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What Has 100 Billion Dollars Worth of Debt Relief Done for Low-Income Countries?

Nicolas Depetris Chauvin
Princeton University and Inter-American Development Bank

Aart Kraay*
The World Bank

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Abstract: Between 1989 and 2003, low-income countries received $100 billion in debt relief. The stated objectives for much of this debt relief have been to reduce debt overhang and to free up recipient government resources for development spending that would otherwise have been used for debt service. In this paper we empirically assess the extent to which debt relief has been successful in meeting these objectives, using a newly-constructed database measuring the present value of debt relief for 62 low-income countries. We find little evidence that debt relief has affected the level and composition of public spending in recipient countries. We also do not find evidence that debt relief has raised growth, investment rates or the quality of policies and institutions among recipient countries. Although we cannot rule out the possibility that our failure to find evidence of positive impacts of debt relief is due to a variety of data and statistical problems, the evidence reported here does suggest that some skepticism is in order regarding the likely benefits of further large-scale debt relief.

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" No civilised country should try to collect the debts of people that are dying of hunger and disease and poverty."
--Jeffrey Sachs, quoted in the Financial Times, July 6, 2004

"The original focus of the Highly Indebted Poor Countries (HIPC) Initiative was on removing the debt overhang and providing a permanent exit from rescheduling. Relief can also be used to free up resources for higher social spending aimed at poverty reduction to the extent that cash debt-service payments are reduced. These are now twin objectives"
--World Bank and IMF Statement of Objective of Enhanced HIPC Initiative, 1999.

"The HIPCs' unmanageable debt is a symptom of deeper structural problems. While the HIPC initiative appears likely to provide much-needed respite from high debt service, debt relief is not a panacea for broader economic development problems, nor is a one-time debt reduction a guarantee that the problem will not re-emerge."
--World Bank Operations Evaluation Department Review of HIPC Initiative, 2003

1. Introduction

Between 1989 and 2003, low-income developing countries received $100 billion in debt relief in 2003 present value terms. The arguments in favor of such substantial debt relief have ranged from the moral to the mundane. The moral argument is that it simply is unjust for rich countries to try to collect on the debts owed to them by countries that are desperately poor. This line of argument has featured prominently among advocates of debt relief, and is exemplified by the first quotation above. Slightly over half of the $100 billion in debt relief has been granted in the context of the Highly-Indebted Poor Countries (HIPC) initiative organized by the World Bank and the International Monetary Fund. Although in part a response to popular pressures for debt relief, the objectives of this initiative have been rather more mundane economic ones, as shown in the second quotation above. They are to free up public resources for development spending that would otherwise have been devoted to debt service, and to eliminate a variety of disincentives for good public policy and private activity caused by high debt burdens that are collectively referred to as debt overhang.

The objective of this paper is to empirically assess how successful debt relief has been in meeting these objectives. A practical difficulty facing such an exercise is that comprehensive data on debt relief among low-income countries does not exist. Although debt relief under the HIPC initiative has been fairly well documented, data on

the very substantial amounts of pre-HIPC and non-HIPC debt relief is scarce and fragmented. The first contribution of this paper is to provide a set of estimates of the present value of debt service reductions due to debt relief granted since 1989 when concessional reschedulings of low-income country debt began in earnest. Our dataset covers 62 low-income countries including the 38 countries currently eligible for the HIPC initiative. We provide two alternative estimates drawing on debtor-reported and creditor-reported data respectively. An important feature of these estimates is that they capture reductions in the present value of debt due to debt relief. These can be very different from reported data on nominal amounts of debt forgiven which do not reflect cross-country differences in the degree of concessionality of the debt eliminated, and also do not capture reductions in the present value of debt due to concessional reschedulings.

The second contribution of this paper is to use this new dataset on debt relief to empirically examine its impacts on outcomes of interest in the recipient countries. We divide the 15-year period spanned by our data into three five-year periods, and examine the impact of debt relief on subsequent changes in several outcome variables suggested by the stated objectives of debt relief programs. In particular we ask whether debt relief systematically affects the level and composition of public spending, consistent with the objective of debt relief to free up resources for public spending on development. We also ask whether debt relief improves the incentives for good policy choices and for investment, as would be the case if debt relief reduces debt overhang problems. We also ask whether there is any discernable impact of debt relief on per capita GDP growth as a crude way of summarizing the overall development impact of debt relief. Our empirical strategy is akin to the standard "difference-in-difference" estimator familiar from the microeconometric literature on impact evaluation. We ask whether countries receiving more debt relief over a given five-year period were more likely to see improvements in average outcomes in the next five-year period relative to the first. As we discuss in more detail below, this strategy addresses a number of econometric concerns that arise from the fact that debt relief is not randomly assigned to countries but may be purposefully targeted to country characteristics, which in turn confounds the assessment of the effects of debt relief.

Our findings are not very supportive of the idea that debt relief has been successful in achieving these economic objectives. We first look for evidence of a linear

relationship between debt relief and subsequent changes in five-year averages of outcome variables, and find little in the way of robustly significant effects. We next consider the possibility that the true effects of debt relief on outcomes are nonlinear. In particular, we hypothesize that debt relief is more likely to be effective in countries with good institutions and policies, and in countries where the debt forgiven was previously being serviced. While these interaction effects appear to us to be plausible a priori, we do not find any evidence that they are empirically important in our sample of countries. Another possibility for the insignificance of debt relief is that debt relief is an imperfect proxy for changes in overall debt burdens, and that it is the latter that matter for outcomes. However, we also do not find any evidence that reductions in total debt burdens from all sources, not just debt relief, are positively associated with outcomes.

Despite its importance and prominence on the development agenda, there has been virtually no empirical work on the growth and development impacts of debt relief among low-income countries.[1] One exception is Easterly (2002), who suggests that the high debt burdens of HIPC countries reflect high discount rates, perhaps due to profligate governments, political instability, or interest group polarization. He shows that among HIPC countries debt relief is positively correlated with new borrowing, and that HIPC countries tended on average to have worse policy performance than non-HIPC countries. In contrast, HIPC countries were not likely to have experienced worse shocks than non-HIPC countries. He therefore argues that debt relief should be granted only to governments that display a fundamental shift in their development orientation. Along similar lines, Arslanalp and Henry (2004) argue that the main problem of the countries receiving debt relief is their lack of good institutions rather than excessive debt burdens. They suggest that the modest improvement in the growth performance observed in these countries over the past few years may be due to the reforms that were required as a precondition for the granting of debt relief, and not to the alleviation of a debt overhang problem. Both of these papers support the rather gloomy assessment of the HIPC initiative captured in the third quotation above: that debt is a symptom rather than a cause of deeper development difficulties. Although neither of these papers provides systematic empirical evidence on the impacts of debt relief as we do here, their

[1] Much more work has been done on the impacts of debt relief in middle-income countries, particularly following the debt crisis of the 1980s. See for example Arslanalp and Henry (2005) for a careful documentation of the positive impact of the Brady Plan. There is also a literature on the effects of overall debt on growth, which we discuss in more detail later on in the paper.

hypotheses are also very consistent with our failure to find evidence of impacts of debt relief on outcomes.[2]

The rest of this paper proceeds as follows. In the next section we provide a brief discussion of the various channels through which debt relief may affect public finances and the incentives for good policy and investment. In Section 3 of the paper we describe our new dataset on the present value of debt relief in low-income countries. In Section 4 we present some simple descriptive evidence regarding the relationship between debt relief and subsequent changes in outcomes. In Section 5 we address concerns about endogeneity in the descriptive evidence and also look at the effects of changes in total debt on outcomes. In Section 6 of the paper we focus on the more recent experience with debt relief under the HIPC initiative. Section 7 concludes. Three appendices provide details on our estimates of debt relief, a decomposition of changes in the present value of debt, and summary statistics on all of our variables.

2. Potential Effects of Debt Relief

Debt relief reduces the present value of the future debt service obligations of the recipient country. Advocates of debt relief suggest that this can benefit the recipient country through its effects on public finances in the recipient country, and through improved incentives for public policy and private activity created by the removal of debt overhang.

Effects on Public Finances

The most direct channel through which debt relief can affect development outcomes is through its effects on public spending. For a given path of future tax and non-tax revenues such as foreign aid, debt relief permits an expansion in public spending by easing the government's intertemporal budget constraint. In fact, a key feature of recent debt relief in the context of the enhanced HIPC initiative is that

[2] Most of the empirical evidence in Easterly (2002) consists of documenting trends in outcome variables for the group of HIPC countries as a whole. While this is useful descriptive analysis it does not provide a comparison with trends in countries not receiving debt relief nor does it relate changes in outcomes to amounts of debt relief received at the country level. Arsanalp and Henry (2004) document that HIPC countries have poor institutional quality and report some comparisons of growth rates in HIPC and non-HIPC countries.

countries receiving debt relief are required to use the resources saved by debt relief to finance increased development spending, particularly in areas like health and education.[3] To the extent that public spending promotes development, this creates a channel from debt relief to development outcomes. In this paper we do not attempt to provide evidence on the links between public spending and development outcomes, which is the subject of a large existing literature with mixed results. Rather we treat increases in spending as an intended intermediate objective of debt relief. We also note that debt relief need not result in an increase in public spending equal (in present value terms) to the reduction in future debt service. Governments might choose to leave non-debt service public spending unchanged and instead use the resources freed by debt relief to finance reductions in taxes and/or reductions in the rate of public debt accumulation.

The effect of debt relief on public spending also depends on what happens to foreign aid in response to debt relief. Much of the debt of the low-income countries that we study is owed to official creditors that also provide foreign aid to these countries. If foreign aid budgets are fixed, the donor's budget constraint implies that foreign aid must fall in response to debt relief.[4] If instead aid other than debt relief does not fall, then debt relief results in an increase in transfers from the creditor/donor country. However, as discussed above these additional resources need not result in greater public spending but could finance lower taxes. Given these potentially offsetting channels, the overall effect of debt relief on public spending, taxation, and foreign aid becomes an empirical question, which we address below.

Debt Overhang Effects

Debt relief may also affect the recipient country by strengthening the incentives for recipient countries to choose good policies. This might simply be due to requirements for better policy imposed by the creditor as a precondition to granting debt

[3] An important consequence of this is that debt relief with this conditionality on spending attached does not improve the fiscal sustainability position of the recipient country. Replacing one form of expenditure (debt service) with another (development spending) leaves the overall budget deficit and the rate of debt accumulation unchanged. See Burnside and Fanizza (2004) for a discussion of this point.

[4] See Bird and Milne (2003) for a discussion of additionality. Powell (2003) and Hepp (2004) provide empirical evidence that debt relief does not crowd out other forms of aid, which we discuss in more detail below.

relief.[5] More interestingly, improved policies may be an endogenous response on the part of the debtor. In his classic model of debt overhang, Krugman (1988) shows that high debt service obligations reduce the incentive of debtors to engage in policy reforms that raise revenues available for debt service, since part of the additional revenues accrue to the creditor. In the model, the resources available to the debtor to spend on debt service depend on effort -- interpreted as policy reforms that raise tax revenues -- and the realization of a shock. Before the shock is realized, the debtor chooses effort to maximize its expected revenues after debt service payments, net of the disutility of effort. The debtor thus chooses effort to equate the marginal disutility of effort with the marginal increase in revenues due to effort. The latter is simply the probability that the country is able to fully service its debt, since the marginal unit of revenue accrues to the debtor only if debt is fully serviced. Since the probability that debt is fully serviced increases as the amount of debt service due decreases, debt relief can raise the marginal benefit of effort and so result in the debtor choosing greater effort. This can in turn also raise the expected value of debt service collected by the creditor.

It is difficult to asses a priori how relevant such incentive effects of debt relief are in low-income countries. As we have noted most of these countries also receive very substantial aid flows from their creditors: the median country in our sample pays 3 percent of GDP in debt service but receives 12 percent of GDP in foreign aid every year. In the context of the Krugman model this suggests that the probability that debt is fully serviced should be equal to one, and so reductions in debt service cannot have positive incentive effects.[6] However, this argument overlooks the fact that much of aid is not provided as general budget support, but rather is earmarked to specific projects and/or to specific purchases from the donor country. If foreign aid in these countries is not very fungible, then the receipt of even very substantial aid inflows need not significantly expand the country's resources available to pay its debt service obligations.[7]

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[5] As we discuss further below, a pre-condition for debt relief from the Paris Club group of creditors has been that recipient countries are in compliance with the demands of an IMF program. Debt relief under the HIPC initiative requires establishment of a track record of good policies as judged by the World Bank.

[6] See Arsanalp and Henry (2004) for this argument.

[7] Feyzioglu, Swaroop and Zhu (1998) find in their core sample of 14 developing countries that aid increases public spending one-for-one, implying a total absence of fungibility for the total volume of aid. Also, the presumption that foreign aid is fungible should be strongest in countries where aid finances only a small portion of public spending, which increases the possibility that governments can re-direct resources away from aid-financed interventions. However, the countries that we study are highly aid-dependent. The median country in our sample has 53 percent of public spending financed by aid.

The fact that debt relief is conditioned on increases in development-related public spending provides a different reason to suppose that debt relief may not have strong incentive effects. In the context of the Krugman model, suppose that the creditor provides debt relief, but at the same time requires that debt relief be spent on development projects. In this case, debt relief does not raise the probability that debt can be serviced: although it lowers the debt service obligation, it also lowers the recipient government's resources available for debt service.[8] As a result, debt relief no longer raises the marginal benefit of policy effort, and so does not induce greater effort on the part of the recipient. These theoretical ambiguities regarding the incentive effects of debt relief suggest that the overall impact of debt relief on policy performance is an empirical question, which we consider below.

Debt relief which reduces the present value of public debt, can also in principle affect the incentives for private sector activity through various channels. Debt relief could reduce both expected future taxes as well as uncertainty about future taxes. If governments use the resources freed by debt relief to lower taxes, then debt relief can make investment more attractive. Debt relief can also reduce the uncertainty about future tax burdens, for example by removing uncertainty about whether accumulated arrears will ever need to be repaid or not. The uncertainty associated with future taxes need not refer only to the total amount of taxes to be collected. The distribution of the tax burden in societies with social and economic conflicts is subject to “negotiation” that increases uncertainty and lowers private investment.

3. Measuring Debt Relief

In this section of the paper we describe a new dataset of debt relief received by a sample of low-income countries. Our objective is to estimate the change in the present value of the stock of debt outstanding due to debt relief on an annual basis. The present value of debt at any point in time is defined as the present value of all future debt service payments associated with this debt. Debt relief itself can consist of outright debt forgiveness as well as concessional debt restructurings. Outright debt forgiveness reduces the present value of debt outstanding by eliminating the future debt service

[8] We are assuming here that the recipient government does not value this mandatory spending on development projects.

obligations associated with the debt forgiven. Concessional debt restructurings reduce the present value of debt by reducing and/or postponing future debt service obligations.

Given the importance of debt relief in current policy discussions, it is somewhat surprising that no comprehensive dataset measuring the present value of debt relief exists. As we discuss in more detail in Appendix A, data on debt relief provided by the World Bank and the OECD Development Assistance Committee in their major publications provides only imperfect information on the nominal amounts of debt forgiven. There are two main drawbacks to this published data. First, this data does not capture reductions in the present value of debt due to debt forgiveness, but only measures the face value of debt forgiven. Forgiving a given nominal amount of highly concessional debt with a low present value of future debt service obligations should have a smaller impact on the recipient country than forgiving the same nominal amount of non-concessional debt. This distinction is important because there is a great deal of variation across countries in the average concessionality of debt. In our sample of low-income countries, the median rate of concessionality of public- and publicly-guaranteed external debt (i.e. the ratio of the present value of debt to the face value of debt) is 70 percent, and the 5th and 95th percentiles range from 47 to 95 percent. The second drawback of these published sources is that they do not adequately capture changes in the present value of debt due to concessional reschedulings as opposed to outright debt forgiveness. This is because reported data focuses on the flow reduction in debt service in a given year due to past reschedulings, rather than measuring the reduction in the present value of future debt service due to a rescheduling. This issue is also important as we will see that concessional reschedulings have been significant for many countries and episodes.

We develop two alternative estimates of debt relief following methodologies that are described in detail in Appendix A. Our first method relies on debtor-reported data as presented in the World Bank's annual Global Development Finance publication. We use published and unpublished data from this source to compile estimates of the nominal value of debt relief and reschedulings. We combine this information with a new database on the present value of public external debt constructed by Dikhanov (2004) to arrive at estimates of changes in the present value of debt due to debt relief. Our second method relies on creditor-reported data on debt relief from four main sources:

bilateral creditors operating through the Paris Club; multilateral creditors operating through the HIPC initiative; a small donor-funded debt-buyback facility for low-income countries operated by the World Bank; and bilateral debt relief provided by Russia. For the first three of these sources we have fairly direct estimates of present value reductions due to debt relief, and for Russian debt relief we base our estimates on certain assumptions capturing the broad regularities of Russia's provision of debt relief.

We are aware of only one other early effort to compile comprehensive estimates of debt relief in low-income countries. Daseking and Powell (1999) provide estimates of the nominal amount of debt relief received by HIPC countries between 1988 and 1997. Our work differs from theirs in that we provide estimates of the present value of debt relief rather than the nominal amount. As detailed in Appendix A, we are also able to better capture the effects of concessional reschedulings. Our dataset also covers a larger sample of countries and for more years. We cover a sample of 62 low-income countries with per capita GDP at PPP in 1996 below $3100, and provide data through 2003. We choose this somewhat arbitrary income threshold because it corresponds to the per capita income of the richest of the 38 countries currently eligible for HIPC assistance, Guyana. Our sample therefore includes all the HIPC countries, as well as 24 other low-income countries that are not eligible for HIPC relief for a variety of reasons. Our full sample of countries is listed in Table 2 below.

Description of Debt Relief Measures

Table 1 and Table 2 report our two sets of estimates of the present value of debt relief for all of the low-income countries in our sample. In Table 1 we report the annual time series of the total amount of debt relief granted to our sample of 62 low-income countries between 1989 and 2003. Debt relief has been substantial, totalling between $34 billion and $42 billion dollars in 1988 present value terms, or between $109 billion and $134 billion in 2003 present value terms.[9] Figure 1 which plots the two estimates of total debt relief over time shows that annual flows of debt relief have increased over time, peaking around 2000 as large amounts of debt relief were granted to HIPC

[9] Throughout the paper we use a constant discount rate of 7.25 percent. This corresponds to the average value over our sample period of the OECD's Commercial Interest Reference Rate (CIRR) for long-maturity US-dollar denominated liabilities. CIRRs are routinely used by the donor community in estimating present values of debt and debt relief.

countries in this year, but declining somewhat afterwards. Importantly for our purposes, pre-HIPC debt relief prior to 2000 has also been quite substantial, as most of the rest of the paper will be devoted to studying the impact of this pre-HIPC debt relief on subsequent outcomes. Another reassuring feature of Figure 1 is that the two alternative estimates track each other fairly closely, although in some years the discrepancies between the two are substantial.

The composition of the different sources of debt relief has also varied over time, as shown in Table 1. In our Method 1 estimates drawing on debtor-reported data, debt relief due to rescheduling has remained fairly stable in the one- to two billion dollar range over our sample period. Outright debt forgiveness has varied considerably more, with sharp peaks in the mid-1990s and in 2000 when substantial HIPC debt relief was delivered. Our estimates for Method 2 drawing on creditor-reported data also show significant variation in the sources of debt relief. Debt relief provided by Russia has been substantial only in a few years when very large amounts were granted to a relatively small number of debtors. For example, virtually all of the $9 billion in debt relief granted by Russia in 2000 went to Vietnam. Similarly almost all of Russian debt relief in 1996 and 1999 went to Nicaragua and Ethiopia, respectively. Debt relief from multilateral creditors emerges only in 1998 as the first few countries in the HIPC process benefit from debt relief. The bulk of multilateral debt relief was provided in 2000 as more countries qualified for relief under the Extended HIPC initiative which featured broader eligibility and weaker conditionality than the original HIPC initiative.

In Table 2 we report the total present value of debt relief by country according to our two methods, expressed in dollar terms and as a share of the present value of the stock of public and publicly-guaranteed debt outstanding. We do this for three five-year subperiods that we will use in our empirical analysis below, 1989-93, 1994-98, and 1999-2003. We obtain the total present value of debt relief for each sub-period by discounting our annual estimates of debt relief back to 1988, 1993, and 1998 respectively. Many of the countries in our sample have benefitted from quite substantial debt relief relative to their stock of debt outstanding. Countries such as Bolivia, Guyana, Mozambique, Niger, and Senegal all were early beneficiaries of debt relief, with debt relief as a share of debt outstanding of more than 20 percent in the first period, according to our Method 1 estimates. These levels of debt relief are comparable to

those observed in the most recent subperiod when debt relief under the HIPC initiative started to increase sharply. The fact that many countries received substantial debt relief early in our sample period is important because it suggests that we will have sufficiently interesting variation in the data when we try to trace out the impact of debt relief over time.

Figure 2 shows that our two estimates of debt relief are fairly highly correlated across countries. To construct this figure we have discounted the annual flows of debt relief for the entire sample period 1989-2003 back to 1988, and expressed them as a fraction of the present value of total debt in 1988. The simple correlation between our two estimates of debt relief is quite high at 0.82. We note however that our Method 1 estimates of debt relief are systematically larger than our Method 2 estimates. The main reason for this is that our Method 2 estimates capture debt relief provided only by major creditor groups, whereas our Method 1 estimates are in principle comprehensive of all creditors as they rely on debtor-reported data covering all debt relief received. Beyond this systematic difference, there are also non-trivial discrepancies between our two methodologies for some countries, suggesting that a significant amount of measurement error remains in our estimates of debt relief. With this caveat in mind, we proceed to investigate the effects of debt relief on outcomes of interest in our sample of developing countries.

4. Descriptive Evidence on Debt Relief and Outcomes

In this section of the paper we use the estimates of debt relief constructed in the previous section to provide some simple descriptive evidence on the relationship between debt relief and changes in a variety of outcomes of interest suggested by the theoretical discussion in Section 2 of the paper. We first document the highly-reduced-form relationship between debt relief and growth, and find no discernible effect of debt relief on growth. We then examine some of the channels through which debt relief might operate. We ask whether debt relief affects the level and composition of government spending and revenues. We also ask whether there is any evidence that debt relief leads to better policies or improves incentives for investment. Finally we look for but do

not find much evidence in support of two plausible (at least to us) nonlinearities in the relationship between debt relief and outcomes.

Empirical Specification

Our objective here is to provide some simple descriptive empirical evidence regarding the relationships suggested by the theory. We do this by estimating a series of regressions of the form:

$$(1) \qquad X_{it} - X_{it-1} = \beta_0 + \beta_1 \cdot DR_{it-1} + \varepsilon_{it}$$

where X_{it} is an outcome variable of interest in country i in period t and DR_{it} is one of our measures of debt relief. We organize our data into three five-year periods, 1989-1993, 1994-1998, and 1999-2003, and compute the outcome variables as averages over these periods. We then regress the changes in the outcome variable on lagged debt relief, separately for the two cross-sections of changes observed in our panel of three quinquennial averages. For example, we regress the change in the five-year average of the outcome variable between 1994-1998 and 1989-1993 on the cumulative amount of debt relief received during 1989-1993.

Our empirical strategy here corresponds to the standard "difference-in-difference" estimator familiar from the microeconometric literature on impact evaluation. If we treat our debt relief variable as a binary indicator, we would be capturing the difference between the mean change in the outcome variable among countries receiving debt relief and the mean change among countries not receiving debt relief. In our reported results we measure debt relief as a continuous variable. We are therefore asking whether countries with large amounts of debt relief saw greater improvements in outcomes. The main advantage of this simple descriptive exercise is that it relates debt relief to subsequent changes in outcome variables. By focusing on changes in outcome variables we are removing any country-specific time-invariant determinants of outcome variables that might also correlated with debt relief. For example, countries that are poor or are highly-indebted might tend to grow more slowly. Since debt relief has been targeted to poor and highly-indebted countries, this would introduce a spurious negative correlation between debt relief and growth. By looking at changes in outcome variables

we remove this potential source of bias. There are however other potential econometric difficulties with Equation (1) which we address in the next section of the paper. For now however we present the results of estimating Equation (1) in the spirit of simple, but useful, data description.

Basic Results

Table 3 summarizes our first set of results. The rows of the table correspond to different outcome variables of interest. The two sets of columns refer to the two sets of quinquennial changes in our dataset. Within each set we report the estimated coefficient on debt relief using our two alternative estimates of debt relief. We measure the debt relief variable as the present value of debt relief over the five-year period normalized by initial exports. This is consistent with the common practice of measuring overall debt burdens relative to exports, and is exactly what we have reported in Table 2. We have also estimated the regressions in Table 3 with three other variants: debt relief as a share of initial GDP; debt relief as a share of total initial present value of debt outstanding; and an indicator variable taking the value of one if either of our two estimates of debt relief represents more than one percent of debt outstanding, and zero otherwise. Results using these alternative normalizations for debt relief are very similar to those we find for debt relief relative to exports, and are not reported to conserve on space.

The first row of Table 3 gives our results for per capita GDP growth. The correlations between debt relief and the subsequent change in five-year average growth are in most cases negative, although none of the estimated effects are statistically significant at conventional levels. Our Method 2 estimate of debt relief comes closest to being significant in the first period, falling just short of the 10 percent level. Figure 3 plots the relationship between debt relief and changes in growth in the first and second periods, using our second measure of debt relief. In our first sub-period, countries such as Niger and Senegal received substantial debt relief and saw increases in growth. But at the same time countries such as Bolivia, Zambia and Mauritania all also received substantial debt relief but experienced stagnant or declining growth. It is also clear from the top panel of Figure 3 that the marginal significance of debt relief for changes in

growth is largely due to one influential observation, Nicaragua. When we drop this one observation the t-statistic on debt relief falls to 1.04.

We next turn to a set of variables intended to capture the effects of debt relief on public finance. As we have discussed above, one of the purposes of debt relief has been to make resources available to governments to spend on development objectives. We therefore begin by asking what is the relationship between debt relief and government spending. We obtain data on the latter from Gupta, Clements, and Tiongson (1998), who compile data on total central government expenditures and its functional breakdown from IMF country reports.[10] Their total government spending variable conventionally includes interest expenditures, but not amortization of principal. To the extent that debt relief results in a reduction in interest payments, this could by construction create a spurious negative relationship between changes in measured government spending and debt relief. We therefore subtract from total government spending interest payments on public and publicly-guaranteed debt as reported in the GDF.

Our results for government spending as a share of GDP are in the second row of Table 3. Here we fail to find any significant relationship between the change in government spending and debt relief. The coefficients vary in sign from one specification to the next, and the t-statistics are all very small. The top panel of Figure 4 plots one representative correlation between our Method 2 of debt relief and the change in government spending as a share of GDP in the first period. It is striking that only a handful of the countries with positive debt relief in the initial period saw increases in government spending, for example Ethiopia, Bolivia, and Nicaragua. Most of the other countries that received debt relief saw declines, rather than increases, in government spending.

Our data on government spending also allows us to separately identify government spending on health and education. Expenditures in these areas are often thought of as having disproportionately large development benefits, and countries

[10] We are grateful to Benedict Clements and Shamsuddin Tarek for kindly making an updated version of this dataset available to us. One of the main advantages of this dataset is that it has much greater country coverage than standard published sources such as the IMF's Government Finance Statistics Yearbook. Nevertheless, our country coverage for this variable is substantially less than for GDP growth, with only 35 and 44 observations in the two sub-periods, as compared with 46 and 54 for growth.

benefiting from HIPC debt relief are expected to increase spending in these areas. We therefore also ask whether spending on health and education as a share of total spending increases faster in countries that receive more debt relief. The results are reported in the third row of Table 3. In the first period, we find no significant effect whatsoever. However, in the second period we find quite strong evidence that spending on health and education increases as a share of total spending. An increase in debt relief relative to exports of 0.5 (which is roughly one standard deviation of this variable) would raise health and education spending as a share of total spending by about two percentage points. The bottom panel of Figure 4 shows the simple correlation between the change in health and education spending and initial debt relief in the second period, using our first measure of debt relief. Two countries, Mozambique and Yemen, are quite influential observations, with large amounts of debt relief between 1994 and 1998, and with a very sharp jumps of 14 and 9 percentage points in the share of health and education spending, respectively. Dropping these two observations eliminates the statistical significance of the correlation between debt relief and the share of health and education spending. It is also interesting to note that many of the countries showing large increases in health and education spending shares are countries that received substantial debt relief under the HIPC initiative after 2000, for example Cameroon, Tanzania, and Guyana. We are therefore reluctant to credit debt relief during 1994-98 as being responsible for the subsequent increase in health and education spending in these countries, as it may instead reflect conditionalities associated with HIPC debt relief that came later.

As we discussed above, one possible reason why government spending might not increase in response to debt relief is that governments instead use the fiscal space created by debt relief to lower taxes. We therefore next ask whether there is any evidence that tax revenues collected as a share of GDP decline in response to debt relief. Our tax variable measures total tax revenue, and is taken from Baumsgaard and Keen (2005), who have constructed a database on tax revenues disaggregated by type, drawing on IMF country reports.[11] As with the expenditure data described earlier, this dataset offers substantially greater country coverage than standard published sources. We report results for this variable in the fourth row of Table 3. Our point estimates are in all cases positive, suggesting that if anything debt relief leads to subsequent increases

[11] We are grateful to Michael Keen and Thomas Baunsgaard for kindly making their dataset available to us.

in taxes rather than reductions in taxes, and in one case this finding is weakly significant at the 10 percent level. This case is shown in the top panel of Figure 5 which plots our second measure of debt relief in the first period and the subsequent change in tax revenues relative to GDP.

Another possible reason why government spending does not appear to change much in response to debt relief is that donors providing debt relief to a country might reduce other forms of aid to the same country. We investigate this in the fifth row of Table 3. Our dependent variable is net official development assistance (ODA) as a share of GDP, as reported by the OECD.[12] In all cases we find that countries receiving more debt relief are more likely to see a subsequent decline in aid inflows. However, this finding is significant at the 10 percent level only for the second measure of debt relief, in the first period. The bottom panel of Figure 5 shows the simple scatter plot between debt relief and subsequent changes in aid relative to GDP. It is clear from this figure that one country, Nicaragua, is quite influential, with large debt relief in the first period, and a sharp decline in aid relative to GDP. Excluding this one observation eliminates even the marginal significance of this result. Our results for this variable are consistent with other work that has looked at the extent to which debt relief is "additional" in the sense of not crowding out other forms of aid. Powell (2003) finds no significant correlation between aid and debt relief, although using a different measure of debt relief and different assumptions regarding the timing of effects. Hepp (2004) reaches the same conclusions, with HIPC countries receiving higher net transfers than non-HIPC countries.

We next consider the possibility that debt relief mitigates debt overhang, and thus should be followed by improvements in policy performance and increases in investment. We use two measures of policy performance. We first use the World Bank's Country Policy and Institutional Assessment ratings that have been prepared by World Bank country economists on an annual basis since 1977. Countries are rated according to a

[12] Data on net ODA for the most part do not reflect aid provided as debt relief. This is because OECD reporting standards require donor countries to report the value of debts cancelled in a given year as gross ODA inflows to the recipient, but then an offsetting cancelling entry is made in outflows so that net ODA does not capture debt forgiveness. Debt rescheduling is reported in a way similar to the GDF. That is, the reduction in debt service payments in a given year due to debt relief is supposed to be reported as an inflow to the recipient with no cancelling outflow. As a result net ODA does capture debt reschedulings. Unfortunately however the OECD does not report these rescheduling flows by recipient country so we are unable to adjust the ODA data for this part of debt relief. See http://www.oecd.org/dataoecd/36/32/31723929.htm#TableDAC2a for details on OECD aid reporting requirements.

range of criteria capturing different dimensions of the institutional and policy environment and a summary score on a scale from one to six is produced.[13] We find a fairly strong positive relationship between debt relief 1989-1993 and the subsequent quinquennial change in CPIA scores. However, this positive correlation is not present in the second period. Upon closer inspection however the finding that policy improves following debt relief does not appear to be so strong. The top panel of Figure 6 plots the simple correlation between our second measure of debt relief and the subsequent change in policy. Three observations are quite influential. The two in the bottom-left corner of the graph are Rwanda and Burundi, which both received no debt relief in the first period, and their CPIA scores fell sharply as these countries fell into civil conflict in the mid-1990s. In contrast Nicaragua in the top-right corner received very substantial debt relief and saw sharp improvements in its CPIA scores as that country emerged from its civil conflict of the 1980s. Excluding these observations sharply reduces the magnitude and significance of the effect of debt relief on policy, although it just remains significant at the five percent level. We have also looked at the link between debt relief and policy using an publicly-available assessment of institutional quality, the widely-used ICRG composite index. Here we find a positive correlation between debt relief and subsequent changes in the ICRG index, that are for the most part not statistically significant.

As another measure of policy performance, we consider the share of trade taxes in total taxes, again taken from Baumsgaard and Keen (2005). Since trade taxes are typically fairly easy to collect, a decline in this ratio can be used as a rough proxy for a country's success in expanding its tax base.[14] These results are reported in the second-last row of Table 3. In the first period we do find a negative relationship and marginally significant relationship at the 10 percent level, with the share of trade taxes falling faster on average in countries receiving more debt relief. The bottom panel of Figure 6 shows this relationship, for the second measure of debt relief in the first period. While the negative correlation is apparent, it is also clear that just two countries, Benin and Uganda, are quite influential, with both receiving substantial debt relief and both seeing large declines in trade taxes as a share of total taxes. Dropping these two observations again eliminates even the marginal significance of this finding.

[13] For details on the CPIA assessments visit http://siteresources.worldbank.org/IDA/Resources/CPIA2004questionnaire.pdf.

[14] There clearly are several difficulties with such a proxy. Notably, in commodity-dependent countries this share tends to depend heavily on fluctuations in commodity prices.

Finally, we examine whether there is any evidence that private sector activity expands in response to debt relief. In our theoretical discussion above we noted that debt relief could improve incentives for investment, particularly if countries used the fiscal space created by debt relief to lower taxes. Ideally we would want to look at how private investment responds to debt relief, but reliable data on private investment for our sample of low-income countries does not exist. We therefore look at total investment as a share of GDP as an imperfect proxy. The results can be found in the last row of Table 3, where we fail to find any significant impact of debt relief on investment. This is consistent with our earlier finding that debt relief does not seem to significantly impact tax collection, and if anything the sign of the effect on taxes is positive.

Are nonlinearities important?

Thus far we have provided descriptive evidence regarding linear relationships between debt relief and changes in outcomes. A possible reason for our failure to find much in the way of a significant effect is that the true effects are nonlinear. One argument is that debt relief will only have positive effects on outcomes in countries with good policies and institutions. This argument is a natural one to consider since debt relief is in some ways similar to foreign aid, and there is some evidence that the effects of foreign aid on growth depend on the quality of policies and institutions.[15] Another possibility is that debt relief matters for outcomes only if the debt is actually being serviced. If in contrast the debt is not being serviced but is simply accumulating arrears, then forgiving this debt will not provide additional resources to the debtor country but simply amounts to cleaning up the books of the creditor.

In order to capture these possible nonlinearities, we consider two sets of regressions following those in Table 3, but adding interaction terms. To capture the differential effect of debt relief in countries with good policy, we introduce the interaction of debt relief with a dummy variable that identifies countries with a CPIA score greater than 3.4, corresponding to the median of our sample. To capture the differential effect of

[15] See for example Burnside and Dollar (2000, 2004). This evidence has been criticized by Easterly, Levine and Roodman (2004) and Rajan and Subramanian (2005). The idea that aid is more effective in a good policy environment remains one that is a priori sensible, and is supported by evidence on the success of individual projects (see for example Isham, Kaufmann and Pritchett (1997) and Isham and Kaufmann (1999)).

debt relief in countries where debt is being serviced, we construct a similar interaction that identifies countries where arrears as a share of debt outstanding fall below the median of our sample, which is 5 percent. The previous discussion suggests that these two interaction terms should enter positively.

We report the results of including these interactions in Table 4. The two panels of Table 4 correspond to these two sets of interactions. Within each panel we consider the same list of outcome variables as before. We report in the columns the coefficient estimates on the linear term in debt relief, and the interaction term, for the two measures of debt relief and the two sets of changes over time. The overall impression from this table is that these a priori plausible interaction terms are not very significant. In only two cases we do find a statistically significant interactive effect. For investment, the interaction with arrears in the second period is significantly positive, consistent with our priors that the impact of debt relief is greater in countries where initial arrears were low. For policy, we find a counterintuitive negative interaction, suggesting that debt relief is more likely to lead to declines in policies when policy performance is initially good. We do not want to assign too much importance to these two cases however as they are just two of 56 possible interactions reported in the table, and we would expect one or two to be significant at the 5 percent level purely by chance. In most cases we do however find that the few cases where direct effects of debt relief were significant in Table 3 continue to be significant here with the addition of interaction terms. As we have discussed above, though, many of these significant direct effects are not particularly robust and are often driven by a few very influential observations. In summary, we do not find much evidence that these interaction effects are important.

Overall, the simple descriptive evidence in this section provides very little support for the idea that debt relief matters for outcomes through the channels we have discussed above. We find very little in the way of significant correlations between debt relief and subsequent changes in growth, investment, total government spending, and tax collection. We do find some modest evidence that debt relief improves policy performance, but only in the first period, and some weak evidence that debt relief is followed by increases in the share of public spending going to health and education. Often even these modestly significant results are driven by a few influential

observations. Taken together, our first look at the data does not provide much support for the idea that debt relief has been effective in achieving its stated objectives.

5. Endogeneity and the Role of Overall Indebtedness

In this section of the paper we generalize the empirical specification of the previous section in order to address two shortcomings. The first one is econometric. We show that if debt relief is correlated with contemporaneous shocks to the levels of our outcome variables the simple difference-in-difference estimator used above will be biased. The second problem is that we have so far ignored changes in debt due to factors other than debt relief, and these might also matter for changes in outcomes.

Empirical Specification

We begin by noting that the theoretical discussion in Section 2 suggests that there is a relationship between levels of outcome variables and levels of indebtedness. For example, high debt levels relative to exports imply high debt service obligations, which in turn constrain government spending as a share of GDP and/or weaken incentives for good policy. Since the theory does not provide us with very precise empirical specifications to test, we instead attempt to crudely capture this idea with the following parsimonious linear specification for the levels of our outcome variables:

$$X_{it} = \rho \cdot X_{it-1} + \beta \cdot D_{it-1} + \lambda_t + \mu_i + \varepsilon_{it} \tag{2}$$

where as before X_{it} is the outcome variable of interest in country i in period t. D_{it-1} is the stock of debt at the end of the previous period; λ_t and μ_i are unobserved period- and country-specific effects; and ε_{it} is an idiosyncratic shock. Since many of our outcome variables exhibit significant persistence over time in levels, we have also included an autoregressive term in Equation (2). This means that we can capture the observed persistence in outcome variables with some combination of country fixed effects as well as an autoregressive component. As before we organize our data into three five-year periods, 1989-1993, 1994-1998, and 1999-2003, and compute the outcome variables as averages over these periods. We measure the stock of debt as the present value of

public and publicly-guaranteed debt relative to exports, as of the end of the previous period, i.e. in end-1988, 1993, and 1998.

First-differencing this equation in order to eliminate unobserved country-specific effects that might be correlated with debt results in:

$$(3) \qquad X_{it} - X_{it-1} = \rho \cdot (X_{it-1} - X_{it-2}) + \beta \cdot (D_{it-1} - D_{it-2}) + \lambda_t - \lambda_{t-1} + \varepsilon_{it} - \varepsilon_{it-1}$$

where we now have the change in the outcome variable regressed on its lagged change, and the lagged change in the present value of debt as a share of exports. We show in Appendix B that the change in this debt ratio can be decomposed into two terms, i.e. $D_{it-1} - D_{it-2} = DR_{it-1} + Z_{it-1}$. The first, DR_{it-1} is the contribution of debt relief to changes in the present value of debt as a share of exports during period t-1. This is simply the present value of all debt relief received during period t-1 expressed as a share of initial exports, and is the same measure of debt relief that we used in the previous section. The second, Z_{it-1}, captures all other sources of changes in the present value of debt relative to exports during period t-1, including net new borrowing, fluctuations in the denominator of the debt ratio, and cross-currency valuation changes.

We next substitute this decomposition of the change in total debt into Equation (3), and we allow the slope coefficients on the two terms to differ in order to capture the possibility that debt reductions due to debt relief have a different impact on outcomes than debt reductions due to other factors, possibly because of policy conditionality associated with debt relief. This results in:

$$(4) \qquad X_{it} - X_{it-1} = \rho \cdot (X_{it-1} - X_{it-2}) + \beta_1 \cdot DR_{it-1} + \beta_2 \cdot Z_{it-1} + \lambda_t - \lambda_{t-1} + \varepsilon_{it} - \varepsilon_{it-1}$$

Equation (4) is a natural generalization of the simple difference-in-difference specification in Equation (1) that we used in the previous section. In particular, in the previous section we were implicitly assuming that there was no autoregressive component in outcomes, i.e. ρ=0, and we also treated changes in debt burdens due to factors other than debt relief, Z_{it-1}, as part of the error term. These simplifying

assumptions delivered Equation (1), which we estimated for our two cross-sections of changes.

Equation (4) illustrates two potential econometric difficulties with the simple descriptive evidence in the previous section. Note first that the error term in Equation (4) includes the shock to the lagged outcome variable, ε_{it-1}. It seems plausible that debt relief during period t-1 might be targetted towards realizations of this shock. Concretely, suppose that the outcome variable of interest is policy, and suppose that during our first five-year period 1989-1993 a country exhibits better-than-expected policy, i.e. there is a positive realization of ε_{it-1}. Thanks to this strong policy performance, the country might be rewarded with more debt relief. This creates a positive correlation between debt relief during period t-1 and shocks to policy. This in turn creates a negative correlation between debt relief and the error term in Equation (4), since higher realizations of policy in the first period make it more likely that the change in policy is smaller between 1989-1993 and the next five year period 1994-1998. This suggests that our simple OLS estimates of Equation (1) will be biased downwards. Of course, the bias could go the other way for other outcome variables. For example, countries with negative shocks to growth during period t-1 might receive more debt relief if donors provide debt relief as a form of aid. In this case our OLS estimates of Equation (1) would be biased upwards.

The second problem is closely related to the first. If outcome variables have a strong autoregressive component, i.e. $\rho>0$, then changes in outcomes will be positively correlated with lagged changes in outcomes. For example, the change in policy between 1989-1993 and the next five year period 1994-1998 will be correlated with the change in policy between 1984-1988 and 1989-1993. If debt relief during 1989-1993 is correlated with this lagged change in policy, then we have a standard omitted variable bias problem. The direction of this bias can again go either way. If countries that showed an improvement in policy between 1984-1988 and 1989-1993 got more debt relief during 1989-1993, then our the simple OLS estimate of the impact of debt relief on the subsequent change in policy will be biased up. If in contrast growth is positively correlated over time and countries with declines in average growth performance between 1984-1988 and 1989-1993 received more debt relief, then we would have a downward bias in our OLS estimates.

We address these two econometric difficulties by adding the lagged change in the outcome variable to our simple descriptive regression to address the omitted variable problem, and by using internal instruments to deal with endogeneity. Note that we need instruments for both debt relief (for the reasons given above) as well as for the lagged change in the outcome variable. This is because the latter is by construction correlated with the error term in Equation (4) since X_{it-1} depends on ε_{it-1}. Our identifying assumptions are as follows: (1) the stock of debt at the end of period t-1 is uncorrelated with subsequent shocks to outcomes, i.e. $E[D_{it-1} \cdot \varepsilon_{it+s}]=0$ for s>1,2,3...; and (2) the shocks to outcomes are uncorrelated over time, i.e. $E[\varepsilon_{it} \cdot \varepsilon_{it+s}]=0$ for s=±1,±2,±3.... These identifying assumptions imply that the necessary exclusion restrictions will be satisfied if we use X_{it-2} as an instrument for X_{it-1} -X_{it-2}, and D_{it-2} as an instrument for DR_{it-1}. We think these identifying assumptions are not too implausible. Barring very strong forward-looking behavior on the part of governments it seems reasonable to assume that the stock of public external debt as a share of exports at a given point in time is not affected by subsequent shocks to outcomes. The assumption of no serial correlation in the error terms also seems reasonable given that we have captured the serial dependence in the outcome variables by both country fixed effects as well as an autoregressive term. There is also reason to believe that these instruments are relevant in the sense of having predictive power for the endogenous variables. If outcomes have an autoregressive component with $\rho<1$ then the twice-lagged level of the outcome variable X_{it-2} will be correlated with the subsequent change in outcomes, X_{it-1} -X_{it-2}. And it also seems reasonable to expect that countries with high debt burdens are more likely to receive debt relief.

Equations (3) and (4) also illustrate a more conceptual problem with the simple descriptive evidence of the previous section. In this simple descriptive evidence we have not allowed for the possibility that changes in debt due to factors other than debt relief might matter for changes in outcomes. While this is consistent with the focus on this paper on the effects of debt relief, at least some of the theoretical discussion in Section 2 suggests that changes in debt from other sources might matter for outcomes as well. For example, in the Krugman model of debt overhang, any shock which reduces the stock of debt and the associated debt service burden will create incentives for good policy by raising the probability that debt will be fully serviced. For this reason

we also provide estimates of Equation (3) where we explain changes in outcomes with changes in overall debt, rather than just debt relief. In Appendix B we also show that the changes in the present value of debt relative to exports over the three five-year periods in our dataset are rather weakly correlated with debt relief. This raises the possibility that our failure to find significant effects of debt relief has simply been due to the fact that debt relief is a poor proxy for overall changes in debt burdens.

Results

We begin by estimating Equation (4) in which we explain changes in outcome variables with debt relief alone, i.e. we impose the restriction that β_2=0. The results are reported in Table 5, where we have also imposed the restriction ρ=0. The only difference therefore between these results and the ones in Table 3 are that we are using the twice-lagged level of the present value of debt relative to exports as an instrument for lagged debt relief. Because not all of our countries have data on this instrument in 1983 and 1988, our sample of observations is somewhat smaller than before. The rows of the table correspond to the same set of outcome variables. At the bottom of the table we report the first-stage regressions for the two sets of changes and the two measures of debt relief. These first-stage regressions perform reasonably well in three of the four cases, with F-statistics ranging from 7.3 to 8.9. Not surprisingly, prior debt stocks are significantly positively correlated with subsequent debt relief, except in the case of our Method 1 debt relief in the second period.[16] Our instrumental variables estimates of the impact of debt relief turn up only two marginally significant coefficients out of the 28 specifications in Table 5. The marginally significant negative impact of debt relief on aid that we also saw in Table 3 remains after we instrument, as does the negative estimated impact of debt relief on the share of trade taxes in total taxes. Overall, the evidence in this table is once again not very supportive of the idea that debt relief has significant effects on outcomes.

Although the estimated coefficients are not very significant, it is nevertheless interesting to compare the magnitude of the uninstrumented and instrumented coefficient

[16] The first-stage regressions are of course slightly different for each of the outcome variables as the sample of countries differs depending on data availability. We report only the first-stage regressions for the largest possible sample to conserve space.

estimates, since this provides information about the direction of the underlying endogeneity bias. In the case of government spending, tax revenues, and investment, the IV estimates are consistently bigger than the OLS estimates. This pattern suggests that donors provide debt relief to countries with experiencing large positive shocks to these variables, so that the OLS estimates are biased downwards. In contrast, the IV estimates are consistently smaller than the OLS estimates for aid as a share of GDP. This suggests that countries with negative shocks to aid are in part compensated by receiving more debt relief.

We have also tried to estimate Equation (4) relaxing the restriction that $\rho=0$. This effort runs into two practical difficulties. The first is that we need data on our outcome variables between 1983 and 1988 in order to construct the lagged change in outcomes. For several of our outcome variables, notably the taxation and spending variables, data is quite sparse during the mid-1980s and this drastically reduces our sample size. The second problem we encounter is one of weak instruments. In the regressions with $\rho\neq0$, we have two endogenous variables (debt relief and the lagged change in the outcome variable) and two candidate instruments (twice-lagged debt and twice-lagged levels of the outcome variables). While we have seen that twice-lagged debt predicts debt relief fairly well, we often find that twice-lagged levels of outcomes are poor predictors of subsequent changes in outcomes. The combination of weak instruments and a very small sample size means that our IV estimator is virtually uninformative. We often find very large standard errors as well as wildly implausible point estimates of the coefficients on the lagged change in the outcome variable. For this reason we do not report these results here. We do note however that in virtually all cases we do not reject the null hypothesis that $\rho=0$, albeit with very low power. This would account for the poor performance of our instrument for lagged changes in outcomes, and it also justifies focusing on the results without the lagged change in outcomes that are reported in Table 5.

We finally turn to the possibility that changes in overall debt burdens rather than just debt relief are what matter for changes in outcomes. Our estimates of Equation (3) are reported in Table 6. We report both simple OLS estimates in order to facilitate comparison with the descriptive evidence in Table 3, as well as IV estimates. For the latter we use the twice-lagged level of debt as an instrument for its lagged change. For

the same reasons given above, we are forced to impose the restriction that ρ=0, as we encounter the same severe weak instruments problems when we try to include, and instrument for, the lagged change in the outcome variable. The OLS estimates show a few significant correlations between changes in debt and changes in subsequent outcomes. There is a significant positive correlation between changes in debt and subsequent changes in growth, and there is a significant negative correlation with changes in government spending in the first subperiod. The latter result is consistent with the idea that high debt service crowds out government spending. We also see a positive and significant correlation between changes in debt and changes in trade taxes as a share of total taxes. This can be interpreted as support for the idea that higher debt burdens lead to worse policy. None of these findings remain significant when we instrument for changes in debt to control for possible correlations between changes in debt and shocks to outcomes. We also note that this loss of significance cannot simply be ascribed to the lower precision of the IV estimator. Although our sample size is quite small, our first-stage regressions deliver fairly respectable F-statistics of 6.3 and 8.3.[17]

Our failure to find a significant negative effect of changes in debt on growth is of particular interest in light of several recent papers that have empirically investigated the impact of debt on growth using cross-country regressions.[18] Three recent papers argue that there are nonlinearities in the relationship between debt and growth, although they disagree as to the shape of the nonlinearity. Patillo, Poirson and Ricci (2002) using a panel of 93 developing countries find that the marginal impact of debt on growth is positive for approximately the least-indebted 10 percent of their sample, but is negative for the remaining 90 percent of the sample.[19] Imbs and Ranciere (2005) use more flexible kernel regressions and find that the relationship between debt and growth is negative only in the most highly-indebted part of their sample. Finally Cordella, Ricci and Ruiz-Arranz (2005) argue for a zone of "debt irrelevance" in which the impact of debt on growth is nil when debt burdens are very high but negative over an intermediate range. All of these papers use estimators that rely on the within-country variation in debt

[17] For the regressions in Table 6 we have restricted the sample to exclude a handful of observations where the absolute change in the present value of debt as a share of exports was greater than 200%. Including these observations resulted in very poor explanatory power for our first-stage regressions.

[18] A few papers have also looked at the impact of debt on investment. Cohen (1993) finds that are debt payments, and not the level of debt, what have an impact in the investment rate for 81 developing countries over the period 1965-1987. Hansen (2004) also finds a negative effect of debt services on investment.

[19] The contribution of Clements, Bhattacharya and Nguyen (2004) is closely related to this paper, using similar data and methodology. They also document a negative impact of debt service on public investment.

and growth rates, and therefore are comparable to our results which examine the partial correlation between changes in debt and subsequent changes in growth.

We do not find the results of these three papers fully persuasive however. One difficulty with the first and third papers has to do with timing: they examine the contemporaneous relationship between debt and growth over three-year periods. This raises the possibility that the negative correlation between debt and growth is driven by shocks to growth rather than debt. Imbs and Ranciere (2005) show that when one switches to the relationship between initial debt and subsequent growth, the partial correlation between the two is mostly insignificant. Imbs and Ranciere (2005) also show that the estimated effect of debt on growth becomes insignificant when proxies for the institutional environment are included in the regression, consistent with the view that countries with weak institutions are more likely to overaccumulate debt and also experience slower growth.[20]

Based on our results as well as our interpretation of the existing literature, we do not think there is a great deal of evidence that total debt stocks matter for growth, at least in the low-income countries that have been beneficiaries of substantial debt relief over the past 15 years that we have studied. While debt relief has been only imperfectly correlated with changes in overall debt, this is unlikely to be the reason why debt relief has had so little development impact. Rather, our results suggest that changes in debt from any source are not significantly correlated with changes in growth or improvements in our other outcome variables.

6. Has Recent Debt Relief Been More Effective?

One drawback of the empirical strategy that we have pursued in the previous two sections is that we have not been able to study the impact of debt relief during the last five-year period for which we have data, 1999-2003. This is because we have been relating debt relief to subsequent changes in outcomes over the following five-year

[20] This latter finding is closely related to Presbitero (2005) who argues that existing evidence on the nonlinear relationship between debt and growth is not robust. He instead finds that high debt levels contribute to macroeconomic instability, suggesting that debt relief can improve growth by reducing instability.

period. However, we do not yet have available five years of data following 2003 so we cannot implement this strategy for debt relief in our last five-year period. This is unfortunate because both of our measures of debt relief show substantial increases in the volume of debt relief during the most recent period, mostly reflecting the delivery of substantial debt relief under the HIPC initiative.

In this part of the paper we look at the relationship between debt relief and changes in outcomes *within* our last five-year period 1999-2003 in order to remedy this gap. To compute average changes in outcomes within the last period, we regress each outcome variable on a time trend and retrieve the slope coefficient for each country. We then estimate a series of regressions of these trend changes in the outcome variable on our cumulative measure of debt relief during the period. We recognize that by doing so we are subject to much greater potential endogeneity biases due to the targetting of debt relief to changes in outcomes, and we therefore interpret these results with greater caution.

We report the results for this exercise in Table 7. The rows correspond to the same set of outcome variables we have considered in previous tables.[21] The results are broadly consistent with our previous findings, in the sense that we find very little evidence of significantly higher rates of improvement in outcomes among countries receiving greater debt relief. There are a few noteworthy exceptions to this. In contrast with our results for earlier periods, we do not see much evidence of sharp upwards trends in health and education expenditures as a share of total spending within the last period. This is somewhat troubling as much of the debt relief we consider during this period was delivered under the HIPC initiative, where one of the stated objectives was to use the resources saved via debt relief to finance higher spending in these areas. We also see very little evidence of policy improvements within the last period. This may however be because countries receiving HIPC relief were required to show improvements in policy prior to receiving debt relief. One piece of modestly good news in these results is that the correlation between debt relief and increases in government

[21] The regressions reported here exclude two very influential observations. Ethiopia has by far the largest debt relief in this sub-period, and Democratic Republic of Congo registers extremely large increases in several of the outcome variables largely reflecting their very low initial values during its civil war in the late 1990s.

spending is at least positive, although it still falls short of significance even at the 10 percent level.

7. Conclusions

In this paper we have used a new database measuring the present value of debt relief to low-income countries to empirically investigate the impacts of debt relief. The evidence we have uncovered so far is rather disappointing. We find very little evidence that debt relief has had any impact on the level or composition of public spending. Nor do we find that debt relief has led to improvements in policy or increases in investment rates. It is therefore also not very surprising that the growth impacts of debt relief do not appear to be significantly different from zero. These negative findings should temper enthusiasm for further debt relief efforts such as those recently proposed by the G-8 countries.

Nevertheless, we recognize that our work so far has several limitations. One has to do with the quality of our data on debt relief. Short of an exhaustive loan-by-loan examination of individual debt relief agreements it is very difficult to come up with accurate measures of the associated reduction in future debt service obligations. This forces us to rely on a number of crude assumptions, which we apply to aggregate data in order to come up with approximations of the present value reduction due to debt relief. Simple measurement error in our right-hand-side variable could contribute to our failure to find significant impacts of debt relief on outcomes in either direction.

A further difficulty has to do with the timing of debt relief and its impacts on outcomes. We have made an effort to measure the reduction in the present value of future debt service obligations due to debt relief in a given country and year. This is a useful way to summarize the effects of both debt forgiveness and concessional reschedulings. Moreover, if the government benefitting from debt relief were not credit constrained, then the timing of the actual subsequent reductions in debt service would not be relevant -- only total reduction in the present value of debt is what matters. But in reality virtually all of the countries receiving debt relief in our sample are likely to be highly credit-constrained. This means that governments will only be able to increase

spending in response to debt relief in years where debt service reductions actually occur. This in turn suggests that changes in spending, and possibly other outcomes, are more likely to be associated with the realized future flow reductions in debt service, rather than with the initial reduction in the present value of debt due to debt relief. Unfortunately, it is very difficult to accurately measure the time path of future reductions in debt service due to debt relief short of delving into loan-by-loan data on debt relief, and this is something that we leave for future research.

Despite these measurement difficulties, we nevertheless think that it is unlikely that even with better data we will be able find very large development impacts of debt relief. While the present value of debt relief granted to low-income countries over the past decade appears to be considerable, it is actually quite small when compared with foreign aid, or with the value of total tax revenues that it is intended to augment. In our sample of 62 low-income countries, the median present value of all debt relief between 1989 and 2003 was between 9 and 12 percent of GDP in 1988, depending on which of our two methods we use. In contrast, the present value of all net aid receipts of these countries over the same period was more than an order of magnitude larger, at 126 percent of GDP, and the present value of tax revenues was greater still at 142 percent of GDP. This simple calculation shows that past debt relief has actually been quite small relative to other forms of aid, and even smaller when compared with domestic tax revenues in recipient countries. Looking forward, even if all of these countries had their entire stock of public and publicly-guaranteed external debt as of end-2003 forgiven, the present value of this debt reduction would be just 49 percent of GDP for the median country in our sample. In contrast, the present value of future aid receipts and tax revenues would be much larger at 134 and 190 percent of GDP for the median country, conservatively assuming that aid receipts and tax revenues remain constant at their historical average values.

As one of our opening quotations noted, debt relief can provide "much-needed respite from high debt service", and as such constitutes a resource transfer to recipient countries, albeit modest in present value terms. We therefore do not claim based on our results that debt relief provides no benefits whatsoever to recipient countries. Rather our findings support the conclusion that, based on past experience, further debt relief is unlikely to have particularly large development impacts. It therefore seems important

that the recent emphasis on debt relief among donors does not divert attention or resources from other forms of aid that may have larger development impacts.

Appendix A: Estimates of the Present Value of Debt Relief

Method 1: Estimates Based on Global Development Finance Data

Our first set of estimates is based on published and unpublished data from the World Bank's Global Development Finance report (GDF). The GDF has reported data on debt forgiveness and debt restructurings since 1989. The aggregate data reported in the GDF is drawn from debtor-reported data on individual loans recorded in the Debtor Reporting System (DRS) database, a very large loan-by-loan dataset maintained by the World Bank containing in principle every external debt obligation of all developing countries. Changes in the present value of debt due to debt relief could be accurately constructed from the loan-by-loan information recorded in the DRS database. This however is a very labour-intensive task requiring extensive familiarity with the DRS database, and it is not something that we have attempted here. Rather, we base our estimates on aggregate data from the DRS, together with certain assumptions detailed below.

We begin by taking the GDF item "debt forgiveness or reduction", which captures the nominal value of debt forgiven (including interest and principal arrears forgiven), or otherwise reduced through deals such as debt buy-backs. In the case of low-income countries these buybacks are typically aid-financed and hence constitute a transfer from donor countries just as is the case for debt forgiveness.[22] Note that debt forgiveness includes only forgiveness of principal, and so we add to this the GDF item "interest forgiven". We next assume that the average rate of concessionality (i.e. the ratio of the present value to the face value of debt) of the debt forgiven or reduced is the same as the average rate of concessionality for the total stock of debt outstanding of the country. The change in the present value of debt due to debt forgiveness or debt reduction is then simply the product of the nominal amount reported in the GDF and the average concessionality rate. We obtain the latter from a new database on the present value of external public and publicly-guaranteed debt constructed by Dikhanov (2004), that we use throughout this paper.[23]

[22] There are however exceptions to this general rule. For example, in Nigeria the GDF reports roughly $2 billion in "debt forgiveness or reduction" in 1992 and which goes into our debt relief estimate. However, this primarily reflects a large debt-buyback agreement concluded between Nigeria and its private creditors in 1992, that was largely financed by Nigeria's own resources. Since we do not have systematic information on the financing of all of these debt-buybacks in low-income countries, we simply include all of them, recognizing that for a few of the richer countries in our sample such as Nigeria this will not capture donor-funded debt relief.

[23] Daseking and Powell (1999) also rely on this data on debt forgiveness and reduction in their first estimate of debt relief. They do not however estimate the reduction in the present value of debt due to debt forgiveness as we do here by using information on the overal concessionality of debt.

We also want to measure the change in the present value of debt due to concessional reschedulings. However, published GDF data on debt rescheduling is problematic for our purposes. To see why, consider the hypothetical example of a country that in 2000 agrees with its creditors to consolidate all of the debt service due between 2000 and 2004 into a new loan with more concessional terms than the original loan. Although no debt forgiveness has taken place, there has been a reduction in the present value of the country's future obligations as a result of the agreement signed in 2000. However, the GDF convention is to report the flows of debt service rescheduled in each of the five years between 2000 and 2004. As a result, the GDF items "Principal Rescheduled" and "Interest Rescheduled" in a given year capture flows of debt service relief due to previous debt relief agreements, rather than the stock of debt rescheduled due to agreements in the current year.[24]

We instead use unpublished GDF data on "Rescheduling Commitments", which captures the total stock of debt that is rescheduled as a result of debt restructurings agreed upon in that year. Once we have the stock of debt rescheduled, we next need to make assumptions regarding the degree of concessionality of the rescheduling. Here we unfortunately have very little information, and the degree of concessionality can in practice vary widely. For example, in Paris Club agreements (described in more detail below) creditors have the choice between reducing the present value of debt through "debt service reductions" or "debt reductions". Under the former option, the total amount of debt covered by the agreement is rescheduled on concessional terms in order to achieve the desired present value reduction. Under the latter option, a fraction of the debt is forgiven outright (and is recorded as "debt forgiveness", but the remainder that is rescheduled is done so with no further concessionality.

Unfortunately, the aggregated GDF data on Rescheduling Commitments does not allow us to distinguish between such cases of concessional and non-concessional reschedulings. Absent better information, we make the following assumptions. For countries that did not have any prior concessional Paris Club agreement, we assume that the rescheduling observed in the GDF was non-concessional, i.e. the change in the present value of the amount restructured is zero. For countries that did have a prior Paris Club agreement, we assume that half of the amount rescheduled was done on non-concessional terms, and the other half was rescheduled on the most recent Paris Club terms received by the country.[25] Finally, we continue to assume that the concessionality of the stock of debt restructured prior to the restructuring is the same as that of the total debt stock of the country.

The main advantage of this GDF-based measure of debt relief is that it is comprehensive in the sense of capturing debt relief granted by all creditors, as it is based on debtor-reported data that in principle is comprehensive. Two disadvantages weigh against this. First, given the weak debt management capacity of many low-income countries and the complexity of many debt restructurings, we expect debtor-reported data on debt relief to be relatively noisy. Second, reported GDF data does not provide information on the terms of reschedulings and hence we need to rely on fairly arbitrary assumptions regarding the extent of concessionality of these restructurings.

[24] Daseking and Powell (1999) simply use the published GDF data on debt rescheduling, but interpret it as if it were the stock of debt rescheduled.

[25] We are assuming here that reschedulings with non-Paris Club creditors have the same terms as thos with Paris Club creditors. This assumption can be justified by the requirement in Paris Club agreements that the debtor seeks "comparable treatment" of debts owed to non-Paris Club creditors.

This is a non-trivial concern, as reschedulings are an important component of overall debt relief. As shown in Table 1, roughly 20 percent of our estimated total amount of debt relief using this method consists of concessional reschedulings.

Method 2: Estimates Based on Creditor-Reported Data

Our second estimate of debt relief is based on creditor-reported data from a variety of sources.[26] We begin with data reported by the Paris Club on its agreements with individual debtor countries. The Paris Club is a group of bilateral official creditors that jointly negotiates debt restructurings with individual debtor countries. Although the Paris Club has been in existence since 1956, only since late-1988 have Paris Club agreements with qualifying low-income countries had the explicit intent of reducing the present value of their outstanding obligations covered by the agreement.[27] In order to benefit from these debt reductions, creditor countries need to be poor, highly indebted, and have an IMF program in effect. Individual Paris Club agreements specify the debt instruments covered by the agreement (known as the amount "treated") and the intended reduction in their present value. The actual reschedulings to achieve the intended present value reduction are carried out through bilateral deals between the country and each of its creditors. As mentioned above, creditor countries can choose from a menu of options for present value of debt reductions. Typically these consist either of outright forgiveness combined with a non-concessional rescheduling of the balance (known as "debt reductions"), or else concessional reschedulings of the entire loan with no forgiveness (known as "debt service reductions"). The Paris Club has successively increased the concessionality of its agreements, with reductions in the present value of the amount of debt "treated" increasing from 33% (known as "Toronto" terms since October 1988), to 50% ("London" terms since December 1991), to 67% ("Naples" terms since December 1994), to 80% ("Lyons" terms since November 1996), and finally to 90% ("Cologne" terms since November 1999).

We have retrieved information on all concessional agreements with low-income countries since 1988 from the Paris Club website (www.clubdeparis.org). This source reports the total amount of debt "treated" under the agreement, as well as the broad terms of the treatment. To estimate the change in the present value of debt due to the agreement, we take the intended present value reduction and multiply it by the nominal amount of debt treated. As above, we convert this to present value terms by multiplying by the average concessionality of the overall stock of debt in the country in that year. There are two drawbacks to this method. The first is that present value reductions under Paris Club agreements typically apply only to non-Official Development Assistance (non-ODA) credits, while the treatment of ODA credits involves less additional concessionality. Unfortunately, the Paris Club does not systematically report the amounts of ODA and non-ODA credits treated. However, in our sample non-ODA credits account for the bulk of bilateral credits, so applying the present value reduction to

[26] We do not separately consider another potential source of creditor-reported data on debt relief, the OECD Development Assistance Committee (DAC) database. The DAC data follow similar reporting conventions as the GDF. In particular, reschedulings are reported by recipient only on a flow basis, and data on rescheduling committments is not reported by recipient. One particular weakness of the DAC data is that its measure of debt cancellation mixes data on present value reductions from Paris Club agreement with data on the face value of debt cancellations from other types of debt forgiveness. See OECD (2000) for details.

[27] Previous Paris Club agreements consisted of reschedulings that were intended to maintain the present value of the debt service flows to creditors. Of course, unanticipated movements in interest rates still affected the present value of repayments, but these were unintended changes in the present value of debt.

the total amount treated rather than only the non-ODA portion will probably not seriously overstate the amount of debt relief.

The second problem involves double counting of debt relief in countries that have benefitted from successive Paris Club agreements. For example, the total amount of debt "treated" under London terms for a country may include some debt that was previously reduced under Toronto terms. The Paris Club agreement will specify that the previously-reduced debt gets only a 17% present value reduction to "top up" the present value reduction from 33% under Toronto terms to the 50% under London terms. By applying the 50% present value reduction to the total amount of debt treated, we are again overestimating the amount of debt relief provided to the extent that this doublecounting is important. Unfortunately there is little we can do about this problem as the Paris Club does not systematically report information at this level of detail.

Our second data source for this method captures debt relief provided by multilateral creditors under the HIPC initiative, which was the first time multilateral creditors have provided relief with respect to their own loans. We have obtained from World Bank country documents estimates of the present value of multilateral debt relief actually implemented, by HIPC country and by year. Since this data is already reported in present value terms we simply add it to the Paris Club data described above.[28]

Our third data source captures donor-financed buybacks of commercial debts owed by low-income countries. The IDA Debt Reduction Facility was created in 1989 and has provided $632 million in grants to 21 low-income countries that qualify for highly-concessional lending from the International Development Association (IDA). Since most of the commercial debt repurchased under this facility was bought at a steep discount, the nominal amount of debt obligations removed from debtor countries is much larger, totalling approximately $8 billion. Since commercial debt is non-concessional, we take the face value of the amount of the debt buybacks for each country and add it to our previous two items.

Our final data source for this method captures debt relief provided by the Russian Federation. Unfortunately, we are not aware of a systematic source of creditor-reported data on this debt relief. Instead, we have obtained unpublished detailed data from the GDF on Russian debt relief, which allows us to isolate, by debtor country and by year, the GDF variables used in our first method (i.e. debt forgiveness or reduction, interest forgiven, and rescheduled commitments). We then use the same assumptions as we did for the first method to move from these variables to estimates of the change in the present value of debt due to debt relief. We multiply "debt forgiveness or reduction", which captures the nominal value of debt forgiven by the concessionality rate to obtain the present value of debt forgiven by Russia. For "Rescheduled Commitments" we again assume that for countries with concessional Paris Club agreements, half of the rescheduled amount is concessional and apply to it the most recent Paris Club terms received by the country. We multiply these figures by the concessionality rate before adding them to the present value of debt forgiven by Russia previously obtained.

This method of estimating debt relief has two main advantages. First, it primarily relies on creditor-reported data which is likely to be of better quality than debtor-reported

[28] We are particularly grateful to Boris Gamarra and Sunyoung Lee at the World Bank for providing this data. The data are reported in present value terms as of the year in which the debt relief is provided.

data. Second, for Paris Club relief and multilateral debt relief, we have more direct information on the intended present value reduction of debt relief, which we did not have in our first method. However, the main disadvantage of this method is that it is not as comprehensive as the debtor-reported data, as it does not capture debt relief granted by non-Paris Club bilateral creditors (other than Russia).[29]

Appendix B: Changes in the Present Value of Debt

In this appendix we show how to measure the contribution of debt relief to changes in the present value of debt. We first introduce notation for the present value of debt and debt relief, and then show the decomposition of changes over time in the present value of debt as a share of exports.

Notation for the Present Value of Debt

Let DS_{it} denote the flow debt service due in period t on loans that were contracted in period i. We assume that $DS_{it}=0$ for $t \leq i$, i.e. debt service obligations are incurred no earlier than the first period after which the loan is contracted. The present value of debt at the end of period T is defined as:

$$\text{(B1)} \qquad PV_T \equiv \sum_{i=0}^{T} \sum_{s=1}^{\infty} (1+\delta)^{-s} \cdot DS_{i,T+s}$$

where δ is the discount rate. The present value of debt at time T consists of the discounted value of all future debt service obligations associated with all loans contracted in periods up to T.

Let $R_{i,T+1+s}^{T+1}$ denote the reduction in debt service due in period T+1+s on loans contracted in period i due to debt relief granted in period T+1. We assume that $R_{i,T+1+s}^{T+1}=0$ if $s \leq 0$, i.e. debt relief involves only the reduction of future debt service obligations, and $R_{i,T+1+s}^{T+1}=0$ if $i>T$, i.e. debt relief can only be granted during period T+1 on loans contracted in period T or earlier. These reductions in debt service may be due either to the outright cancellation of a loan (which eliminates the future associated debt service), or due to a concessional restructuring of a loan (in which case $R_{i,T+1+s}^{T+1}$ captures the net change in debt service associated with the elimination of the old loan and its replacement with a new loan with easier terms). The change in the present value of debt between period T and period T+1 can be written as:

$$\text{(B2)} \qquad PV_{T+1} = (1+\delta) \cdot PV_T + PVNF_{T+1} - PVDR_{T+1}$$

[29] For some countries and years this type of debt relief is substantial. For example, in the early 1990s Egypt was the beneficiary of very large writeoffs of military debt owed to the United States, and writeoffs of other debts owed to other Arab states. These bilateral writeoffs were done outside the Paris Club and hence are not captured by our Method 2, but they are captured by Method 1 as they were reported to the GDF.

where $PVNF_{T+1} \equiv \sum_{s=1}^{\infty}(1+\delta)^{-s} \cdot DS_{T+1,T+s} - \sum_{i=0}^{T} DS_{i,T+1}$ measures the present value of new borrowing (i.e. the present value of future debt service associated with new loans contracted in period T+1) less debt service due in period T+1 on all loans contracted in the past; and $PVDR_{T+1} \equiv \sum_{i=0}^{T}\sum_{s=1}^{\infty}(1+\delta)^{-s} \cdot R_{i,T+1+s}^{T+1}$ measures the present value of reductions in future debt service granted during period T+1. As detailed in the text, we provide estimates of $PVDR_T$ on an annual basis for 62 low-income countries between 1989 and 2003.

Changes over Time in the Present Value of Debt

It is useful to interate Equation (B2) forward k periods and reorganize to obtain the following expression for the discounted growth rate of the present value of debt between period T and T+k:

$$\text{(B3)} \qquad \frac{(1+\delta)^{-k} \cdot PV_{T+k}}{PV_T} - 1 = \frac{\sum_{s=1}^{k}(1+\delta)^{-s} \cdot PVNF_{T+s}}{PV_T} - \frac{\sum_{s=1}^{k}(1+\delta)^{-s} \cdot PVDR_{T+s}}{PV_T}$$

The left-hand side of Equation (B3) is the discounted growth rate of the present value of debt in period T values between period T and T+k. The first term on the right-hand side captures the present value of all new borrowing during periods T+1 through T+k, discounted back to period T and expressed as a share of debt in period T. The second term captures the same present value of debt relief granted during periods T+1 through T+k, again discounted back to period T and normalized by period T debt. In the text we refer to this second term, evaluated between T=1988, T=1993, and T=1998 with k=5 years as the debt relief ratios for these three periods.

We can also divide Equation (B3) by exports, X_t, and rearrange to obtain the following expression for the change in the present value of debt relative to exports, which is a standard measure of the (change in the) debt burden of a country. This results in the following expression:

$$\text{(B4)} \qquad \frac{PV_{T+k}}{X_{T+k}} - \frac{PV_T}{X_T} = \frac{\sum_{s=1}^{k}(1+\delta)^{-s} \cdot PVNF_{T+s}}{X_T} - \frac{\sum_{s=1}^{k}(1+\delta)^{-s} \cdot PVDR_{T+s}}{X_T} + \left(1 - \left(\frac{1+g}{1+\delta}\right)^{k}\right) \cdot \frac{PV_{T+k}}{X_{T+k}}$$

where g is the average annual growth rate of exports between periods T and T+k. The first two terms again capture the present value of new borrowing and the present value of debt relief, now normalized by exports. The third term captures the contribution of export growth to changes in the present value of debt. The higher is export growth g, the lower is the change in the ratio of the present value of debt to exports.

We use Equation (B4) to account for the contribution of debt relief to changes in the present value of debt relative to exports over our three periods, 1988-1993, 1993-1998, and 1998-2003. We measure the left-hand side directly using the measure of the present value of total debt described in the text, and we also measure the second term on the right-hand side directly using our estimates of debt relief. We use a discount rate of 7.25%, which is the average over the period 1989-2003 of the OECD's "commerical interest reference rate" for long-maturity US-dollar-denominated obligations. We also directly measure the third term capturing the contribution of export growth, using the growth rate of current dollar exports over each period for each country. We measure the first term as a residual. In the data, this residual will consist of not only the present value of net flows described above, but also two other factors not captured in the above decomposition for notational convenience: changes in the US dollar value of debt due to cross-currency valuation changes, and changes in the present value of debt due to fluctuations over time in the discount rate used to construct the present value series on the left-hand side.

Appendix Table B provides some summary statistics on this decomposition. We first decompose the variance of changes in the present value of debt relative to exports into the the parts attributable to the three terms in Equation (B4). We do this by reporting the slope coefficients of regressions of each of the individual components on the total change in debt. The slope coefficient can be interpreted as the share of the variance of changes in debt due to the corresponding component. We do this for changes in debt over our three subperiods, again eliminating a handful of observations with extreme changes in the present value of debt to exports. The first row reports the share of the variance due to growth in exports in the denominator of the debt ratio. This share is substantial in all three periods, ranging from 52 percent to 79 percent. The next two rows report the contribution of debt relief, for our two alternative estimates. This contribution is small for all periods, and is at most 24 percent in the first period, using our first measure of debt relief. The remaining rows of the table report the share of the variance in changes in debt due to a combination of other factors including net new borrowing, cross-currency valuation changes, and changes in discount rates used to calculate present values. These other factors unrelated to debt relief also contribute importantly to changes in debt, particularly in the last subperiod.[30]

We also report the simple correlation between the debt relief component and the change in total debt. These correlations are small in most cases, and rise to 0.56 only in the first period and for the first measure of debt relief. Together the results in this table highlight the fact that debt relief has typically had only a small impact on overall debt burdens measured as the present value of debt relative to exports.

[30] This finding is consistent with Easterly (2002) who documents that among HIPC countries, debt relief and new borrowing are positively correlated.

Appendix Table B: Variance Decomposition for Changes in Present Value of Debt Relative to Exports

	1988-1993	1993-1998	1998-2003
Share of Variance of Total Change in PV of Debt/Exports Due to:			
Growth Component	0.64	0.79	0.52
Debt Relief Component			
Method 1	0.24	0.08	0.13
Method 2	0.04	0.05	0.00
Residual			
Method 1	0.12	0.13	0.35
Method 2	0.32	0.16	0.48
Correlation of Debt Relief With Total Changes in PV of Debt/Exports			
Method 1	0.56	0.17	0.33
Method 2	0.30	0.14	0.00

Appendix C: Data Definitions and Sources

Variable*	Sources	Definitions/Explanations
Real Per Capita GDP Growth	WDI	GDP data in constant local currency unit (LCU).
Aid/GDP	GDF and WDI	Official development assistance and official aid as a share of GDP (both in current USD)
Government Spending/GDP	Gupta, Clements, Tiongson (1998), and GDF	Total government expenditures as a share of GDP net of interest payments on public and publicly-guaranteed debt.
Share of Health and Education	Gupta, Clements, Tiongson (1998), and GDF	Government expenditure in education and health as a share of total government expenditures.
Tax Revenues/GDP	Baunsgaard and Keen (2004), and GFS	Tax revenue as a share of GDP. Data from 1975 to 2000 in Baunsgaard-Keen, extended using changes in GFS data.
Share of Trade Taxes	Baunsgaard and Keen (2004), and GFS	Trade taxes as a share of GDP. Data from 1975 to 2000 in Baunsgaard-Keen, extended using changes in GFS data
Policy Quality	World Bank Data	Country Policy and Institutional Assessment (CPIA). Rating: 1-6.
Investment/GDP	WDI	Gross capital formation as a share of GDP (both in constant LCU)
Arrears/Total Outstanding Debt	GDF	Sum of interest and principal arrears (both in current USD)
Debt Relief 1	Own estimates based on GDF and Reschedule Commitments (GDF unpublished)	Reductions during the period in the present value of debt due to debt relief as share of exports at the beginning of the period. See section 3 (Method 1) of the paper for details.
Debt Relief 2	Own estimates based on Paris Club, Multilateral (WB data), IDA Debt Reduction Facility, and GDF on Russian debt relief	Reductions during the period in the present value of debt due to debt relief as share of exports at the beginning of the period. See section 3 (Method 2) of the paper for details.
Present Value of Debt/Exports	Dikhanov (2004) and WDI	Present value of public and publicly-guaranteed debt as a share of Exports (both in current USD)

Appendix Table C: Summary Statistics

	1989-1993		1994-1998		1999-2003	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Debt Relief 1/Exports	0.383	0.580	0.365	0.537	0.264	0.533
Debt Relief 2/Exports	0.091	0.137	0.308	0.454	0.529	0.880
Aid/GDP	0.166	0.164	0.151	0.154	0.120	0.107
Real Per Capita GDP Growth	-0.007	0.037	0.015	0.030	0.019	0.029
Government Spending/GDP	0.284	0.118	0.257	0.115	0.287	0.134
Share of Health and Education	0.198	0.069	0.215	0.067	0.232	0.076
Tax Revenues/GDP	0.140	0.064	0.138	0.068	0.141	0.058
Share of Trade Taxes	0.290	0.138	0.261	0.117	0.215	0.125
Policy Quality	3.144	0.838	3.197	0.789	3.071	0.598
Investment/GDP	0.199	0.103	0.212	0.117	0.233	0.129
Arrears / Outstanding Debt	0.120	0.140	0.154	0.182	0.148	0.203
PV of Debt / Exports	2.502	1.537	2.957	1.910	2.600	2.044

Note: Summary statistics refer to five-year averages of indicated variables.

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Table 1: Estimates of Total Debt Relief

	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003
Millions US Dollars															
Method 1															
Debt Forgiven or Reduced	3270	2180	2390	2910	1150	3320	3750	9180	8000	944	4130	9810	4800	5080	2480
Reschedulings	498	753	775	918	347	990	870	1050	2460	630	1030	317	638	1610	176
Total Method 1	3770	2930	3160	3830	1490	4310	4620	10200	10500	1570	5150	10100	5440	6690	2650
Method 2															
Paris Club	551	657	434	1090	566	1510	2360	2490	3050	1760	2020	661	4160	7540	160
Multilateral Creditors	0	0	0	0	0	0	0	0	0	558	775	7980	1870	1460	2390
IDA Facility	0	0	231	69	323	210	1330	350	45	724	34	62	452	0	266
Russian Debt	1	670	0	0	60	0	1	6560	2530	43	3610	9270	217	360	239
Total Method 2	551	1330	664	1160	949	1720	3690	9390	5630	3090	6430	18000	6700	9360	3060

Present Value of Debt Relief as of:	1988	2003
Method 1		
Debt Forgiven or Reduced	34563	109938
Reschedulings	7502.8	23865
Total Method 1	42043	133729
Method 2		
Paris Club	14490	46090
Multilateral Creditors	6039	19210
IDA Facility	2280	7251
Russian Debt	11459	36449
Total Method 2	34278	109031

Table 2: Estimates of Debt Relief by Country

		Total Debt Relief, Millions USD						Total Debt Relief, Share of Initial Debt					
		Method 1			Method2			Method 1			Method2		
Code	Country	1989-1993	1994-1998	1999-2003	1989-1993	1994-1998	1999-2003	1989-1993	1994-1998	1999-2003	1989-1993	1994-1998	1999-2003
AGO	Angola	0	2830	8	0	2830	0	0.0%	30.1%	0.1%	0.0%	30.1%	0.0%
ARM	Armenia	0	0	25	0	0	24	..	0.0%	7.0%	..	0.0%	6.6%
AZE	Azerbaijan	0	17	0	0	0	0	..	48.8%	0.0%	..	0.0%	0.0%
BDI	Burundi	45	7	8	0	0	0	11.8%	1.2%	1.3%	0.0%	0.0%	0.0%
BEN	Benin	172	121	65	65	60	187	21.8%	14.1%	7.1%	8.2%	6.9%	20.4%
BFA	Burkina Faso	108	78	91	18	18	368	23.2%	11.8%	12.4%	3.8%	2.7%	49.8%
BGD	Bangladesh	117	0	53	0	0	0	2.3%	0.0%	0.6%	0.0%	0.0%	0.0%
BOL	Bolivia	1230	523	995	187	1020	853	36.0%	16.7%	29.8%	5.5%	32.8%	25.6%
BTN	Bhutan	0	0	0	0	0	0	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%
CAF	Central African Repu	75	47	5	1	21	0	22.1%	9.1%	0.9%	0.2%	4.0%	0.0%
CHN	China	0	0	0	0	0	0	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%
CIV	Cote d'Ivoire	290	5110	578	0	1980	850	3.4%	44.5%	6.0%	0.0%	17.3%	8.8%
CMR	Cameroon	276	1270	971	0	1290	1190	8.9%	21.3%	14.9%	0.0%	21.6%	18.2%
COG	Congo, Rep.	151	639	19	0	899	0	4.5%	14.4%	0.4%	0.0%	20.3%	0.0%
COM	Comoros	22	3	0	0	0	0	19.7%	2.6%	0.0%	0.0%	0.0%	0.0%
DJI	Djibouti	12	7	1	0	0	0	12.9%	4.8%	0.3%	0.0%	0.0%	0.0%
ERI	Eritrea	0	0	0	0	0	0	..	0.0%	0.0%	..	0.0%	0.0%
ETH	Ethiopia	126	217	3280	137	396	3880	2.7%	2.6%	37.5%	3.0%	4.8%	44.4%
GHA	Ghana	121	39	169	0	0	943	9.5%	1.6%	4.4%	0.0%	0.0%	24.8%
GIN	Guinea	285	143	87	80	79	361	17.9%	6.9%	3.8%	5.0%	3.8%	15.6%
GMB	Gambia, The	0	0	6	0	0	36	0.1%	0.0%	2.4%	0.0%	0.0%	14.9%
GNB	Guinea-Bissau	11	75	168	6	90	283	2.9%	12.5%	23.4%	1.5%	15.0%	39.2%
GNQ	Equatorial Guinea	14	0	0	13	21	0	9.7%	0.0%	0.0%	9.5%	10.9%	0.0%
GUY	Guyana	253	396	281	100	349	463	26.0%	27.2%	32.1%	10.3%	24.0%	52.8%
HND	Honduras	575	90	261	57	60	503	23.6%	2.6%	7.8%	2.3%	1.7%	15.1%
HTI	Haiti	71	32	0	0	32	0	19.0%	7.5%	0.0%	0.0%	7.4%	0.0%
IND	India	0	0	0	0	0	0	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%
KEN	Kenya	400	14	7	0	0	0	12.1%	0.3%	0.2%	0.0%	0.0%	0.0%
KHM	Cambodia	115	61	0	0	105	0	11.2%	4.4%	0.0%	0.0%	7.7%	0.0%
LAO	Lao PDR	5	0	0	0	0	0	1.2%	0.0%	0.0%	0.0%	0.0%	0.0%
LBR	Liberia	1	0	0	0	0	0	0.1%	0.0%	0.0%	0.0%	0.0%	0.0%

Table 2, Cont'd: Estimates of Debt Relief by Country

		Total Debt Relief, Millions USD							Total Debt Relief, Share of Initial Debt					
		Method 1			Method2				Method 1			Method2		
Code	Country	1989-1993	1994-1998	1999-2003	1989-1993	1994-1998	1999-2003		1989-1993	1994-1998	1999-2003	1989-1993	1994-1998	1999-2003
LSO	Lesotho	4	0	0	0	0	0		2.5%	0.0%	0.0%	0.0%	0.0%	0.0%
MDA	Moldova	0	0	116	0	0	105		..	0.0%	14.6%	..	0.0%	13.3%
MDG	Madagascar	521	236	209	29	463	474		19.0%	8.1%	6.5%	1.1%	16.0%	14.7%
MLI	Mali	19	267	336	8	30	620		2.1%	13.8%	17.1%	0.9%	1.5%	31.6%
MMR	Myanmar	45	0	0	0	0	0		1.5%	0.0%	0.0%	0.0%	0.0%	0.0%
MNG	Mongolia	0	0	0	0	0	0		0.0%	0.0%	0.0%	0.0%	0.0%	0.0%
MOZ	Mozambique	1110	706	2260	949	255	3570		33.7%	17.6%	47.8%	28.8%	6.3%	75.4%
MRT	Mauritania	158	56	358	74	69	564		12.0%	3.5%	21.6%	5.6%	4.3%	34.0%
MWI	Malawi	27	0	89	0	0	424		4.3%	0.0%	6.9%	0.0%	0.0%	32.9%
NER	Niger	210	136	116	104	84	312		26.6%	15.1%	12.0%	13.2%	9.3%	32.2%
NGA	Nigeria	2190	1440	125	0	0	0		8.2%	5.1%	0.4%	0.0%	0.0%	0.0%
NIC	Nicaragua	748	5620	1180	272	3860	1400		10.1%	56.2%	22.6%	3.7%	38.6%	26.9%
NPL	Nepal	4	0	1	0	0	0		0.7%	0.0%	0.1%	0.0%	0.0%	0.0%
PAK	Pakistan	0	0	0	0	0	0		0.0%	0.0%	0.0%	0.0%	0.0%	0.0%
RWA	Rwanda	22	8	25	0	14	325		8.2%	1.8%	4.0%	0.0%	3.1%	52.4%
SDN	Sudan	1	0	0	0	0	0		0.0%	0.0%	0.0%	0.0%	0.0%	0.0%
SEN	Senegal	632	331	57	83	311	234		26.8%	13.8%	2.7%	3.5%	13.0%	11.0%
SLE	Sierra Leone	75	234	80	47	227	283		16.8%	22.0%	10.2%	10.7%	21.4%	36.2%
SOM	Somalia	0	0	0	0	0	0		0.0%	0.0%	0.0%	0.0%	0.0%	0.0%
STP	Sao Tome and Princi	0	6	18	0	9	65		0.5%	4.9%	11.4%	0.0%	7.2%	41.9%
TCD	Chad	40	50	24	4	11	111		19.1%	11.1%	4.1%	2.1%	2.3%	18.9%
TGO	Togo	129	148	2	41	117	0		17.8%	17.7%	0.2%	5.7%	13.9%	0.0%
TJK	Tajikistan	0	0	13	0	0	0		..	0.0%	2.9%	..	0.0%	0.0%
TZA	Tanzania	534	646	1340	241	648	2210		13.6%	12.5%	24.6%	6.1%	12.6%	40.5%
UGA	Uganda	128	329	197	173	267	524		12.3%	19.6%	10.0%	16.7%	15.9%	26.5%
UZB	Uzbekistan	0	0	0	0	0	0		..	0.0%	0.0%	..	0.0%	0.0%
VNM	Vietnam	227	496	7730	180	0	7670		46.7%	2.4%	37.2%	37.0%	0.0%	36.9%
YEM	Yemen, Rep.	12	2170	163	0	2680	433		0.3%	35.4%	5.0%	0.0%	43.9%	13.3%
ZAR	Congo, Dem. Rep.	328	0	2970	372	0	5060		6.6%	0.0%	25.5%	7.5%	0.0%	43.4%
ZMB	Zambia	844	542	302	482	415	1750		21.1%	14.7%	7.2%	12.1%	11.2%	41.9%
ZWE	Zimbabwe	16	48	4	0	0	0		0.9%	1.7%	0.1%	0.0%	0.0%	0.0%
	Total	**12499**	**25187**	**24792**	**3723**	**18707**	**36075**	**Avg**	**11.0%**	**9.1%**	**7.9%**	**3.6%**	**7.0%**	**15.5%**

Table 3: Estimated Impact of Debt Relief

	Change 1994-1998 Over 1989-1993			Change 1999-2003 Over 1994-1998		
	Method 1	**Method 2**	**# Obs**	**Method 1**	**Method 2**	**# Obs**
Real Per Capita GDP Growth	**-0.001**	**0.042**	46	**-0.011**	**-0.008**	54
	0.13	1.61		1.51	0.95	
Government Spending/GDP	**0.003**	**-0.001**	35	**0.004**	**-0.001**	44
	0.20	0.02		0.35	0.09	
Share of Health and Education	**-0.006**	**0.028**	35	**0.039**	**0.043**	43
	0.30	0.48		3.55	2.99	
Tax Revenues/GDP	**0.013**	**0.047**	40	**0.004**	**0.011**	41
	1.45	1.75		1.01	1.38	
Aid/GDP	**-0.001**	**-0.167**	47	**-0.020**	**-0.025**	54
	0.09	1.94		1.15	0.80	
Policy Quality	**0.390**	**1.989**	48	**0.041**	**0.047**	54
	3.37	3.76		0.48	0.42	
Share of Trade Taxes	**-0.076**	**-0.332**	40	**0.009**	**0.006**	41
	1.61	1.91		0.64	0.22	
Investment/GDP	**0.001**	**0.125**	44	**0.005**	**-0.020**	48
	0.10	0.93		0.31	1.02	

Note: Each row of the table corresponds to a regression of the change in the five-year average of the indicated variable on cumulative debt relief over previous five-year period. Coefficient estimates in bold and absolute value of t-statistics below

Table 4: Nonlinear Impacts of Debt Relief

	Change 1994-1998 Over 1989-1993				Change 1999-2003 Over 1994-1998			
	Linear Term		Interaction Term		Linear Term		Interaction Term	
	Method 1	Method 2	Method 1	Method 2	Method 1	Method 2	Method 1	Method 2
Part 1: Interactions With Arrears								
Real Per Capita GDP Growth	**0.000**	**0.041**	**-0.003**	**0.048**	**-0.012**	**-0.007**	**-0.014**	**-0.217**
	0.03	1.23	0.15	0.36	1.32	0.71	0.32	0.62
Government Spending/GDP	**-0.003**	**-0.006**	**0.028**	**0.134**	**0.005**	**-0.002**	**-0.012**	**0.009**
	0.14	0.10	0.79	0.59	0.32	0.12	0.16	0.16
Share of Health and Education	**-0.036**	**-0.029**	**0.046**	**0.245**	**0.038**	**0.047**	**-0.082**	**-0.076**
	1.53	0.45	1.22	1.01	2.66	2.67	1.12	1.35
Tax Revenues/GDP	**0.009**	**0.045**	**0.011**	**0.054**	**0.005**	**0.010**	**0.036**	**0.009**
	0.60	1.16	0.51	0.44	0.80	1.23	1.31	0.40
Policy Quality	**0.446**	**2.023**	**-0.670**	**-1.225**	**0.040**	**0.074**	**-0.449**	**-0.570**
	2.67	2.93	1.55	0.44	0.36	0.56	0.81	1.30
Share of Trade Taxes	**-0.107**	**-0.344**	**0.087**	**0.233**	**0.010**	**0.004**	**0.035**	**0.035**
	2.06	2.74	1.20	0.57	0.56	0.17	0.43	0.54
Investment/GDP	**0.002**	**0.128**	**-0.012**	**-0.086**	**0.007**	**-0.022**	**0.238**	**0.023**
	0.07	1.06	0.16	0.18	0.33	0.78	2.23	0.25

Note: Each row of the table corresponds to a regression of the change in the five-year average of the indicated variable on cumulative debt relief over previous five-year period. Coefficient estimates in bold and absolute value of t-statistics below.

Table 4, Cont'd: Nonlinear Impacts of Debt Relief

	Change 1994-1998 Over 1989-1993				Change 1999-2003 Over 1994-1998			
	Linear Term		Interaction Term		Linear Term		Interaction Term	
	Method 1	Method 2	Method 1	Method 2	Method 1	Method 2	Method 1	Method 2
Part 2: Interactions With Policy								
Real Per Capita GDP Growth	**0.006**	**0.047**	**-0.016**	**-0.281**	**-0.006**	**-0.002**	**-0.022**	**-0.029**
	0.40	1.33	0.85	0.38	0.62	0.21	1.39	1.46
Government Spending/GDP	**-0.006**	**-0.005**	**0.021**	**0.118**	**0.005**	**-0.009**	**0.000**	**0.033**
	0.23	0.08	0.67	0.55	0.29	0.45	0.02	0.98
Share of Health and Education	**-0.028**	**-0.024**	**0.007**	**0.111**	**0.050**	**0.056**	**-0.044**	**-0.056**
	1.04	0.36	0.22	0.48	3.25	3.01	1.78	1.79
Tax Revenues/GDP	**-0.005**	**0.028**	**0.028**	**0.091**	**0.008**	**0.017**	**-0.010**	**-0.019**
	0.26	0.69	1.39	1.24	1.09	1.89	0.99	1.46
Policy Quality	**0.533**	**2.216**	**-0.904**	**-1.581**	**0.109**	**0.116**	**-0.259**	**-0.322**
	3.27	3.10	2.78	1.05	0.89	0.82	1.29	1.28
Share of Trade Taxes	**-0.112**	**-0.259**	**0.055**	**-0.344**	**0.017**	**0.011**	**-0.024**	**-0.014**
	1.74	1.97	0.78	1.44	0.84	0.41	0.78	0.37
Investment/GDP	**0.002**	**0.124**	**-0.005**	**0.014**	**0.013**	**-0.015**	**-0.027**	**-0.027**
	0.06	0.99	0.09	0.05	0.50	0.49	0.66	0.53

Note: Each row of the table corresponds to a regression of the change in the five-year average of the indicated variable on cumulative debt relief over previous five-year period. Coefficient estimates in bold and absolute value of t-statistics below.

Table 5: Instrumental Variables Estimates of Effects of Debt Relief

	Change 1994-1998 Over 1989-1993			Change 1999-2003 Over 1994-1998		
	Method 1	**Method 2**	**# Obs**	**Method 1**	**Method 2**	**# Obs**
Coefficient on Debt Relief in IV Regression where Dependent Variable Is Change in:						
Real Per Capita GDP Growth	**0.008**	**0.024**	41	**0.110**	**0.047**	43
	0.53	0.56		1.28	1.63	
Government Spending/GDP	**0.033**	**0.114**	31	**0.115**	**0.048**	33
	1.45	1.47		0.58	0.59	
Share of Health and Education	**-0.038**	**-0.133**	31	**0.059**	**0.024**	32
	1.01	1.07		0.36	0.38	
Tax Revenues/GDP	**0.021**	**0.068**	39	**0.035**	**0.017**	39
	1.02	1.03		0.89	0.92	
Aid/GDP	**-0.116**	**-0.338**	41	**-0.204**	**-0.088**	43
	1.57	1.95		0.73	0.74	
Policy Quality	**0.551**	**1.581**	42	**-0.029**	**-0.012**	43
	1.17	1.32		0.03	0.03	
Share of Trade Taxes	**-0.128**	**-0.412**	39	**-0.092**	**-0.045**	39
	1.33	1.77		0.98	1.01	
Investment/GDP	**0.118**	**0.351**	39	**0.099**	**0.044**	38
	0.97	1.09		0.43	0.43	
First-Stage Regression: (Dependent Variable is debt relief as share of exports)						
5-year lag of PV of debt/exports	**0.195**	**0.068**		**0.040**	**0.094**	
	2.71	2.91		1.59	2.99	
F-Statistic	7.34	8.48		2.52	8.93	
# of Observations	42	42		43	43	

Note: Each row of the table corresponds to a regression of the change in the five-year average of the indicated variable on cumulative debt relief over previous five-year period. Coefficient estimates in bold and absolute value of t-statistics below.

Table 6: Impact of Changes in Total Debt on Outcomes

	Change 1994-1998 Over 1989-1993				Change 1999-2003 Over 1994-1998			
	OLS	# Obs	IV	# Obs	OLS	# Obs	IV	# Obs
Coefficient on Change in PV of Total Debt/Exports in IV Regression where Dependent Variable Is Change in:								
Real Per Capita GDP Growth	**0.010**	33	**0.004**	31	**0.011**	45	**-0.026**	36
	1.72		0.51		2.17		1.18	
Government Spending/GDP	**-0.013**	27	**-0.018**	25	**0.005**	37	**-0.039**	28
	1.99		0.75		0.52		0.58	
Share of Health and Education	**-0.017**	27	**0.011**	25	**-0.004**	36	**-0.018**	27
	1.21		0.33		0.43		0.44	
Tax Revenues/GDP	**-0.003**	32	**-0.019**	31	**0.003**	34	**-0.003**	33
	0.67		1.47		0.82		0.04	
Policy Quality	**0.022**	33	**-0.006**	31	**-0.034**	45	**-0.058**	36
	0.20		0.02		0.35		0.17	
Share of Trade Taxes	**0.047**	32	**0.066**	31	**0.006**	34	**0.022**	33
	2.38		1.25		0.42		0.88	
Investment/GDP	**-0.001**	31	**0.018**	29	**0.003**	39	**-0.047**	31
	0.14		0.93		0.42		0.50	
First-Stage Regression: (Dependent Variable is Change in PV of Total Debt/Exports)								
5-year lag of PV of debt/exports			**-0.431**				**-0.188**	
			2.88				2.50	
F-Statistic			8.27				6.27	
# of Observations			31				36	

Note: Each row of the table corresponds to a regression of the change in the five-year average of the indicated variable on cumulative debt relief over previous five-year period. Coefficient estimates in bold and absolute value of t-statistics below.

Table 7: Impact of Debt Relief 1999-2003

	Coefficient on Debt Relief		
	Method 1	**Method 2**	**# Obs**
Real Per Capita GDP Growth	**0.000**	**0.000**	54
	0.04	0.38	
Government Spending/GDP	**0.004**	**0.001**	41
	1.63	1.47	
Share of Health and Education	**0.000**	**0.000**	41
	0.09	0.09	
Tax Revenues/GDP	**0.005**	**0.003**	40
	1.06	1.31	
Aid/GDP	**0.003**	**0.001**	54
	1.44	1.13	
Policy Quality	**0.008**	**0.004**	54
	1.42	1.22	
Share of Trade Taxes	**0.007**	**0.004**	40
	1.02	1.56	
Investment/GDP	**-0.002**	**-0.001**	46
	1.17	1.31	

Note: Each row of the table corresponds to a regression of the change in the five-year average of the indicated variable on cumulative debt relief over previous five-year period. Coefficient estimates in bold and absolute value of t-statistics below.

Figure 1: Present Value of Total Debt Relief, Alternative Estimates

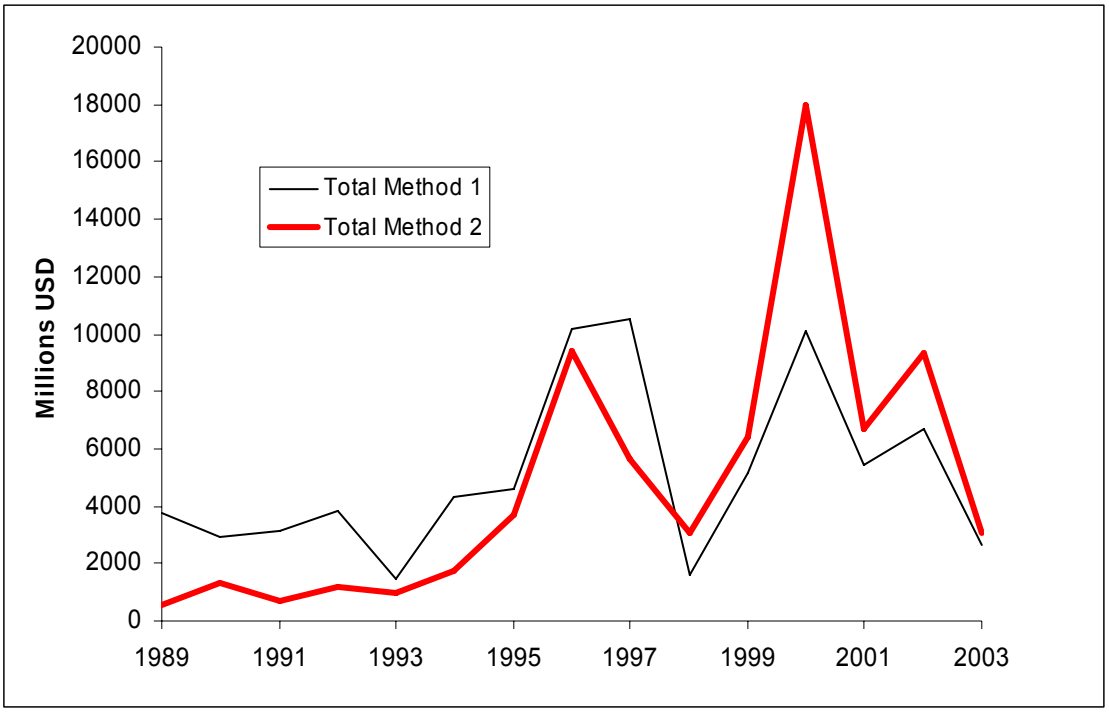


Figure 2: Present Value of Debt Relief 1989-2003
(Share of Present Value of Debt in 1988)

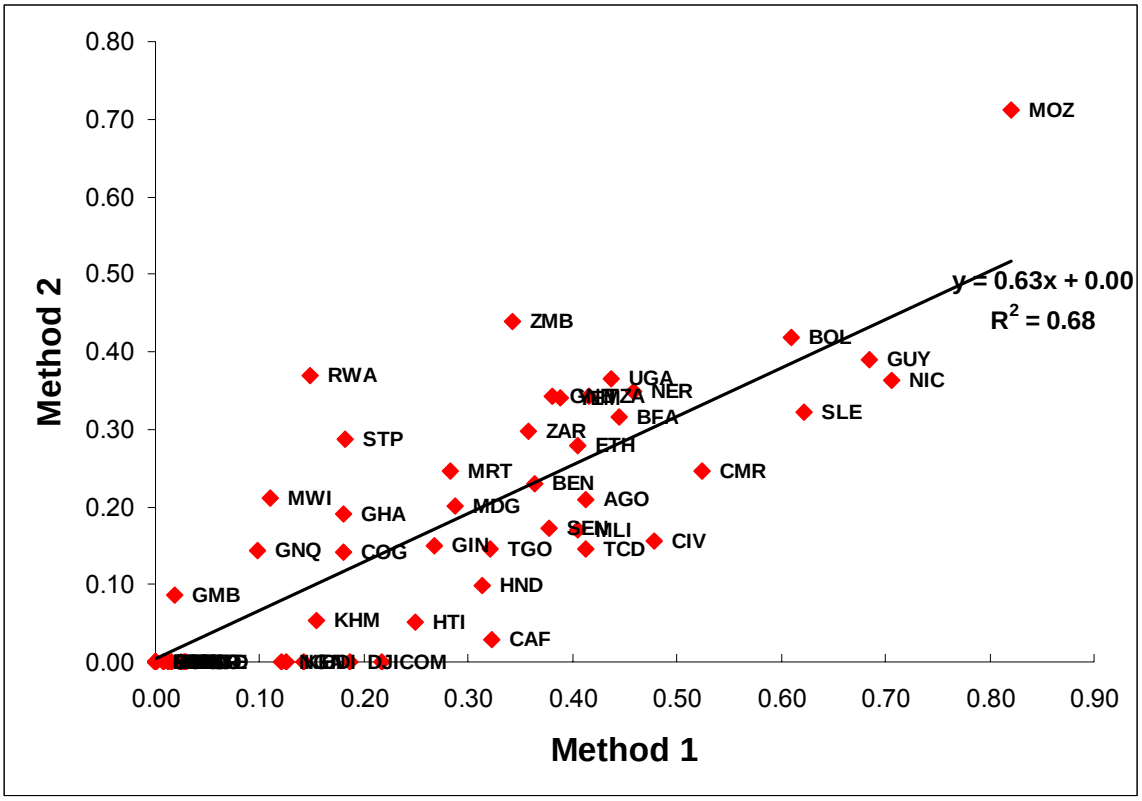
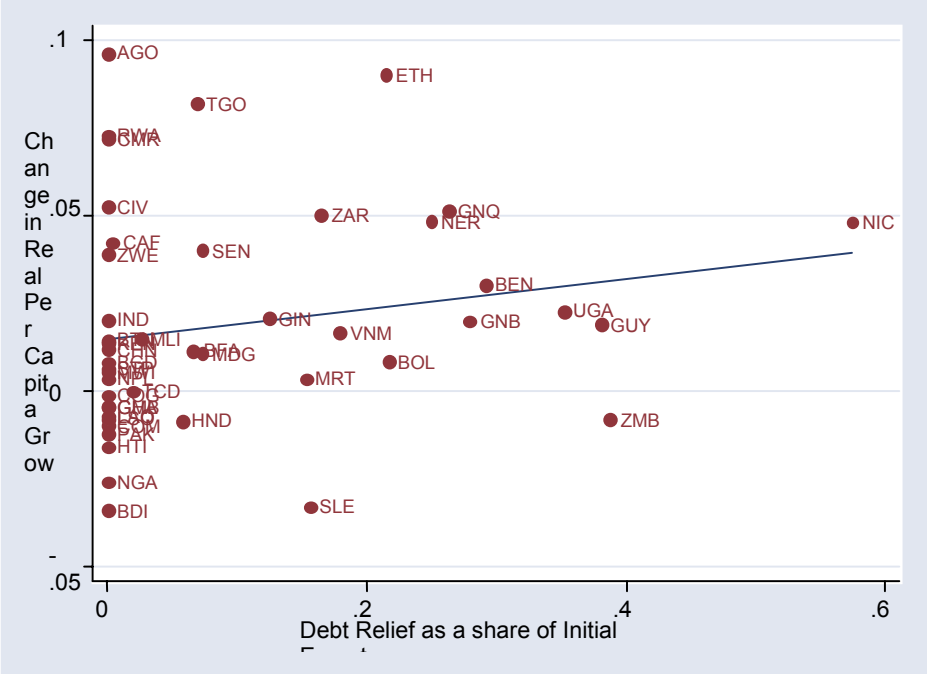


Figure 3: Debt Relief and Growth

Real Per Capita Growth
(Change 1994-1998 Over 1989-1993, Method 2 Debt Relief)



Real Per Capita Growth
(Change 1999-2003 Over 1994-1998, Method 2 Debt Relief)

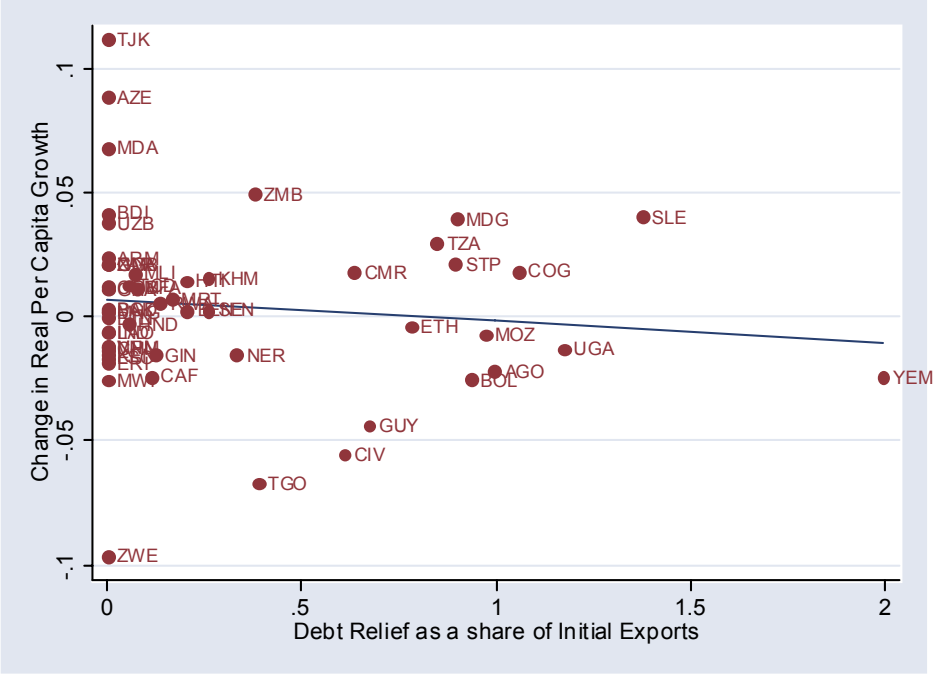
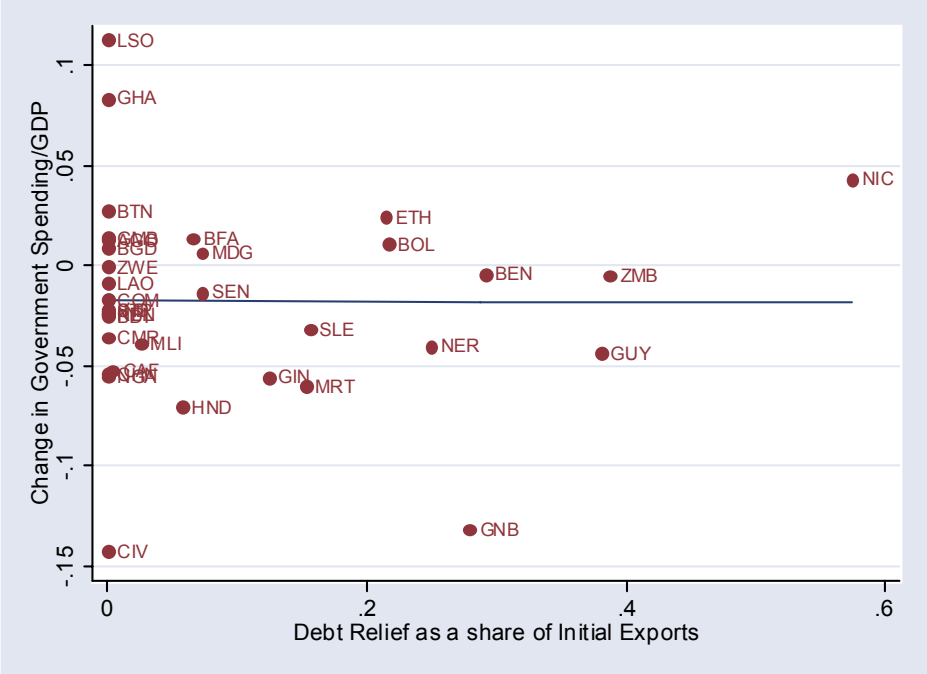


Figure 4: Debt Relief and Government Spending

Government Spending/GDP
(Change 1994-1998 Over 1989-1993, Method 2 Debt Relief)



Health and Education Spending/Total Spending
(Change 1999-2003 over 1994-1998, Method 1 Debt Relief)

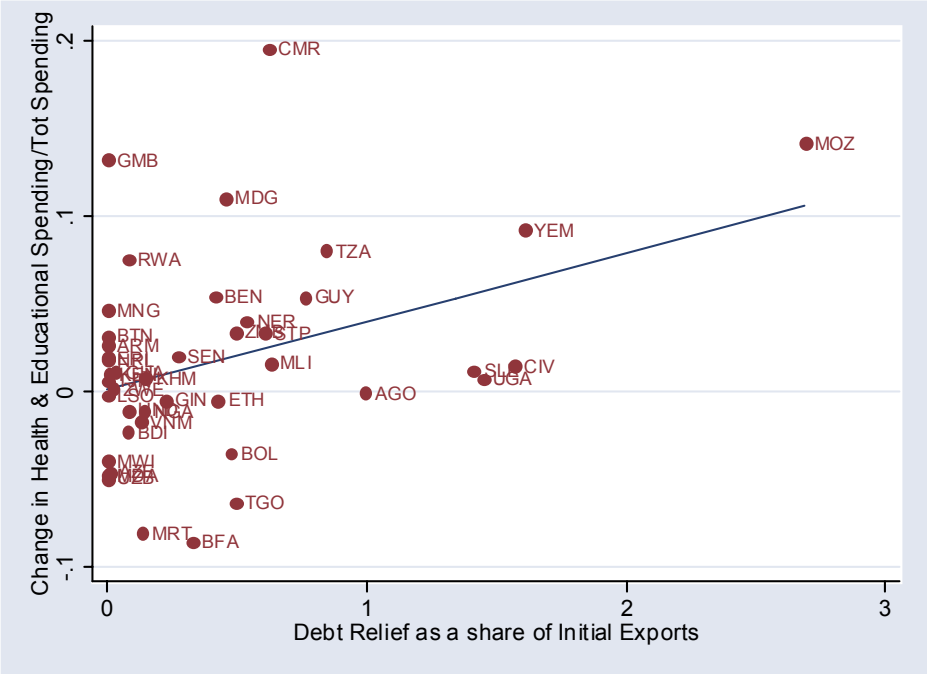
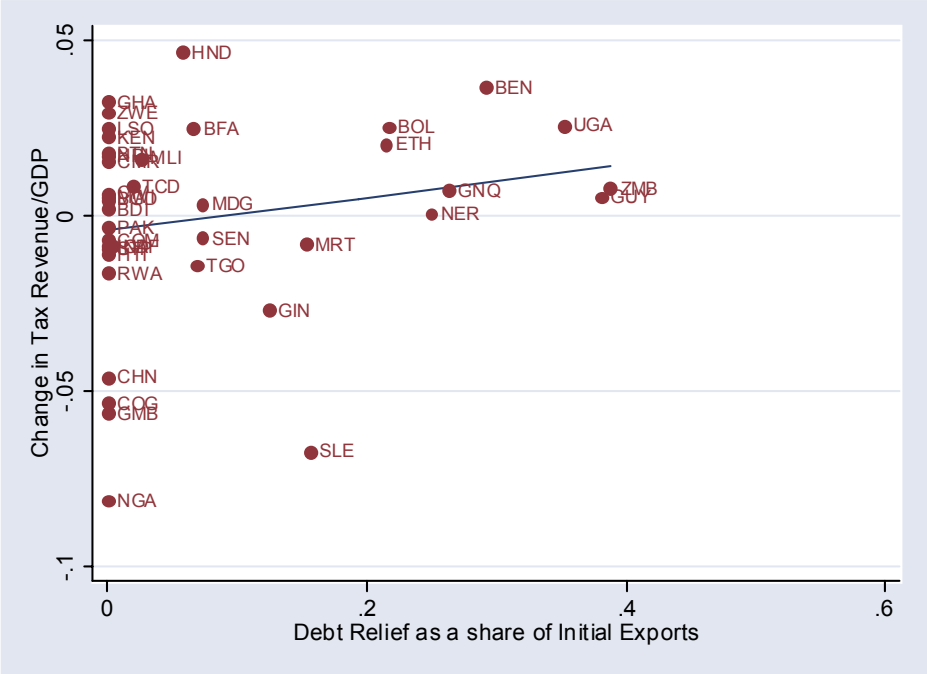


Figure 5: Debt Relief, Tax Revenues, and Aid

Total Tax Revenue/GDP
(Change 1994-1998 over 1989-1993, Method 2 Debt Relief)



Aid/GDP
(Change 1994-1998 over 1989-1993, Method 2 Debt Relief)

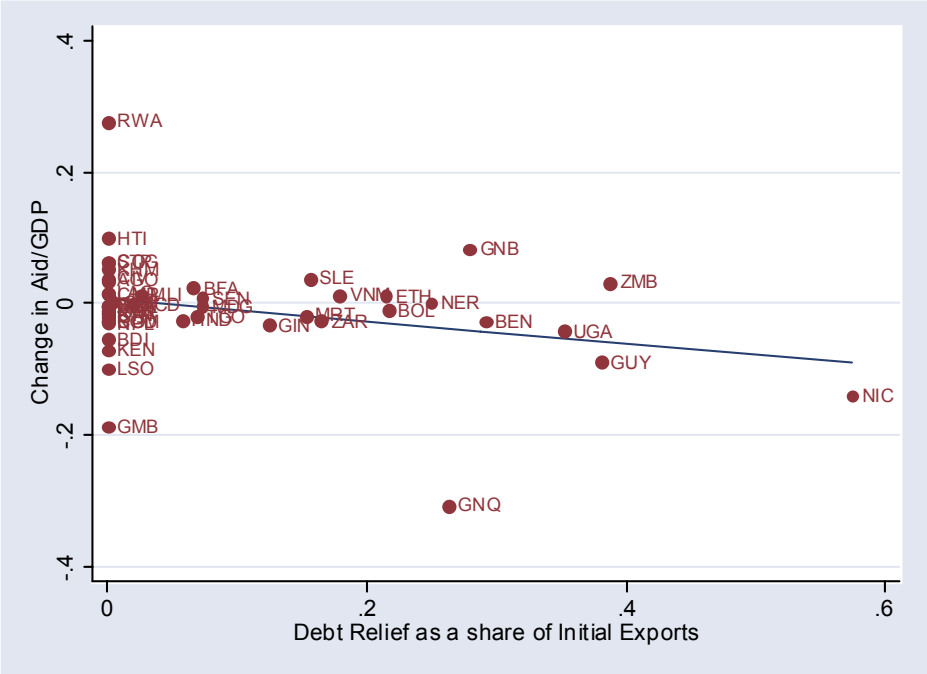
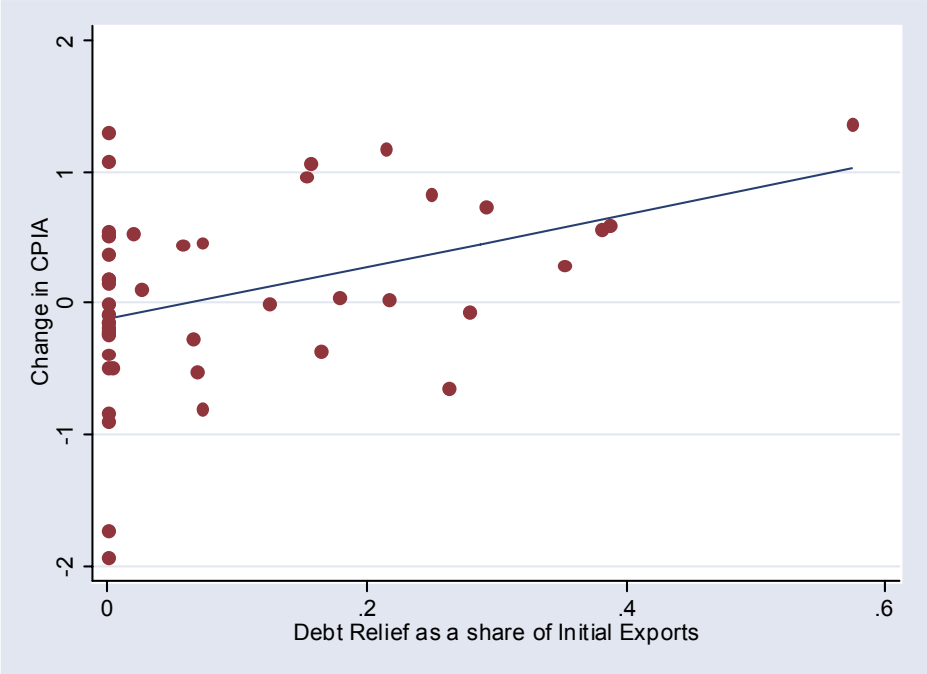
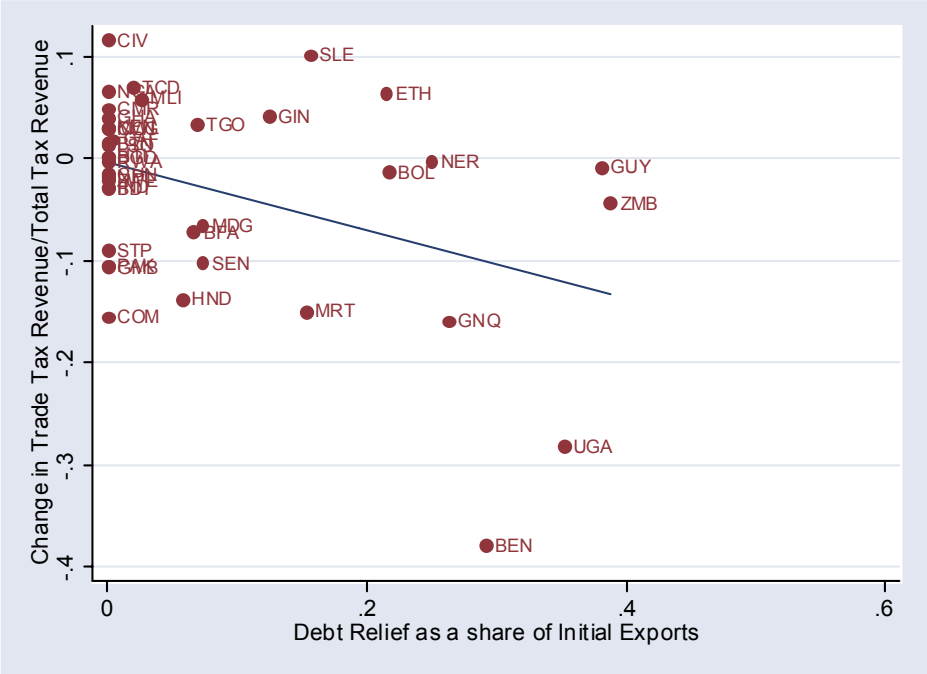


Figure 6: Debt Relief and Policy Outcomes

Country Policy and Institutional Assessment
(Change 1994-1998 over 1989-1993 , Method 2 Debt Relief)



Trade Tax Revenue/Total Tax Revenue
(Change 1994-1998 over 1989-1993, Method 2 Debt Relief