

Dodging Adverse Selection: How Donor Type and Governance Condition Aid's Effects on School Enrollment

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Abstract

We employ AidData to test the effects of primary-education aid on school enrollment. We argue that the problem of adverse selection complicates both the allocation and the effectiveness of aid. We hypothesize that bilateral donors ought to have greater freedom to condition aid on recipient governance quality than multilateral donors, which are often bound by institutional rules to provide aid more impartially. Compared to their multilateral counterparts, bilateral donors may have advantages in overcoming adverse selection, resulting in bilateral aid's boosting enrollments to a greater degree. AidData's extensive coverage of multilateral aid enables this analysis for up to 100 low- and low-middle-income countries from 1995 to 2008. Latent growth regression analysis suggests that, compared to multilateral donors, bilateral donors indeed condition their primary education aid on recipient control of corruption and that bilateral aid is significantly related to improved enrollments.

Keywords

Less-developed countries; foreign aid; adverse selection; enrollment rates; education; latent growth

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“Education is development. It creates choices and opportunities for people, reduces the twin burdens of poverty and disease, and gives a stronger voice in society. For nations it creates a dynamic workforce and well-informed citizens able to compete and cooperate globally – opening doors to economic and social prosperity.”

-Millennium Development Goals #2

1. INTRODUCTION

Grinding poverty plagues Eritreans, who eke out less than one dollar per day in per capita income and thus rank seventh on the list of poorest peoples in the world (World Bank, 2010). Apparently noticing Eritrea’s plight and seeking to boost its scarce human capital, donors sent roughly \$220 million in foreign aid for improving Eritrean education between 1998 and 2008 (Findley *et al.*, 2009). By the budget standards of advanced industrial economies the amount appears trivial, but at \$20 million per year the aid represented more than 75 percent of total Eritrean government expenditures on education (World Bank, 2010). Effectively, foreign aid *is* the Eritrean education budget.

But an odd pattern has emerged in the beleaguered country: each year from 2005 to 2009 the net primary school enrollment rate – the proportion of school-aged children actually signed up for school – has dropped. It plummeted from 50 percent in 2005 to 39 percent in 2009 (World Bank, 2010). Many factors – including border disputes, regional war, political repression, flagging labor opportunities, volatile commodities markets, and climate strain, among others – likely contributed to this trend. But whatever donors intended with their education aid to Eritrea, it does not appear to have enabled more children to attend school.

Cases like Eritrea raise the possibility that education aid may prove ineffective more generally. And they provoke a more nuanced research question targeting the specific conditions under which aid may make a meaningful difference. Indeed, in this article we argue that the problem of adverse selection – the recipients most likely to seek and receive aid may be the least likely to use it effectively – complicates both aid allocation and aid effectiveness. Specifically, we contend that, in order to overcome adverse selection, donors must allocate aid strategically. And these selection effects in allocation will later condition aid effectiveness. We argue that, compared to bilateral donors, institutional rules and practices more tightly constrain multilateral donors, where broad coalitions of developing countries have seats and voting shares on development banks' executive boards and can collude to demand financing with few strings attached. Thus, we expect bilateral donors to be more discriminating about the quality of governance among recipients and thus to act more strategically when allocating aid for primary education. These allocation strategies, then, should then influence the effectiveness of the aid in boosting primary-school enrollment rates.

We address this research question and test the argument empirically by employing AidData (Tierney *et al.*, 2011), which, as other articles in this special issue have noted, significantly expands the coverage of aid information. Largely through adding money from multilateral banks and other donors that do not report to the OECD's Creditor Reporting System (CRS), AidData nearly doubles the amount of development finance tracked by a single source, from \$2.9 to \$4.9 trillion. Given the amount of activity by multilaterals in the social sectors, particularly since the mid-1990s (see Lyne *et al.*, 2009), these new data should prove especially useful for understanding the relationship between education aid and education outcomes. Indeed, omission of these data – covering more than forty percent of all development finance –

from prior work may have biased previous results. For our empirical tests we apply latent growth (hierarchical linear modeling) estimation, which, by employing both random intercepts and random coefficients, is a more conservative technique for analyzing pooled time series data.

To date related studies have largely confined themselves to assessing education aid's overall impact on economic growth, saying little about whether or not education aid actually leads directly to its more proximate anticipated outcomes. More recently, two studies have suggested that specifically-targeted education aid may have a positive influence on the key outcome of enrollment rates ([Dreher *et al.*, 2008] and [Michaelowa and Weber, 2007]). We build on these studies by using AidData to nearly double the amount of development finance considered, isolating the effects of aid to primary schools, assessing bilateral and multilateral aid independently, and, critically, modeling the effects of adverse selection – measured as recipient corruption and autocracy – on the allocation and effectiveness of aid from bilateral vs. multilateral donors. In what follows we introduce the substantive issue area of education aid, develop expectations for how education aid ought to affect enrollment rates conditional on donor type and governance, describe the data and estimation methods, present results, and draw conclusions.

2. EDUCATION AID AND SCHOOL ENROLLMENT

The bulk of the aid effectiveness literature has focused on broad questions of aggregate aid and its relationship to economic growth ([Burnside and Dollar, 2000], [Easterly, 2006], [Easterly, Levine and Roodman, 2004] and [Rajan and Subramanian, 2008]). Results have been mixed, but the center of gravity has gathered around findings that suggest no relationship

between aid and growth ([Easterly, Levine and Roodman, 2004] and [Rajan and Subramanian, 2008]).

But economic growth is notoriously hard to predict, so a closer look at the effect of sector-specific aid on sector-specific outcomes may help unravel the mystery of aid's impacts. It should enable us to better identify causal chains and to reduce the statistical noise caused by intervening variables (Michaelowa and Weber, 2007). By understanding the specific effects of sector-focused aid on discrete sectoral outcomes we can learn with greater precision whether aid is accomplishing its intended purposes. Education provides a particularly important policy domain in which to explore the more direct effects of specifically targeted aid on corresponding outcomes.

The debate over the effectiveness of education aid is relatively recent, and we categorize the limited literature into two broad approaches: (1) the effects on general outcomes such as economic growth and (2) the impact on educational outcomes specifically. Examinations of economic outcomes most often employ increases in gross domestic product (GDP) or GDP per capita as broad measures of effectiveness, suggesting that education aid's improvement of human capital should stimulate economic growth ([Asiedu and Nandwa, 2007] and [Pritchett, 2001]). These studies seldom uncover a positive correlation between education aid and growth, reflecting the general finding that aid has little discernible impact on expansion of the overall economy.

We argue, however, that sector-specific outcomes provide more accurate indicators for aid effectiveness. Prominently, the 2000 Millennium Development Goals called for universal primary education by 2015 (UN, 2010). This suggests inquiry into a direct outcome of education aid: is primary education aid increasing primary school enrollment? Recently, analysts have

moved in this direction, assessing education aid's impact on specific measurable outcomes of education. These studies use enrollment rates as the dependent variable, both because enrollments relate to the MDGs and because they boast better data availability. Initial studies by Michaelowa and Weber (2007, 2008) and Dreher *et al.* (2008) found positive though rather modest relationships between education aid and education outcomes, suggesting that aid may indeed boost school enrollments over time.

However, in these studies the statistically significant relationships were less significant substantively, and data availability and quality problems make the findings somewhat tentative. They particularly beg the question of whether the findings generalize to multilateral aid, which is overwhelmingly excluded from the OECD-CRS data. Both sets of authors acknowledge the flaws of the CRS database and its potential effect on results. They also discuss the poor coverage and reporting of primary enrollment data. Moreover, both Dreher *et al.* (2008) and Michaelowa and Weber (2007) evaluated the impact of *all* education aid on primary school enrollment – they did not isolate the effects of primary school aid on primary enrollment. Dreher *et al.* recommend that future research focus on the outcomes of primary-education-specific aid, promoting the follow-up we undertake here.

Admittedly, enrollment rates are not the only focus of education aid, nor are they the sole gauge of educational success. A wide literature clearly indicates that universal education of a reasonable *quality* is the lynchpin for success in educational development (Bennell, 2002). Much research has gone on to explore the quality of third-world education and to suggest factors that may influence educational quality and effectiveness ([Banerjee *et al.*, 2007], [Behrman, 1996], [Case and Deaton, 1999] and [Scheerens, 2001]). Indeed, intuition coupled with much empirical evidence suggests that focusing solely on enrollment rates will tell an incomplete story of

education outcomes. However, while better educational quality measures would be the preferred outcome to use for our estimation, sufficient cross-national data on standardized education outcomes is not widely available ([Barro and Lee, 2001] and [Dreher *et al.*, 2008]). Considering these limitations, we focus on enrollment rates as our education outcome measure. Since all of the valuable benefits of education require children to go to school, studying enrollments seems a credible first step in evaluating educational quality more generally.

Unfortunately, a number of issues plague enrollment data (see Urquiola and Calderon, 2006). In addition to poor historical data coverage for many developing countries, recent research suggests that international policy initiatives to increase enrollment have come at the cost of decreased educational quality, further frustrating this measure of education outcomes ([Clemens, 2004] and [Deininger, 2003]). However, following processes outlined by Dreher *et al.* (2008), while acknowledging the data limitations, we have moved forward with the analysis using the best available information.

Beyond the aid literature, our evaluation of the effects of education aid on enrollment fits within the broader discussion of school enrollment in developing nations. For decades, researchers have attempted to ascertain which factors influence school attendance among the world's poorest students. A number of "household factors" are purported to affect enrollment, including gender, family socioeconomic status, urban vs. rural location, parental education, and so forth ([Huisman and Smits, 2009] and [Kazeem *et al.*, 2010]). To estimate these effects we use GDP per capita as a proxy. Other scholars demonstrate the effects of more direct educational inputs, like pupil-teacher ratios, schooling costs, and the like ([Case and Deaton, 1999] and [Deininger, 2003]). Most prominent in the literature, however, is the significant relationship between child health and school enrollment ([Alderman *et al.*, 2001], [Glewwe and Jacoby,

1995], [Handa and Peterman, 2007] and [Schultz, 1999]). For example, Beasley *et al.* shows significant health differences between enrolled and non-enrolled Tanzanian children (2000). And in a path-breaking field experiment establishing causal effects, Miguel and Kramer find that deworming treatments increase school attendance (2004). Clearly, the importance of health when considering third-world school enrollment cannot be overstated. We include infant mortality rates in our models to account for this effect.

Other scholars look more deeply at the effect of various government variables on school enrollment. For example, Brown suggests that more democratic poor countries are more likely than autocratic nations to have higher school enrollment (1999). Rajkumar and Swaroop look more deeply at the effects of government spending on education outcomes, concluding that public spending improves education outcomes in well-governed countries but has no impact in poorly-governed nations (2008). Dreher *et al.* corroborate these findings, concluding in their analysis that government spending on education has virtually no effect on overall education outcomes in poor nations (2008). Filmer and Pritchett find a similarly miniscule effect between government health spending and child health, an area that is typically highly correlated with education (1999). From an aid standpoint, Michaelowa and Weber (2007) conclude that education aid is more effective when coupled with good governance, following the broader conclusions on aid and governance outlined by Burnside and Dollar (2000). This body of evidence suggests that governance quality is a key factor conditioning the effects of education aid on enrollment in poor countries.

We build on this literature by underscoring the important role that adverse selection plays in aid effectiveness. Economists have offered insurance markets as the classic context to illustrate adverse selection (see [Arrow, 1965] and [Pauly, 1974]). For example, smokers face a

greater risk of health problems than non-smokers and, knowing this, may be more motivated to buy health insurance. Unless health insurers have some way of identifying smokers and charging a premium for the insurance, the companies will suffer from the problem of adverse selection: the riskiest and least profitable customers are the most likely to seek and obtain health insurance in the first place.

We argue that a similar problem holds in foreign aid. Recipient governments that pocket or mismanage aid money are also the most likely to seek it. Donor governments may lack knowledge about recipient type, called information asymmetry, or they may possess the relevant information but be constrained by norms, rules, or other values to still provide aid to the worst-governed countries. Thus, we argue that models seeking to explain aid effectiveness must consider the problem of adverse selection. We address the issue below by treating our measures of governance quality – corruption and democracy levels – both as factors that affect aid allocation and as variables that condition the effectiveness of the aid once allocated. Thus, in our empirical models we consider that donors strategically allocate aid to the less corrupt and more democratic countries (see also Bermeo, 2011).

Specifically, we argue that multilateral donors face more constraints in addressing adverse selection than bilateral donors. Multilateral donors, particularly development banks, for most of their histories operated under the norm of political neutrality: regime type and governance quality were explicitly omitted as considerations when negotiating loans. In recent years this *de jure* rule has relaxed, but it has persisted *de facto*. For example, the World Bank has explicit policies meant to condition aid toward countries that manage their public expenses well (World Bank, 2005). However, there is evidence that these standards are not consistently applied

across recipients and World Bank programs (Winters, 2010). We suggest that the banks' institutional structures provide the motivation that constrains this reform.

Namely, developing countries – the very recipients that present the adverse-selection problem – have ample formal voting power on the executive boards of the international financial institutions. For example, at both the World Bank and the Asian Development Bank, developing-country vote shares sum to nearly half of the totals. At the Inter-American Development Bank, developing countries hold an actual majority of votes. These developing-country vote shares, both within executive director constituencies¹ and among directors themselves, can prove pivotal in setting policy at the banks (see Lyne, Nielson and Tierney, 2009). It takes little imagination to envision recipient countries coalescing around policies that diminish the weight given to questions of governance when banks make allocation decisions. Indeed, qualitative interviews with former executive directors at multiple banks suggest that these blocs of developing countries work hard for policies that favor them jointly and that collusion frequently occurs.² Moreover, multilateral banks often face a strong need to lend money to justify their existence, so recipients receive further funding even after failing to conform to loan conditions or despite weak institutions and governance (see [Easterly, 2006] and [Knack, 2001]).

On the other hand, while bilateral donors also face constraints driven by military alliances, trade partnerships, and former colonial ties, they appear to exercise significant discretion in allocating aid among different sectors. Bermeo, for example, presents compelling evidence that bilateral donors allocate their portfolios strategically in ways that constrain – and even bypass – corrupt and poorly managed governments (2011). Because bilateral donors are not

institutionally beholden and officially accountable to recipients in the same ways that bind multilaterals, bilateral aid agencies should be able to address the adverse selection problem by allocating aid selectively to the recipients most likely to use it well. We measure governance quality as levels of democracy and corruption and assess its effects both on the allocation and the effectiveness of aid from bilateral vs. multilateral donors. We turn now to a discussion of the data used to estimate the model.

3. DATA

While the ability of foreign aid to improve education outcomes is important in all recipient countries, we limit our analysis roughly 100 low- and lower-middle-income countries as defined by the World Bank. This is chiefly because primary enrollment rates frequently approach 100 percent among high- and upper-middle-income countries. The fact that the upper bound constrains the variance – added to the reality that the wealthier countries receive less aid per GDP (or none at all) – suggests that the safest practice is to exclude them from the analysis. Furthermore, in low-income nations, increased enrollment is likely a major focus of development assistance, and, as noted above, increased enrollment is a reasonable outcome of improved educational systems and opportunities, despite measurement flaws. Thus, enrollment rates, at least in developing-country settings, provide a reasonable way to assess the impact of education aid.

Our aid variables are taken from AidData 1.9 (Tierney *et al.*, 2011). We define primary aid as the nominal commitment amounts reported for each project that was coded as having primary education as its major purpose. For projects not included in the CRS, AidData coders assigned each project an overall purpose code describing its general thrust. The five-digit codes are extrapolated from the OECD-CRS coding schema and represent a reasonably comprehensive

set of aid activities. Also, commitment values are given preference over disbursement values because of their broader availability. While it seems logical to look at aid that has actually been disbursed instead of money that has only been promised, the poor coverage of disbursement data precludes their use. Furthermore, some studies suggest that commitments predict disbursements reliably – or at least that little evidence suggests otherwise ([Nielsen and Tierney, 2005], [Neumayer, 2003] and [Wilson, 2009]).

We then generated a variable for projects with any education purpose code *except* primary education (e.g. secondary, tertiary, combined or unspecified education projects), which we include in the models as a control variable to better isolate the effects of primary-specific education aid. Following the same logic, we created a variable representing aggregate aid flows for all sectors except education. For each of these aid variables, we generated a series of sums totaling aid from one to five previous years (i.e. $t-1+t-2+. . .+t-5$) to help account for the delay between commitments and actual “on the ground” effects of aid for enrollments. In other words, we presume that aid flows each year have an aggregate effect on aid outcomes. So, for each country-year observation in our data, we sum aid totals from one year prior to the observation up to nine years prior to the observation to account for these aggregate effects over time. We report results of the five-year sum variable ($t-1+t-2+. . .+t-5$) in the statistical tables below, but we note that results were robust across specifications ranging from three-year sums to nine-year sums.

All aid variables were measured in per capita terms to control for variance in population. The amounts reported were divided by the total population of the recipient country as reported in the World Development Indicators. Due to limited coverage of population figures, the population values were smoothed over a five-year period, allowing us to calculate per capita figures for as many aid variables as possible. Skewness tests indicate skewness statistics of 10.1, 23.5 and 5.2

for primary aid per capita, other education aid per capita, and all other aid, respectively. Given this skewness, we logged the variables to help eliminate potential bias. The patterns in primary education aid over time can be seen in Figure 1.

****Figure 1 here****

Figure 1 suggests that per-capita primary education aid concentrates at a very low level: between 0 and 10 dollars per capita, with a low average of roughly 1 dollar per capita. In essence, while we see that the values of primary education aid nominally increase over time, most aid per capita levels remain fairly static. Thus, over the past three decades, the majority of primary aid has remained at a relatively low level per-capita, though some countries have seen substantial increases over time.

To more fully specify our model, we have included a number of control variables. These controls follow standards set forth in prior literature on both aid effectiveness and school enrollments. Most of our control variable data comes from the 2009 World Development Indicators (WDI) published by the World Bank (2010). Our measure for income is the WDI's Gross Domestic Product per capita (US\$ 2000), as included both by Michaelowa and Weber (2007) and Dreher *et al.* (2008).

Considering the important connection between child health and school attendance demonstrated by the enrollment literature, we include infant mortality rates as a proxy variable for health, presuming that a country's infant mortality rates should have significant impacts on enrollments. We use the WDI's infant mortality rate per 1,000 births as our proxy measure.

Following previous literature, we also include a control for the percentage of the population in a country that is below the age of 15 ([Dreher *et al.*, 2008] and [Michaelowa and Weber, 2007]). This measure is intended to hold constant the level of strain that the education system places on a society. Countries with higher proportions below the age of 15 will have more potential students attending school and a smaller proportion of adults to provide and pay for education.

Moving to our variables of interest, much literature on aid effectiveness in general and education aid in particular includes measures for governance and democracy, largely following the findings of Burnside and Dollar that the quality of government institutions influences aid outcomes (2000). Previous education and aid articles use the Freedom House index, government expenditures, and other proxy variables to account for government influence on education outcomes ([Dreher *et al.*, 2008] and [Michaelowa and Weber, 2007]). In a similar manner, we include the Polity 2 measure published in the 2008 edition of the Polity IV database as one of two proxy measures for governance. This measure provides a scale for the level of democracy in a country from -10 to 10. Countries with a -10 score are highly authoritarian and countries with a 10 score are highly democratic. Previous literature suggests that countries with more democratic institutions should more effectively use education aid (Michaelowa and Weber, 2007).

Additionally, we include a measure of corruption provided by the International Country Risk Guide (ICRG) as a second measure of governance. The ICRG measure assesses perceptions of corruption by businesspeople and diplomats working with the countries in question. It is scaled from 1 to 6, with higher values indicating lower levels of corruption (PRS Group, 2009). We note that all covariates are specified under a number of lag structures ranging from two to nine years, corresponding with the rolling sums applied to the aid independent variables. All models were conducted separately for each lag specification. Data on school enrollment – which is the

measure of our dependent variable – are taken from the WDI (World Bank, 2010). All raw enrollment data are measured in percentage values. Data coverage becomes more thorough in the mid-1990s, which is when we begin our analysis. The WDI dataset records the proportion of children per year enrolled in primary education programs. Data on enrollments are also skewed, so we again transformed the data using the logarithm. Some of the enrollment data were missing from the sample, but the values universally changed slowly over time. This enabled us to smooth the values over the gaps using a two-year rolling average and thus to increase the number of observations accordingly.

****Table 1 here****

As the table indicates, primary education aid evinces a startlingly low mean – barely more than 1 dollar per capita each year. As Figure 1 suggests, the majority of aid totals have remained at roughly this level across the years in the sample. The minimal average amount of primary education aid per capita over time suggests neglect by donors of basic schooling in recipient countries. As such, we worry at the outset that the extremely low amounts of education aid may not have significant effects on outcomes – not so much because they are targeted poorly or captured as because the project dollars are ultimately trivial. When pooled with all aid for education the total is still quite low but rises to nearly \$7 per capita. To put this in context, the median primary education expenditure per capita for the countries in our sample is \$33 (mean = \$51). So education aid represents, on average, roughly one fifth to one eighth of government expenditures on primary education in poor countries. By comparison, upper-middle income

countries spend an average of \$237 (median = \$220) per capita on primary education per year and upper-income countries \$1,325 (median = \$1,228).³

Nevertheless, the average amounts of education aid mask some significant cross-sectional variation, as the high standard deviations in Table 1 suggest. Graph 1 (below) shows the variation in the average annual primary education aid received per capita for a random sample of twenty low- and low-middle income countries. Indeed, our initial example of Eritrea suggests that aid sometimes provides a very large share of government education expenditures. Roughly 16 percent of poor countries receive education aid dollars that exceed the average basic education expenditures of \$33 per capita for the sample countries.

Notably, the French collectivity of Mayotte near Mozambique in the Indian Ocean received an astounding \$59 million in primary education aid per year in 2007 and 2008 to provide primary schooling for an estimated population of 194,000 people. The archipelago nation is so small that statistics are missing from official compendiums, but if the islands host roughly the same proportion of children with the same enrollment rates as the average from our sample (37 and 83 percent, respectively), per-pupil aid for primary education would be \$994 – on par with *total* primary education expenditures in upper-middle-income countries. Additionally, the tiny French island territory of Wallis and Futuna in the South Pacific received \$10 million per capita in basic education aid in 2007 and 2008 for its population of 15,000 (our estimated aid per pupil is \$849). France, no surprise, provided all four aid projects, suggesting that it pays to be schoolchildren in at least some French island colonies. Such wide cross-sectional (and time series) variation in education aid suggests the need for a systematic assessment of its effects on enrollment rates in recipient countries.

****Graph 1 here****

4. METHODS

For our initial estimation, we employed a latent-growth, structural equation, mixed effects, or hierarchical linear model (HLM). This type of multilevel model is appropriately used to analyze time-series cross-sectional data like those found here. One drawback of the pooled time-series OLS method often used in aid-growth research is the significant differences in both intercepts and slopes across the countries being analyzed. With the type of data used for aid research, individual observations are at the country-year level, and OLS models require the trajectory for each group over time to be uniform. However, we should expect each country in the model to have very different characteristics, starting points, and trajectories, so the coefficients likely vary significantly by country. In other words, countries have different numbers of aid observations, different aid growth rates, and other diverse characteristics that create significant variance across panels that OLS cannot capture. Latent growth curve models account for such variation (Meredith and Tisak, 1990). HLM allows the parameters of the regression to vary by country.

We submit that HLM provides a good solution for two-level, multiple-group datasets like the one employed here. While not yet applied widely in the development literature, latent growth models have found frequent use in research evaluating group trends in education outcomes while allowing for significant variation across individuals ([Li *et al.*, 2000], [Burchinal and Appelbaum, 1991] and [Duncan and Duncan, 1995]). In terms of the wide variation in intercept and slope of our countries across time, we face similar variance as would be found when measuring individual responsiveness to group education interventions. In a more familiar development context, latent growth estimation has recently been used to model the effectiveness of health-sector aid (Wilson, 2009).

HLM also poses a more challenging hurdle for the argument that education aid boosts enrollments and thus provides a more conservative test of the model. Specifically, HLM methods account for potentially unobserved development factors that cluster by country and that stem from those idiosyncratic development trajectories. However, while HLM solves issues inherent in comparability of variables across time, it does not specifically address endogeneity concerns. Reverse causality is a frequent problem in analyses of aid effectiveness. In our case, aid allocation may be influenced by a country's enrollment rates just as aid may influence enrollment. To address potential endogeneity in our model, we estimate the allocation of education aid separately from enrollments. Then we include the relevant control variables that affect allocation when estimating enrollment. A more elaborate two-stage model was attempted to estimate these related effects and reduce the potential endogeneity. However, we were unable to find appropriately exogenous instruments for such a model, and have thus forgone simultaneous estimation.⁴ Bereft of appropriate instruments for two-stage estimation, our perceived causal effects of bilateral primary education aid on enrollments may be misconstrued. Potentially, some bilateral donors, using some unobserved criteria, select aid recipients that are more prone to increase enrollments than do multilateral donors. Our controls, determined partially through the allocation model, are meant to reduce these selection effects. However, the endogeneity potential in these models prompts us to modulate the claims about that the results suggest. Our analysis proceeds in two steps: (1) Estimation of primary aid allocation using HLM and (2) estimation of the effects of primary education aid on school enrollment using HLM.

(a) Education Aid Allocation

Modeling our argument that donors may condition their aid on recipient governance, we first employ HLM methods to estimate a model of primary-education aid allocation. The model may also suggest control variables for the estimation of enrollment that may help to reduce endogeneity bias. Dependent variables are current year per-capita primary education values, divided between bilateral and multilateral totals. For clarification, this model does not consider the determinants of dyadic aid allocation. Rather, it evaluates the gross amount of primary education aid allocated to a specific country from bilateral or multilateral donors as wholes. We accept that donor strategies are a key player in aid allocation; however, this model attempts to capture bilateral donor strategy *writ large* instead of at the individual donor level.⁵

****Table 2 here****

These results indicate that democracy is a significant predictor of aid allocation for both bilateral and multilateral donors (see Table 2). Against expectations, multilateral donors do seem to discriminate by democracy levels of recipients when allocating primary-education aid. The results suggest that bilateral donors, as predicted, also condition primary-education aid on democracy levels. Substantively, the results also appear significant. The results suggest that a ten-point shift in the polity score (say, from a partial to full democracy) may lead to a 13 percent increase in primary-education aid. While encouraging, we note that democracy may be a less direct indicator of governance quality than is control of corruption. Despite the intrinsic importance of democratic institutions, democratic countries do not necessarily have more efficient or less corrupt governments than do autocratic nations. It encouraged us normatively

that multilaterals condition on democracy, but we also note that corruption-perception indices may say more about a country's proclivity to use aid dollars well.

We further find that the corruption index serves as a significant predictor of primary education aid for bilateral donors only. Substantively, a shift from partial to full control of corruption (from 3 to 6 on the ICRG scale) could result in an 18 percent increase in aid for primary education. These results suggest that bilateral donors consider both democracy levels and corruption levels when allocating aid for primary education, which indicates that they may indeed be attempting to address the adverse-selection problem by focusing on governments that may use aid dollars more productively. While not conclusive, this result suggests some evidence indicates for our hypothesis concerning bilateral aid allocation. Bilateral donors seem to pay more attention than multilaterals to a recipient's control of corruption when allocating educational foreign aid. While multilateral donors also seem attentive to democracy levels, we note that corruption levels may provide a more telling proxy for overall government quality. The results of the allocation model also recommend the inclusion of democracy and corruption as control variables in our estimation of the effects of aid on enrollment to reduce selection bias. If democracy and corruption influence donor allocation, their inclusion as controls should address some of the potential endogeneity in our estimation of aid on enrollment.

(b) Primary education aid and primary school enrollments

We next estimate the overall effect of primary-education aid on school enrollment. We employ control variables for all foreign aid (minus aid to education) and all education aid (minus aid to primary education). While these variables allow us to isolate the effects of primary-only aid, collinearity may reduce the precision of the estimates, but they should not bias results and

are warranted theoretically. We note again that the aid figures are analyzed as a sum of five previous years (t-1 to t-5), allowing us to observe *cumulative* effects of primary education aid on education outcomes. Intuitively, aid effectiveness, when measured at the country level, should be viewed aggregately over time. Consider, for example, time-intensive processes such as school construction, teacher training, or education-sector reform that may take several years to show effects. Since projects have differing timeframes for implementation, this summation method accounts for the time needed to observe “on-the-ground” effects of aid. For clarification, these five-year rolling sums do not reduce our observations to five-year intervals. Instead, every country-year observation in the dataset has an aid total that reflects an aid sum for the previous one to five years of aid receipts. Thus, for each observation, we look at an aggregate effect of aid over a period of years, instead of just one allocation for one lagged year.

We also estimate each model for both aggregated aid flows and for disaggregated bi- and multilateral aid flows, allowing us to better discern the conditions under which aid appears to have the greatest relationship with enrollment. The primary connection between this estimation and the allocation model is the inclusion of democracy and corruption control variables. As is shown in the allocation model, these variables likely play a role in donor’s allocation decisions for primary education aid, especially among bilateral stakeholders. Thus, we use these indicators to control for potential reverse-causality between aid allocation and enrollment rates.

****Table 3 here****

Initial estimations utilizing aggregate aid as the major descriptive variable are inconclusive about the relationship between aid and enrollment (see Table 3). Control variables

here act as expected – significant coefficients bear the expected signs. Notably, the results suggest that control of corruption significantly boosts enrollment. Likewise, democracy may positively affect enrollment, though the results are less stable across specifications, and often show statistical significance below acceptable levels.⁶ Furthermore, results indicate a strong effect of infant mortality rates – the health proxy – on enrollment. Across specifications, the variable shows significance above the 5 percent level, and results appear substantive as well. We note that all explanatory variables were included in the regression under various specifications ranging from the cumulative effects from the previous two to nine years. Furthermore, the control variables were taken as smoothed values from the previous two to nine years. While our primary regressions reported here use five-year aid sums with five-year rolling lagged averages for the other variables, our relevant findings remained robust across each specification.

Our most telling results emerge as we move to the models disaggregating bilateral and multilateral aid flows. Across all models, control-variable effects hold constant, but bilateral primary-education aid shows positive and significant effects on enrollment rates; multilateral aid remains insignificant. *Ceterus paribus*, bilateral aid to primary education appears to be more significantly related to education outcomes than is comparable multilateral aid. When evaluated aggregately, collective aid to education appears unrelated to enrollment rates at conventional significance levels. However, when disaggregated, bilateral aid is significantly related to enrollment; multilateral aid does not appear to affect enrollment significantly. Corruption remains an important predictor of enrollment, as does child health, for both bilateral and multilateral estimations. Both are significant at the 5 percent level or better. Adult literacy also remains a highly significant predictor of enrollment growth.

The substantive significance of bilateral primary-education aid is fairly modest, however. It would take a doubling of bilateral aid for primary education to raise enrollments by 2.3 percent. The very low median levels of primary-education aid further weaken our ability to draw conclusions from this analysis. However, these very low levels of bilateral primary-education aid also indicate that perhaps a doubling (or more) is within the realm of possibility, particularly if donors reallocate existing aid from other sectors to primary education. In sum, while they are not definitive, the results suggest that bilateral donors may condition their primary-education aid on control of corruption better than multilateral donors. This conditioning may then result in more effective use of education aid dollars to improve primary school enrollment.

5. CONCLUSIONS AND IMPLICATIONS

The results included here suggest some reinforcing evidence for previous findings that primary-education aid may cause limited increases to school enrollment. However, the results also suggest that those effects may not be universal. Indeed, separating bilateral from multilateral aid produces significantly different results for the two types. Consistent with our hypothesis, the results suggest that bilateral aid may overcome the problem of adverse selection somewhat better than multilateral aid. The allocation models included point to the possible mechanism: bilateral donors may condition their primary-education aid on recipients' quality of governance. Without simultaneous estimation, we cannot definitely state that education aid effectiveness results from governance-based bilateral aid allocation. However, our two models are reasonably suggestive of such a connection, and further research may be directed toward substantiating these claims.

Since governance quality remains a broadly specified concept that is difficult to measure, we rely more heavily on corruption-perception indices as a proxy for governance. Multilateral donors appear also to select recipients according to democracy levels, which we argue may be a weaker indicator of governance quality – at least as it pertains to making aid effective. But, unlike multilateral donors, bilateral donors appear to add control of corruption as a criterion for allocation, which may help explain the greater effectiveness of bilateral aid suggested by the results of the enrollment models. Corruption levels are indicative of a government's ability to allocate resources efficiently and administer effectively, and we expect that such factors should have the greatest impact on aid effectiveness. Interestingly, Eritrea has received twice as much multilateral primary education aid as bilateral education aid, lending some anecdotal support to the suggestive findings on aid type and effectiveness.

We also caution that these results rest on objectively low levels of primary education aid in the first place. Perhaps we simply should not expect a bare \$1 per capita (or \$3.50 per pupil) to do much to compel children to go to school at higher rates. So while aid advocates may be tempted to use these results as evidence that more aid is needed, we emphasize that we encountered minimal evidence that more aid leads to better education outcomes in general. We tentatively suggest that bilateral donors appear to be able to make their primary-education aid effective in promoting school enrollment; however, these results are substantively small and not definitive.

The results presented may provide a less sanguine view of education aid than prior studies. Three factors may account for the disparity. These same factors serve to highlight the major value this study adds to the aid effectiveness literature. First, the more comprehensive project information provided by AidData broadened the sample in a heterogeneous way,

allowing for a more complete assessment of multilateral development finance alongside bilateral aid. Second, we focus analysis on the specific effects of aid exclusively directed to primary education as opposed to education aid in the main. This enabled a finer-grained analysis of education-aid effectiveness, and it suggested room for reassessment of education aid priorities.

Third, we used an estimation procedure, latent-growth modeling, that we believe provides a more conservative and appropriate test for pooled time-series data. In pooled time series, not only do intercepts differ markedly among the panels, but coefficients also diverge substantially. Accounting for differences in country trajectories – rather than pooling all coefficients jointly – should provide a higher hurdle for observational studies of aid effectiveness. Our view is that scholars should err on the side of caution, particularly when their findings have policy relevance. While our findings proved somewhat robust to the Generalized Method of Moments (GMM) model used by Dreher *et al.*, (2008), we failed to find adequately exogenous instruments. Instead, we sought to reduce endogeneity by (1) modeling factors that affect donor allocation of educational aid and then (2) including the key variables from that model as covariates in our analysis of education aid effectiveness. While a two-stage model is preferable, the absence of adequate instruments in our initial tests demands this less elegant method. What is more, latent-growth models preclude simultaneous estimation with instruments. Latent growth models appear appropriate methodologically for our data, even if the results are less suggestive than GMM of positive general effects for basic education aid on primary school enrollments. We again note, however, that our model does not account for all potential endogeneity, somewhat decreasing our confidence in the findings.

An interesting finding presented here involved the consistent results across the various models suggesting that public health in developing countries appears to be a robust predictor of

school enrollments. This result reinforces theory and findings from prior studies. We recommend that future analyses continue comprehensive examinations of the potential effects of health levels on education outcomes. To the degree that health aid can be made effective (and we emphasize that such associations need to be credibly established *a priori*), attention should be given to its potential positive spillover effects on education outcomes.

Little robust evidence suggests a broad and general causal link between primary education aid and primary school enrollment, and our results, again reinforcing prior findings, suggest some mechanisms that may condition aid effectiveness. To what degree does corruption prevent aid dollars from reaching their intended recipients? Consider the well-known study by Reinikka and Svensson, which presented (contested) evidence that bureaucratic and political “capture” prevented education funding from reaching many Ugandan schools (2005).

We of course risk banality when we suggest that further research may most effectively focus on a narrower picture of education aid effectiveness. Further studies on specific aspects of education and development, in the vein of Reinikka and Svensson, may help ascertain which types of education projects are most effective, thereby making a more helpful contribution to the policy discussion on aid. Better evaluations of *who* gets education aid and *why* they receive it may help us understand the effects of donor aid policies on enrollment growth. While this study suggests caution in associating primary education aid generally with school enrollments, a more “bottom-up” perspective on aid donations may help us tease out the ways that developed nations and multilateral institutions can effectively contribute to education improvement and human development.

Endnotes

¹ At all international financial institutions multiple countries share the vast majority of seats on the executive boards within “constituencies.” Only a few of the largest and most powerful countries hold their own, unshared seats.

² Interviews with Cinnamon Dornsife and John Austin, former Executive Directors at the Asian Development Bank, and Jan Piercy, former Executive Director at the World Bank. Interviews conducted in Washington, D.C. in June 2005.

³ Per pupil – as opposed to per capita – figures are higher: means of \$171 per pupil in low- and lower-middle-income countries, \$1,034 in upper-middle-income countries, and \$7,405 in high-income countries. Aid per pupil to poor countries also rises concomitantly: \$3.55 per pupil for primary education aid, \$22.56 for all education aid. Percentages, of course, remain the same (World Bank, 2010).

⁴ We performed a robustness check using generalized method of moment (GMM) estimation employing instruments (UN Security Council membership and energy aid). The effects of bilateral aid on enrollments were significant in some specifications using different lag structures, controls, and instruments. But the results were not stable across specifications. However, exogeneity tests for all of the instruments suggested that they were inappropriately weak and not sufficiently exogenous. By contrast, Dreher *et al.* found that the same instruments were exogenous but weak. Thus, we place little emphasis on this robustness check.

⁵ Following prior research on dyadic aid allocation, we included recipient membership on the UN Security Council as an explanatory variable in model (Dreher *et al.*, 2009). However, the results were not significant, and were excluded from the final table. Lagged enrollment rates were also included as explanatory variables in the allocation model and were also insignificant.

The table reports our most efficient estimations of the effects of governance – measured by corruption and democracy levels – on bilateral and multilateral aid allocation.

⁶ Another possible estimation strategy would interact both corruption and democracy with bi- vs. multilateral aid to probe the conditional effects of aid multiplied by governance. We attempted this specification, but results were not robust to the inclusion of the interaction terms, likely due to high collinearity. However, since the allocation model demonstrated significant governance selection effects, particularly for bilateral aid, we suggest here that the conditioning effects of governance likely occur at the allocation stage. We thus contend that estimating allocation and enrollment separately is the more appropriate estimation strategy.

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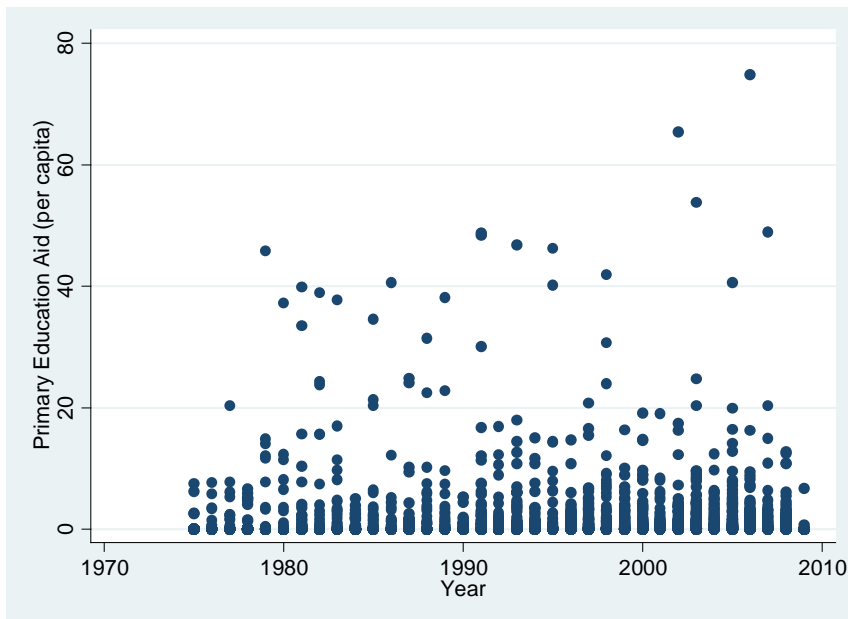
Figure 1: Primary Education Aid by Year

Table 1: Descriptive Statistics

Variable	N	Mean	Std. Dev.	Minimum	Maximum
WDI Enrollment Rate	921	80.33	17.30	21.43	99.78
Primary Education Aid (pc)	2313	1.55	6.11	0	148.73
Bilateral Primary Education Aid (pc)	2313	.71	4.39	0	148.34
Multilateral Primary Education Aid (pc)	2313	.84	3.35	0	74.86
Other Education Aid (pc)	2313	6.10	26.89	0	840.56
All Other Aid (pc)	2313	89.82	163.84	0	2640.53
Corruption (ICRG)	1062	2.68	1.01	0	5
Polity2	1742	1.61	6.14	-10	10
GDP (pc)	2313	1154.72	977.73	62.24	3854.10
Infant Mortality Rate	750	77.57	59.18	7.2	286.9
Adult Literacy Rate	243	73.29	22.46	9.39	99.75
Child Population %	2263	37.03	8.44	13.41	51.46

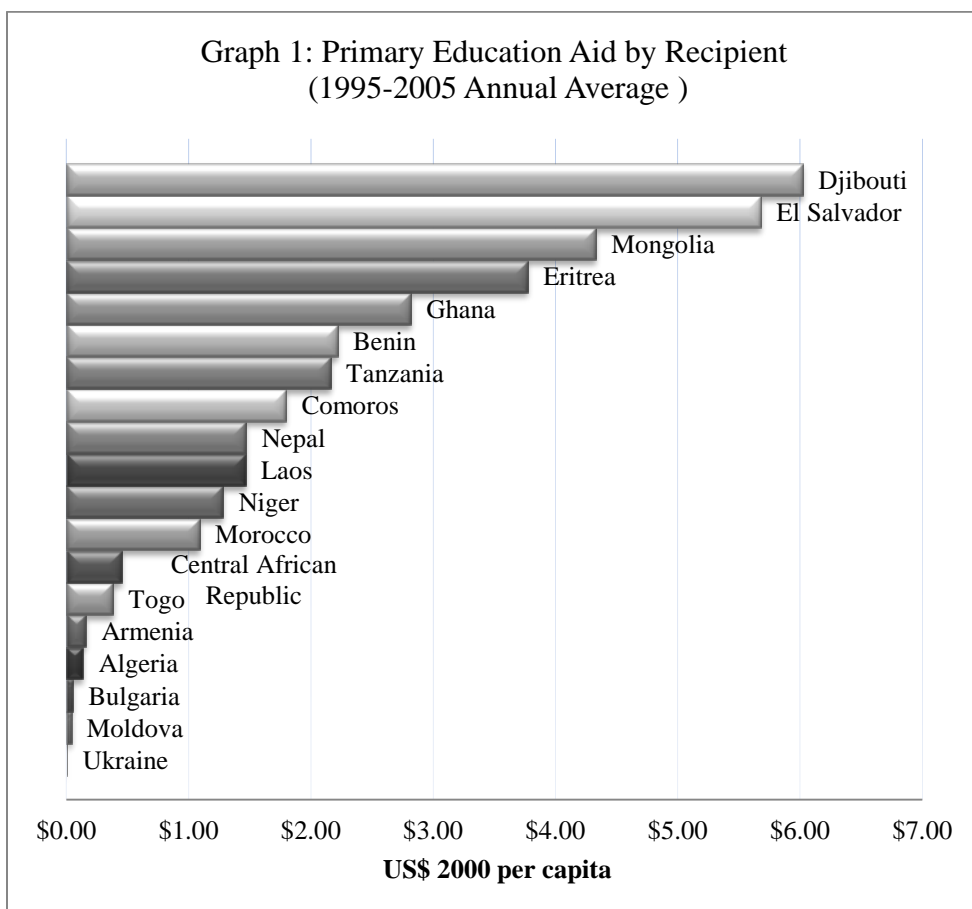


Table 2: Aid Allocation and Donor Selectivity

Dependent Variable: Bilateral and Multilateral Primary Education Aid

	Bilateral Aid 1	Bilateral Aid 2	Multilateral Aid 1	Multilateral Aid 2
GDP (log, per capita)	***-0.1020 (0.0332)	-0.0484 (0.0703)	-0.0353 (0.0227)	0.0841 (0.0521)
Democracy	***0.0132 (0.0043)	*0.0119 (0.0071)	***0.0112 (0.0034)	**0.0117 (0.0052)
Control of Corruption	***0.0630 (0.0215)	***0.1250 (0.0417)	0.0285 (0.0213)	0.0115 (0.0369)
Infant Mortality Rate		0.0021 (0.0021)		*0.0030 (0.0016)
Adult Literacy Rate		0.0037 (0.0028)		0.0011 (0.0019)
Year	***0.0242 (0.00361)	***0.0258 (0.00729)	0.0044 (0.0032)	*-0.0117 (0.0068)
Constant	***-47.790 (7.203)	***-51.320 (14.71)	-8.498 (6.407)	*22.890 (13.600)
<i>N</i>	1330	555	1330	555
Countries	85	74	85	74

Standard errors in parentheses

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

All independent variables reported as 5-yr. rolling average

Table 3: Primary Education Aid & Primary Enrollments

Dependent Variable: Primary School Enrollment (log)

	(1)	(2)	(3)	(4)	(5)	(6)
Primary Education Aid	0.00363 (0.00)	0.00686 (0.00)	0.00622 (0.00)			
Bilateral Primary Education Aid				0.00742** (0.00)	0.0232*** (0.01)	0.0221*** (0.01)
Multilateral Primary Education Aid				-0.00128 (0.00)	-0.00372 (0.00)	-0.00371 (0.00)
Other Education Aid (total)	0.00227 (0.00)	0.00357 (0.01)	0.0032 (0.01)	0.00235 (0.00)	0.00279 (0.01)	0.00228 (0.01)
All Other Aid (total)	0.00538 (0.01)	0.00842 (0.01)	0.00787 (0.01)	0.00549 (0.01)	0.0096 (0.01)	0.00929 (0.01)
GDP (per capita)	0.0697*** (0.02)	0.0804*** (0.02)	0.0624** (0.03)	0.0693*** (0.02)	0.0847*** (0.02)	0.0642** (0.03)
Adult Literacy Rate	0.00124* (0.00)	0.00236*** (0.00)	0.00214** (0.00)	0.00114* (0.00)	0.00228*** (0.00)	0.00204** (0.00)
Control of Corruption		0.0244** (0.01)	0.0228** (0.01)		0.0236** (0.01)	0.0224** (0.01)
Democracy		0.00349 (0.00)	0.00391* (0.00)		0.00398* (0.00)	0.00428* (0.00)
Infant Mortality Rate			-0.00144** (0.00)			-0.00132** (0.00)
% Population Under 14			0.00224 (0.00)			0.00113 (0.00)
Year	0.0105*** (0.00)	0.0111*** (0.00)	0.00995*** (0.00)	0.0105*** (0.00)	0.0103*** (0.00)	0.00902*** (0.00)
Constant	-17.28*** (4.86)	-18.82*** (6.53)	-16.29** (6.46)	-17.23*** (4.87)	-17.12*** (6.52)	-14.42** (6.51)
<i>N</i>	592	384	384	592	384	384
Countries	97	63	63	97	63	63

Standard errors in parentheses

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

Aid variables represent logged sum of previous 5 years of aid

Rolling average of previous five years applied to other variables