The Foreign Aid Effectiveness Debate: Evidence from Malawi

Rajlakshmi De and Charles Becker

Abstract:

Understanding the role of foreign aid in poverty alleviation is one of the central inquiries of development economics. To augment past cross-country studies, this paper offers a first step toward addressing the absence of disaggregated estimates of the allocation and impact of foreign aid. Newly geocoded aid project data from Malawi are used in combination with multiple rounds of living standards data to assess the allocation and impact of health aid, water aid, and education aid. Allocation is modeled using living standards variables, geographic indicators, and other aid bundling. Significant, positive effects of health aid on decreasing disease severity and of water aid on decreasing diarrhea incidence were estimated through both IV and PSM difference-in-differences approaches. An appropriate instrument for education aid could not be determined, but propensity score matching methods indicate a potential positive effect of education aid on school enrollment. Different aid donors' allocation behaviors are also assessed. The aid impact results suggest that a sub-national framework provides sufficient granularity for detecting the impacts of foreign aid on poverty alleviation in Malawi and that policymakers and governments should use geographic living standards information to inform future aid allocation.

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AidData

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AidData – a joint venture of the College of William and Mary, Development Gateway and Brigham Young University – is a research and innovation lab that seeks to make development finance more transparent, accountable, and effective. Users can track over $40 trillion in funding for development including remittances, foreign direct investment, aid, and most recently US private foundation flows all on a publicly accessible data portal on AidData.org. AidData's work is made possible through funding from and partnerships with USAID, the World Bank, the Asian Development Bank, the African Development Bank, the Islamic Development Bank, the Open Aid Partnership, DFATD, the Hewlett Foundation, the Gates Foundation, Humanity United, and 20+ finance and planning ministries in Asia, Africa, and Latin America.

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1. Introduction

This paper offers a first, small step in addressing the absence of disaggregated estimates of the impact of aid on beneficiaries, and in the process suggests an approach that may lead to more systematic assessments of at least a substantial portion - project aid with clearly measurable outcomes - of foreign aid. In contrast to cross-country studies, this sub-national approach stems from the need to assess aid on living standards indicators that are likely to be directly targeted by aid projects, such as disease reduction and increases in educational attainment. Employing sub-national variation also should reduce the vast unobservable differences that may be present in cross-country variation. In addition, using disaggregated variation within Malawi allows a less tenuous assumption of who is treated versus untreated by aid, compared with the assumptions in studies without this level of granularity.

Newly geocoded aid data for Malawi allow for sub-national variation. Aid projects are segmented by aid type and geographic administrative boundary and their allocations and impacts within Malawi are explored through living standards indicators such as disease incidence and severity. We also discuss donor allocation strategy. We find non-random aid allocation models as well as differences between whether donor agencies disburse aid to areas based on the level of poverty. We find evidence for positive impacts of all three aid types, suggesting that aid plays a useful role in poverty alleviation when it is assessed on the living standards variables targeted directly by the aid type and when the impact is measured using sub-district granularity. These impacts are economically significant. The average population for our geographic units is 36,000, and our local average treatment effects imply that a health aid project of $100,000 increases the economic productivity of one of these units by 33,000 days of increased economic activity as a result of less disease burden. We also estimate significant economic impacts of water aid on water-related diseases – a typical $200,000 project leads to 150 fewer cases of diarrhea, and most likely reduces other water-related illnesses and provides other lifestyle benefits through improved sanitation. We further find that an education project of $150,000 leads to an additional 300 people attending school at some point in their lives.

Many studies have explored the questions of aid allocation and impact using aggregate, cross-country variation. UNU-WIDER (2014) uses meta-analysis of an array of recent macro-economic studies to find consistent support that aid has a positive average effect on growth. They assert that a sustained aid inflow of 10 percent of GDP can be expected to raise growth rates by approximately one percentage point on average (p.15). However, the literature skeptical of the value of foreign aid, at least to the poor in recipient countries, is voluminous, damning, and replete with entertaining stories. The most prominent critic today may well be William Easterly (2002), but the literature began more than half a century ago, virtually concurrent with the first large scale aid projects (in particular, see Dumont, 1966). Feyzioglu et al. (1998) finds substantial and possibly full fungibility in foreign aid in certain sectors, though this paper is based on data from 1970-1990; since then, donors have imposed more restrictions on recipients. Brautigam and Knack (2004) emphasize the increased dependency that results from foreign aid, which is particularly relevant because Malawi has the second highest ODA as a share of government expenditure in their sample of 27 selected sub-Saharan African countries in 1999. Jackson (2014) uses the response of shared donors to aid neighbors as an instrumental variable and finds no long-run effects of aid on national accounts. Similarly to Brautigam and Knack, Page (2012) warns about the effect of aid on retarding structural change. Yet, establishing that there are many ill-conceived and ill-administered projects or that there is massive corruption is not the same as establishing that targeted foreign assistance projects as a class have zero or negative impacts.

The problems with aggregate estimates are numerous. Obviously, they are very coarse: small projects will be drowned in measurement noise, even if in fact they are highly successful. Consequently, aggregation levels must be high - but, clearly, not all project aid is remotely alike, nor does all aid have immediate impacts: the idea that education projects will affect current or near term GDP is completely unfounded, for example. And GDP is often badly measured. Indeed, even relatively well measured GDP in low-income African, Asian, or Latin American countries is unlikely to respond to gradual health improvements. Not only do these improvements tend to lead to a diffuse set of practices, only a few of which will be captured in near term GDP, but they are likely to be drowned out by fluctuations in weather, political disturbances, traded goods' prices, and a myriad of other factors. A finer lens is needed.

At the other end of the spectrum, there has been an explosion of carefully constructed randomized control trials (RCTs) that examine individuals' responses to specific interventions and, when well conducted, clearly delineate different pathways. Examples of health project assessments include Ashraf and Shapiro (2010; focusing on the use of water purification chemicals in Zambia), Björkman and Svensson (2009; analyzing community-based monitoring of health staff in Uganda), Cohen and Dupas (2010; analyzing demand for insecticide-coated mosquito bednets in Kenya), and Kremer and Miguel (2007; analyzing use of deworming drugs in Kenya).

In these cases, the question to arise concerns the extent to which one can generalize from the findings. Some projects have huge returns; others have virtually no return at all. Failures can occur because the interventions were ill-conceived, because they were unsustainable, or because they failed to account for a myriad of interaction effects. However, while demonstrating both potential and also that returns would soar if all projects were on the frontier, the literature gives little sense as to whether there is a high return to an average project. Doing that requires abstracting over idiosyncratic differences of individual projects and assessing the average treatment effects of large numbers of interventions.

An intermediate lens is the topic of this paper. Our goals are modest. We seek to identify whether impacts can be found in a setting of considerable need, and in which effects are fairly readily measurable and attributable. We do not attempt to estimate a social return to all foreign aid, or even to targeted assistance projects in one country during one period, and we do not attempt to estimate losses due to corruption, incompetence, failure to follow through or to provide for maintenance, or due to poor selection. Rather, we simply seek to establish a lower bound – that some positive outcomes do appear – and hint at possible gains due to apparent differential behaviors by different donors.

Malawi is the country of interest in this research for several reasons. It is the first country to have comprehensive, geocoded data on aid projects. This dataset is compiled by AidData and is publicly available at aiddata.org[1]. Malawi received 5.3 billion dollars in foreign aid during the aid project data's time period of 2004 to 2011. The nation's population and GDP in 2011 were 15.4 million and 5.8 billion dollars, respectively. These figures reflect a relatively small population for a developing nation and aid inflows over eight years that are almost equivalent to current annual economic output. Together with substantial population homogeneity and a very large number of administrative units, the combination of high

[1] Peratsakis, Christian, Joshua Powell, Michael Findley, and Catherine Weaver. 2012. Geocoded Activity-Level Data from the Government of Malawi's Aid Management Platform. Washington D.C. AidData and the Robert S. Strauss Center for International Security and Law.

aid and low population makes Malawi an appealing country in which to attempt to detect an effect of aid from sub-national variation across time. For this project, aid is investigated through three different sub-categories of aid: health aid, water and sanitation aid (which is referred to simply as water aid), and education aid. The geographic variation in aid allocations is based on the administrative boundaries of the 216 Traditional Authorities (TAs) monitored by the National Statistical Office of Malawi. The aid project data were captured in the Malawi Aid Management Platform (AMP) and geocoded by AidData and CCAPS. Living standards data were taken from the IHS2 and IHS3 rounds of the Malawi Living Standards Measurement Study (LSMS) from the National Statistical Office of Malawi and the World Bank[2]. The timing of these two rounds of living standards data enclose the time period of aid projects, providing before and after snapshots of living standards conditions.

By employing the sub-national model, this study contributes empirical results for the allocation and impact of health aid, water aid, and education aid. The allocation models are found to vary greatly in terms of the relationships with living standards and geographic indicators. However, all aid types have positive relationships with allocations of other types of aid, which we use as evidence of aid bundling when it occurs by a single donor agency[3]. By comparing t_0 and t_1 conditions between those untreated and treated by aid,

[2]Malawi 2010-2011 Third Integrated Household Survey and Malawi 2004-2005 Second Integrated Household Survey, conducted as part of the Living Standards Measurement Study – Integrated Surveys on Agriculture (LSMS-ISA) project, executed by Malawi National Statistics Office, and available through the World Bank.

[3]A limitation of our study is that our data are restricted to projects funded under official development assistance (ODA) from bilateral and multilateral donors and therefore exclude aid from private voluntary organizations (PVOs). This is an inherent limitation of the study that cannot be easily addressed. It is possible that some official donors incur costs in order to reduce expenditure burdens for PVOs and that apparent inefficiency in selection and modest returns simply reflects measurement error on our part, since we are not capturing the whole benefit. On the other hand, given the very large extent of ODA in Malawi - more than 10 percent of GDP - it seems unlikely that PVOs are comparable in scale, especially on a net basis. Many PVOs receive a large share of their funding by bidding on contracts to implement ODA projects, in which case the AidData database will include the projects. We do not have a general figure for the importance of subcontracting, but do note a painstaking effort by Dreher et al. (2012) that finds that German PVOs involved in development assistance receive 39 percent of their funding from public sources. Also noteworthy is the finding by Koch et al. (2009) that NGOs tend to follow the location choices of their official supporters ("backdonors") and also to cluster with other NGOs. Thus, in a small country like Malawi, it seems likely that most ODA ends up being coordinated with religious bodies.

We can think of no reason to believe that ODA projects are modified to make *independent* PVO projects "look good". Nonetheless, an obvious extension to the work here would involve identifying privately funded development projects in these areas. A second, comparably thorny issue is that we do not consider possible coordination by different PVOs. The authors' (Becker's) field experience is that official donors tend to be mutually supportive (when not engaging in turf or prestige wars) and make an effort not to duplicate efforts the impact regressions employ a differences-in-differences (D-i-D) setting using two complementary methods – instrumentation and propensity score matching – that account for the endogeneity of aid allocation. Though both forms of D-i-D identification, instrumentation and PSM utilize different mechanisms for reducing endogeneity bias, serving as robustness checks of the impact estimates. Health aid is found to have a positive impact on decreased disease severity and water aid is found to have a positive effect on decreased diarrhea occurrence. Propensity score matching also provides evidence that education aid increases school exposure, but the lack of an appropriate instrument for education aid prevents use of the IV method for education aid. The overall importance of these empirical results is that not only are these aid types beneficial to poverty alleviation, but that the sub-national granularity in Malawi and using living standards data is a useful framework for modeling aid.

We also assess whether aid is more effective in areas with lower levels of development. We then analyze different donor organizations - African Development Bank, Australian Aid, EU, GIZ (Germany), Iceland, Ireland, JICA (Japan), KFW Bankengruppe, NORAD (Norway), DfID (UK), USAID, and the World Bank. In this donor analysis, we assess which organizations allocate based on need, including after controlling for the possibility of aid project bundling. Knack et al. (2011) also uses the AidData database to assess the performance of 38 different bilateral and multilateral aid donors, but on a national level. They use 18 specific measures of selectivity (recipient poverty and governance), alignment (whether or not aid is tied, its predictability, coordination with national strategies, and assessment practices), harmonization, and specialization measures that capture fragmentation. In their study, in terms of donors most active in Malawi, the World Bank rates highest, followed by the UK, Norway, Australia, African Development Bank, Germany, Japan, and, near the bottom, the US. Their poor rating of the US is supported by Easterly and Williamson (2011), who assess the performance of 43 different aid donors. For donors in Malawi, their rankings are as follows, with scores from best to worst: African Development Bank (71), UK (70), Japan (63), Germany (62), World Bank (61), Australia (59), USA (45). Curiously, from our perspective, efficiency at the country level may have little relationship to local efficiency of project design, siting, and implementation within a

by deferring to first-comers in terms of locations chosen and project types. However, complete coordination of activities seems implausible: local aid coordinators report first to superiors back at headquarters, and end up implementing projects that are consistent with a donor's projects elsewhere and with its prevailing (though oft-changing) ideology.

country. Using our disaggregated method, USAID is perhaps the best overall in terms of selecting need-based sites within Malawi. Perhaps this divergence reflects the greater political considerations of a super-power coupled with greater capacity because of the size of the US effort.

The rest of the paper is organized as follows: In Sections 2 and 3, previous research is discussed and a theoretical framework is established. Sections 4 and 5 describe the data and empirical strategies. Finally, concluding remarks are provided in Section 6.

2. Previous Research

As noted in the introduction, the current literature lacks a sub-national model that predicts aid allocation or detects impact. However, there is an expansive aid literature studying both allocation and impact.

Allocation models have studied cross-country variation to determine the importance of economic needs, policy performance, political considerations, and strategic interests in explaining aid variation. Alesina and Dollar (2000) find that foreign aid is dictated as much by political considerations as by recipient economic needs and policy performance. They find that colonial past and political alliances are major determinants of foreign aid, and on the margin, countries that democratize receive more aid. Alesina and Dollar also find significant differences between donor countries in their aid allocation. The Nordic countries respond more to economic incentives, like income levels, good institutions and openness. France gives to former colonies tied by political alliances, without much regard to other factors, including poverty levels or choice of politico-economic regimes. The United States' pattern of aid giving is vastly influenced by interests in the Middle East.

In contrast, Lumsdaine (1993) concludes that the donor country's humanitarian concern forms the basis of support for aid, not the donor's political and economic interests. He introduces the concept of moral vision wherein the donor nations view themselves as interdependent with recipient nations. Maizels and Nissanke (1984) use the notion of "strategic foreign policy" to explain patterns of bilateral foreign aid. They also find allocation models to be very different for bilateral and multilateral aid. Multilateral aid appears to compensate for shortfalls in domestic resources. In contrast, bilateral aid flows are consistent with a model in which aid serves donor political, security, investment and trade interests. Kim and Oh (2012) focus their study on South Korea's aid allocation to 154 recipient countries, and find that South Korea provides more aid to higher-income developing countries with higher growth rates. They also find that the relationship between per capita incomes of the recipient country are negatively correlated with aid allocation only for middle-income or lower-middle-income group recipients and is correlated positively for the rest. No significant differences over decades or political regimes are found.

Understanding aid's role in poverty alleviation is one of the most central inquiries for development economists. Despite the importance of the question, economists have not agreed on whether aid policies are useful or destructive in developing economies. Burnside and Dollar (2000) conduct a cross-country analysis of the role of good economic policies in aid effectiveness and find that aid has a positive impact on per capita growth in developing countries with good fiscal, monetary, and trade policies but has little effect in the presence of poor policies. They employ a modified neoclassical growth model that includes foreign aid receipts. Their policy variables are proxies constructed from the budget surplus, the inflation rate, and an openness dummy developed by Sachs and Warner (1995). Rajan and Subramanian (2005) also employ cross-country data and use instrumentation to correct for the endogenous allocation of aid to poorer countries, but unlike Burnside and Dollar (2000), they do not find evidence for an effect of aid inflows on national economic growth, even when economic policies are good. An issue not addressed in this paper is the impact of macroeconomic support (usually tied to fiscal and monetary reforms) as we focus on a single country; the absence of a visibly positive outcome is highlighted in Przeworski and Vreeland (2000).

The two investigations by Burnside and Dollar (2000) and Rajan and Subramanian (2005) highlight the large extent of disagreement with regard to aid impact when studied through cross-country comparisons. In part, this may be because of the difficulty of detecting impacts on economic growth rates, which represent many more facets of the economy than the bottom of the social pyramid targeted (in principle) by most aid projects. Moreover, foreign assistance is a multi-dimensional vector with potentially long and various lags, further obscuring causal links.

Collier (2006) also reviews the effectiveness of aid, and in particular argues that in theory the impact of aid could be greater or smaller than windfall gains, due, in particular, to an unanticipated increase in the price of mineral exports. He finds evidence for the oil curse and that, while aid has all sorts of problems, it does tend to have real, positive effects on average. Nonetheless, Collier acknowledges both the popular sentiment that aid is useless, and the more common academic perspective (with empirical backing) that aid is subject to diminishing returns; he cites 8 percent of GDP as ”the point at which it ceases to contribute to growth.”

In contrast to cross-country studies, randomized control trials have been able to focus more on development outcomes and living standards rather than economic growth. In addition, their causal interpretation is more direct due to their inherent randomized design. This method has been employed to study various development questions surrounding issues such as hunger, savings, consumption, and decisions surrounding health and employment; examples from health studies are noted in the preceding section. As a variation on randomized control trials, Miguel and Kremer (2004) evaluate a Kenyan project that treated intestinal helminthes, including hookworm, roundworm, whipworm, and schistosomiasis by using the fact that the program was randomly phased into schools. They estimate the overall program's effects to be that it reduced school absenteeism by one-quarter and was far cheaper than alternative ways of boosting school participation. Because they used the random phasing in, rather than a randomized control trial, they were able to find the effects of externalities as well, such as improved participation among neighboring schools, also a feature of Kremer and Miguel (2007).

A study by Baranov, Bennett, and Kohler (2012) investigates the indirect impacts of aid projects in Malawi. In particular, they investigate the impact of antiretroviral therapy (ART) on the indirect variables of the community's perceptions of mortality risk, mental health, and agricultural labor supply and output. Employing a difference-in-difference identification strategy, they find that the ART availability substantially reduced subjective mortality risk and improved mental health in rural Malawi. However, their study lacks a selection model, and hence makes the assumption that ART allocation is random or exogenous. Furthermore, because their study attempts to answer a question of indirect impact but only analyzes ART as the exogenous project, there may be a problem of omitted variable bias. Much of the impact they find could be attributed to other simultaneous aid projects, such as other health clinics or rural development programs that may influence subjective mortality risk and labor force participation. As with RCTs, this study also suffers from narrowness: even if the ART project is effective, one cannot generalize that finding to broader aid measures.

Our findings of positive impacts are surprising in many ways. Thirkeldsen (1988) provides a detailed analysis of many foreign aid-funded rural water supply projects in Tanzania. A central point is that there is a tradeoff between delivering projects quickly and ensuring community buy-in and ultimate sustainability. Furthermore, while aid agencies fund wells, piping, schools, clinics, roads, and equipment, in many cases they make no provision for future maintenance; nor do they ensure that the recipient communities or governments have the capacity to sustain these projects. As Thirkeldsen (1998:15) concludes, "schemes often cease to function soon after being handed over to their users and the Tanzanian authorities." In short, aid may fail to have an impact because it is poorly designed, poorly located, fails to meet recipient demands, crowds out private supplies, and because it is not designed to be sustainable. Given the range of pitfalls, it is perhaps surprising that any projects are even slightly successful. From this gloomy perspective, the high reforms identified for example by Björkman and Svensson (2010) are likely anomalous rather than typical.

Of these limitations, the most difficult to resolve in the context of a low-income country is that between running fewer projects slowly but while achieving community involvement, which is not a guarantee, and the rapid construction and delivery of projects to meet dire needs. While early development projects were of the latter "control-oriented" (in Thirkeldsen's terminology) type, and have been resoundingly criticized, the "adaptive approach" is unlikely to yield quick results. Our empirical work does not identify different approaches, but it does allow us to distinguish among different funders, and hence (in future work) could shed light on the effectiveness of different styles.

It is impossible to assess the effectiveness of investment projects, or of policy measures on economic growth more broadly, without focusing on the quality of governance. It is a theme that permeates Easterly (2002), Calderisi (2006), Martens et al. (2002), and thousands of World Bank and other international financial institution, bilateral aid organization, and NGO documents. As these authors show, governance quality boils down to incentives. In recent decades, incentives and transparency in public sector management have improved in many – likely a large majority – of developing and transition economies. If this is indeed the case, then it seems likely that specific projects that might have failed in the 1980s will be found to alter health, productivity, and education outcomes in the 2000s.

Thus, the development aid skepticism of the 1990s and early 2000s may be misplaced: by learning from the failures of earlier projects, and emphasizing key complementary, necessary conditions, contemporary projects may lead to far more positive outcomes.

However, an important implication of this literature is that identifying target sites based on need may not be optimal. Extremely needy but poorly governed districts may be less appropriate than less needy but well-governed districts. Still, if aid is visibly linked to governance, in the longer run aid itself may encourage improved governance in lagging regions. While we cannot assess the strategic effectiveness of linking aid to governance, to some degree we can assess the link between need and impact of outcomes given the way in which aid is currently distributed.

3. Theoretical Framework

Identifying the impact of foreign assistance is not a straightforward task. At the subnational level, we must search for projects that have (a) reasonably immediate, measurable outcomes; (b) that do not simply displace private initiatives or domestic government spending; (c) that are not confounded with other projects; and (d) that are provided selectively rather than ubiquitously. Furthermore, even if one does find candidate items that meet these criteria, the task of identifying the manner in which regions were selected for treatment then emerges.

Schematically, consider a model with $t = 1, 2, 3$ three periods; three types of exposure e (e^1 = private sector or NGO treatment; e^2 = government treatment; e^3 = foreign aid); $n = 1, ..., N$ different types of projects; and $r = 1, ..., R$ regions. Projects have, in principle, measurable impacts $I_{e,n,r,t}$. Then the ideal project m is one in which:

- $I_{e,m,r,1} = I_{e,m,r,2} = 0$ (no project in pre-treatment baseline period; project in period 2; measurable impact if any in period 3).

- For some $r^* \in \{R\}, I_{e,m,r^*,1} = I_{e,m,r^*,2} = I_{e,m,r^*,3} = 0$. That is, some regions must be untreated by foreign aid.

- For some $\eta \in \{e_1, e_2\}, I_{\eta,m,r,1} = I_{\eta,m,r,2} = I_{\eta,m,r^*,3} = 0$. That is, regions treated by foreign aid must not be exposed to treatment by other agents (or, strictly, to non-ubiquitous treatment by these agents).

- For other non-ubiquitous treatments $n \neq m \in \{N\}$, $\frac{dI_{e=3,m,r,t=3}}{dI_{e,n,r,t=3}} = 0$. That is, there are no confounding effects from other projects, whether funded by foreign aid or another source.

- For other regions $\rho \neq r \in \{R\}$, $\frac{dI_{e=3,m,\rho,t=3}}{dI_{e,m,r,t=3}} = 0$. That is, there are no spillover effects from foreign aid projects into other regions.

It is fairly easy to exclude several types of financial assistance according to these criteria. First, any aid that does not directly raise physical or human capital (including health) is excluded: this would include humanitarian relief aid, other food aid and donations of consumption items, military aid, and direct government budgetary aid or balance of payments assistance. Also excluded are longstanding projects that involve multi-year expenditure commitments. Not only are impacts difficult to determine; it is also possible that the effects of increased local hiring may confound the aid impact. Programs that are national in scope – immunization programs, electricity grids, or free textbook programs, for example – must be excluded as well since there is insufficient regional variation. Large infrastructure projects (airports, dams) also are poor choices both since they are few in number and since there are likely to be spillover effects. Policy or technical assistance also fails the tests above since, if successful, all regions are treated.

Given this long list of excluded forms of aid, what might remain? The answer will depend on a country's level of economic development and the nature and efforts of its government and NGOs. The ideal government, from the standpoint of finding an impact, is one that ensures domestic stability but is otherwise fairly indifferent to the welfare of its populace, and which is deeply hostile to both domestic and foreign NGOs. Kleptocracies and military juntas spring to mind: Myanmar under the generals and before the democratizing reforms, Zimbabwe, and Turkmenistan are all plausible candidates. Very poor countries are generally good candidates, and Malawi fits this bill, though its government's concern for rather than indifference to the welfare of its citizenry is problematic for identification purposes.

However, the possibility of expenditures in similar areas by NGOs or national governments is only a modest problem, especially in very poor countries, for two reasons. First, the bias is generally downward – against finding an impact. Second, desperate fiscal situations often lead governments to abandon entire sectors to NGOs and international donor organizations. This is particularly true in the case of capital expenditures – insofar as governments are involved, they will tend to focus on meeting recurrent needs. Moreover, especially in poor, small countries, international donors and NGOs (both domestic and international) tend to collaborate. Indeed, many aid projects are funded by bilateral or multilateral donors, but are contracted to NGOs.

The sorts of projects for which state abandonment in very poor countries and international donor-NGO collaboration are most readily observed include health and education projects. These are the subjects of our study. Water and health projects tend to be dominated by external funders, have overwhelmingly local effects, tend to have fairly immediate post-project impacts, and involve little displacement. Outcome measurement is also straightforward. The same conditions hold, with only slightly greater leakage and government competition, for capital projects in education.

In response to concerns over the problem of fungibility, Collier (2006) notes that when aid is very large relative to government budgets as is often the case in Africa, fungibility is reduced at least at the margin - once government funding of the development budget has fallen to zero, as is the case in some countries, there is simply no further scope for fungibility. This is applicable in Malawi, where aid is a large share of the economy.

There are many policies that can have positive effects that are not measured. Agricultural production and marketing programs may be chief among these, but there are others as well. The key problem is that there are likely many necessary conditions C_m for $m = 1, ..., M$ to achieving a measurable boost in an economic objective O, where these conditions are met in ascending order $1, ..., M$. There are likely far fewer (or no) sufficient conditions S_n for $n = 1, ..., N$ to achieving a measurable boost. Thus, while $\frac{\partial O}{\partial S_n} > 0$ and $\frac{\partial O}{\partial C_m} > 0$, conditional on other necessary conditions being met one will empirically observe $\frac{\partial O}{\partial C_m} = 0$ for $m < M$. Yet, it is unreasonable to argue that only activity M and the S_n are useful. Consequently, the findings in this paper identify a lower bound – there may be many more types of aid that are extremely valuable (Commins, 1998).

4. Data and Background on Aid to Malawi

Malawi is one of the countries in the World Bank's International Development Association (IDA), which provides assistance for the world's poorest countries. The Country Policy and Institutional Assessment (CPIA)[4] rates IDA countries' economic management, structural policies, and public sector management. Using the 2013 ratings, we can determine how Malawi's administrations compare to other IDA nations. For example, Malawi ranks poorly in the business regulatory rating, which assesses the extent to which the legal, regulatory, and policy environments help or hinder private businesses. Of the 81 countries in the IDA sample in 2013, only 6 rated lower than Malawi (Eritrea, Angola, Central African Republic, Micronesia, Timor-Leste, and Zimbabwe). In terms of public sector management, Malawi ranks in the median of the IDA sample. The public sector management rating involves property rights and rule-based governance, quality of budgetary and financial management, efficiency of revenue mobilization, and transparency and corruption in the public sector. In terms of property right and rule-based governance, which measures the extent to which private economic activity is facilitated by an effective legal system, Malawi ranks ahead of 57 (of the 81) countries in the sample. Overall, when considering all 20 of the various ratings available through the World Bank, Malawi falls slightly below the median of the IDA countries; 52 of the 81 countries receive better scores.

This project is possible because of recent advancements in data collection and geocoding. Two important sources of data are used: aid project data from aiddata.org[5] and living standards data from the National Statistical Office of Malawi. Both sources have a high level of geographic specificity. The aid project dataset is based on information captured in the Malawi Aid Management Platform (AMP), hosted by the Malawi Ministry of Finance. In total, projects from 30 donor agencies were geocoded for 548 projects between 2004 and 2011, representing 5.3 billion dollars in total commitments. This dataset represents the first effort to sub-nationally geocode all donors in a single partner country, making it essential for our analysis. However, the aid data are not free from limitations.

[4] Country Policy and Institutional Assessment, World Bank Group, http://data.worldbank.org/data-catalog/CPIA

[5] Peratsakis, Christian, Joshua Powell, Michael Findley, and Catherine Weaver. 2012. Geocoded Activity-Level Data from the Government of Malawi's Aid Management Platform. Washington D.C. AidData and the Robert S. Strauss Center for International Security and Law.

The aid project data contain missing values for some geographic coordinates, and some projects are geocoded but have locations in multiple traditional authorities with only one value for cumulative disbursement of aid. Our methodology has been to exclude projects without any geographic information, because the empirical analysis is entirely geography-dependent. With regard to the cumulative disbursement figure for projects spanning multiple TAs, we have made an estimate of proportional project allocation by weighting aid project allocation by TA population size. The assumption inherent with this method is that if a project spans two TAs, with one having twice the population as the other, and the data do not give detail about how it is divided, our analysis treats the aid as allocated with two-thirds to the large TA and one-third to the smaller TA because it has one-third of the total project population. Though this assumption is required, it should not pose major problems because there are many TAs that did not receive any aid. That means the analysis is very much driven by a binary presence or absence of aid, and exact allocations among the TAs that received aid are unlikely to dramatically alter findings. Insofar as there is a bias, these assumptions work against finding an impact.

Living standards data were provided by the National Statistical Office of Malawi and the World Bank. In particular, two rounds of the Living Standards Measurement Survey (LSMS) are used: the 2nd Integrated Household Survey (IHS2) from 2004-2005 and the 3rd Integrated Household Survey (IHS3) from 2010-2011. The living standards data were already coded by TA boundaries. The survey samples for IHS2 and IHS3 were drawn using a stratified sampling procedure and included 11,280 households and 51,127 individuals, and 12,271 households and 59,251 individuals, respectively. We rely on the 2008 Malawi Census for TA population statistics.

As noted, the per capita aid measurements are calculated on the TA level. These TA-wide figures for aid per capita are merged with living standards data from IHS2 and IHS3 based on TA boundaries. The amount of living standards data available through the IHS surveys varies across TAs. The number of individuals surveyed in a particular TA ranges from 55 to 1163. Because of variation in data comprehensiveness, we estimate two versions of the analysis whenever possible, which we refer to as the weighted and unweighted specifications, abbreviated as *w* and *u*, respectively. In the weighted version, the analysis is conducted on the survey-level unit of analysis, such that TAs with more data observations receive greater weight in the regressions. For the unweighted specification, we collapse the data to one observation per TA. We use both weighted and unweighted versions whenever possible, but Propensity Score Matching methods are not suited for the unweighted form due to small sample size.

Figure 1 shows the geographical mapping of all aid projects into the 216 TA boundaries. We provide variable definitions and descriptive statistics of both rural and urban areas in Table 1. Using our aid dataset, average per capita health and water aid allocations are about $8 during the time period of investigation. Education aid per capita is about $4. There is wide variation; about two-thirds of TAs do not receive a given type of aid, and some TAs' per capita allocations are over $100 for a single type of aid. The differences between urban and rural TAs is stark. Using Table 1, rural TAs receive over twice as many per capita dollars of aid. Unsurprisingly, rural areas have higher diarrhea incidence, lower school exposure, more missed employment due to illness, and less than half as much per capita expenditures (in real terms). However, when aid is viewed as a dummy variable, a much greater share of rural TAs are ignored; in other words, most of the areas that receive no aid are rural TAs, but the rural TAs that do receive aid are provided with large amounts.

5. Empirical Specification

The empirical specification consists of six components. The first constructs allocation models to predict how health aid, water aid, and education aid are allocated among TAs and uses OLS and Tobit regression techniques. We then apply the allocation models as first stage regressions within a D-i-D instrumentation approach to measure the impact of aid types. The third component takes an alternative approach of propensity score matching (PSM) to determine aid impact by simulating treatment and control groups of individuals. Afterwards, we estimate average treatment effects of our findings. In order to suggest donor allocation strategy, the next section determines whether aid is more or less effective in needier, less developed TAs. Finally, we investigate how particular donor agencies allocate their aid, and in particular consider whether they do so based on the level of need, including after adjustments for aid bundling.

Using both instrumentation and PSM to measure impact serves as a robustness check, because instrumentation methods hinge on finding instruments that are exogenous to the dependent impact variables, whereas PSM bias is actually reduced when the matching characteristics are related to the outcome (Rosenbaum and Rubin 1983). For example, instrumenting Health Aid to find impact on disease severity cannot rely on an instrument like diarrhea incidence, because diarrhea incidence may affect disease severity independently of Health Aid. PSM methods, on the other hand, can successfully incorporate these confounding characteristics, but the limitation is that substantial overlap of the matching characteristics is required between treatment and control groups. These criteria are discussed in further detail below, but an important note is the complementarity of the instrumentation and PSM methods.

5.1. Allocation Models

The per capita aid amounts by TA exhibit clumping at zero due to the nonnegative nature of cost data. As a result, both OLS and Tobit specifications (using both the weighted and unweighted versions of the data) are estimated for aid allocation models. Statistically significant coefficients are found in health, water, and education aid models, indicating that aid allocation is nonrandom across Malawi TAs. OLS and Tobit models generally produce models with the same signs of coefficients, with the exception of per capita expenditure and regional variables, such as Northern and Central, which had different directions of influence on aid allocation depending on model specifications. The weighted models produce greater levels of statistical significance than the unweighted models, but both versions always estimate the same signs of coefficients when both are statistically significant.

Our aid allocation models implicitly presume that donors have objective functions that increase with (a) likely project impact, and hence target community need, (b) administrative convenience, and hence the presence of other projects, as they imply the existence of aid infrastructure, especially if the projects are complementary and imply knowledge of a region, as well as trained local staff who can be poached for one's own projects, and (c) unit costs. Unit costs depend on the same factors as administrative convenience factors mentioned above, but also may depend on an area's receptiveness to a project. It is possible that less needy areas are more receptive, an issue we explore in section 5.5. Administrative convenience also may include proximity to major towns; for this reason we include regional dummy variables in allocation tables 2-4.

The results for health aid allocation are summarized in Table 2. The results of the living standards variables are mixed. More health aid is allocated to areas with more school exposure and lower diarrhea incidence, both of which indicate higher levels of existing development. In terms of disease burden, areas with greater *proportions of people who have had to stop employment* actually receive less health aid; however, a greater *number of employment days lost due to illness* is positively related with health aid allocation, which seems appropriate since days lost is a measure of the severity of disease burden. As expected, the *number of employment days lost due to illness* and *proportions of people who have had to stop employment* variables share a strong positive correlation with each other (0.88) because they are both measures of disease burden; however, including both of them allows distinction between many people afflicted by sickness versus people afflicted severely for many days. Accordingly, the signs on the variables, when controlling for one another, may vary within a model. Areas with greater per capita expenditures receive more health id on the margin. TAs located in Northern and Central Malawi are associated with about 2 dollars less and 5 dollars less, respectively, in per capita aid (mean per capita health aid is 7.42 dollars), whereas the association between urban regions and health aid is unclear because the OLS^w model produced a significant, negative coefficient whereas the $Tobit^w$ model produced a significant, positive one. Finally, we find evidence of aid projects being located in similar areas, because the coefficients on water aid per capita and education aid per capita were both positive and significant.

We present the results of water aid allocation models in Table 3. As with health aid, we again find allocation to be non-random, including the trend that all three types of aid are correlated with one another in terms of geographic placement - the health aid per capita and education aid per capita covariates were significant and positive across all four OLS and Tobit models. In terms of living standards variables, water aid is allocated more to areas that have less exposure to education and greater diarrhea. More water aid goes to areas with greater proportions of people who stop employment due to illness, but allocation is negatively correlated with the average number of days lost due to illness. If waterborne diseases result in short term employment lapses rather than multiple days lost, it may be reasonable to allocate based more on the proportion of people who stopped employment rather than the number of days lost, though that would seem to require an unusually sophisticated allocation strategy on the part of donors. The relationship between per capita expenditures and water aid is unclear because it varies between OLS and Tobit models. Geographic variables are conflicting in this model despite being statistically significant.

As with the other two types of aid, education allocation also exhibits non-random patterns, including positive relationships with health and water aid allocation. In terms of living standards variables, more educational aid is given to less educated areas and those with greater diarrhea. Areas with greater proportions of people stopping employment receive less education aid, but those with greater numbers of days lost receive more. Per capita expenditures are positively associated with education aid. TAs in Northern and Central Malawi receive more aid than those in Southern Malawi, but the magnitude of this difference is unclear; in the OLS^w models the magnitudes are around 1-2 dollars whereas the $Tobit^w$ models estimate 10 and 12 dollars of extra aid for Central and Northern, respectively, where the mean education per capita aid is 4.15 dollars.

5.2. Impact Models - Instrumentation Methods

Instrumentation is one method for overcoming the difficulties of endogeneity within the aid impact process. Instrumental variables (IV) techniques hinge on two assumptions: relevance and exclusion. Relevance was tested through standard first stage regressions and exclusion was based on both theoretical knowledge–such as not using a diarrhea variable to measure the change in diarrhea incidence–and passing the overidentification test. The dependent variables are the changes in living standards for each TA between time t_0 and t_1, such as the change in diarrhea incidence between the two periods. The methodology is difference-in-difference identification because we compare changes over time between those who are treated by aid and those who are not, though the measure of aid treatment is a continuous variable of per capita aid allocation, rather than a dummy variable as is most common in D-i-D studies. In addition, whereas many D-i-D studies consider the treatment group to be similar to the control group (due to geographic proximity, etc.), we have already shown nonrandom allocation and thus use instrumentation in order to account for differences in initial conditions.

In these models, it is important to control for other types of aid because of the correlations discussed previously, but the causal estimation can only be applied to the variable that is being instrumented. For example, in Table 6, water and education are included in the regression because they may have a relationship with the dependent variable, but only health aid is instrumented, so only its coefficient is interpreted causally.[6]

Table 6 shows results from a D-i-D IV estimation of the causal effects of health aid on the number of days lost due to illness in the two weeks before survey data were collected. Using instruments of Northern and Central regional dummies, we find that health aid decreases disease severity with high statistical significance in the weighted model. For the results in Table 6, the instruments passed the overidentification test, but when the same instruments were used to assess the impact of health aid on the proportion of people who stopped employment in the two weeks prior to survey responses due to illness, these instruments failed the overidentification test for exogeneity. It is difficult to understand why these regional instruments confound an effect on stopping employment but do not confound the length of time that an individual stopped employment. The regional variables could potentially be proxies for factors that are more closely associated to disease duration. For example, perhaps some of the more severe, long-lasting illnesses occur in certain geographic regions.

We present an estimate for the impact of water aid on diarrhea incidence in Table 7 using school exposure and the Central dummy as its instruments. These pass the overidentification test for exogeneity of instruments. Table 7 shows a positive, significant effect of water aid on reducing diarrhea disease[7]. The effects are economically significant as well, as we discuss in section 5.4.

Unfortunately, despite high relevance, none of the covariates could pass the overidentification tests for Education Aid. This suggests that issues of geography and development are linked importantly to educational enrollment, so further research is required to find appropriate instruments for education. However, due to the differing assumptions of PSM methods, the impact of Education Aid will be explored in the next section.

Overall, the process of instrumenting aid is tenuous at best. It is likely that any factors that one can conjure as predictive of aid allocation are accompanied by some theoretical

[6] For robustness we also estimate the IV models without other covariates int he Appendix section. We find similar results, except that the overidentification test for exogeneity of instruments fails, suggesting that the instruments may affect the outcome variable through other aid channels; it is important to control for them.

[7] For robustness, we estimate the IV models with all urban areas removed, presented in the Appendix; results are similar but overidentification tests fail.

link through which the factor affects aid effectiveness as well, undermining the instrumentation assumptions. Dollar and Levin (2005) use various instruments for aid: share of the population speaking English, share speaking a continental European language, distance from the equator, level of population, and each of the above multiplied by population. Even these careful instruments may threaten the exclusion principle of instrumentation; English or European language skills may facilitate growth for reasons of international business or attracting foreign investment, distance from the equator may affect GDP through climate, and population has a myriad of channels through which it affects national accounts - employment, economies of scale, demography, structural transformation, to name a few. Due to the difficulty of appropriate instruments, the question of aid impact may be better suited by methods such as Propensity Score Matching, which we discuss below.

5.3. Impact Models - Propensity Score Matching Methods

Propensity Score Matching is a separate method for assessing causality within observational data. Because its assumptions are modeled differently than those for IV, these PSM tests serve as robustness checks for the IV method. In Propensity Score Matching, confounding characteristics that both affect aid allocation and living outcomes are used as matching characteristics between individuals who receive aid and those who do not. These matching characteristics are converted to a Propensity Score for each individual and treated individuals are compared to untreated individuals with the nearest Propensity Score.

Overall, the use of PSM in our context is a form of difference-in-difference identification strategy. The natural treatment is whether an individual's TA received aid, and treated individuals are compared to individuals who are untreated but who have similar initial conditions in time t_0 (by using the matching attributes), and the dependent variable is the change in health or educational outcomes between t_0 and t_1. This before and after snapshot allows us to account for initial conditions by tailoring the untreated counterfactual group such that it is similar to the treated aid recipients. As a result, we can attribute the difference between treated and untreated over time to the effects of aid.

Because PSM is a method for treatment and control groups, aid is converted to a dummy variable where Health Aid=1 if any health aid was received and Health Aid=0 if none was received. The same specification is used for Water Aid and Education Aid. Therefore, these average treatment effect estimates will not be directly comparable to the IV marginal effect estimates, but the signs of impact should be consistent.

An important requirement for PSM is that the treatment and control groups' matching attributes need to contain significant overlap, yet these matching attributes still need to predict the treatment condition. The first step is therefore to find a specification of matching attributes that both predicts aid allocation and is somewhat balanced between individuals who have both received and not received aid. Our results previously discussed in the allocation models showed that the neediest regions are not the only places receiving aid; this works to our advantage in estimating impacts through matching techniques. Table 8 shows the Probit regression for matching characteristics that fulfill the balancing property for Health Aid. It is especially important to include Water Aid because of the possibility that Water Aid may affect decreased days lost due to illness, which is the dependent variable for Health Aid impact.

Once the balancing property is satisfied, each treated individual is matched to an untreated individual while minimizing the differences in Propensity Scores between matched individuals. This is referred to as nearest neighbor matching, where neighbor refers to individuals with very similar Propensity Scores. In the case of Health Aid, individuals with similar Water Aid and Stopped Employment (the matching attributes) but different allocations of Health Aid are matched with each other to find the average treatment effect. Table 9 presents the results of the PSM for Health Aid treatment on days lost, the number of days in the past 2 weeks spent unable to work, due to disease. Whereas the IV estimates signified a marginal effect per dollar of per capita aid, the PSM estimates need to be interpreted as the average effect of aid for those who received it compared to those who did not. The Table 9 estimate therefore indicates that Health Aid, on average, causes 0.035 fewer days lost every 2 weeks due to illness, which corresponds to 0.91 days per person per year of increased productivity in areas that receive health aid compared to those areas that do not. The Stopped Employment variable is very well suited for inclusion in the model. This is because we want to simulate direct comparisons between TAs that have similar rates of people who are too sick to work, and then exploit variation in Health Aid to determine the differential effects on the number of days lost due to illness.

Table 10 presents the matching attributes, health aid and days lost to illness, used for water aid impact. In Table 11, we present the causal estimate, finding that water aid decreases diarrhea incidence with an average treatment effect of 0.004. Though this seems rather low for an average treatment effect, average levels of diarrhea incidence are also very low because they only represent incidence in the last 2 weeks before survey data were collected. The average diarrhea incidence for TAs is 0.019, so the average effect of 0.004 indicates a 21 percent decrease in diarrhea rates and is therefore economically significant. We also expect other water-related diseases to decrease, so diarrhea amelioration is only one (easily measured) form of impact of water aid.

The impact of Education Aid could not be estimated in the IV model since no instruments passing the overidentification tests for exogeneity. However, PSM methods can be applied to endogenous matching characteristics. In fact, PSM bias is actually reduced when the matching characteristics are related to the outcome (Rosenbaum and Rubin 1983). Table 12 presents the Probit regression for Education Aid in which Water Aid, Health Aid, Urban and Stopped Employment were used as the endogenous matching attributes to fulfill the balancing property.

The impact of Education Aid is estimated on Increased School Exposure, where School Exposure is the proportion of the TA that has ever attended school. Table 13 presents the result of the Propensity Score Match and finds a positive impact of Education Aid on exposure to schooling and is significant at $p<0.10$. Though there is no IV estimate to check for the robustness of this estimate, the PSM results alone suggest that receiving Education Aid causes an average treatment effect of 0.009 increase in school exposure, which considering that average TA population is 36,000 people, corresponds to 324 extra people receiving exposure to school for an average-treated TA. If this is representative of the actual effect of education aid, it may suggest an important economic significance through more people gaining access to literacy and growth of human capital.[8]

[8] We attempted specifications that incorporated expenditures per capita as a matching attribute, to control for income levels, but these specifications failed the balancing criterion for all aid types. We also present specifications with all urban areas removed in the Appendix. Interestingly, the positive effects of water aid and education aid are amplified, whereas the positive effect of health aid reverses to become a negative one.

5.4. Average Treatment Effects

Using the average treatment effect estimates from propensity score matching, we can estimate how economically large these impacts are, based on average TA population sizes, average aid allocations in treatment areas, and average project size (in dollars). An average health project is $88,373, which leads to 0.91 fewer work days lost due to illness, per person per year. Given that an average TA is 36,000 people, this corresponds to 32,760 extra days of economic productivity for a single TA per year during the duration of the aid effect. Water projects are somewhat larger–the average size is $229,194, leading to 144 fewer cases of diarrhea. That effect alone would be costly; it would cost over $1500 per case of diarrhea averted. However, it is extremely likely that if diarrhea is being ameliorated by water aid, that there are real economic benefits from reductions in other water-related diseases, as well as lifestyle benefits of improved sanitation, such as better nutrition. Finally, we estimate that an extra 324 people receive educational exposure, defined as having attended school at some point, for an average sized educational aid project of $160,792. Similar to water aid, educational aid most likely also affects other metrics in addition to our indicator for school exposure. School exposure measures the proportion of a TA that has ever attended school, whereas educational aid may be better measured on indicators of quality or duration of education, rather than simple exposure.

5.5. Varying Aid Effectiveness

Overall, we have found evidence for positive effects of health, water, and education aid on their respective targeted outcomes. Are these positive effects ubiquitous, or do they vary based on how developed an area is? Dollar and Levin (2006) explore determinants of the allocation of foreign aid across countries during the period 1984-2003, and during this period there was a dramatic change: the emphasis on providing aid to the poorest (often dysfunctional, undemocratic, corrupt, and ill-governed in other ways as well) countries shifted to those with better governance and more democratic institutions. This change reflects empirical evidence that the impact of aggregate aid is greater when recipient governments are more competent and democratic, open, and less repressive. Their paper documents the change for a large number of multilateral and bilateral donors using pooled Tobit regressions. The multilaterals in their study for the most recent (2000-2003) period seemed less concerned about democracy and governance than most; the US and UK were most concerned about democracy, followed by Germany and Japan, then Norway, and then Australia. Rule of law mattered the most to Germany, followed by Norway and Australia; it did not matter to the others. Poverty (low GDP per capita) mattered most to the African Development Fund, then to the US, UK, and Norway, followed by Japan, Germany, and Australia in that order; the IBRD (World Bank) coefficient was not significant.

The question of whether to allocate based on highest need (higher poverty) versus better governance and institutions (most likely, lower poverty) hinges partly on where aid is more effective. In order to investigate varying levels of effectiveness, we stratify the population in terms of more developed and less developed areas and re-run the impact specifications to check for differences. For example, with regards to health aid, the less developed areas are those with greater-than-average number of days lost due to illness. For water aid, less developed areas are those with higher-than-average diarrhea incidence, and for education aid, it is areas with lower-than-average school exposure.

The results are summarized and compared to our original baseline results in Table 14. Health aid and education aid are more effective in less developed areas. Water aid is less effective in less developed areas, perhaps because of greater remoteness or lack of necessary complementary factors. For health and education aid, these results signify that the optimal strategy for donors is to allocate based on need, because the needier regions are also the areas with greater effectiveness. In the case of water aid, we still suggest allocating based on need, despite some evidence that the returns in needier regions may be less than more developed areas. If aid is visibly linked in part (though not fully) to improved outcomes, in the longer run, allocating based on need can improve lagging areas such that they reach a higher level of aid effectiveness as they develop stronger infrastructures and capacities.

Collier and Dollar (2002) estimate how aid would be reallocated across countries if poverty reduction were the sole rationale for providing foreign aid. They note that aid is allocated inefficiently with respect to poverty alleviation because is is partly used as an inducement to policy reform and partly for a variety of historical and strategic reasons (p.1497). Using our estimates, allocating health aid based on need would lead to an additional 9,360 days of productivity annually for an average TA of 36,000 people due to reduced disease burden, about a 30 percent gain over the local average treatment effect. An average TA would see a boost of an additional 4,752 people receiving exposure to school if education aid were allocated purely based on need. This is not surprising, given that the TA with the lowest school exposure had only 16 percent of its population that had ever attended school. It also points to the far greater effectiveness of health aid: potential efficiency gains from better-sited education projects would reach an order of magnitude more people than the average actual treatment effect indicate.

5.6. Donor Aid Allocation

Rao (1997) explores the extent to which foreign aid is provided based on apparent need as defined by per capita income differences. This paper has information from, in most cases, annual aid flows for 1970-1993 from 18 countries plus the IBRD and IDA components of the World Bank. Clearly, not all aid goes to the neediest places, and there is also a substantial difference in aid-to-the-poorest from the top ranked country (usually Denmark) and the lowest ranked country (after 1975, generally the United States).

The AidData dataset for our paper contains information about which donor country or organization funded each aid project. In order to analyze efficiency of donor allocation, we estimate two models for each donor. In one model, we regress per capita aid on the pertinent living standards variable; for example, health aid per capita versus the percent of the population that stopped employment due to illness. In the second model, we account for the presence of aid bundling by including a variable that represents all of that donor's other aid allocations within a TA. Our rationale for the second model is that donors can maximize resources by bundling multiple aid projects in one place, so we investigate whether aid is going to the neediest regions after controlling for the donor's other aid efforts in the region. Both models are estimated using both the weighted and unweighted versions of the data for robustness checks.

Table 15 shows the results of donor analysis on health aid allocation. Because the relationship of interest is between health aid allocation and disease severity, we consider positive relationships to be more desirable, because they indicate higher health aid allocation to TAs with more severe disease burden, where disease severity is measured by the proportion of people who stopped employment due to illness. In terms of overall allocation, GIZ (Germany), KFW Bankengruppe (also Germany) and USAID show desirable allocation in the weighted models, whereas the African Development Bank, EU, Iceland, JICA (Japan), NORAD (Norway), and DfID (United Kingdom) have the opposite relationship - more aid is allocated to areas with less severe diseases. Almost half of the donors show evidence of aid bundling. Even after controlling for aid bundling, African Development Bank, EU, Iceland, JICA (Japan), NORAD (Norway), and DfID still allocate in the undesirable direction. USAID presents an interesting example because before controlling for aid bundling, allocation seemed to be towards the needier areas. Once aid bundling is controlled for, USAID allocation changes signs. This means that given two areas with equal amounts of other USAID aid, the less needy TA is more likely to get health aid on the margin, which is not optimal. Most of the unweighted models do not produce statistical significance on the percent stopped work variable, though some have significance on the aid bundling variable. The lack of significance in the unweighted versions indicates that none of the donors allocate with enough non-randomness to be detectable when each TA is unweighted down to one data point. Large TAs may be the ones that are exposing the direction of statistical significance in the weighted version of the data.

Table 16 shows the results of the analogous analysis on water aid allocation with respect to diarrhea incidence. Positive relationships are again desirable, indicating higher aid for regions with high diarrhea illness. Water aid allocation appears to be fairly efficient, with DfID, USAID, and the World Bank all allocating in the positive direction. Australian Aid, EU, and JICA (Japan) allocate undesirbly. Australian Aid only allocated to water aid, so it cannot be checked against aid bundling. African Development Bank is especially interesting because it allocates randomly, but when controlling for aid bundling, allocates in the desirable direction.

Table 17 displays results from the donor analysis on education aid allocation with respect to percent school exposure, which is the percentage of a TA's population that has ever attended schooling. Signs have the opposite interpretation for this analysis compared to those for health and water aid. We consider negative relationships to be desirable, indicating that education aid goes to TAs with the least exposure to education. The results are mixed; African Development Bank, GIZ (Germany), and USAID all have the desirable allocation direction in the simple allocation model. Iceland, Ireland, JICA (Japan), DfID (UK) and the World Bank have the opposite sign, but the magnitudes of their coefficients are much smaller than those of the desirable donors. Interestingly, once aid bundling is accounted for, the significance on the USAID coefficient is eroded, meaning that given two

TAs with equal other aid allocation, education aid is allocated randomly with regards to school exposure rates. Examination of specific TAs shows that there are very unbalanced areas. TA Mphuka in the Thyolo District had the lowest school exposure - only 16 percent, whereas the next lowest was much higher at 42 percent and with most TAs between 60 to 80 percent. Despite being an outlier, Mphuka did not receive any education aid in the aid data set.

We have attempted a simple analysis of donor aid allocation, but in reality the model may be much more complex. In addition to controlling for aid project bundling, as we have done, it may be important to control for patterns across time. A donor may choose to allocate an aid project based on which sub-districts have not recently received exposure from other donor organizations. Rather than allocating to the neediest region, for example, an optimal donor allocation strategy may be to allocate to the neediest region *that has thus far been ignored*. Investigating this question of first mover and second mover donor strategies will require greater attention to the time dimension of the data. The time dimension should also be employed to investigate the effects of aid over time. We address both of these analyses in a subsequent paper.

6. Conclusion

Through a sub-national analysis of Malawi, we find that aid allocation models vary greatly across type of aid. Evidence of positive causal relationships between aid and living standards also emerges. In particular, Health Aid reduces disease severity, Water Aid reduces diarrhea illness, and Education Aid increases school exposure. These rather obvious seeming statements reveal one of the important aspects of the methodology used here: if the impact of aid is being investigated, the living standards variables most targeted by aid projects are the ones that should be modeled. Greater data availability can allow this shift from cross-country macroeconomic investigations to sub-national living standards inquiries.

One of the arguments against foreign aid is that the resources fund corruption and do not impact poverty variables such as health and education. Easterly (2002) provides explanations for why foreign aid fails in many situations, such as mechanism design failures, principal-agent problems, lack of coordination, warped incentives for host countries, excessive bureaucracy, and unsustainable demands on host country skilled labor. Unfortunately, our methodology cannot validate or reject these hypotheses nor assess how much of the aid projects are actually given to their intended recipients rather than siphoned off by corruption or bureaucracy, and in fact it is clear that many projects are not well targeted. However, our results do suggest that resources on the whole are being funneled to poverty efforts because the overall impact can be detected across time when controlling for the endogeneity of aid allocation. Policymakers should attempt to design aid policies that are dependent on living standards data for all three of these aid categories: health aid should be concentrated where diseases are burdening people beyond the capacity to attend to employment, water aid should be reallocated towards high diarrhea regions, and education aid should be funneled to the areas with the least exposure to schools.

Goal 1 of the Millennium Development Goals is to reduce extreme poverty, and projections used by the United Nations indicate that almost one billion people will still be living on less than 1.25 dollars per day in 2015 (United Nations). Overcoming poverty is also one of the major goals for many different disciplines that are concerned with the economic, ethical and humanitarian implications of global suffering. However, this is an exciting time for development economists to identify effective approaches to poverty reduction. Sub-national modeling should be further explored as a means for understanding the intricacies of development while maintaining external validity for an entire nation. Malawi's next round of living standards data will contain panel observations linked to IHS3 and provide an even stronger basis for assessing the impacts of various aid types, and as data become available for other nations' aid projects, sub-national analyses should be employed to assess foreign aid around the world.

7. Appendix

Figure 1. Aid projects in Malawi

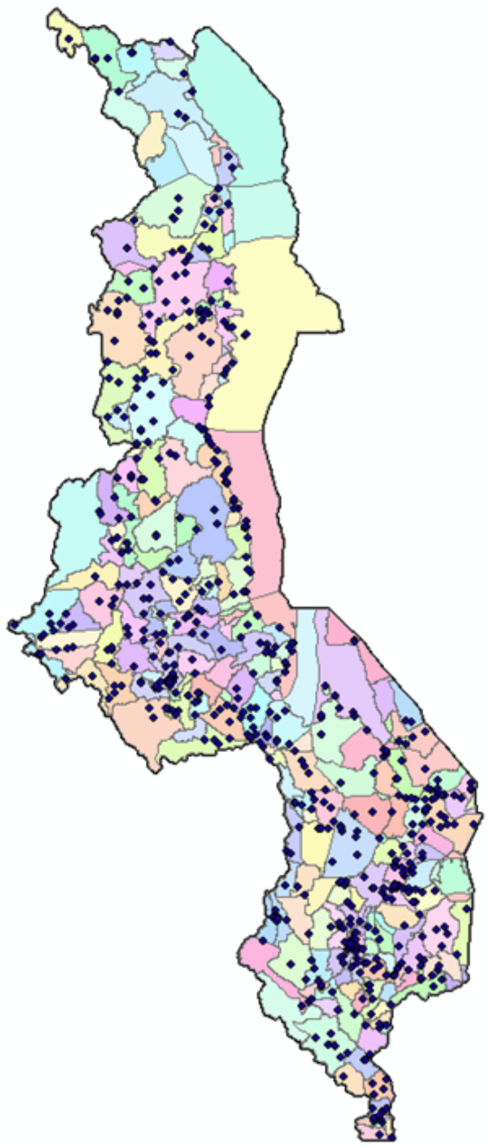

Table 1. Variable names and descriptive statistics

Name	Description	Source	Overall	Urban	Rural
Per capita health aid	health aid per person, in USD, for a given TA	aiddata.org and Census data	$7.42 (23.72)	$2.67 (3.09)	$8.04 (25.12)
Per capita water aid	water and sanitation aid per person, in USD, for a given TA	aiddata.org and Census data	$8.85 (25.28)	$1.26 (0.66)	$9.82 (26.70)
Per capita education aid	education aid per person, in USD, for a given TA	aiddata.org and Census data	$4.15 (13.07)	$1.89 (1.26)	$4.44 (13.86)
Diarrhea incidence	the proportion of a TA that reports diarrhea illness in the past two weeks	LSMS	0.02 (0.01)	0.01 (0.01)	0.02 (0.01)
School exposure	the proportion of a TA that reports having ever attended school	LSMS	0.77 (0.12)	0.92 (0.06)	0.76 (0.12)
Stopped employment	the proportion of a TA that had to stop employment at some point during the past two weeks due to illness	LSMS	0.15 (0.07)	0.08 (0.05)	0.16 (0.07)
Days lost	the average number of days per person, for a given TA, of stopped employment due to illness within the past two weeks	LSMS	0.68 (0.38)	0.38 (0.29)	0.72 (0.37)
Expenditure Per Capita	the average real expenditures (Malawian Kwacha) per person, for a given TA, calculated from household expenditures and where children under 15 and adults over 80 are given reduced weightage of 0.8	LSMS	22073 (12689)	41327 (28100)	19589 (5092)
Northern	dummy variable for the northern region of Malawi	LSMS	0.16 (0.37)	0.18 (0.38)	0.16 (0.37)
Central	dummy variable for the central region of Malawi	LSMS	0.41 (0.49)	0.31 (0.46)	0.42 (0.49)
Urban	dummy variable for urban TAs of Malawi	LSMS	0.11 (0.32)	1.0 (0.0)	0.0 (0.0)
Health Aid (PSM)	dummy variable for TAs that received any health aid	aiddata.org	0.33 (0.47)	0.88 (0.33)	0.26 (0.44)
Water Aid (PSM)	dummy variable for TAs that received any water aid	aiddata.org	0.31 (0.46)	0.88 (0.33)	0.24 (0.43)
Education Aid (PSM)	dummy variable for TAs that received any education aid	aiddata.org	0.22 (0.42)	0.88 (0.33)	0.14 (0.35)

Table 2. Health aid allocation models

	OLS^{w}	$Tobit^{w}$	OLS^{u}	$Tobit^{u}$
School Exposure	9.734*** (0.313)	38.832*** (1.421)	12.440** (5.986)	60.840* (31.724)
Diarrhea Incidence	-44.361*** (3.570)	-0.748 (13.608)	25.843 (53.550)	165.056 (186.838)
Stopped Employment	-36.324*** (1.796)	-189.028*** (7.111)	-39.036 (31.420)	-160.354 (104.859)
Days Lost to Illness	8.243*** (0.482)	22.379*** (1.351)	9.579 (8.848)	23.938 (21.767)
Expenditure Per Capita	$3.83e^{-5}$*** ($2.15e^{-6}$)	$7.77e^{-5}$*** ($5.43e^{-6}$)	$5.13e^{-6}$ (2.91^{-5})	$1.47e^{-5}$ ($6.67e^{-5}$)
Northern	-2.022*** (0.315)	-1.370* (0.701)	0.469 (5.551)	0.934 (10.133)
Central	-5.897*** (0.117)	-6.377*** (0.297)	-4.159** (1.656)	-6.893 (5.000)
Urban	-4.364*** (0.121)	9.087*** (0.431)	-2.954* (1.644)	18.969** (8.732)
Water Aid	0.272*** (0.008)	0.503*** (0.013)	0.254 (0.199)	0.457 (0.278)
Education Aid	0.992*** (0.014)	1.259*** (0.015)	0.915*** (0.309)	1.391*** (0.331)
Constant	-3.540*** (0.295)	-43.379*** (1.513)	-7.592 (6.373)	-74.473** (36.580)

Notes: *** indicates 1 percent significance; ** indicates 5 percent; * indicates 10 percent. Robust standard errors are employed.

Table 3. Water aid allocation models

	OLS^{w}	$Tobit^{w}$	OLS^{u}	$Tobit^{u}$
School Exposure	-3.057*** (0.392)	-30.080*** (1.300)	-4.450 (4.323)	-31.584* (16.071)
Diarrhea Incidence	39.884*** (2.964)	44.914*** (13.552)	10.361 (27.017)	-1.942 (113.389)
Stopped Employment	71.365*** (1.927)	269.471*** (8.024)	34.793 (25.731)	134.696 (92.949)
Days Lost to Illness	-19.697*** (0.485)	-79.2151*** (2.005)	-9.295* (5.443)	-35.114* (20.028)
Expenditure Per Capita	$-2.63e^{-5}$*** ($2.5e^{-6}$)	$6.07e^{-5}$*** ($6.08e^{-6}$)	$-2.95e^{-6}$ ($2.96e^{-5}$)	$-4.08e^{-6}$ ($5.64e^{-5}$)
Northern	-3.727*** (0.241)	0.599 (0.624)	-1.089 (3.358)	3.129 (7.987)
Central	-0.532*** (0.136)	16.352*** (0.416)	-0.581 (1.863)	7.169 (5.352)
Urban	-3.327*** (0.139)	32.004*** (0.508)	-2.702 (1.654)	29.876*** (6.829)
Health Aid	0.319*** (0.009)	0.569*** (0.008)	0.156* (0.081)	0.335*** (0.085)
Education Aid	1.042*** (0.017)	1.516*** (0.021)	1.392*** (0.257)	1.849*** (0.346)
Constant	8.224*** (0.292)	-4.693*** (0.960)	6.142* (3.602)	-5.580 (11.637)

Notes: *** indicates 1 percent significance; ** indicates 5 percent; * indicates 10 percent. Robust standard errors are employed.

Table 4. Education aid allocation models

	OLS^w	$Tobit^w$	OLS^u	$Tobit^u$
School Exposure	-5.324*** (0.183)	-33.565*** (1.141)	-1.193 (2.330)	-11.846 (11.325)
Diarrhea Incidence	1.169 (1.767)	27.131*** (8.638)	-20.747 (18.794)	-21.154 (74.141)
Stopped Employment	-23.578*** (0.777)	-144.324*** (4.776)	-8.457 (10.725)	-34.087 (48.833)
Days Lost to Illness	6.354*** (0.172)	22.850*** (0.725)	2.430 (2.035)	5.139 (7.297)
Expenditure Per Capita	$8.49e^{-6}$*** ($1.28e^{-6}$)	$5.7e^{-5}$*** ($4.59e^{-6}$)	$3.04e^{-6}$ ($1.57e^{-5}$)	$3.11e^{-5}$ ($4.02e^{-5}$)
Northern	1.738*** (0.111)	12.404*** (0.413)	-0.360 (1.487)	2.820 (4.150)
Central	1.761*** (0.082)	9.630*** (0.285)	0.302 (1.078)	2.726 (2.839)
Urban	1.834*** (0.058)	27.720*** (0.378)	0.909 (0.746)	18.596*** (3.817)
Health Aid	0.255*** (0.008)	0.385*** (0.009)	0.131 (0.100)	0.228** (0.110)
Water Aid	0.229*** (0.006)	0.446*** (0.008)	0.325*** (0.094)	0.466*** (0.104)
Constant	2.146*** (0.128)	-4.026*** (0.888)	1.182 (1.484)	-9.497 (9.057)

Notes: *** indicates 1 percent significance; ** indicates 5 percent; * indicates 10 percent. Robust standard errors are employed.

Table 5. Aid correlations

	Health	Water	Education
Health	1.0000		
Water	0.7106	1.0000	
Education	0.7758	0.7605	1.0000

Notes: These correlations were calculated using the weighted version of the data. They were also calculated using the unweighted data, with similar results of high correlation between aid categories.

Table 6. Impact of health aid on days lost to illness

	Days $Lost^w$	Days $Lost^u$
Health Aid (Instrumented)	-0.011*** ($8.8e^{-4}$)	-0.014 (0.015)
Water Aid	0.004*** ($2.4e^{-4}$)	0.003 (0.004)
Education Aid	0.008*** ($9.1e^{-4}$)	0.012 (0.016)
Constant	-0.139*** (0.002)	-0.112*** (0.041)

Notes: The Northern and Central regional dummy variables were instruments for Health Aid. *** indicates 1 percent significance; ** indicates 5 percent; * indicates 10 percent. Robust standard errors are employed. F-statistics from first-stage regressions (relevance test) are 542.43 and 1.96 for the weighted and unweighted models, respectively.

Table 7. Impact of water aid on diarrhea incidence

	Diarrhea Incidence[w]	Diarrhea Incidence[u]
Water Aid (Instrumented)	-0.002*** ($7.7e^{-5}$)	-0.003 (0.002)
Health Aid	0.001*** ($6.9e^{-5}$)	0.002 (0.001)
Constant	-0.006*** ($2.5e^{-4}$)	-0.004 (0.005)

Notes: School Exposure and the Central dummy variable were instruments for Water Aid. *** indicates 1 percent significance; ** indicates 5 percent; * indicates 10 percent. Robust standard errors are employed. F-statistics from the first-stage regressions (relevance test) are 550.05 and 1.52 for the weighted and unweighted models, respectively.

Table 8. Health aid probit regression of balancing covariates

	Probit
Water Aid Dummy	1.790*** (0.015)
Stopped Employment	-5.828*** (0.109)
Constant	-0.286*** (0.017)

Notes: *** indicates 1 percent significance; ** indicates 5 percent; * indicates 10 percent.

Table 9. Impact of health aid on days lost to illness

	Days Lost
Health Aid	-0.035*** (0.009)

Notes: Health Aid was a dummy variable. Percent who Stopped Employment and a Water Aid Dummy were the matching characteristics. ***indicates 1 percent significance; ** indicates 5 percent; * indicates 10 percent.

Table 10. Water aid probit regression of balancing covariates

	Probit
Health Aid Dummy	1.780*** (0.014)
Days Lost to Illness	-0.318*** (0.020)
Constant	-0.997*** (0.017)

Notes: *** indicates 1 percent significance; ** indicates 5 percent; * indicates 10 percent.

Table 11. Impact of water aid on diarrhea incidence

	Diarrhea Incidence
Water Aid	-0.004*** (0.000)

Notes: Water Aid was a dummy variable. Days Lost to Illness and a Health Aid Dummy were the matching characteristics. *** indicates 1 percent significance; ** indicates 5 percent; * indicates 10 percent.

Table 12. Education aid probit regression of balancing covariates

	Probit
Water Aid Dummy	1.235*** (0.021)
Health Aid Dummy	1.795*** (0.024)
Urban	1.621*** (0.039)
Stopped Employment	-0.040 (0.164)
Constant	-2.540*** (0.018)

Notes: *** indicates 1 percent significance; ** indicates 5 percent; * indicates 10 percent.

Table 13. Impact of education aid on school exposure

	School Exposure
Education Aid	0.009* (0.006)

Notes: Education Aid was a dummy variable. A Water Aid Dummy, a Health Aid Dummy, Urban and Percent who Stopped Employment were matching characteristics. *** indicates 1 percent significance; ** indicates 5 percent; * indicates 10 percent.

Table 14. Impacts of health, water and education aid in less developed areas

	Less Developed Area	Baseline
Health Aid on Days Lost (IV)	-0.020***	-0.014***
Health Aid on Days Lost (PSM)	-0.045***	-0.035***
Water Aid on Diarrhea (IV)	-0.001***	-0.002***
Water Aid on Diarrhea (PSM)	0.002***	-0.004***
Education Aid on School Exposure (PSM)	0.141***	0.009*

Notes: *** indicates 1 percent significance; ** indicates 5 percent; * indicates 10 percent. Robust standard errors are employed.

Table 15. Health aid allocation and percent who stopped work due to disease

Donor	Percent Stopped Work	Other Aid
African Dev Bank^{w}	-2.780***	
	-1.563***	0.050***
African Dev Bank^{u}	-0.666	
	-0.626	0.075**
EU^{w}	-0.387***	
	-0.389***	-0.0003***
EU^{u}	-0.558***	
	-0.558***	-0.0004***
GIZ (Germany)^{w}	0.784***	
	0.378***	0.136***
GIZ (Germany)^{u}	0.626	
	0.498	0.131**
Iceland^{w}	-0.028***	
	-0.028***	$-9.8e^{-5}$***
Iceland^{u}	-0.039***	
	-0.039***	-0.0002**
JICA^{w}	-0.002***	
	-0.002***	$-1.5e^{-6}$
JICA^{u}	-0.003***	
	-0.003***	$-2.0e^{-6}$
KFW Bankengruppe^{w}	3.624***	
	3.681***	-0.034***
KFW Bankengruppe^{u}	10.211	
	10.276	-0.086
NORAD (Norway)^{w}	-1.797***	
	-1.720***	0.010***
NORAD (Norway)^{u}	-0.288	
	-0.284	0.008
DfID (UK)^{w}	-1.906***	
	-1.897***	-0.006***
DfID (UK)^{u}	-1.362	
	-1.357	-0.005
USAID^{w}	2.794***	
	-1.050***	0.606***
USAID^{u}	4.552	
	1.775	0.678***

Notes: ***indicates 1 percent significance; ** indicates 5 percent; * indicates 10 percent. Robust standard errors are employed.

Table 16. Water aid allocation and diarrhea incidence

Donor	Diarrhea Incidence	Other Aid
African Dev Bank^{w}	-0.232	
	2.443**	0.057***
African Dev Bank^{u}	-1.870	
	-0.229	0.096**
Australian Aid^{w}	-0.270*	
	—	—
Australian Aid^{u}	1.104	
	—	—
EU^{w}	-0.232***	
	-0.230***	-4.8e^{-5}
EU^{u}	-0.229*	
	-0.232*	-7.3e^{-5}
JICA^{w}	-0.001***	
	-0.001***	-2.9e^{-7}
JICA^{u}	-0.0003	
	-0.0003	-5.5e^{-7}
DfID (UK)^{w}	4.761***	
	5.054***	0.051***
DfID (UK)^{u}	1.911	
	2.139	0.054
USAID^{w}	16.668***	
	15.468***	0.157***
USAID^{u}	-10.041	
	-8.134	0.234**
World Bank^{w}	35.361***	
	16.361***	0.099***
World Bank^{u}	10.141	
	1.385	0.118***

Notes: ***indicates 1 percent significance; ** indicates 5 percent; * indicates 10 percent. Robust standard errors are employed.

Table 17. Education aid allocation and percent school exposure

Donor	Percent School Exposure	Other Aid
African Dev Bank^w	-2.357***	
	-2.675***	0.031***
African Dev Bank^u	-1.496	
	-1.552*	0.039**
GIZ (Germany)^w	-0.248***	
	-0.232***	-0.018***
GIZ (Germany)^u	-0.416	
	-0.391	-0.016**
Iceland^w	0.012***	
	0.012***	-5.5e^{-5}***
Iceland^u	0.016***	
	0.016***	-8.7e^{-5}
Ireland^w	0.119***	
	0.118***	-0.002***
Ireland^u	0.138***	
	0.138***	-0.004**
Japan^w	5.45e^{-5}***	
	5.63e^{-5}***	-0.006***
Japan^u	1.5e^{-5}	
	1.6e^{-5}	-0.006
DfID (UK)^w***	0.007***	
	0.007***	-3.0e^{-5}***
DfID (UK)^u***	0.009***	
	0.009***	-4.9e^{-5}*
USAID^w	-0.553***	
	0.046	0.028***
USAID^u	0.115	
	0.391	0.025*
World Bank^w	1.127***	
	1.210***	0.003***
World Bank^u	1.569	
	1.663	0.008

Notes: ***indicates 1 percent significance; ** indicates 5 percent; * indicates 10 percent. Robust standard errors are employed.

Table 18. Impact of health aid on days lost to illness

	Days Lost^w	Days Lost^u
Health Aid (Instrumented)	-0.007***	-0.009
	(6.1e^{-4})	(0.011)
Constant	-0.105***	-0.078
	(0.004)	(0.072)

Notes: The Northern and Central regional dummy variables were instruments for Health Aid. *** indicates 1 percent significance; ** indicates 5 percent; * indicates 10 percent. Robust standard errors are employed. This specification failed the overidentification test for exogeneity of instruments. F-statistics from first-stage regressions (relevance test) are 542.43 and 1.96 for the weighted and unweighted models, respectively.

Table 19. Impact of water aid on diarrhea incidence

	Diarrhea Incidence^{w}	Diarrhea Incidence^{u}
Water Aid (Instrumented)	-0.0007*** (2.9e^{-5})	-0.0007 (0.0007)
Constant	-0.006*** (2.3e^{-4})	-0.006 (0.004)

Notes: School Exposure and the Central dummy variable were instruments for Water Aid. *** indicates 1 percent significance; ** indicates 5 percent; * indicates 10 percent. Robust standard errors are employed. This specification failed the overidentification test for exogeneity of instruments.F-statistics from the first-stage regressions (relevance test) are 550.05 and 1.52 for the weighted and unweighted models, respectively

Table 20. Impact of health aid on days lost to illness - urban removed

	Days Lost^{w}	Days Lost^{u}
Health Aid (Instrumented)	-0.012*** (8.8e^{-4})	-0.014 (0.015)
Water Aid	0.004*** (2.4e^{-4})	0.003 (0.004)
Education Aid	0.008*** (9.1e^{-4})	0.012 (0.016)
Constant	-0.157*** (0.002)	-0.110*** (0.041)

Notes: The Northern and Central regional dummy variables were instruments for Health Aid. *** indicates 1 percent significance; ** indicates 5 percent; * indicates 10 percent. Robust standard errors are employed. This specification failed the overidentification test for exogeneity of instruments in the weighted model. F-statistics from first-stage regressions (relevance test) are 560.76 and 1.85 for the weighted and unweighted models, respectively.

Table 21. Impact of water aid on diarrhea incidence - urban removed

	Diarrhea Incidence^{w}	Diarrhea Incidence^{u}
Water Aid (Instrumented)	-0.002*** (0.0001)	-0.003 (0.002)
Health Aid	0.001*** (9.4e^{-5})	0.002 (0.001)
Constant	-0.006*** (4.2e^{-4})	-0.004 (0.005)

Notes: School Exposure and the Central dummy variable were instruments for Water Aid. *** indicates 1 percent significance; ** indicates 5 percent; * indicates 10 percent. Robust standard errors are employed. This specification failed the overidentification test for exogeneity of instruments in the weighted model. F-statistics from the first-stage regressions (relevance test) are 340.40 and 0.90 for the weighted and unweighted models, respectively.

Table 22. PSM: Impact of health aid on days lost to illness - urban removed

	Days Lost
Health Aid	0.096*** (0.008)

Notes: Health Aid was a dummy variable. Percent who Stopped Employment and a Water Aid Dummy were the matching characteristics. ***indicates 1 percent significance; ** indicates 5 percent; * indicates 10 percent.

Table 23. PSM: Impact of water aid on diarrhea incidence - urban removed

	Diarrhea Incidence
Water Aid	-0.007*** (0.000)

Notes: Water Aid was a dummy variable. Days Lost to Illness and a Health Aid Dummy were the matching characteristics. *** indicates 1 percent significance; ** indicates 5 percent; * indicates 10 percent.

Table 24. PSM: Impact of education aid on school exposure - urban removed

	School Exposure
Education Aid	0.152*** (0.004)

Notes: Education Aid was a dummy variable. A Water Aid Dummy, a Health Aid Dummy, Urban and Percent who Stopped Employment were matching characteristics. *** indicates 1 percent significance; ** indicates 5 percent; * indicates 10 percent.

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