ÉQUIPE DE RECHERCHE SUR L'UTILISATION
DES DONNÉES INDIVIDUELLES EN LIEN
AVEC LA THÉORIE ÉCONOMIQUE

Sous la co-tutelle de :
UPEC • UNIVERSITÉ PARIS-EST CRÉTEIL
UPEM • UNIVERSITÉ PARIS-EST MARNE-LA-VALLÉE

Series of ERUDITE Working Papers

N° 23-2019

Title

Did Debt Relief Initiatives help to reach the MDGs?

A Focus on Primary Education

Authors

Marine DE TALANCÉ, Marin FERRY, Miguel NIÑO-ZARAZÚA

Did Debt Relief Initiatives help to reach the MDGs? A Focus on Primary Education

Marine de Talancé*[1,2], Marin Ferry†[1,2], and Miguel Niño-Zarazúa‡[3]

[1]ERUDITE, Université Paris-Est Marne-la-Vallée, France
[2]UMR LEDa [260], IRD, CNRS, Université Paris-Dauphine, DIAL, Paris, France
[3]UNU-WIDER, Helsinki, Finland

Abstract

This paper investigates whether debt relief provided under the Enhanced Heavily Indebted Poor Country (HIPC) and the Multilateral Debt Relief (MDRI) initiatives helped improve primary school enrollment in recipient countries. Combining individual-level data from Demographic and Health Surveys (DHS) and country-level data on debt relief events, we identify children that were potentially impacted by debt relief. We compare their enrollment likelihood with that of children living in non-HIPCs or being too old to have been exposed to debt relief. Our results suggest that debt relief have significantly contributed to increase primary school enrollment. By freeing up additional resources that could be invested in education, debt relief has improved human capital accumulation. This effect is particularly strong for children from poor and rural households, suggesting that debt relief has helped reduce educational inequalities.

Keywords: Debt relief, education, MDGs, financing for development.

JEL codes: O23, D22, D24, O43.

*marine.talance@gmail.com
†ferry@dial.prd.fr
‡miguel@wider.unu.edu

1 Introduction

Over the past twenty years, 36 low-income countries (LICs) have benefited from substantial debt cancellations under the Heavily Indebted Poor Countries (HIPC) initiatives and the Multilateral Debt Relief Initiative (MDRI). The primary objective of the HIPC initiative was to reduce debt down to sustainable levels and hence contain the potential debt overhang that was hampering LICs' economic development. Yet, besides debt sustainability, debt relief also sought to free-up additional resources (especially under the MDRI) to the finance core infrastructures in light of the Millennium Development Goals' achievements (MDGs).

Although debt relief initiatives for developing countries are now more than twenty years old and their effects on recipient countries' development have been quite intensively reviewed, there is still little evidence about the role that such initiatives might have played in the pursue towards the MDGs. This paper aims at filling this gap by investigating the effects of debt relief programs (the Enhanced HIPC initiative and the MDRI) on primary school enrollment which represented one of the several MDGs outlined by the development agenda. To do so, we adopt a multi-level empirical approach by combining micro and macro data. Using Demographic and Health Surveys (DHS) for 44 countries (both HIPCs and non-HIPCs) between 1990 and 2015, we empirically assess the contribution of debt relief to the probability of having been enrolled in primary school (for at least one year) by isolating children cohorts potentially affected by the debt relief initiatives.

Our empirical approach consists in a difference-in-differences model where "treated" individuals are children living in HIPCs and in age of attending school during the post-debt relief period. "Control" units are either children living in the same country and being in age of attending school before debt relief was granted, or children in age of attending school and living in a country which did not benefit from debt relief under the Enhanced HIPC initiative (and therefore under the MDRI as well). Consequently, while the dependent variable is observed at the child level, our variable of interest, the exposition to the Enhanced HIPC initiative, is observed at the cohort-country level. The inclusion of country, cohort and year of survey fixed effects leads to observe the relationship between debt relief and the likelihood of being enrolled in primary school, for a given age-cohort, in a given country, for a given year of survey, and as compared with the primary school enrollment likelihood in control countries. Additional controls also capture, to some extent, the contribution of observed characteristics at the child level as well as the effect of national trends in primary school enrollment among developing countries.

Our results suggest that being in age of attending primary school when the country benefits from debt relief is associated with a higher probability of being enrolled in primary school by around 11 additional percentage points. The positive association between debt relief and primary school enrollment seems to be larger for poor children, and for those living in rural areas. We also find that this relationship is mostly driven by debt relief granted under the Enhanced HIPC initiative (thus highlighting the potential contribution of the conditionality attached to this program) and that the magnitude of the debt relief contribution positively depends upon the amount of debt cancelled. We challenge these findings with multiple robustness checks that all support the main results, hence suggesting that debt relief can be as effective as "more traditional" financing means, turning it into a credible alternative for development financing.

The paper is organized as follows. The rest of the introduction presents the two debt relief initiatives on which this study focuses as well as their expected effects on education. Section 2 describes the data (both micro and macro) and the empirical strategy. Section 3 discusses the results and provides several robustness checks. Section 4 discusses the potential fiscal space channel explaining the main observed effects. Section 5 concludes.

1.1 The multilateral debt relief initiatives

In reaction to the increasing debt burden of low-income countries at the end of the 1980s, large-scale debt relief program for LICs was initiated in 1996 with the Heavily Indebted Poor Countries initiative (HIPC I). The implementation of the very first coordinated debt relief initiative stemmed from the failure of traditional treatments at the Paris Club in restoring debt sustainability (Thugge and Boote, 1997; Daseking and Powell, 1999). Debt distress in LICs was by the time considered as liquidity issues and thus only treated with interest and capital payments rescheduling while the real issue was debtors' solvency, hence calling for significant debt write-offs.

The first version of the HIPC initiative (HIPC I) thus aimed at writing off around 90% of the bilateral debt claims and, for the very first time, cancelled some multilateral liabilities. The program was then enhanced in 1999 (the Enhanced HIPC initiative or HIPC II) since the indebtedness eligibility criteria[1] of the first initiative were considered as too stringent (debt-to-exports ratio superior to 250% in present value) and prevented poor and highly indebted countries to benefit from such cancellations (Thugge and Boote, 1997). The required indebtedness threshold for being eligible under the Enhanced HIPC initiative was lowered down to 150% of the exports (in present value) and the delivery process of debt relief (which is a two-stage process, as shown by figure 1 below) was also sped up.

Insert Figure 1 here

Once considered as eligible for the initiative, the country reaches the "decision-point" where debt service relief starts being granted. These cancellations are nevertheless made conditional to the implementation by the beneficiary government of a Poverty Reduction Strategy Paper (PSRP) identifying mid-term targets regarding development outcomes improvement (health, education, etc.) that are supposed to be financed with the debt service savings stemming from the debt service relief.[2] Once these targets are reached, the country reaches the "completion point" which marks the end of the HIPC process and leads the government to benefit from additional and irrevocable debt relief on its debt stock by an amount determined ex-ante (with a possible topping-up).

In addition to that, in 2005, the G8 summit of Gleneagles decided to accentuate debt relief for LICs with the Multilateral Debt Relief Initiative (MDRI) in order to release financing sources and achieve the MDGs by 2015. They thus agreed to cancel the entire remaining multilateral debt stock of HIPCs that have reached the completion point.[3] These bilateral and multilateral debt cancellations resulted in a strong reduction in external debt varying from 14% to 72% (Table S.A2 in the supplementary appendix), and estimated in overall at nearly 77 billions of USD, in net present value.

[1]The two others eligibility criteria consist in being classified as a LIC by the World Bank, and having implemented an IMF macro-stabilizing program.

[2]Table S.A1 in the supplementary appendix presents the educational targets.

[3]Debt owed to the World Bank, the International Monetary Fund, and the African Development Fund prior to 2003/2004.

1.2 Expected impacts of debt relief on education

The seminal work that granted foundations for debt relief was initiated by Krugman (1988) and Sachs (1989) who developed the "Debt Overhang" theory. They define as a "Debt Overhang", a situation where public debt is so large that it starts to slow down capital accumulation, and therefore the development process of highly indebted countries. Under such circumstances, they argue that it would be beneficial for both the debtor and its creditors to cancel a certain amount of public debt in order to preserve the growth prospects of the debtor, and in fine, its capacity to pay the remaining liabilities. According to Sachs (1989), one of the reason why a massive public debt hampers capital accumulation, posits that when public indebtedness rises up to a significant level, debt repayments become so large that they crowd out public investment and basic needs expenditures. This effect, called the "real burden effect", tends to undermine growth prospects of the debtor through the deterioration of public capital.

Considering the "real burden effect", it becomes rather intuitive to understand how debt relief can affect educational outcomes in beneficiary countries. Large public debt service monopolises the use of public resources and potentially crowds out educational spending, thus weakening public goods provision dedicated to education. Debt cancellations might therefore help in freeing up public money (i.e. generating " fiscal space" as coined by Heller (2005)) initially intended to debt servicing, and in reallocating it to the education sector. In this vein, the Enhanced HIPC initiative aims at enlarging access to primary education (the second Millennium Development Goal). Debt relief under this initiative is coupled with strong conditionality, hence making further debt cancellations conditional to the sound use of debt service savings for social sectors such as education (and primary schooling in particular).

This "fiscal space" effect induced by debt relief has been investigated in few papers. Although a first wave of studies in the early 2000s concludes to no effect stemming from debt relief initiatives (Chauvin and Kraay, 2005; Presbitero, 2009), more recent articles identify a positive contribution of debt relief to public expenditures. Thomas (2006) shows that a decline in debt-service costs significantly raises social expenditures in education and health sectors in low-income countries. Making use of a longer post-debt relief period, Cassimon et al. (2015) suggest that an increase in debt services savings is positively associated with larger current and capital spending. Public investment is found to be more reactive to debt service savings stemming from the Enhanced HIPC initiative rather than those induced by cancellations under the MDRI because of a stronger conditionality. More recently and using a double-difference approach, Djimeu (2018) confirms the positive effect of the Enhanced HIPC initiative on public investment, but mainly for recipient countries having a restricted access to international capital markets.

Yet, have these freed-up resources from debt relief been used to finance public expenditures for education? And did they improve educational outcomes? To our knowledge, only two papers investigate the effects of debt relief on education. First, using a first-difference specification similar to Chauvin and Kraay (2005), Dessy and Vencatachellum (2007) look at the effect of debt relief granted to African countries on education and health expenditures between 1989 and 2003 and find that debt relief is negatively associated with education spending. These results, while rather counter-intuitive, are however not robust to alternative measures of debt forgiveness. Dessy and Vencatachellum (2007) argue that this negative effect reflects a moral hazard-like behavior since once debt relief is granted, benefiting countries are free to reconsider the development strategy they committed to before receiving debt cancellations. Nevertheless, these results could also be explained by their limited study period which does not allow enough years elapsed since debt relief and makes them unable to fully capture debt relief granted under the Enhanced HIPC initiative and the MDRI. Focusing on a more recent period (1998-2005), Cuaresma and Vincelette (2008) investigate this relationship for 33 HIPCs having at least reached their decision point (as compared to less than 10 HIPCs in Dessy and Vencatachellum (2007)). Even though they find no effect on education expenditures or on student-teacher ratios, their results based on propensity score matching methods and Heckman's sample selection estimator, show that countries having reached their completion point record lower primary school dropout rates than interim-period HIPCs.

To our knowledge, the study by Cuaresma and Vincelette (2008) is the only one attempting to assess debt relief effectiveness on educational outcomes. Yet they consider this relationship in a linear way while heterogeneity in debt relief effects might be at play. As explained by Dabla-Norris et al. (2004) with a model predicting the contribution of debt relief to education, the positive impact of debt relief is likely to be more important for poor households facing more stringent constraints in their schooling decisions.

As compared with previous studies, our paper considers a long-enough period of study and takes the analysis of debt relief effectiveness down to the individual probability of being enrolled in primary school. In the same vein as Djimeu (2018), we exploit variability in decision-point attainment over time and across countries as well as in individual characteristics (some of them being relatively exogenous such as gender and year of birth) to identify average and heterogeneous effects of debt relief on primary school enrollment.

2 Data and Model

2.1 Data

Sample of study

We make use of Demographic and Health Surveys (DHS) which consist in nationally-representative household surveys standardized across country and over time, hence allowing cross- and within-country comparisons. For the purpose of this study, we collect DHS in all available developing countries between 1990 and 2015, both HIPCs and non-HIPCs, hence ending up with a large set of repeated cross-sectional data. We restrict the sample to HIPCs that have at least one DHS before and after the year they reached their decision point. For non-HIPCs, we only consider countries with at least two rounds of DHS; one before 2000 and one after.[4] Figures S.A1 and S.A2 in the supplementary appendix present the evolution of the database over time.

As we assess the impact of debt relief on primary school enrollment, we focus on children that are in age for primary schooling at the time of the survey. We therefore use UNESCO data on the official entrance age to primary school and the theoretical duration of primary schooling in each country in order to identify these children. Children kept in the final sample are on average between 6 and 12 years old. Primary school enrollment is measured at the extensive margin and consists in a dummy variable equal to 1 if the child attended primary school for at least one

[4] As most of the HIPCs reached their decision point that year.

year while being in age for primary school, and zero otherwise. Because in some countries, two consecutive rounds of surveys were conducted in a short period of time, the same individuals could appear twice in our data. In order to avoid this, we compute year-of-birth thresholds that prevent the same age-cohorts to appear twice in two consecutive DHS conducted in a given country.[5]

The final sample contains 44 countries (22 HIPCs and 22 non-HIPCs) and 177 DHS. It gathers information on 1,704,762 individuals eligible for primary schooling including 537,501 individuals who have potentially been affected by the HIPC Initiative. 80% of the sample children have ever attended primary school (for at least one year). When parental education is imposed as control, the overall sample is significantly reduced, down to 648,962 individuals. Tables S.A5 and S.A6 in the supplementary appendix describe the sample and the surveys used for non-HIPCs and HIPC, respectively.

Micro-level determinants of primary school enrollment

Several controls at the individual level are added to isolate the impact of debt relief programs. They are factors that usually affect the probability of enrollment in primary school : parents' education, wealth, child's gender, age, relationship to head of household and geographical location. Table S.A7 in the supplementary appendix presents the main descriptive statistics. We control for parents' education as it is expected to positively affect schooling participation (Buchmann and Brakewood, 2000; Colclough et al., 2000; Glick and Sahn, 2000; Huisman and Smits, 2009; Lincove, 2015). In our sample, 43% of mothers and 34% of fathers have never been to school. As suggested by the human capital theory (Becker, 1964; Mincer, 1958), children from wealthier households are more likely to be enrolled in school because costs associated with schooling are less likely to be an obstacle for better-off households (Glick and Sahn, 2000; Huisman and Smits, 2009; Lincove, 2015). They also tend to be less affected by credit constraints and imperfect credit markets which, in developing countries, are severe obstacles to school participation (Edmonds,

[5]For instance, for Nigeria, one survey is available for 2008 and another one for 2013. Given our selection strategy, we should therefore keep all the individuals aged between 6 and 12 years at the time of the survey, that are children born between 1996 and 2002 and those born between 2001 and 2007 for the first and second surveys, respectively. However, individuals born in 2001 and 2002 could have been surveyed in both rounds. To avoid overlapping, we therefore restrict the sample for the first survey to children born between 1996 and 2000. Tables S.A3 and S.A4 in the supplementary appendix discuss the potential bias implied by this selection process.

2006; Ersado, 2005; Huisman and Smits, 2009). We use a Principal Component Analysis (PCA) to compute a wealth index (see Filmer and Pritchett (2001)) derived from seven household asset indicators and define wealth quintiles (at the country-survey year level).[6] Because older children have higher labor opportunities or contribute more to household chores, younger children are expected to have a higher probability of being in school (Huisman and Smits, 2009; Lincove, 2015). Consequently, we control for age by adding year-of-birth fixed effects (sample children are aroung 10 years old). We also control for child's gender (half of sample children are girls) since parents may prefer to invest their limited resources in their sons' education because of girls' lower future labor opportunities. Relationship to the head of household has also been proven to explain enrollment. Indeed, parents are more likely to rely on adopted or fostered children instead of their biological children to help in domestic chores or to contribute to the household income (Fafchamps and Wahba, 2006). Being the child of the head of the household is therefore expected to positively impact schooling participation (Huisman and Smits, 2009). We also expect children living in rural areas to be less likely to attend school due to lower educational supply or lower future labor opportunities (Fafchamps and Wahba, 2006; Huisman and Smits, 2009).

Macro-level determinants of primary school enrollment

Although most of the determinants of primary school enrollment take place at the household level, country-level factors can also affect school attendance. Moreover, since our strategy aims at comparing cohorts observed at different periods in several countries, it is necessary to account for potential changes in the economic, demographic and institutional environment that might affect the probability of being enrolled in primary school. Consequently, we first control for per capita GDP (expressed in 2010 constant USD and in logarithm) since we expect a higher probability of primary school enrollment for children in richer countries, as emphasized by the aid effectiveness literature (Michaelowa and Weber, 2006, 2008; Dreher et al., 2008; d'Aiglepierre and Wagner, 2013). We then consider the under 15 population (expressed in logarithm) in order to control for the contribution of demographic pressures to primary school attendance (Dreher et al., 2008; d'Aiglepierre and Wagner, 2013).

[6] See Figure S.A3 and Table S.A8 in the supplementary appendix for more details.

Lastly, we decide to impose the level of public expenditures dedicated to the education sector (in percentage of GNI) as a control. Yet, education spending is likely to capture the potential effect of our variable of interest since exposure to debt relief period should be associated with larger expenditures for education as regards the "real burden effect" superimposed with the strong conditionality of the HIPC initiative. Public spending for education can thus be thought as a "bad control". However, extra public funds stemming from debt relief do not essentially go to education sector. Theoretically, debt service savings should be allocated to social sectors such as health and education. But in practice and as regards the variety of MDGs to achieve, only a fraction of the overall resources freed-up was intended to primary and secondary schooling. Lastly, changes in public spending are not entirely determined by debt relief and depend on a wide range of financing sources (domestic or external).[7]

2.2 Empirical Specification

In order to assess the effect of debt relief provided under the Enhanced HIPC initiative on the probability of having been enrolled (for at least one year) in primary school, we make use of a difference-in-differences strategy which consists in estimating the effect of living in a HIPC and being in age of primary schooling during the post-decision point period on primary school enrollment. A first naive specification could be:

$$PS\_ENROLL_{i,a,c,j} = \alpha + \beta POST\_DP_{a,c,j} + \epsilon_{i,a,c,j} \tag{1}$$

where $PS\_ENROLL_{i,a,c,j}$ is the dummy variable equal to one if the child i of age-cohort a living in country c, and observed in the survey year j has been enrolled in primary school for at least one year (zero otherwise). $\epsilon_{i,a,c,j}$ is the classic idiosyncratic disturbance term.

Our variable of interest, $POST\_DP_{a,c,j}$ is a dummy variable identifying cohorts of children in age of being at primary school, living in a country c that benefited from debt relief, and observed in survey-year j conducted after the country c reached its decision point. To be considered as treated, children must therefore respect three conditions: (1) they live in a HIPC, (2) they are

[7] All country-level variables are retrieved from the *World Development Indicator (WDI)* database.

still in age of attending primary school when their country reaches the decision point, (3) they are observed after the country reached this stage of the debt relief process.[8] Conversely, this dummy variable is equal to zero for all children observed in non-HIPCs (control countries) and for children in benefiting countries but who were too old to be enrolled in primary school when the country benefited from debt relief under the Enhanced HIPC initiative (and those who were in the eligible age-cohort but observed prior to the decision point year). Under this specification, β thus represents the unconditional difference in the probability of being enrolled in primary school (at least one year) between "treated" and "control" children.

However, the repeated cross-sectional nature of our data imposes to control for a large range of factors that might affect the likelihood of primary school attendance across countries and over time. In order to capture this conditional variation in primary school enrollment and control for time-invariant country-specific characteristics, we therefore augment our specification with country fixed effects (δ_c). They thus control for structural features at the country-level that might explain differences in primary schooling performance as well as time-invariant factors influencing the participation to the Enhanced HIPC initiatives (such as public debt and income levels prior to 1996 or having benefited from the initial version of the HIPC initiative, as emphasized by Djimeu (2018)). We then also impose controls for potential cohort-related events common to all developing countries that might affect primary school enrollment using year-of-birth fixed effects (i.e. age-cohort fixed effects) (η_a). In addition, since we pooled data from multiple DHS rounds, we also account for potential differences in question forms by including survey-year fixed effects (ρ_j).[9] Lastly, as discussed in the previous section, we augment our model with a set of individual-household characteristics ($H_{i,a,c,j}$) and time-varying macroeconomic covariates ($Z_{c,j}$). The refined

[8] Table S.A9 in the supplementary appendix presents the minimum year of birth required for each HIPC country to be considered as treated and the date when the country reached the decision point. For instance, to be considered as treated, Beninese children must be born in 1988 (or later) and must be observed in 2000 (or later).

[9] Note that given the structure of the repeated cross section data, imposing country×survey-year fixed effects or country×cohorts fixed effects would confound the effect of the debt relief initiative since $POST\_DP_{a,c,j}$ is observed at the country-cohorts-survey-year level; cohorts and survey years being, by construction, closely related. Younger children —the more likely to benefit from the HIPC initiative— are observed in the most recent surveys.

version of our model takes the following form:

$$PS\_ENROLL_{i,a,c,j} = \alpha + \beta POST\_DP_{a,c,j} + \delta_c + \eta_a + \rho_j + \phi H_{i,a,c,j} + \gamma Z_{c,j} + \epsilon_{i,a,c,j} \quad (2)$$

The β coefficient represents the contribution of having been granted debt relief under the HIPC initiative on the within-country probability of having attended primary school, as compared to what happens in control countries (hence controlling for generational effects, individual characteristics and changes in the macroeconomic environment). In other words, β can be defined as the difference-in-differences (DID) coefficient assessing the debt relief effect on primary school attendance which, in what follows, is estimated using OLS estimators with DHS sampling weights. Yet, before running DID estimates, one needs to ensure that the outcome variable did not already diverge between HIPCs and non-HIPCs before they start receiving debt relief i.e. before their decision point date. To do so, we apply basic parallel trends tests using various specifications. A special section in the supplementary appendix discusses the parallel trend assumptions (Table S.A10). Tests conducted all support the existence of an ex-ante common trend regarding primary school enrollment between HIPCs and non-HIPCs' children, hence allowing us to grant enough confidence to the β in equation 2 as the potential effect of debt relief on primary school enrollment.

3 Results and robustness checks

3.1 Baseline regressions

Table 1 reports the main results of equations (1) and (2). Column (1) shows the unconditional effect of being in age of attending primary school during the post-decision period of an HIPC on the probability of having actually been enrolled in primary school (β of equation (1)). Without imposing any controls but fixed effects, one can see that having been exposed to the HIPC initiative significantly increases the likelihood of having attended primary school by 11 additional percentage points. This result is observed over our full sample of 1,548,492 individuals of which 535,749 are considered as "treated".

Column (2) presents results when we augment our specification with individual characteristics.

As expected, girls are significantly less likely to attend primary school. Not surprisingly and in line with Buchmann and Brakewood (2000); Colclough et al. (2000); Glick and Sahn (2000); Huisman and Smits (2009); Lincove (2015), children with educated parents are more likely to be enrolled in primary school. In keeping with Huisman and Smits (2009), this probability also seems to be higher for household head's children. Chances of attending primary school are found to reduce when children live in rural areas which can be explained by the difficulty of reaching educational facilities when people are living in isolated places (Fafchamps and Wahba, 2006; Huisman and Smits, 2009). In line with Glick and Sahn (2000); Huisman and Smits (2009); Lincove (2015), this negative effect is attenuated when the household is relatively rich and can provide means to the children for reaching distant schools (such as motorcycle or bicycle). The positive contribution of our aggregated wealth index indeed suggests that children living in wealthier households are more likely of having one school nearby or easily reachable and thus being enrolled in primary school. Overall, controlling for individual characteristics significantly improves the explanatory power of our model (with an increase by around 10 percentage points in the R^2). Yet, since household characteristics are not reported for every household interviewed, the sample size drastically reduces down to 648,962 individuals spread across 40 countries, of which 289,971 are defined as "treated". Moving to our variable of interest, one can see that conditionally to these individual features, being exposed to the Enhanced HIPC initiative still has a pretty large and positive effect on primary school attendance since it contributes to rise the probability of being enrolled by more than 17 percentage points.

Column (3) reports approximately the same estimate results except that our wealth index is now disaggregated in quintiles specific to each country and survey-year (the reference being the fifth quintile). Not surprisingly, we note that children living in poorer households have fewer chances to be enrolled in primary school as compared with those belonging to the richest quintile. The gap gradually reduces as we get closer to the fifth quintile, suggesting that the likelihood of attending primary school is a linear function of the household's income. Yet this change does not alter the significant effect of debt relief which remains around 16 additional percentage points.

We then add country-level variables to our specification to make sure that the observed debt relief effect on primary school does not reflect the contribution of another time-varying country-specific development occurring at the same period. Including per capita GDP and under 15 population in column (4) does not affect the significance of the HIPC initiative but reduces the size of the coefficient down to 12 additional percentage points. Controlling for education public expenditures (column (5)) slightly reduces the "HIPC treatment" coefficient (by around 0.5 percentage point). This could mean that the effect of debt relief on primary school attendance partly goes through additional public spending, highlighting fiscal space as one of the mechanisms at play in this relationship.

Lastly, in column (6), we replace our variable of interest, the "$POST\_DP$", by an interactive term between this variable and the duration of exposition to debt relief. A child having one year to go before completing primary school when the country enters the HIPC initiative should be less affected by debt relief than a child who, at the same period, has still many years ahead for attending primary school. Indeed, not only is the latter exposed to the program for a longer period of time, but many educational investments (school construction, etc.) can take time to be effective. The duration of exposure varies by individual and country, since the official age for leaving primary school is country-specific. In addition, HIPCs did not meet their decision point the same year thus leading to define a HIPCs-specific minimum year of birth for which individual had still a chance of being enrolled one year in primary school during the debt relief period (before being older than the official leaving age). The "$POST\_DP\times$ Duration of exposure" variable represents a "continuous HIPC treatment" where we assume a linear effect of being exposed one more year to debt relief. Results in column (7) show that the effect of being "exposed" one more year to the post-decision point period (regardless to the years exposed left) is positive and statistically significant with however, a slight marginal effect. One additional year of exposure to the debt relief initiative indeed leads to an increase in the probability of attending primary school by only 1.3 percentage points.

Insert Table 1 here

3.2 Non-linearities in individual characteristics

One main advantage of our empirical strategy compared to previous studies is the individual level of observation and the resulting ability of investigating potential heterogeneous effects with respect to individual characteristics such as gender, urban versus rural region of residence, and wealth.

The Enhanced HIPC initiative and the MDRI set within the prior agenda for development which aimed at reaching the Millennium Development Goals by 2015 (now coined SDGs, which encompasses more targets to reach by 2030). Within this framework, and besides the simple universal primary schooling target, countries were also urged to reduce income, gender and regional gaps in terms of education. Gender-specific targets were defined within the poverty reducing plan that HIPCs had to conduct during their interim period (Table S.A1 in supplementary appendix). For instance, Bolivia committed to increase the number of girls completing the 5th grade in rural areas. Debt relief was thus expected to benefit disproportionately to girls in recipient countries. In the same way, rural children were also expected to benefit more from debt relief outcomes than urban children for whom school services remain less scarce (to a certain extent). Lastly, and given that richer people have (in overall) greater opportunities in finding alternatives to poor public education services, one could also expect debt relief initiatives to have more impact on children living in poorer households.

In order to test these potential non-linearities in debt relief effects, we run both sub-samples regressions as well as interaction terms models. The interest of interaction terms models lies in the ability of allowing various explanatory variables to affect individuals in a different manner with respect to their gender, their living area or their ranking in the wealth distribution. Formally the interacted model takes the following form:

$$PS\_ENROLL_{i,a,c,j} = \alpha + \beta_1 POST\_DP_{a,c,j} + \beta_2 GIRL_i + \beta_3 GIRL_i \times POST\_DP_{a,c,j} +$$
$$\delta_c + \eta_a + \rho_j + \phi H_{i,a,c,j} + \gamma Z_{c,j} + \sum_{k=\{\delta,\eta,\rho,H,Z\}} \beta_k (GIRL_i \times k) + \epsilon_{i,a,c,j} \qquad (3)$$

where the last component of the equation (right before the error terms) denotes the interaction terms between individual heterogeneity (here gender with $GIRL_{i,a,c,j}$, but alternately rural area dummy variable or wealth quintiles) and various explanatory variables (fixed effects included). Table 2 reports the results by gender and region. Columns 1, 2, 5 and 6 present the findings for sub-samples and columns 3, 4, 7 and 8 estimates with interaction terms models. While we observe no significant differences between boys and girls (when imposing controls for the under 15 female population), the HIPC Initiative appears to have affected mainly rural children. This is consistent with the commitments made by many HIPC countries (building schools and classrooms in rural and remote areas; see Table S.A1 in supplementary appendix).Table 3 reports results by quintile of wealth. Even though being exposed to the HIPC Initiative increases significantly the odds of attending primary school for all individuals, the effect of the reform appears to be more pronounced for poorer households.

Taking the identification strategy one step further, we then re-run the interaction terms models changing the fixed effects composition. We replace country (δ_c) and survey-year (ρ_j) fixed effects by country $\times$ survey-year ($\mu_{c,j}$) fixed effects in order to control for time-variant factors at the country-level that might affect primary school attendance. Imposing country $\times$ survey-year fixed effects conducts to assess the effect of debt relief on primary school enrollment for a given survey-year in a given country. Yet, since most of the HIPCs have been granted full debt relief after 2006, these fixed effects no longer allows to differentiate treated from control individuals in most recent HIPCs' DHS. Indeed, every children in age of attending primary school, leaving in HIPCs and surveyed in last DHS's rounds are potentially affected by debt relief (regardless their gender, living areas, or household's wealth), and all defined as "treated".

In order to not confound the effect of debt relief with country $\times$ survey-year fixed effects, we need to keep within country-survey year variation in the debt relief treatment (especially for most recent DHS's rounds). This leads us to consider the duration of exposure to debt relief as our treatment variable (rather than the basic $POST\_DP_{a,c,j}$ dummy variable). The duration of exposure to debt relief indeed varies at the individual level with respect to the child's year of birth and the year of survey. Results are reported in Table S.A11 in supplementary appendix and confirm previous findings based on interaction terms models. Children in rural areas as well as those located at the bottom of the wealth distribution tend to benefit more from debt relief.

Overall, these additional findings suggest that international debt relief has helped reducing regional and economic educational inequalities, contributing (partly) to reach some of the MDGs.

Insert Table 2 here

Insert Table 3 here

3.3 Robustness checks

Concurrent large-scale programs for education and traditional financing flows

Results reported so far suggest that the positive contribution of debt relief to primary school attendance is robust to the inclusion of macroeconomic determinants identified as key drivers of school enrollment. Yet, one might doubt that the coefficient associated with the $POST\_DP$ does not fully reflect the contribution of debt relief to primary school enrollment. Indeed, the post-decision period covers a period where large-scale aid programs for primary education were launched in some countries of our sample (both HIPCs and non-HIPCs). Consequently, one should be cautious before granting unquestioningly the effect of the $POST\_DP$ variable on primary school attendance to the sole debt relief initiatives.

In order to control for the contribution of other education programs, we first add to our set of country-level covariates the amount of aid received. Using the Development Assistance Committee (DAC) database, we retrieve net official development assistance (ODA) disbursements (in percentage of GDP) for each country. Moreover, since most of received aid ends up in public budget, we intentionally omit public expenditures dedicated to education from our specification when controlling for ODA. Results in column (1) of Table 4 report the baseline results. Column (2) then shows that, while larger amounts of received net ODA (as a share of recipient country's GDP) are associated with higher primary school enrollment, its inclusion as control does not alter the coefficient associated with debt relief exposure. If anything, the coefficient associated with debt relief increases after controlling for aid, suggesting that both variables are negatively correlated, as suggested by Powell and Bird (2010).[10]

[10]Having both government spending and net ODA in the same specification does not change the results

Yet, since not all aid disbursements go to education, one could suggest foreign aid to education as a more relevant control. But due to data availability, this strategy would lead to considerably reduce the size of our sample, since sectoral aid data from the *Creditor Reporting System* (CRS) only cover years from 1995 onwards, while data for disbursements start in 2002. Although less exhaustive, Figure S.A4 in the supplementary appendix suggests that this measure of aid to education sector is strongly correlated with the aggregate net ODA supporting the latter as a good proxy for other external support to education.[11]

We then consider the concurrent effect of the *Global Partnership for Education* (GPE) — one of the most important education program (financed by international financial institutions) of the past decades — which could blur the effect of debt relief if not accounted for. As control for the presence of large-scale education program such as GPE, only countries having joined the GPE are kept in the control group. The "treatment" group still comprises all the HIPCs (both those which have benefited from GPE resources and those which did not).[12] Results are reported in columns (3) and (4) of Table 4 (where we alternately control for public education spending and ODA received). The effect of debt relief is still positive and significant without encountering any loss in terms of magnitude.[13]

In columns (5) and (6), we consider the entire sample and add a control for participation in the GPE program. Results underline that being exposed to international debt relief still leads to a positive effect on primary school attendance while exposition to the GPE has no significant impact.[14] This might be explained by the amounts of debt cancelled which resulted in significantly larger funds for HIPCs than those provided under the GPE (although only a share of the money freed up by the HIPC initiative was dedicated to primary education).[15]

---

neither (results available on demand).

[11]Note that replacing net ODA by the measure of aid to education provided by the CRS (see Table S.A12 in the supplementary appendix) does not alter the effect of the HIPC initiative on primary school enrollment, despite a significant sample cut.

[12]See Table S.A13 in the supplementary appendix.

[13]Note that reducing the pool of control group countries based on their participation to GPE leads to drop around 25% of our observations as compared with baseline estimates.

[14]When excluding the debt relief treatment, the coefficient associated with the GPE program remains not significant (results available on demand).

[15]Debt relief provided under the Enhanced HIPC initiative and the MDRI amounted to 77bn of USD as compared to 2.5bn granted under the overall GPE (Table S.A13).

Insert Table 4 here

Sample dependence

We then test the sensitivity of our results to the sample composition. We first re-run our baseline estimate but dropping each country from the sample (both HIPCs and non-HIPCs) one by one. This leads to 40 estimates for which coefficients of our variable of interest are reported in Table S.A15 in the supplementary appendix. We observe that, while the number of observations substantially differs with respect to the country excluded, coefficients debt relief remains positively and significantly associated with primary school enrollment, suggesting that baseline results are not driven by outlying countries from the treatment or control group.

We then repeat the exercise but this time dropping all children from control countries belonging to the same geographical region to ensure that results are not driven by regional trends. Results reported in Table S.A16 in supplementary appendix support that the geographical localization of control individuals does not alter our results. Column (7) of the Table S.A16 lastly reports results when the control sample is limited to children living in non-HIPCs which recorded relatively large public debt ratios prior 1996 (the year the original HIPC initiative was disclosed). Considering only their level of indebtedness (regardless their income classification at that time), most of these countries might have been eligible for the Enhanced HIPC initiative and therefore constitute a better control group at the country level than the entire sample of non-HIPCs. One can see that considering only individuals from heavily indebted countries (HICs) as control units, leads to a slight decrease in the magnitude of the debt relief effect on primary school attendance, albeit it could also be due to the loss of observations.

Lastly, we include individuals who were initially dropped to avoid overlapping. Results remain unaltered (Table S.A17 in supplementary appendix).

Heterogeneity by initial level of education

Countries that benefited from the HIPC initiative had significantly lower enrollment rates before the program began. The average gross primary enrollment rate in 1999 was 80% in HIPC countries and 105% in non-HIPC countries. HIPCs thus had larger room for improvement in terms of primary school enrollment with respect to control group countries. This could partly explain the positive effect found. Even more worrying regarding our empirical strategy, the average increase observed in primary school enrollment in the post-decision point period might also reflect a catching up process among HIPCs, which would have taken place anyway (i.e. even in the absence of debt relief). To account for initial differences in terms of primary school enrollment for both HIPC and non-HIPC countries, we interact our treatment variable ($POST\_DP$) with initial level of education (Table S.A18 in the supplementary appendix). The pre-HIPC level of education is computed using two different methods. First, we use the gross primary enrollment rate in 1999 provided by the World Bank (columns (1) and (2)). Second, DHS are used to compute net primary enrollment rates before 2000 (columns (3) and (4)). The problem with this second method is that all countries were not surveyed in 1999. We therefore use, for each country, the closest survey to 1999. To avoid considering surveys that are too old, surveys before 1996 are excluded, which leads to reduce the sample by 26%. Results confirm that the program had a higher impact on countries that were initially lagging behind in terms of primary school enrollment.

Given these differences in initial level of education, we reduce the control sample to include only non-HIPC countries that were below a certain threshold before the initiative was launched. Even though the coefficient associated with the treatment variable slightly decreases, the main results still hold (Table S.A19 in the supplementary appendix).[16]

Educational trends in developing countries

Lastly, we extend our baseline model with time trends computed at different levels. Within our sample, HIPCs and control group countries could have experienced different development patterns in terms of education. Therefore, the effect identified by our difference-in-differences specification could just capture a temporal trend in education performances, different for HIPCs and non-HIPCs, which should not be granted to the debt relief initiatives. In order to account for potential time trends effects, we first augment the baseline specification with HIPCs-specific year-of-birth trends (columns (1) to (4) in Table S.A20 in the supplementary appendix). We then add country-specific year-of-birth linear trends (columns (5) and (8)) and its quadratic term

[16] Results are not altered when using the net enrollment rates from DHS even though the sample is significantly reduced.

(columns (9) to (12)). Adding these specific time trends does not alter the results: the coefficient associated with our $POST\_DP$ variable remains significant and of the same magnitude (even when interacted with the duration of exposure to debt relief).

4 Debt relief effectiveness: investigating fiscal space heterogeneity

Results suggest that debt relief initiatives have helped improve primary education enrollment, at the extensive margin. In what follows, we investigate whether the effect of debt relief on enrolment is heterogeneous and depends on the fiscal space that recipient countries have benefited from.

Sections 2 and 3 expose the functioning of the debt relief initiatives under review and the theoretical impacts they might have had on education in beneficiary countries. Two complementary theoretical concepts provide potential explanations regarding debt relief effectiveness. The "real burden" effect stemming from the debt overhang theory (Krugman, 1988; Sachs, 1989) and the resulting "fiscal space" put forward by Heller (2005). According to the first one, substantial debt service hampers the financing of core infrastructure in debtor countries. Therefore, once relieved from debt servicing, beneficiary states could use public resources to invest in development-oriented sectors. According to Heller (2005), which closely follows Krugman and Sachs' arguments, debt cancellations would result in additional cash-flows, available for public spending (only in the situation where debt relief is additional). Indeed, since the debtor should have put money aside dedicated to debt service payments, the public savings resulting from debt service relief could now be used for another purpose, such as development expenditures.

4.1 Public spending for education

Consequently, countries that have received larger debt relief, and obtained significant freed up resources, are expected to invest more in education and experience better results in terms of primary school enrollment. In order to capture such public finance channel, we first augment our initial specification (2) with an interaction term between the dummy variable flagging "treated" children (the $POST\_DP$ variable) and public expenditures dedicated to the education sector. Results of column (1) in Table 5 suggest that having been exposed to debt relief is associated with a larger probability of being enrolled in primary schooling and that this effect is reinforced when the home country of the child has experienced an increase in public spending to education over the same period.

4.2 Debt service savings from debt relief

Yet public spending to education have probably been financed by many other means than debt relief such as official development assistance, domestic resources, or non-concessional lending. We thus suggest using a more accurate proxy of the cash-flows resulting from debt relief that might have helped financing education in beneficiary countries. Building on the fiscal space theory and previous empirical studies by Cassimon and Campenhout (2008); Cassimon et al. (2015), we compute the debt service savings stemming from debt relief. This variable represents the gap between what would have been the debt service of a debtor country without debt relief, and the actual debt service after debt relief. Using multiple Decision Point Documents from the IMF and the International Development Association (IDA), we retrieved debt service before and after both the HIPC initiative and the MDRI. This led us to compute debt service savings from debt relief stemming from the HIPC initiative and from the MDRI, as well as the aggregate cash-flows resulting from these two debt relief programs (Figure 2).

Insert Figure 2 here

We interact these cash-flows with our "treatment" variable. Results of column (2) suggest that HIPCs that experienced more debt service savings from debt relief (both from the Enhanced HIPC initiative and the MDRI, in percentage of GDP) are also those that recorded the largest improvements in terms of primary school enrollment.[17] The effect is pretty important with an

[17]Since such measure is only calculable for HIPCs and is equal to zero for non-HIPCs, this amounts to replacing our dichotomous treatment variable ($POST\_DP$) with a continuous treatment. This is why columns (2) to (7) in Table 5 do not display coefficient for $POST\_DP$.

additional debt service saving of one percentage point of GDP being associated with an increase in likelihood of attending primary school by around 12 additional percentage points. Yet, the average of debt service savings for a given year is well inferior to one percentage point of GDP.

Column (3) then displays results when debt service savings are separated by debt relief initiatives. They suggest that the positive correlation between debt service savings and primary school enrollment is mostly driven by cash-flows resulting from debt cancellations provided under the Enhanced HIPC initiative. This result is not surprising since most of investment in primary education occurred during the interim period, when debt service relief was granted providing a sound use of resulting cash-flows in targeted sectors, such as health and education. Yet, one might argue that this effect is therefore only conditionality-driven. But if conditionality was the only channel explaining the effect of debt relief on education, it should be regardless of the amount of debt service savings (and the coefficient should not be significant).

Insert Table 5 here

These several measures aim at capturing the savings in terms of debt service that directly results from debt relief. Yet, one needs to be cautious with the interpretation of results using such variable. Indeed, as emphasized by Cohen (2001), debt service savings are based on the hypothetical debt service in absence of debt relief. However, it is impossible to claim that this hypothetical debt service is what the debtor country would have paid in absence of debt relief. It is most likely that bad payers (or HIPCs that accumulated large amounts of interest and capital arrears prior to the HIPC initiative) would not have fully paid their debt service in absence of debt relief. Debt service savings only result in additional cash-flows if the debtor would have honored its debt in absence of debt relief. In order to account for such condition, we interact the continuous treatment with a dummy variable flagging bad and good payers among HIPCs. Following Cassimon et al. (2015), bad payers are defined as HIPCs that recorded debt service arrears (interest and capital) superior to 10% of their total debt stock prior to 1996 and the announcement of the original HIPC initiative. Good payers are HIPCs with a ratio of debt service arrears to total debt stock inferior to 10% over the same period.

Results from columns (4) to (7) in Table 5 report the estimated effects of debt service savings conditional to the HIPCs' debtor history (good versus bad payers). In accordance with the idea raised by Cohen (2001), results suggest that debt service savings have benefited solely to HIPCs that were more likely to reimburse their debt in absence of debt relief (columns (4) and (5)). This finding is also supported by results from columns (6) and (7) where the debt service savings interacted with the bad payers dummy is significantly negative (especially regarding debt service savings stemming from the MDRI) while the same variable not interacted (and thus capturing the effect for the reference i.e. good payers) is positive and significant. These results remain robust to alternative denomination of debt service savings (cf. Table S.A21 in the supplementary appendix where debt service savings are measured in USD (log), not in percentage of GDP). They also support the heterogeneous effect of debt relief on primary schooling with respect to the living area and the position of children in the income distribution.[18]

5 Conclusion

Using a dual approach (both micro and macro), this paper investigates the effect of debt relief on primary school attendance. Exploiting the variability in debt relief events and in individuals' year of birth, we apply a difference-in-differences methodology including multiple fixed effects as well as primary school enrollment determinants (both at the individual and country-level) which helps us to isolate factors affecting primary school attendance or confounding the effect of international debt relief. This empirical strategy leads to appreciate the contribution of debt relief provided under the HIPC initiative and the MDRI on the likelihood of being enrolled in primary school, as compared to a situation where debt relief would not have been granted. We then conduct a battery of robustness checks to ensure that our main findings are not subject to sample dependence, are not biased by educational trends nor do they reflect the structurally lower education level of benefiting countries.

Our results support that debt relief provided under the Enhanced HIPC initiative (onwards) have contributed to tend towards universal primary school. Indeed, results suggest that being in

[18]Results of section 3.2 where the dichotomous "treatment" variable is replaced with the "continuous treatment" reflecting the debt service savings from debt relief are available upon request to the author.

age of attending primary school in a country which is granted debt relief improves the probability of being enrolled by around 11 percentage points, as compared with a situation where the home country of the children does not receive any debt relief. Additional findings suggest that the probability of being enrolled is larger for poorer children and for those living in rural areas. Debt relief therefore has therefore contributed to reducing geographical and income educational inequalities. Lastly, results also suggest that debt relief helped improve human capital accumulation because it freed additional resources (debt service savings) that could have been invested in education. The effect seems to be reinforced for countries having been granted significant debt service savings and recording sound public institutions (proxied by debtor credit history).

Overall, our findings support that debt relief, for countries that did not benefit from it yet, might be considered as an interesting mean to finance infrastructures intended to improve human capital in LICs (at the extensive margin). The effect of debt relief on primary schooling at the intensive margin is not investigated in this paper but would consist in an interesting research avenue as a complementary work of the present study. Given the MDGs have not been entirely satisfied in 2015, there still is much to do, especially regarding primary school. Consequently, in light of our results, traditional external financial supports for low-income countries might be reconsidered and could be contemplated as a mixture of diverse financing flows, of which debt relief should not be moved aside.

Figures and Tables

Figure 1: Debt relief initiatives for LICs

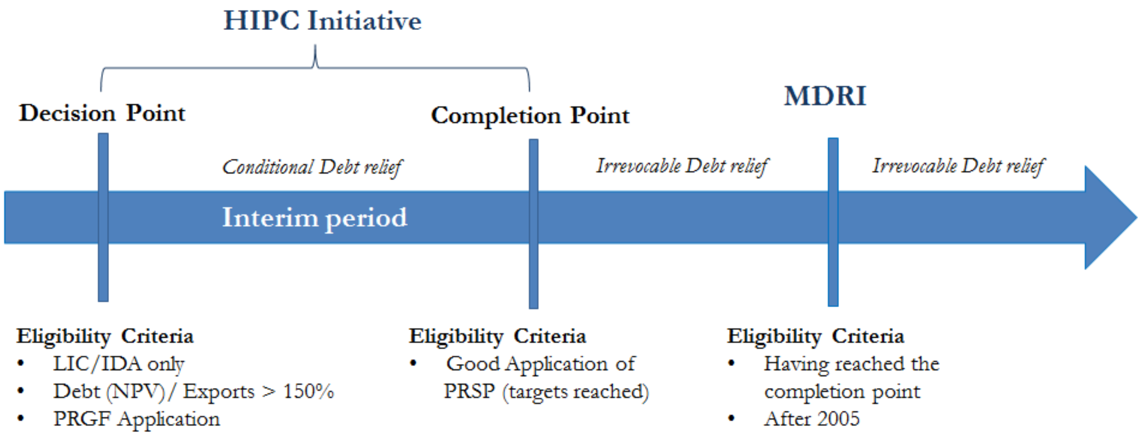


Figure 2: Debt service savings from debt relief initiatives

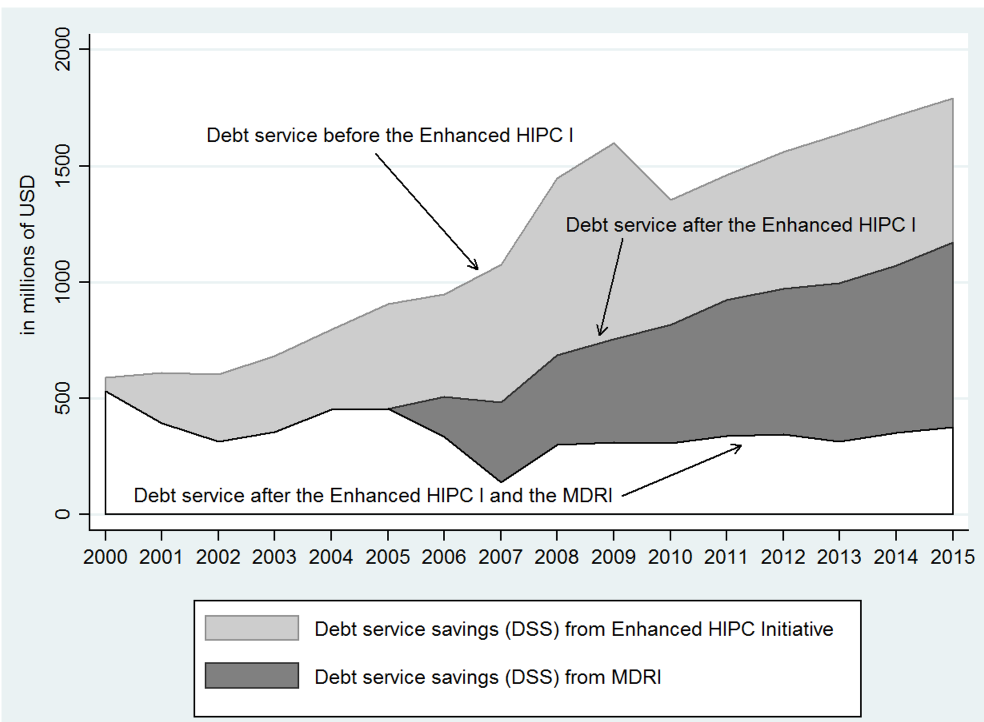


Table 1: Baseline Regressions

Estimators: LPM	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.:	Primary School Attendance (at least 1 year)					
POST_DP	0.110***	0.172***	0.160***	0.118***	0.112***	
	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)	
POST_DP × Duration of exposure						0.013***
						(0.00)
Girl		-0.026***	-0.026***	-0.026***	-0.026***	-0.026***
		(0.00)	(0.01)	(0.00)	(0.00)	(0.00)
Mother Educ: Primary		0.099***	0.097***	0.097***	0.097***	0.097***
		(0.00)	(0.01)	(0.00)	(0.00)	(0.00)
Mother Educ: Secondary or Tertiary		0.073***	0.077***	0.078***	0.079***	0.079***
		(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Father Educ: Primary		0.158***	0.155***	0.156***	0.149***	0.150***
		(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Father Educ: Secondary or Tertiary		0.169***	0.169***	0.169***	0.163***	0.163***
		(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Head's Child		0.013***	0.013	0.012***	0.011***	0.012***
		(0.00)	(0.01)	(0.00)	(0.00)	(0.00)
Rural		-0.053***	-0.060***	-0.060***	-0.059***	-0.059***
		(0.00)	(0.02)	(0.00)	(0.00)	(0.00)
Wealth index		0.035***				
		(0.00)				
1st Wealth Quintile (Q1)			-0.126***	-0.126***	-0.123***	-0.124***
			(0.01)	(0.00)	(0.00)	(0.00)
2nd Wealth Quintile (Q2)			-0.088***	-0.088***	-0.088***	-0.088***
			(0.01)	(0.00)	(0.00)	(0.00)
3rd Wealth Quintile (Q3)			-0.067***	-0.067***	-0.068***	-0.068***
			(0.01)	(0.00)	(0.00)	(0.00)
4th Wealth Quintile (Q4)			-0.034***	-0.034***	-0.034***	-0.034***
			(0.01)	(0.00)	(0.00)	(0.00)
GDP per capita (log, constant USD)				-0.004	-0.001	0.003
				(0.03)	(0.04)	(0.04)
Population under 15 (log)				0.235***	0.229**	0.342***
				(0.09)	(0.09)	(0.09)
Gov. Educ. Spending (% GNI)					0.002**	0.003**
					(0.00)	(0.00)
Observations	1,548,492	648,962	648,962	647,381	623,888	623,888
R-squared	0.235	0.326	0.327	0.329	0.330	0.329
No. of countries	44	40	40	40	40	40
No. of indiv. treated	535,749	289,971	289,971	289,971	266,445	266,445
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Survey-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	CXS	CXS	CXS	CXS	CXS	CXS

Notes: OLS estimates using DHS sampling probability weights. Robust standard-errors clustered at the country X survey-year (CXS) level are exposed in parentheses. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10%.

Table 2: Estimates by gender and living area

Estimators: LPM	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Gender analysis				Rural/urban analysis			
VARIABLES	Girls	Boys	Interacted model		Rural	Urban	Interacted model	
POST_DP	0.113***	0.113***	0.120***		0.139***	0.006	0.009	
	(0.02)	(0.02)	(0.02)		(0.02)	(0.03)	(0.02)	
POST_DP × Girl			0.002					
			(0.01)					
POST_DP × Rural							0.142***	
							(0.02)	
Duration of exposure								
POST_DP × Duration				0.014***				-0.005
				(0.00)				(0.01)
POST_DP × Duration × Girl				0.002*				
				(0.00)				
POST_DP × Duration × Rural								0.028***
								(0.01)
Indiv. charact.								
Female			-1.751***	-1.711***	-0.035***	-0.004**	-0.004**	-0.004**
			(0.30)	(0.27)	(0.00)	(0.00)	(0.00)	(0.00)
Rural	-0.066***	-0.052***					3.330**	1.552
Observations	304,300	319,588	623,888	623,888	426,816	197,072	623,888	623,888
R-squared	0.358	0.307	0.333	0.332	0.334	0.251	0.346	0.346
No. of countries	40	40	40	40	40	40	40	40
No. of indiv. treated	129,675	136,770	266,445	266,445	196,804	69,641	266,445	266,445
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Interacted controls	No	No	Yes	Yes	No	No	Yes	Yes
Clustering	CXS	CXS	CXS	CXS	CXS	CXS	CXS	CXS

Notes: OLS estimates using DHS sampling probability weights. Controls for population are gender-specific in columns (1) and (2). Robust standard-errors clustered at the country X survey-year (CXS) level are exposed in parentheses. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10%.

Table 3: Estimates by quintile of wealth

Estimators: LPM	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. var.:	Primary School Attendance (at least 1 year)						
	Quintiles subsamples					Interacted Model	
	Q1	Q2	Q3	Q4	Q5		
POST_DP	0.145***	0.059*	0.124***	0.067***	0.041**	0.056***	0.009
	(0.02)	(0.04)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)
POST_DP × Wealth index						-0.053***	
						(0.01)	
POST_DP × Q1							0.136***
							(0.02)
POST_DP × Q2							0.043
							(0.03)
POST_DP × Q3							0.112***
							(0.02)
POST_DP × Q4							0.057***
							(0.02)
Observations	174,285	123,599	115,488	104,745	105,771	623,888	623,888
R-squared	0.328	0.342	0.313	0.311	0.219	0.352	0.359
No. of countries	40	40	40	40	40	40	40
No. of indiv. treated							266,445
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Ye	Yes
Interacted controls	No	No	No	No	No	Yes	Yes
Clustering	CXS	CXS	CXS	CXS	CXS	CXS	CXS

Notes: OLS estimates using DHS sampling probability weights. Robust standard-errors clustered at the country X survey-year (CXS) level are exposed in parentheses. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10%.

Table 4: Robustness checks - Controlling for macro-covariates and other education programs

Estimators: LPM	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.:	Primary School Attendance (at least 1 year)					
POST_DP	0.112***	0.132***	0.126***	0.125***	0.114***	0.136***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
GPE exposure					-0.007	-0.012
					(0.02)	(0.02)
Govt. Educ. Spending (% GNI)	0.002**		0.001		0.002**	
	(0.00)		(0.00)		(0.00)	
Net ODA received (% GDP)		0.004***		0.003**		0.004***
		(0.00)		(0.00)		(0.00)
Observations	623,888	542,665	463,304	419,597	623,888	542,665
Sample	All	All	hipcs/gpe	hipcs/gpe	All	All
R-squared	0.329	0.332	0.308	0.303	0.329	0.332
p-value (F-test)	0.000	0.000	0.000	0.000	0.000	0.000
No. of countries	40	36	30	29	40	36
Indiv. treated (HIPC)	266,445	282,354	266,445	282,354	266,445	282,354
Indiv. treated (GPE)	.	.	.	.	251,123	224,634
Indiv. treated (GPE only)	.	.	.	.	77,725	35,327
Country-FE	Yes	Yes	Yes	Yes	Yes	Yes
Survey-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	CXS	CXS	CXS	CXS	CXS	CXS

Notes: OLS estimates using DHS sampling probability weights. Robust standard-errors clustered at the country X survey-year (CXS) level are exposed in parentheses. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10%.

Table 5: Investigating fiscal space heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CHANNEL	Govt.Educ.Exp.	Debt service savings from debt relief (DSS)					
Debtor History (DH)				Good		Bad	
POST_DP	0.058*						
	(0.03)						
POST_DP X CHANNEL	0.018**	0.122***		0.009		0.185***	
	(0.01)	(0.04)		(0.05)		(0.05)	
POST_DP X CHANNEL_HIPC			0.154***		0.089		0.192***
			(0.04)		(0.06)		(0.05)
POST_DP X CHANNEL_MDRI			0.070		-0.048		0.220***
			(0.06)		(0.08)		(0.08)
Conditional effect w/r to DH							
POST_DP X CHANNEL X DH				0.176***		-0.176***	
				(0.06)		(0.06)	
POST_DP X CHANNEL_HIPC X DH					0.102		-0.102
					(0.07)		(0.07)
POST_DP X CHANNEL_MDRI X DH					0.268***		-0.268***
					(0.10)		(0.10)
Observations	623,888	616,066	616,066	616,066	616,066	616,066	616,066
R-squared	0.320	0.331	0.331	0.331	0.331	0.331	0.331
No. of countries	40	40	40	40	40	40	40
No. of indiv. treated	266,445	260,214	260,214	260,214	260,214	260,214	260,214
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	CXS	CXS	CXS	CXS	CXS	CXS	CXS

Notes: Debt service savings from debt relief have been computed using debt service information from the *Statistical update* about the Heavility Indebted Poor Countries (HIPC) initiative and Multilateral Debt Relief Initiative (MDRI) of September 2017 (IMF). Debt service savings have been computed by the authors as the difference between the debt service due before the debt relief initiative and the one recorded after these initiatives. OLS estimates using DHS sampling probability weights. Robust standard-errors clustered at the country X survey-year (CXS) level are exposed in parentheses. Country and survey-year fixed as well as prior controls are imposed. Constant terms are not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10%.

References

Becker, G. S. (1964). Human capital theory. *Columbia, New York.*

Buchmann, C. and Brakewood, D. (2000). Labor structures and school enrollments in developing societies: Thailand and kenya compared. *Comparative Education Review*, 44(2):175–204.

Cassimon, D. and Campenhout, B. V. (2008). Comparative Fiscal Response Effects Of Debt Relief: An Application To African Hipcs. *South African Journal of Economics*, 76(3):427–442.

Cassimon, D., Van Campenhout, B., Ferry, M., and Raffinot, M. (2015). Africa: Out of debt, into fiscal space? dynamic fiscal impact of the debt relief initiatives on african heavily indebted poor countries (hipcs). *International Economics*, 144:29–52.

Chauvin, N. D. and Kraay, A. (2005). What has 100 billion dollars worth of debt relief done for low-income countries? *International Finance*, 510001.

Cohen, D. (2001). The hipc initiative: true and false promises. *International Finance*, 4(3):363–380.

Colclough, C., Rose, P., and Tembon, M. (2000). Gender inequalities in primary schooling: The roles of poverty and adverse cultural practice. *International Journal of Educational Development*, 20(1):5–27.

Cuaresma, J. C. and Vincelette, G. A. (2008). Debt relief and education in hipcs. *University of Innsbruck, Department of Economics, and World Bank, Economic Policy and Debt Department.*

Dabla-Norris, E., Matovu, J. M., and Wade, P. R. (2004). Debt relief, demand for education and poverty. In *Debt relief for poor countries*, pages 241–266. Springer.

Daseking, C. and Powell, R. (1999). *From Toronto Terms to the HIPC Initiative-A Brief History of Debt Relief for Low-Income Countries.* International Monetary Fund.

Dessy, S. E. and Vencatachellum, D. (2007). Debt relief and social services expenditure: The african experience, 1989–2003. *African Development Review*, 19(1):200–216.

Djimeu, E. W. (2018). The impact of the heavily indebted poor countries initiative on growth and investment in africa. *World Development*, 104:108–127.

Dreher, A., Nunnenkamp, P., and Thiele, R. (2008). Does aid for education educate children? evidence from panel data. *The World Bank Economic Review*, 22(2):291–314.

d'Aiglepierre, R. and Wagner, L. (2013). Aid and universal primary education. *Economics of Education Review*, 37:95–112.

Edmonds, E. V. (2006). Child labor and schooling responses to anticipated income in south africa. *Journal of Development Economics*, 81(2):386–414.

Ersado, L. (2005). Child labor and schooling decisions in urban and rural areas: comparative evidence from nepal, peru, and zimbabwe. *World Development*, 33(3):455–480.

Fafchamps, M. and Wahba, J. (2006). Child labor, urban proximity, and household composition. *Journal of Development Economics*, 79(2):374–397.

Filmer, D. and Pritchett, L. H. (2001). Estimating wealth effects without expenditure data—or tears: an application to educational enrollments in states of india. *Demography*, 38(1):115–132.

Glick, P. and Sahn, D. E. (2000). Schooling of girls and boys in a west african country: the effects of parental education, income, and household structure. *Economics of Education Review*, 19(1):63–87.

Heller, M. P. S. (2005). *Understanding Fiscal Space (EPub)*. International Monetary Fund.

Huisman, J. and Smits, J. (2009). Effects of household-and district-level factors on primary school enrollment in 30 developing countries. *World Development*, 37(1):179–193.

Krugman, P. (1988). Financing vs. forgiving a debt overhang. *Journal of Development Economics*, 29(3):253–268.

Lincove, J. A. (2015). Improving identification of demand-side obstacles to schooling: Findings from revealed and stated preference models in two ssa countries. *World Development*, 66:69 – 83.

Michaelowa, K. and Weber, A. (2006). Chapter 18 aid effectiveness in the education sector: A dynamic panel analysis. In *Theory and practice of foreign aid*, pages 357–385. Emerald Group Publishing Limited.

Michaelowa, K. and Weber, A. (2008). Aid effectiveness in primary, secondary and tertiary education. *Background paper prepared for the Education for All Monitoring Report.*

Mincer, J. (1958). Investment in human capital and personal income distribution. *Journal of Political Economy*, 66(4):281–302.

Powell, R. and Bird, G. (2010). Aid and debt relief in africa: have they been substitutes or complements? *World Development*, 38(3):219–227.

Presbitero, A. F. (2009). Debt-relief effectiveness and institution-building. *Development Policy Review*, 27(5):529–559.

Sachs, J. D. (1989). Conditionality, debt relief, and the developing country debt crisis. In *Developing Country Debt and Economic Performance, Volume 1: The International Financial System*, pages 255–296. University of Chicago Press, 1989.

Thomas, A. H. (2006). *Do Debt-Service Savings and Grants Boost Social Expenditures?* Number 2006-2180. International Monetary Fund.

Thugge, K. and Boote, A. R. (1997). *Debt relief for low-income countries and the HIPC initiative.* International Monetary Fund.

Supplementary Appendix

Did Debt Relief Initiatives help to reach the MDGs?
A Focus on Primary Education

Table S.A1: Educational targets of the HIPC initiative

	Targets concerning education	Status at completion point
Bolivia	Increase public expenditures on basic education	Met
	Develop a plan for reducing expenditures on higher education as a share of total education expenditures	Met
	Improve coverage of basic education in rural areas, especially for females	Met
	Improve quality of basic education (development of an action program, provision of textbooks to all students in primary and secondary education, development of a national assessment system)	Met
	Improve access to early childhood education	Met
	Adapt education reform to popular participation and decentralization	Met
	Increase the number of girls completing the 5th grade in rural areas (increase of 34,000)	Partially met
Benin	Elimination of schools fees for all pupils in rural schools	Met
	Provision of grants to rural schools to compensate for the loss of revenue from school fees	Met
	Provision of grants to local communities prepared to assume the responsibility for hiring teachers to fill school vacancies	Met
	Eliminate repetition at grade 1	Met
	Reduce repetition between grades 2 and 6 to less than 15%	Lack of data
	Increase the rate of completion primary education to 70%	Not met
Burkina Faso	Adopt an action plan to recruit additional teachers	Met
	Increase efficiency of primary school and limit grade repetition	Met
Cameroon	Construction of 2500 new classrooms	Met
	Decentralization of teacher management and implementation of new teacher statutes	Met
Chad	Increase the GER to at least 61% for girls and 85% for boys vs. 50 and 85% in 98-99	Met
	Reduce the repetition rate from 26% in 98-99 to at most 22%	Not met
Cote d'Ivoire	90% of students enrolled in primary school receive three textbooks covering French, Mathematics and Civic education	Met
Ethiopia	Reduced repetition rate at the primary level from 9% in 99/00 to 7%	Lack of data
	Increased the GER of girls in primary level from 40.7% in 99/00 to 50%	Met
Ghana	Primary GER for girls increased from 72% in 2000 to 74%	Met
Guinea	Increase GER for primary school from 56% in 1999 to 62% in 2001 and 71% in 2002	Met
	Increase GER for primary school for girls from 40% in 99 to 51% in 2001 and 61% in 2002	Met
	Increase the no. of new primary school teachers by 1,500 per year	Met
Haiti	Help poor families to pay school fees and allow enrollment of an additional 50,000 out-of-school children in primary school	Met
	Actual recurrent expenditures for education reach at least 21% of actual total recurrent government spending, of which 50% at least spent on education	Partially met
	Training of 2,500 new primary teachers	Met
	Two visits on average per year to all primary schools by inspectors	Partially met
Madagascar	Formalizing and implementing new financial incentives for teachers to serve in rural public schools	Met
	Recruiting at least 3,500 new teachers from 2,000 for public primary schools	Met
	And deploying at least 60% of them in remote areas	Lack of data
Malawi	Share of education sector expenditure in discretionary recurrent budget of at least 23%	Met
	Reallocate budgetary resources from secondary school boarding to teaching and learning materials	Met
	Pre-packaging of donor-supplied primary textbooks + direct supply from the supplier to the schools	Met
	Yearly enrollment of 6,000 students for teacher training	Not met
	Creation of in-service training for primary teachers (at least once each year)	Met
Mali	Teacher recruitment (2,206)	Met
	Allocation for teaching material in primary school (billion of CFA francs): 2.6	Met

Continued on next page

Following the previous table

	Limiting higher education scholarship (billion of CFA francs): 4.5	Met
	Total budget allocation (billion of CFA francs): 20.8	Not met
Nicaragua	Approval of a satisfactory school autonomy law to strengthen the legal foundation	Met
Niger	Construction of at least 1,000 new classrooms, 85% of which in rural areas	Met
	Recruiting 1,200 new volunteer primary school teachers, 75% of whom will be placed in rural schools	Met
	Complete a countrywide school map and a report on demand- and supply-side impediments to primary school enrollment	Not met
	Limit grade-6 repetition rates to 15% at least	Not met
Mozambique	None	
Rwanda	Increasing NER in primary school from 69% in 1999 to 73% in 2001	Met
	Making operational at least 6 primary teacher training centers offering full-time and in-service training programs	Met
	Establishment of a framework for community participation in support of primary and secondary education	Met
	Implementation of a capacity-building program for the management of education at the central and decentralized levels	Met
Senegal	Recruitment of 2,000 teachers each year	Met
	Recruitment of contract teachers and elimination of recruitment of teachers into the civil-service structure	Met
	Maintain budgetary increases for primary education as a % of the education budget, 44% in 2003	Met
Tanzania	Completion of mapping of schools covering 50% of all local authorities	Met
Togo	Training at least 500 new teachers	Met
	Conducting remedial training of at least 4,000 existing teachers	Met
Uganda	NA	NA
Zambia	Increasing the share of education in the domestic discretionary budget from 18.5 in 1999 to at least 20.5%	Met
	Raising the starting compensation of teachers in rural areas above the poverty line for a household	Met
	Implement an action plan for increasing student retention in Northeast, Luapula, Eastern, Northwestern and Western Provinces	

Notes: GER stands for gross enrollment ratio. NER stands for net enrollment ratio.
Source: Authors, using decision and completion point papers from the IMF.

Table S.A2: Debt relief under the HIPC initiatives

Country	Debt relief (NPV) US$ million	Common reduction factor	% Bilateral debt	% Multilateral debt
Bolivia	854	14%	31%	69%
Haiti	140.3	15%	15%	86%
Togo	282	19%	55%	45%
Senegal	488	19%	43%	57%
Cote d'Ivoire	3109.3	24%	22%	74%
Cameroon	1267	27%	69%	25%
Chad	170.1	30%	21%	79%
Benin	265	31%	29%	71%
Guinea	639	36%	40%	60%
Mali	539	37%	31%	69%
Uganda	656	38%	17%	83%
Madagascar	836	40%	57%	43%
Malawi	646.2	44%	24%	75%
Burkina Faso	424	46%	16%	84%
Ethiopia	1982	47%	32%	66%
Niger	663.1	54%	35%	65%
Tanzania	2026	54%	50%	50%
Ghana	2186	56%	50%	50%
Zambia	2499	63%	46%	53%
Rwanda	695.5	71%	9%	91%
Mozambique	306	72%	63%	37%
Nicaragua	3300	72%	-	-

Source: Authors, using decision and completion point documents from the IMF.

Sample of study: DHS surveys for HIPC and non-HIPC countries

Figure S.A1: Sample evolution

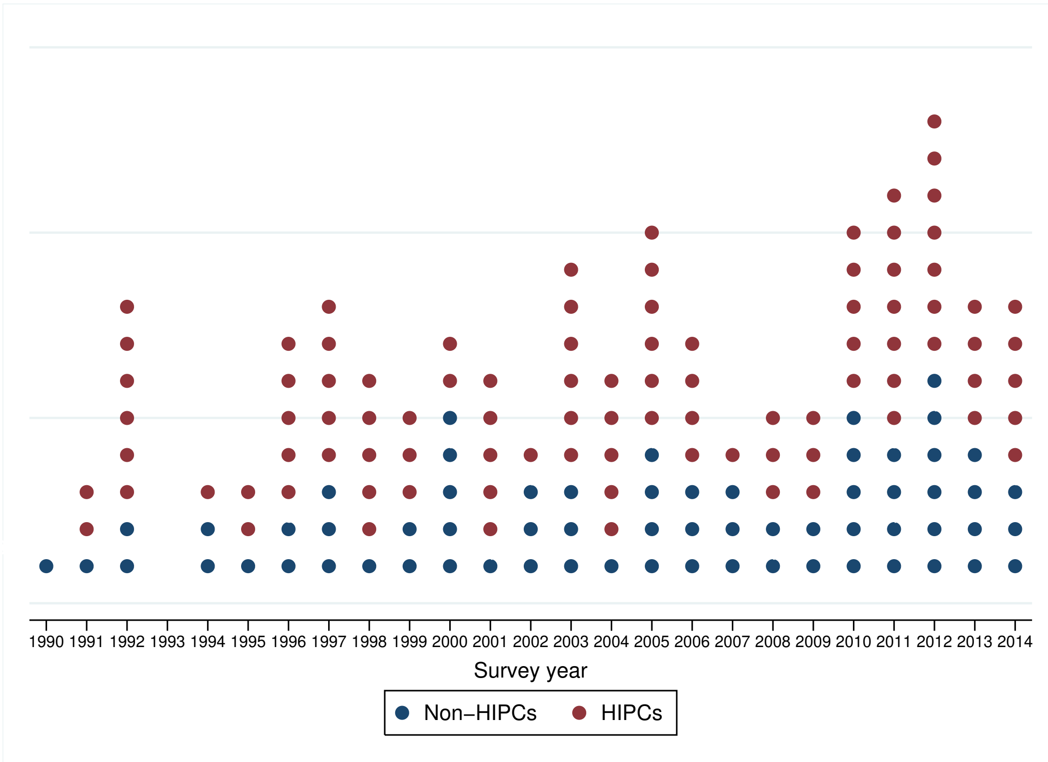
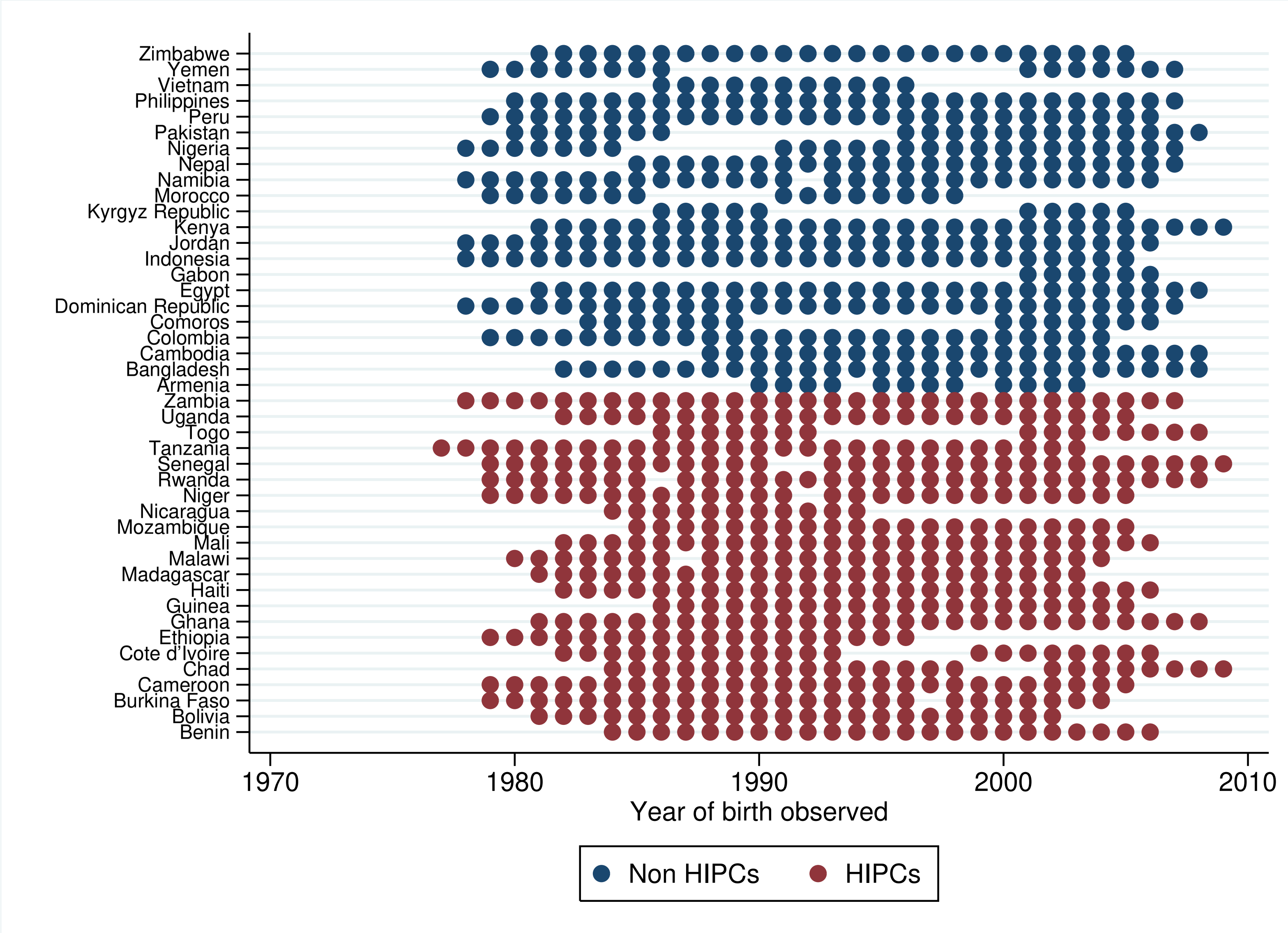


Figure S.A2: Sample evolution



Selection due to overlapping issues

In order to avoid observing twice the same individuals, we exclude certain children from the sample as described in the main text. Table S.A3 presents the descriptive statistics for excluded and selected individuals in surveys where a selection was implemented. Our strategy leads to select older individuals who are more likely to have ever attended primary school. Sample children come from poorer households and their parents are less educated in comparison with excluded individuals.

Table S.A3: Selection of individuals due to overlapping cohorts

Sample	Excluded individuals		Included individuals		Diff	
	Mean	SD	Mean	SD	Diff	T-test
Ever Attended Primary School	0.65	0.50	0.83	0.40	0.182***	(234.46)
Age	7.31	1.30	10.27	1.70	2.962***	(1019.75)
Girl	0.49	0.50	0.49	0.50	0.00140	(1.50)
Mother Educ: None	0.32	0.50	0.38	0.50	0.0542***	(47.53)
Mother Educ: Primary	0.41	0.50	0.38	0.50	-0.0224***	(-19.26)
Mother Educ: Secondary or Tertiary	0.27	0.40	0.24	0.40	-0.0318***	(-30.82)
Father Educ: None	0.24	0.40	0.29	0.50	0.0491***	(42.70)
Father Educ: Primary	0.40	0.50	0.39	0.50	-0.00873***	(-6.90)
Father Educ: Secondary or Tertiary	0.35	0.50	0.31	0.50	-0.0404***	(-33.30)
Head's Child	0.77	0.40	0.77	0.40	0.000896	(1.14)
Wealth Index (WI)	0.02	1.60	-0.01	1.60	-0.0326***	(-9.52)
Rural	0.65	0.50	0.65	0.50	-0.00658***	(-7.37)
GDP per capita (log, constant USD)	7.18	0.90	7.05	0.90	-0.130***	(-78.31)
Population under 15 (log)	16.15	1.10	16.05	1.20	-0.0925***	(-42.92)
Gov. Educ. Spending (% GNI)	3.21	3.70	3.23	3.70	0.0201***	(2.88)
Observations	450209		782828		1233037	

Notes: This table represents the descriptive statistics for all the selected surveys where a selection was implemented, hence the lower number of observations. T-tests are computed on pooled data regardless the year of survey and the age of individuals. ***, ** and * denote a significance at respectively 1%, 5% and 10%.

We then compare selected and non-selected individuals born the same year and in the same country. In order to account for the year at which we observe selected and non-selected individuals born the same year (people born in 1988 and surveyed in 2005 were younger at the time of observation than those surveyed in 2008) we also include survey-year fixed effects along with country fixed effects. Results reported in Table S.A4 show that many differences disappear or are very low.

Table S.A4: Within cohort, year of birth and country selection of individuals due to overlapping cohorts

Estimator: LPM Dep. var.	(1) Ever Attended Primary School	(2) Age	(3) Girl	(4) Mother educ: None	(5) Father educ: None	(6) Head's Child
Included individual	0.012	0.000***	0.002	0.000	0.000	-0.007***
	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	1227496	1233037	1232904	745395	634038	1232870
Yob*survey year FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes

Estimator: LPM Dep. var.	(7) Wealth Index	(8) Rural	(9) GDP per capita (log, constant USD)	(10) Pop under 15 (log)	(11) Educ Spending (% GNI)	(12) Polity IV
Included individual	0.022	-0.016**	0.008	0.001	0.164	0.000*
	(0.02)	(0.01)	(0.01)	(0.01)	(0.36)	(0.00)
Observations	941557	1233037	1228235	1233037	1218840	1233037
Yob*survey year FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table represents the descriptive statistics for all selected surveys where a selection was implemented. Robust standard-errors clustered using the DHS clusters are exposed in parentheses. ***, ** and * denote a significance at respectively 1%, 5% and 10%.

Table S.A5: HIPC countries in the Sample

HIPCs	DHS	Individuals	Ind Treated	HIPCs	DHS	Individuals	Ind Treated
Benin	1996	4319	0	Mali	1995/1996	7519	0
	2001	4738	4738		2001	9517	9517
	2006	14055	14055		2006	12777	12777
	2011/2012	21597	21597		2012/2013	12863	12863
Bolivia	1993	210	0	Nicaragua	1997/1998	5925	0
	1994	3870	0		2001	12523	12523
	1998	7612	0	Niger	1992	5733	0
	2003/2004	10722	10722		1998	7613	0
	2008	14669	14669		2006	9089	9089
Burkina Faso	1992/1993	5130	0		2012	15299	15299
	1998/1999	4175	0	Mozambique	1997	6566	0
	2003	13242	13242		2003	10699	10699
	2010	18652	18652		2009	1296	1296
Cameroon	1991	4315	0		2011	15581	15581
	1998	4668	2382	Rwanda	1992	6636	0
	2004	10514	10514		2000	7212	5608
	2011	14862	14862		2005	6573	6573
Chad	1996/1997	8367	0		2010/2011	5229	5229
	2004	6716	6716		2014/2015	10950	10950
	2014/2015	26480	26480	Senegal	1992/1993	3691	0
Cote d'Ivoire	1994	4324	0		1997	9405	0
	1998/1999	2638	0		2005	9551	9551
	2011/2012	10387	10387		2010/2011	2739	2739
Ethiopia	1992	9518	0		2012/2013	2468	2468
	1997	12266	0		2015	9062	9062
	2003	16661	16661	Tanzania	1991/1992	5910	0
Ghana	1993	3182	0		1996	3352	0
	1998/1999	3013	0		1999	3138	0
	2003	3847	3847		2005	1652	1652
	2008	7683	7683		2010/2009	11455	11455
	2014	8521	8521	Togo	1998	10443	0
Guinea	1999	6118	0		2013/2014	10467	10467
	2005	8757	8757	Uganda	1995	7474	0
	2012	10092	10092		2000/2001	6195	5657
Haiti	1994/1995	4017	0		2006	7332	7332
	2000	6604	0		2011	14604	14604
	2005/2006	8442	3948	Zambia	1992	3737	0
	2012	10389	10389		1996	5592	0
Madagascar	1992	4267	0		2001/2002	5600	5600
	1997	5959	0		2007	6412	6412
	2003/2004	4711	4711		2013/2014	21244	21244
	2008/2009	16643	16643				
Malawi	1992	5526	0				
	2000	6862	5039				
	2004/2005	10872	10872				
	2010	27457	27457				
Total							
No of HIPCs		22					
No of surveys		87					
No of individuals		748792					
No of individuals treated		537501					

Table S.A6: Non-HIPC countries in the Sample

Non-HIPCs	DHS	Individuals	Non-HIPCs	DHS	Individuals
Armenia	2000	1911	Kenya	1993	6225
	2005	1436		1998	5952
	2010	1050		2003	5143
Bangladesh	1993/1994	3383		2008/2009	6015
	1996/1997	3229		2014/2015	39487
	1999/2000	6155	Kyrgyz Republic	1997	2113
	2004	4012		2012	3529
	2007	5062	Morocco	1992	7473
	2011	5941		2003/2004	9905
	2014	11385	Namibia	1992	5475
Cambodia	2000	10360		2000	5001
	2005/2006	9578		2007	4709
	2010/2011	6888		2013	7806
	2014	11704	Nepal	1996/1997	6008
Colombia	1990	4292		2001/2002	7285
	1995	5045		2007/2008	4825
	2000	4064		2011/2012	7439
	2005/2004	14826	Nigeria	1990	11087
	2009/2010	25775		2003	4757
Comoros	1996	2985		2008	21672
	2012	4715		2013	37687
Dominican Republic	1991	5227	Pakistan	1990/1991	10765
	1996	2837		2006/2007	121947
	1999	433		2012/2013	15367
	2002	13960	Peru	1991/1992	8745
	2007	17800		1996	14825
	2013	5765		2000	13522
Egypt	1992/1993	4920		2004/2006	5625
	1995/1996	12062		2009	2534
	2000	8997		2010	2226
	2005	7194		2011	2247
	2008	12042		2012	15102
	2014	17647	Philippines	1993	11080
Gabon	2012	6508		1998	8129
Indonesia	1991	9620		2003	7804
	1994	12276		2008	7274
	1997	18453		2013	11516
	2002/2003	14398	Vietnam	1997	4026
	2007	18407		2002	4184
	2012	27110	Yemen	1991/1992	21542
Jordan	1990	11828		2013	25322
	1997	5926	Zimbabwe	1994	4828
	2002	6147		1999	5067
	2007	10376		2005/2006	6022
	2012	13384		2010/2011	9565
Total					
No of countries	22				
No of surveys	90				
No of individuals	955970				

Main descriptive statistics

Table S.A7: Descriptive statistics: HIPCs and non HIPCs

Sample	All		HIPCs		Non-HIPCs		Diff	
	Mean	SD	Mean	SD	Mean	SD	Diff	T-test
Ever Attended Primary School	0.78	0.40	0.71	0.50	0.83	0.40	0.126***	(196.56)
Age	9.54	2.00	9.79	2.10	9.34	2.00	-0.451***	(-143.87)
Girl	0.49	0.50	0.50	0.50	0.49	0.50	-0.00795***	(-10.31)
Mother Education: None	0.43	0.50	0.56	0.50	0.30	0.50	-0.255***	(-266.22)
Mother Education: Primary	0.34	0.50	0.33	0.50	0.35	0.50	0.0275***	(28.98)
Mother Education: Secondary or Tertiary	0.22	0.40	0.11	0.30	0.34	0.50	0.228***	(283.31)
Father Education: None	0.34	0.50	0.47	0.50	0.19	0.40	-0.275***	(-276.27)
Father Education: Primary	0.35	0.50	0.34	0.50	0.37	0.50	0.0227***	(21.63)
Father Education: Secondary or Tertiary	0.31	0.50	0.19	0.40	0.44	0.50	0.252***	(259.00)
Head's Child	0.76	0.40	0.74	0.40	0.78	0.40	0.0456***	(69.39)
Wealth Index (WI)	0.00	1.60	-0.59	1.30	0.59	1.60	1.184***	(471.38)
Rural	0.65	0.50	0.70	0.50	0.61	0.50	-0.0919***	(-125.76)
GDP per capita (log, constant USD)	7.02	0.80	6.44	0.60	7.48	0.70	1.038***	(1035.52)
Population under 15 (log)	16.04	1.2	15.41	0.6	16.54	1.3	1.129***	(688.9)
Govt. Educ. Spending (% GNI)	3.20	2.80	3.31	1.20	3.12	3.50	-0.189***	(-43.57)
Polity IV	1.58	4.9	2.14	4.4	1.15	5	-0.189***	(-131.50)
Observations	1704762		748792		955970		1704762	

Notes: ***, ** and * denote a significance at respectively 1%, 5% and 10%.

Principal component analysis

Figure S.A3: Graphical analysis of PCA

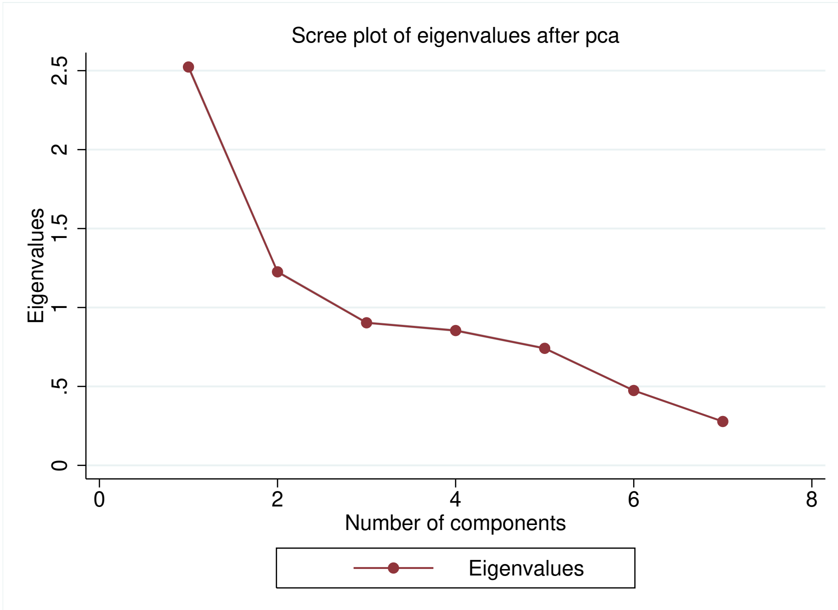


Table S.A8: Coefficients used to generate wealth index

Variable	Coefficients used to estimate individual wealth scores
Electricity	0.5214
Radio	0.2104
Television	0.5439
Refrigerator	0.4927
Bicycle	0.0489
Motorcycle	0.2195
Car	0.3077

Table S.A9: Minimum year-of-birth for being exposed at least one year to debt relief under the Enhanced HIPC initiative (HIPC II)

HIPCs	Decision Point under the HIPC II	Official leaving age to primary school	Minimum year-of-birth required
Benin	2000	12	1988
Bolivia	2000	12	1988
Burkina Faso	2000	13	1987
Cameroon	2000	12	1988
Chad	2001	12	1989
Cote d'Ivoire	2009	12	1997
Ethiopia	2001	13	1988
Ghana	2002	12	1990
Guinea	2000	13	1987
Haiti	2006	12	1994
Madagascar	2000	11	1989
Malawi	2000	12	1988
Mali	2000	13	1987
Mozambique	2000	12	1988
Nicaragua	2000	13	1987
Niger	2000	13	1987
Rwanda	2000	13	1987
Senegal	2000	12	1988
Tanzania	2000	14	1986
Togo	2008	12	1993
Uganda	2000	13	1987
Zambia	2000	14	1986

Notes: Figures for the official leaving age to primary school and for the minimum year-of-birth for treated are average figures. For some HIPCs, the official leaving age to primary school has changed over time leading thus, for some surveys, to changes in the minimum year-of-birth required for being considered as treated.

Parallel trend discussion

Before running a difference-in-differences (DiD) model, we must make sure that there is no divergence in the evolution of outcome variable prior to the "treatment". This condition, known as the parallel or common trend hypothesis, is indeed essential since, when holding, it gives credit to the interpretation of the DiD estimator as a causal impact running from the "treatment" to the outcome variable. If one observes that the outcome variable evolves in different ways for control and treatment units prior to the treatment implementation, it then becomes unrealistic to grant the post-treatment evolution of this variable to the treatment itself.

In order to test for this common trend hypothesis, we restrict the sample to DHS surveys completed before 2000, and consider only children born no later than 1987 (i.e. whom, on average, could not be exposed to the Enhanced HIPC initiative, since it was launched in 1999 and implemented in 2000 at the earliest).

Using this sample, we try alternative specifications to check for the ex-ante common trend hypothesis. We first run our baseline specification (as exposed in equation 1, but without the POST_DP variable) on the restricted sample. We augment this specification with a survey linear trend (i.e. a continuous variable for survey years) and an interaction term between the survey time trend and a dummy variable flagging countries that will benefit from the HIPC initiative after 2000. The coefficient associated with the survey trend thus captures the linear evolution in primary school enrollment between 1990 and 2000, while the one for the interaction term captures a potential different evolution in primary school enrollment for HIPCs ("treated" countries). Estimates in Column (I) of Table S.A10 suggest that while primary school enrollment significantly increases (in a linear way) over the 1990-2000 period for the whole sample, such evolution has not been significantly different in HIPCs (the associated coefficient being not statistically significant). Column (II) of Table S.A10 reports results for the same estimate, but augmented with a HIPC-survey year trend squared. While the HIPC-specific trend in level becomes marginally significant (at the 10 % level), the squared term remains not significant. These results (Column (I) and (II)) thus support the absence of a diverging path in primary school enrollment for HIPCs (on average) prior to the debt relief initiatives. We then also test the common trend hypothesis switching the survey-year trend by a year-of-birth (so cohorts) trend. The interaction terms, both in level and squared are not statistically significant hence supporting the common trend hypothesis as well (Columns III and IV).

Lastly, we implement two placebo tests. In Column (V), we define a placebo treatment for HIPCs by considering that children born between 1984 and 1987 are treated. This test thus aims at comparing the probability of being enrolled in primary school (on average) for children born between 1984 and 1987, with respect to older children as well as to those in control countries.

Table S.A10: Investigating the common trend hypothesis

Estimator: LPM	(I)	(II)	(III)	(IV)	(V)	(VI)
Restrictions:	Period: 1990-2000 & Year-of-birth (YoB) ≤1987					
Dep. var:	Primary School Attendance (at least 1 year)					
Time trend						
Survey Trend	0.029***	0.029***				
	(0.01)	(0.01)				
HIPC×Survey Trend	0.005	0.067*				
	(0.00)	(0.04)				
HIPC×Survey Trend^2		-0.003				
		(0.00)				
YoB Trend			-0.023**	-0.023**		
			(0.01)	(0.01)		
HIPC×YoB Trend			-0.003	-0.016		
			(0.01)	(0.01)		
HIPC×YoB Trend^2				0.001		
				(0.00)		
Placebo treatments						
HIPC×YoB[1984-1987]					0.028	
					(0.039)	
HIPC×YoB[1984]						0.048
						(0.06)
HIPC×YoB[1985]						0.006
						(0.043)
HIPC×YoB[1986]						0.029
						(0.033)
HIPC×YoB[1987]						0.019
						(0.028)
Observations	345,319	345,319	345,319	345,319	345,319	345,319
R-squared	0.286	0.286	0.284	0.284	0.287	0.287
Indiv. Treated (placebo)	-	-	-	-	83,560	83,560
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Survey-Year FE	No	No	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	No	No	Yes	Yes
Clustering	CXS	CXS	CXS	CXS	CXS	CXS

Notes: In order to investigate the hypothesis of no diverging path in the outcome variable prior to the treatment we restrain the sample to children born no later than 1987 and to surveys that took place no later than 2000 i.e. before the effects of the HIPC initiative might have materialized (since most of treated countries reached their decision point in late 2000-early 2000s). OLS estimates using DHS sampling probability weights are reported. Robust standard-errors clustered at the country X survey-year (CXS) level are exposed in parentheses. Constant terms are not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10%

Results show that HIPC's children did not experience an increase in their probability of being enrolled in primary school before the Enhanced HIPC initiative and as compared to children in control countries. Column (VI) show the results when we apply a gradual treatment instead of a classic treatment (before and after). These findings remain unchanged and comfort us regarding the common trend hypothesis and the relevance of the DiD specification in our context of study.

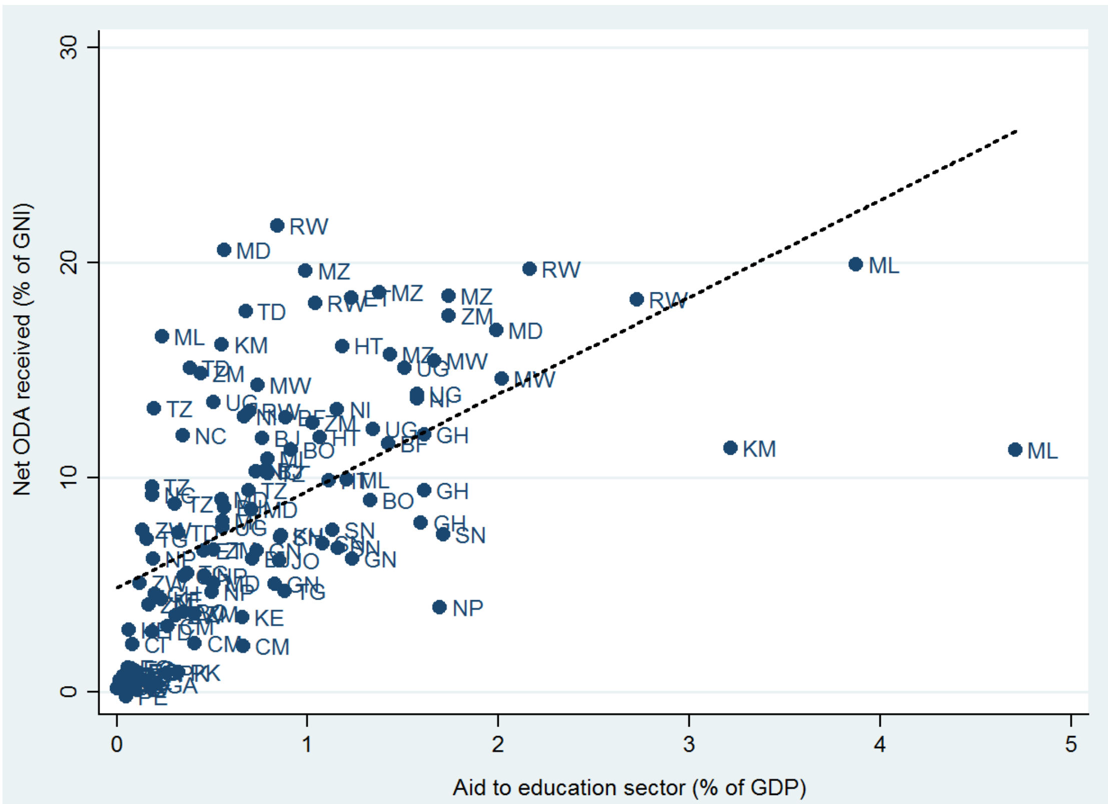
Table S.A11: Estimates w/r to individual heterogeneity - Interacted models

Estimators: LPM	(1)	(2)	(3)
Dep. var:	Primary School Attendance (at least 1 year)		
POST_DP X Duration	-0.002	-0.006	-0.009
	(0.01)	(0.01)	(0.01)
POST_DP X Duration X Girl	0.001		
	(0.00)		
POST_DP X Duration X Rural		0.006**	
		(0.00)	
(1) POST_DP X Duration X Q1			0.012***
			(0.00)
(2) POST_DP X Duration X Q2			0.008***
			(0.00)
(3) POST_DP X Duration X Q3			0.007***
			(0.00)
(4) POST_DP X Duration X Q4			0.007***
			(0.00)
T-test $\beta(1) = \beta(2)$	.	.	4.81
p-value			0.028
T-test $\beta(1) = \beta(3)$	.	.	7.37
p-value			0.007
T-test $\beta(1) = \beta(4)$			7.10
p-value			0.008
Observations	623,888	623,888	623,888
R-squared	0.338	0.345	0.344
No. of countries	40	40	40
Country-Survey Year FE	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
Controls	Ind. level	Ind. level	Ind. level
Interacted controls (Ind. & Cntry level)	Yes	Yes	Yes
Clustering	CXS	CXS	CXS

Notes: OLS estimates using DHS sampling probability weights. Macro-level controls are not included in the estimations because of country $\times$ survey-year fixed effects. Yet, there are included when interacted with the individual characteristics of interest (Female, Rural, Q1, Q2, Q3, Q4). Interacted models thus only consider as explanatory variables controls at the individual level (Ind.-level - so far used in other estimates) as well as cohort and country $\times$ survey-year fixed effects. Note that interacted controls encompass interaction terms between individual characteristics of interest and individual- and macro-level controls, as well as cohort fixed effects but not country $\times$ survey-year (since it would lead to capture the treatment effect). T-tests suggest that the effect of the exposure's length to debt relief on primary school attendance is larger for poorer children than for those in the upper categories of the wealth distribution (as compared to richer kids). Robust standard-errors clustered at the country X survey-year (CXS) level are exposed in parentheses. Constant terms are not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10%.

Concurrent large scale programs for education and sector-specific foreign aid.

Figure S.A4: Correlation between Net ODA received and aid to education sector



Notes: Each dot of the scatter represents a country-survey-year observation. The x-axis denotes the amount of aid to education sector (commitments) in percentage of GDP for a given year and a given country. Data have been retrieved from the *Creditor Reporting System (CRS)* database of the OECD-DAC, The y-axis represent net ODA received (disbursements) in percentage of GNI retrieved from the *World Development Indicators* database.

Table S.A12: Control for aid to education sector

Estimators: LPM	(1)	(2)	(3)	(4)	(5)
Dep. var.:	Primary School Attendance (at least 1 year)				
POST_DP	0.112***	0.134***	0.124***		0.137***
	(0.02)	(0.02)	(0.02)		(0.02)
GPE treatment				0.004	-0.012
				(0.02)	(0.02)
Aid to education (% GDP)		0.007	0.010	0.010	0.007
		(0.01)	(0.01)	(0.01)	(0.01)
Observations	623,888	612,465	467,263	612,465	612,465
Sample	All	All	hipc/gpe	All	All
R-squared	0.330	0.326	0.303	0.324	0.326
p-value (F-test)	0.000	0.000	0.000	0.000	0.000
No. of countries	40	39	30	39	39
Indiv. treated (HIPC)	266,445	286,971	286,971	.	286,971
Indiv. treated (GPE)	.			271,649	271,649
Indiv. treated (GPE only)	.			77,725	77,725
Controls	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes
Survey-Year FE	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes

Notes: Data for *Aid to education* have been retrieved from the *Creditor Reporting System* (CRS) database of the OECD-DAC which contains data for sector-specific aid (both commitments and disbursements). Due to data availability, we consider commitments in order to limit the restriction on our study sample. Data for disbursements start in 2002 while data for commitments can be obtained back to 1995. OLS estimates using DHS sampling probability weights. Robust standard-errors clustered at the country X survey-year (CXS) level are exposed in parentheses. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10%.

Table S.A13: Global Partnership for Education (GPE) - Commitments and Disbursments

Country	Joined GPE in:	Commitments	Disbursments	Partners
Bangladesh	2015	100 100 000	20 000 000	IBRD
Benin	2007	117 893 019	105 072 988	IBRD, Swiss Dev. coop.
Burkina Faso	2002	180 452 926	155 100 000	IBRD, AFD, UNICEF
Cambodia	2006	96 503 808	89 042 431	IBRD, UNESCO, UNICEF
Cameroon	2006	100 754 750	63 800 188	IBRD
Chad	2012	54 853 988	41 602 505	UNESCO, UNICEF
Comoros	2013	5 194 274	3 508 934	UNICEF
Cote d'Ivoire	2010	41 620 219	38 665 235	IBRD, UNICEF
Ethiopia	2004	337 750 477	235 212 358	IBRD, UNICEF
Ghana	2004	94 500 000	94 500 000	IBRD
Guinea	2002	102 200 000	71 183 758	IBRD
Haiti	2008	46 389 169	45 531 321	IBRD
Kenya	2005	209 943 488	132 503 817	IBRD
Kyrgyz	2006	27 799 008	23 331 674	IBRD
Madagascar	2005	209 850 000	189 767 679	IBRD, UNICEF
Malawi	2009	135 469 114	90 313 569	IBRD
Mali	2006	48 896 151	39 171 867	IBRD, UNICEF
Mozambique	2003	227 100 000	187 199 155	IBRD
Nepal	2009	177 705 947	154 968 359	IBRD, UNICEF
Nicaragua	2002	41 200 000	41 119 516	IBRD
Niger	2002	105 089 826	41 993 251	IBD, UNICEF
Nigeria	2012	100 729 900	18 805 807	IBRD
Pakistan	2012	100 440 000	37 155 826	IBRD, UNICEF
Rwanda	2006	200 200 000	175 000 000	IBRD, DfID
Senegal	2006	127 024 938	115 877 118	IBRD
Tanzania	2013	100 432 850	63 408 176	SIDA, UNESCO
Togo	2010	73 148 450	52 294 646	IBRD, UNICEF
Uganda	2011	100 550 000	21 465 793	IBRD
Vietnam	2003	84 833 650	84 288 433	IBRD, UNESCO
Yemen	2003	122 366 772	59 663 194	IBRD, UNICEF
Zambia	2008	95 898 391	77 934 492	DfID, Netherlands, UNICEF
Zimbabwe	2013	44 450 000	19 073 262	UNICEF, IBRD
Total		**3 611 341 115**	**2 588 555 352**	

Notes: Disbursments and commitments are expressed in current USD. All these figures have been retrieved from the *Global Partnership for Education*'s website.

Table S.A14: Effects of GPE participation on Primary School Attendance

Estimators: LPM	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.:	Primary School Attendance (at least 1 year)					
GPE exposure	-0.001	0.004	-0.007	-0.001	-0.153**	-0.198***
	(0.05)	(0.05)	(0.05)	(0.05)	(0.07)	(0.06)
Observations	306,153	306,153	288,744	288,744	188,645	188,645
Sample excl.:	hipcs + gpe	hipcs + gpe	all hipcs	all hipcs	all hipcs	all hipcs
R-Squared	0.352	0.351	0.356	0.355	0.418	0.420
p-value (F-test)	0.000	0.000	0.000	0.000	0.000	0.000
Measure of wealth	Index	Quintile	Index	Quintile	Quintile	Quintile
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Control for Educ exp. (% GNI)	Yes	Yes	Yes	Yes	No	Yes
Control for net ODA (%GDP)	No	No	No	No	Yes	Yes
No. of countries	19	19	18	18	14	14
Indiv. treated (GPE only)	77,725	77,725	77,725	77,725	35,327	35,327
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Survey-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: OLS estimates using DHS sampling probability weights. Robust standard-errors clustered at the country X survey-year (CXS) level are exposed in parentheses. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10%.

Sample dependence

Table S.A15: Dropping each country one after another

Estimators: LPM	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. var.:	Primary School Attendance (at least 1 year)							
Country dropped:	ARM	BFA	BEN	BOL	CIV	CMR	COL	DOM
POST_DP	0.108***	0.114***	0.112***	0.117***	0.115***	0.117***	0.117***	0.127***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Obs.	620,135	603,502	607,003	606,479	617,354	606,128	606,061	609,557
Obs. dropped	3,753	20,386	16,885	17,409	6,534	17,76	17,827	14,331
Country dropped:	EGY	ETH	GAB	GHA	GIN	HTI	IDN	JOR
POST_DP	0.109***	0.101***	0.112***	0.116***	0.113***	0.108***	0.105***	0.113***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Obs.	590,699	605,017	621,690	611,057	609,292	615,768	583,563	620,702
Obs. dropped	33,189	18,871	2,198	12,831	14,596	8,120	40,325	3,186
Country dropped:	KEN	KHM	COM	KGZ	MAR	MDG	MLI	MWI
POST_DP	0.092***	0.110***	0.121***	0.111***	0.112***	0.110***	0.098***	0.112***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Obs.	600,317	593,729	620,300	619,566	620,294	607,307	593,356	603,979
Obs. dropped	23,571	30,159	3,588	4,322	3,594	16,581	30,532	19,909
Country dropped:	MOZ	NIC	NGA	NER	NAM	NPL	PER	PAK
POST_DP	0.113***	0.116***	0.113***	0.117***	0.109***	0.115***	0.112***	0.116***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Obs.	611,682	614,540	588,203	600,436	618,943	614,514	586,652	611,001
Obs. dropped	12,206	9,348	35,685	23,452	4,945	9,374	37,236	12,887
Country dropped:	RWA	SEN	TCD	TOG	TZA	UGA	ZMB	ZWE
POST_DP	0.104***	0.109***	0.116***	0.106***	0.117***	0.108***	0.127***	0.112***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Obs.	613,749	615,426	610,215	613,941	611,177	608,911	604,073	615,314
Obs. dropped	10,139	8,462	13,673	9,947	12,711	14,977	19,815	8,574

Notes: All regressions include the individual and country-level explanatory variables used for the benchmark estimates (cf. Table 1). We also control for country, survey-year, and cohort fixed effects. F-Statistics are not reported in order to save space but are all statistically significant at the 0.01% level. R-Squared are really similar to those obtained for the benchmark results (around 30%). OLS estimates using DHS sampling probability weights. Lastly, robust standard-errors clustered at the country X survey-year (CXS) level are exposed in parentheses. Constant terms are not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10%.

Table S.A16: Dropping each region one after another

Estimators: LPM	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. var.:	Primary School Attendance (at least 1 year)						
Sub-sample excluded:	none	**EE-ME**	**AFR**	**SSA**	**LATAM**	**ASIA**	**non-HICs**
POST_DP	0.112***	0.107***	0.110***	0.111***	0.128***	0.107***	0.100***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Observations	623,888	612,627	508,544	545,327	554,494	531,143	527,613
R-squared	0.329	0.327	0.330	0.330	0.326	0.314	0.319
p-value (F-test)	0.000	0.000	0.000	0.000	0.000	0.000	0.000
No. of countries	40	37	32	34	37	36	32
No. of obs. dropped	0	11,261	115,344	78,561	69,394	92,745	96,275
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	CXS	CXS	CXS	CXS	CXS	CXS	CXS

Notes: Column (1) reports benchmark results. EE-ME stands for East-Europe and Middle-East countries; AFR for African countries; SSA for Sub-Sahara African countries; LATAM for Latin American countries, and ASIA for Asian countries. Lastly, the sample considered for estimate of column (7) comprises only Highly Indebted Countries (HICs) so both HIPCs and other heavily indebted countries that did not benefit from the HIPC initiative (HICs' sample: Bangladesh, Cambodia, Egypt, Jordan, Kenya, Morocco, Nepal, Nigeria, Pakistan, Peru, Vietnam, and Yemen). OLS estimates using DHS sampling probability weights. Robust standard-errors clustered at the country X survey-year (CXS) level are exposed in parentheses. Constant terms not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10%.

Estimates without taking into account overlapping

Table S.A17: Benchmark results with overlap

Estimator: LPM	
Dep. var.	Primary School Attendance (at least 1 year)
POST_DP	0.117***
	(0.02)
Observations	805544
R-squared	0.314
p-value (F-test)	0.000
Micro controls	Yes
Macro controls	Yes
No. of countries	40
No. of indiv. treated	311504
Country FE	Yes
Survey-Year FE	Yes
Cohort FE	Yes

Notes: Robust standard-errors clustered are exposed in parentheses. ***, ** and * denote significance at 10%, 5% and 1%.

Initial level of education and educational trends.

Table S.A18: Interaction with initial level of education

Estimator: LPM	(1)	(2)	(3)	(4)
Dep. var.:	Primary School Attendance (at least 1 year)			
POST_DP	0.209***		0.310***	
	(0.04)		(0.04)	
POST_DP x Initial level of education	-0.001**		-0.387***	
	(0.00)		(0.06)	
POST_DP x Duration of exposure		0.015		0.022*
		(0.01)		(0.01)
POST_DP x Duration of exposure x Initial level of education		-0.000		0.001
		(0.00)		(0.02)
Observations	602881	602881	446911	446911
R-squared	0.321	0.321	0.336	0.333
Initial level of education	GER in 1999 (World Bank)	GER in 1999 (World Bank)	NER prior to 2000 (DHS)[a]	NER prior to 2000 (DHS)[a]
Micro controls	Yes	Yes	Yes	Yes
Macro controls	Yes	Yes	Yes	Yes
Country-FE	Yes	Yes	Yes	Yes
Survey-Year FE	Yes	Yes	Yes	Yes
Cohort-FE	Yes	Yes	Yes	Yes
Clustering	CXS	CXS	CXS	CXS

Notes: OLS estimates using DHS sampling probability weights. NER stands for net enrollment rate and GER for gross enrollment rate. [a] : NER are computed using, for each country, DHS closest to 1999 and excluding those before 1996. Robust standard-errors clustered at the country X survey-year (CXS) level are exposed in parentheses. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10%.

Table S.A19: Reduced control group (according to the initial level of education)

Estimator: LPM	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. var.:	Primary School Attendance (at least 1 year)							
POST_DP	0.112***	0.124***	0.120***	0.092***				
	(0.02)	(0.02)	(0.02)	(0.03)				
POST_DP × Duration of exposure					0.013***	0.019***	0.021***	0.018***
					(0.00)	(0.00)	(0.00)	(0.00)
Non-hipc countries selected	All	PSE <105	PSE <100	PSE <95	All	PSE <105	PSE <100	PSE <95
Observations	623,888	481,012	442,279	437,957	623,888	481,012	442,279	437,957
R-squared	0.330	0.305	0.304	0.303	0.329	0.305	0.304	0.303
Micro controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Macro controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	CXS	CXS	CXS	CXS	CXS	CXS	CXS	CXS

Notes: OLS estimates using DHS sampling probability weights. Robust standard-errors clustered at the country X survey-year (CXS) level are exposed in parentheses. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10%.

Table S.A20: Controlling for country-specific time trends

Estimators: LPM	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dep. var.:	Primary School Attendance (at least 1 year)											
POST_DP	0.042**	0.103***			0.080***	0.100***			0.137***	0.172***		
	(0.02)	(0.02)			(0.02)	(0.03)			(0.02)	(0.03)		
POST_DP×			0.002	0.009			0.010**	0.011*			0.018***	0.025***
Duration of exposure			(0.01)	(0.01)			(0.00)	(0.01)			(0.00)	(0.00)
Observations	1548492	623888	1548492	623888	1548492	623888	1548492	623888	1548492	623888	1548492	623888
R-squared	0.235	0.330	0.235	0.329	0.250	0.341	0.250	0.341	0.259	0.349	0.259	0.349
p-value (F-test)	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
No. of countries	44	40	44	40	44	40	44	40	44	40	44	40
No. of indiv. treated	535,749	289,971	535,749	289,971	535,749	289,971	535,749	289,971	535,749	289,971	535,749	289,971
HIPC-specific yob trend	Yes	Yes	Yes	Yes	.	.	.	.	.	.	.	.
Country-specific yob trend	.	.	.	.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific yob trend^2	.	.	.	.	.	.	.	.	Yes	Yes	Yes	Yes
Individual controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Country-level controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: As reported in the Table, each regression includes country- and individual-level control variables, the same as for the baseline regressions. Regressions also includes, country, survey-year, and year of birth cohorts fixed effects. OLS estimates using DHS sampling probability weights. Robust standard-errors clustered at the country X survey-year (CXS) level are exposed in parentheses. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10%.

Table S.A21: Investigating the public finance channel

	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
CHANNEL	Govt.Educ.Exp.	Debt service savings from debt relief (DSS)					
Debtor History (DH)				Good		Bad	
Duration	0.009						
	(0.01)						
Duration X CHANNEL	0.002	0.021**		0.025**		0.019*	
	(0.00)	(0.01)		(0.01)		(0.01)	
Duration X CHANNEL_HIPC			0.017*		0.029**		0.013
			(0.01)		(0.01)		(0.01)
Duration X CHANNEL_MDRI			0.029**		0.023		0.030*
			(0.01)		(0.02)		(0.02)
Conditional effect w/r to DH							
Duration X CHANNEL X DH				-0.006		0.006	
				(0.01)		(0.01)	
Duration X CHANNEL_HIPC X DH					-0.016		0.016
					(0.02)		(0.02)
Duration X CHANNEL_MDRI X DH					0.008		-0.008
					(0.02)		(0.02)
Observations	623,888	616,066	616,066	616,066	616,066	616,066	616,066
R-squared	0.317	0.328	0.328	0.328	0.328	0.328	0.328
No. of countries	44	40	40	40	40	40	40

Notes: Debt service savings from debt relief have been computed using debt service information from the *Statistical update* about the Heavility indebted poor countries (HIPC) initiative and Multilateral Debt Relief Initiative (MDRI) of September 2017 (IMF). Debt service savings have been computed by the authors as the difference between the debt service due before the debt relief initiative and the one recorded after these initiatives have been implemented. OLS estimates using DHS sampling probability weights. Robust standard-errors clustered at the country X survey-year (CXS) level are exposed in parentheses. Country and survey-year fixed as well as prior controls are imposed. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10%.