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Nicolas Van de Sijpe

This paper adopts a new approach to the issue of foreign aid fungibility. Unlike most existing empirical studies, I employ panel data that contain information on the specific purposes for which aid is given. This approach enables me to link aid that is provided for education and health purposes to recipient public spending in these sectors. In addition, I distinguish between aid flows that are recorded on a recipient's budget and those that are not recorded, and I illustrate how the previous failure to differentiate between on- and off-budget aid produces biased estimates of fungibility. Sector program aid is the measure of on-budget aid, whereas technical cooperation serves as a proxy for off-budget aid. I show that the appropriate treatment of off-budget aid leads to lower fungibility estimates than those reported in many previous studies. Specifically, I find that in both sectors and across a range of specifications, technical cooperation, which is the largest component of total education and health aid, leads to, at most, a small displacement of recipient public expenditures. JEL: E62, F35, H50, O23

The effect of foreign aid on economic growth, poverty, and developmental outcomes may depend heavily on the fiscal response of recipient governments. One

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aspect of this fiscal response is the possibility that aid may be fungible (i.e., the net effect of earmarked aid differs from the intended effect).

This paper endeavors to determine the extent to which earmarked education and health aid are fungible. Many studies of foreign aid fungibility are hampered by a lack of comprehensive data pertaining to the intended purpose of aid. I use the OECD's Creditor Reporting System (CRS), which disaggregates aid by sector, to overcome this problem. To cope with the incompleteness of the CRS data, I propose a novel data construction method that begins with the CRS and adds information from other OECD aid databases to provide more complete measures of education and health aid disbursements.

These data also enable me to divide education and health aid into on- and off-budget components. I demonstrate how a failure to adequately deal with off-budget aid (aid that is not recorded in a recipient government's budget) may have biased previous estimates of fungibility. When donor-based measures of aid are employed, a potentially large fraction of this aid is off-budget aid. Hence, even if aid is used in the targeted sector, some of it may not be recorded as the sectoral expenditures of a recipient government. This failure to record some aid reduces the estimated marginal effect of total sectoral aid on government sectoral expenditures and thus leads to an overestimation of the extent of fungibility. Other papers employ aid data that are reported by recipient governments. In this case, the effect of on-budget aid on government expenditures is estimated, and off-budget aid acts as an omitted variable. Hence, the first problem is that we cannot estimate the degree of fungibility of off-budget aid. Moreover, because off- and on-budget aid are likely correlated, the estimated effect of on-budget aid is biased unless the marginal effect of off-budget aid on government spending is zero.

I use sector program (SP) aid as a measure of on-budget aid and technical cooperation (TC) as a proxy for off-budget aid. Fixed effects (FE) results illustrate the need to consider on- and off-budget aid separately. In both sectors, SP aid has an approximately one-to-one correlation with the public sectoral expenditures of recipient countries. For TC, the proxy for off-budget aid, the same result of limited fungibility is found: its coefficient is close to and typically not significantly smaller than zero, indicating that TC does not displace recipients' own public spending in either sector. The result of limited fungibility for TC, which constitutes the bulk of total education and health aid, is robust across a range of specifications. In contrast, although the effect of SP aid is robust in the context of a static panel data model that is estimated with FE, the coefficient of SP aid becomes imprecise and volatile in a dynamic model that is estimated with system GMM because of the lack of variation in SP aid.

This paper follows the example of Feyzioglu, Swaroop, and Zhu (1998) and Devarajan, Rajkumar, and Swaroop (2007), among others, in estimating the degree of fungibility from a panel consisting of a large number of countries. For each country, the maximum time span for which data on both government education/health expenditure and education/health aid disbursements are available is 14 years. Therefore, I avoid estimating country-specific degrees of fungibility, an approach followed by some researchers in this body of literature (e.g., Pack and Pack, 1990, 1993, 1999). In addition, this paper does not examine the potential consequences of fungibility (for examples of papers that do so, see McGillivray and Morrissey, 2000; Pettersson, 2007a, 2007b; Wagstaff, 2011). Rather, the paper draws attention to a significant weakness of previous studies that do not adequately address the presence of off-budget aid.

The next section illustrates how the inappropriate treatment of off-budget aid may yield biased estimates of the degree of fungibility. Section II briefly explains why aid may not be fungible. Section III discusses the data and the empirical model, and section IV presents the results. Section V concludes the paper.

I. Fungibility and Off-budget Aid

Fungibility occurs when aid is not used for the purpose that is intended by donors (McGillivray and Morrissey, 2004). More precisely, targeted aid is fungible if it is transformed into a pure revenue- or income-augmenting resource that can be spent in any manner in which a recipient government chooses (Khilji and Zampelli, 1994). For instance, earmarked health aid would be fungible if, rather than leading to a one-to-one increase in government health expenditures, this aid were used to finance other types of spending, lower taxes, or reduce the deficit.[1] In this section, I discuss how the presence of off-budget aid may lead to an inaccurate assessment of the degree of fungibility; throughout this section, for the sake of concreteness, I focus on the fungibility of health aid.

First, consider a simple regression of government health spending (*HSP*) on on- and off-budget health aid (*HAIDON* and *HAIDOFF*, respectively):

$$HSP = \beta_0 + \beta_{ON} HAIDON + \beta_{OFF} HAIDOFF + u_1. \qquad (1)$$

Off-budget health aid is aid that is not recorded on a recipient government's budget and that arises from the direct provision of goods and services by donors that does not involve channeling resources through the recipient government's budget (e.g., donors building hospitals, training medical personnel, or hiring consultants). In equation (1), we assess the degree of fungibility of health aid via our estimates of β_{ON} and β_{OFF}. On-budget health aid is not fungible if $\widehat{\beta}_{ON}$ is greater than or equal to 1, in which case every dollar of health aid that is channeled through a recipient government's budget increases government health expenditures by at least one dollar. On-budget health aid is

1. Even if every dollar of health aid is spent in the health sector, health aid may still be fungible if the recipient government reduces health expenditures from its own resources. I discuss this situation in greater detail below with respect to the fungibility of off-budget aid.

fungible if $\widehat{\beta}_{ON}$ is smaller than 1, and full fungibility entails that $\widehat{\beta}_{ON}$ is not greater than the marginal effect of unconditional resources R (resources that are not earmarked for any of the expenditure categories: the sum of domestic revenue and net borrowing). A coefficient $\widehat{\beta}_{ON}$ that is significantly larger than 1 would suggest that a recipient government matches on-budget health aid by increasing its own health expenditures.

To determine the degree of fungibility of off-budget health aid, however, we must compare $\widehat{\beta}_{OFF}$ to a different benchmark. Because off-budget health aid is not considered part of a government's health expenditure *HSP* even if there is no fungibility, a lack of fungibility for off-budget health aid occurs when $\widehat{\beta}_{OFF}$ is greater than or equal to 0, not 1. Off-budget health aid is fungible if $\widehat{\beta}_{OFF}$ is negative. For instance, if a donor finances the building of new hospitals with off-budget health aid, then fungibility would occur if the recipient government reacted by building fewer hospitals and reallocating some of its health spending to other sectors. In that case, the off-budget health aid of the donor is at least partly fungible because the total amount of resources devoted to the health sector (the sum of government health spending and off-budget health aid) increases by less than the amount of off-budget health aid.[2] Full fungibility occurs if $\widehat{\beta}_{OFF}$ is not greater than the marginal effect of unconditional resources R minus 1, whereas a significantly positive coefficient for *HAIDOFF* constitutes evidence of matching behavior by recipient governments.

We are now in a position to discuss how previous studies may have produced biased fungibility estimates. Some studies have relied on aid data reported by donors. These data are either collected directly from donors or obtained from databases managed by the OECD's Development Assistance Committee (DAC) (e.g., McGuire 1982; Khilji and Zampelli, 1994; Pettersson, 2007a, 2007b). In this case, an equation of the following form is estimated:

$$HSP = \beta_0 + \beta HAID + u_2 \tag{2}$$

where $HAID = HAIDON + HAIDOFF$ is total health aid, the sum of on- and off-budget health aid. The estimated marginal effect of health aid on recipient government health expenditures, $\widehat{\beta}$, is used to evaluate whether aid is fungible; a $\widehat{\beta}$ value that is close to 1 is evidence of low fungibility, whereas an estimate

2. Implicitly, this test assumes that off-budget aid resources cannot be directly diverted to other purposes because this direct diversion of off-budget aid would not reduce *HSP*. For example, if medicines are supplied by donors as off-budget health aid, then this assumption implies that a recipient government cannot sell these medicines and spend the proceeds in another sector. As a result, the only way for a recipient government to render off-budget health aid fungible is to reduce its own health expenditure, which is tested in equation (1). The exclusion of off-budget aid from budgetary records reflects a lack of exclusive control of the government over these resources; thus, according to its nature, most off-budget aid should fall into this category of aid that cannot directly be diverted to other sectors. Even if this categorization does not apply to all types of off-budget aid, in the empirical application below, I focus on a specific type of off-budget aid, technical cooperation, for which this assumption is plausible.

that is close to 0 leads to the conclusion that health aid is mostly fungible. The OLS estimate of β can be written as a weighted average of the OLS estimates of β_{ON} and β_{OFF} in equation (1) (see, e.g., Lichtenberg, 1990):

$$\widehat{\beta} = \widehat{\beta}_{ON}\frac{\sigma^2_{ON} + \sigma_{ON,OFF}}{\sigma^2} + \widehat{\beta}_{OFF}\frac{\sigma^2_{OFF} + \sigma_{ON,OFF}}{\sigma^2}. \tag{3}$$

The weights depend on the sample variances of on- and off-budget health aid (σ^2_{ON} and σ^2_{OFF}; σ^2 is the variance of total health aid) and the sample covariance between on- and off-budget health aid ($\sigma_{ON,OFF}$).[3] Because off-budget health aid is not counted as part of government health spending even when it is used within the health sector, $\widehat{\beta}_{OFF}$ will be close to zero even if there is no fungibility. More generally, if on- and off-budget health aid are equally fungible, then we observe that $\widehat{\beta}_{OFF} = \widehat{\beta}_{ON} - 1$. As a result, the presence of off-budget aid in the donor-based aid measure lowers the estimated marginal effect of total health aid on health spending and leads to an overestimation of the degree of fungibility. A marginal effect that is smaller than 1 does not necessarily indicate that aid is fungible; such a value could simply indicate that some aid is not recorded on a recipient government's budget. This bias in the assessment of the degree of fungibility is larger if the variance of off-budget health aid is larger than the variance of on-budget health aid.[4]

Other studies have estimated fungibility for a single country using a time series of recipient-based aid data (e.g., Pack and Pack, 1990, 1993; Franco-Rodriguez, Morrissey, and McGillivray, 1998; Feeny, 2007). In this case, because a recipient government's reports of aid, by definition, exclude off-budget aid, only the effect of on-budget aid on government expenditures is estimated:

$$HSP = \beta_0 + \beta_{ON}HAIDON + u_{3.} \tag{4}$$

Hence, the first problem is that we cannot estimate the degree of fungibility of off-budget health aid. Moreover, because off-budget health aid acts as an omitted variable and off- and on-budget health aid are most likely correlated, $\widehat{\beta}_{ON}$ is biased unless the marginal effect of off-budget health aid on health spending is zero. The sign of the bias is ambiguous because it depends on the

3. For simplicity, the exposition focuses on a cross-sectional case without control variables. Later in the paper, I will primarily examine panel data models that include control variables and that use a fixed effects estimator. In these models, the variables in equation (1) and (2) can be understood as the residuals of the variables after the fixed effects and control variables have been partialled out. In that case, in (3), σ^2_{ON}, σ^2_{OFF}, σ^2 and $\sigma_{ON,OFF}$ refer to the variances and covariance of the partialled-out versions of the relevant variables.

4. I am grateful to an anonymous referee for suggesting this framework to discuss the bias that may be caused by off-budget aid.

partial correlation between on- and off-budget health aid, which could be positive or negative.

This section has clarified the criticism of McGillivray and Morrissey (2000, p. 422) who claim that because a large portion of the aid that is reported by donors is not reflected in the public sector accounts of recipients, such aid measures "... are inappropriate for analyzing fungibility." In addition, this section has shown that the use of recipient-reported aid data is also problematic unless separate data exist that can measure off-budget aid such that equation (1) can be estimated rather than equation (4). Off-budget aid is likely to be sizable in many countries and to vary both between and within countries. Thus, the effects of its inappropriate treatment may be important. With regard to aggregate aid, Fagernas and Roberts (2004a) show that OECD DAC figures for Uganda exceed the external financing recorded by the government by substantial margins (in some years, in excess of 10% of GDP). In Zambia, the gap is as wide as 20–40% of GDP in some years (Fagernäs and Roberts, 2004b). In both countries, the amount of off-budget aid varies substantially over time. Thus, for aggregate aid, σ^2_{OFF} in (3) is unlikely to be small relative to σ^2_{ON}. For Senegal, Ouattara (2006) finds that OECD DAC aid during the 1990s was, on average, twice as high as the aid reported by the local Ministry of Finance (12% vs. 6% of GDP, respectively), although his plots appear to suggest that the variation in aggregate aid over time is predominantly driven by on-budget aid.[5]

The correct method of assessing whether earmarked aid is fungible involves separating on- and off-budget sectoral aid and comparing the marginal effect of on-budget aid on recipient sectoral spending to 1 and the marginal effect of off-budget aid to 0. The aim of this paper is to apply this method in the education and health sectors using a newly constructed dataset of disaggregated aid disbursements. Before presenting the empirical analysis, the next section of this article discusses some of the reasons that earmarked aid may not be fungible.

II. Why Aid May Not Be Fungible

As illustrated in a number of papers (e.g., Pack and Pack, 1993; Feyzioglu et al., 1998; McGillivray and Morrissey, 2000), standard microeconomic theory predicts that fungibility arises as the natural response of a rational government to an inflow of earmarked aid. However, several reasons may explain why aid may not be fully fungible. The most compelling reason may be donor conditionality. The earmarking of aid is automatically accompanied by a

5. Other studies report similarly large shares of off-budget aid out of total aid but do not allow us to assess the extent of variation in off-budget aid over time. In Fiji and Vanuatu, 70% of all aid is off-budget aid (Feeny, 2007). In Malawi, approximately 40% is off-budget aid (Fagernäs and Schurich, 2004), and in Liberia, approximately 75% is off-budget aid (Republic of Liberia Ministry of Finance, 2009).

certain type of conditionality: that aid leads to a full increase in expenditures in the targeted sector. If a donor is able to monitor the fiscal policy choices of a recipient government and to enforce conditionality in a credible manner, then fungibility can be reduced (Adam, Andersson, Bigsten, Collier, and O'Connell, 1994).

A lack of information on the part of a recipient government may also reduce the degree of fungibility. McGillivray and Morrissey (2001) argue that even if policymakers in a recipient country intend for earmarked aid to be fully fungible, fungibility may be reduced as a result of errors in the perception of the implementing officials ("aid illusion"). Incomplete information may contribute particularly to a reduction in the fungibility of off-budget aid. If governments in aid-receiving countries are not aware of the extent to which donors directly provide goods and services in a sector via off-budget aid, then they may not realize that the amount of resources spent in the sector is higher than what they consider optimal. As a result, they may neglect to reduce their own expenditures in the sector when they encounter an inflow of off-budget aid.

There is a final reason to expect less than full fungibility for off-budget aid. The presence of off-budget health aid that cannot directly be diverted to other sectors determines a lower bound for the total amount of resources spent in the health sector (the sum of government health expenditures and off-budget health aid). If the government's desired amount of total resources spent in the health sector is exceeded by the amount of non-divertible off-budget health aid, then fungibility is necessarily reduced.[6] This reason becomes more relevant if we think of the government as separately targeting optimal amounts of various types of health goods that cannot easily substitute for one another rather than one aggregate health good. In that context, the non-divertible off-budget health aid that is directed toward one or several of these specific health goods (e.g., hospitals, syringes, health technical cooperation) would be more likely to exceed the government's preferred expenditure for that good, such that the fungibility of earmarked health aid as a whole is decreased (Gramlich, 1977, makes exactly this point in the context of intergovernmental grants).

Thus, the extent to which earmarked aid is fungible must ultimately be determined empirically. The remainder of this paper is devoted to this task.

6. For example, suppose that in the absence of any health aid, a recipient government spends 100 million dollars in the health sector. If a donor provides 200 million dollars of off-budget health aid, then full fungibility would entail that the recipient government reduces its own health expenditures at an approximately one-to-one rate (i.e., the recipient government reduces its health expenditure by 200 million dollars). However, the government cannot implement such a reduction because health expenditure would need to decrease below zero. The most that this government can do is to reduce its health expenditure by 100 million dollars; in this situation, health aid is only partially fungible.

III. Data and Empirical Model

Sectoral Aid Data

Knowledge of the intended purpose of aid is crucial to obtain an accurate estimate of the degree of fungibility. Therefore, the use of sectorally disaggregated aid in this paper constitutes a marked improvement over previous studies that lack complete information on the purposes for which aid is given. Fiscal response models (FRMs) typically focus on the effect of aggregate aid on a recipient's budget and evaluate aid as being fungible if it is diverted away from public investments or developmental expenditures (e.g., Heller, 1975; Franco-Rodriguez et al., 1998; Feeny, 2007).[7] Early fungibility studies (McGuire, 1982, 1987; Khilji and Zampelli, 1991, 1994) distinguish between military and economic aid and evaluate how these types of aid affect public military and non-military expenditures. Other studies (Feyzioglu et al., 1998; Swaroop, Jha, and Rajkumar, 2000; Devarajan et al., 2007) attempt to investigate aid at the sectoral level but are only able to disaggregate concessionary loans; thus, the omission of sectoral grants may influence their results. In this body of literature, Pack and Pack (1990, 1993, 1999) are the only studies that employ a comprehensive sectoral disaggregation of foreign aid by focusing on countries whose recipient governments report both public expenditures and aid received in a disaggregated form.[8]

In addition, several recent studies (Chatterjee, Giuliano, and Kaya, 2007; Pettersson, 2007a, 2007b) have used sectorally disaggregated aid data from the OECD's Creditor Reporting System (CRS), as described in OECD (2002), to study fungibility.[9] The CRS database disaggregates foreign aid according to a number of dimensions, most importantly the sector or purpose of aid, but has two main disadvantages. First, the CRS data are incomplete. Only some of the total disbursements that flow from each donor to each recipient in any given year are reported. Coverage becomes weaker as one examines earlier periods in time. Second, although information pertaining to commitments is available beginning from 1973, disbursement information is available only for the period after 1990. As a result, many existing papers utilize sectoral commitments even when disbursements are the more relevant quantity.

7. Many of the papers in this body of literature disaggregate aid into grants and loans, multilateral and bilateral aid, or by aid modality, but not by sector.

8. The studies that are referenced in this paragraph estimate the degree of fungibility using panel data for either a large (Feyzioglu et al., 1998; Devarajan et al., 2007) or small (Heller, 1975; Feeny, 2007) number of countries, or they report country-specific estimates of fungibility (all other studies referenced in this paragraph).

9. I describe the OECD's aid databases as they were when I began to construct the sectoral aid data (December 2006). Since then, the CRS and DAC Directives have been updated, and the databases have undergone minor changes (see OECD, 2007a, 2007b).

Several studies (e.g., Mavrotas, 2002; Pettersson, 2007a, 2007b) attempt to avoid these problems with the assistance of data from OECD DAC table 2a, as described in OECD (2000a). DAC2a contains *complete* aggregate aid disbursements but does not include sectoral disaggregation. These studies estimate sectoral disbursements for each recipient and each year ($\widehat{d}^{s}_{RY}$) by calculating the share of each sector s in total CRS commitments and then multiplying these shares by aggregate disbursements from DAC2a ($DAC2a^{agg}_{RY}$):[10]

$$\widehat{d}^{s}_{RY} = DAC2a^{agg}_{RY}\left(\frac{CRS^{s,comm}_{RY}}{CRS^{agg,comm}_{RY}}\right) \tag{5}$$

for $s = 1, \ldots, S$. This strategy yields sectoral aid disbursements even for those years in which only commitment information is available in CRS. Moreover, because $DAC2a^{agg}_{RY}$ is complete, it corrects for the incomplete nature of the CRS data in a simple manner.

This method assumes that the sectoral distribution of incomplete CRS commitments is a good guide to the actual distribution of total disbursements across sectors. This assumption may not hold if, for instance, a donor's propensity to report disaggregated aid to the CRS database varies by sector, or if donors that report a good deal of their aid to CRS have different sectoral preferences than donors that largely fail to report disaggregated aid. As a result, equation (5) may yield highly imperfect measures of sectoral disbursements, especially if CRS coverage is low, such that the sectoral distribution of CRS commitments that is used to allocate aggregate DAC2a disbursements across sectors is based on only a small subset of the total aid committed to a recipient.

To address these problems, I first restrict the analysis to the 1990-2004 period, for which CRS disbursement information is available. More importantly, I construct more complete data on earmarked education and health aid disbursements by accounting for additional information available in DAC table 2a and DAC table 5. Because the method is described in detail in the supplemental appendix, available at http://wber.oxfordjournals.org/, I provide only a brief summary here.

I begin with aggregate and sectoral gross CRS disbursements in a recipient-donor-year (RDY) format, labeled CRS^{agg}_{RDY} and CRS^{s}_{RDY} (for $s = 1, \ldots, S$), respectively. For each RDY observation, the amount of aid that is absent from CRS is calculated as the difference between DAC2a and CRS disbursements:

$$RES^{agg}_{RDY} = DAC2a^{agg}_{RDY} - CRS^{agg}_{RDY}. \tag{6}$$

10. RY denotes recipient-year, *agg* denotes aggregate aid, and *comm* denotes commitments. No superscript is used for disbursements.

The aim is to allocate this total residual (RES^{agg}_{RDY}) across sectors, thereby generating sectoral residuals that can be added to the CRS sectoral disbursements to compensate for the incomplete nature of the latter.

To achieve this goal, I use data from DAC table 5. DAC5 comprises aggregate aid and its sectoral distribution but organizes information only by donor and not by recipient ($DAC5^{agg}_{DY}$ and $DAC5^{s}_{DY}$, respectively). However, DAC5 has an advantage in that these data contain more complete information than CRS.[11] By converting the CRS data into the same donor-year (DY) format, I can calculate the amount of sectoral aid that is absent from CRS in each DY (RES^{s}_{DY}) for each sector. As a result, for each DY and sector, I can compute the share of the sectoral residual in the total residual:

$$SHRES^{s}_{DY} = \frac{RES^{s}_{DY}}{\sum_{s=1}^{S} RES^{s}_{DY}}. \tag{7}$$

This donor- and year-specific allocation of the total residual across sectors is then applied to the total residual in the original recipient-donor-year format:

$$\widehat{RES}^{s}_{RDY} = SHRES^{s}_{DY} RES^{agg}_{RDY}. \tag{8}$$

This procedure yields sectoral residual variables ($\widehat{RES}^{s}_{RDY}$) that are added to CRS sectoral disbursements to create more complete measures of sectoral aid (labeled $\widetilde{CRS}^{s}_{RDY}$). Summing across donors arranges the sectoral disbursements in the required recipient-year format. For some donors, insufficient information is available in DAC5 to allocate the total residual across sectors; therefore, for some observations, the constructed sectoral aid variables still do not reflect the total amount of aid received. Therefore, as a final step, I scale the sectoral disbursements to ensure that their sum matches aggregate disbursements ($DISB_{RY}$):

$$\widehat{CRS}^{s}_{RY} = DISB_{RY} \left(\frac{\widetilde{CRS}^{s}_{RY}}{\sum_{s=1}^{S} \widetilde{CRS}^{s}_{RY}} \right). \tag{9}$$

Aid disbursements are constructed for the following sectors: education (DAC5 sector code 110), health (120), commodity aid/general program assistance (500), action relating to debt (600), donor administrative costs (910), support to NGOs (920) and other sectors (the sum of all remaining sector codes). In addition, data that partition education and health disbursements into four prefix codes or aid types are constructed: investment projects (IP), sector program (SP) aid, technical cooperation (TC), and other (no mark) (ONM). As I explain below, the prefix

11. The data in DAC5 are a mix of disbursements and commitments. To account for this, I scale the DAC5 data to ensure that the sum of the sectoral aid variables matches the aggregate disbursements from DAC2a for every donor-year.

codes are useful because, to some extent, they allow for the separation of on- and off-budget aid flows and thus enable a test of fungibility that is consistent with the framework that is discussed in section I.

This data construction method takes into account that donors that report only a small portion of their aid to CRS might allocate aid across sectors differently than donors that report a larger portion of their aid. Similarly, this method considers that, for a given donor, the sectoral allocation of unreported aid may differ from that of the reported portion. The method ensures that the distribution of aggregate aid across sectors for each donor-year closely follows the sectoral allocation in DAC5, which contains complete disaggregated aid data. Subsequently, the main assumption is that the donor-year-specific sectoral allocation of the total residual applies equally to each recipient that receives aid from the donor in that year that is not accounted for in CRS.

In the final step of the data construction, I scale the sectoral aid variables such that their sum matches aggregate aid received, similar to the scaling performed in previous studies (recall equation (5)). However, because the sectoral disbursements prior to scaling are based on more extensive information than in previous studies, these disbursements are more likely to provide a useful guide to the true sectoral allocation of total disbursements. Therefore, the scaling should be less problematic. On average, the constructed disbursements before scaling constitute more than 76% of the complete aggregate disbursements, whereas this value for CRS disbursements is only 31.9% (see table S1.1 and the surrounding text in the supplemental appendix). For the majority of observations, the scaling that is performed in the final step is limited in magnitude and is substantially smaller than if the CRS sectoral disbursements were scaled without any adjustment. For instance, for more than three-quarters of the observations, the CRS disbursements constitute less than half of the aggregate aid. The constructed sectoral disbursements constitute less than half of the aggregate aid for fewer than 10% of observations. Thus, the sectoral allocation of the aid data before scaling is more likely to provide a reasonable reflection of the actual sectoral allocation that one would find if the data were complete. The failure to scale the sectoral disbursements would increase the risk of underestimating the amount of aid received.[12]

12. Since the construction of the data for this paper, two new disaggregated aid datasets have become available. Ravishankar, Gubbins, Cooley, Leach-Kemon, Michaud, Jamison, and Murray (2009) construct data on health aid by estimating disbursements on the basis of the less incomplete CRS commitments and by adding data from separate reports for a number of NGOs and multilateral and private donors. These data are used by Lu et al. (2010) to estimate the fungibility of health aid. One disadvantage is that a large portion of the data cannot be allocated by recipient country. Lu et al. (2010, p. 1379) state that only 21% of all health aid in 1995 can be traced to recipient countries, and 30% of this aid can be traced to recipient countries in 2006. In addition, it is not immediately clear how one would further divide health aid into on- and off-budget components in these data. A second recent dataset, AidData (http://www.aiddata.org), attempts to construct a more complete disaggregation of aggregate aid into all of its constituent parts according to a number of dimensions but focuses almost exclusively on commitments.

Empirical Model and Other Data

First, I consider models that do not distinguish between on- and off-budget sectoral aid:

$$SSP_{it} = \beta SAID_{it} + \gamma \boldsymbol{A}_{it} + \boldsymbol{\delta X}_{it} + \lambda_t + \eta_i + \varepsilon_{it} \tag{10}$$

for $i = 1, \ldots, N$ and $t = 1, \ldots, T$. SSP_{it} denotes recipient government spending on education or health, whereas $SAID_{it}$ are disbursements that are earmarked for the same sector. $\boldsymbol{A}_{it}$ and $\boldsymbol{X}_{it}$ contain other aid variables and control variables that are described below. λ_t is a set of year dummies, η_i captures country-specific time-invariant effects, and ε_{it} is the transient error. Aid and spending variables are expressed as percentages of GDP.[13] High-income countries (2005 GNI per capita of 10726 US$ or more, following World Bank, 2006c) are eliminated from the sample. I begin with a static panel data model similar to that employed by cross-country fungibility studies that utilize information on the intended purpose of aid, particularly Feyzioglu et al. (1998) and Devarajan et al. (2007). This allows for an easier comparison of the results. Later in the paper, I briefly discuss the results from more general models that allow for some dynamics.

I focus on education and health for a number of reasons. First, education and health play a prominent role in the Millennium Development Goals (MDGs). In addition to their importance in the first goal, which involves eradicating extreme poverty and hunger, several other goals explicitly establish targets related to education and health. This suggests that donors have preferences for education and health spending and should be concerned about the extent of fungibility in these sectors. Second, as partially evidenced by their prominent role in the MDGs, there is a widespread belief that better education and health have immediate consequences for human welfare and play important roles in spurring development and alleviating poverty. This belief suggests that the fungibility of aid that is directed toward these sectors may be relevant for the welfare of the population in recipient countries and may influence the overall effectiveness of aid. Third, these areas are rather clearly defined areas of spending, which should increase the definitional overlap between sectoral aid and sectoral spending.

Public education and health expenditure are staff estimates from the IMF's Fiscal Affairs Department (FAD) and are available for the period prior to 2003.[14] The data are obtained from IMF country documents and have been verified and reconciled by country economists (Baqir, 2002). The main

13. Current US$ GDP from World Bank (2006c) is used to express sectoral aid disbursements as a percentage of GDP.

14. These data are not publicly available, although they have been used in a variety of publications (e.g., Gupta, Clements, and Tiongson, 1998; Baqir, 2002). I am grateful to Gerd Schwartz for sharing these data and to Ali Abbas for assistance in obtaining them.

advantage over other datasets (International Monetary Fund, 2006; World Bank, 2006a, 2006c) is the significantly improved coverage. Moreover, although the level of government (central or general, in which the latter also includes state and local government) spending differs across countries, it is fixed over time. Thus, average differences in government expenditure shares in GDP between countries that result from differences in the government level on which reporting is based can be absorbed by fixed effects (Baqir, 2002).[15]

A_{it} includes commodity aid/general program assistance (henceforth called general aid) and support to NGOs. If targeted toward education and health, support to NGOs may have an effect on a recipient government's spending in these sectors (Lu, Schneider, Gubbins, Leach-Kemon, Jamison, and Murray, 2010, find that health aid to NGOs increases the health spending of recipient governments from their own resources). General aid may partially finance education and health spending or, if linked to structural adjustment programs, may be conditional on lowering public spending. The final variable in A_{it} is other non-education or non-health aid. In the equation for public education spending, other non-education aid includes health aid, and vice versa.

Another aid variable, action relating to debt, is not included in the regression model. Debt relief may be important, but it is not adequately captured by actions relating to debt, including debt forgiveness, debt rescheduling, and other actions (such as service payments to third parties, debt conversions, and debt buybacks) (OECD, 2000b). The debt forgiveness component measures the face value of total debt that is forgiven in a year rather than its present value (PV). Because the average concessionality of debt varies strongly across countries, this may be misleading (Depetris Chauvin and Kraay, 2005). For most types of debt rescheduling, the reduction in debt service in a given year as a result of present and past rescheduling is recorded. Again, this fails to capture the PV of current and future reductions in debt service as a result of debt rescheduling in the current year.[16] For these reasons, I omit action relating to debt as a regressor and instead control for the PV of public and publicly guaranteed long-term external debt as well as public and publicly guaranteed long-term external debt service. These variables should capture most of the effects of debt relief on social spending. Less debt service means that more resources are available to spend on other purposes, whereas a lower stock of debt means that the intertemporal budget constraint is loosened, which may increase the government's appetite for spending. The PV of debt is obtained from Dikhanov

15. For Fiji, the observation in 1998 for both sectors is approximately ten times smaller than that in the surrounding years, most likely due to a typographical error. For instance, public education expenditures account for 0.572% of the GDP in 1998, whereas these expenditures range from 5.19% to 6.37% of the GDP in all other years from 1993 to 2002. Hence, I change this value to 5.72. Similarly, I adjust the public health expenditure value for 1998 from 0.253% to 2.53% of GDP.

16. Only for Paris Club concessional debt reorganizations is the net present value reduction in debt achieved by the current rescheduling recorded (OECD, 2000b, p. 17).

(2004), which is updated through 2004.[17] The source for debt service is the Global Development Finance database (World Bank, 2006b). Again, I use current US$ GDP from (World Bank, 2006c) to express both variables as percentages of GDP.

Other control variables that are included in X_{it} are real GDP per capita (thousands of constant 2000 international dollars) and its growth rate, urbanization (urban population, % of total) and trade (% of GDP) (all from World Bank, 2006c). Because aid that is expressed as a % of GDP is likely to be correlated with GDP (per capita), excluding the latter may induce a spurious relationship between aid and expenditure. Growth is included to capture the reaction of expenditure to short-term shocks in GDP per capita. If government education and health expenditure do not immediately adjust to a higher (lower) level in the event of a positive (negative) growth shock, then a negative coefficient is expected. The effect of trade is a priori ambiguous (e.g., Rodrik, 1998). Greater openness may erode a government's capacity to finance expenditure as tax bases become more mobile. Moreover, tariff reductions may increase trade openness while starving the government of revenue, which again suggests a negative association between trade and public education or health expenditure. However, openness to trade may also increase the demand for social spending to insure against increased external risk and to redistribute gains from trade, and public education and health expenditure may play a role in these effects. Urbanization may also have a positive or negative effect. Some services should be easier to administer in a more urbanized society (Hepp, 2005), and urbanization may create more opportunities for economies of scale. However, lower transportation costs and easier lobbying for government services in urbanized societies may increase the demand for education and health services (Hepp, 2005; Baqir, 2002). For health spending, the risk of contagion and pollution may be higher in cities (Gerdtham and Jönsson, 2000).

Table 1 shows summary statistics for the education and health regression samples. Education aid constitutes approximately 28% of public spending in the education sector, whereas health aid accounts for approximately 22% of public health spending. Slightly less than one-fifth of aid (excluding actions relating to debt and donor administrative costs) is targeted toward education or health.

Hypothesis Tests for No Fungibility and Full Fungibility

As discussed in section I, the presence of off-budget aid in the donor-based measure of sectoral aid ($SAID_{it}$) decreases the estimate of β, thereby overstating the true degree of fungibility. For a correct assessment of fungibility, it is necessary to distinguish between on- and off-budget sectoral aid. Consequently, I also estimate models that partition education and health disbursements into

17. I am grateful to Ibrahim Levent for sending me the updated data (received December 2006) and the Dikhanov paper.

TABLE 1. Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.
Education sector: 1082 observations (108 countries, annual data for 1990–2003)				
Public education expenditure	4.02	1.92	0.38	13.61
Education aid	1.13	1.45	0.01	14.19
Education IP	0.13	0.23	0	3.6
Education SP	0.04	0.09	0	0.95
Education TC	0.81	1.1	0	10.85
Education ONM	0.16	0.34	0	5.83
General aid	1.2	1.92	0	22.78
Support to NGOs	0.13	0.24	0	3.02
Other non-education aid	5.84	6.78	0.01	62.84
Real GDP per capita	3.63	2.98	0.47	17.96
Real GDP per capita growth	1.6	5.46	−30.28	49.86
Urbanization	42.4	20.36	6.3	91.56
Trade	78.11	41.06	10.83	280.36
PV debt	52.15	60.07	0.09	892.12
Public debt service	4.02	3.47	0	35.24
Health sector: 1087 observations (108 countries, annual data for 1990–2003)				
Public health expenditure	1.96	1.25	0.17	7.44
Health aid	0.44	0.54	0	3.63
Health IP	0.11	0.18	0	1.69
Health SP	0.05	0.1	0	1.75
Health TC	0.18	0.23	0	1.91
Health ONM	0.1	0.18	0	1.46
General aid	1.21	1.97	0	22.78
Support to NGOs	0.13	0.24	0	3.02
Other non-health aid	6.56	7.5	0.02	66.11
Real GDP per capita	3.64	2.98	0.47	17.96
Real GDP per capita growth	1.58	5.4	−30.28	28.5
Urbanization	42.24	20.4	6.3	91.56
Trade	77.8	41.2	10.83	280.36
PV debt	51.12	59.14	0.09	892.12
Public debt service	3.91	3.24	0	35.24

Note: All variables as % of GDP except real GDP per capita (thousands of constant 2000 international dollars) and its growth rate and urbanization (urban population, % of total).

Source: Author's analysis based on data described in the text.

the four prefix codes:

$$\begin{aligned} SSP_{it} = {} & \beta_{IP} SAIDIP_{it} + \beta_{SP} SAIDSP_{it} + \beta_{TC} SAIDTC_{it} \\ & + \beta_{ONM} SAIDONM_{it} + \gamma A_{it} + \boldsymbol{\delta X}_{it} + \lambda_t + \eta_i + \varepsilon_{it} \end{aligned} \tag{11}$$

where *IP* represents investment projects, *SP* denotes sector program aid, *TC* represents technical cooperation, and *ONM* denotes other (no mark) aid.

SP aid should primarily be on-budget aid because, by definition, program aid involves a government-to-government transfer of resources. In contrast, TC is a good proxy for off-budget aid. The costs of providing training and scholarships in donor countries, remunerating experts and consultants, and financing equipment and administrative costs associated with TC primarily involve direct payments from donor governments rather than transfers of money to recipient governments. In fact, Sundberg and Gelb (2006) argue that many aspects of TC, such as finance for training programs, analytical reports and expert advice, involve resources that never even leave donor countries. For the seven countries that they study, IDD and Associates (2006, p. 23 in annex B) indicate that off-budget aid is explained, among other things, by "aid in kind e.g. TA [technical assistance] and other aid where expenditure is undertaken directly by the donor." Similarly, Fagernäs and Roberts (2004b) argue that technical assistance involves donors making direct payments that are not reflected in budget documents, and Feeny (2007, p. 442) states that "the salaries of external consultants will not enter public sector accounts." Feeny argues that a larger share of aid is off-budget in Fiji and Vanuatu compared with Papua New Guinea and the Solomon Islands because the former two countries receive a large proportion of their aid in the form of technical assistance. In addition, Fagernäs and Roberts (2004a) attribute discrepancies between donor and recipient reports of aid in Uganda at least partially to the omission of TC from the budgets of recipient governments. Johnson and Martin (2005, p. 6) conclude that "HIPCs see direct payments by donors to foreign suppliers as highly problematic, as they are often not informed of the actual disbursements. This is especially true for technical assistance provided by expatriate experts, who are hired and paid by the donor." Baser and Morgan (2001) find that TC is off-budget in the six African countries that they investigate. Drawing from the experiences of a much larger group of countries, OECD (2008, p. 59) notes that "technical co-operation expenditures are described as a particular problem in recording aid on budget." Mokoro (2008), which is a detailed study of the role of aid in the budget process based on both an extensive literature review and case studies of ten Sub-Saharan African countries, identifies a clear hierarchy in the extent to which different aid modalities are disbursed via the treasuries of recipient governments and are captured in their accounts: most likely for general budget support and program aid, much less likely for project aid, and even less likely for technical assistance.

The summary statistics in table 1 suggest that education aid is more than 70% TC, whereas approximately 40% of aid is TC in the health sector. This dominant role of TC in health aid and, especially, education aid is confirmed in the CRS directives (OECD, 2002, p. 26). Average SP aid is small and reflects that for many country-years, education and health SP aid are nearly zero. Particularly in the education sector, the variance in TC is large compared with that of the other sectoral aid modalities, which further reinforces the notion that the bias created by the failure to adequately address off-budget aid may be substantial (recall equation (3)).

The extent to which IP and ONM aid are reported in government budgets is more uncertain. Thus, the estimates of β_{IP} and β_{ONM} are less informative for

TABLE 2. Null Hypotheses for No and Full Fungibility with On- and Off-budget Aid

	No fungibility	Full fungibility
Theoretical null hypothesis:		
Aid on-budget (SP)	$\beta_{SP} \geq 1$	$\beta_{SP} \leq \frac{\partial SSP_{it}}{\partial R_{it}}$
Aid off-budget (TC)	$\beta_{TC} \geq 0$	$\beta_{TC} \leq \frac{\partial SSP_{it}}{\partial R_{it}} - 1$
Implemented null hypothesis:		
Aid on-budget (SP)	$\beta_{SP} \geq 1$	$\beta_{SP} \leq 0$
Aid off-budget (TC)	$\beta_{TC} \geq 0$	$\beta_{TC} \leq -1$

gauging the degree of fungibility.[18] However, using $SAIDSP_{it}$ and $SAIDTC_{it}$ as measures of on- and off-budget sectoral aid, respectively, it is possible to test the null hypothesis of no fungibility and the null of full fungibility in a manner consistent with the analysis in section I, as shown in table 2.

The full fungibility tests require knowledge of the marginal effect of unconditional resources R (typically measured as government expenditure net of aid), which may be obtained by following the two-stage procedure outlined in Devarajan et al. (2007).[19] Nevertheless, the data that I received from the IMF's FAD do not contain total expenditures, revenue or borrowing. Because data availability for these variables in other databases is significantly more limited, a large fraction of the sample would be lost by following this procedure. Instead, I set $\partial SSP_{it}/\partial R_{it} = 0$, such that the implemented tests become those shown in the bottom half of table 2.

In practice, $\partial SSP_{it}/\partial R_{it}$ should be close to zero in both sectors. Unless there is a substantial break in policy, the marginal effect of R should be close to the average share of unconditional resources that are spent in the education and

18. Mokoro (2008) expressly warns against the assumption that aid projects are always off-budget (p. 7) but suggests that the degree to which these projects are captured in budgets is low. (See, e.g., p. 23: "levels of aid on budget are strongly driven by budget support aid (which, by definition, is on budget). In many cases, off budget proportions for other aid modalities still remain very high" and p. 52: "however, budget support has limits, and project aid has been growing. The problems associated with poorly integrated project aid still loom large. The bigger challenge, therefore, is to bring project aid on budget.") In section IV, I discuss how we can gain insight into the degree of fungibility of IP and ONM aid despite the greater uncertainty regarding the extent to which these types of aid are on- or off-budget.

19. As explained in Devarajan et al. (2007), unconditional resources R (or their component parts, domestic revenue and net borrowing) should be excluded from the estimated equation to ensure that the full effect of earmarked aid on sectoral spending is captured. For instance, if sectoral aid reduces tax revenue but the latter is held fixed, then the effect of aid on spending may be overestimated. This two-step procedure entails the inclusion of the residual from a regression of R on the right-side variables in equation (11) as an explanatory variable in the model. Because this residual is, by construction, orthogonal to the other right-side variables, its inclusion does not alter the sectoral aid coefficients, which capture the full effect of earmarked aid. However, its inclusion facilitates the estimation of $\partial SSP_{it}/\partial R_{it}$.

health sectors. As an approximation, if I proxy this share by the share of public education and health expenditure in total government expenditure, then for government expenditure in the range of 20% to 30% of GDP, the figures in table 1 suggest a marginal effect of unconditional resources of approximately 0.13–0.2 for education expenditure and 0.07-0.1 for health expenditure. Devarajan et al. (2007) estimate the effect of unconditional resources on public education (health) spending to be 0.12 (0.04). Feyzioglu et al. (1998) find even smaller effects of 0.08 (0.02) for education (health) expenditure. Therefore, setting $\partial SSP_{it}/\partial R_{it} = 0$ is unlikely to have a significant influence on the conclusions that are drawn from the estimated coefficients and the full fungibility tests, although the probability of rejecting the null hypothesis of full fungibility may be slightly increased.

IV. Results

Table 3 presents the results of the OLS and fixed effects (FE) estimations of equation (11), with total donor-reported education or health aid as the main regressor of interest. Therefore, the hypothesis tests for no fungibility and full fungibility in this table are based on the assumption that education and health aid are completely on-budget. All reported standard errors are robust to heteroskedasticity and are clustered at the country level, thereby allowing for serial correlation in the error term (Arellano, 1987; Bertrand, Duflo, and Mullainathan, 2004).

In both the OLS and FE estimations, public education expenditure has no discernible correlation with education aid, and the null hypothesis of no fungibility is strongly rejected. By contrast, public health expenditure is positively correlated with health aid, and this effect is estimated precisely enough to reject the null hypothesis of full fungibility and the null hypothesis of no fungibility. However, the size of the FE coefficient of health aid is small: an increase in health aid of 1% of GDP is associated with an increase in public health expenditure of only 0.26% of GDP. On the basis of this result, one would still conclude that health aid is mostly fungible.

The results in table 3 are likely to overestimate the extent of fungibility because the presence of off-budget aid decreases the estimated effect of sectoral aid on public sectoral expenditure. Table 4 presents the results from the estimation of equation (11), in which sectoral aid is further partitioned into four prefix codes. This partitioning enables the implementation of the more appropriate fungibility tests described in table 2, using SP aid as a measure of on-budget aid and TC as a proxy for off-budget aid.

The further disaggregation of sectoral aid markedly changes the results. In both sectors, the marginal effect of SP aid in the FE model is close to 1; this result suggests that the bulk of SP aid is used in the intended sector. Full fungibility can be rejected, but the null hypothesis of no fungibility cannot be rejected. The effect of TC is close to zero in both sectors, and the null hypothesis of

TABLE 3. Total Education and Health Aid

	Public education exp.		Public health exp.	
	OLS	FE	OLS	FE
Education aid	0.047	0.0042		
	(0.082)	(0.068)		
Health aid			0.47***	0.26**
			(0.18)	(0.12)
General aid	−0.0032	0.032	0.016	0.0037
	(0.053)	(0.029)	(0.030)	(0.019)
Support to NGOs	−0.41	−0.38*	−0.13	−0.18***
	(0.33)	(0.21)	(0.17)	(0.091)
Other non-education aid	0.0026	−0.0041		
	(0.022)	(0.018)		
Other non-health aid			0.0084	−0.012
			(0.017)	(0.012)
GDP per capita	0.085	0.26*	0.17***	0.14*
	(0.059)	(0.14)	(0.048)	(0.085)
GDP per capita growth	−0.049***	−0.028***	−0.025***	−0.020***
	(0.016)	(0.0093)	(0.012)	(0.0074)
Urbanisation	−0.010	0.080	0.0026	0.056*
	(0.0083)	(0.056)	(0.0053)	(0.033)
Trade	0.015***	−0.014***	0.010***	−0.0075*
	(0.0038)	(0.0068)	(0.0031)	(0.0041)
PV debt	−0.0038	−0.0025	0.00025	0.000032
	(0.0035)	(0.0017)	(0.0022)	(0.00056)
Public debt service	0.050	−0.063***	−0.040**	−0.024**
	(0.062)	(0.022)	(0.019)	(0.012)
R^2	0.178	0.207	0.294	0.171
Hausman		0.000		0.000
$\beta \leq 0$	0.285	0.475	0.005	0.019
$\beta \geq 1$	0.000	0.000	0.002	0.000
Countries	108	108	108	108
Observations	1082	1082	1087	1087

Note: OLS and fixed effects (FE) results, annual data, 1990−2003. All regressions include time dummies, coefficients not reported. Heteroskedasticity-robust standard errors, clustered by country, in brackets. *, **, and *** denote significance at 10, 5 and 1%, respectively. In the case of FE estimation, R^2 refers to the within R^2. Hausman shows the p-value of a generalized Hausman test of the null hypothesis that η_i is uncorrelated with the regressors. $\beta \leq 0$ ($\beta \geq 1$) is the p-value for the test of full (no) fungibility for total sectoral aid.

Source: Author's analysis based on data described in the text.

full fungibility is strongly rejected. The hypothesis of no fungibility cannot be rejected; thus, there is no evidence that sectoral TC displaces a recipient government's own expenditure in either sector. The TC effect is similar in OLS, whereas the coefficients of SP aid become larger but are also estimated less precisely. The larger SP aid coefficients in OLS may indicate that time-invariant unobservables are positively correlated with both SP aid and sectoral public expenditures. In the FE estimation, the coefficients are identified from the within-country variation in the data, which reduces the problem of omitted variables

TABLE 4. Disaggregated Education and Health Aid

	Public education exp.		Public health exp.	
	OLS	FE	OLS	FE
Education IP	0.091	0.12		
	(0.25)	(0.12)		
Education SP	2.53*	1.21**		
	(1.35)	(0.55)		
Education TC	0.032	−0.0070		
	(0.10)	(0.082)		
Education ONM	0.14	0.021		
	(0.21)	(0.19)		
Health IP			0.40	0.20
			(0.34)	(0.21)
Health SP			1.19*	0.84***
			(0.60)	(0.31)
Health TC			−0.12	0.0067
			(0.35)	(0.32)
Health ONM			0.74**	0.41*
			(0.36)	(0.23)
General aid	−0.0012	0.031	0.023	0.0055
	(0.051)	(0.029)	(0.031)	(0.019)
Support to NGOs	−0.56*	−0.39**	−0.15	−0.16
	(0.30)	(0.19)	(0.16)	(0.11)
Other non−education aid	−0.0081	−0.0055		
	(0.022)	(0.018)		
Other non−health aid			0.014	−0.013
			(0.017)	(0.011)
GDP per capita	0.084	0.29*	0.17***	0.15*
	(0.060)	(0.15)	(0.048)	(0.085)
GDP per capita growth	−0.051***	−0.029***	−0.028**	−0.021***
	(0.015)	(0.0091)	(0.011)	(0.0072)
Urbanisation	−0.0089	0.085	0.0026	0.055*
	(0.0081)	(0.055)	(0.0053)	(0.031)
Trade	0.016***	−0.013**	0.011***	−0.0071*
	(0.0039)	(0.0067)	(0.0032)	(0.0040)
PV debt	−0.0040	−0.0027*	−0.000074	−0.000092
	(0.0034)	(0.0016)	(0.0021)	(0.00059)
Public debt service	0.052	−0.065***	−0.039**	−0.022*
	(0.062)	(0.021)	(0.019)	(0.011)
R^2	0.187	0.215	0.302	0.183
Hausman		0.000		0.000
$\beta_{SP} \leq 0$	0.032	0.015	0.026	0.004
$\beta_{SP} \geq 1$	0.870	0.645	0.621	0.307
$\beta_{TC} \leq -1$	0.000	0.000	0.006	0.001
$\beta_{TC} \geq 0$	0.621	0.466	0.363	0.508
Countries	108	108	108	108

(Continued)

TABLE 4. Continued

	Public education exp.		Public health exp.	
	OLS	FE	OLS	FE
Observations	1082	1082	1087	1087

Note: OLS and fixed effects (FE) results, annual data, 1990-2003. All regressions include time dummies, coefficients not reported. Heteroskedasticity-robust standard errors, clustered by country, in brackets. *, **, and *** denote significance at 10, 5 and 1%, respectively. In the case of FE estimation, R^2 refers to the within R^2. Hausman shows the p-value of a generalized Hausman test of the null hypothesis that η_i is uncorrelated with the regressors. $\beta_{SP} \leq 0$ ($\beta_{SP} \geq 1$) and $\beta_{TC} \leq -1$ ($\beta_{TC} \geq 0$) are p-values for the test of full (no) fungibility for sector program aid and technical cooperation, respectively.

Source: Author's analysis based on data described in the text.

in instances in which such variables do not change substantially over time. For the FE results in tables 3 and 4, a generalized Hausman test that allows for heteroskedasticity and serial correlation is reported (Arellano, 1993; Wooldridge, 2002, pp. 290–291).[20] The null hypothesis that η_i is uncorrelated with the regressors is always rejected; this result suggests that FE should be preferred over random effects. Growth consistently has a negative effect, which suggests that education and health expenditures do not immediately adjust to a higher (lower) level in the event of a positive (negative) short-term shock to GDP per capita (Dreher, 2006, obtains a similar result for total and social expenditures in OECD countries).

As a robustness test, I obtain qualitatively similar FE results with aid variables that are constructed by scaling up sectoral CRS disbursements to ensure that their sum matches the aggregate DAC2a disbursements (equation (5) but applied to CRS disbursements rather than commitments). The main change is that for some aid variables, the estimated coefficients are closer to zero and/or estimated less precisely, which is consistent with greater measurement error in the aid data that are constructed using this short-cut method.[21]

Table 4 illustrates that a failure to properly address the presence of off-budget aid may yield misleading conclusions. After on- and off-budget aid are separated and their effects are assessed against appropriate benchmarks, the FE results suggest that there is little if any fungibility. This conclusion is robust to a large number of specification changes. I replace the PV of debt with a non-PV measure of long-term external public and publicly guaranteed debt expressed as a percentage of GDP (from World Bank, 2006b). I also add to the

20. This test is performed in Stata using the xtoverid command (Schaffer and Stillman, 2006).

21. For instance, the coefficient on education SP aid is almost halved, to 0.64, and full fungibility is therefore rejected less strongly. The coefficient on health aid is reduced to 0.07, whereas in the disaggregated model, the coefficients on health IP and health ONM are much closer to zero. In both sectors, the coefficient on support to NGOs is estimated less precisely and/or substantially reduced in magnitude. The only exception is that the coefficient on health SP aid nearly doubles with the short-cut method (from 0.84 to 1.61), but its standard error rises commensurably.

model, in turn, two different measures of the PV of debt relief constructed by Depetris Chauvin and Kraay (2005).[22] Because debt relief is often linked to higher social expenditure, one might expect it to have a larger positive effect on public education and health expenditure than the effect achieved by a reduction in debt or debt service that arises through means other than debt relief. If this effect is indeed larger, then we would expect a positive effect of debt relief even after controlling for the level of debt and debt service. However. I do not find evidence of this effect. Even without controlling for debt and debt service, I find no effect of the PV of debt relief. I further include GDP per capita in log form rather than in thousands of dollars. I add (one at a time) control variables for female labor force participation or the birth rate (both from World Bank, 2006c), measures of corruption, the rule of law and bureaucratic quality from the International Country Risk Guide (ICRG) (The Political Risk Services Group, 2008), the sum of these three ICRG variables (as a general measure of institutional quality), and measures of democracy obtained from Polity IV (Marshall and Jaggers, 2007). Feyzioglu et al. (1998) control for the share of agriculture in GDP rather than urbanization. Therefore, I replace urbanization with the share of agriculture in GDP (from WDI) or add the share of agriculture in GDP alongside urbanization. Many papers also control for the size and composition of the population when explaining variation in public expenditures (e.g., Baqir, 2002; Rodrik, 1998). As a result, I consider models that add the percentage of the population under 15 and/or the percentage of the population over 65 to the model, the age dependency ratio (dependents to working-age population) or the log of population (all from WDI). Finally, Feyzioglu et al. (1998) control for lagged infant mortality, whereas Devarajan et al. (2007) control for lagged secondary and primary school enrollment in the education expenditure equation and for lagged infant mortality in the health expenditure equation. A possible concern is that such variables may be more fruitfully viewed as outcomes than as determinants of public education and health expenditures. Nonetheless, I include either the current value or the once-lagged value of primary gross enrollment, secondary gross enrollment, infant mortality or under-five mortality (mortality data from WDI and enrollment data from Edstats). In all cases, the results are qualitatively unchanged. The only exception is that when the ICRG measures are added, the coefficient of health TC decreases to approximately -0.25, and I can reject the null hypothesis of no fungibility, implying partial (but low) fungibility of health TC.[23]

Influential Observations

Especially given the limited variation in education and health SP aid and, to a lesser extent, TC, one concern may be that the effects of these variables are

22. I am grateful to Nicolas Depetris Chauvin for sharing these data.

23. In addition, no clear evidence is found to suggest that the degree of fungibility depends on the quality of institutions.

TABLE 5. Disaggregated Education and Health Aid, Marginal Effects of the Log-linear Model

	Public education exp.	Public health exp.
$\widehat{\beta}_{SP}$	1.342	1.092
$\beta_{SP} \leq 0$	0.005	0.006
$\beta_{SP} \geq 1$	0.750	0.585
$\widehat{\beta}_{TC}$	0.0522	0.0602
$\beta_{TC} \leq -1$	0.000	0.000
$\beta_{TC} \geq 0$	0.632	0.591

Note: $\widehat{\beta}_{SP}$ and $\widehat{\beta}_{TC}$ are marginal effects, calculated at the sample means, based on the fixed effects estimation of equation (11) in log-linear form. Annual data, 1990–2003. All regressions include time dummies and the standard set of control variables (coefficients not reported) and are estimated with heteroskedasticity-robust standard errors, clustered by country. $\beta_{SP} \leq 0$ ($\beta_{SP} \geq 1$) and $\beta_{TC} \leq -1$ ($\beta_{TC} \geq 0$) are p-values for the test of full (no) fungibility for sector program aid and technical cooperation, respectively.

Source: Author's analysis based on data described in the text.

driven by a small number of observations. Although the inclusion of additional control variables generally does not change the conclusions, the point estimates on the variables of interest shift by a relatively large amount in several instances, especially when the inclusion of an additional variable leads to a large decrease in sample size. Such a shift always results from a change in the sample composition and not because the additional control variable eliminates some of the explanatory power of sectoral SP aid or TC.[24]

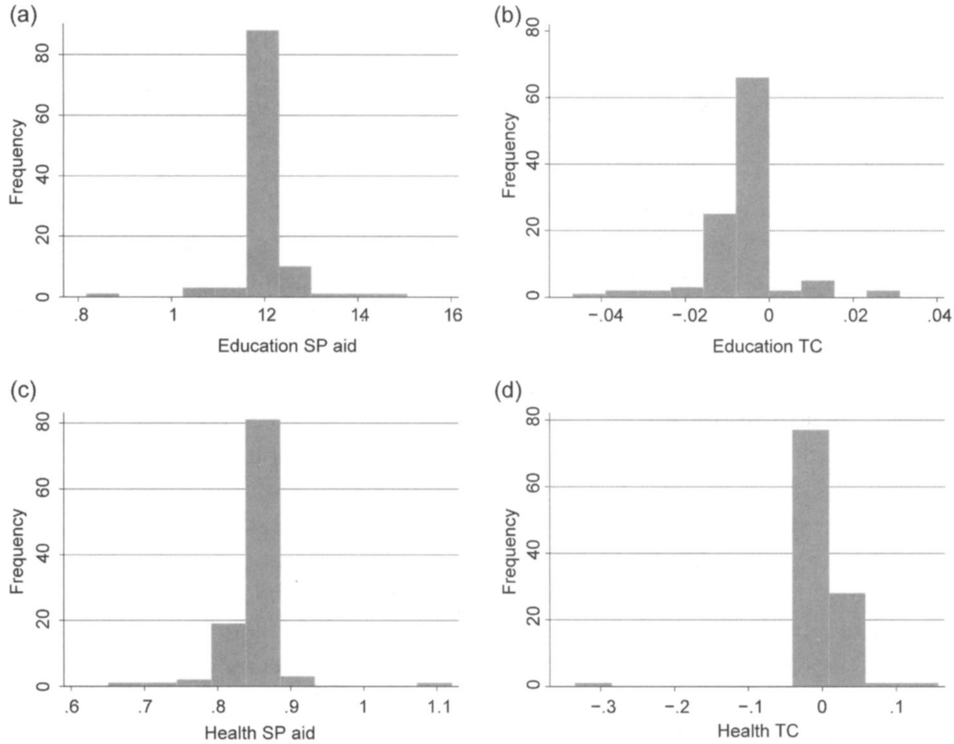
As a first attempt to evaluate the sensitivity of the results to outliers, I re-estimate equation (11) in log-linear form. Taking the natural logarithm of all variables compresses the upper tail and is thus likely to reduce the influence of observations with larger values of education and health SP aid or TC on the estimated regression line.[25] Table 5 displays the marginal effects for SP aid and TC calculated at the sample means (the full results are available upon request). The results are similar to those obtained in the linear model. In both sectors, the effect of TC is close to zero, and the effect of SP aid on public expenditure is close to 1. Full fungibility is rejected across the board, but the null hypothesis of no fungibility cannot be rejected in any of the cases.

As a more direct and arguably superior approach to determine the effects of influential observations, I re-estimate equation (11) by eliminating one country at a time. Figure 1 shows the resulting distribution of the estimated SP aid and TC coefficients. The marginal effect of TC is more stable than that of SP aid in both sectors, which is consistent with the more limited variation in SP aid. A small number of countries induce fairly large changes in the effect of SP aid.

24. The most extreme deviation occurs when the birth rate is added: the sample size in the health model decreases to 612, and the effect of health SP aid in the FE model rises to 1.34.

25. To address zero values in the public expenditure, aid and debt variables, I add 1 before taking the log. Because GDP per capita growth can be negative, I include this variable without taking its log.

FIGURE 1. Distribution of Coefficients When Dropping One Country at a Time



Source: Author's analysis based on data described in the text.

For instance, when Lesotho is eliminated, the effect of education SP aid decreases to 0.82. When Tonga is excluded, this effect increases to 1.51. In contrast, the distribution of the estimated coefficient of education TC has a substantially smaller range. For health TC, two countries have a sizable influence on the estimated coefficient when they are omitted from the sample, but the remainder of the distribution is substantially narrower.[26]

To examine how sensitive the results are to the removal of countries that appear to exert an undue influence on the coefficients of interest, I omit countries for which the absolute value of the $DFBETA_i$ influence statistic for SP aid or TC exceeds the size-adjusted cut-off value of $2/\sqrt{N}$ (in this case, N is the number of countries) proposed by Belsley, Kuh, and Welsch (1980).[27] This

26. Without Eritrea, the estimated effect of education TC becomes -0.33. Without Guinea-Bissau, the effect is 0.16.

27. Using SP aid as an example, I calculate $DFBETA_i$ as $DFBETA^i_{SP} = (\widehat{\beta}^i_{SP} - \widehat{\beta}_{SP})/(\widehat{SE}_{\widehat{\beta}^i_{SP}})$, where $\widehat{\beta}_{SP}$ is the estimated coefficient in the full sample, $\widehat{\beta}^i_{SP}$ is the estimate when country i is eliminated and $\widehat{SE}_{\widehat{\beta}^i_{SP}}$ is the estimated standard error of the coefficient in the model without country i (see, e.g., Bollen and Jackman, 1990).

procedure removes 14 countries in the education sector and 5 countries in the health sector.[28] Table 6 presents the results of estimating equation (11) for this reduced sample, and figure 2 shows partial scatter plots of the key relationships in the FE regressions for both the full and reduced samples. The FE results in the reduced sample are similar to those in the full sample. The effect of TC in both sectors remains close to zero, and full fungibility is easily rejected. The effect of education SP aid decreases sharply to 0.83, which is also the size of the nearly unchanged coefficient of health SP aid. However, full fungibility is rejected in both cases. This result suggests that the conclusions from table 4, namely that the fungibility of education and health SP aid and TC is limited, are not solely driven by the particular experience of a small number of aid recipients.[29] In what follows, I continue to work with this reduced sample.

To interpret the FE coefficients in a causal way requires a potentially strong assumption of strict exogeneity. This assumption would be violated if, for instance, the allocation of education (health) SP aid and TC were partially determined on the basis of past or current values of public education (health) expenditure. In fact, table 6 contains some evidence indicating that strict exogeneity for SP aid is unlikely to hold. If a first-differenced version of equation (11) is estimated with OLS (columns 2 and 4, labeled FD), then the effect of SP aid differs markedly from its FE estimate and even becomes negative. This stark difference between the FE and FD estimates of the SP aid coefficients suggests a violation of the strict exogeneity assumption because such a violation causes both FE and FD to be inconsistent and to have different probability limits (Wooldridge, 2002, pp. 284-285). However, the effect of TC is similar in the first-differenced model. There is some evidence of a negative effect of TC, especially in the education sector, in which the hypothesis of no fungibility can be rejected at a 10% significance level, but any displacement of sectoral public expenditure is minimal. Hence, the conclusion that the fungibility of TC is limited is confirmed in the FD model. A second indication that the FE model may be misspecified emerges from a serial correlation test of the idiosyncratic errors.[30] For both sectors, I reject the null hypothesis of no serial correlation at a significance level of less than 1%. Although clustering standard errors on the recipient country should ensure that inferences are valid, the presence of a

28. These countries are Burkina Faso, Côte d'Ivoire, Eritrea, Guinea-Bissau, Guyana, Lesotho, Mozambique, Nicaragua, Papua New Guinea, Samoa, Seychelles, Sierra Leone, Tajikistan and Tonga for education and Eritrea, Guinea-Bissau, Sierra Leone, Tajikistan, and Zambia for health.

29. The elimination of outliers that are identified by either the method proposed by Hadi (1992, 1994) (hadimvo in Stata) or the method proposed by Billor, Hadi and Velleman (2000) (bacon in Stata, see Weber, 2010) yields similar results. I follow Roodman (2007) in applying these methods to the partialled-out versions of public sectoral expenditure and sectoral SP aid and TC (i.e., the residuals that are obtained from the FE regressions of these variables on the other variables).

30. Under the null hypothesis of no serial correlation, the residuals in the first-differenced model should have an autocorrelation of -0.5. Thus, a Wald test of this hypothesis can be performed to test for the presence of serial correlation in ε_{it} (Wooldridge, 2002, p. 283; Drukker, 2003). I conduct this test in Stata using the xtserial command.

TABLE 6. Disaggregated Education and Health Aid, Reduced Sample

	Public education exp.		Public health exp.	
	FE	FD	FE	FD
Education IP	0.22	0.34***		
	(0.15)	(0.12)		
Education SP	0.83**	−0.34		
	(0.34)	(0.55)		
Education TC	0.024	−0.070		
	(0.059)	(0.046)		
Education ONM	−0.25	−0.044		
	(0.24)	(0.11)		
Health IP			0.17	−0.19*
			(0.19)	(0.11)
Health SP			0.83**	−0.19
			(0.36)	(0.24)
Health TC			−0.15	−0.040
			(0.20)	(0.10)
Health ONM			0.31*	0.095
			(0.17)	(0.12)
General aid	0.027	0.00092	0.0082	−0.0074
	(0.020)	(0.015)	(0.018)	(0.011)
Support to NGOs	−0.48	−0.17	−0.055	−0.025
	(0.31)	(0.23)	(0.14)	(0.073)
Other non-education aid	0.00046	0.0049		
	(0.016)	(0.010)		
Other non-health aid			−0.019*	0.0039
			(0.011)	(0.0047)
GDP per capita	0.22*	−0.058	0.093	0.13*
	(0.12)	(0.088)	(0.071)	(0.063)
GDP per capita growth	−0.019***	−0.0079**	−0.015***	−0.011***
	(0.0045)	(0.0031)	(0.0043)	(0.0031)
Urbanisation	0.039	0.0033	0.019	0.017
	(0.045)	(0.064)	(0.025)	(0.026)
Trade	−0.0035	−0.0025	−0.0013	0.00046
	(0.0041)	(0.0037)	(0.0023)	(0.0021)
PV debt	−0.0055**	−0.0038	−0.00026	−0.00017
	(0.0025)	(0.0038)	(0.00056)	(0.00072)
Public debt service	−0.059***	−0.024*	−0.019	−0.0031
	(0.019)	(0.012)	(0.012)	(0.0057)
R^2	0.183	0.062	0.135	0.051
Hausman	0.000		0.000	
$\beta_{SP} \leq 0$	0.008	0.731	0.012	0.781
$\beta_{SP} \geq 1$	0.307	0.008	0.313	0.000
$\beta_{TC} \leq -1$	0.000	0.000	0.000	0.000
$\beta_{TC} \geq 0$	0.658	0.066	0.239	0.347
Countries	94	94	103	102

(Continued)

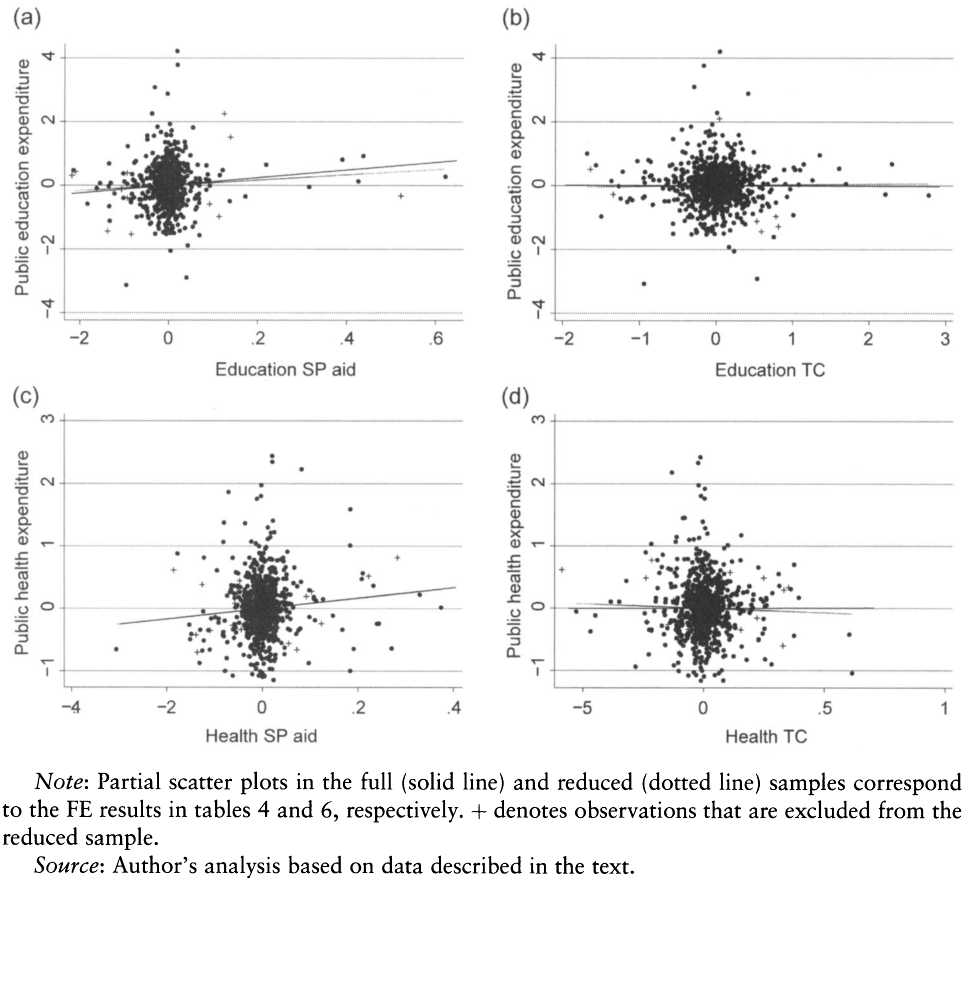
TABLE 6. Continued

	Public education exp.		Public health exp.	
	FE	FD	FE	FD
Observations	921	819	1024	912

Note: Fixed effects (FE) and first−differenced OLS (FD) results, annual data, 1990−2003, reduced sample. All regressions include time dummies, coefficients not reported. Heteroskedasticity-robust standard errors, clustered by country, in brackets. *, **, and *** denote significance at 10, 5 and 1%, respectively. In the case of FE estimation, R^2 refers to the within R^2. Hausman shows the p−value of a generalized Hausman test of the null hypothesis that η_i is uncorrelated with the regressors. $\beta_{SP} \leq 0$ ($\beta_{SP} \geq 1$) and $\beta_{TC} \leq -1$ ($\beta_{TC} \geq 0$) are p-values for the test of full (no) fungibility for sector program aid and technical cooperation, respectively.

Source: Author's analysis based on data described in the text.

FIGURE 2. Partial Scatter Plots



Note: Partial scatter plots in the full (solid line) and reduced (dotted line) samples correspond to the FE results in tables 4 and 6, respectively. + denotes observations that are excluded from the reduced sample.

Source: Author's analysis based on data described in the text.

serial correlation in ε_{it} may indicate that the model is dynamically misspecified, which would again render the FE estimates inconsistent.

Therefore, I also examine the results that are obtained when the strict exogeneity assumption is relaxed by employing a system GMM estimator (Arellano and Bover, 1995; Blundell and Bond, 1998). This estimator further enables the consistent estimation of a more general model that includes a lagged dependent variable (which removes the serial correlation in ε_{it}):[31]

$$\begin{aligned} SSP_{it} = {} & \alpha SSP_{i,t-1} + \beta_{IP} SAIDIP_{it} + \beta_{SP} SAIDSP_{it} + \beta_{TC} SAIDTC_{it} \\ & + \beta_{ONM} SAIDONM_{it} + \gamma A_{it} + \boldsymbol{\delta} \mathbf{X}_{it} + \lambda_t + \eta_i + \varepsilon_{it}. \end{aligned} \tag{12}$$

Equation (12) is estimated using a two-step system GMM estimator applying Windmeijer's (2005) correction for the downward bias in the two-step standard errors. All education (health) aid prefix code variables, support to NGOs, and trade are treated as endogenous, whereas all other variables are treated as predetermined. Time dummies are treated as strictly exogenous and are thus added to the instrument matrix without transformation. I reduce the risk of overfitting by restricting the maximum number of lags of the level variables that are used as instruments for the differenced equation[32] and by collapsing the instrument matrix, which creates an instrument for each variable and lag distance rather than for each variable, time period, and lag distance (Roodman, 2009a, 2009b). To conserve space, I do not report the system GMM results and discuss them only briefly (the full results and a more detailed discussion are available in the working paper version of this article).

The short-term effect of SP aid in both sectors is near zero but is volatile across the different instrument configurations and is estimated imprecisely. As a result, neither the null hypothesis of full fungibility nor the null hypothesis of no fungibility can typically be rejected at conventional significance levels. This volatility and imprecision carry over to the estimate of the long-term effect of education SP aid, $\widehat{\beta}_{SP}^{LR} = \widehat{\beta}_{SP}/(1 - \widehat{\alpha})$. This imprecision likely results from the lack of variation in SP aid. The effect of education TC is close to zero, and the null hypothesis of full fungibility is always strongly rejected. No fungibility cannot be rejected, and the point estimate suggests, at most, only minor displacement of public education expenditures by education TC in the short term. Given the persistence in public education expenditures, the estimate of the long-term effect of education TC is more negative (with -0.3 as the lowest estimate), but even in the long term, full fungibility is rejected and no fungibility is

31. Briefly, the GMM estimator differences equation (12) to remove the fixed effect and uses suitably lagged levels of the dependent variable and the right-side variables as instruments for the differenced equation. In addition, the system GMM estimator utilizes the equation in levels, using suitably lagged differences as instruments.

32. I examine a number of different instrument configurations, from the use of a single lag of each variable to instrument the differenced equation to the use of four lags of each variable.

not rejected. In the health sector, full fungibility of TC in the short term is also rejected across the board. In fact, health TC is found to have a positive effect, although the estimate is never significantly different from zero. The average estimated LR effect is approximately 2.6 but has a large standard error. Nonetheless, in all cases except when only a single lag of the variables is used to instrument the differenced equation, full fungibility in the long term can still be rejected. Similar long-term effects are found when a lag of TC aid is added as an explanatory variable in equation (12).

An Alternative Assessment

Finally, it is worthwhile to consider an alternative approach that allows for a broader assessment of the degree of fungibility of education and health aid while allowing for some uncertainty in the measurement of on- and off-budget aid. Beginning with (3), the estimated coefficient of health aid in (2) can be written as follows:[33]

$$\widehat{\beta} = \widehat{\beta}_{ON} w + \widehat{\beta}_{OFF}(1 - w), \tag{13}$$

with weight w

$$w = \frac{1 + \rho\sqrt{\delta}}{1 + \delta + 2\rho\sqrt{\delta}} \tag{14}$$

and with $\rho = \sigma_{ON,OFF}/(\sigma_{ON}\sigma_{OFF})$ as the correlation between on- and off-budget health aid and $\delta = \sigma^2_{OFF}/\sigma^2_{ON}$ as the relative variance of off- versus on-budget health aid. If we impose that on- and off-budget health aid have the same degree of fungibility ($\widehat{\beta}_{OFF} = \widehat{\beta}_{ON} - 1$), then we can rearrange equation (13) to express $\widehat{\beta}_{ON}$ as a function of $\widehat{\beta}$:

$$\widehat{\beta}_{ON} = \widehat{\beta} + 1 - \frac{1 + \rho\sqrt{\delta}}{1 + \delta + 2\rho\sqrt{\delta}}. \tag{15}$$

This equation demonstrates how, for given values of ρ and δ, our naive estimate of β can be used to generate an estimate ($\widehat{\beta}_{ON}$) that can be used to determine the degree of fungibility: a value of $\widehat{\beta}_{ON}$ that is close to 1 indicates that there is little or no fungibility, whereas a value that is closer to 0 suggests a greater degree of fungibility.[34] Table 7 performs this computation for total aid and for each of the 4 aid types in both sectors, beginning with the FE coefficients that are estimated in tables 3 and 4, respectively. For each variable, the

33. As in section I, I focus on the fungibility of health aid for the sake of concreteness.

34. As noted previously, in a model that includes control variables and that is estimated using a FE estimator, ρ refers to the correlation between the partialled-out versions of off- and on-budget aid, and δ refers to the relative variance of the partialled-out versions of off- versus on-budget aid.

TABLE 7. Fungibility of Education and Health Aid

		ρ						
		−1	−3/4	−1/2	0	1/2	3/4	1
(a) Education aid								
δ	1/4	−1.00	−0.25	0.00	0.20	0.29	0.32	0.34
	1/2	−2.41	−0.06	0.19	0.34	0.39	0.41	0.42
	3/4	−6.46	0.23	0.36	0.43	0.46	0.46	0.47
	1	.	0.50	0.50	0.50	0.50	0.50	0.50
	3/2	5.45	0.88	0.70	0.60	0.57	0.56	0.55
	2	3.42	1.07	0.82	0.67	0.62	0.60	0.59
	4	2.00	1.25	1.00	0.80	0.72	0.69	0.67
(b) Health aid								
δ	1/4	−0.74	0.01	0.26	0.46	0.54	0.57	0.59
	1/2	−2.16	0.19	0.44	0.59	0.65	0.66	0.67
	3/4	−6.21	0.48	0.62	0.69	0.71	0.72	0.72
	1	.	0.76	0.76	0.76	0.76	0.76	0.76
	3/2	5.71	1.14	0.96	0.86	0.83	0.82	0.81
	2	3.67	1.33	1.07	0.93	0.87	0.86	0.84
	4	2.26	1.51	1.26	1.06	0.97	0.95	0.93
(c) Education IP								
δ	1/4	−0.88	−0.13	0.12	0.32	0.41	0.44	0.46
	1/2	−2.29	0.05	0.31	0.46	0.51	0.53	0.54
	3/4	−6.34	0.35	0.48	0.55	0.57	0.58	0.59
	1	.	0.62	0.62	0.62	0.62	0.62	0.62
	3/2	5.57	1.00	0.82	0.72	0.69	0.68	0.67
	2	3.54	1.19	0.94	0.79	0.74	0.72	0.71
	4	2.12	1.37	1.12	0.92	0.84	0.81	0.79
(d) Health IP								
δ	1/4	−0.80	−0.05	0.20	0.40	0.49	0.51	0.53
	1/2	−2.21	0.13	0.38	0.53	0.59	0.60	0.61
	3/4	−6.26	0.42	0.56	0.63	0.65	0.66	0.66
	1	.	0.70	0.70	0.70	0.70	0.70	0.70
	3/2	5.65	1.08	0.90	0.80	0.77	0.76	0.75
	2	3.61	1.27	1.02	0.87	0.81	0.80	0.79
	4	2.20	1.45	1.20	1.00	0.91	0.89	0.87
(e) Education SP aid								
δ	1/4	0.21	0.96	1.21	1.41	1.49	1.52	1.54
	1/2	−1.21	1.14	1.39	1.54	1.59	1.61	1.62
	3/4	−5.26	1.43	1.56	1.63	1.66	1.66	1.67
	1	.	1.71	1.71	1.71	1.71	1.71	1.71
(f) Health SP aid								
δ	1/4	−0.16	0.59	0.84	1.04	1.13	1.16	1.18
	1/2	−1.57	0.78	1.03	1.18	1.23	1.25	1.26
	3/4	−5.62	1.07	1.20	1.27	1.30	1.30	1.31
	1	.	1.34	1.34	1.34	1.34	1.34	1.34
(g) Education TC								
δ	1	.	0.49	0.49	0.49	0.49	0.49	0.49
	3/2	5.44	0.87	0.69	0.59	0.56	0.55	0.54
	2	3.41	1.06	0.81	0.66	0.61	0.59	0.58
	4	1.99	1.24	0.99	0.79	0.71	0.68	0.66

(Continued)

TABLE 7. Continued

		ρ						
		−1	−3/4	−1/2	0	1/2	3/4	1
(h) Health TC								
δ	1	.	0.51	0.51	0.51	0.51	0.51	0.51
	3/2	**5.46**	**0.88**	**0.70**	**0.61**	**0.57**	**0.56**	**0.56**
	2	**3.42**	**1.08**	**0.82**	**0.67**	**0.62**	**0.60**	**0.59**
	4	**2.01**	**1.26**	**1.01**	**0.81**	**0.72**	**0.69**	**0.67**
(i) Education ONM aid								
δ	1/4	$\underline{-0.98}$	$\underline{-0.23}$	$\underline{0.02}$	$\underline{0.22}$	$\underline{0.31}$	$\underline{0.33}$	$\underline{0.35}$
	1/2	$\underline{-2.39}$	$\underline{-0.05}$	$\underline{0.21}$	$\underline{0.35}$	$\underline{0.41}$	$\underline{0.42}$	$\underline{0.43}$
	3/4	$\underline{-6.44}$	$\underline{0.24}$	$\underline{0.38}$	$\underline{0.45}$	$\underline{0.47}$	$\underline{0.48}$	$\underline{0.48}$
	1	.	$\underline{0.52}$	$\underline{0.52}$	$\underline{0.52}$	$\underline{0.52}$	$\underline{0.52}$	$\underline{0.52}$
	3/2	**5.47**	**0.90**	**0.72**	$\underline{0.62}$	$\underline{0.59}$	$\underline{0.58}$	$\underline{0.57}$
	2	**3.43**	**1.09**	**0.84**	**0.69**	$\underline{0.63}$	$\underline{0.62}$	$\underline{0.61}$
	4	**2.02**	**1.27**	**1.02**	**0.82**	**0.73**	**0.71**	**0.69**
(j) Health ONM aid								
δ	1/4	$\underline{-0.59}$	$\underline{0.16}$	$\underline{0.41}$	$\underline{0.61}$	**0.70**	**0.73**	**0.75**
	1/2	$\underline{-2.00}$	$\underline{0.35}$	$\underline{0.60}$	**0.75**	**0.80**	**0.82**	**0.83**
	3/4	$\underline{-6.05}$	**0.64**	**0.77**	**0.84**	**0.87**	**0.87**	**0.88**
	1	.	**0.91**	**0.91**	**0.91**	**0.91**	**0.91**	**0.91**
	3/2	**5.86**	**1.29**	**1.11**	**1.01**	**0.98**	**0.97**	**0.96**
	2	**3.83**	**1.48**	**1.23**	**1.08**	**1.03**	**1.01**	**1.00**
	4	**2.41**	**1.66**	**1.41**	**1.21**	**1.13**	**1.10**	**1.08**

Note: The entries in this table are the values of $\widehat{\beta}_{ON}$ computed according to equation (15) for different aid variables, starting from the FE coefficients estimated in tables 3 and 4. ρ is the correlation between the on- and off-budget components of the aid variable, δ the relative variance of the off- versus on-budget component of the aid variable. Bold (underlined) entries indicate that the null of full (no) fungibility is rejected at a 5% significance level.

Source: Author's analysis based on data described in the text.

entries in the table calculate $\widehat{\beta}_{ON}$ for different values of the relative variance of off- versus on-budget aid in the aid type considered (δ, ranging from 1/4 to 4) and the correlation between its on- and off-budget components (ρ, ranging from -1 to 1). A bold (underlined) entry indicates that the null hypothesis of full (no) fungibility can be rejected at a 5% significance level.

After partialling out the fixed effects and the control variables, the correlations between the four different aid types (IP, SP, TC and ONM) are a useful indication of the most plausible values of ρ for total education and health aid. In both sectors, these correlations are close to zero. The most negative correlation is between health SP aid and TC (-0.15), and the most positive correlation is between education TC and ONM (0.14). Hence, ρ is not expected to be far from 0. Meanwhile, it is very likely that most of the variation in total education and health aid is driven by off-budget aid (implying $\delta \geq 1$). Technical assistance, which I have argued is almost entirely off-budget, dominates the variation in health and, especially, education aid (see table 1), while there is some evidence to suggest that the other non-program components are also not well captured in the budgets of recipient governments (see section III). Hence, the entries in the bottom four rows of tables 7a and 7b are the most plausible. Especially in the health sector, these entries indicate a low degree of fungibility. For health aid, the null hypothesis of no fungibility is never rejected for $\delta \geq 3/2$; even for a δ value as low as 1/2, a fairly low degree of fungibility is found for most values of ρ.

With regard to the aid types, even under the assumption that SP aid is completely on-budget, its estimated FE coefficient in table 4 for both sectors implies low fungibility. Hence, it is not surprising that this conclusion is confirmed in tables 7e and 7f.[35] Tables 7g and 7h relax the assumption that TC is completely off-budget. In almost all cases, the null hypothesis of full fungibility can still be rejected, and most entries suggest limited fungibility. For health TC, the null hypothesis of no fungibility is never rejected. The vast majority of entries in table 7j indicate a low degree of fungibility of health ONM aid, with few rejections of the null hypothesis of no fungibility. The degree of fungibility is higher for education ONM aid and is more difficult to assess. Both null hypotheses are typically rejected; thus, the results suggest partial fungibility, but the exact degree of fungibility depends on the relative variation of off-budget versus on-budget aid, which is difficult to determine. The discussion in section III suggests that aid projects (IP) are frequently not captured in the budgets of recipient governments. Even when the relative variance of off- versus on-budget IP aid is 1 or slightly below 1, the entries in tables 7c and 7d again indicate fairly low degrees of fungibility, especially in the health sector. Hence, unless ρ is very negative, we would only be comfortable concluding that IP is mostly fungible if we believe that the variance in off-budget IP is substantially lower than the variance in on-budget IP.

V. Conclusion

This paper presents new empirical evidence to provide insight into the difficult issue of foreign aid fungibility. I construct data on earmarked education and health aid disbursements that also distinguish between on- and off-budget components of aid. Sector program aid measures on-budget aid, whereas technical cooperation proxies for off-budget aid. I illustrate how a failure to adequately address the presence of off-budget aid may have biased previous estimates of foreign aid fungibility.

Overall, I find little evidence that aid is fully or even largely fungible; rather, most point estimates suggest limited fungibility. In both sectors, technical cooperation leads to, at most, a small displacement of a recipient's own public spending. This effect is estimated relatively tightly, especially in the education sector. Thus, the results suggest a genuine effect rather than merely noise in the

35. For SP aid, I consider only $\delta \leq 1$, and for TC, I consider only $\delta \geq 1$.

data. The effect of technical cooperation is robust across a range of models, whereas the effect of sector program aid is more volatile. In a static panel data model, fixed effects results suggest an approximately one-to-one correlation between sector program aid and public sectoral expenditure, which is robust to a large number of specification changes. However, when system GMM is used to estimate a dynamic model, the effect of sector program aid is imprecisely estimated. Thus, no firm conclusions can be drawn with respect to the fungibility of sector program aid.

Therefore, the result of limited fungibility for education and health aid specifically pertains to technical cooperation. Because technical cooperation is the dominant modality in both sectors, however, it plays a large role in determining the overall degree of fungibility of earmarked education and health aid. The extent to which investment projects and other aid are on- or off-budget is more uncertain, making it more difficult to determine the degree to which these projects are fungible. However, the analysis in section IV suggests that unless we believe that the variance of the on-budget components of investment projects and other aid dominates the variance of their off-budget components, both types of aid are far from fully fungible.

The lack of fungibility may be a consequence of effective donor conditionality. If donors are able to monitor the spending of recipient governments, then they may be able to credibly enforce the condition that aid adds to the resources that are spent in the targeted sector. An additional reason for the low degree of fungibility primarily applies to technical cooperation and is less applicable to other aid types. This explanation is the observation made by Gramlich (1977) that heterogeneity in government expenditures may contribute to reduced fungibility. To the extent that governments in developing countries spend few resources on the type of goods and services that are provided by technical cooperation, it becomes impossible to significantly reduce this class of expenditure because these expenditures rapidly approach the lower bound of zero. If the substitutability between different types of expenditures in a recipient government's utility function is also limited, then low fungibility for technical cooperation may ensue. Finally, a lack of information on the part of a recipient government, which is particularly relevant for off-budget aid, may also reduce the degree of fungibility that is observed in practice.

From the donor perspective, the results in this paper suggest that the costly effort associated with earmarking (e.g., monitoring costs) may not be futile. From the perspective of the population in a recipient country, the limited fungibility of education and health aid can be perceived as a positive result if we believe that better education and health have positive consequences for human welfare. However, this positive interpretation persists only if the aid in these sectors effectively produces valuable outcomes. Moreover, if the low fungibility of off-budget aid arises because a recipient government is not fully aware of this aid, then any positive effects of non-fungible off-budget aid must be balanced against the possible deleterious effects on government capacity and ownership that are incurred when channeling funds outside of a budget. In general, not a great deal is known about the normative consequences of fungibility (for papers that look at this issue, see McGillivray and Morrissey, 2000; Pettersson, 2007a, 2007b; Wagstaff, 2011), and this constitutes an important area for future study.

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