Vertical Aid Externalities: Evidence from the Case of PEPFAR[*†]

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Abstract

Do vertical aid programs have unintended consequences outside of the target issue area? We investigate this question with an examination of one of the largest vertical aid programs in the world, the President's Emergency Plan for AIDS Relief (PEPFAR). Since 2003, the U.S. government has committed tens of billions of dollars to PEPFAR to combat HIV/AIDS in developing countries, the majority of which are in Africa. Critics of PEPFAR worry that a vertical program focusing on a single diseases has a negative externality, in which the influx of massive amounts of vertical aid damages broader public health systems in countries that receive PEPFAR funds. Using a difference-in-differences identification strategy, we find statistical evidence that supports PEPFAR critics: among African countries that received PEPFAR aid, six primary health outcomes improved less quickly than the African countries that received no PEPFAR aid. In other words, in the absence of PEPFAR, our analysis suggests that recipient countries would have enjoyed better rates of improvement in mortality and immunization coverage. We thus find evidence that foreign aid delivered as vertical programming may indeed come with unintended negative externalities.

Keywords: Foreign aid, state capacity, health, PEPFAR, mortality, Africa

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1 Introduction

Do vertical aid programs have harmful unintended consequences? Although vertical aid programs have enjoyed a meteoric rise in popularity over the last twenty years, their effects, particularly spillover effects, have not been rigorously evaluated to date. Vertical aid targets a specific development issue within a sector, and can be contrasted with horizontal aid, which targets one or more sectors. Vertical aid programs became popular in the late 1990s, and are most common in the areas of health and the environment (IDA 2007). Along with foreign direct investment and international remittance flows, vertical aid is one of the largest capital flows in terms of dollars across borders today. Like other international capital flows, vertical aid may have positive or negative externalities, but there is little quantitative research to provide evidence as to whether these externalities exist, and what their net effects may be. In this paper, we investigate potential spillover effects of one of the world's largest vertical aid programs, the U.S. President's Emergency Plan for AIDS Relief (PEPFAR). In particular, we investigate the effect of this HIV/AIDS relief program on *non-HIV* health outcomes.

Much of the debate regarding vertical aid programs has centered around the potential for harmful spillover effects. The literature on foreign aid has found little evidence that foreign aid has improved economic growth or development, and many scholars and policymakers have worried that aid simply lines the pockets of dictators who never had any intention to spend the money as donors wished. Vertical aid programs in the health sector have been subjected to especially withering criticism, including the accusation that foreign aid for health has done more harm than good.

Though aid and philanthropic efforts are intended to improve the lives and livelihoods of recipients, too often the full range of the impact of aid is not carefully examined. Development assistance for health (DAH) has increased greatly in the past two decades, growing from $5.7 billion to $26.9 billion between 1990 and 2010, a four-fold increase, and health aid

has surely saved many lives. But however well-intentioned, rigorous evaluation of not only intended effects, but also spillover effects, of foreign aid is necessary to determine whether aid is doing harm in the process of doing good.

In this paper, we examine the effect of vertical aid for health on health sectors in developing countries. Because we cannot directly test for the effects of vertical aid, we instead look for evidence from the case of PEPFAR, the U.S. President's Emergency Plan for AIDS Relief - the world's largest bilateral vertical aid program. We evaluate the effect of PEPFAR on non-HIV-related health outcomes, and find evidence that suggests that vertical programs may indeed have unintended negative effects on the health sector. Our analysis uncovered evidence of PEPFAR's adverse effects in recipient countries on neonatal mortality rates, an outcome measure that is extremely sensitive to the quality and performance of the state health system in developing countries. Neonatal mortality rates improved less quickly in countries that received PEPFAR funding compared to countries that received no PEPFAR funding, suggesting that PEPFAR harmed health outcomes through its deleterious effect on the state health system. Because we estimate PEPFAR's effects using a difference-in-differences identification strategy, we can attribute the detected deleterious effect to PEPFAR rather than to unobservable differences between focus and non-focus countries, or to other factors occurring around the time that President Bush authorized the program in 2003.[1]

The remainder of the paper proceeds as follows. Section 2 discusses vertical aid and its potential deleterious effect on state capacity. Section 3 discusses the case of PEPFAR. Section 4 discusses the methods and data, and Section 5 presents the key findings. Section 6 concludes.

[1]Because the Obama Administration broadened the goals of PEPFAR, partially in response to criticism about the program's vertical nature, we confine our analysis to outcomes during the first six years of the program (2003-2009).

2 Vertical Aid, Health, and State Capacity

The efficacy of foreign aid continues to be debated hotly among both policy and scholarly communities. In 2011 alone, official development assistance (ODA) exceeded 139 billion U.S. dollars in net disbursements, but evaluation of foreign aid programs and projects is often weak or lacking altogether. Much of the scholarly literature in political science and economics has examined who gives aid (and to whom), and whether aid has been effective in promoting economic growth, reducing poverty, or improving development outcomes across a variety of sectors. The evidence on aid effectiveness is decidedly mixed. While there is little evidence of any effect of aid on growth (Easterly 2009, Doucouliagos and Paldam 2009, Rajan and Subramanian 2008), a variety of studies have come to opposite conclusions on the role of aid in improving educational or health outcomes (for a sampling of findings related to health outcomes, see Mishra and Newhouse 2009, Girod 2009, Masud and Yoncheva 2005, and Boone 1996).

In part because of the poor record of foreign aid, donors have experimented with a variety of aid modalities designed to foster the development outcomes donors seek. These modalities include budget support, sectoral (program) aid, project aid, "sector-wide approaches" (SWAPs) that mix program and project aid, and technical assistance, among others. Although the allocation of aid to each type of modality varies by donor, different approaches have been favored at different times. One example of the evolving, often experimental nature of foreign aid is the meteoric rise of vertical aid and the recent backlash against this approach because of perceived deficiencies and possible negative externalities. Although vertical aid programs have been on the receiving end of much criticism, scholars know little about whether vertical aid programs indeed have the deleterious effects that critics claim. We attempt to address this question in our paper.

We use the term vertical aid to refer to aid programs that target particular development

issue. In the health context, a vertical program targets a single disease, such as malaria or HIV/AIDS. In contrast, horizontal aid programs tend to take a sectoral approach to development - strengthening the health system as a whole, rather than fighting one disease at a time (horizontal aid programs can be considered a type of SWAP or a sectoral program more generally). Vertical programming is at least forty years old, but the recent boom in vertical aid programs can be traced to the late 1990s with the creation of partnerships like the Global Fund for AIDS, Tuberculosis, and Malaria and the President's Malaria Initiative (PMI) in the health sector and the Education for All/Fast Track Initiative in the education sector (IDA 2007). Such earmarking of aid funds for very specific purposes in part reflects donor frustration with neglected development problems, the failures of budget support or sectoral support, and the greater ease of quantifying aid effectiveness at the project level.

Critics in the academic, medical, and policy communities are divided about the potential spillover effects of vertical programs. Some proponents argue that vertical aid has been particularly effective in health, especially regarding infectious diseases (one can imagine how blanketing an area with key immunizations would reduce mortality from those specific diseases), but Easterly notes, "vertical programs were not sufficient to resolve Africa's health crisis, because each program eventually reached some point of diminishing returns where there remained a segment of the population beyond its reach" (Easterly 2009, 408). Other critics worry that vertical programming comes with a number of unintended negative externalities that harm state capacity and have deleterious effects on development outcomes (Piller and Smith 2007). This concern about state capacity arises because in developing countries, the state is often the only actor with the resources, infrastructure, and ability to provide services to its population. Vertical programming may cause a number of distortions that harm the state's service infrastructure and reduce the state's ability to provide a broad range of services.

One problem is the possibility of crowding out cheaper interventions for widespread chal-

lenges in the process of implementing expensive interventions that affect only a small subset of the population (Easterly 2009). Consider the example of HIV/AIDS. The HIV/AIDS epidemic has hit hardest in sub-Saharan Africa, where the majority of countries are low or lower-middle income developing countries. In some of these countries, HIV prevalence is as high as 25% of the population. However, these countries battle not only HIV/AIDS but also a myriad of other health challenges. HIV/AIDS treatment is typically far more expensive that treatments for diseases that cause as many or more deaths. Pneumonia, malaria, diarrhea, and other infections are often the primary causes of death and disease in children below the age of 5 (Black et al. 2010), but are often prevented or treated at very low costs. The funneling of sizable sums of external funding to a handful of high-profile diseases risks neglecting not only other diseases, but also state health systems as whole. In fact, in 2007, while 23 cents of every dollar of DAH financing went towards HIV/AIDS, only 5 cents went towards broader health sector support (IHME 2010). DAH often makes up a considerable percentage of total health expenditures in developing countries, in some cases greater than 50% of a country's entire health budget. Policymakers, scholars, and medical practitioners have expressed concern that heavy reliance on donor funding for health, combined with donors' propensity to fund vertical programs, distorts domestic health priorities and neglects development of health systems (Sridhar and Batniji 2008). This financial and political prioritization of high-profile diseases over competing health priorities may have deleterious effects on public health systems in recipient countries.

Another criticism concerns brain drain. To use another example from the health sector, an influx of targeted external funding (along with the plethora of public programs and NGOs that have come into existence over the past twenty years to take advantage of this funding) has the potential to draw human resources and funding for health away from other important health programs, such as primary health care (Farag et al. 2009, Rabkin et al. 2009, England 2007). Vertical aid resources sometimes dwarf the entire country budget for the sector in

question, and are often directed toward the private or NGO sector rather than the public sector. Higher wages, better equipment, and increased resources may pull the best and the brightest health workers and other personnel away from other important yet underfunded health programs and into disease-specific programs. Health workers in developing countries often practice in both the public and private sector, and more lucrative opportunities in the private sector may lead to greater absenteeism in the public sector. Of course, no one begrudges civil service workers the right to secure better employment. Yet few developing states can afford to match wages or improve working conditions when they are already strapped for cash. As a result, while the country may enjoy some success at combating the targeted disease, vertical programming may have the unintended effect of weakening the overall capacity of the health system.

Moreover, a massive increase in vertical funding (albeit limited to a narrow cause) may result in recipient country governments choosing to allocate scarce resources to other service sectors that have not seen such a large influx of resources. Reallocation can also occur when states move their own funds from broad sector support into the narrow areas favored by donors. If a donor seeks to reduce the prevalence of malaria and demands that the recipient government demonstrate a commitment toward malaria reduction, the recipient government may reallocate general health funds for fighting malaria instead. England (2007) notes that in 2004, although HIV/AIDS was responsible for 17.6% of all deaths in Africa, 40% of all health spending went toward this sector. The sheer size of donor funds delivered as vertical aid programming can thus distort the national priorities of countries highly dependent on foreign aid, either toward or away from the issues being targeted by donors. Reallocation away from broad health system support in favor of a particular disease might increase the success of efforts targeting that disease, but are also likely to weaken the capacity of an already underfunded health system to provide basic services.

Despite these criticisms, vertical aid is not without its champions. Some members of the

health policy community argue that PEPFAR, and HIV funding more generally, will in fact have a positive spillover effect on the health sector as a whole. According to this argument, vertical funding earmarked for a single disease will inevitably enter the broader health system, increasing the overall amount of money available for public health. For example, funding to increase HIV testing of pregnant mothers may result in an increase in funding for and use of antenatal care more generally. El-Sadr and Abrams (2007) argue that HIV funding can be integrated into the wider health system, and can be used as a catalyst for developing the entire health sector.

There are thus arguments for both positive and negative spillover effects of vertical aid on outcomes not directly related to the targeted area. This paper seeks to identify whether a spillover effect does exist, and if it does, whether it helps or hurts state capacity. Because we cannot directly test the hypothesis that vertical aid harms state capacity, we examine evidence from the case of PEPFAR. In the next section, we discuss PEPFAR in more detail.

3 PEPFAR as a Case of Vertical Aid

We investigate the consequences of vertical aid programming using evidence from the case of PEPFAR, the U.S. President's Emergency Plan for AIDS Relief. Enacted in 2003 under President George W. Bush, PEPFAR is the largest bilateral aid program targeting a single disease. In the first phase of the program (2003-2008), the U.S. has disbursed more than $25 billion for HIV/AIDS prevention, treatment, and palliative care in 15 states, called focus countries (Lyman and Wittels 2010). The program's targets for the first five years were to prevent 7 million new HIV infections, treat 2 million people living with HIV/AIDS, and provide palliative care for 10 million people affected by HIV/AIDS (including those living with the disease and orphans/vulnerable children). These targets reflect the vertical nature of PEPFAR; not until the reauthorization of the program in 2008 did the scope of PEPFAR

funding expand beyond HIV/AIDS relief.

In terms of its core mission, PEPFAR has been relatively successful. In an evaluation of the program's progress toward meeting these goals, Bendavid and Bhattacharya (2009) found that between 2004 and 2007 deaths due to HIV had decreased in African focus countries, compared to African non-focus countries with an HIV epidemic, but that adult HIV prevalence had not changed during this time. While the effect of PEPFAR funding appears to have had a beneficial effect on focus countries' HIV death rates, its effect on other health outcomes is not well understood.

To give an rough idea of the scale of PEPFAR funding relative to domestic funding, consider the case of Uganda. HIV/AIDS prevalence in Uganda stands at 7.3%, while the under-5 mortality rate is 118 deaths per 1000 births. Malaria is responsible for over 30% of all child deaths, while HIV/AIDS is responsible for 6% (DHS 2007). In 2007/08, PEPFAR funding to Uganda amounted to approximately $295 million, and accounted for nearly 80% of HIV/AIDS funding in the country. In the same year, the government of Uganda spent Ushs 275 billion, approximately $160 million, on the entire health sector. Donor funding other than PEPFAR amounted to nearly $90 million (Okwero et al. 2010). In other words, donor health funding provided nearly 2.5 times the funding for health as the government itself, while at least half of the total health funding (domestic and foreign) was spent on HIV/AIDS.

Due to its size, scale, and narrowness of mission, PEPFAR offers a unique opportunity for investigating potential positive and negative externalities of vertical aid programs. Furthermore, in the initial phase of the program, only 15 states received PEPFAR aid funds. 12 of these states were located in sub-Saharan Africa. Many other sub-Saharan African states with HIV epidemics did not receive any money, despite relatively similar health conditions and HIV prevalence rates. This fact allows us to create a comparison group of countries that *could* have received PEPFAR funds but did not. Another advantage of the program for

empirical testing is that PEPFAR was a new aid program that launched in all focus countries at the same time.

To assess whether PEPFAR has weakened health systems as whole, we examine the effect of PEPFAR on health outcomes outside its stated goals in sub-Saharan Africa. Our outcomes of interest are non-HIV-related primary health outcomes.[2] Because PEPFAR's mission has expanded in its second phase (2009-present) to include public health systems strengthening, we restrict our analysis to the first phase of the program (2003-2009), during which the vast majority of PEPFAR funding was disbursed for HIV/AIDS projects.

4 Methods and Data

4.1 Sample

Our sample covers all sub-Saharan African states with an HIV prevalence of over 1% of the population, a threshold used by the Centers for Disease Control and World Health Organization to designate a prevalence rate of a disease or condition as a severe (generalized) epidemic. Of the 48 states of sub-Saharan Africa, 36 have a generalized HIV epidemic in 2003. 12 of these countries were among the first cohort of PEPFAR focus states (Table 1).[3]

4.2 Dependent Variables

Our primary dependent variable is the annual percentage change in *neonatal mortality rates*. The neonatal mortality rate is defined as the number of deaths in the first month of life per 1,000 live births. This indicator is highly sensitive to the quality of the primary health

[2]Ideally, we would also like to investigate the effect of PEPFAR on civil service employment, absenteeism rates, and recipient government funding for HIV and the health sector more broadly, but data on these measures do not exist. We therefore concentrate only on health outcome data.

[3]We follow the U.S. Agency for International Development (USAID) definition of sub-Saharan Africa. The main difference between the USAID definition and other definitions more commonly used in political science is the inclusion of Sudan in sub-Saharan Africa.

Table 1: Countries in the Sample

African Focus Countries	African Non-Focus Countries
Botswana	Angola
Cote d'Ivoire	Benin
Ethiopia	Burkina Faso
Kenya	Burundi
Mozambique	Cameroon
Namibia	Central African Republic
Nigeria	Chad
Rwanda	Congo
South Africa	Democratic Republic of Congo
Tanzania	Djibouti
Uganda	Eritrea
Zambia	Gabon
	Ghana
	Guinea
	Guinea-Bissau
	Lesotho
	Liberia
	Malawi
	Mali
	Sierra Leone
	Sudan
	Swaziland
	Togo
	Zimbabwe

system, and we argue that it is therefore a suitable proxy for non-HIV/AIDS health outcomes more generally. Nearly 75% of global neonatal deaths occur in the first week of life, and 25% of all neonatal deaths occur during the first 24 hours after birth. Interventions that can save infants in the first month of life largely require the presence of the state in the form of public health infrastructure, trained medical personnel, and individualized clinical care (Lawn, Cousens, and Zupan 2005). In developing countries, the state is often the only actor that possesses the capital, resources, and infrastructure to deliver these services. Neonatal mortality rates tend to be highest in countries where women are less likely to deliver in a health facility and where the delivery occurs without a skilled birth attendant present (Lawn,

Cousens, and Zupan 2005).

Before proceeding, it is worth pointing out a few characteristics of mortality data. First, mortality rates are heavily time-trended. That is, mortality rates tend to decline over time due to advances in medicine and the dissemination of technology, medication, and better health practices. Mortality data also have a "floor" in the sense that reductions in mortality become more difficult as mortality rates decrease. Because of the time trend and floor effect, we transform the base neonatal mortality data from IHME into annual percentage changes. Percentage changes reduce the effect of the time trend and obviate the need to include a time trend in the model we describe later in this paper. Percentage changes also address the floor effect by recognizing that there exists increasing marginal costs to further reductions in mortality - in other words, the change from 10 to 5 deaths per 1,000 live births is closer to the reduction of 50 to 25 than the reduction of 50 to 45 deaths.

Our data come from the University of Washington's Institute of Health Metrics and Evaluation (IHME). IHME calculates total neonatal mortality rates based on data from vital registration systems, surveys, summary birth histories, and complete birth histories. Data is available annually and covers nearly all countries in the world. Unfortunately, due to lack of quality data, it is not possible to distinguish between neonatal deaths due to HIV/AIDS and neonatal deaths due to other causes. However, we do not believe this inability to disaggregate the data is of much concern. Even in countries with high HIV prevalence, the percentage of neonatal deaths due to HIV is suspected to be very small when compared with the percentage caused by common killers: sepsis and pneumonia, complications of asphyxia, preterm birth, congenital problems, tetanus, diarrhea (Lawn, Cousens, and Zupan 2005). These causes are responsible for nearly 95% of the estimated four million neonatal deaths every year. HIV/AIDS likely accounts for a small fraction of the remaining 5-6% of deaths.

In addition, it is unlikely that the disease will have progressed far enough to severely compromise the immune systems of HIV-infected neonates to be a major cause of death in

the first month of life. Evidence from South Africa suggests that mortality due to HIV/AIDS peaks at age 2-3 months, rather than in the first month captured by measures of neonatal mortality rates (Bourne et al 2009). The authors suggest that the peak in mortality occurs at this period because of the loss of acquired maternal immunity that protects the infant. For neonates not infected *in utero* or during the intrapartum or early post-partum periods (i.e., when transmission occurs through breastfeeding), the deadly effects of HIV may not manifest until even later in infancy, and are therefore unlikely to bias our analysis.

As a robustness check, we run our analysis using seven additional dependent variables. *Under-5 mortality rates* is an indicator commonly used in the health politics literature. This measure is defined as the number of deaths before age 5 per 1,000 live births, and as with neonatal mortality, we calculate annual percentage changes in under-5 mortality. Like neonatal mortality, under-5 mortality rates in developing countries respond to basic health conditions and are correlated with neonatal mortality because the latter is a component of the former. However, it is less ideal as a proxy for the health system because some of the interventions that decrease under-5 mortality can be delivered in the home or in the community, and are therefore dependent less on the state or the public health system. Data are from IHME.

Because under-5 mortality rates include neonatal mortality rates, we separate out *age 1 to 5 mortality*. We include this dependent variable to determine whether any results we observe are being driven solely by neonatal mortality, our preferred dependent variable. This measure is defined as number of deaths between ages 1 and 5 per1,000 live births (i.e., it excludes "infant" mortality - deaths before the age of 1). Base data are from IHME, and we transform the data into annual percentage changes.

Maternal mortality rate (deaths per 100,000 live births) is another measure of the quality of the health care system. Because much maternal mortality in developing countries is due to hemorrhage and postpartum infection, maternal death occurs where mothers cannot reach

health services, or where health clinics do not have basic treatments (antibiotics or uterotonic agents such as misoprostol) or emergency care for pregnant or postpartum mothers. Maternal mortality is thus another measure of primary and emergency health care. Data on maternal mortality comes from IHME, which has calculated maternal mortality rates and deaths not due to HIV (Hogan et al. 2010). We calculate annual percentage changes from this base data.

We also test the effect of PEPFAR on four additional proxies for the performance of the public health system, namely vaccination rates. Unlike mortality rates, vaccination rates are narrow measures for health system performance because they capture the ability of the health system to carry out a single, defined task. Vaccination rates are measured as the percentage of one-year-olds who have received adequate immunization for each vaccine. We include immunization rates (as levels) for the four vaccines covered under the World Health Organization's Expanded Program on Immunization: tuberculosis; diphtheria, tetanus, and pertussis (third dose, DTP3); measles (first dose); and polio (third dose). Vaccination data come from the World Health Organization.

Table 2 shows summary statistics for the dependent variables for focus counties and non-focus countries in 2003, the year PEPFAR was announced but before funds reached recipient countries. With the exception of maternal mortality rates, focus countries have slower rates of reduction in mortality (perhaps because of better levels of mortality) but higher levels of immunization coverage compared to non-focus countries. Again with the exception of maternal mortality, these differences are not statistically significant at conventional levels.

4.3 Independent Variables

Our independent variable is the *focus country* status in the PEPFAR program, and we operationalize this status as a dichotomous variable that takes 1 if the country was a focus country in the initial cohort of PEPFAR recipients, and 0 otherwise. Because we use a

Table 2: Means and Standard Deviation for Dependent Variables in 2003

Dependent Variable	Focus Countries	Non-Focus Countries	Difference is Significant?
Neonatal mortality	-1.39 (2.65)	-1.70 (1.55)	No
Under-5 mortality	-1.73 (4.13)	-2.18 (2.01)	No
Age 1 to 5 mortality	-2.20 (5.93)	-2.82 (2.58)	No
Maternal mortality	-4.90 (2.58)	-2.83 (2.72)	Yes
Tuberculosis	82.67 (15.19)	78.42 (16.81)	No
DTP	72.92 (19.57)	67.64 (18.81)	No
Measles	71.75 (18.11)	65.17 (16.06)	No
Polio	75.17 (17.30)	66.72 (16.89)	No

difference-in-difference-in-differences model (discussed below), we include a second dichotomous variable, *policy year*, which takes 1 if the PEPFAR program is active that year and 0 otherwise. As discussed above, President Bush announced PEPFAR in January 2003, but disbursed aid funds could not have reasonably reached the focus countries until 2004.[4] We therefore consider 2003 to be the threshold year (the "break" in the data) and code 2004 as the first year in which the PEPFAR program is active.

4.4 Covariates

We include a number of covariates in our analysis to increase the precision of the estimate of the effect of PEPFAR funding. The overall level of economic development is strongly associated with better primary health outcomes (Pritchett and Summers 1996). Higher levels of development could affect health outcomes through better health system infrastructure, through greater individual ability to access the health system, or both. We control for the natural log of *GDP per capita* using data from the Penn World Table 7.1. Figures are constant 2005 U.S. dollars.

[4]We do not have any evidence, based on PEPFAR budget reports, that funds were disbursed prior to Fiscal Year 2004, which runs from 1 October 2003 to 30 September 2004. To receive money, organizations had to submit a proposal, funds had to be disbursed to the recipients, and the recipients have to spent the money. We therefore think that, despite the January 2003 authorization date, the program could not have had much effect prior to January 2004.

Because PEPFAR aid is a subset of health-designated foreign assistance (itself a subset of total development aid), we control for the log of *health aid per capita* in 2009 constant U.S. dollars. Health aid figures come from IHME's development assistance for health dataset. This variable includes foreign aid disbursements earmarked for health from DAC countries, UN agencies, the World Bank and regional development banks, and some public-private partnerships like the GAVI Alliance. Figures for nonprofit organizations and non-DAC countries are unavailable, but DAC and multilateral flows represent a substantial portion of total development aid that we do not expect the missing data to bias our results. HIV aid is not included in this variable. Data are given in constant 2009 U.S. dollars.

Recipient countries vary dramatically in per capita government spending on the public health sector. We control for this variation using *government health expenditures per capita* in 2005 constant U.S. dollars. We calculate these data using variables from the World Development Indicators (WDI) and the Penn World Table 7.1 (Heston, Summers, and Aten 2011). A major drawback of this variable is that we cannot separate out domestically-sourced government expenditures from foreign-sourced government expenditures. However, the correlation between health aid and government health expenditures is not high (0.40) and substantially lower when these values are expressed in logged per capita terms as they are in the regression models (0.19). Moreover, as noted, PEPFAR funds are generally considered off-budget because aid bypasses recipient governments and is controlled directly by institutions of the U.S. government.

A large body of literature argues that democracy has a positive causal effect on public goods provision relative to authoritarianism (Lake and Baum 2001; Bueno de Mesquita, Smith, Siverson, and Morrow 2003; Besley and Kudamatsu 2006; Kudamatsu 2010). If so, then regime type might be associated with better health outcomes. We control for the effect of *regime type* using the Polity IV index's polity2 variable (Marshall et al. 2009).

Total *population size* can affect health outcomes through an economy of scale effect. As

the population increases, so should the return to investments in the health system, from hospitals to clinics to physicians to large scale health interventions. However, it is unclear whether population size affects rates of change in our health outcomes of interest. Population is expressed in thousands and is logged to reduce skew in the distribution. Data are from the Penn World Tables 7.1

Finally, we include a covariate for the degree of *urbanization*, which is defined as the percent of the population living in urban areas and comes from the WDI. Underserved populations tend to reside in rural areas, which are hard to access from a health intervention perspective. Rural areas may also have reduced or poor quality public services, including primary health services. States with a higher degree of urbanization are therefore more likely to have better health outcomes, all else equal, than states with a greater proportion of rural areas.

Table 3 displays summary statistics for the transformed covariates for 2003, the year in which PEPFAR was enacted. Focus countries are more populous and more democratic than non-focus countries, but were not statistically significantly different from non-focus countries on the other covariates.

Table 3: Means and Standard Deviations for Covariates in 2003

Control Variable	Focus Countries	Non-Focus Countries	Difference is Significant?
GDP per capita	7.25 (0.96)	6.89 (0.85)	No
Health aid per capita	1.68 (0.74)	1.24 (0.74)	No
Gov't health spending per capita	4.40 (1.09)	3.98 (0.74)	No
Polity	3.17 (4.45)	-0.13 (4.64)	Yes
Population	16.74 (1.30)	15.64 (1.10)	Yes
Urbanization	32.66 (15.69)	36.87 (17.91)	No

4.5 Model

The main threat to identification of a causal effect of PEPFAR on neonatal mortality is the non-random selection of focus countries. The 2003 authorization legislation that specified

the focus countries does not indicate the selection criteria, but HIV prevalence does not appear to be the sole determining factor. In fact, several high prevalence countries, such as Swaziland and Lesotho, were not included in the first round of PEPFAR funding. Country selection likely depended on political and diplomatic considerations, including the presence of an existing USAID infrastructure.[5]. However, focus countries may differ from non-focus countries in ways that can bias our analysis even after controlling for observable factors.

To address this problem, we exploit the fact that PEPFAR is a new program announced in 2003 and use a difference-in-differences design to estimate the causal impact of PEPFAR on neonatal mortality rates. This model allows us to construct a counterfactual for what would have happened to focus countries had they not received funding by generating counterfactual post-PEPFAR focus country outcomes based on the change in outcomes for non-focus countries after 2003. We compare changes in the rates of decline of neonatal mortality for focus and non-focus countries for the period 1998-2009, which covers six years before and after PEPFAR was announced in 2003 for a total of twelve years. If PEPFAR adversely affects public health systems, we expect to observe a slowing in the annual percentage change in neonatal mortality rates. That is, we expect that countries that received PEPFAR aid experienced slower improvements in mortality relative to countries that did not receive such aid.

The difference-in-differences method does not require that focus and non-focus countries be similar on observables, only that the changes in neonatal mortality rates for focus countries in the absence of PEPFAR funding can be validly estimated by changes in the outcomes for non-focus countries. Few factors at or around 2003 would have systematically affected only non-focus states in ways that would bias our interpretation of the effect of PEPFAR funding. Civil war could be one potential candidate, but while civil war onset or termination could worsen or improve mortality outcomes in individual countries, the effect is likely to disappear

[5]Personal communication from a USAID official

when neonatal mortality is averaged across focus and non-focus states. Moreover, neonatal mortality continues to decline across countries, even among those experiencing civil conflict.[6] Nevertheless, if civil war did affect the outcome trends for the comparison countries, the bias would be small (due to averaging).

We estimate the following difference-in-difference model:

$$y = \beta_0 + \beta_1(F_i) + \beta_2(P_t) + \beta_3(F_i * P_t) + X_{it} + \epsilon_{it}$$

where F equals 1 if a country is a focus country and 0 otherwise, P (policy year) equals 1 if the year is 2004 or after and 0 otherwise, subscript i indexes countries, subscript t indexes time in years, y is the dependent variable (here, the annual percentage change in neonatal mortality rate), X is a vector of covariates, and ϵ is the error term.The coefficient on the interaction term, β_3, captures the effect of interest: whether PEPFAR affected the rate of change of neonatal mortality trends in focus countries compared to non-focus countries.

5 Results and Discussion

Table 4 shows the main results for neonatal mortality. In the model without covariates, we find evidence of a substantively small deleterious effect of PEPFAR on neonatal mortality rates. Countries that received PEPFAR funding experienced on average a 0.64% slowing in the rate of change in neonatal mortality, a result that is not quite statistically significant at the 0.05 level (p=0.077). Annual percentage changes in neonatal mortality rates among focus countries range overall years in our sample from -5.22% (relatively fast annual decreases of mortality to 6.01% (relatively fast increases) with a mean of -1.41% and standard deviation of 1.72. A change of 0.64% is thus less than a one standard deviation decrease in improvement due to PEPFAR. The good news is that the harmful effect of PEPFAR on health systems is

[6] A major exception is Rwanda during the 1994 genocide. Annual changes mortality rates immediately returned to pre-conflict rates after the genocide, prior to the period under study in this paper.

small - enough to slow the rate of reduction in mortality but not enough to reverse progress or increase mortality rates.

The model with covariates corroborates our findings from the unadjusted model. The addition of covariates improves the statistical significance on the coefficient of the interaction term (p=0.027), while the substantive size of the coefficient is marginally larger at 0.72%. Again, a quick look at the coefficients on the other covariates indicates that PEPFAR slowed mortality improvement rates but did not reverse progress. Focus country mortality rates are thus worse compared to pre-PEPFAR rates and non-focus country rates, but mortality continues to decline - albeit more slowly - for the focus countries.

Most of the covariates behaved as expected. Higher levels of per capita health aid and per capita government health spending are associated with faster reductions in neonatal mortality. Greater urbanization also brings mortality rates down faster than lower urbanization, a result consistent with the notion that it is harder to deliver services to rural populations. One surprising was result was the coefficient and sign on (logged) per capita GDP, which is associated with slower reductions in neonatal mortality rates. This result may stem from the fact that some countries in Southern Africa have relatively high levels of GDP per capita but relatively slow improvements in mortality. However, removing these countries from our sample does not alter our conclusions or the positive sign on the GDP covariate.

For robustness, we examined the effect of PEPFAR on seven other dependent variables. Tables for these results are included in the appendix. The interaction term was positive and statistically significant in models using under-5 mortality and age 1 to 5 mortality, indicating that PEPFAR slowed the rate of improvement for these outcomes (Tables 5 and 6). As with neonatal mortality, the harmful effect of PEPFAR on health systems was not great enough to cause increases in mortality. The significance on the interaction term for under-5 mortality was not surprising since neonatal mortality is a component of under-5

Table 4: Difference-in-differences regression

	(1)	(2)
	Neonatal mortality	Neonatal mortality
Focus country	-0.110	-0.448
	(0.840)	(0.318)
Policy year	-0.107	0.329
	(0.524)	(0.142)
Focus*Policy	0.635	0.722*
	(0.077)	(0.027)
GDP		1.373***
		(0.001)
Health aid		-0.430**
		(0.001)
Gov't health spending		-0.289*
		(0.045)
Polity		0.0362
		(0.261)
Population		-0.0269
		(0.872)
Urbanization		-0.0314*
		(0.020)
Constant	-1.565***	-8.088*
	(0.000)	(0.040)
N	432	424
r2	0.0160	0.282

p-values in parentheses

Standard errors clustered by country

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

mortality. The evidence of a deleterious effect of PEPFAR on mortality rates of children between the ages of 1 to 5 does demonstrate that the results we observe are not being driven solely by the data on neonatal mortality.

Maternal mortality rates are the only mortality variable for which we do not observe a statistically significant deleterious effect of PEPFAR on state health systems. Although the sign on the interaction term is positive, indicating a slower decline in mortality, the coefficient is not statistically significant at conventional levels (Table 7 in the appendix). There are two possible reasons why we do not observe a statistically significant coefficient in the case of maternal mortality. First, we do not have access to reliable data on maternal mortality not due to HIV past the year 2008. Models using maternal mortality rates therefore use a balanced window of 1999-2008, which contains 71 fewer observations than the full twelve-year window of 1998-2009 for our other dependent variables. The loss of observations makes it difficult to detect a statistically significant effect of PEPFAR, if any, for this dependent variable.

A second reason we believe that we do not observe the same harmful effect of PEPFAR on maternal mortality rates is because of a potential positive spillover from PEPFAR. Many HIV programs target mothers in an effort to prevent mother-to-child-transmission (PMTCT) of HIV. PMTCT programs include a variety of services for the care and treatment of pregnant women which can reduce the likelihood of maternal death. The most common causes of maternal death include bleeding, high blood pressure, infection, and difficult labor (The Partnership for Maternal, Newborn & Child Health 2011, 6). Many interventions that reduce maternal mortality must be delivered by professional health workers at the primary care level. Mothers who receive HIV-related services, such as antiretroviral drugs funded by PEPFAR, may be more likely to utilize maternal services that can reduce the risk of maternal death. In other cases, PMTCT programs may explicitly include a maternal care component along with HIV-related services. In both scenarios, increased access to and uptake of maternal care

can explain why we do not observe a harmful effect of PEPFAR on maternal mortality. We do not expect to see a similar positive spillover for other primary health outcomes because the overlap between maternal

We also examined the effect of PEPFAR on coverage rates of four common vaccinations (Table 8 in the appendix). Across all four immunizations, we found evidence corroborating our mortality results: among focus countries, PEPFAR was responsible for declines of between 8 to 12% in coverage compared to what those countries would have achieved in the absence of that funding.[7]

The above conclusions are robust to alternate model specifications.[8] First, we experimented with different bandwidths. Our preferred model included a balanced window of twelve years from 1998-2009. This window allow us to utilize all the data we have available to us, but a wider bandwidth reduces our ability to estimate the true causal effect of PEPFAR. Shorter windows lose observations but allow for a more precise estimation of PEPFAR's effect by getting closer to the time at which PEPFAR was enacted. A longer window of fourteen years as opposed to twelve allows for more time to observe PEPFAR's effect on health outcomes, but this bandwidth is not ideal because we lack health aid data for five countries in 2010. We reran the analysis with alternate windows, including fourteen-, ten-, eight- and six-year windows. The model with fourteen years of data strongly corroborates

[7]We believe that immunization coverage rates should be measured as levels of coverage rather than annual percent changes in levels. We used annual percent changes for mortality because mortality data are heavily time-trended and because of the floor effect in which marginal reductions in mortality are increasingly hard to achieve. This floor effect arises because the causes of mortality at high and mid-range levels or mortality are fundamentally different from the causes of mortality at low levels of mortality. In light of the differential causes of mortality, it is not appropriate to compare levels of mortality directly. Immunization rates do have a ceiling (more than 100% of one-year-olds cannot be immunized), but unlike mortality, there are not differential causes of higher or lower immunization. In other words, the process of immunization is the same whether 65% or 95% of the population of one-year-olds are vaccinated, but the process through which mortality rates are reduced is not the same at reductions from 50 to 45 deaths per 1,000 compared to 10 to 5 deaths per 1,000. Nevertheless, we did run a model with immunization rates measured as annual percentage changes, and in this model, the interaction term never reaches statistical significance (see Table 8 in the appendix).

[8]Supplemental tables are available by request from the authors.

our findings from the main analysis. The models with shorter windows also support our conclusions, although the statistical significance falls between each model (p=0.05, p=0.068, and p=0.119, respectively). This loss of significance is not surprising given that each alternate window represents a loss of between 33 and 67 observations from the longer window.

Second, we adjusted the sample to reflect the sample used in the Bendavid and Bhattacharya (2009) paper on PEPFAR. The Bendavid and Bhattacharya paper used the same generalized epidemic criteria for case selection, but their data on HIV prevalence yielded five additional cases: Gambia, Madagascar, Niger, Senegal, and Somalia. Our results do not change whether we use the Bendavid and Bhattacharya sample or our smaller sample of 36 countries.

Third, we added an initial levels term to our preferred model. The initial levels term captured the baseline level of dependent variable in 1997. The inclusion of this term did not alter our main results.

Fourth, we ran a model that included HIV prevalence as a covariate. Although all countries in the sample have a generalized HIV epidemic, the severity of the epidemic could affect non-HIV health outcomes through the burden HIV imposes on human and other resources for health. For example, if many health workers themselves contract HIV, this is likely to further decrease the number of health workers available to service the entire sector and result in a deterioration of the health sector as a whole (Tawfik and Kinoti 2003). The drawback of using HIV prevalence data is that two countries - Democratic Republic of the Congo and Ethiopia - are entirely missing from the dataset. Moreover, Ethiopia is a focus country, so its absence reduces the number of focus countries in our sample to 11. Our results are weakly significant (at the p<0.10 level) for neonatal, under-5, and age 1 to 5 mortality, and strongly significant (p<0.05 or better) for the immunization coverage variables.

Fifth, we tried an alternate construction of the health aid covariate. Our health aid covariate only included aid that could plausibly affect neonatal mortality. We excluded aid

coded in the IHME database as aid designated for for HIV programs and noncommunicable diseases (NCDs) such as asthma, cataracts, and heart disease. As we stated earlier, HIV is responsible for a very small proportion of neonatal deaths (and a somewhat larger proportion of deaths between ages 1 to 5). Still, we recalculated the health aid variable to include HIV aid, and found that our analysis was robust to this alternate construction. We prefer to exclude HIV aid from the health aid covariate because we cannot easily separate out the portion of HIV aid due to PEPFAR. We did not include aid for NCDs in this robustness check because NCDs are very rarely causes of mortality among neonates and children in developing countries.

Finally, we tried a model where relevant covariates were entered as both levels and annual percent changes in levels. We prefer levels because we believe that the level of GDP per capita or the level of health aid per capita is more important for achieving continued reductions in mortality than changes in funding. Very poor countries that spend a tiny proportion of their GDP on health are unlikely to boost the rate of decline in mortality by increasing spending if the absolute level of funds available remains very small. However, we did try a model in which we included levels and rates of change for the three fiscal covariates (GDP per capita, health aid per capita, and government health expenditures per capita) and found that our results were robust to this alternate model specification.

These results provide evidence that vertical aid programs like PEPFAR come with an unintended negative externality. While PEPFAR has been relatively successful at saving the lives of HIV/AIDS patients, it has done so at the cost of weakening the capacity of the state health system. Focus countries have seen a slowing in the reduction of neonatal mortality, an indicator that closely tracks with the performance of the public health system. PEPFAR does not appear to have harmed state capacity so greatly that neonatal mortality is increasing. Rather, our evidence suggests that the harmful effect is small and reduces the rate of improvement. Unfortunately, our research design and lack of cross-country data do

not allow us to directly test the hypothesis that vertical aid programs harm state capacity or the mechanisms through which PEPFAR has harmed the health system as a whole, but the fact that we did find evidence of harm does suggest that the aid community's partial turn away from vertical programs and the 2008 expansion of PEPFAR's mandate to include health systems strengthening is not unwarranted.

6 Conclusion

The landscape of foreign aid has undergone a rapid change in the last twenty years. Vertical aid programs have become a favored modality for donors, and vertical aid for HIV, malaria, and tuberculosis often dwarfs the entire health budgets of recipient states. Without a doubt, vertical programs have achieved some degree of success at fulfilling their own goals, such as reducing mortality due to malaria or HIV/AIDS. However, critics have assailed vertical programs for real and perceived deficiencies, including distorting recipient country priorities, promoting absenteeism or brain drain, and weakening state capacity more generally. This line of criticism reflects a general question about spillovers, both negative and positive, of international capital flows.

Our analysis of the health sector and the case of PEPFAR largely supports these critics' concerns about vertical aid spillover. We examined the effect of PEPFAR on neonatal mortality rates as an indirect test of PEPFAR's effect on public health systems in focus countries. Interventions that reduce neonatal mortality are highly dependent a functioning public health system, and we uncovered evidence that neonatal mortality fell more slowly in countries that received PEPFAR funding. While this evidence is far from conclusive, it does suggest that the aid critics have been right to worry about the proliferation of vertical aid programming and the harmful effect on the state's ability to provide for sustainable development solutions within its own borders.

Nevertheless, much more work is needed to better understand the conditions under which negative spillovers could occur, as well as the mechanisms through which vertical programs actually harm state capacity. Our analysis provides suggestive evidence using national-level data. Future research will make use of individual-level survey data from the Demographic and Health Surveys. We will also continue to look for a more direct test of the mechanisms through which PEPFAR harms state capacity, as we lack data to examine mechanisms in this paper.

Our findings contribute to the literature on foreign aid and to the international political economy literature in general. Few cross-national studies have found any evidence that foreign aid improves growth or development, and our finding suggests that in some cases, foreign aid can unintentionally hurt development outcomes even as programs are successful in achieving their own limited goals.

For policymakers, this finding suggests that donors should be careful in how and where they allocate aid. Vertical programming has benefits (perhaps greater likelihood of achieving the intended outcome) and drawbacks (unintended negative consequences on state infrastructure). We do not advocate that vertical programs should necessarily be avoided, only that donors be aware of how foreign aid alters incentives for both partners and recipients of aid in ways that may not produce a net social benefit. Undoubtedly PEPFAR has saved many lives of HIV patients, especially adults, but the evidence we have presented here suggests it has done so at the cost of saving fewer infants and children.

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A Appendix

Table 5: Difference-in-differences regression: Under-5 mortality

	(1)	(2)
	Under-5 mortality	Under-5 mortality
Focus country	-0.252	-0.720
	(0.742)	(0.263)
Policy year	-0.244	0.386
	(0.383)	(0.228)
Focus*Policy	0.881	1.035*
	(0.091)	(0.030)
GDP		1.680**
		(0.002)
Health aid		-0.641***
		(0.001)
Gov't health spending		-0.277
		(0.265)
Polity		0.0566
		(0.230)
Population		-0.0398
		(0.864)
Urbanization		-0.0414*
		(0.018)
Constant	-1.986***	-9.946
	(0.000)	(0.070)
N	432	424
r2	0.0117	0.235

p-values in parentheses

Standard errors clustered by country

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6: Difference-in-differences regression: Age 1 to 5 mortality

	(1)	(2)
	Age 1 to 5 mortality	Age 1 to 5 mortality
Focus country	-0.434	-1.117
	(0.677)	(0.193)
Policy year	-0.302	0.535
	(0.402)	(0.214)
Focus*Policy	1.143	1.361*
	(0.097)	(0.030)
GDP		2.203**
		(0.002)
Health aid		-0.878***
		(0.001)
Gov't health spending		-0.322
		(0.365)
Polity2		0.0824
		(0.198)
Population		-0.0228
		(0.942)
Urbanization		-0.0543*
		(0.016)
Constant	-2.545***	-13.56
	(0.000)	(0.068)
N	432	424
r2	0.00966	0.228

p-values in parentheses

Standard errors clustered by country

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7: Difference-in-differences regression: Maternal mortality (1999-2008)

	(1) Maternal mortality	(2) Maternal mortality
Focus country	-1.363	-0.399
	(0.157)	(0.677)
Policy year	-1.480***	-1.139*
	(0.001)	(0.013)
Focus*Policy	1.056	1.334
	(0.201)	(0.132)
GDP		-1.769**
		(0.002)
Health aid		-0.327
		(0.289)
Gov't health spending		0.0951
		(0.723)
Polity		-0.0460
		(0.444)
Population		0.00190
		(0.994)
Urbanization		0.0625**
		(0.003)
Constant	-2.678***	7.126
	(0.000)	(0.242)
N	360	353
r2	0.0749	0.248

p-values in parentheses

Standard errors clustered by country

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8: Difference-in-differences regression: Immunizations as levels

	(1) Tuberculosis	(2) DTP	(3) Measles	(4) Polio
F	3.264	8.438	6.414	7.048
	(0.491)	(0.200)	(0.304)	(0.257)
P	5.394	11.54**	7.115*	10.54**
	(0.134)	(0.002)	(0.030)	(0.003)
FP	-9.774**	-11.82**	-8.963**	-11.26**
	(0.002)	(0.002)	(0.006)	(0.001)
GDP	7.994*	10.96**	8.914**	10.51**
	(0.020)	(0.003)	(0.005)	(0.003)
Health aid	5.033**	4.286	5.334*	4.500*
	(0.009)	(0.053)	(0.010)	(0.044)
Gov't health spending	-1.545	-3.262	-2.216	-2.826
	(0.202)	(0.050)	(0.116)	(0.115)
Polity	0.407	0.481	0.254	0.529
	(0.151)	(0.288)	(0.523)	(0.240)
Population	-1.580	-2.504	-1.896	-2.216
	(0.374)	(0.276)	(0.371)	(0.302)
Urbanization	-0.370*	-0.624***	-0.484**	-0.608***
	(0.048)	(0.000)	(0.004)	(0.000)
Constant	60.02	53.87	49.12	51.08
	(0.098)	(0.229)	(0.171)	(0.222)
N	422	422	422	422
r2	0.340	0.393	0.359	0.403

p-values in parentheses

Standard errors clustered by country

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9: Difference-in-differences regression: Immunizations as percent changes

	(1) Tuberculosis	(2) DTP	(3) Measles	(4) Polio
Focus country	0.884	-2.344	-2.577	-3.060
	(0.501)	(0.130)	(0.153)	(0.077)
Policy year	2.222*	1.940	-0.297	-0.995
	(0.042)	(0.463)	(0.899)	(0.532)
Focus*Policy	0.307	1.409	2.749	1.694
	(0.830)	(0.539)	(0.332)	(0.385)
GDP	-2.510	-2.984	-3.379	-2.646
	(0.056)	(0.082)	(0.111)	(0.091)
Health aid	-1.878**	-2.123**	-1.105	-1.005
	(0.007)	(0.007)	(0.180)	(0.062)
Gov't health spending	0.767	1.096	1.186	1.009
	(0.063)	(0.076)	(0.116)	(0.110)
Polity	-0.0844	-0.101	-0.178	-0.0809
	(0.352)	(0.388)	(0.123)	(0.443)
Population	-0.0519	0.198	0.193	0.644
	(0.923)	(0.763)	(0.754)	(0.318)
Urbanization	0.0984*	0.0908	0.109	0.112
	(0.035)	(0.251)	(0.161)	(0.076)
Constant	15.46	17.21	17.93	6.813
	(0.194)	(0.271)	(0.232)	(0.653)
N	420	420	420	420
r2	0.0420	0.0329	0.0206	0.0347

p-values in parentheses

Standard errors clustered by country

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$