilateral and multilateral donors, in column (3), all the presented results remain qualitatively unchanged.

In relation to the point above, it should be noted that similar location choices may also be based on need considerations. NGOs may work in the same regions and sectors because of the urgent needs in these fields. In columns (4)-(6), we therefore augment the model with province-sector fixed effects again. By controlling for province-sector fixed effects, we assess coordination among NGOs abstracting from similar location choices of NGOs across regions and sectors. The negative and quantitatively relatively large marginal effect in column (4) indicates that, in contrast to the results with respect to coordination of NGOs with official donors, NGOs seem to coordinate with each other.[33] By differentiating between international and national NGOs we find that NGOs take account of projects by international and national NGOs to a different degree: On

[29] The effect turns out to be relatively large (0.09 percent).
[30] In Table V.8, NGO fixed effects are included in columns (1) and (4).
[31] The quantitative effect is the same as in the case of an additional project by a bilateral or multilateral donor (0.02 percent).
[32] For international NGOs, however, an alternative explanation is also possible: as they prefer to closely work together with their national counterparts when implementing their projects they may choose regions and sectors where (enough) national NGOs are present.
[33] Keeping in mind that the average likelihood for NGOs to start a project in a certain province, sector and year is just 0.5 percent, an effect of 0.08 percent can be considered to be relatively large.

the one hand, national NGOs do not take the projects by their peers into account. Likewise, the evidence of international NGOs considering the projects of their national counterparts is weak (negative marginal effect only significant at the ten percent level). On the other hand, both international and national NGOs clearly take projects by the peers of international NGOs into account (both marginal effects significant at the one percent level).[34] When again including the explanatory variables of both Tables V.6 (V.7) and V.8 (V.9), the results remain largely unchanged. A notable exception is that, in contrast to above, projects by bilateral donors are not only taken into account by national but also by international NGOs.

V.5 Conclusion

Recent international summits have put emphasis on aid fragmentation and donor coordination in order to improve the effectiveness of aid. In the 2005 Paris Declaration, donors promised to render aid more effective by "eliminating duplication of efforts and rationalizing donor activities to make them as cost-effective as possible" (OECD 2005: paragraph 3). Aid coordination *within* developing countries has been highlighted as a particularly crucial issue in the 2008 Accra Agenda for Action. Nevertheless, rigorous quantitative analyses of within-country aid coordination are largely missing. This study helps to close this gap by investigating the degree of donor coordination among all official donors and NGOs present in Cambodia.

We find a relatively high degree of coordination among donors between 2004 and 2007 although a relatively large number of donors in each province-sector pair reveals room for improvement. All the same, bilateral donors seem to coordinate with each other to a lesser extent than multilateral donors do, revealing the Paris Declaration of 2005 as not having made a difference. This may indicate that aid continues to be regarded as a political or commercial tool in the competition among donor countries.

It cannot be ruled out that coordination, in particular among bilateral donors, has improved in more recent years. We fail to capture the possible impact of the Accra Agenda for Action in 2008 that highlighted the importance of within-country division of labor. However, large and sudden changes are unlikely to occur as long as aid is used as a means to foster political and economic interests and donor countries have an interest to "plant their flag" in any field

[34] However, this result may reflect crowding out of other NGOs by international NGOs rather than coordination in a more narrow sense; in particular national NGOs may be kept off from engaging in a region and sector where international NGOs are already active.

where it is highly visible in order to demonstrate their engagement to the tax payers at home and secure future funding (Nunnenkamp et al. 2013).

A particular challenge for the future will be the inclusion of the numerous international (and national) NGOs in coordination efforts (besides important non-DAC donor countries like China). NGOs do not appear to work in different fields than official donors, implying coordination problems between the two groups of donors. The situation is being aggravated by the fact that NGOs appear to cluster in the regional-sectoral space although they seem to coordinate with each other once similar location choices across regions and sectors are controlled for.

It is open to question whether the findings of the present study carry over to other recipient countries. Only further research on other countries can show to what extent these results can be generalized. The crucial point here lies in the availability of data that allows identifying the locations of projects within countries. Some other recipient governments have taken the effort to collect data on official aid in their country (e.g., Mozambique).[35] The initiative by AidData of geocoding projects by official donors is also promising in this regard.[36]

[35] However, to the best of our knowledge, no other recipient country has included NGO aid in their databases.
[36] See: http://www.aiddata.org/content/index/AidData-Raw/geocoded-data (accessed: November 2012).

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Figure V.1: Number of projects by official donors and NGOs

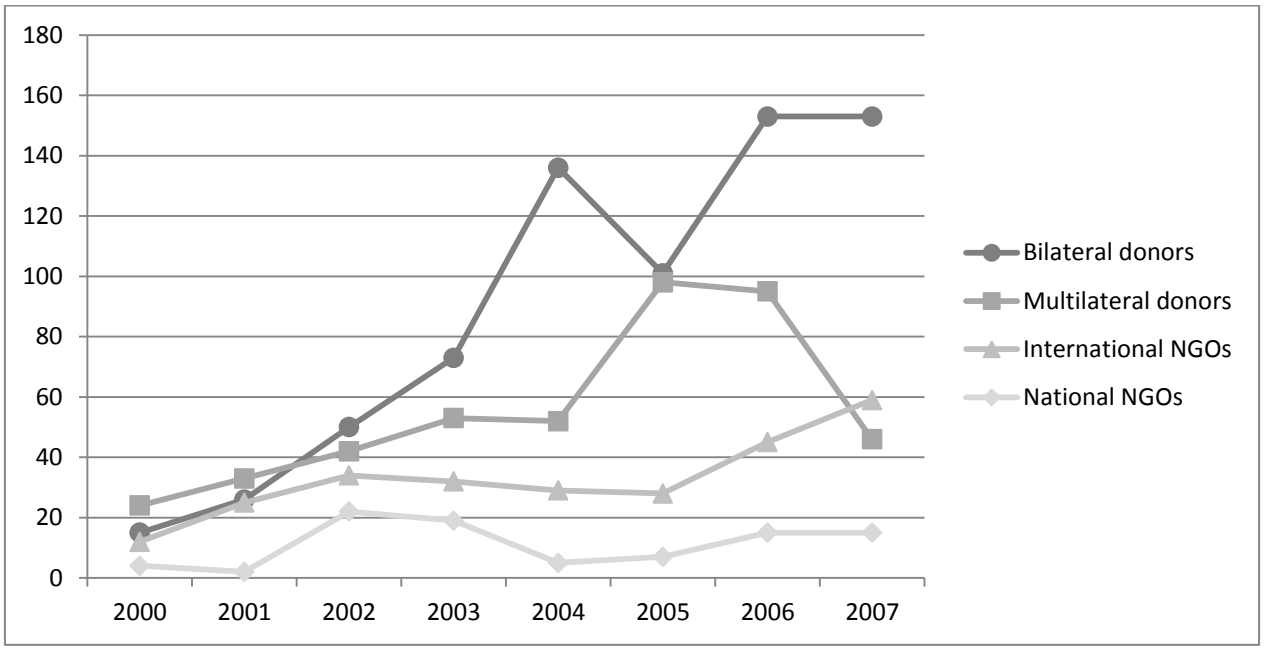

Figure V.2: Number of official donors and NGOs

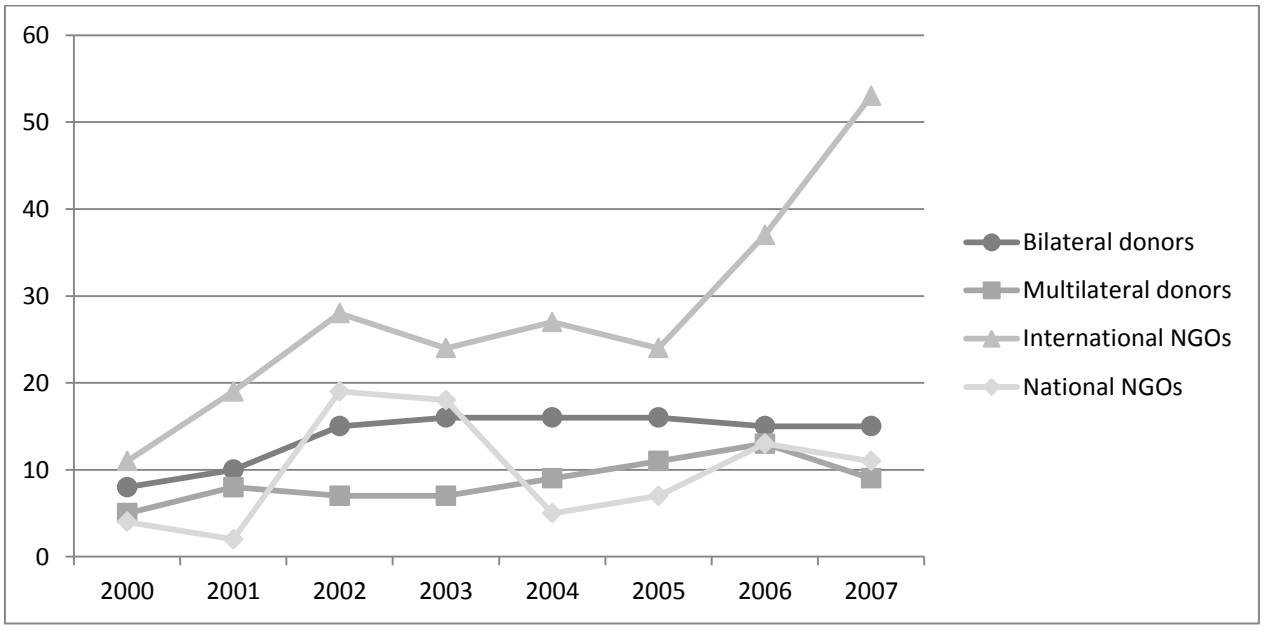

Table V.1: Average project size by donor (2000-2007)

	Average project size (in US$)	Std. Dev.	Min	Max
NGOs	**3,803,388**	**7,841,503**	**65,515**	**56,700,000**
International NGOs	3,760,726	7,174,211	65,515	56,700,000
National NGOs	4,032,883	10,900,000	76,000	55,100,000
Official donors	**4,992,827**	**13,700,000**	**2,982**	**244,000,000**
Bilateral donors	4,280,994	13,300,000	3,503	244,000,000
Multilateral donors	6,026,640	14,200,000	2,982	144,000,000

Table V.2: Logit estimations, whole period, before vs. after Paris Declaration, bilateral vs. multilateral donors

	(1)	(2)	(3)	(4)	(5)
	2004-2007	2004-2005	2006-2007	2004-2007	2004-2007
Projects of other bi- and multilateral donors	-0.300***	-0.965***	-0.849***	-0.345***	
	(0.040)	(0.180)	(0.114)	(0.040)	
Multilateral donor				0.234*	0.217
				(0.130)	(0.135)
Projects * Multilateral				-0.055***	
				(0.021)	
Projects of (other) bilateral donors					-0.194***
					(0.061)
(Other) bilateral projects * Multilateral					-0.105**
					(0.052)
Projects of (other) multilateral donors					-0.436***
					(0.071)
(Other) multilateral projects * Multilateral					-0.030
					(0.031)
Constant	0.586*	9.937***	-2.115***	-0.079	-0.253
	(0.337)	(1.486)	(0.248)	(0.339)	(0.331)
Observations	20,608	7,682	7,488	20,608	20,608

Notes: Robust standard errors clustered by province-sector pair in parentheses; province-sector pair and year dummies included in all columns; donor dummies included in columns (1)-(3); *** p<0.01, ** p<0.05, * p<0.1.

Table V.3: Marginal effects based on Table 2, columns (4) and (5)

	column (4)	column (5)	
	Projects by other donors	Projects by other bilateral donors	Projects by other multilateral donors
Bilateral donor	-0.009***	-0.005***	-0.011***
	(0.001)	(0.002)	(0.002)
Multilateral donor	-0.010***	-0.008***	-0.012***
	(0.001)	(0.002)	(0.002)
Observations	20,608	20,608	20,608

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table V.4: Logit estimations, *Log aid funds* as (additional) explanatory variable

	(1) 2004-2007	(2) 2004	(3) 2005-2007	(4) 2004-2007	(5) 2004-2007	(6) 2004-2007	(7) 2004	(8) 2005-2007	(9) 2004-2007	(10) 2004-2007
Projects by other bi- and multilateral donors						-0.285***	-0.360***	-0.319***	-0.348***	
						(0.040)	(0.054)	(0.051)	(0.041)	
Multilateral donor				0.651***	0.413**				0.643***	0.393**
				(0.203)	(0.173)				(0.219)	(0.188)
Projects * Multilateral									-0.013	
									(0.024)	
Projects by (other) bilateral donors										-0.223***
										(0.075)
(Other) bilateral projects * Multilateral										0.031
										(0.066)
Projects by (other) multilateral donors										-0.403***
										(0.067)
(Other) multilateral projects * Multilateral										-0.013
										(0.039)
Log aid funds by other bi- and multilateral donors	-0.076***	-0.087***	-0.104***	-0.068***		-0.056***	-0.066***	-0.074***	-0.044**	
	(0.014)	(0.018)	(0.017)	(0.018)		(0.016)	(0.019)	(0.018)	(0.022)	
Log aid funds * Multilateral				-0.047***					-0.046**	
				(0.014)					(0.018)	
Log aid funds by (other) bilateral donors					-0.012					0.006
					(0.016)					(0.022)
Log (other) bilateral aid funds * Multilateral					-0.050***					-0.056***
					(0.012)					(0.017)
Log aid funds by (other) multilateral donors					-0.107***					-0.074***
					(0.017)					(0.021)
Log (other) multilateral aid funds * Multilateral					0.005					0.000
					(0.013)					(0.019)
Constant	-0.740***	-0.378	-0.368	-2.030***	-1.251***	1.325***	2.185***	1.859***	0.672	0.770
	(0.259)	(0.320)	(0.302)	(0.302)	(0.381)	(0.419)	(0.508)	(0.487)	(0.483)	(0.537)
Observations	20,608	10,688	16,732	20,608	20,608	20,608	10,688	16,732	20,608	20,608

Notes: Robust standard errors clustered by province-sector pair in parentheses; province-sector pair and year dummies included in all columns; donor dummies included in columns (1)-(3) and (6)-(8); *** p<0.01, ** p<0.05, * p<0.1.

Table V.5: Marginal effects based on Table 4, columns (4), (5), (9) and (10)

	Marginal effects based on Table 4								
	column (4)	column (5)		column (9)			column (10)		
	Aid funds of other donors	Aid funds of (other) bilateral donors	Aid funds of (other) multilateral donors	Projects of other donors	Aid funds of other donors	Projects of (other) bilateral donors	Projects of (other) multilateral donors	Aid funds of (other) bilateral donors	Aid funds of (other) multilateral donors
Bilateral donor	-0.002***	-0.000	-0.003***	-0.008***	-0.001**	-0.005***	-0.010***	0.000	-0.002***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.000)
Multilateral donor	-0.003***	-0.002***	-0.003***	-0.009***	-0.002***	-0.005**	-0.010***	-0.001**	-0.002***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.002)	(0.001)	(0.000)	(0.000)
Observations	20,608	20,608	20,608	20,608	20,608	20,608	20,608	20,608	20,608

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table V.6: Logit estimations, international and national NGOs, coordination with official donors, coefficients

	(1)	(2)	(3)	(4)	(5)	(6)
Projects by official donors	0.074***			-0.021		
	(0.025)			(0.038)		
Projects by bilateral donors		0.038	-0.131		-0.081*	-0.258**
		(0.064)	(0.108)		(0.048)	(0.101)
Projects by multilateral donors		0.094***	0.149***		0.030	0.098
		(0.030)	(0.042)		(0.056)	(0.065)
International NGO			-0.035			0.011
			(0.156)			(0.181)
Projects by bilateral donors * INGO			0.204***			0.210**
			(0.078)			(0.084)
Projects by multilateral donors * INGO			-0.071**			-0.089**
			(0.031)			(0.040)
Constant	-5.255***	-5.232***	-5.202***	-5.783***	-5.755***	-5.707***
	(0.114)	(0.125)	(0.188)	(0.333)	(0.321)	(0.364)
Observations	109,824	109,824	109,824	85,228	85,228	85,228

Notes: Robust standard errors clustered by province-sector pair in parentheses; year dummies included in all columns; NGO dummies included in columns (1), (2), (4) and (5); province-sector pair dummies included in columns (4)-(6); *** p<0.01, ** p<0.05, * p<0.1.

Table V.7: Logit estimations, international and national NGOs, coordination with official donors, marginal effects

	(1)	(2)	(3)		(4)	(5)	(6)	
			International NGO	National NGO			International NGO	National NGO
Projects by official donors	0.0002**				-0.0001			
	(0.0001)				(0.0001)			
Projects by bilateral donors		0.0001	0.0003	-0.0005		-0.0002*	-0.0002	-0.0009***
		(0.0002)	(0.0003)	(0.0004)		(0.0001)	(0.0002)	(0.0003)
Projects by multilateral donors		0.0003***	0.0003***	0.0006***		0.0001	0.00004	0.0004
		(0.0001)	(0.0001)	(0.0002)		(0.0001)	(0.0002)	(0.0002)
Observations	109,824	109,824	109,824	109,824	85,228	85,228	85,228	85,228

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table V.8: Logit estimations, coordination among international and national NGOs, coefficients

	(1)	(2)	(3)	(4)	(5)	(6)
Projects of other NGOs	0.093***			-0.416***		
	(0.009)			(0.090)		
International NGO		0.052	-0.007		0.028	0.015
		(0.132)	(0.168)		(0.154)	(0.196)
Projects by (other) international NGOs		0.010	0.036		-0.540***	-0.547***
		(0.028)	(0.031)		(0.143)	(0.111)
Projects by (other) international NGOs * INGO		0.054**	0.021		0.048**	0.025
		(0.024)	(0.031)		(0.023)	(0.032)
Projects by (other) national NGOs		0.363***	0.274***		-0.095	-0.248
		(0.080)	(0.096)		(0.169)	(0.205)
Projects by (other) national NGOs * INGO		-0.176**	-0.082		-0.145*	-0.089
		(0.083)	(0.099)		(0.077)	(0.097)
Projects by bilateral donors			-0.152			-0.465***
			(0.104)			(0.148)
Projects by bilateral donors * INGO			0.203**			0.162**
			(0.091)			(0.075)
Projects by multilateral donors			0.127***			0.070
			(0.039)			(0.072)
Projects by multilateral donors * INGO			-0.074**			-0.066*
			(0.036)			(0.039)
Constant	-5.527***	-5.564***	-5.651***	-4.955***	-4.944***	-3.569***
	(0.091)	(0.150)	(0.174)	(0.168)	(0.243)	(0.737)
Observations	109,824	109,824	109,824	85,228	85,228	85,228

Notes: Robust standard errors clustered by province-sector pair in parentheses; year dummies included in all columns; NGO dummies included in columns (1), (2), (4) and (5); province-sector pair dummies included in columns (4)-(6); *** p<0.01, ** p<0.05, * p<0.1.

Table V.9: Logit estimations, coordination among international and national NGOs, marginal effects

	(1)	(2)		(3)		(4)	(5)		(6)	
		International NGO	National NGO	International NGO	National NGO		International NGO	National NGO	International NGO	National NGO
Projects by other NGOs	0.0002***					-0.0008***				
	(0.0000)					(0.0001)				
Projects by (other) international NGOs		0.0002***	0.00004	0.0002***	0.0001		-0.0015***	-0.0015***	-0.0015***	-0.0014***
		(0.0001)	(0.0001)	(0.0001)	(0.0001)		(0.0003)	(0.0003)	(0.0003)	(0.0003)
Projects by (other) national NGOs		0.0007***	0.0013***	0.0007***	0.0009***		-0.0007*	-0.0003	-0.0010**	-0.0007
		(0.0002)	(0.0003)	(0.0002)	(0.0003)		(0.0004)	(0.0005)	(0.0005)	(0.0006)
Projects by bilateral donors				0.0002	-0.0005				-0.0009**	-0.0012***
				(0.0001)	(0.0003)				(0.0005)	(0.0004)
Projects by multilateral donors				0.0002**	0.0004***				0.00001	0.0002
				(0.0001)	(0.0001)				(0.0002)	(0.0002)
Observations	109,824	109,824	109,824	109,824	109,824	85,228	85,228	85,228	85,228	85,228

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Appendix V.1: Infant mortality and per capita official aid and NGO aid per province

Province	Infant mortality	Per capita official aid (in US$)	Per capita NGO aid (in US$)
Preah Vihear	95	14.4	33.6
Stung Treng	95	37.1	25.7
Mondul Kiri	82	107.2	31.8
Ratanak Kiri	82	20.9	46.4
Kampong Chhnang	78	21.5	42.2
Svay Rieng	78	13.2	24.8
Kracheh	76	60.2	32.7
Takeo	68	20.6	50.6
Kampong Speu	65	15.7	69.5
Prey Veng	64	17.2	21.2
Banteay Meanchey	61	22.8	34.9
Kandal	61	29.8	44.8
Kampot	60	31.3	24.2
Krong Kep	60	4.1	32.5
Kampong Thom	57	31.5	39.9
Kampong Cham	54	21.8	36.4
Pursat	53	17.6	30.2
Koh Kong	50	6.9	43.3
Krong Preah Sihanouk	50	95.1	80.9
Siem Reap	50	38.8	122.4
Battambang	45	28.6	60.3
Krong Pailin	45	8.8	11.3
Otdar Meanchey	42	22.3	35.0
Phnom Penh	13	39.3	124.4

Notes: Per capita aid is calculated as an average over the 2000-2007 period. Data on infant mortality per province are taken from the 2010 Cambodia Demographic and Health Survey (CDHS), http://www.measuredhs.com/pubs/pdf/GF22/GF22.pdf (accessed: March 2012). Note that some provinces are merged in the CDHS. In these cases, we take the combined figures for the respective provinces.

Appendix V.2: Number of projects and aid funds by official donors and NGOs

Sector	Official donors		NGOs	
	# of projects	Aid funds	# of projects	Aid funds
Governance & Administration	378	937,000,000	55	32,500,000
Agriculture	358	707,000,000	65	9,436,127
Health	341	953,000,000	142	77,400,000
Education	267	564,000,000	197	147,000,000
Rural Development	252	654,000,000	66	21,500,000
Community and Social Welfare	186	197,000,000	328	237,000,000
Environment and Conservation	120	171,000,000	39	45,200,000
HIV/AIDS	109	474,000,000	122	99,300,000
Transportation	99	835,000,000	0	
Water and Sanitation	80	148,000,000	4	922,505
Manufacturing, Mining Trade	54	242,000,000	1	
Gender	48	35,500,000	2	1,466,981
Energy, Power & Electricity	43	339,000,000	0	
Information and Communications	34	125,000,000	2	
Culture & Arts	31	35,100,000	3	1,604,333
Banking and Business Services	26	70,500,000	0	
Emergency & Food Aid	26	48,900,000	3	
Tourism	25	34,200,000	0	
Urban Planning & Management	18	22,000,000	0	
Budget & BoP Support	6	89,700,000	0	
Other	51	82,800,000	1	2,165,777

Notes: Number of projects and aid funds are accumulated over the 2000-2007 period. The total aid funds spent by NGOs are downward biased as the aid amounts of NGO projects are sometimes missing. Multi-sector projects are counted more than once.