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Aid Withdrawal as Punishment for Defaulting Sovereigns? An Empirical Analysis

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Working Paper

Aid Withdrawal as Punishment for Defaulting Sovereigns? An Empirical Analysis

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This paper empirically investigates whether donor countries punish sovereign defaults by reducing foreign aid flows. Our findings reject the hypothesis formulated in the theoretical literature that a default leads to a loss of foreign aid for the defaulting country. Creditor countries directly affected by the default do not reduce their aid disbursements. Hence, foreign aid is not used as a punishment instrument. Neither can it therefore serve as an enforcement mechanism for international debt contracts. Furthermore, other donors even raise the amount of development assistance allocated to the delinquent country by about 15% on average. Overall the amount of foreign aid given to the defaulting country increases by 6.4%.

Journal of Economic Literature Classification Codes: F34, F35, C23, C24

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1. Introduction

In contrast to private debt markets cross-border lending to sovereign entities is not enforced by any international bankruptcy law. If a private firm does not pay its liabilities, creditors have a legal claim to the firm's assets. In case of sovereign debt, creditors have no such tool to recoup the defaulted amount.[1] Nevertheless, we observe high quantities of sovereign debt in many countries which are generally repaid. This raises the question why this is the case. The answer is that there must be some mechanisms that make a default costly for the debtor country and thus deter sovereign defaults. In the following we investigate the existence of one specific type of default costs: a reduction in aid flows.

The literature on sovereign debt differentiates between two categories of default costs. First, a default causes a loss of reputation which in turn leads to rising borrowing costs or even to capital market exclusion. The second category covers direct sanctions. These may trigger, e.g., reductions in international trade after a default. The decline in aid flows as an additional sanctioning mechanism has been recently discussed by Asiedu and Villamil (2002). They argue that a defaulting country does not only suffer from a reduction in FDI inflows, but also from a loss of foreign aid. This raises the costs of a default and therefore makes it a less attractive option. Hence, foreign aid would reduce country risk and promote capital inflows to the debtor country. Following the theoretical argumentation of Asiedu and Villamil (2002), Asiedu et al. (2009) empirically investigate how foreign aid changes the sensitivity of FDI to country risk.[2] They show that foreign aid can in fact mitigate the adverse effect of sovereign risk on FDI. However, up to now, no study examines whether foreign aid is really used as a punishment instrument against defaulting countries. Is a default actually followed by a decline in foreign aid given to the defaulting country? The goal of this paper is to answer this question by relating aid flows to default events.

The existence of the transmission channel proposed by Asiedu and Villamil (2002) and Asiedu et al. (2009) is based on the assumption that foreign aid is granted because of strategic motives and is used for punishment in case of a default. To be specific, the

[1] It should be noted that the term default covers any change in the original debt contract leading to a loss of value for the creditor, e.g. debt rescheduling.

[2] In contrast to Asiedu and Villamil (2002), who assume that countries lose FDI and aid in case of a default, Asiedu et al. (2009) argue that a country loses both when expropriation occurs. Apart from this semantic difference, their model is identical. Furthermore, the empirical analysis does not distinguish between expropriation and default risk as it rests on a composite risk indicator that covers both concepts.

government of the donor country directs foreign aid to the debtor country to enforce debt repayment and to ensure FDI made by domestic firms. The idea that foreign aid is not solely given because of altruistic motives but is also determined by strategic and political considerations is not new. In their seminal work Alesina and Dollar (2000) highlight the importance of colonial past and political alliances as explanatory variables for foreign aid. They find that strategically important countries and former colonies receive much more foreign aid than comparable countries without one of these attributes; e.g. the U.S. gives the biggest part of its total foreign aid to Egypt and Israel and France directs most of its aid to former colonies. Using foreign aid to generate incentives for countries to pay their debt would be a further strategic motive.

If aid is used to punish a defaulting country we would expect to find a significant decrease in aid flows coming from creditor countries that are affected by the default. From the theoretical point of view, the reactions of other donor countries are not clear. To capture this heterogeneity we use data on debt rescheduled at the Paris Club and on bilateral aid flows from the OECD Development Co-operation Directorate. The information offered by the Paris Club show which countries restructured their debt and which creditor countries were affected in each case. Using bilateral data on aid flows and default events allows us to identify a differentiated default effect on affected and non-affected creditors.

Our findings indicate that foreign aid flows are not reduced after a default. This result holds not only for the aggregate amount of foreign aid received by the delinquent country but also for the amount granted by aggrieved creditor countries. On the contrary, our estimation results indicate a positive effect of a default on the aggregate amount of foreign aid received by the defaulting country. This finding reflects significantly increased aid flows given by non affected creditor countries. One intuitive explanation may be that governments of donor countries focus on foreign aid determinants other than the possibility to punish the default, e.g. the receiving country's needs. This might be especially important in times of a default since the economic situation of the debtor tends to be worse for the foreseeable future.

This paper is organized as follows. Section 2 gives a brief overview of the related literature concerning default costs and the linkage between aid and sovereign debt. In section 3 we take a closer look at the bilateral data on foreign aid flows and default events that are used in this paper. The econometric methodology is described in section 4 and section 5 presents the results of our empirical estimation. Section 6 concludes.

2. Related Literature

In this section we will review the existing literature dealing with default costs.[3] After that we will take a brief look at the literature that links foreign aid to sovereign debt.

Referring to the two categories of default costs, reputational costs and sanctions, four reasons for the repayment of international debt are typically mentioned in the literature. First, Alesina and Tabellini (1989) argue that delinquent countries may simply have their overseas assets seized by foreign creditors. This would be a direct sanction for countries that renege on their debt. However, the feasibility of this enforcement mechanism is limited, e.g. because of sovereign immunity. Second, Eaton and Gersovitz (1981) emphasize the importance of a borrower's repayment reputation when the country wants to issue further sovereign debt.[4] They suggest that a default impairs this reputation and leads to an exclusion from international capital markets. To the extend this embargo is permanent the defaulting country loses its ability to smooth consumption over time.[5]

The argument that the threat of capital market exclusion as a result of a bad reputation can effectively deter sovereign default is criticized for several reasons.[6] On the one hand, Kletzer (1994) mentions that a permanent exclusion from capital markets lacks commitment if both, creditors and donors, can benefit from interacting on capital markets after a default. On the other hand, Bulow and Rogoff (1989b) point out that a defaulting country may still be able to smooth consumption even without access to foreign borrowing, e.g. by drawing on accumulated buffer-stock savings. Both arguments indicate that there has to be at least a third type of default costs. Bulow and Rogoff (1989a) and Fernandez and Rosenthal (1990) mention reductions in international trade as consequences of defaults. Theoretically, reduction in trade could occur because creditor countries impose trade sanctions to discourage future defaults or because the defaulting country loses access to trade credit, which is needed to finance international trade.

[3]See Eaton and Fernandez (1995) for a detailed literature review on repayment incentives.

[4]For further work concerning reputation and sovereign debt see, e.g. Kletzer (1984) and Grossman and Van Huyck (1988).

[5]Eaton and Gersovitz (1981) implicitly assume that international debt is the only way to achieve consumption smoothing.

[6]Cole and Kehoe (1998) build a general model of reputation in which the government loses its trustworthiness and overall reputation in case of a default. A default therefore affects more than the ability to borrow again after a default. The model of Cole and Kehoe (1998) thereby can support large amounts of sovereign debt.

Finally, countries that renege on their debt may also lose the benefits of development assistance as the international community withdraws foreign aid. This fourth type of default costs has been recently discussed by Asiedu and Villamil (2002). The key assumption in their theoretical model is that a country that repudiates its foreign debt will lose access to FDI and aid.

Several empirical studies investigate the different types of default costs outlined above. Typically, they use bilateral data to distinguish between the reaction of countries directly affected by the default and of those countries that are not. This differentiation is highly important as the aggregate effect of a default may mask the punishment imposed by creditor countries. The necessary information is obtained from the Paris Club which provides data on the debtor countries that restructured their debt as well as information about the affected creditors.

Fuentes and Saravia (2010) use bilateral data on FDI flows and sovereign debt renegotiation to analyze weather a default leads to capital market exclusion in terms of a decline in FDI inflows. The data on FDI flows identifies the source as well as the recipient country. They find a significant decline of FDI inflows coming from the defaulter's creditor countries. FDI inflows from countries not affected by the default rise but the aggregate effect on FDI remains negative. Overall, FDI inflows of a country that renege on its debt fall by about 0.05 percentage points of its GDP. These results indicate that countries whose debt claims have not been settled impose a penalty on the defaulter in form of a reduction in FDI.

Without looking at the theoretical question why trade could be reduced in case of a default Rose (2005) and Martinez and Sandleris (2011) empirically investigate the relationship between international trade and sovereign default. Using bilateral data on trade and default events Rose (2005) analyzes trade between country pairs. His findings indicate that a default leads on average to an 8% decline in trade between the defaulting country and its creditors. This effect persists for about 15 years. Furthermore, Rose (2005) does not find strong evidence for trade diversion. Hence, trade reduction from creditor countries is not compensated by a rise in trade with other countries. A default therefore leads to an overall decline in trade for the delinquent country. Martinez and Sandleris (2011) even argue that trade reduction also occurs between the defaulting country and non-creditor countries.

Until now, no empirical analyses investigates if a default leads to a decline of foreign aid allocated to the delinquent country. Even though the empirical analysis of Asiedu et al. (2009) examines how foreign aid affects the relationship between FDI and country risk the assumption that a default is followed by a loss of foreign aid is not empirically studied. One first step to evaluate the relationship between the allocation of aid and sovereign defaults is made by Powell and Bird (2010). They analyze if a debt relief leads to an in- or decrease of aggregate foreign aid transferred to the corresponding country. In their empirical analysis Powell and Bird (2010) focus on countries in Sub Saharan Africa (SSA). They find a significant increase in aggregate aid transfers after a country received a debt relief.

At first glance, one could think of debt reliefs to be nothing else than defaults. Creditor countries might know that their debt claims will not be served and therefore decide for a voluntary debt relief. The findings of Powell and Bird (2010) would then indicate the absence of punishment in form of aid reduction after a default. However, this interpretation might be misleading. First, donor countries may not judge debt reliefs for SSA countries as a default but as a kind of aid for extremely poor and highly indebted countries. We therefore try to shift the focus to the relationship between foreign aid and *real* defaults by taking a look at a boarder set of countries and different default indicators. Second, Powell and Bird (2010) only analyze aggregate aid flows. Unilateral punishment by creditor countries might therefore remain undetected. To capture this we use bilateral data as it is common in the literature on defaults and trade or FDI. Our empirical approach is therefore related to Rose (2005) and Fuentes and Saravia (2010).

3. Data and Hypotheses

Empirical studies on the determinants of foreign aid allocation typically draw upon bilateral data from the OECD Development Co-operation Directorate. This comprehensive data base offers information on committed and actually disbursed aid flows for a large number of donors and recipients. Unfortunately, the raw data on official development assistance (*ODA*) is ill-suited for our analysis of the relationship between foreign aid and sovereign defaults. The reason is that our key explanatory variables, the default variables, affect standard indicators of foreign aid via an accounting relationship. To see this point, notice that our definition of sovereign defaults refers to the renegotiation of *official* external debt through the Paris Club. Each default event thus reflects either a postponement or an outright reduction of a country's debt service obligations owed to other sovereigns. The outcome of the renegotiation process, however, is also recorded in the OECD database as new aid payments from the affected creditors to the delinquent debtor country. This increases measured ODA although the debtor country does not receive additional financial support. The transaction enters either as a new ODA loan (subheading "rescheduled debt") in the case of a mere rescheduling or as a debt forgiveness grant. While this treatment might be sensible from an accounting point of view, it also introduces a spurious positive correlation between sovereign defaults and foreign aid.[7] As a consequence, the results from a regression of ODA on indicators of sovereign defaults would be biased against finding evidence for punishment. To address this issue we resort to the concept of "gross aid transfers" (GAT) proposed by Roodman (2011). His measure of foreign aid builds upon the official OECD statistics on actually disbursed aid but excludes all transactions that are directly related to debt renegotiations:

$$\text{GAT} = \text{ Gross ODA} - \text{debt forgiveness grants} - \text{rescheduled debt.}$$

Information on gross aid transfers is available on a bilateral basis, covering 34 different international donors and 190 recipients of foreign aid.[8] Even though the panel is unbalanced, data on some donor-recipient pairs cover the entire period from 1960-2009. Inspection of the data set further reveals that the distinction between GAT and ODA is economically important. Take US bilateral aid to the Dominican Republic as an example. After the latter country renegotiated its debt through the Paris Club in 2004 and 2005 US official development assistance in 2006 still added-up to 52.75 millions, measured in 2008 US \$. However, more than 17 % of this sum (9.02 mill. US \$) are due to the direct effects of debt forgiveness and rescheduling. Similar large discrepancies can be found for other years and country pairs. Measuring aid appropriately is thus clearly essential from the perspective of our study. In the following, we therefore use the logarithm of real GAT scaled by the recipient's population (Aid) as our dependent variable.

We follow Rose (2005), Fuentes and Saravia (2010) and Martinez and Sandleris (2011)

[7]The accounting rule introduces additional problems concerning the treatment of canceled loans that were originally meant for non-development purposes like military spending. See Roodman (2011) for an extensive discussion of this point.

[8]It is also possible to calculate a measure of *net* aid transfers that subtracts debt service on ODA loans from the GAT statistics. While the new statistic might be an even better approximation of the recipient's benefit from foreign aid (Roodman, 2011) it has the drawback of being partly determined by past aid disbursement. Since our focus is on current policy choices, we follow Dollar and Levin (2006) and opt for a measure of gross aid flows.

in using information from the Paris Club to construct different indicators of sovereign defaults. The Paris Club's website is the most comprehensive data source on sovereign defaults in terms of coverage and detail. It comprises more than 400 debt restructurings that took place between 1956 and 2011. For each restructuring deal, the dataset contains information on the amount of debt rescheduled and on the type of treatment which specifies its degree of concessionality. Most important for the purpose of this study, it lists not only the defaulting sovereign but also the affected creditor countries. This allows us to test two variants of the hypothesis that aid withdrawal is actually used as punishment for sovereign defaults.

A strong version of the punishment hypothesis states that international donors as a group sanction defaults by reducing foreign aid to delinquent sovereign debtors. The collective withdrawal of foreign aid thus represents an additional cost to the affected country that may influence its decision to default in the first place. Hence, foreign aid would serve as an enforcement mechanism as modeled by Asiedu and Villamil (2002) and Asiedu et al. (2009). We test this hypothesis by adding a default indicator (*Default*) to an otherwise standard set of foreign aid determinants. This variable takes the value 1 whenever an aid recipient restructured its debt through the Paris Club. The variable's coefficient should take a negative value according to the hypothesis. As another test we also include the size of the Paris Club deal (*Amount*) in some specifications. Assuming that larger defaults are viewed as particularly inexcusable and thus deserve even more punishment, we expect to find a negative coefficient on this variable as well.

The second, weaker version of the same hypothesis allows for heterogeneous responses of donor countries. In fact, it seems reasonable to assume that coordination among donors is too weak to ensure collective sanctioning. A reduction in aid disbursements might thus only be observed for those creditor countries to which the recipient defaulted. Whether foreign aid functions as an enforcement mechanism for international debt contracts then depends on the strength of this reaction and on the behavior of the remaining donors. Their response is theoretically ambiguous. On the one hand, limited coordination might still induce those donors to cut their aid flows as well, perhaps by a smaller amount. On the other hand, aid granted to defaulting countries might even increase out of altruistic motives. The reason is that sovereign defaults typically coincide with periods of economic hardship which renders the crisis-stricken countries more needy. In either case, allowing for a differentiated reaction of donor countries depending on their role in the debt restructuring is empirically important. We do this by including a bilateral default dummy (*Bilateral Default*) as an additional regressor. This second default dummy indicates whether an aid recipient defaulted on the debt owed to a specific donor in a given year. While we do not have any priors regarding the reaction of the remaining donors, the idea of punishment implies that aid flows from the defaulter's creditor countries should decline after a sovereign default.

Our analysis includes a large set of control variables that might influence the allocation of foreign aid. We follow Hoeffler and Outram (2011) in considering variables that measure the recipient's need and merit as well as indicators of strategic motives. The need of a specific recipient is approximated by its income per capita (*GDP pc*) and by the amount of aid it receives from other donors (*Other Aid pc*). Poorer countries are expected to receive relatively more aid while the relationship between aid from different donors could either be complementary or substitutive. The merit of aid recipients is captured by three different indicators. The first, the growth rate of the recipient's GDP per capita (*Growth*), serves as a proxy for beneficial economic policies and should thus be positively related to aid inflows. The two remaining variables are an indicator of human rights violations (*Human Rights*) and the polity2 index of democracy (*Democracy*). We expect that democracies which honor human rights (low value of *Human Rights*) attract relatively more aid compared to dictatorships with a history of human rights abuses. Strategic concerns of the donor countries are proxied by two variables: bilateral trade (*Trade*) and voting allegiance in the UN General Assembly (*UN Friend*). Donors are likely to favor countries that are either important trading partner or close political allies. We thus expect to find a positive relationship between both variables and foreign aid disbursements. Random effects specifications further contain an indicator of the donor's and recipient's colonial past (*Colony*) as another time-invariant measure of political allegiances. Finally, we also include the logarithm of the recipient's population (*Population*) as an additional regressor. This variable does not fit into any of the three categories mentioned so far. Rather, it is meant to capture the stylized fact that small countries tend to attract disproportionately large amounts of foreign aid in per capita terms. Appendix A contains further information on the construction of all included variables along with their data sources.

Due to limited data availability on the UN voting variable and on some other regressors our final sample comprises 1309 different donor-recipient pairs with annual data from 1970 to 2008. The reduction in the number of observations on aid flows mainly reflects our focus on the G7 donors: Canada, France, Germany, Italy, Japan, the United

Kingdom, and the United States. These countries accounted for roughly two-thirds of all bilateral aid disbursements throughout our sample period. Table 1 reports some descriptive statistics on our dependent and explanatory variables for this final sample. Notably, the fifth column of this table shows that the minimum value of our aid variable is negative. Negative gross aid transfers will occur if recipients return unspent, previously granted aid to the respective donor. With only 57 observations, these cases are quite rare. They mask, however, another important feature of the data as 5,306 of the 36,512 observations on gross aid transfers take the value zero. We address this issue in the next section.

≪ insert Table 1 here ≫

4. The Econometric Framework

The empirical analysis of bilateral foreign aid flows involves at least two key specification choices. The first choice concerns the appropriate use of the data's panel structure. Most earlier studies reduce the dimensionality of the data, which typically covers annual aid flows from multiple donors to a large number of recipients, by resorting to donor-specific estimations.[9] Alternatively, information from multiple donors could be pooled. Focusing on the average donor in this way drastically increases sample size but possibly neglects heterogeneity in individual donor behavior. These differences can be captured by dyad-specific fixed effects if they are limited to time-constant characteristics of the donor-recipient pair. Past colonial ties between countries or the USA's special relationship to Israel and other strategically important countries in the Middle East region are examples that fit into this category. We follow Berthélemy and Tichit (2004), Chong and Gradstein (2008) and Claessens et al. (2009) and use the latter approach. We do, however, check whether our findings are sensitive to this choice and also report results from donor-specific regressions.

The second important choice involves the specification of the dependent variables' data generating process. The chosen model should address the fact that bilateral aid flows – though generally nonnegative – are equal to zero for a substantial number of observations. Three different estimation strategies have been proposed in the literature that fulfill this requirement. Each of them has specific strengths and shortcomings:

[9] The seminal work of Alesina and Dollar (2000) offers one prominent example for this approach.

1. *Two part model.* Two part models differentiate between a participation (whether aid is supplied or not) and an amount decision (aid flows given that aid is positive). The model's most critical assumption states that both decisions are independent after controlling for observed explanatory variables. In this case, the parameters of interest can be estimated from separate binary and linear models. In our application, the two estimation equations are

$$\ln\left(Aid_{ijt}\right) = \theta_t + \boldsymbol{d_{ijt}\beta} + \boldsymbol{x_{ijt}\gamma} + c_{ij} + u_{ijt} \quad \text{and} \tag{1}$$

$$Aid^*_{ijt} = \boldsymbol{z_{ijt}\eta} + \zeta_{ij} + a_{ijt}; \quad Aid_{ijt} > 0\left[Aid^*_{ijt} > 0\right], \tag{2}$$

where index i and j refer, respectively, to the donor and recipient country and Aid_{ijt} is our indicator of bilateral aid flows in year t. Vector $\boldsymbol{d_{ijt}}$ contains our main variables of interest that describe the default status of countries i and j. Equation 1 further includes a vector of control variables $\boldsymbol{x_{ijt}}$ and a time varying constant θ_t. The participation decision also depends on the covariates in $\boldsymbol{z_{ijt}}$ which contains at least one variable that is not already included in $\boldsymbol{d_{ijt}}$ or $\boldsymbol{x_{ijt}}$.[10] Furthermore, both equations include unobserved country-pair specific effects (c_{ij} and ζ_{ij}) and an idiosyncratic error term (u_{ijt} and a_{ijt}). Most studies only report results for equation 1 which is estimated on the subsample of positive observations using standard panel techniques.

2. *Selection model.* Estimation of a selection model is appropriate if the conditional independence assumption of the two part model is not met. This would be the case if the unobserved effects or the idiosyncratic error terms of equations 1 and 2 were correlated. Estimates from the two part model would be inconsistent in either case. One approach that does consistently estimate the parameters of the amount equation in a panel context is Wooldridge's (1995) variant of Heckman's two-step estimator. Details on this approach, which rests on quite restrictive assumptions, can be found in Appendix B.

3. *Tobit model.* The Tobit model resorts to a latent variable specification to account for corner solutions of the dependent variable. In our application, the latent variable can be thought of as a donor's desired amount of aid for a particular recipient. If this amount is negative, no aid is distributed. Otherwise, y^*_{ijt} determines the amount actually disbursed. Again assuming that aid is lognormally distributed we

[10] This assumption is not essential for the two part model. The related selection model, however, might be only poorly identified without this assumption.

get the model

$$y^*_{ijt} = \theta_t + \boldsymbol{d_{ijt}}\boldsymbol{\beta} + \boldsymbol{x_{ijt}}\boldsymbol{\gamma} + c_{ij} + u_{ijt} \quad \text{with} \tag{3}$$

$$\ln\left(Aid_{ijt}\right) = y_{ijt} = \left\{ \begin{array}{ll} y^*_{ijt}, & \text{if } y^*_{ijt} \geq \min\left(y_{ijt}\right) \\ -, & \text{if } y^*_{ijt} < \min\left(y_{ijt}\right) \end{array} \right\} \quad .$$

The major drawback of the tobit model for panel data is that it only allows for a random effects specification of the unobserved effects c_{ij}. This requires the strong assumption that all observable covariates are uncorrelated with the c_{ij}.

Given their different limitations, we draw on all of the three approaches in our study. This procedure allows us to investigate whether our results are robust or just the reflection of a more or less arbitrary specification choice. However, following the majority of existing studies, we use the simple two part model as starting point for our analysis. A final problem common to all three approaches is that our regressors are not necessarily strictly exogenous. For some variables, even contemporaneous exogeneity seems questionable. Following Hoeffler and Outram (2011) we therefore include lagged instead of current realizations of most control variables in our regression. This procedure should at least mitigate concerns of endogeneity.[11] We do not lag the "UN Friend" and "Other Aid" variable which are clearly endogenous. Their coefficients should thus not be interpreted causally. Potential endogeneity of the default indicators as our main variables of interest is addressed separately in subsection 5.2.

5. Empirical Evidence on Punishment through Aid Withdrawal

5.1. Baseline Results

We begin our empirical analysis by estimating equation 1 on the subsample of observations with positive bilateral aid flows. The classic fixed effects estimator is used for this exercise, as it seems likely that unobserved characteristics of the donor-recipient relationship correlate with our variables of interest. To account for potentially heteroscedastic and autocorrelated error terms we further employ cluster-robust standard errors. Table 2 shows the results. Column (1) starts with the estimates from a baseline specification

[11] The fixed effects estimator, which is predominantly used in this paper, is still inconsistent in the presence of regressors that satisfy contemporaneous but not strict exogeneity. This inconsistency shrinks to zero at the rate $1/T$. Given that our sample period spans 39 years this problem should be negligible.

that does not include any indicator of sovereign debt crises. The results are generally in line with our priors and with previous findings in the literature. G7 donors allocate foreign aid towards countries that are relatively poor but fast growing. Herding also seems to be an important characteristic of donor behavior as the coefficient on the variable measuring donations from other countries is positive and highly significant. This effect, however, may also reflect that some recipients are considered needy in dimensions not well captured by our other control variables and thus receive more aid from all donors. Regarding our political variables, we find only mixed evidence in favor of the hypothesis that a recipient's merit is an important determinant of aid inflows. While human right abuses tend to be sanctioned, democracies are not rewarded with significant additional financial support. At the same time, the significantly positive coefficient on the trade variable indicates that strategic concerns affect the allocation of foreign aid. The finding of a positive relationship between voting behavior in the UN and foreign aid disbursements would further support this line of argument. The coefficient on the "UN friend" variable, however, has the correct sign but is not statistically significant. Finally, we also find evidence for a small country bias meaning that less populated countries receive relatively more aid in per capita terms.[12]

≪ insert Table 2 here ≫

The lower part of Table 2 contains results from the Robust Hausman test and different measures of the model's fit. The robust and the standard Hausman test share the same null hypothesis, namely that the regressors and the unobserved effects are uncorrelated. The random effects model is consistent under this assumption. It is, however, not necessarily fully efficient since both the unobserved effects and the error terms may fail to meet the usual i.i.d. assumption. A violation of this assumption, which is used for inference in the standard Hausman test, is likely in our application.[13] We therefore use the robust version of this test which is based on an artificial random effects regression that also includes the demeaned variables as additional regressors (Arellano, 1993). Under the null hypothesis, the coefficients on these transformed fixed effects regressors should be

[12]Robustness exercises show that this effect is still present when only recipients with a population above a certain minimum threshold (e.g. 1,000,000) are included. Hence, the effect at least partly reflects that large countries like China receive relatively less foreign aid. In accordance with the literature, we nevertheless stick to the term "small country bias" although it might be a misnomer.

[13]Large differences between clustered and normal standard errors obtained for the random effects model support this notion.

zero. Inference can be based on clustered standard errors which are robust to violations of the i.i.d. assumptions. The results from the test strongly reject the random effects model.[14] Confirming our priors, the fixed effects model is thus the preferred one. Turning next to the three different R^2 measures, the model's explanatory power seems modest at first glance. Other studies in the field like Berthélemy (2006) or Hoeffler and Outram (2011) routinely report R^2s of more than 50%. This difference, however, can be explained by the fact that our measures focus on the explanatory power of the observed variables and ignore the contribution of the fixed effects. Adding their contribution yields a comparable R^2 of 0.74.

The remaining three columns of Table 2 are devoted to our main research question. They report results from specifications that add our two different default indicators, either separately or jointly, to the standard set of control variables. Again, all regressions also include country-pair specific fixed effects. This specification is generally supported by the Robust Hausman test.

We start with a test of the strong version of the punishment hypothesis which states that donors sanction sovereign defaulters collectively. The absolute amount of aid received by these countries should thus decrease. Our results – displayed in column (2) – clearly reject this hypothesis. The coefficient on the common default dummy is positive and statistically significant at the 5 percent level. Furthermore, the effect is also significant in economic terms. Overall, donors increase aid disbursement to delinquent borrowers by 6.4 % on average. These results are at odds with Asiedu and Villamil's (2002) idea that foreign aid may function as an enforcement mechanism for sovereign debt. On the contrary, the documented surge in aid flows in the aftermath of debt restructurings even increases the attractiveness of sovereign defaults. An explanation for this finding might be that donors contemporaneously react to other objectives that dominate the strategic motive for punishment. Altruistic motives, e.g., may play a role as a sovereign default may be seen as an indicator for a persistent economic crises that renders the already poor country even more needy.

Another possibility is that only those countries that are directly affected by a default resort to sanctions. A common default dummy would fail to capture this behavior if it is counteracted by increased aid disbursements from the remaining donors. The specifications in column (3) and (4) test this weak variant of the punishment hypothesis by including a bilateral default dummy. Again, we do not find any evidence for sanctioning.

[14] Results from standard Hausman tests (not shown) point to the same conclusion.

The coefficient on the bilateral default dummy variable – when considered in isolation as in column (3) – is insignificant and positive. Thus, even directly affected donors fail to sanction their former debtor by reducing foreign aid. This conclusion is supported by the results in column (4). Here, the inclusion of both default variables explicitly allows for heterogenous responses from affected and not affected creditor countries. The finding of a large positive coefficient on the common default dummy confirms our previous notion that the increase in foreign aid documented in column (2) predominantly reflects additional aid disbursements from the latter group of donors. Their support increases by roughly 15 percent after a sovereign default. Less is known about the reaction of former creditors. The negative effect on the bilateral default dummy indicates that these donors tend to give less aid after a default than their peers. However, this effect is not statistically significant. Furthermore, the overall effect of a default on bilateral aid disbursements would still be positive for the countries that are involved in a renegotiation, even if the coefficient were significant.

The results presented so far strongly reject both the weak and the strong version of the punishment hypothesis. One possible explanation for this finding could be that our binary default indicators fail to differentiate between different default events. This would bias our estimates if reactions to a default differed depending, e.g., on some characteristics of the restructuring deal like the size of the haircut. Existing empirical work by Trebesch (2010), Cruces and Trebesch (2011) and Jorra (2011) support this idea. A heterogenous reaction in terms of sanctioning could also be justified theoretically. Grossman and Van Huyck (1988), e.g., differentiate between excusable and inexcusable defaults. In their model, only the latter events are punished. We try to capture this distinction by including the size of the Paris Club deal as an additional variable. The underlying hypothesis is that larger defaults are considered less excusable and are thus punished harder (Fuentes and Saravia, 2010). The first two columns of Table 3 contain the results. Again, these are not supportive for the sanctioning hypothesis. The additional variable enters with a coefficient that is correctly signed but insignificant. Moreover, none of the two coefficients on our binary default indicators is affected by the inclusion of the previously omitted variable. The insignificance of the "amount" variable does not merely reflect problems of multicollinearity. It is also present in specifications that include only one of the two other default variables (column 2).

≪ insert Table 3 here ≫

The last two columns of Table 3 investigate whether a default has harmful longer-term consequences for a recipient of foreign aid. In principal, defaults could have a lagged effect on aid disbursement as a result of a lengthy budgeting process which impairs its instantaneous reallocation. So far, evidence for such time lags has been documented for trade (Rose, 2005) and FDI related default costs (Fuentes and Saravia, 2010). To analyze this issue, we add two lagged default indicators to our baseline specification. These take the value one whenever a recipient defaulted at least once during a specified five year period ($t-1$ to $t-6$ and $t-6$ to $t-10$) on the debt owed to a particular donor. They are thus meant to capture the delayed response of former creditor countries that were directly affected by a specific default decision.

According to our results, sovereign defaults indeed have a delayed effect on the aid allocation decision. Its sign, however, is inconsistent with the idea of long-term punishment. On the contrary, directly affected donor countries only seem to lag their peers in increasing foreign aid disbursements to defaulting sovereigns. Interestingly, the coefficient on the dummy variable for a bilateral default during the most recent 5 year period is similar in absolute magnitude to the negative coefficient on the bilateral contemporaneous dummy which is now significant. A possible interpretation of this finding is that aggrieved creditors hesitate to reward a default by raising aid instantaneously. In the medium term, however, they also react to the increased need of the recipient. Column (4) of Table 3 further shows that this effect is restricted to the first five post-default years. Renegotiations that took place in any year between $t-6$ and $t-10$ do not trigger additional aid flows.

5.2. Addressing Corner Solutions and Endogeneity

This section investigates whether the sound rejection of the punishment hypothesis throughout all previously discussed regressions is related to our specification choice. We start with an analysis of the two alternative models for corner solution outcomes. As discussed in section 4, these are the tobit and selection model. Table 4 displays results from both models. For comparison purposes, column (1) further contains the findings from a random effects specification of the two part model. Such a comparison could be especially valuable for the tobit results which are also estimated from a specification that includes random effects.[15]

[15] As discussed above, this specification choice is necessary as no consistent fixed effect estimator exists for the tobit model.

≪ insert Table 4 here ≫

Column (2) displays the results for the tobit model. We report marginal effects of our default variables on foreign aid disbursement, conditional on observing positive aid flows. These are calculated as the average of the marginal effects obtained for the subsample of observations where aid was actually disbursed.[16] The results should thus be directly comparable to those reported for the two part and selection models. The finding of a positive and significant marginal effect of defaults on aid from not directly affected donors can therefore be directly interpreted as further evidence against the strong variant of the punishment hypothesis. Confirming our previous findings, the reaction from donors that participated in a renegotiation tends to be more muted, although the difference is not statistically significant. Surprisingly, this changes when bootstrapped clustered instead of the usual standard errors are considered.[17] However, these results, reported in column (3), still do not support any variant of the punishment hypothesis. Rather, they imply that even aggrieved creditors increase their support to defaulting sovereigns by nearly 10 percent on average.

We next turn to Wooldridge's (1995) selection model for panel data. Although it is not necessarily required for identification we include two additional variables in the participation equation to improve the precision of our estimates. Following Koch et al. (2009) we reason that joint religion affects the decision to supply aid to a particular recipient. At the same time, we assume that the additional variable can be excluded from the allocation equation. Borrowing from the related literature on the gravity model of international trade, we further include a common language dummy as another additional regressor in the participation equation (Helpman et al., 2008). The final column of Table 4 contains the results. Again, our main conclusions remain unaltered, although many terms containing the inverse Mills ratio turn out statistically significant (not shown). The effect of selection bias thus seems at least economically negligible. In this respect, our results complement previous findings in the literature, documented, e.g., by Berthélemy and Tichit (2004) and Berthélemy (2006). We therefore resort to the simple two part model for the remainder of this paper.

[16] Our calculation also takes the binary nature of the default variables into account. In both cases, the reported results thus measure the effect of a discrete change from 0 to 1.

[17] This unexpected finding of smaller clustered than normal standard errors is limited to the default variables. Standard errors for the remaining marginal effects rise markedly when switching from column (2) to column (3).

The second part of this section deals with another source of biases arising from endogeneity. Problems of this kind are indeed likely to occur in our application as the relationship between sovereign defaults and foreign aid flows is potentially bidirectional. At least two different explanations for a causal link between aid disbursement and the probability of debt crises come to mind. The first explanation is related to the classical source of simultaneity bias that plagues all studies on the consequences of sovereign defaults. It states that a reduction in foreign assistance – or, more generally, in economic activity – causes debt crises as it impairs both, a sovereign's ability and willingness to serve its debt. In accordance with the punishment hypothesis, this theory thus predicts a negative correlation between aid flows and sovereign defaults. Since our results show the opposite, the case for the empirical relevance of punishment is even weaker in light of this argument. The second, alternative explanation is more troublesome for our conclusions as it implies that our coefficient estimates are biased upwards. It draws on the large literature on the economic effects of foreign aid. Studies in this field repeatedly document that aid has negative unintended side effects. Djankov et al. (2008), e.g., find a significant negative effect of aid on the quality of political institutions. Other studies summarized in McGillivray et al. (2006) document that even the effect of foreign aid on growth can be negative in some circumstances. Both effects might in turn increase sovereign risk and introduce a positive correlation between foreign aid and defaults. A careful examination of the importance of endogeneity is thus necessary since it is a priori unclear which of the two effects dominates.

Unfortunately, controlling for endogeneity turns out rather difficult empirically. The reason is a lack of variables which predict sovereign defaults but do not influence foreign aid disbursements. Similar problems have plagued previous econometric studies of default costs. Typically, it remains unclear whether the finally chosen variables in an IV approach are suitable, weak or even invalid instruments. Reviewing these past attempts Borensztein and Panizza (2009) even conclude that convincing instruments might simply not exist. These limitations in mind, we resort to three variables that have been repeatedly used for this purpose. Following Rose (2005) and Martinez and Sandleris (2011) our instrumental variables are the budget deficit (in percent of GDP), the CPI inflation rate and the ratio of the current account surplus/deficit to GDP. The results from both two-stage least-squares (2SLS) and GMM estimation techniques are shown in Table 5.

≪ insert Table 5 here ≫

Encouragingly, the results from both specifications are quite similar and generally in line with our previous findings. All coefficients of our default dummies are positive but statistically insignificant. Hence, we still do not find any evidence for punishment through a reduction in foreign aid. Furthermore and despite our initial objections, none of the reported diagnostic tests indicates misspecification. All four specifications easily pass the Kleibergen and Paap's (2006) underidentification test.[18] Testing for the likely consequences of weak instruments following the procedure of Stock and Yogo (2005) further reveals only a moderate potential bias. Finally, Hansen's J statistic does not reject the hypothesis that our instruments are valid. Although this test might fail to detect all incidences of instrument invalidity, it seems very unlikely that controlling for any remaining bias would switch the sign of the default effect. In light of the robustness of our results we are thus confident that the hypothesis that sovereign defaulters are punished through a reduction in foreign aid is empirically irrelevant.

5.3. Further Robustness Exercises

All results previously discussed rest implicitly on three assumptions that are not necessarily met. So far, we have assumed that Paris Club renegotiation dates correctly identify sovereign defaults, that the relationship between aid disbursements and defaults has not been subject to structural change, and that this relationship is the same for all donor countries. This section investigates whether our results are robust to relaxing these assumptions.

A concern with the Paris Club data is that it does not distinguish between sovereign defaults and incidences of debt relief. Both events are recorded in the data set as they imply a change in the terms of the original debt contracts that favors debtors at the expense of creditors. However, from an economic perspective defaults and debt reliefs are clearly distinct. The first type of event is typically seen and modeled as a deliberate policy choice of a debtor government. By contrast, the cancelation of loans through a debt relief is often initiated by former creditor countries. Their active role in the restructuring process does not square with the concept of punishment as envisioned by Asiedu and Villamil (2002) and Asiedu et al. (2009). Including data on incidences of

[18]This is not true for our standard specification that includes both default indicators simultaneously. The specification is underidentified since both deficit variables are highly correlated while the predictive power of the inflation rate is very limited. This leaves us with only one effective instrument for two endogenous variables. We therefore do not report these results although they do not contradict our general conclusions.

debt relief might thus bias the results on the effects of sovereign defaults. This may even lead to a false rejection of the punishment hypothesis. The problem will be especially severe if donor countries view a debt relief as a complement rather than a substitute to foreign aid (Powell and Bird, 2010).

We attempt to address this issue by employing Paris Club information on the "type of treatment" to identify those renegotiations that might be better described as debt relief. Restructurings that took place under the Heavily Indebted Poor Countries (HIPC) initiative constitute a first candidate group. A broader definition of debt relief also includes other highly concessional Paris Club deals that were negotiated under "Cologne" or "Naples" terms.[19] A final categorization treats every restructuring of countries that are eligible for the HIPC initiative as debt relief. For each of these three definitions we reestimate the amount equation 1 excluding all incidences of debt relief. Table 6 contains the results which are in line with our previous findings. Sovereign defaulters attract significantly more, not less, aid in per capita terms according to the results that rest on either of the two broader default definitions (columns (1) to (4)). As before, this mainly reflects increased aid disbursement from countries that were not affected by the default. This effect vanishes in the specifications (5) and (6) which exclude all HIPC observations from the sample. Here, the reactions of both donor groups are statistically indistinguishable from zero. Still, we do not find any evidence for negative effects of sovereign defaults on aid inflows. Hence, both variants of the punishment hypothesis are rejected across all specifications and default definitions.

≪ insert Table 6 here ≫

We next analyze whether the impact of sovereign defaults on foreign aid flows has changed over time. In principle, such structural change seems plausible given that our sample period covers four decades and includes, e.g., the end of the Cold War. This event in particular might have affected the costs and benefits of punishments after a sovereign default. An aid withdrawal probably has never been a realistic option during the Cold War period, especially not when the allegiance of a strategically important country was at stake. Following this line of argument we would thus expect to find a more negative effect of defaults on aid starting in the 1990s. However, since previous research has

[19]This definition includes renegotiations under "Toronto" and "London" terms as well as those under "Lyon" terms which were, respectively, the predecessors of "Cologne" and "Naples" treatments.

documented an increased importance of altruistic motives of aid disbursements for the same period (Claessens et al., 2009), the opposite reaction also seems possible.

In light of the theoretical arguments, we tackle the issue of structural change by reestimating the simple log-linear model of positive aid disbursements separately for each decade. Table 7 shows the results. These are generally quite similar to each other and to the results that have been obtained for the complete sample period. This might either reflect that the two arguments for structural change are empirically irrelevant or that the opposing effects cancel each other out. The most noticeable change concerns the coefficient on the common default dummy which is now insignificant in all specifications. For the 1970s and 1980s this predominantly reflects the increase in standard errors brought about by the reduced sample size. In addition to this, the point estimates also decrease in the last two decades. Here, the negative coefficient of the bilateral default dummy is even larger in absolute size indicating that aid from directly affected donors might decrease after a default. This effect, however, is economically negligible and far from being statistically significant. The results thus still do not provide any evidence in favor of the punishment hypotheses.

≪ insert Table 7 here ≫

In a final step we investigate the importance of donor-specific heterogeneity that will result if individual donors are differently inclined to use foreign aid as a disciplining device. To detect these deviations from the average behavior we resort to donor-specific estimations of the amount equation 1. The upper part of Table 8 contains the regression output for a specification that includes both default dummies. At first glance, the deviations from our earlier results seem substantial as the coefficients on our two variables of interest repeatedly change signs from specification (1) to (7). However, nearly all of these differences can be attributed to a differentiated reaction to defaults which do not affect the respective donor country. By contrast, the effect of a bilateral default, measured by the sum of the two default coefficients, is nearly identical for all donors and close to zero. This again contradicts the idea that aggrieved creditor countries punish sovereign defaults by withdrawing foreign aid.

≪ insert Table 8 here ≫

One possible explanation for the puzzling variation in the donors' reaction to a recipient's default on debt owed to a third country is that these effects might be only poorly identified. This conjecture is supported by the high degree of multicollinearity between the two default indicators. In most samples, both variables take identical values for roughly 98 per cent of the observations. We therefore rerun the donor-specific regressions, each time including only one of the two dummy variables. The lower part of Table 8 shows the results for the specifications that add only the common default dummy to the standard set of control variables.[20] According to these estimates, no single donor country punishes sovereign defaults by reducing aid disbursements significantly. Japan even increases its development assistance to crisis stricken countries. Recipient countries thus do not receive less foreign aid after a default implying that aid can not work as an enforcement mechanism for sovereign debt. Furthermore, a F-test fails to reject the hypothesis that all default coefficients are jointly equal to zero, justifying our earlier focus on the pooled data set.

6. Conclusion

Looking at the long history of sovereign debt shows that defaults occur rather rarely. Countries typically pay their debt even if no international bankruptcy laws force them to do so. In the literature, this is explained by the existence of several kinds of default costs, e.g. exclusion from capital markets or trade sanctions. We focus on one specific reason for debt repayment, a possible decline in foreign aid allocated to the delinquent country in the aftermath of a default. The withdrawal of foreign aid would make a default more costly for the concerned country and hence function as an enforcement mechanism for sovereign debt. This type of default costs is theoretically analyzed by Asiedu and Villamil (2002). Based on the assumption that a country loses access to foreign aid and FDI in case of a default their model shows a decline in default risk for a country that receives positive aid inflows. However, to the best of our knowledge, we are the first who empirically investigate the validity of the underlying assumption, namely that a default leads to a reduction in foreign aid directed to the defaulting country.

We use bilateral data on foreign aid flows and debt renegotiations that identify the source and recipient countries of aid as well as the debtor and creditor countries involved in

[20] Similar results can be obtained by including only the bilateral default dummy. These results are available from the authors upon request.

the debt restructuring process. We can therefore differentiate between the reaction of creditor countries directly affected by the default and of countries that are not affected. Investigating only the aggregate effect of default events on aid could mask a punishment imposed by the the first group of donors.

Our results indicate that a default does not lead to a loss of foreign aid received by defaulting countries. On the contrary, it even raises the aggregate amount of foreign aid directed to these countries by about 6.4% on average. This effect is mainly driven by the reaction of those donor countries that are not directly affected by the default. Foreign aid given by these countries rises significantly by about 15% on average. Nevertheless, creditor countries that suffer directly from the default also tend to give more aid and not less after the default. This increase is statistically insignificant and much smaller than the increase coming from the remaining donors. However, it is far from being significantly negative. These findings contradict the hypotheses of Asiedu and Villamil (2002) who assume that creditor countries punish a default by reducing the amount of foreign aid given to the defaulting country. Our findings are robust to different empirical model specifications and several robustness checks. Overall, foreign aid therefore seems not to work as an enforcement mechanism for sovereign debt repayment.

Our findings raise questions regarding the interpretation of the results of Asiedu et al. (2009). Their study indicates that foreign aid mitigates the adverse effect of country risk on FDI. Our empirical results show that this effect is not caused by the threat of losing access to foreign aid in case of a default. Providing another theoretical explanation for this effect is surely an important area for future research.

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Table 1: Summary Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Aid	36512	9.310	113.853	-11.826	15381.240
Default	51051	0.053	0.223	0.000	1.000
Bilateral Default	51051	0.041	0.198	0.000	1.000
Amount	51051	0.006	0.048	0.000	1.619
ln GDP pc	39725	8.067	1.172	4.767	11.722
ln Other Aid pc	36291	3.120	1.544	-8.504	9.680
Growth	39200	0.038	0.083	-1.101	0.827
Human Rights	29918	2.551	1.125	1.000	5.000
Democracy	33236	-0.989	6.977	-10.000	10.000
ln Trade	39737	-5.147	2.481	-16.784	1.687
UN Friend	38278	0.376	0.174	0.000	0.940
ln Population	44772	8.219	2.062	2.485	14.091
Colony	50232	0.099	0.298	0.000	1.000

Table 2: Aid and Sovereign Defaults: Baseline Results

	(1)	(2)	(3)	(4)
Dependent variable: Gross Aid per capita				
Default		0.064**		0.147**
		(2.37)		(2.42)
Bilateral Default			0.037	-0.106
			(1.29)	(-1.60)
ln GDP pc (t-1)	-0.188*	-0.190*	-0.189*	-0.188*
	(-1.71)	(-1.73)	(-1.72)	(-1.71)
ln Other Aid pc	0.455***	0.451***	0.453***	0.451***
	(10.95)	(10.83)	(10.89)	(10.84)
Growth (t-1)	0.303**	0.307**	0.305**	0.308**
	(2.47)	(2.50)	(2.48)	(2.51)
Human Rights (t-1)	-0.066***	-0.066***	-0.066***	-0.066***
	(-3.02)	(-3.01)	(-3.02)	(-3.01)
Democracy (t-1)	0.003	0.003	0.003	0.003
	(0.72)	(0.71)	(0.71)	(0.72)
ln Trade (t-1)	0.125***	0.126***	0.126***	0.126***
	(4.06)	(4.10)	(4.08)	(4.10)
UN Friend	0.279	0.275	0.277	0.274
	(1.53)	(1.50)	(1.52)	(1.50)
ln Population	-0.615**	-0.633**	-0.625**	-0.629**
	(-2.24)	(-2.30)	(-2.27)	(-2.29)
Constant	5.837**	6.016**	5.936**	5.968**
	(2.06)	(2.13)	(2.10)	(2.11)
Dyad Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
Robust Hausman[a]	182.542***	192.956***	203.574***	204.606***
N	22086	22086	22086	22086
R^2 overall	0.158	0.158	0.158	0.158
R^2 within	0.136	0.136	0.136	0.136
R^2 between	0.143	0.143	0.143	0.143

Note: [a] The robust Hausman statistic is distributed as $\chi^2(N)$ where N denotes the number of explanatory variables.

Table 3: Aid and Sovereign Defaults: Default Size & Reputation Effects

	(1)	(2)	(3)	(4)
Dependent variable: Gross Aid per capita				
Default	0.159**	0.081***	0.150**	0.151**
	(2.55)	(2.58)	(2.47)	(2.48)
Bilateral Default	-0.102		-0.122*	-0.119*
	(-1.55)		(-1.88)	(-1.82)
Amount	-0.147	-0.162		
	(-1.07)	(-1.18)		
Default between t-1 and t-5			0.108***	0.100***
			(2.71)	(2.64)
Default between t-6 and t-10				0.041
				(0.99)
Controls	Yes	Yes	Yes	Yes
Dyad Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
Robust Hausman[a]	212.487***	200.918***	201.234***	200.839***
N	22086	22086	22086	22086
R^2 overall	0.158	0.158	0.157	0.157
R^2 within	0.136	0.136	0.137	0.137
R^2 between	0.143	0.143	0.142	0.142

Notes: [a] The robust Hausman statistic is distributed as $\chi^2(N)$ where N denotes the number of explanatory variables. Additional control variables included, but not reported.

Table 4: Aid and Sovereign Defaults: Tobit and Selection Models

	(1) *Linear, RE*	(2) *Tobit, RE*	(3) *Tobit, RE*	(4) *Selection*
Dependent variable: Gross Aid per capita				
Default	0.142**	0.241***	0.241***	0.157**
	(2.35)	(2.70)	(2.79)	(2.09)
Bilateral Default	-0.097	-0.143	-0.143*	-0.151*
	(-1.48)	(-1.46)	(-1.68)	(-1.86)
Controls	Yes	Yes	Yes	Yes
Dyad Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
Standard errors	Clustered	Standard	Clustered (Boot)	Clustered (Boot)
N	22086	24051	24051	18285

Notes: Additional control variables included, but not reported. Selection equation includes *Joint Religion* and *Common Language* as additional variables.

Table 5: Aid and Sovereign Defaults: Endogeneity

	(1) *2SLS*	(2) *GMM*	(3) *2SLS*	(4) *GMM*
Default	0.318	0.385		
	(0.48)	(0.59)		
Bilateral Default			0.348	0.451
			(0.44)	(0.58)
Controls	Yes	Yes	Yes	Yes
Dyad Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
N	5639	5639	5639	5639
Underid. Test	39.15***	39.15***	39.01***	39.01***
Weak Id. Test	10.59$^+$	10.59$^+$	9.90$^+$	9.90$^+$
Hansen J statistic	3.06	3.06	3.07	3.07

Notes: $^+$ denotes maximum bias due to weak instruments $\leq$ 10% of the bias of OLS according to critical values calculated by Stock and Yogo (2005). *Budget Deficit*, *Current Account* and *Inflation* are used as instruments. Additional control variables included, but not reported.

Table 6: Aid and Sovereign Defaults: Different Default Definitions

	No HIPC Terms		*No HIPC, Cologne or Naples Terms*		*No HIPC Countries*	
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable: Gross Aid per capita						
Default	0.066**	0.144**	0.071**	0.123*	0.038	0.108
	(2.37)	(2.23)	(2.41)	(1.79)	(0.93)	(1.14)
Bilateral Default		-0.099		-0.065		-0.082
		(-1.41)		(-0.88)		(-0.84)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Dyad Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	21933	21933	21679	21679	15489	15489
R^2 overall	0.160	0.159	0.162	0.162	0.169	0.169
R^2 within	0.136	0.136	0.136	0.136	0.131	0.131
R^2 between	0.145	0.145	0.147	0.147	0.156	0.156

Note: Additional control variables included, but not reported.

Table 7: Aid and Sovereign Defaults: Different Decades

	(1) *70s*	(2) *80s*	(3) *90s*	(4) *00s*
Dependent variable: Gross Aid per capita				
Default	0.120	0.139	0.034	0.002
	(0.17)	(1.19)	(0.54)	(0.03)
Bilateral Default	-0.090	-0.089	-0.038	-0.020
	(-0.13)	(-0.72)	(-0.52)	(-0.24)
Controls	Yes	Yes	Yes	Yes
Dyad Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
N	1211	5922	7741	7212
R^2 overall	0.078	0.191	0.117	0.045
R^2 within	0.028	0.077	0.133	0.040
R^2 between	0.088	0.176	0.099	0.029

Note: Additional control variables included, but not reported.

Table 8: Aid and Sovereign Defaults: Donor Specific Estimates

	(1) *CAN*	(2) *DEU*	(3) *FRA*	(4) *GBR*	(5) *ITA*	(6) *JPN*	(7) *USA*
Dependent variable: Gross Aid per capita							
a) Both default dummies							
Default	0.074	-0.104*	0.251***	-0.056	0.515***	0.276**	-0.042
	(1.03)	(-1.74)	(2.75)	(-0.33)	(3.46)	(1.99)	(-0.41)
Bilateral Default	-0.104	0.149**	-0.264**	0.148	-0.509***	-0.208	0.125
	(-1.16)	(2.09)	(-2.60)	(0.86)	(-3.04)	(-1.30)	(1.07)
b) Only unilateral default dummy							
Default	0.016	0.020	-0.001	0.065	0.088	0.131*	0.058
	(0.35)	(0.43)	(-0.03)	(0.93)	(1.00)	(1.81)	(0.96)
Specifications a) and b):							
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dyad Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	3019	3442	3313	3025	2923	3351	3013

Note: Additional control variables included, but not reported.

Appendix A. Data Sources and Definitions

Name	Source	Definition
Dependent variable		
Aid	Roodman (2011) and Heston et al. (2011)	Gross aid transfers (GAT) over recipient's population with GAT = Gross ODA − debt forgiveness grants − rescheduled debt.
Default variables		
Default	Rose (2005) and Paris Club (2011)	Default indicator. 1 whenever an aid recipient restructured its debt through the Paris Club.
Bilateral Default	Rose (2005) and Paris Club (2011)	Bilateral default indicator. 1 whenever an aid recipient defaulted on the debt owed to a specific donor.
Amount	Rose (2005), Paris Club (2011) and World Bank (2011)	Amount of rescheduled debt over recipient's GDP (both variables in current US$).

Appendix A. - continued

Name	Source	Definition
Control variables		
ln GDP pc	Heston et al. (2011)	Natural logarithm of the recipient's PPP converted GDP per capita.
ln Other Aid pc	Roodman (2011) and Heston et al. (2011)	Natural logarithm of Aid (see def. above) disbursed by other G7 donors.
Growth	Heston et al. (2011)	Growth rate of the recipient's GDP per capita.
Human Rights	Cornett et al. (2011)	Index of human rights violations based on US State Department human rights reports. Ranging from 1 -5 with higher values indicating more human insecurity.
Democracy	Polity IV (2009)	Policy score ranging from - 10 (strongly autocratic) to +10 (strongly democratic)
ln Trade	IMF (2011a) and World Bank (2011)	Natural logarithm of bilateral trade measured in percent of the donor's GDP.
UN Friend	Dreher and Sturm (2012)	Voting inline with donor in the UN General Assembly. Definition accoding to Kegley and Hook (1991)
ln Population	Heston et al. (2011)	Natural logarithm of the recipient's population.
Colony	CEPII (2011)	Dummy for common colonial past. 1 for pairs that were ever in a colonial relationship.
Additional variables for IV and selection models		
Joint Religion	Helpman et al. (2008)	Index for common religion. Higher values indicate more similar country pairs.
Common Language	CEPII (2011)	Dummy for common language. 1 if donor and recipient share the same official language
Budget Deficit	World Bank (2011)	Recipient's budget deficit (cash) in percent of GDP
Current Account	World Bank (2011)	Recipient's current account balance in percent of GDP
Inflation	World Bank (2011)	Recipient's CPI inflation rate

Appendix B. A Selection Model for Panel Data

The starting point for Wooldridge's (1995) selection model for panel data are equations 2 and 1, repeated here for convenience:

$$\ln\left(Aid_{ijt}\right) = \theta_t + \boldsymbol{d_{ijt}\beta} + \boldsymbol{x_{ijt}\gamma} + c_{ij} + u_{ijt} \quad \text{and} \tag{1}$$

$$Aid^*_{ijt} = \boldsymbol{z_{ijt}\eta} + \zeta_{ij} + a_{ijt}; \quad Aid_{ijt} > 0\left[Aid^*_{ijt} > 0\right]. \tag{2}$$

As in every fixed effects model, ζ_{ij} might be correlated with $\boldsymbol{z_{ijt}}$. Furthermore, a_{ijt} is independent of $\boldsymbol{z_{ijt}}$ with $\mathrm{E}(a_{ijt}) = 0$ and (ζ_{ij}, a_{ijt}) is jointly normally distributed. Aid flows are positive ($Aid_{ijt} > 0$) and $\ln\left(Aid_{ijt}\right)$ is defined if $Aid^*_{ijt} > 0$. The correlation between the unobservables in the participation ($\zeta_{ij} + a_{ijt}$) and allocation equation ($c_{ij} + u_{ijt}$) then introduces the selection problem. Wooldridge's (1995) solution for this problem rests on four assumptions:

1. The correlation between ζ_{ij} and $\boldsymbol{z_{ijt}}$ can be described by the equation

   $$\zeta_{ij} = \tau_0 + \boldsymbol{\bar{z}_{ij}\tau_1} + c_{ij},$$

   where $\boldsymbol{\bar{z}_{ij}}$ denotes the time average of $\boldsymbol{z_{ijt}}$.[21] Equation 2 therefore simplifies to

   $$Aid^*_{ijt} = \tau_0 + \boldsymbol{\bar{z}_{ij}\tau_1} + \boldsymbol{z_{ijt}\eta} + c_{ij} + a_{ijt}. \tag{4}$$

2. The new reduced form probit model has a random effects representation, i.e. $\nu_{ijt} = a_{ijt} + c_{ij}$ are independent of $\boldsymbol{z_{ij}} = (z_{ij1}, \ldots, z_{ijT})$. Furthermore, $\nu_{ijt} \sim \mathrm{No}(0, \sigma^2_t)$.

3. The two error terms of the participation (ν_{ijt}) and amount equation (u_{ijt}) are jointly normal distributed:

   $\mathrm{E}(u_{ijt}|\boldsymbol{z_{ij}}, \nu_{ijt}) = \mathrm{E}(u_{ijt}|\nu_{ijt}) = \rho_t\nu_{ijt}$.

4. The conditional expectation of c_{ij} is given by

   $\mathrm{E}(c_{ij}|\boldsymbol{z_{ij}}, \nu_{ijt}) = \boldsymbol{\bar{z}_{ij}\psi} + \phi_t\nu_{ijt}$.

[21] Wooldridge (1995) actually proposes to use all leads and lags of the explanatory variables in this equation. Replacing the non-contemporary values of these variables with their time averages has been suggested by Dustmann and Rochina-Barrachina (2007) and Wooldridge (2010).

These assumptions imply

$$\mathrm{E}\left[\ln\left(Aid_{ijt}\right)|\boldsymbol{z_{ij}}\right] = \theta_t + \boldsymbol{d_{ijt}\beta} + \boldsymbol{x_{ijt}\gamma} + \boldsymbol{\bar{z}_{ij}\psi} + \kappa_t\nu_{ijt}$$

with $\kappa_t = \phi_t + \rho_t$. Conditioning on observations with positive aid flows we then get

$$\mathrm{E}\left[\ln\left(Aid_{ijt}\right)|\boldsymbol{z_{ij}}, Aid_{ijt} > 0\right] = \theta_t + \boldsymbol{d_{ijt}\beta} + \boldsymbol{x_{ijt}\gamma} + \boldsymbol{\bar{z}_{ij}\psi} + \kappa_t\lambda\left(H_{ijt}\right) \tag{5}$$

where $H_{ijt} = \tau_0 + \boldsymbol{\bar{z}_{ij}\tau_1} + \boldsymbol{z_{ijt}\eta}$ denotes the index value from the selection equation and $\lambda(\cdot)$=$\phi(\cdot)/\Phi(\cdot)$ is the inverse Mills ratio.

We then estimate Equation 5 using a two step procedure:

1. Estimate Equation 4 using a pooled probit model and calculate the estimated values $\lambda\left(\hat{H}_{ijt}\right)$.
2. Estimate Equation 5 with pooled OLS using the estimates for $\lambda(\cdot)$ obtained in step 1. These are interacted with time dummies to account for the fact that their influence is not restricted to be constant across time.

As suggested by Wooldridge (2010), bootstrapped standard errors are used to account for cluster specific autocorrelation and the first sage estimation of $\lambda(\cdot)$.