Less debt, more schooling? Evidence from cross-country micro data

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Abstract

Soaring levels of public debt in low-income countries are fuelling concerns about their ability to achieve the Sustainable Development Goals such as free access to primary education. In the late 1990s and 2000s, international financial institutions introduced a series of debt relief initiatives aimed to restore debt sustainability among highly indebted countries. This study examines the impact of these initiatives on primary school attendance. We exploit the temporal variation in the implementation of these policies, in combination with individual-level data from 177 Demographic and Health Surveys covering more than 1.5 million school age children from 44 low-income countries to implement difference-in-differences and spatial difference-in-discontinuity estimators. Results suggest that debt relief initiatives, by freeing up additional public resources, have significantly contributed to increasing primary school attendance among heavily indebted countries. Impact heterogeneity analysis also shows that debt relief has been effective at reducing wealth-based, intergenerational, religious, ethnic and spatial inequalities in education. Our results provide robust evidence to assert that debt relief, in combination with other financing sources, can contribute to improving educational outcomes in highly indebted poor countries.

Keywords: Debt relief, education, financing for development.

JEL codes: F34, H63, I21, I22, I25, I28.

1 Introduction

Over the past decade, low-income countries (LICs), have experienced a significant increase in their level of public debt. A recent World Bank report draws attention to the increasing reliance on private creditors by governments in LICs as a major threat to the sustainability of their public debt (World Bank, 2020). [1]

The loan volumes, conditions and interest rates charged by private creditors tend to be higher than those of international financial institutions (IFIs) (Boz, 2011), which in turn can contribute to increasing debt servicing, and ultimately lead to public resources being channelled to debt repayment rather than to economic and social development programs. In the area of education, for instance, despite significant improvements in primary school attendance rates since the World Declaration on Education for All in 1990, and the subsequent adoption of the Millennium Development Goals in 2000 (Niño-Zarazúa, 2016; Riddell and Niño-Zarazúa, 2016), estimates indicate that about 258 million children aged 6 to 17 were still out of school in 2018 (UNESCO, 2019). Thus, unfavourable borrowing conditions raise concerns about the feasibility of achieving the sustainable development goals.

This study investigates whether debt relief initiatives can be a credible alternative for development financing, especially in the current context of debt-distressed LICs. Although debt relief initiatives are now more than twenty years old and their effects on recipient countries' development have been extensively studied, there is still little evidence on the role that such initiatives might have played in advancing educational outcomes. This paper aims to fill this gap by investigating the effects of debt relief initiatives on primary school attendance. We exploit the original context of the Enhanced Heavily Indebted Poor Countries (HIPC) initiative and of the Multilateral Debt Relief Initiative (MDRI) to adopt a multi-level empirical approach, combining children-level and country-level data from 177 Demographic and Health Surveys (DHS) for 44 countries (both HIPCs and non-HIPCs) between 1990 and 2015, covering more than 1.5 million children eligible to attend primary education. The time covered in this study is much longer than in previous studies, allowing us to study both short- and long-term effects of debt relief on school attendance.

The empirical strategy relies on a difference-in-differences (DiD) model where "treated" individuals are school-age children living in HIPC during the post-debt relief period, while the "control" group is made up either of school-age children living in the same country before debt relief was granted, or school-age children living in a country which did not benefit from the Enhanced HIPC (and hence MDRI) initiatives. Consequently, while the dependent variable is observed at the child level, the variable of interest - exposure to the Enhanced HIPC initiative - is observed at the cohort (year of birth)-country-survey year level. The inclusion of country, cohort and survey-year fixed

[1] In 2013, 4% of LICs with a Debt-Sustainability Analysis were in a position of debt distress, while 19% had a high-risk public debt rating. By 2019, just before the onset of the Covid-19 pandemic, the levels of debt had deteriorated substantially, with 15% reaching a position of debt distress, while 50% having a high-risk public debt rating (International Monetary Fund, 2019).

effects allows us to observe the relationship between debt relief initiatives and the likelihood of being attending primary school, keeping age-cohort, country and survey-year characteristics constant, compared to trends in primary school attendance in control countries. In addition, time-varying controls both at the micro and macro levels allow us to control for the effects of children's characteristics as well as trends in aggregate determinants of primary school attendance among LICs.

Results show that school-age children at the time their countries participated in debt relief initiatives had a higher probability, of around 10 percentage points, of attending primary school. These results are robust to multiple robustness tests such as the introduction of a large number of additional controls and the estimate of a spatial difference-in-discontinuity model.

In investigating the potential mechanisms, we find that debt relief programs increased school attendance partly because they led to an increase in public expenditure in education and because they freed up resources (fiscal space) that could be spent on education. We find that the strength of the relationship is mostly driven by debt relief granted under the Enhanced HIPC initiative, which highlights the potential contribution of conditionalities attached to this program. Furthermore, we also find that the magnitude of the debt relief contribution depends on the amount of debt cancellation as well as on the country's debtor history.

Since within-country disparities in education persist in many countries, we use the micro dimension of the data to investigate whether debt relief initiatives have contributed to reducing educational inequalities. Thus, our study contributes to a growing literature that examines inequalities in education in terms of gender, wealth, household structure, religion, ethnicity and spatial characteristics. Results show that debt relief helped reduce wealth-based and intergenerational inequalities in education by affecting more children from poor and uneducated households. Debt relief also disproportionately affected children from ethnic and religious minority groups as well as those with non-Christian religious background, thus contributing to reducing educational inequalities based on ethnicity and religion. Finally, debt relief is found to reduce spatial educational disparities by affecting more children residing in rural and remote areas.

The remainder of the paper is organized as follows. Section 2 presents the two debt relief initiatives examined in this study, and their expected effects on education. Section 3 describes the data and the empirical strategy. Section 4 presents the main results, multiple robustness tests, and the potential mechanisms at play. Section 5 investigates the effect of debt relief on several educational inequalities whereas Section 6 concludes.

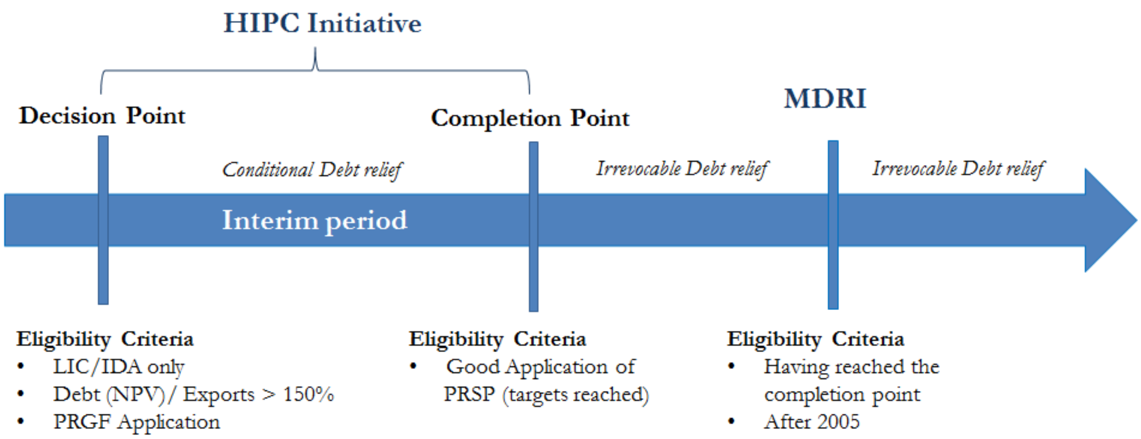
2 Potential links between debt relief and education

2.1 Debt relief initiatives

In response to the increasing debt burden of many governments in LICs at the end of the 1980s, the International Monetary Fund and the World Bank initiated in 1996 a large-scale debt relief program, the Heavily Indebted Poor Countries initiative (HIPC I). The implementation of this first-ever coordinated debt relief effort stemmed from the failure of traditional debt measures by the Paris Club to effectively restore debt sustainability among LICs (Daseking and Powell, 1999; Ferry and Raffinot, 2019).[2].

The initial version of the HIPC initiative (HIPC I) aimed at writing off around 90% of the bilateral debt claims and, for the very first time, cancelled some multilateral liabilities. HIPC I required countries to meet various eligibility criteria.[3] The over-indebtedness requirement was lowered down to 150% of the exports (in present value) in 1999 under the Enhanced HIPC initiative (or HIPC II) since it was considered as too stringent in the prior version, hence preventing poor and highly indebted countries from benefiting from debt cancellations (Thugge and Boote, 1997). The delivery of debt relief, based on a two-stage process reported in Figure 1 below, was also sped up.

Figure 1: Debt relief initiatives for LICs



Once deemed to be eligible for the HIPC initiative, a country reached the "decision-point" at which debt service relief is granted. Debt cancellations were nevertheless made conditional upon the implementation of a Poverty Reduction Strategy Paper (PSRP), with the aim of increasing social spending and boosting poverty reduction efforts (Gautam, 2003; Independent Evaluation Group, 2016). Compared to previous official debt restructurings, the HIPC initiative explicitly required countries entering the program to commit themselves to actions in social sectors to reduce poverty, particularly in the health and education sectors. Public investments in those sectors were also

[2]LICs' insolvency prior to 1996 was addressed by conventional measures such as rescheduling interest and capital payments

[3]1) Having a debt-to-exports ratio greater than 250% in present value; 2) being classified as a LIC by the World Bank and 3) having implemented an IMF macro-stabilizing program. Public finance reforms defined within the PRGF mostly focused on fiscal deficit reduction and improvement in domestic resource mobilization (Ferry, 2019; Ghosh et al., 2005). There were no particular requirements regarding education spending, and, overall, no incentives for increasing poverty-reduction expenditures as these objectives were the heart of the subsequent PSRP implemented from the decision point on.

expected to be funded with debt service savings stemming from the debt service relief. Table S.A1 in the Supplementary Appendix presents the educational targets set under the HIPC initiative. Many HIPC countries directly committed themselves to increasing school attendance. To this end, they used various tools: increasing public spending in education, reducing the cost of schooling through the elimination of school fees, providing more school inputs (construction of schools, recruitment and training of teachers, teaching material), and changing school management. Once the targets set up in the anti-poverty strategy were achieved (or sufficient progress was made), the country reached the "completion point", which marked the end of the HIPC process, allowing governments to benefit from additional and irrevocable debt relief on its stock of debt by an amount determined ex-ante and up to a certain limit.

In addition to the HIPC initiative, country members of the Group of Eight (G8) and international financial institutions agreed during the Gleneagles summit of 2005 to reinforce debt relief efforts for LICs through the Multilateral Debt Relief Initiative (MDRI), with the objective of releasing financial resources to support the MDG targets. They committed to cancel the outstanding multilateral debt of those countries that had reached their completion point under the Enhanced HIPC initiative.[4] As of 2021, 37 countries have benefited from debt relief under the HIPC and MDRI initiatives, which provided \$76 billion in debt-service relief over time. As a result, between 2001 and 2015, debt service paid declined by around 1.5 percentage points of GDP among beneficiary countries.

2.2 Expected impacts of debt relief on education

The HIPC and MDRI initiatives could have helped increase primary school attendance through several channels. First, the "Debt Overhang" theory provides the theoretical foundations for debt relief effects, predicting a situation in which public debt is so large that it slows down capital accumulation, and threatens the capacity of highly-indebted low-income countries to repay the remaining liabilities (Krugman, 1988; Sachs, 1989). According to Sachs (1989), significant scaling-up in public indebtedness fuels heavy debt repayments that ultimately crowd out public investment and social expenditures (in the education sector for instance), thus undermining economic growth through the deterioration of infrastructure and human capital accumulation. This mechanism, termed the "real burden effect", intuitively illustrates how debt relief efforts can positively affect educational outcomes in beneficiary countries. When heavy public debt servicing absorbs a substantial proportion of public resources, part of which is dedicated to education, debt relief could help to generate the "fiscal space" needed to support the education sector with resources that were initially intended for debt servicing (Heller, 2005).

A second possible channel through which the Enhanced HIPC initiative could have impacted public education spending is through the explicit strong conditionalities attached to the initiative, in which further debt cancellations rested upon the sound use of debt service savings for social sectors

[4] Multilateral debt owed to the World Bank, the International Monetary Fund, and some Regional Development Banks prior to 2003/2004.

such as education and health. The HIPC initiative could therefore have had a direct impact on education as it required recipient countries to invest freed-up resources in education. It could also have a more indirect impact on education. And because countries were also required to invest in the health sector, the HIPC initiative may have indirectly impacted school attendance by improving the health condition of children. A third possible channel through which the Enhanced HIPC initiative could have impacted public education spending is via the required implementation of macroeconomic and structural reforms and other poverty-reducing programs that could have boosted economic growth and reduced poverty, leading to a potential increase in education.[5] Thus, the combination of a fiscal space together with conditionalities is expected to have provided, throughout the interim period, the financial means and sound use of fiscal resources to achieve the primary education goals set within the PSRP.

The formation of a "fiscal space" induced by debt relief has been investigated by a small number of studies. While studies from the early 2000s conclude that there are no sizable effects from debt relief initiatives (Chauvin and Kraay, 2005; Presbitero, 2009), more recent studies find a positive effect of official debt cancellation on government expenditures. For example, Thomas (2006) shows that a decline in debt-service significantly increases social expenditure. Cassimon et al. (2015) provide evidence of a positive correlate between increased debt service savings and larger current and capital public spending, using a longer post-debt relief period. Cassimon et al. (2015) and Djimeu (2018) also find that public investment is more responsive to debt relief granted under the Enhanced HIPC initiative, compared to the MDRI, because of the stronger conditionality associated with the former.

To our knowledge, only two studies have investigated the effects of debt relief specifically on education expenditures. First, Dessy and Vencatachellum (2007) investigate the effect of debt relief granted to African countries on aggregated education and health expenditures between 1989 and 2003. The authors find that debt relief is positively associated with education spending, mostly for HIPCs with sound institutions, although their findings are sensitive to the measure of debt relief, which can be explained by the limited time window of the analysis. Second, Cuaresma and Vincelette (2008) focus on a more recent period (1998-2005) to investigate the effect of debt relief for 33 HIPC countries that had reached their decision point, compared to fewer than 10 HIPCs in Dessy and Vencatachellum (2007)'s sample. Their results, which are based on propensity score matching and a sample selection (Heckman) model, show no effect of debt relief on government education spending and student-teacher ratios.

The specific question of whether freed-up resources from debt relief have been used to finance education spending and improve educational outcomes remains open. Based on the existing literature, we suspect that most of the effects of debt relief initiatives on education are channelled through a liquidity shock resulting from the provision of debt service savings and the required conditionalities

[5] For instance, some HIPC countries, such as Bolivia and Burkina Faso, implemented rural development policies.

imposed by IFIs. Ex-ante conditionalities, focusing mainly on stabilizing the public deficit are not be expected to have impacted education as it is discussed and confirmed by various tests that we conduct in Section 4.3.

3 Empirical approach

3.1 Data

We use the Demographic and Health Surveys (DHS), which are nationally representative household surveys standardized across countries and over time. For the purpose of this study, we collected DHS data for all available developing countries between 1990 and 2015, both HIPCs and non-HIPCs. We restricted the sample to HIPCs that had at least one DHS before and after the year they reached their decision point and thus started receiving debt relief.[6] Figures S.A1 and S.A2 in the Supplementary Appendix present the evolution of the database.

Since we assess the impact of debt relief on primary school attendance, we focus on children that were of primary school age at the time of the survey. For that purpose, we draw on the UNESCO (2020) database and use the official entrance age to primary school in each country to identify eligible children.[7] Children kept in the final sample are on average between 6 and 12 years old. Primary school attendance is measured at the extensive margin and consists of a dummy variable equal to 1 if a child attended primary school for at least one year while being of primary school age, and zero otherwise.[8] Because two consecutive rounds of surveys were conducted in some countries within a short period of time, the same individuals may appear twice in the data. In order to avoid double counting, we compute year-of-birth thresholds that prevent the same age-cohorts appearing twice in two consecutive DHS conducted in a given country.[9]

[6]As most HIPCs reached their decision point in 2000, for non-HIPCs, we only consider countries with at least two rounds of DHS; one before 2000 and one after.

[7]The official entrance age captures the age at which children would enter a given level of primary education assuming they start at the official entrance age for the lowest level of primary education, and that they study full-time throughout and progressed through the system without repeating or skipping a grade. We acknowledge that these criteria may not always apply. Therefore, the possibility of measurement error from repetition rates or late school entrance rates may yield lower bound estimates.

[8]We use question hv106 in the DHS questionnaire that collects information on the highest level of education household members attended. This is a standardized variable providing level of education in the following categories: No education, Primary, Secondary, and Higher. Specifically, the question asks: "What is the highest level of school (NAME) has attended?". With this information, we were able to identify the total number of children that attended at least one year of primary school - regardless of age – relative to the school-age population in country x at year t. Thus, our indicator of primary school attendance is a close approximation of primary school gross attendance.

[9]For instance, in Nigeria, one survey was conducted in 2008 and another in 2013. Given the selection strategy, we kept children aged 6 and 12 years at the time of the survey, i.e., those individuals born between 1996 and 2002, and those born between 2001 and 2007 for the 2008 and 2013 DHS rounds, respectively. However, individuals born in 2001 and 2002 could have been surveyed in both rounds. To avoid double counting, we therefore restrict the sample for the 2008 round to children born between 1996 and 2000. Tables S.A2 and S.A3 in the Supplementary Appendix discuss in more detail the potential bias from this selection

Overall, we were able to sample 1,548,492 primary school-age children from 177 DHS covering 44 countries (22 HIPCs and 22 non-HIPCs), including 535,749 individuals potentially affected by the Enhanced HIPC Initiative. 80% of the children in the sample attended primary school for at least one year. However, when we enter parental education as a control in the econometric analysis, the overall sample is reduced to 962,944 individuals. Tables S.A4 and S.A5 in the Supplementary Appendix report samples and surveys used for non-HIPCs and HIPCs, respectively. Table S.A7 in the Supplementary Appendix presents the main descriptive statistics.

3.2 Empirical Specification

In order to assess the effect of debt relief under the Enhanced HIPC initiative and the MDRI on the probability of primary school attendance, we implement a difference-in-differences (DiD) strategy, which consists of estimating the effect of living in a HIPC and being of primary school age during the post-decision point period on primary school attendance. The specification can be expressed as follows:

$$PS\_ATTEND_{i,a,c,j} = \alpha + \beta POST\_DP_{a,c,j} + \phi H_{i,a,c,j} + \gamma X_{c,j} + \delta_c + \eta_a + \rho_j + \epsilon_{i,a,c,j} \qquad (1)$$

where the dependent variable $PS\_ATTEND_{i,a,c,j}$ is a dummy variable equal to one if the child i of age-cohort a living in country c, and observed in the survey year j attended primary school for at least one year, and zero otherwise. The variable of interest, $POST\_DP_{a,c,j}$ is a dummy variable identifying cohorts a of children of primary school age, 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. Therefore, in order to be considered as treated, cohorts of children must satisfy three conditions: (1) they must live in a HIPC; (2) they should be of primary school age by the time their country reached the decision point; and (3) they should be observed after the country began the debt relief process.[10] Conversely, $POST\_DP_{a,c,j}$ is equal to zero for two groups of children: first, children living in non-HIPC countries, and second, children living in HIPCs but who were either too old to be attending primary school when the country benefited from the Enhanced HIPC initiative or were in the eligible age-cohort but observed prior to the decision point year.

The vector $H_{i,a,c,j}$ captures individual and household-level controls including child gender, parental education, relationship to the head of household, household wealth and place of residence.[11] Macroe-

process.

[10]Table S.A6 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 have been born in 1988 (or later) and be observed in 2000 (or later).

[11]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. See Figure S.A3 and Table S.A8 in the Supplementary Appendix for more details.

conomic covariates, denoted $X_{c,j}$, include GDP per capita and the under-15 population.[12]

Country fixed effects, which are measured by δ_c in equation 1, capture time-invariant country-specific characteristics and remove the structural conditions at the country-level that explain differences in primary school attendance as well as time-invariant factors influencing participation in 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. We also include year-of-birth fixed effects, denoted by (η_a), to control for potential age-cohort related events that may affect primary school attendance. In addition, since we pooled data from multiple DHS rounds, we also account for potential differences in questionnaires and sampling frames by including survey-year fixed effects, which are denoted by (ρ_j).[13] Finally, $\epsilon_{i,a,c,j}$ is the idiosyncratic disturbance term. To account for potential serial correlations within age cohorts in each sample country and within survey wave, we impose a double clustering of standard errors at both the country $\times$ survey-year and country $\times$ year-of-birth levels.

The DiD parameter, β, measures the effect of debt relief under the HIPC initiative on the within-country probability of having attended primary school, compared to what it is observed in non-HIPCs, after controlling for generational effects, individual characteristics and changes in the macroeconomic environment. In order to check whether the outcome variable did not diverge between HIPCs and non-HIPCs before the implementation of debt relief, we apply several parallel trends tests using various specifications. Section 3 in the Supplementary Appendix discusses the importance of ex-ante parallel trends. Results in Table S.A9 support the existence of an ex-ante common trend regarding primary school attendance between children in HIPCs and non-HIPCs, giving us confidence about the assumptions underpinning the difference-in-differences empirical strategy, and supporting the absence of effects stemming from ex-ante conditionalities associated with eligibility criteria.

4 Main results

4.1 Average effect of debt relief on education

Table 1 presents the DiD estimation results based on DHS sampling weights. Column (1) shows the unconditional effect of being of primary school age during an HIPC's post-decision period on the probability of having attended primary school. Imposing no controls but fixed effects, estimate results, based on the full sample of 1,548,492 children, 535,749 of whom are considered as "treated",

[12] Both per capita GDP at 2010 constant US dollars and under-15 population are taken from the *World Development Indicator (WDI)* database and are expressed in logarithm.

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

suggest that the exposure to the HIPC initiative increased the likelihood of having attended primary school for at least one year by 11 percentage points.

Table 1: Main results

Estimators: DiD	(1)	(2)	(3)	(4)
Dep. var. ($PSA_{i,a,c,j}$):	Primary School Attendance (at least 1 year)			
$POST\_DP_{a,c,j}$	0.110***	0.161***	0.149***	0.102***
	(0.02)	(0.02)	(0.02)	(0.02)
$Girl_{i,a,c,j}$		-0.027***	-0.027***	-0.027***
		(0.01)	(0.01)	(0.01)
Parent Educ: $Primary_{i,a,c,j}$		0.195***	0.191***	0.191***
		(0.01)	(0.01)	(0.01)
Parent Educ: Sec. or $tertiary_{i,a,c,j}$		0.218***	0.218***	0.219***
		(0.02)	(0.02)	(0.02)
Head's $child_{i,a,c,j}$		0.002	0.002	0.001
		(0.00)	(0.00)	(0.00)
Wealth $index_{i,a,c,j}$		0.033***		
		(0.00)		
1st wealth quintile $(Q1)_{i,a,c,j}$			-0.122***	-0.121***
			(0.01)	(0.01)
2nd wealth quintile $(Q2)_{i,a,c,j}$			-0.086***	-0.086***
			(0.01)	(0.01)
3rd wealth quintile $(Q3)_{i,a,c,j}$			-0.063***	-0.064***
			(0.01)	(0.01)
4th wealth quintile $(Q4)_{i,a,c,j}$			-0.033***	-0.033***
			(0.01)	(0.01)
Rural		-0.057***	-0.064***	-0.064***
		(0.01)	(0.01)	(0.01)
GDP per cap. (log, const. $USD)_{c,j}$				-0.023
				(0.04)
Population under 15 $(log)_{c,j}$				0.272***
				(0.09)
Observations	1,548,492	962,944	962,944	960,010
No. of indiv. treated	535,749	412,972	412,972	412,972
No. of countries	44	41	41	41

Notes: DiD estimates using DHS sampling probability weights. Robust standard-errors clustered at both the country × year-of-birth and country × survey-year levels are shown in parentheses. All regressions include country, survey-year and cohort fixed effects. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10% levels.

Column (2) presents the results when we extend the specification with individual characteristics. As expected and in line with Baker and Jacobsen (2007); Lavy (2008); Pitt et al. (2012), girls are significantly less likely to attend primary school, likely because of lower labour market opportunities. Not surprisingly and in line with Buchmann and Brakewood (2000); Colclough et al. (2000); Glick and Sahn (2000); Huisman and Smits (2009) and Lincove (2015), children with educated parents are more likely to attend primary school. In keeping with Huisman and Smits (2009), this probability also seems to be higher for children of household heads. The likelihood of attending primary school is lower for children living in rural areas, which can partly be explained by the difficulty for people living in remote areas to reach school facilities (Fafchamps and Wahba, 2006; Huisman and Smits, 2009). As already shown by Glick and Sahn (2000); Huisman and Smits (2009) and Lincove (2015), children from richer households are more likely to attend schools. This is observed in column (3) where we present the results with the wealth index disaggregated by quintiles for each country and survey-year. Children living in poorer households are less likely to attend primary school than those belonging to the wealthiest quintile. The gap is gradually reduced as we get closer to the fifth quintile, suggesting that the likelihood of attending primary school is a linear function of household wealth. As underlined before, debt relief could have an effect on school attendance through several mechanisms, including a potential reduction in overall poverty. To the extent that household wealth is a proxy for poverty, when this variable is included, the conditional effect of debt relief is probably due to other mechanisms such as the fiscal space channel. Overall, the results indicate that conditional upon these individual features, being exposed to the Enhanced HIPC initiative has a large and positive effect, in the order of 15 percentage points, on primary school attendance.

We then include country-level controls to see whether the observed debt relief effect on primary school actually reflects the contribution of other time-varying country-specific developments occurring at the same period. Including the per capita GDP and the under 15 years old population in column (4) does not affect the significance of the HIPC initiative but it reduces the size of the coefficient to 10 percentage points.[14]

4.2 Timing of effects

In Table 1, we have presented the average treatment effects of having been exposed to debt relief initiatives on the probability of having attended primary school. However, the duration of exposure to debt relief is not homogeneous among "treated" children. For instance, some children might have been observed at the age of 12 or 7, six years after their country reached the decision point. The former would thus have been exposed to debt relief for six consecutive years and would probably have had a greater chance of having attended primary school compared to children aged seven who had been exposed to debt relief for just one year. In order to investigate heterogeneous effects of the duration of exposure, we estimate equation 1 but change the $POST\_DP$ dummy variable for a continuous measure of the years of exposure to debt relief, which is linked to the age of the children at the time of the survey, the theoretical entrance age, and the year their home country reached the decision point. This new measure ranges from 0 to 6, and potentially 7 in some countries. Column (1) of Table A1 in the Appendix shows no significant conditional correlation of being exposed for an additional year on the probability of having attended primary school. Column (2) splits the linear duration of exposure in categories in order to estimate the effects of different exposure's duration on

[14]The fact that the coefficient associated with the treatment variable remains significant even after controlling for differences in GDP over time suggests that the effect of debt relief on education is not entirely determined by the potential increase in GDP following the implementation of macro-stabilizing reforms.

primary school attendance. Results suggest that most of the effect is observed for children exposed between 2 and 5 years to the debt relief initiatives.

It is reasonably to assume that the debt relief effect on education is observed with a lag, as building schools and deploying teachers in less favoured areas within HIPCs takes some time to materialized. This could explain why we observe a significant effect only after at least two years of exposure. In order to further assess the timing of debt relief effects, we interact the $POST\_DP$ variable with dummy variables measuring whether children are observed over a period of 5 years after, between 6 and 10 years after, or more than 10 years after the decision point, respectively. Table A2 in the Appendix displays a fairly homogeneous effect of the debt relief initiatives on primary school attendance irrespective of the period during which "treated" children were surveyed, thus suggesting that debt relief effectiveness was not transitory but contributed to improving primary school attendance over the medium and long-run.

4.3 Robustness checks

The above results suggest that the positive effect of debt relief on primary school attendance is robust to the inclusion of two macroeconomic determinants (GDP per capita and population under 15). However, one could ask whether the coefficient associated with the $POST\_DP$ variable fully reflects the contribution of debt relief to primary school attendance and not of other economic and institutional factors or concurrent programs. There may also be concerns about sample dependence and the sensitivity of the results to clusters. Moreover, countries that benefited from the HIPC initiative had significantly lower primary school attendance rate before the program began. Thus, HIPCs had greater room for improvement in terms of primary school attendance with respect to control group countries, which could partly explain the positive effects we observe. We conduct a series of robustness checks to investigate possible threats to the validity of findings, including additional country-level controls, the possibility of confounding effects from official development assistance and large scale education programs, possible sample dependence issues as well as sensitivity to clusters and educational trends. Overall, the results hold (see Section 4 in the Supplementary Appendix for a a more detailed discussion).

4.4 Spatial difference-in-discontinuity as an alternative model

The baseline results suggest a positive effect of debt relief on primary school attendance. While the various robustness tests partially attenuate some of the classical econometric issues that usually arise when assessing the effectiveness of a national-scaled program, some concerns remain about the identification of the causal effect of debt relief. One of the shortcomings of the baseline DiD specification is the difficulty of controlling for the effect of unobserved individual characteristics, such as preferences for education or the quality of education, which could affect the probability of attending primary school. Addressing the potential biases from unobserved individual heterogeneity is challenging in the absence of a random assignment to treatment.

In order to mitigate the threat of unobserved individual heterogeneity in the impact estimates, we advance our identification strategy and implement a spatial difference-in-discontinuity model that allow us to estimate the effect of debt relief on primary school attendance in localities close to country borders. This strategy is motivated by the idea that, in the context of developing countries, especially in sub-Saharan Africa, national borders were defined in a discretionary manner by former colonial powers, so the distribution of unobserved individual characteristics on both sides of the border are likely to be randomly assigned.

For operationalization of the difference-in-discontinuity model, we use geolocation data from DHS respondents to compute the straight distance ($Dist$) between each HIPC (resp. non-HIPC) respondent's enumeration area location and the national border with their non-HIPC (resp. HIPC) neighbors.[15] The spatial difference-in-discontinuity model takes the following form:

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

with
$$\begin{aligned} f(Distance) &= \lambda Dist._{i,a,c,j} \text{ , or alternately} \\ &= \lambda Dist._{i,a,c,j} + \mu[POST\_DP_{a,c,j} \times Dist._{i,a,c,j}] \text{ , or alternately} \\ &= \lambda Dist._{i,a,c,j} + \Theta Dist.^2_{i,a,c,j} \text{ , or alternately} \\ &= \lambda Dist._{i,a,c,j} + \Theta Dist.^2_{i,a,c,j} + \mu[POST\_DP_{a,c,j} \times Dist._{i,a,c,j}] + \tau[POST\_DP_{a,c,j} \times Dist.^2_{i,a,c,j}] \end{aligned}$$

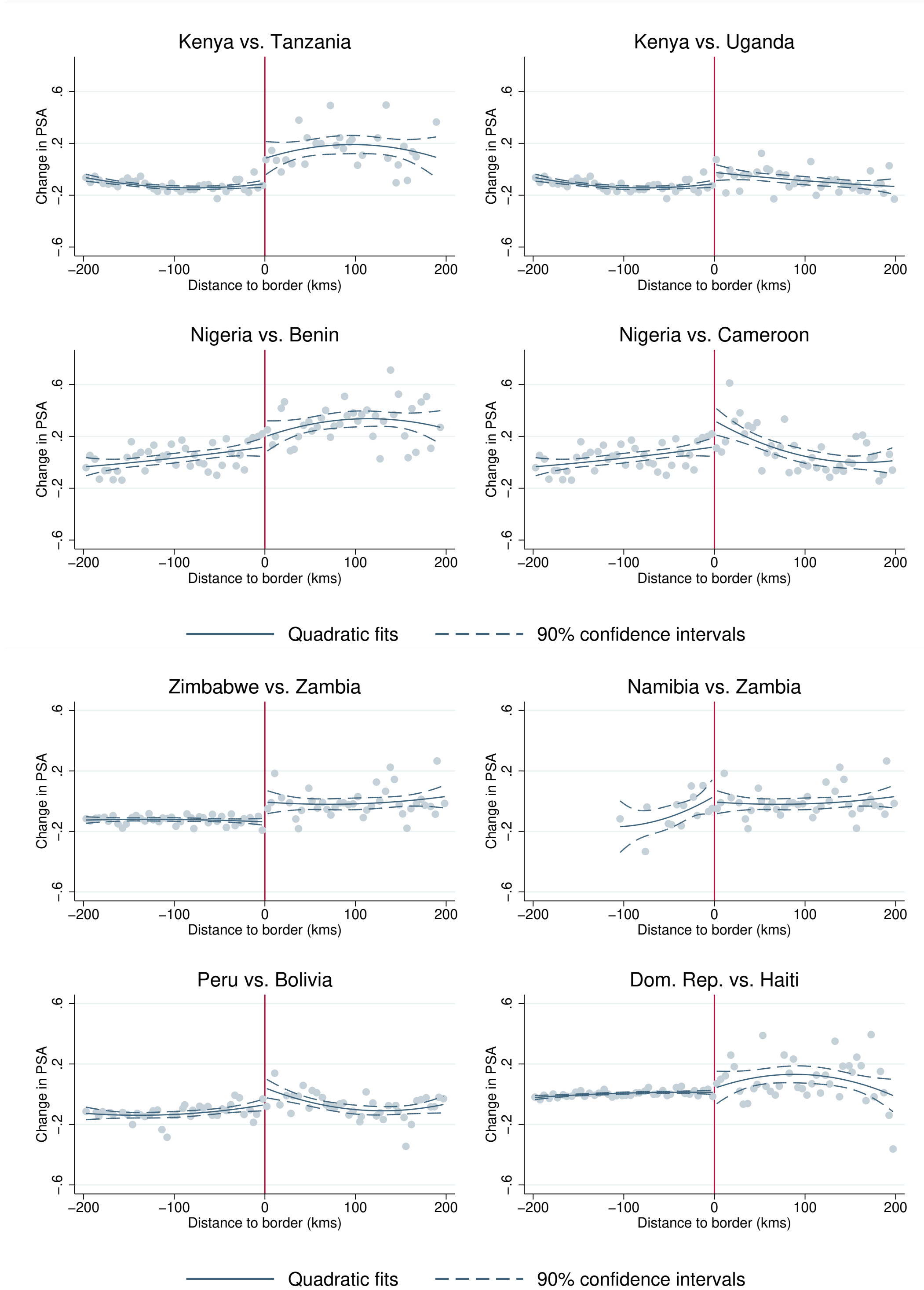
A constraint in our strategy is that latitude and longitude data is not available for all DHS. Indeed, most of the older DHS surveys did not collect geolocation information, which prevented us from having DHS data before and after the decision point year for some HIPCs, and before and after 2000 for some control countries. In order to resolve this constraint, we depart from the original sample covering solely children of primary school age, and consider all respondents, regardless of their age at the time of the interview. We then identified whether these individuals attended primary school for at least one year and, using information on their year of birth, we were able to find out whether they were of school age before or after the debt relief period. This strategy allows us to work with cohorts that were too old to benefit from debt relief, and to add the before/after dimension to the spatial discontinuity design model. When restricting the sample to HIPC countries with non-HIPC border countries and with geo-coded information for old and young cohorts, we are left with eight pairs of countries (Figure 2). Results from this alternative specification produce local average treatment effects and therefore should be interpreted with caution as they draw on a small number of countries.

[15] The DHS program provides for a subset of surveys the longitude and latitude coordinates of respondents' enumeration areas, allowing us to identify the place of residence of interviewed households, with an intentional, and maximum, measurement error of 5km and 2km for rural and urban households, respectively. For a detailed discussion on the spatial treatment of geo-coded DHS, see Section 6 in the Supplementary Appendix.

Our identification strategy could be questionable if individuals had migrated since they went to primary school. In particular, if older cohorts constituting the "before" group were actually attending primary school elsewhere, further away from the border, they could not be considered as a relevant control group. Fortunately, respondents were asked how long they had lived in their current village. Thus, we select individuals who had lived in the same place since they were in primary school. Relying on older cohorts to provide a before dimension has another drawback, as information on parental education is missing for old individuals who no longer live with their parents. For the spatial difference-in-discontinuity design, we therefore remove controls for parental education, relationship to the head of the household, and quintiles of wealth as they are not representative of the wealth situation of older cohorts at the time they were in primary school age.

Figure 2 plots the average change in primary school attendance rates by distance cells around the border within a 200-km window. The difference in primary school attendance is computed by comparing old and young cohorts in each distance cell. Linear quadratic trends on each side of the cut-off are displayed, with dashed lines representing 90 percent confidence intervals. Individuals to the right of the cut-off live in HIPC countries. This figure reveals a significant discontinuous jump in the evolution of primary schooling around the border for 5 out of 8 pairs of countries. Results from the difference-in-discontinuity model are presented in Table 2. Four different bandwidths are used for each specification: 200-km, 100-km, 50-km and 20-km windows around the border. Corroborating the graphical evidence and findings from the baseline model, results from non-spline linear and quadratic models show a significant positive impact of the HIPC initiative on primary school attendance around the border for bandwidths between 200 and 50 kilometers. We test an alternative strategy where we control for parental education and rely on a much more restricted number of countries (Section 4.5 of the Supplementary Appendix). Results based on 20 and 50-km bandwidths support our main findings.

Figure 2: Change in primary school attendance around the border



Notes: This figure displays the graphical difference-in-discontinuity analysis. Dots (measured on the vertical axis) represent changes in primary school attendance (PSA) rates by distance cell around the border. Changes in PSA are computed by comparing attendance before (older cohorts) and after (younger cohorts) the HIPC initiative was implemented in the HIPC country. Distance cells represent distance intervals of five kilometers. Trend lines are obtained using a quadratic spline fit. Dashed lines represent the 90 percent confidence bounds.

Table 2: Difference-in-Discontinuity estimates

	(1)	(2)	(3)	(4)
Dep. var.:	Primary School Attendance$_{i,a,c,j}$ (at least 1 year)			
Bandwidth:	200km	100km	50km	20km
Specification (smooth function for distance)				
Linear				
POST_DP$_{a,c,j}$	0.047*	0.065**	0.067**	0.056
	(0.03)	(0.03)	(0.03)	(0.04)
Linear spline				
POST_DP$_{a,c,j}$	0.017	0.003	0.041	0.004
	(0.03)	(0.03)	(0.04)	(0.05)
Quadratic				
POST_DP$_{a,c,j}$	0.047*	0.066**	0.067*	0.055
	(0.03)	(0.03)	(0.03)	(0.04)
Quadratic spline				
POST_DP$_{a,c,j}$	-0.01	0.01	0.067	-0.019
	(0.03)	(0.04)	(0.05)	(0.05)
Observations	359,810	201,8495	98,883	38,934
No. of indiv. treated	98,739	61,371	31,053	11,859

Notes: Difference-in-discontinuity results stem from estimates using DHS sampling probability weights. Robust standard-errors clustered at both the country × year-of-birth and country × DHS GPS id levels are shown in parentheses. The sample includes Kenya, Tanzania, Uganda, Nigeria, Benin, Cameroon, Zimbabwe, Zambia, Namibia, Peru, Bolivia, Dom. Rep. and Haiti. Each regression includes country, survey-year and cohort fixed effects, controls for gender and rural residence, as well as country-level controls presented in Table 1. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10% levels.

4.5 Debt relief and fiscal space for education

A very scant literature investigating the fiscal space in the aftermath of debt relief initiatives shows that, on average, those programs have led to increases in both current and capital public spending (Cassimon and Campenhout, 2008; Cassimon et al., 2015; Djimeu, 2018). Yet for the main theoretical channel explaining the effect of debt relief on education to materialize, such liquidity shocks need to feed through the education sector. The existence of such "fiscal space for education" seems to be true in this context. Indeed, Figure A1 in the Appendix shows the jump of government education spending around the decision point for HIPC and non-HIPC countries and suggests that debt reductions resulted in additional budgets for funding education expenditure, probably because of the conditionalities attached to the HIPC initiative.

Consequently, countries that benefited from larger debt cancellation, and obtained significant freed up resources, are expected to invest more in education and obtain better results in terms of primary school attendance. In order to capture this public finance channel, we first augment the baseline DiD specification with an interaction term between the "treatment" variable and public expenditure dedicated to the education sector. Results of column (1) in Table 3 suggest that the positive effect of debt relief on primary school attendance is reinforced when the children's home country increased public spending on education over the same period.

Public spending on education is nonetheless financed by sources other than debt relief, such as foreign aid, domestic resources, or non-concessional lending. Thus, following the fiscal space theory and previous empirical studies (Cassimon and Campenhout (2008) and Cassimon et al. (2015)), we consider debt service savings from debt relief as the second proxy for the channel through which debt relief impacts primary education.[16] Using data about debt service before and after both the Enhanced HIPC initiative and the MDRI, retrieved from IMF documents, we estimate debt service savings from the Enhanced HIPC initiative and the MDRI, as well as the aggregate cash-flows resulting from both debt relief initiatives (see Figure A2 in the Appendix). We then interact these cash-flows measured as a percentage of GDP with the "treatment" variable.[17] Results presented in column (2) in Table 3 indicate that an additional debt service saving of one percentage point of GDP is associated with an increase in the likelihood of attending primary school of around 12 percentage points.[18] This suggests that HIPCs benefiting from larger debt service savings recorded the largest improvements in primary school attendance.

Column (3) presents the results of debt service savings separated by debt relief initiatives. The positive correlation between debt service savings and primary school attendance is essentially driven

[16] Debt service savings are measured as 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. This distinction is important as changes in debt service can be attributed to factors that are uncorrelated with debt relief, such as changes in borrowing strategies (Thomas, 2006).

[17] Since this measure is available for HIPCs only and is equal to zero for non-HIPCs, it amounts to replacing the dichotomous treatment variable ($POST\_DP$) by a continuous treatment. This is why in columns (2) to (5) in Table 3 coefficients for $POST\_DP$ are not displayed.

[18] Note that the average debt service savings for a given year is around 0.5 to 0.7 percentage point of GDP.

Table 3: Investigating fiscal space heterogeneity

Dep. var.: PSA	(1)	(2)	(3)	(4)	(5)
Channel (% of GDP):	Govt.Educ.Exp.	Debt service savings from debt relief (DSS)			
Debtor History (DH:)				Good	
$\text{POST\_DP}_{a,c,j}$	0.052*				
	(0.03)				
$\text{POST\_DP X Channel}_{a,c,j}$	0.016**	0.121***		0.037	
	(0.01)	(0.04)		(0.05)	
$\text{POST\_DP X Channel\_HIPC}_{a,c,j}$			0.137***		0.064
			(0.04)		(0.06)
$\text{POST\_DP X Channel\_MDRI}_{a,c,j}$			0.090		0.023
			(0.06)		(0.08)
Conditional effect w/r to DH					
$\text{POST\_DP X Channel X DH}_{a,c,j}$				0.136**	
				(0.06)	
$\text{POST\_DP X Channel\_HIPC X DH}_{a,c,j}$					0.119*
					(0.07)
$\text{POST\_DP X Channel\_MDRI X DH}_{a,c,j}$					0.151
					(0.09)
Observations	926,513	948,574	948,574	948,574	948,574
No. of countries	41	41	41	41	41

Notes: Debt service savings from debt relief have been computed using debt service information from the *Statistical update* about the Heavily 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 and after the debt relief initiatives. DiD estimates are presented using DHS sampling probability weights. Robust standard-errors clustered at both the country × year-of-birth and country × survey-year levels are shown in parentheses. Country, survey-year, and year-of-birth fixed effects 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% levels.

by cash-flows from the Enhanced HIPC initiative. These results are not surprising given that most investments in primary education took place during the interim period, when debt service relief was tied to the explicit conditionalities attached to the Enhanced HIPC initiative. However, if conditionalities were the main channel driving the effect of debt relief on education, we would have expected to find an insignificant effect of the amount of debt service savings, and this was clearly not the case.

Yet, debt service savings are based on the hypothetical level of debt service in the absence of debt relief. But we cannot claim that this hypothetical debt service is what debtor countries would have actually paid in the absence of debt relief (Cohen, 2001). It is likely that "bad" payers, i.e., HIPCs that accumulated large amounts of interest and capital arrears prior to the HIPC initiatives, would have not fully paid their debt service in the absence of debt relief. Debt relief would result in additional cash-flows only if the debtor had honored its debt repayments in the absence of debt relief. Following Cassimon et al. (2015), we thus interact our continuous treatment with a dummy variable capturing "good" payers as those HIPCs that recorded debt service arrears (interest and capital) below 10% of their total debt stock prior the announcement of the original HIPC initiative in 1996. Results reported in columns (4) and (5) of Table 3 suggest that debt service savings primarily benefited children from HIPCs that were more likely to repay their debt in the absence of debt relief, thus reinforcing the existence of the fiscal space channel.[19]

5 Individual heterogeneity effects

One major advantage of our empirical strategy, relative to the previous literature, is the feasibility of investigating potential individual heterogeneous effects with respect to characteristics such as gender, parents' education, household wealth and household structure, and cultural and spatial dimensions. In addition, the individual level structure of our data allows us to see whether debt relief programs helped decrease educational inequalities. In order to investigate these potential non-linearities in debt relief effects, we run both sub-sample regressions and models with interaction terms. Models with interaction terms allow us to assess the extent to which explanatory variables affect individuals differently, with respect to a given characteristic. Formally the interactions model (refer to hereafter as the saturated model) takes the following form:

$$PS\_ATTEND_{i,a,c,j} = \alpha + \beta_1 POST\_DP_{a,c,j} + \beta_2 HET_{i,a,c,j} + \beta_3 HET_{i,a,c,j} \times POST\_DP_{a,c,j} + \delta_c + \eta_a + \rho_j + \phi H_{i,a,c,j} + \gamma X_{c,j} + \sum_k \beta_k (HET_{i,a,c,j} \times k) + \epsilon_{i,a,c,j} \quad (3)$$

with $k = \{\delta_c, \eta_a, \rho_j, H_{i,a,c,j}, X_{c,j}\}$

where HET represents the characteristic on which we test for heterogeneity (gender, household wealth, household structure, parental education, and cultural and spatial characteristics). The last component of the equation, just before the error term, denotes the interaction terms between individual characteristics and key explanatory variables, fixed effects included.

5.1 Gender, wealth and intergenerational inequalities

The Enhanced HIPC and MDRI initiatives were set within the broader development agenda to support the achievement of the Millennium Development Goals by 2015. Within the MDG (and now SDG) framework, countries were urged, in addition to meeting the target of universal primary education, to reduce the gender gaps in education still found in many developing countries (Evans et al., 2020). Gender-specific targets were defined within the poverty reducing plan that HIPCs had to conduct during their interim period (see Table S.A1 in the Supplementary Appendix).[20] Other targets aiming specifically at children from disadvantaged backgrounds were also specified. Debt

[19] These results remain robust to alternative denomination of debt service savings. See Table S.A22 in the Supplementary Appendix where debt service savings are measured in US dollars per capita.

[20] For instance, Bolivia agreed to increase the number of girls completing the 5th grade in rural areas.

relief could in principle have reduced gender and wealth inequalities in education if poorer children and girls actually benefited more from these initiatives than richer children and boys. Results show that while there are no significant differences between boys and girls, debt relief initiatives decreased wealth-based educational inequalities, as they affected more children from poorer households (Table 4).

Furthermore, a growing literature investigating recent trends in intergenerational mobility in education[21] reveals heterogeneous patterns across countries, regions and contexts (Alesina et al., 2020, 2021; Asher et al., 2021; Azam and Bhatt, 2015; Card et al., 2018; Daude, 2011; Fletcher and Han, 2018; Hertz et al., 2008; Narayan et al., 2018) as well as across castes and ethnic and religious groups (Alesina et al., 2020; Asher et al., 2021; Card et al., 2018; Hilger, 2015). In line with this literature, we investigate whether the HIPC initiative helped improve the intergenerational transmission of human capital by disproportionately affecting children with uneducated parents. Results reported in Table 5 show that debt relief helped decrease intergenerational inequalities in education, as it led to higher increases in school attendance, primarily among children of less-educated parents.

5.2 Household structure

An extensive literature has also shown that household structure affects children's educational outcomes in several ways. Consistently with this work, this section assesses the potential heterogeneous effect of debt relief on different household structures.

First, we consider household size, as several studies have suggested that household size is negatively correlated with schooling. In line with the quantity-quality theory (Becker, 1960; Becker and Lewis, 1973), if human and financial resources within a household are limited, parents with fewer children can invest more per child.[22] We investigate whether children living in larger households, who are on average less educated, disproportionately benefit from debt relief. We use several measures for household size: total number of household members, members under 15 and dependent members (under 15 or above 64). Results show that the impact of debt relief does not vary substantially with family size (Table A3 in the Appendix).

A related strand of the literature suggests that the share of household resources that each child receives may differ in accordance with birth order, so potentially impacting school attendance differently (Black et al., 2005; de Haan et al., 2014; Emerson and Souza, 2008; Kantarevic and Mechoulan, 2006). In line with this literature, we test whether the impact of debt relief varies according to the order of children's birth, but find no significant differences between first- and later-born children (Table A4 in the Appendix, columns (1) to (3)).

[21] See Black and Devereux (2011) and Blanden (2013) for reviews of the literature.

[22] This relationship has nevertheless been challenged by recent literature (see Guo et al. (2017) for a review of the literature).

Table 4: Gender and wealth heterogeneous effects

Dep. var.: PSA	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Gender analysis			Wealth analysis					
	Subsamples		Saturated	Subsamples					Saturated
VARIABLES	Girls	Boys	model	Q1	Q2	Q3	Q4	Q5	model
POST_DP$_{a,c,j}$	0.105***	0.099***	0.098***	0.143***	0.100**	0.104***	0.050	0.028	0.049**
	(0.02)	(0.03)	(0.03)	(0.03)	(0.05)	(0.03)	(0.03)	(0.02)	(0.02)
POST_DP X Girls$_{i,a,c,j}$			0.008						
			(0.01)						
POST_DP X Wealth index$_{i,a,c,j}$									-0.054***
									(0.01)
Female$_{i,a,c,j}$			-1.382***	-0.038***	-0.037***	-0.024***	-0.021***	-0.007*	-0.027***
			(0.41)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)
Wealth index$_{i,a,c,j}$									-0.690
									(0.63)
Observations	467,747	492,263	960,010	291,787	179,397	175,324	156,492	157,010	960,010
Average dep. var.	0.74	0.77	0.76	0.64	0.74	0.78	0.82	0.90	0.76
Interacted controls	No	No	Yes	No	No	No	No	No	Yes

Notes: DiD estimates using DHS sampling probability weights. Robust standard-errors clustered at both the country × year-of-birth and country × survey-year levels are shown in parentheses. All regressions include the controls presented in Table 1 and country, survey-year and year-of-birth fixed effects. Controls for population are gender-specific in columns (1) and (2). Constant term are not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10% levels.

Table 5: Intergenerational inequality analysis

Dep. var.: PSA	(1)	(2)	(3)
	Subsamples		Saturated model
	Uneducated parents	Educated parents	
$\text{POST\_DP}_{a,c,j}$	0.133***	0.079***	0.079***
	(0.03)	(0.02)	(0.02)
$\text{POST\_DP}_{a,c,j}$ X Uneducated $\text{parents}_{i,a,c,j}$			0.054**
			(0.03)
Uneducated $\text{parents}_{i,a,c,j}$			-0.500
			(1.52)
Observations	321,471	638,539	960,010
Average dep. var.	0.51	0.88	0.76
Interacted controls	No	No	Yes

Notes: DiD estimates using DHS sampling probability weights. Educated parents are those who completed at least primary school. Robust standard-errors clustered at both the country × year-of-birth and country × survey-year levels are shown in parentheses. All regressions include the controls presented in Table 1 and country, survey-year and year-of-birth fixed effects. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10% levels.

Another aspect of family structure that can affect children's education is whether one or both parents are present in the household. Children who grow up in single-parent families tend to have lower educational outcomes, potentially because of a reduced investment in children's human capital, as parents are unable to devote sufficient time and resources to them, but also because these children are more likely to work to compensate the income from the missing parent (Antman, 2011; Ermisch and Francesconi, 2001; Huisman and Smits, 2009; McLanahan and Sandefur, 1994; Zhang et al., 2014). However, We find no evidence that debt relief has a different effect on children living in single-parent or two-parent households, except for children living in single-father households, who are less affected by debt relief (Table A4 in the Appendix, columns (4) to (9)).

Lastly, a quarter of the children in the sample live in polygamous households, in which children from different wives may compete to access limited material and emotional resources (co-wife rivalry). Results show that children in polygamous households, who are on average less educated, tend to be more positively impacted by the HIPC initiative compared to children in monogamous households (Table A5 in the Appendix). This result suggests that debt relief also helps reduce educational gaps between monogamous and polygamous households.

5.3 Ethnic and religious diversity

Since the pioneering study by Easterly and Levine (1997) underlining the role of ethnic fractionalization for explaining underdevelopment in Africa, an extensive literature has assessed the role of ethno-linguistic diversity in public good provision, conflict and economic development (Alesina et al., 2003, 2016; Esteban et al., 2012; Fearon, 2003; Montalvo and Reynal-Querol, 2005; Gisselquist et al., 2016). Similarly, several studies have documented the influence of religious identities in schooling decisions (Sander, 1992; Mueller, 1980; Norton and Tomal, 2009; Lehrer, 1999; Glaeser and Sacerdote, 2008). In many African countries, religious segregation is prominent (Alesina and Zhuravskaya, 2011). Christians exhibit much higher educational attainment and upward intergenerational mobility in education than Muslims or Africans adhering to local religions, reflecting colonial investment in human capital and the Christian missionary role in education, in-group preferences and religious segregation (Alesina et al., 2020; Manglos-Weber, 2017; Platas, 2018). Motivated by this literature, we assess whether the HIPC initiative helped reduce educational disparities between ethnic and religious groups.

We start by distinguishing children from main, secondary or minority ethnic and religious groups.[23] Findings presented in columns (1) to (8) of Table 6 suggest that debt relief disproportionately affected children from ethnic and religious minority groups, thus contributing to reducing educational inequalities based on ethnicity and religion.[24] We then distinguish children of Christian, Muslim and other religious backgrounds. Results show that, even though all children were positively affected by debt relief, the program helped reduce religious inequalities as it disproportionately benefited non-Christian children, who were initially lagging behind (see columns (9) to (12) in Table 6).

[23]The methodology used to compute these groups is detailed in Table 6.

[24]Instead of distinguishing ethnic and religious groups according to their population size, we also differentiate groups with low and high primary school attendance rates. Results go in the same direction, since debt relief has improved school attendance, especially for children from ethnic or religious groups that are lagging behind (see Table A6 in the Appendix).

Table 6: Ethnicity and religion heterogeneity

Dep. var.: PSA	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Ethnic group analysis				Religious group analysis							
	Subsamples by ethnic group			Saturated	Subsamples by religious group			Saturated	Subsamples by religious group			Saturated
	Main	Secondary	Minority	model	Main	Secondary	Minority	model	Christian	Muslim	Other	model
Post_DP$_{a,c,j}$	0.024	0.110**	0.158***	0.062	0.095***	0.126***	0.168***	0.114***	0.079***	0.211***	0.262***	0.079***
	(0.04)	(0.05)	(0.04)	(0.05)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.04)	(0.03)	(0.02)
Post_DP X Minority ethn. group$_{i,a,c,j}$				0.096**								
				(0.04)								
Post_DP X Minority relig. group$_{i,a,c,j}$								0.054				
								(0.03)				
Post_DP X Muslim$_{i,a,c,j}$												0.130***
												(0.04)
Post_DP X Other$_{i,a,c,j}$												0.183***
												(0.04)
Minority ethn. group$_{i,a,c,j}$				1.347								
				(2.73)								
Minority relig. group$_{i,a,c,j}$								2.007				
								(1.74)				
Muslim$_{i,a,c,j}$												5.454**
												(2.60)
Other$_{i,a,c,j}$												6.276*
												(3.46)
Observations	200,237	171,744	102,666	474,647	373,686	184,648	52,277	610,611	193,176	188,428	38,660	420,231
Average dep. var.	0.72	0.68	0.67	0.69	0.74	0.78	0.72	0.75	0.83	0.69	0.65	0.75
Interacted controls	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes

Notes: DiD estimates using DHS sampling probability weights. Children's ethnic or religious groups are based on their mother or the oldest woman in the household if no information was found on the mother. In columns (1) to (8), we use DHS data to compute, for each country and each survey-year, the importance of the child's religion or ethnicity in the population. Main ethnic groups represent the largest ethnic group in the country, those that account for more than 25% of the population or those who are almost as numerous as the largest ethnic group (less than 5% difference). Secondary ethnic groups are groups that are not main ethnic groups but account for more than 5% of the population. Ethnic minority groups include groups comprising 5% or less of the population. Main religious groups are the largest religious group in the country. Secondary religious groups represent religious groups that are not the largest but account for more than 10% of the population. Minority religious groups include religions that account for 10% or less of the population. In columns (9) to (12), we restrict the sample to countries with both Muslims and Christians. The category Other includes children with Animist-Traditional religions and those with no religion. In all columns, robust standard-errors clustered at both the country × year-of-birth and the country × survey-year levels are shown in parentheses. All regressions include the controls presented in Table 1 and country, survey-year and year-of-birth fixed effects. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10% levels.

5.4 Spatial inequality

Finally, several studies have underlined the role of geography in explaining underdevelopment (Acemoglu et al., 2002; Bloom et al., 1998; Henderson et al., 2001; Sachs, 2001), drawing attention to the geographical concentration of poverty (Kanbur and Venables, 2005) between but also within countries. A growing number of studies investigate the determinants of spatial inequalities, particularly when measured in terms of income and educational intergenerational mobility, in the United States (Chetty et al., 2014), Australia (Deutscher, 2020), India (Asher et al., 2021; Azam and Bhatt, 2015), and African countries (Alesina et al., 2021), with the latter showing that regional geographical characteristics that relate to remoteness of the place of residence have strong impacts on educational mobility.

Following this strand of the literature, we investigate whether debt relief initiatives have helped reduce spatial inequality in education by disproportionately benefiting children living in administrative regions under-performing in educational attainment, or residing away from poles of economic development and public infrastructure. We start by investigating whether the effect of debt relief on primary school attendance differs for rural and urban children. Results presented in columns (1) to (3) in Table 7 show that the positive effect of debt relief initiatives is entirely driven by rural children, suggesting that they contributed to reducing the rural-urban educational gap. These results are consistent with the commitments made by many HIPC countries to build schools and open additional classrooms in rural and remote areas (Table S.A1 in the Supplementary Appendix). We also assess whether these initiatives helped mitigate regional educational inequalities by disproportionately affecting children living in regions with lower levels of education. To this end, we compute primary school attendance (PSA) rates by region using DHS data.[25] Then, for each country and survey-year group, regions are divided equally into two groups: regions with low and high PSA rates. Results suggest that debt relief has reduced regional disparities in education as it disproportionately affects children in regions that were lagging behind (see columns (4) to (7) in Table 7).

Another related strand of the literature has shown that density affects preferences for human capital investment in children (Gibbons and Silva, 2008; van Maarseveen, 2020). In line with these studies, an alternative way to investigate a potential urban-rural divide is to distinguish children living in high-density areas from those in low-density areas. Similarly, children residing in remote areas that are far from large cities or with low connectivity are less likely to be attending school (Alesina et al., 2021). In order to investigate this potential differential effects of debt relief initiatives, we use geo-coded DHS data that provides longitude and latitude coordinates for each enumeration area. However, since geolocation data are not available for all DHS surveys in the baseline sample, the sample is reduced to 468,611 children when imposing individual controls, among which 195,330 are "treated".[26]

Based on this geo-coded sample, we conduct several analyses. First, we investigate the potential

[25] DHS data remain representative at the first administrative level.

[26] See discussion in Section 6 of the Supplementary Appendix.

Table 7: Educational spatial inequality

Dep. var.: PSA	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Rural / urban analysis			Regional education analysis			
	Subsamples		Saturated	Subsamples : regional PSA		Saturated	
	Rural	Urban	model	Low	High	models	
POST_DP$_{a,c,j}$	0.130***	0.000	0.000	0.114***	0.055*	0.055*	0.038
	(0.02)	(0.03)	(0.03)	(0.03)		(0.03)	(0.03)
POST_DP$_{a,c,j}$ X Rural			0.130***				
			(0.02)				
Rural			3.645***				
			(1.38)				
POST_DP$_{a,c,j}$ X Low regional PSA						0.058**	0.077**
						(0.03)	(0.03)
Low regional PSA						1.999	1.233
						(1.41)	(2.05)
Observations	646575	313435	960010	471712	384211	855923	855923
Average dep. var.	0.70	0.88	0.76	0.71	0.81	0.76	0.76
Interacted controls	No	No	Yes	No	No	Yes	Yes
Regional FE	No	No	No	No	No	No	Yes

Notes: For each country and survey year, we use DHS data to calculate primary school attendance (PSA) rates by region. We then divide, for each country and survey year, regions into two equal groups (so relative to the median PSA): regions with low PSA and regions with high PSA. Children with regional low PSA are therefore those living in regions that are lagging behind in terms of education in the country. DiD estimates using DHS sampling probability weights. Robust standard-errors clustered are shown in parentheses. In columns (1) to (3), robust standard-errors are clustered at both the country × survey-year and country × year-of-birth levels. In columns (4) to (7), robust standard-errors are clustered at both the country × survey-year and the country × Region id (C×Reg_id) levels in order to account for spatial correlation in error terms. All regressions include the controls presented in Table 1, survey-year and year-of-birth fixed effects. Columns (1) to (6) include country fixed effects and column (7) region fixed effects. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10% levels.

heterogeneous effect of debt relief according to the population density in children's surrounding area. Using the United Nations' World Population Prospects (UN WPP)-Adjusted Population Count from the Gridded Population of the World v.4 (SEDAC, 2018), we compute the average number of inhabitants (per square kilometer) in a buffer zone of 20 and 50 kilometers radius (alternately), with the GPS coordinates of children's enumeration area as a centroid.[27] We then define density quartiles by country and survey year to identify children living in more (or less) dense areas at the time of observation. Sub-sample and saturated model estimates in Table 8 show that the effect of debt relief is stronger for children belonging to the lowest density quartile (1st quartile) and that the effect is robust to the size of the buffer area, as seen in column (6) in Table 8.

Second, we investigate the differential effect of debt relief on primary school attendance for remote children by interacting the $POST\_DP$ variable with the distance of the children's enumeration area to the closest large urban areas and cities. We draw on two data sources providing GPS coordinates of either historical large urban areas since 1950 (the World Database of Large Urban Areas, WDLUA[28])

[27] See Section 6.3 of the Supplementary Appendix for additional information on the GPW v4 and the computational method.

[28] https://nordpil.com/resources/world-database-of-large-cities/

Table 8: Density heterogeneity

Dep. var.: PSA	(1)	(2)	(3)	(4)	(5)	(6)
	Subsamples: density quartiles				Saturated models	
	Q1	Q2	Q3	Q4		
$\text{POST\_DP}_{a,c,j}$	0.203***	0.138***	0.131***	0.063***	0.112***	0.116***
	(0.04)	(0.04)	(0.03)	(0.02)	(0.03)	(0.03)
POST_DP X 1st density $\text{quartile}_{i,a,c,j}$					0.091***	0.083**
					(0.03)	(0.03)
1st density $\text{quartile}_{i,a,c,j}$					-1.990	-3.085
					(1.83)	(2.16)
Observations	116,144	115,992	115,916	115,676	463,728	463,839
Average dep. var.	0.60	0.70	0.75	0.86	0.74	0.74
Interacted controls	No	No	No	No	Yes	Yes
Density: radius buffer area	20km	20km	20km	20km	20km	50km

Notes: DiD estimates using DHS sampling probability weights. Robust standard-errors are shown in parentheses and are clustered at both the country×survey-year (C×S) and country × DHS GPS id (C×GPS_id) levels in order to account for spatial correlation in error terms. All regressions include the controls presented in Table 1, country, survey-year and year-of-birth fixed effects. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10% levels.

or the largest cities as observed in 2020 (the World Cities Database[29]).[30]

Results in Table 9 using distance from both historical large urban areas and large cities provide evidence of a larger effect of debt relief on primary school attendance for children living further away from urban economic centers.[31]

[29]https://simplemaps.com/data/world-cities

[30]At first glance, the WDLUA data seems to be better suited to the sample, as the largest urban areas recorded since 1950 were already large at the beginning of the period of study and are a good proxy for economic concentration for sampled children observed in the earlier DHS. But for most African countries, urban areas recorded within the WDLUA lie in the capital city only, while some secondary cities were probably less populated at that time but important enough to provide public services such as primary education. For this reason, we alternately use the World Cities Database, which records the more populated cities for African countries (as of today), on the assumption that they were large enough in the early 1990s to host education facilities. Section 6.4 of the Supplementary Appendix discusses the data.

[31]We also consider distance to roads as another proxy for children remoteness (See Section 6.5 of the Supplementary Appendix for a discussion about roads data). Results report a larger effect of debt relief initiatives on primary for children with less connectivity and transport infrastructure in their surrounding area, although this effect does not seem to be statistically significant (see Table A7 in the Appendix).

Table 9: Analysis of distance to large cities

Dep. var.: PSA	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Distance var:	Distance to the closest large city (>200,000 inhab.)						Distance to the closest large city (>10,000 inhab. in 2020)					
	Subsamples: distance to large city				Saturated models		Subsamples: distance to large city				Saturated models	
	<10km	[10-50[km	[50-150[km	≥150km			<5km	[5-10[km	[10-20[km	≥20km		
POST_DP$_{a,c,j}$	0.001	0.101**	0.118***	0.127***	0.056**	0.096***	0.046**	0.116***	0.174***	0.150***	0.064***	0.108***
	(0.01)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.04)	(0.03)	(0.02)	(0.02)
POST_DP X Remoteness$_{i,a,c,j}$					0.083***	0.032					0.094***	0.042***
					(0.02)	(0.03)					(0.02)	(0.01)
Remoteness$_{i,a,c,j}$					0.410	2.179					-1.679	-2.208
					(1.75)	(2.32)					(1.74)	(1.88)
Observations	27,466	43,049	123,936	251,312	445,763	445,763	82,725	46,927	73,266	260,934	463,852	463,852
Average dep. var.	0.91	0.86	0.77	0.66	0.74	0.74	0.88	0.88	0.80	0.66	0.74	0.74
Interacted controls	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes
Remoteness var.					≥50km	≥150km					≥10km	≥20km

Notes: DiD estimates using DHS sampling probability weights. Robust standard-errors are shown in parentheses and are clustered at both the country × survey-year (C×S) and country × DHS GPS id (C×GPS_id) levels in order to account for spatial correlation in error terms. All regressions include the controls presented in Table 1, country, survey-year and year-of-birth fixed effects. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10% levels.

6 Conclusion

In this study we examine the effect of debt relief on primary school attendance using a multi-level approach both at the micro and macro levels. Exploiting the temporal variation in the implementation of the debt relief initiatives and children's year of birth, we implement difference-in-differences estimators. The method considers multiple fixed effects and the inclusion of key determinants of primary school attendance at the individual and country level, which help us mitigate the possibility of having confounders when estimating the effect of debt relief on primary school attendance. The empirical strategy has allowed us identify the contribution of the Enhanced HIPC initiative and the MDRI, separately, on the probability of attending primary school. We have also implemented a battery of robustness checks to verify that the results are not driven by omitted variables, sample dependence, educational trends or the poorer conditions of education systems in beneficiary countries.

Overall, we find robust evidence indicating that debt relief under the Enhanced HIPC initiative contributed to the achievement of universal primary education. School-age children have a 10 percentage point increased probability of attending primary education if their country was granted debt relief, compared to children living in non-HIPCs. Heterogeneity analysis also shows that debt relief helped mitigate wealth-based, intergenerational, religious, ethnic and spatial inequalities in education. Our analysis indicates that an improved fiscal space is the main channel underpinning the results, insofar debt relief freed up additional resources that in turn were allocated to the education sector. The multiple specifications, controls and extensive robustness checks allow us to confidently assert that debt relief has contributed to improving school attendance and reducing educational gaps in poor and heavily indebted countries.

Our findings are important in light of recent increases in public debt and rising costs of service debt that have left about half of LICs in a 'debt distress' situation (IMF, 2021). High levels of indebtedness can undermine public spending in social services such as education, with detrimental effects on economic and social development. Thus, under certain conditions, debt relief can be an alternative source of financing primary (and other levels of) education in LICs at the *extensive margin*, alongside other forms of financing such as taxes and foreign aid. Although not investigated in this study, questions on the effect of debt relief on the quality of education, i.e. at the *intensive margin*, remain open and represent a promising area for future research.

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Appendix

Table A1: Effect of duration of exposure to debt relief

Dep. var.: PSA	(1)	(2)
Linear duration of exposure	0.009	
	(0.01)	
Duration of exposure:		
1 year		0.029
		(0.04)
2 years		0.150***
		(0.03)
3 years		0.170***
		(0.03)
4 years		0.132***
		(0.03)
5 years		0.115***
		(0.03)
>5 years		0.048
		(0.03)
Observations	960,010	960,010

Notes: DiD estimates using DHS sampling probability weights. Robust standard-errors clustered at both the country × year-of-birth and country × survey-year levels are shown in parentheses. All regressions include the controls presented in Table 1 and country, survey-year and year-of-birth fixed effects. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10% levels.

Table A2: Transitory versus long-lasting effect of debt relief

Dep. var.: PSA	(1)	(2)	(3)
POST_DP$_{a,c,j}$	0.094***		
	(0.02)		
POST_DP$_{a,c,j}$ X			
observed 0-5 years after DP		0.094***	0.104***
		(0.02)	(0.04)
observed 6-10 years after DP	0.017	0.111***	0.125***
	(0.02)	(0.03)	(0.05)
observed 10 years & more after DP	0.000	0.094**	0.107**
	(0.02)	(0.04)	(0.05)
Observations	960,010	960,010	960,010
Control for duration of exposure	No	No	Yes

Notes: DiD estimates using DHS sampling probability weights. Robust standard-errors clustered at both the country × year-of-birth and country × survey-year levels are shown in parentheses. All regressions include the controls presented in Table 1 and country, survey-year and year-of-birth fixed effects. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10% levels.

Figure A1: Educational spending

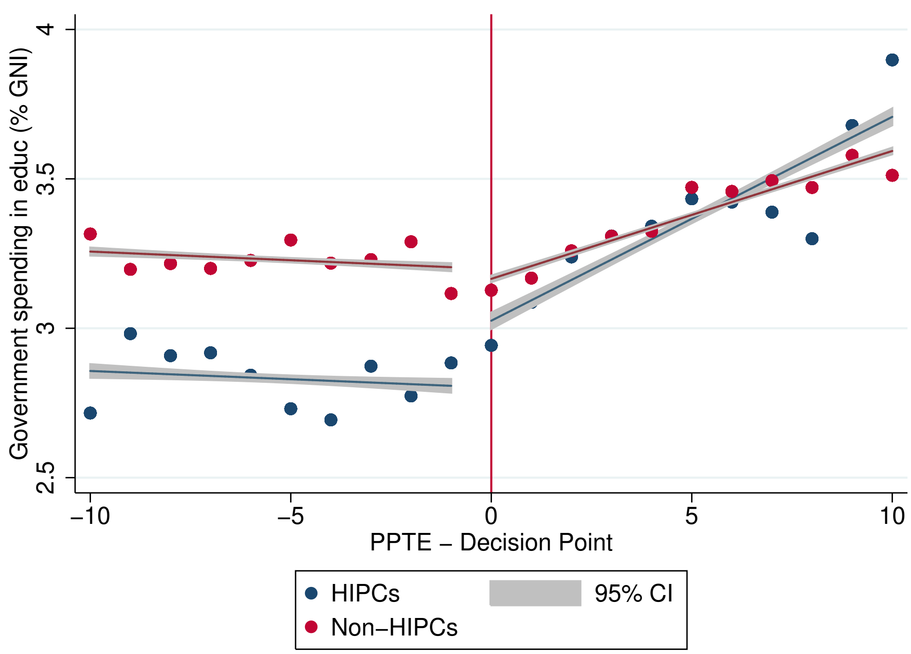

Figure A2: Debt service savings from debt relief initiatives

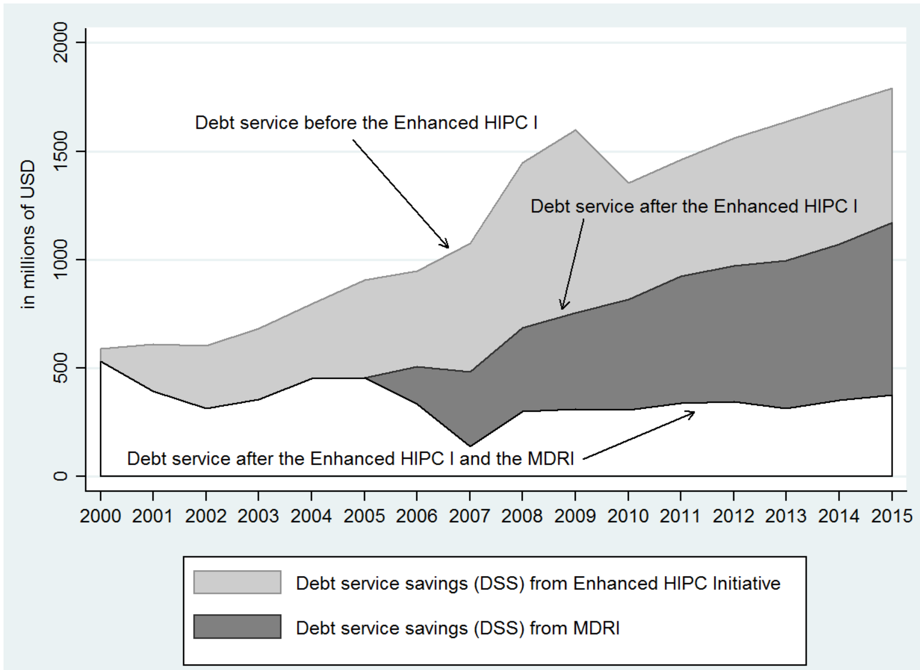

Table A3: Household size analysis

Dep. var.: PSA	(1)	(2)	(3)
$\text{VAR.}_{\cdot i,a,c,j}$	No. of household members		
	Total	<15 years	<15 or >64 years
$\text{POST\_DP}_{a,c,j}$	0.142***	0.120***	0.120***
	(0.03)	(0.02)	(0.02)
POST_DP X $\text{VAR.}_{\cdot i,a,c,j}$	-0.006**	-0.006	-0.005
	(0.00)	(0.00)	(0.00)
$\text{VAR.}_{\cdot i,a,c,j}$	-0.231	-0.275	-0.280
	(0.15)	(0.25)	(0.23)
Observations	960,010	960,010	960,010
Interacted controls	Yes	Yes	Yes

Notes: DiD estimates using DHS sampling probability weights. Robust standard-errors clustered at both the country × year-of-birth and country × survey-year levels are exposed in parentheses. All regressions include the controls presented in Table 1, as well as country, survey-year and year-of-birth fixed effects. In column (4), a control for household size is added but the results remain unchanged when it is removed. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10% levels.

Table A4: Birth order and family structure analysis

Dep. var.: PSA	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Birth order analysis			Parents' presence analysis					
	Subsamples		Saturated model	Subsamples				Saturated models	
	First-born child	Higher birth order		Two-parent household	Single-parent household	Single-mother household	Single-father household		
POST_DP$_{a,c,j}$	0.101***	0.108***	0.108***	0.108***	0.090***	0.097***	0.059**	0.108***	0.108***
	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
POST_DP X First born$_{i,a,c,j}$			-0.007						
			(0.02)						
POST_DP X Single parent$_{i,a,c,j}$								-0.017	
								(0.01)	
POST_DP X Single mother$_{i,a,c,j}$									-0.010
									(0.02)
POST_DP X Single father$_{i,a,c,j}$									-0.049***
									(0.01)
First born$_{i,a,c,j}$			-0.330						
			(1.03)						
Single parent$_{i,a,c,j}$								-0.795	
								(0.77)	
Single mother$_{i,a,c,j}$									-0.646
									(0.84)
Single father$_{i,a,c,j}$									-0.893
									(0.68)
Observations	122,161	609,195	731,356	666,515	229,680	176,348	53,332	896,195	896,195
Average dep. var.	0.83	0.76	0.77	0.76	0.79	0.81	0.75	0.77	0.77
Interacted controls	No	No	Yes	No	No	No	No	Yes	Yes

Notes: DiD estimates using DHS sampling probability weights. Robust standard-errors clustered at both the country × year-of-birth and country × survey-year levels are exposed in parentheses. All regressions include the controls presented in Table 1, as well as a control for household size and country, survey-year and year-of-birth fixed effects. Constant term not reported in order to save space. Note that samples include households with at least one parent present, as we control for parental education. ***, ** and * denote significance at 1%, 5% and 10% levels.

Table A5: Polygamous versus monogamous households

Dep. var.: PSA	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Subsamples		Saturated	Subsamples			Saturated
	Monogamous	Polygamous	model	Monog.	Polyg. senior wife	Polyg. junior wife	model
$POST\_DP_{a,c,j}$	0.081***	0.115***	0.081***	0.081***	0.056*	0.084***	0.081***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)
POST_DP X Polyg.$_{i,a,c,j}$			0.035				
			(0.02)				
POST_DP X Polyg. senior wife$_{i,a,c,j}$							-0.025
							(0.03)
POST_DP X Polyg. junior wife$_{i,a,c,j}$							0.004
							(0.02)
Polygamous$_{i,a,c,j}$			1.421				
			(1.80)				
Polygamous senior wife$_{i,a,c,j}$							0.128
							(2.25)
Polygamous junior wife$_{i,a,c,j}$							1.313
							(1.91)
Observations	490,873	158,983	649,856	490,873	59,083	54,060	604,016
Average dep. var.	0.77	0.59	0.72	0.77	0.59	0.59	0.73
Interacted controls	No	No	Yes	No	No	No	Yes

Notes: DiD estimates using DHS sampling probability weights. Robust standard-errors clustered at both the country × year-of-birth and country × survey-year levels are shown in parentheses. We use individual women's datasets that provide detailed information for women in the household (Individual recode (IR) surveys). Children in polygamous households are those whose mother is in a polygamous union, or -when the information was not available for the mother- those who live in a household where one woman is in a polygamous union. In columns (5) and (6), we distinguish children whose mother is the first wife (senior wife) from those whose mother is the second (or more) wife (junior wife). All regressions include country, survey-year and year-of-birth fixed effects. A control for household size is added, even though the results remain unchanged when it is removed. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10% levels.

Table A6: Ethnicity and religion supplementary analysis

Dep. var.: PSA	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Ethnic group analysis					Religious group analysis		
	Subsamples (PSE quartiles)				Saturated model	Subsamples (PSE terciles)		Saturated model
	1st	2nd	3rd	4th		1st	2nd or 3rd	
Post_DP$_{a,c,j}$	0.191***	0.177***	0.033	0.017	0.019	0.126***	0.085***	0.085***
	(0.06)	(0.06)	(0.06)	(0.04)	(0.05)	(0.04)	(0.02)	(0.02)
Post_DP X Low-PSE ethnic group$_{i,a,c,j}$					0.172***			
					(0.05)			
Post_DP X Low-PSE religious group$_{i,a,c,j}$								0.041
								(0.04)
Low-PSE ethnic group$_{i,a,c,j}$					12.488**			
					(4.96)			
Low-PSE religious group$_{i,a,c,j}$								-3.939*
								(2.01)
Observations	138,835	132,520	145,373	57,919	474,647	233,335	376,741	610,076
Average dep. var.	0.56	0.70	0.75	0.86	0.69	0.66	0.80	0.75
Interacted controls	No	No	No	No	Yes	No	No	Yes

Notes: DiD estimates using DHS sampling probability weights. Children's ethnic or religious groups are based on their mother or the oldest woman in the household if no information was found on the mother. We use DHS data to compute, for each country and each survey-year, primary school attendance (PSA) rate by ethnic and religious group. We then divide, for each country and survey-year, ethnic (resp. religious) groups into four (resp. three) categories depending on these PSA rates. Children belonging to the 1st PSA quartile (resp. tercile) ethnic (resp. religious) group are children whose ethnic (resp. religious) group has the lowest primary school attendance rate in the country at the time of the survey. Robust standard-errors clustered at both the country × year-of-birth and country × survey-year levels are exposed in parentheses. All regressions include the controls presented in Table 1 and country, survey-year and year-of-birth fixed effects. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10% levels.

Table A7: Distance to roads heterogeneity

Dep. var.: PSA	(1)	(2)	(3)	(4)	(5)	(6)
	Subsamples: distance to roads				Saturated models	
	≥1km	]2-5]km	]5-10]km	>10km		
Post_DP	0.110***	0.132***	0.156***	0.198***	0.125***	0.131***
	(0.04)	(0.03)	(0.05)	(0.05)	(0.03)	(0.03)
Post_DP X Far from road					0.038	0.067
					(0.02)	(0.04)
Far from road					-1.904	1.599
					(1.85)	(2.83)
Observations	134,828	188,731	52,713	64,809	441,081	441,081
Average dep. var.	0.78	0.75	0.69	0.69	0.74	0.74
Interacted controls	No	No	No	No	Yes	Yes
Far from road var.: Distance from road					≥ 5km	>10 km

Notes: DiD estimates using DHS sampling probability weights. Robust standard-errors are shown in parentheses and are clustered at both the country×survey-year (C×S) and country × DHS GPS id (C×GPS_id) levels in order to account for spatial correlation in error terms. All regressions include the controls presented in Table 1, country, survey-year and year-of-birth fixed effects. P-values for coefficients associated with the "Post_DP X Far from road" variable stand at 0.11. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10% levels.

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Less debt, more schooling? Evidence from cross-country micro data

Supplementary Appendix

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1 HIPC initiatives: educational targets

Table S.A1: Educational targets of the HIPC initiatives

	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

Continued on next page

Following the previous table

	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
	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.

2 Sample and database

2.1 Sample's composition

Figure S.A1: Sample evolution per HIPC/Non-HIPC group

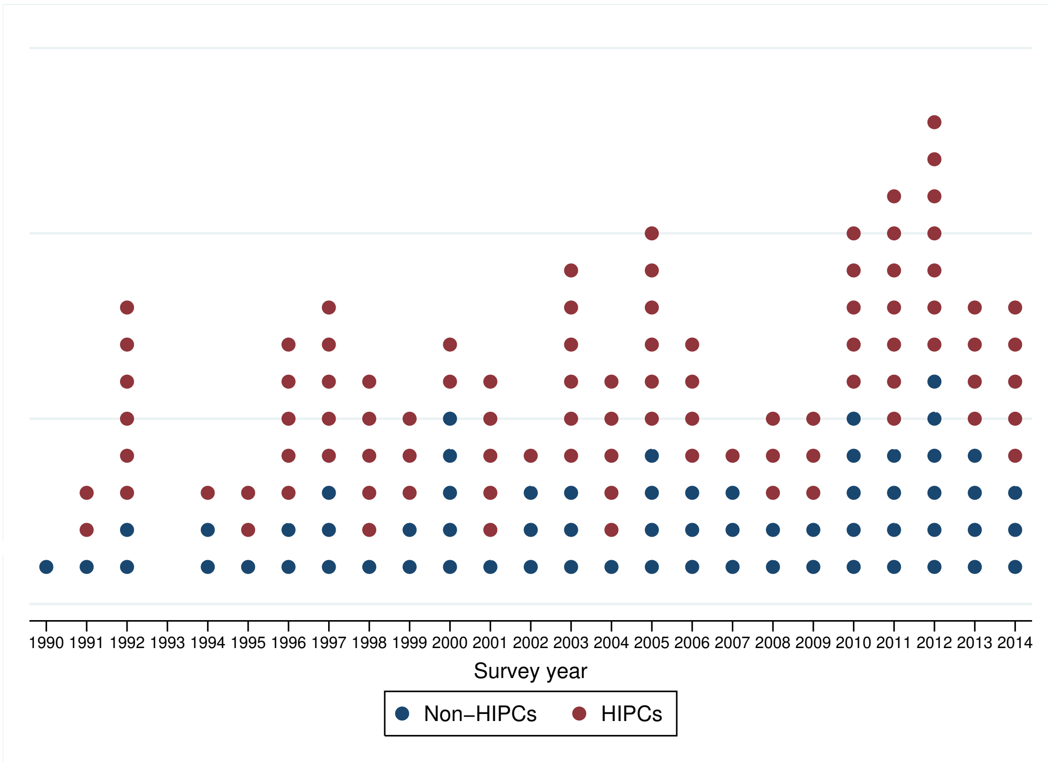
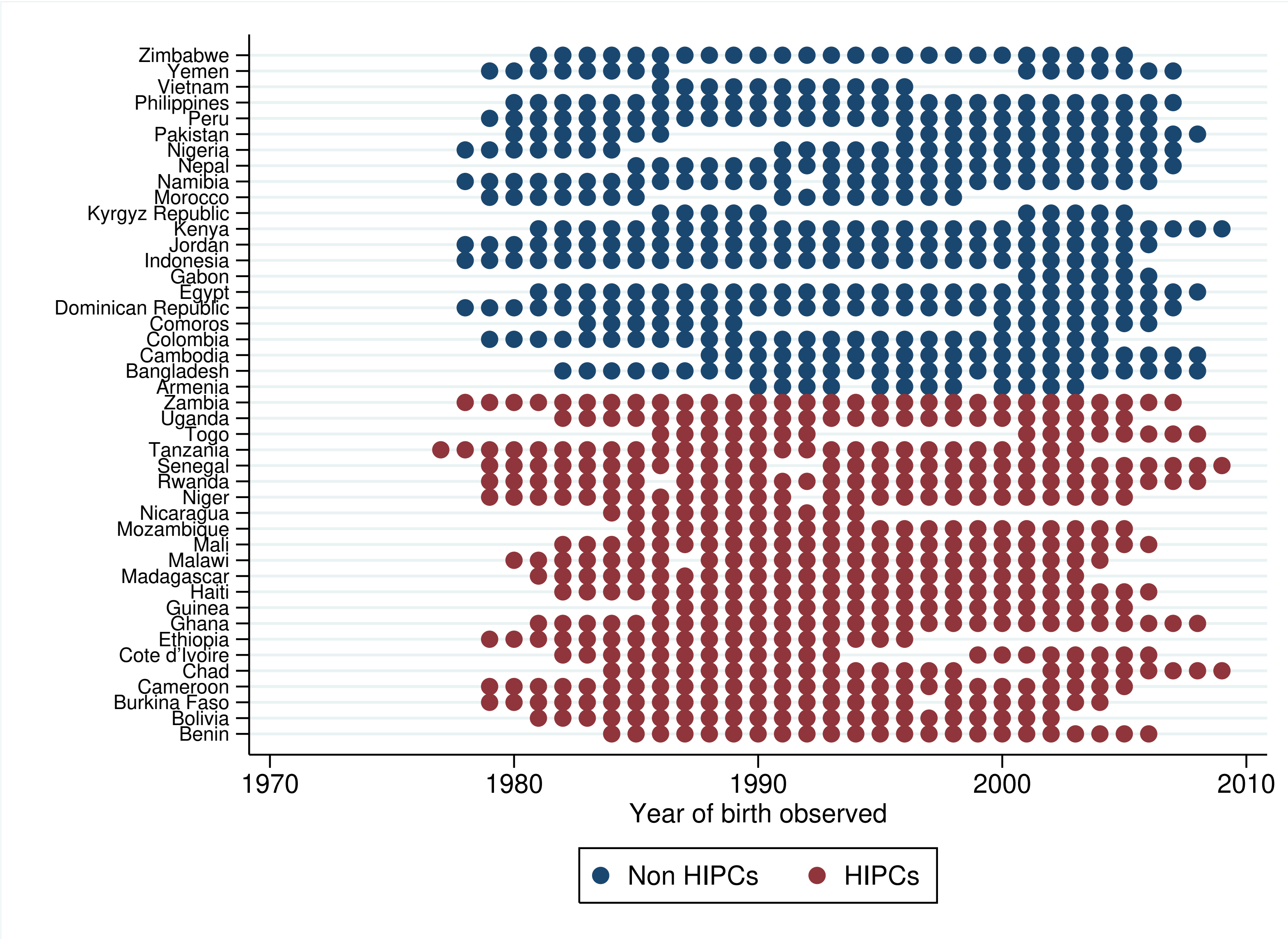


Figure S.A2: Sample evolution per country



2.2 Selection due to double counting issues

In order to avoid observing twice the same individuals in two consecutive DHS, we exclude certain children from the sample, as described in the main text. Table S.A2 below presents the descriptive statistics for excluded and selected individuals in surveys where a selection was implemented. Overall, our selection strategy leads to select older children who are more likely to have ever attended primary school. Sample children also come from poorer households and their parents are on average less educated in comparison with excluded individuals.

Table S.A2: Selection of individuals due to overlapping cohorts (1/2)

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	0.18***	(234.46)
Age	7.31	1.30	10.27	1.70	2.96***	(1019.75)
Girl	0.49	0.50	0.49	0.50	0.00	(1.50)
Mother Educ: None	0.32	0.50	0.38	0.50	0.05***	(47.53)
Mother Educ: Primary	0.41	0.50	0.38	0.50	-0.02***	(-19.26)
Mother Educ: Secondary or Tertiary	0.27	0.40	0.24	0.40	-0.03***	(-30.82)
Father Educ: None	0.24	0.40	0.29	0.50	0.05***	(42.70)
Father Educ: Primary	0.40	0.50	0.39	0.50	-0.01***	(-6.90)
Father Educ: Secondary or Tertiary	0.35	0.50	0.31	0.50	-0.04***	(-33.30)
Head's Child	0.77	0.40	0.77	0.40	0.00	(1.14)
Wealth Index (WI)	0.02	1.60	-0.01	1.60	-0.03***	(-9.52)
Rural	0.65	0.50	0.65	0.50	-0.01***	(-7.37)
GDP per capita (log, constant USD)	7.18	0.90	7.05	0.90	-0.13***	(-78.31)
Population under 15 (log)	16.15	1.10	16.05	1.20	-0.09***	(-42.92)
Observations	450,209		782,828		1,233,037	

Notes: The sample includes all 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% levels.

We then compare selected and non-selected individuals of the same cohort×year-of-survey and of the same country but observed in different DHS. Results reported in Table S.A3 show that many of the differences observed in Table S.A2 disappear or are very low.

Table S.A3: Selection of individuals due to overlapping cohorts (2/2)

Dep. var.	(1) Ever Attended Primary School	(2) Age	(3) Girl	(4) Parents' educ: None	(5) Head's Child
Included individual	0.012	0.000***	0.002	-0.001	-0.007***
	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
(0.00)					
Observations	1,227,496	1,233,037	1,232,904	819,918	1,232,870
Yob*survey year FE	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes

Dep. var.	(6) Wealth Index	(7) Rural	(8) GDP per capita (log, constant USD)	(9) Pop under 15 (log)
Included individual	0.022	-0.016**	0.008	0.001
	(0.02)	(0.01)	(0.01)	(0.01)
Observations	941,557	1,233,037	1,228,235	1,233,037
Yob*survey year FE	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes

Notes: The sample includes all selected surveys where a selection was implemented. All regressions include survey year × year of birth fixed effects as well as country fixed effects. Robust standard-errors clustered at both the country × year-of-birth and the country × survey-year levels are exposed in parentheses. ***, ** and * denote a significance at respectively 1%, 5% and 10%.

2.3 Samples of HIPC and non-HIPC children

Tables S.A4 and S.A5 show for each country (both HIPCs and non HIPCs), the survey year of each DHS mobilized alongside the number of primary school age children, regardless of the availability of micro data. Each HIPC displays a DHS before and after its decision point year (see Table S.A6 below). Each non-HIPC country displays a DHS before and after 2000.

Table S.A4: HIPC countries

HIPCs	DHS	Individuals	Ind Treated	HIPCs	DHS	Individuals	Ind Treated
Benin	1996	4,319	0	Mali	1995/1996	7,519	0
	2001	4,738	4,738		2001	9,517	9,517
	2006	14,055	14,055		2006	12,777	12,777
	2011/2012	21,597	21,597		2012/2013	12,863	12,863
Bolivia	1993	210	0	Nicaragua	1997/1998	5,925	0
	1994	3,870	0		2001	12,523	12,523
	1998	7,612	0	Niger	1992	5,733	0
	2003/2004	10,722	10,722		1998	7,613	0
	2008	14,669	14,669		2006	9,089	9,089
Burkina Faso	1992/1993	5,130	0		2012	15,299	15,299
	1998/1999	4,175	0	Mozambique	1997	6,566	0
	2003	13,242	13,242		2003	10,699	10,699
	2010	18,652	18,652		2009	1,296	1,296
Cameroon	1991	4,315	0		2011	15,581	15,581
	1998	4,668	0	Rwanda	1992	6,636	0
	2004	10,514	10,514		2000	7,212	5,608
	2011	14,862	14,862		2005	6,573	6,573
Chad	1996/1997	8,367	0		2010/2011	5,229	5,229
	2004	6,716	6,716		2014/2015	10,950	10,950
	2014/2015	26,480	26,480	Senegal	1992/1993	3,691	0
Cote d'Ivoire	1994	4,324	0		1997	9,405	0
	1998/1999	2,638	0		2005	9,551	9,551
	2011/2012	10,387	10,387		2010/2011	2,739	2,739
Ethiopia	1992	9,518	0		2012/2013	2,468	2,468
	1997	12,266	0		2015	9,062	9,062
	2003	16,661	16,661	Tanzania	1991/1992	5,910	0
Ghana	1993	3,182	0		1996	3,352	0
	1998/1999	3,013	0		1999	3,138	0
	2003	3,847	3,847		2005	1,652	1,652
	2008	7,683	7,683		2010/2009	11,455	11,455
	2014	8,521	8,521	Togo	1998	10,443	0
Guinea	1999	6,118	0		2013/2014	10,467	10,467
	2005	8,757	8,757	Uganda	1995	7,474	0
	2012	10,092	10,092		2000/2001	6,195	5,657
Haiti	1994/1995	4,017	0		2006	7,332	7,332
	2000	6,604	0		2011	14,604	14,604
	2005/2006	8,442	3,948	Zambia	1992	3,737	0
	2012	10,389	10,389		1996	5,592	0
Madagascar	1992	4,267	0		2001/2002	5,600	5,600
	1997	5,959	0		2007	6,412	6,412
	2003/2004	4,711	4,711		2013/2014	21,244	21,244
	2008/2009	16,643	16,643				
Malawi	1992	5,526	0				
	2000	6,862	5,039				
	2004/2005	10,872	10,872				
	2010	27,457	27,457				
Total No of HIPCs		22					
Total No of surveys		87					
Total No of individuals		748,792					
No of individuals treated		537,501					

Table S.A5: Non-HIPC countries

Non-HIPCs	DHS	Individuals	Non-HIPCs	DHS	Individuals
Armenia	2000	1,911	Kenya	1993	6,225
	2005	1,436		1998	5,952
	2010	1,050		2003	5,143
Bangladesh	1993/1994	3,383		2008/2009	6,015
	1996/1997	3,229		2014/2015	39,487
	1999/2000	6,155	Kyrgyz Republic	1997	2,113
	2004	4,012		2012	3,529
	2007	5,062	Morocco	1992	7,473
	2011	5,941		2003/2004	9,905
	2014	11,385	Namibia	1992	5,475
Cambodia	2000	10,360		2000	5,001
	2005/2006	9,578		2007	4,709
	2010/2011	6,888		2013	7,806
	2014	11,704	Nepal	1996/1997	6,008
Colombia	1990	4,292		2001/2002	7,285
	1995	5,045		2007/2008	4,825
	2000	4,064		2011/2012	7,439
	2005/2004	14,826	Nigeria	1990	11,087
	2009/2010	25,775		2003	4,757
Comoros	1996	2,985		2008	21,672
	2012	4,715		2013	37,687
Dominican Republic	1991	5,227	Pakistan	1990/1991	10,765
	1996	2,837		2006/2007	12,1947
	1999	433		2012/2013	15,367
	2002	13,960	Peru	1991/1992	8,745
	2007	17,800		1996	14,825
	2013	5,765		2000	13,522
Egypt	1992/1993	4,920		2004/2006	5,625
	1995/1996	12,062		2009	2,534
	2000	8,997		2010	2,226
	2005	7,194		2011	2,247
	2008	12,042		2012	15,102
	2014	17,647	Philippines	1993	11,080
Gabon	2012	6,508		1998	8,129
Indonesia	1991	9,620		2003	7,804
	1994	12,276		2008	7,274
	1997	18,453		2013	11,516
	2002/2003	14,398	Vietnam	1997	4,026
	2007	18,407		2002	4,184
	2012	27,110	Yemen	1991/1992	21,542
Jordan	1990	11,828		2013	25,322
	1997	5,926	Zimbabwe	1994	4,828
	2002	6,147		1999	5,067
	2007	10,376		2005/2006	6,022
	2012	13,384		2010/2011	9,565
Total No of countries	22				
Total No of surveys	90				
Total No of individuals	955,970				

Table S.A6: Minimum year-of-birth required for debt relief exposure

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, thus leading to changes in the minimum year-of-birth required for being considered as treated.

2.4 Descriptive statistics

Table S.A7 reports the descriptive statistics for children in the sample. It also provides T-tests comparing HIPC and non-HIPC children for each covariate used in the baseline regressions. A first look at the results suggests that HIPC and non-HIPC children are quite different on several socio-economic characteristics. The statistical significance associated with T-tests might result from the large sample size, which means that tiny differences may be statistically significant (such as the gender composition of the two samples).

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	-0.007***	(-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	0.027***	(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	0.022***	(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	0.045***	(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	-0.091***	(-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)
Observations	1,704,762		748,792		955,970		1,704,762	

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

These static differences, which are computed over all the DHS, regardless of when they were conducted, do not invalidate the DiD empirical strategy. Indeed, the DiD design allows for ex-ante or ex-post differences in the outcome and covariates. What is essential is the ex-ante evolution of these variables and the existence of a common trend between HIPC and non-HIPC children in the period preceding debt relief initiatives.

The following section (Section 3) discusses this assumption and presents several tests investigating the existence of a common trend in primary school attendance between HIPCs and non-HIPCs. Before proceeding to this section, we further discuss the other covariates that have been constructed using the information available in the DHS.

Considering some of the criticisms formulated against the wealth index reported in recent surveys and its unavailability in older surveys, we recalculated a wealth index based on households' assets. Figure S.A3 and Table S.A8 report the eigenvalues and coefficients associated with each asset, respectively, used to conduct our PCA analysis and define an individual wealth index at the country × survey-year levels.

Figure S.A3: Graphical analysis of PCA

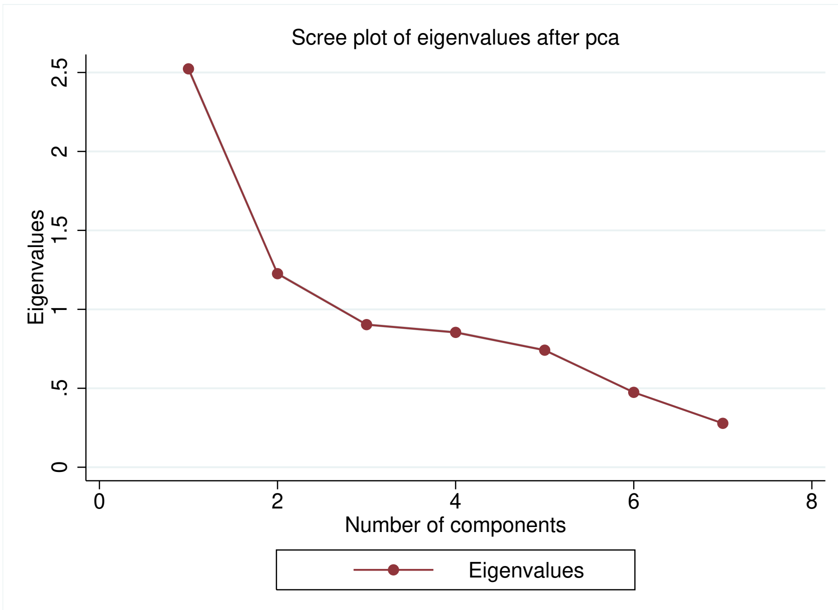


Table S.A8: Coefficients used to generate wealth index

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

3 Parallel trend discussion

Before running a difference-in-differences (DiD) model, one must make sure that there is no divergence in the evolution of the outcome prior to the "treatment". This condition, known as the parallel or common trend hypothesis, is essential since, when holding, it gives credit to the interpretation of the DiD estimator as a causal impact of the "treatment" on the outcome. Suppose one observes that the outcome variable follows a different pattern for the control and treatment units before the treatment. In this case, it becomes unrealistic to attribute the post-treatment evolution of the outcome to the treatment itself.

In order to test for this common trend hypothesis, we restrict the sample to surveys completed before 2000 and consider only children born no later than 1987 (i.e., who 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 test the ex-ante common trend hypothesis. We first run the baseline specification (equation (1) in the main article) without the $POST\_DP$ variable. We augment this specification with a survey-year linear trend (i.e., a continuous variable for survey years) and an interaction term between this linear trend and a dummy variable flagging countries that will benefit from the HIPC initiative after 2000. The coefficient associated with the survey-year trend aims at capturing the linear evolution in primary school attendance between 1990 and 2000, while the one for the interaction term captures a potential different evolution in primary school attendance for HIPCs. Estimates in column (1) of Table S.A9 suggest that while primary school attendance significantly increased (in a linear way) over the 1990-2000 period for the whole sample, such evolution has not been significantly different in HIPCs (the interaction term being not statistically significant). Column (2) of Table S.A9 reports results for the same estimate but augmented with the quadratic expression of the HIPC specific survey-year trend. The HIPC-specific trends remain not significant. Thus, these results support the absence of a diverging path in primary school attendance for HIPCs (on average) prior to debt relief initiatives. We then test the common trend hypothesis switching the survey-year trend by a year-of-birth (i.e., age cohorts) trend. The linear and quadratic interaction terms are not statistically significant hence supporting the common trend hypothesis as well (columns (3) and (4)).

Lastly, we implement two placebo tests. In column (5), we define a placebo treatment for HIPCs by considering children born between 1984 and 1987 as treated. This test compares the probability of attending primary school for children in HIPCs born between 1984 and 1987 with both older children in HIPCs and children in control countries. Results show that HIPC children are not more likely to attend primary school than older children or children in control countries before the Enhanced HIPC initiative. Column (6) shows the results when we apply a gradual yearly treatment instead of a classical before/after treatment. Results remain unchanged and

Table S.A9: Investigating the common trend hypothesis

Estimator: DiD	(1)	(2)	(3)	(4)	(5)	(6)
Restrictions:	Period: 1990-2000 & Year-of-birth (YoB) $\leq$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				
	(0.01)	(0.06)				
HIPC×Survey Trend^2		-0.003				
		(0.00)				
YoB Trend			-0.023	-0.023		
			(0.02)	(0.02)		
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]					0.029	
					(0.05)	
HIPC×YoB[1984]						0.049
						(0.07)
HIPC×YoB[1985]						0.007
						(0.05)
HIPC×YoB[1986]						0.029
						(0.04)
HIPC×YoB[1987]						0.019
						(0.04)
Observations	345,319	345,319	345,319	345,319	345,319	345,319
Indiv. Treated (placebo)	-	-	-	-	83,560	83,560
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Survey-Year FE	No	No	Yes	Yes	Yes	Yes
YoB FE	Yes	Yes	No	No	Yes	Yes

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). Estimates using DHS sampling probability weights are reported. Robust standard-errors clustered at both the country × year-of-birth and the country × survey-year × levels are shown in parentheses. Constant terms are not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10%

comfort us regarding the common trend hypothesis and the relevance of the DiD specification in the context of this study.

4 Robustness checks

4.1 Additional country-level control variables

This section challenges the robustness of the main results by entering (alternately) multiple country-level control variables that might affect average primary school attendance at the national scale, either through demand- or supply-driven mechanisms. More specifically, and as discussed in the core article, debt relief initiatives for low-income countries were granted subject to the implementation of several economic and development programs that might have fueled policy changes likely to affect labor market structure, and thus education demand and/or supply.

Retrieving variables from multiple databases, we re-run the main estimate (column (4) of Table 1), subsequently adding each additional country-level control reflecting several economic improvements. Additional country-level controls encompass trade openness (**TRADE**), remittances receipts (**REMITT.**), gross fixed capital formation (**INVT.**), GDP growth (**GROWTH**), inflation rate (**CPI**), natural resources rents (**RES._RENT.**) (all retrieved from World Bank Development Indicators database) and foreign direct investment inflows (**FDI._INF.**) (retrived from the UNCTAD database). We also try accounting for the contribution of the demand for education which, at the national scale, can be proxied by the share of total value added stemming from the manufacturing (**MANUF.**) and the services (**SERV.**) industry, as well as by demographic variables such as the share of urban population (**URB._POP.**) and population density (**POP._DENS.**) (all retrieved from the World Bank Development Indicators database).

Results including those additional controls are displayed in Table S.A10 and support the main findings without affecting the significance or the magnitude of the correlation between debt relief exposure and primary school attendance.

The money freed up by debt relief may have particularly increased primary school attendance especially in countries with sound institutions where such money was not wasted. To test this assumption, we estimate the correlation between debt relief and primary school attendance at a given level of institutional and governance quality. Democracy is indeed expected to increase the demand for redistribution, which should consequently foster the provision of public services such as education (Acemoglu et al., 2015; Boix, 2003; Knutsen and Wegmann, 2016; Niño-Zarazúa et al., 2021). Consequently and similarly to Table S.A10, we successively add to the specification control variables capturing alternately the extent of democracy (with the **POLITY_V** combined score, as well as its democracy (**DEMOC.**) and autocracy (**AUTOC.**) components in a separate regression). We also consider other types of governance measures such as the political stability (the number of years since last political regime change (**DURABLE**), also retrieved from the Polity V database), and the World Governance Indicators of government effectiveness (**GOV._EFF.**) and of control for corruption (**CONT.COR.**). Lastly, we include similar institutional quality measures stemming from alternative data sets such as V-DEM or the International Country Risk Guide (ICRG). Again, results from Table S.A11 highlight the stability of the coefficient associated with debt relief exposure, which ranges between 9 and 12 additional percentage points.

Table S.A10: Additional macro-economic covariates

Dep. var.: $PSA_{i,a,c,j}$	(1)	(2)	(3)	(4)	(5)	(6)
$VAR_{c,j}$:		TRADE	FDI_INF.	REMITT.	INVT.	GROWTH
$POST\_DP_{a,c,j}$	0.102***	0.102***	0.101***	0.143***	0.119***	0.100***
	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)
$VAR_{c,j}$		-0.000	-0.002	0.004**	0.001*	-0.003***
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	960,010	913,259	928,617	892,316	926,230	960,010
	(7)	(8)	(9)	(10)	(11)	(12)
$VAR_{c,j}$:	MANUF.	SERV.	CPI	RES_RENT.	URB_POP.	POP_DENS.
$POST\_DP_{a,c,j}$	0.086***	0.081***	0.118***	0.101***	0.100***	0.082***
	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)
$VAR_{c,j}$	-0.001	-0.003***	0.001***	0.001	0.002	0.275*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.17)
Observations	901,650	943,727	922,744	960,010	960,010	960,010

Notes: DiD estimates using DHS sampling probability weights. Robust standard-errors clustered at both the country × year-of-birth and the country × survey-year levels are shown in parentheses. All regressions include country, survey-year and year-of-birth fixed effects, as well as individual and macro-level controls (similar to those included in column (4) of Table 1 in the main manuscript, of which the regression results are reproduced in column (1) of the current table). In the above table, **TRADE** denotes trade openness (in % of GDP), **FDI_INF.** foreign direct investment inflows (in % of GDP), **REMITT.** remittances receipts (in % of GDP), **INVT.** gross fixed capital formation (in % of GDP), **GROWTH** the GDP per capita (in constant USD) growth rate (in %), **MANUF.** the manufacturing sector value added (as a GDP share), **SERV.** the services sector (as a GDP share), **CPI** the consumer price index (annual change, in %), **RES_RENT.** natural resource rents (in % of GDP), **URB_POP.** the urban population (in % of the total population), and **POP_DENS.** the population density (the average number of inhabitants per square km). ***, ** and * denote significance at 1%, 5% and 10% levels.

Table S.A11: Additional covariates for institutions quality

$PSA_{i,a,c,j}$	(1)	(2)	(3)	(4)	(5)	(6)
$VAR_{c,j}$:		POLITY_V	DEMOC.	AUTOC.	DURABLE	GOV_EFF.
$POST\_DP_{a,c,j}$	0.102***	0.088***	0.087***	0.086***	0.102***	0.120***
	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)
$VAR_{c,j}$		-0.003	-0.002	0.004	-0.000	-0.003
		(0.00)	(0.00)	(0.00)	(0.00)	(0.02)
Observations	960,010	960,010	905,594	905,594	960,010	872,806
$VAR_{c,j}$:	CONT.COR.	V-DEM	GOV._STAB.	DEMOC._ACC.	CORR.	BUR._QUAL.
$POST\_DP_{a,c,j}$	0.126***	0.094***	0.108***	0.104***	0.108***	0.111***
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
$VAR_{c,j}$	-0.030	-0.055	0.003	-0.017**	0.017**	0.007
	(0.02)	(0.05)	(0.01)	(0.01)	(0.01)	(0.02)
Observations	872,806	960,010	818,622	818,622	818,622	818,622

Notes: DiD estimates using DHS sampling probability weights. Robust standard-errors clustered at both the country $\times$ year-of-birth and country $\times$ survey-year levels are shown in parentheses. All regressions include country, survey-year and year-of-birth fixed effects, as well as individual and macro-level controls (similar to those included in column (4) of Table 1 in the main text, of which the regression is reproduced in column (1) of the current table). In the above table, *POLITY_V*, *DEMOC.*, *AUTOC.*, and *DURABLE* come from the **POLITY_V** database and denote the revised combined score of democracy extent, the extent of democracy, of autocracy and the regime durability, respectively. *GOV_EFF.* and *CONT.COR.* then measure government effectiveness and the degree of control over corruption, respectively and as defined by the **World Governance Indicators**. *V-DEM* is the electoral democratic index from the **V-DEM** database. Lastly *GOV._STAB.*, *DEMOC._ACC.*, *CORR.*, *BUR._QUAL.* represent the assessment of government stability, democracy accountability, control for corruption and bureaucratic quality, respectively and as assessed by the **International Country Risk Guide** data (ICRG). ***, ** and * denote significance at 1%, 5% and 10% levels.

4.2 Official development assistance and large scale programs for education

In order to control for the contribution of other types of financing for education, we add government education expenditures (measured in percentage of GNI) to the set of country-level covariates. While being potentially a bad control, we challenge the robustness of our results to its inclusion in the main regressions. Results in column (2) of Table S.A12 show that government spending dedicated to education is positively associated with a larger probability of attending primary school. When adding this control, we observe a slight reduction in the coefficient's magnitude. The lower contribution of debt relief to primary school attendance might result from the smaller study sample when controlling for public spending. It might also indicate that part of the debt relief effect on primary school attendance goes through providing additional financial means stemming from debt cancellations that helped finance more education expenditures, hence emphasizing fiscal space as a potential mechanism for debt relief effects.

In addition, since most low-income countries have benefited from official development assistance over the period studied, the effect of debt relief on education identified so far may be confounded with the scaling-up of foreign aid that the international community has committed to in view of the Millennium Development Goals. In order to control for such confounding factors, we add to the main regression the amount of aid received by sample countries (both HIPCs and non-HIPCs).[1] Since most foreign aid ends up in the public budget, we intentionally omit public expenditures dedicated to education in regressions when controlling for ODA. Results in column (3) of Table S.A12 show that, while larger amounts of net ODA (as a share of recipient country's GDP) are associated with a higher probability of primary school attendance, the coefficient associated with debt relief exposure is not altered.[2]

Yet, since not all official development assistance goes to education, one could suggest aid to the education sector as a more relevant control. However, due to data availability, this strategy would lead to considerably reduce the size of the sample, since sectoral aid commitments from the *Creditor Reporting System* (CRS) only cover years from 1995 onward, while data for disbursements start in 2002. Although less exhaustive, Figure S.A4 below suggests that aid to the education sector is strongly correlated with the aggregate net ODA supporting the latter as a good proxy for external official support to education. We nevertheless include aid to education as a control in column (4) of Table S.A12 which, when measured in terms of commitments, slightly reduces the sample size (as few HIPCs and non-HIPCs record DHS before 1995). Replacing net ODA by

[1]Using the Development Assistance Committee (DAC) database, we retrieve net official development assistance (ODA) disbursements (in percentage of GDP) for each country and year. Following Roodman (2006), we remove debt forgiveness grants from official grants and rescheduled debt from ODA loans to avoid accounting for debt relief effects in aid data.

[2]Having both government spending and net ODA in the same specification does not change the results either (results available on demand).

aid to education does not alter the effect of debt relief initiatives on primary school attendance, despite sample size reduction.

We then consider the contribution of the *Global Partnership for Education* (GPE) - one of the most important education programs (financed by international financial institutions) of the past decades - which could blur the effect of debt relief if not accounted for. In order to challenge the robustness of the correlation between exposure to debt relief and primary school attendance conditional upon the implementation of the GPE, we solely keep countries who joined the GPE into the control group. The "treatment" group still comprises all HIPCs, regardless of whether they have benefited from GPE resources or not.[3] Results are reported in columns (5) to (7) of Table S.A12. The effect of debt relief is still positive and significant without encountering any loss in terms of magnitude.[4]

Lastly, columns (8) and (9) show results when considering the entire sample and adding 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.[5] Such a differential effect might be explained by the amounts of debt cancelled, resulting in 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).[6]

[3] See Table S.A13 below for more information on GPE per country.

[4] Note that reducing the pool of control group countries based on their participation to GPE leads to drop around 25% of the observations, compared to main estimates.

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

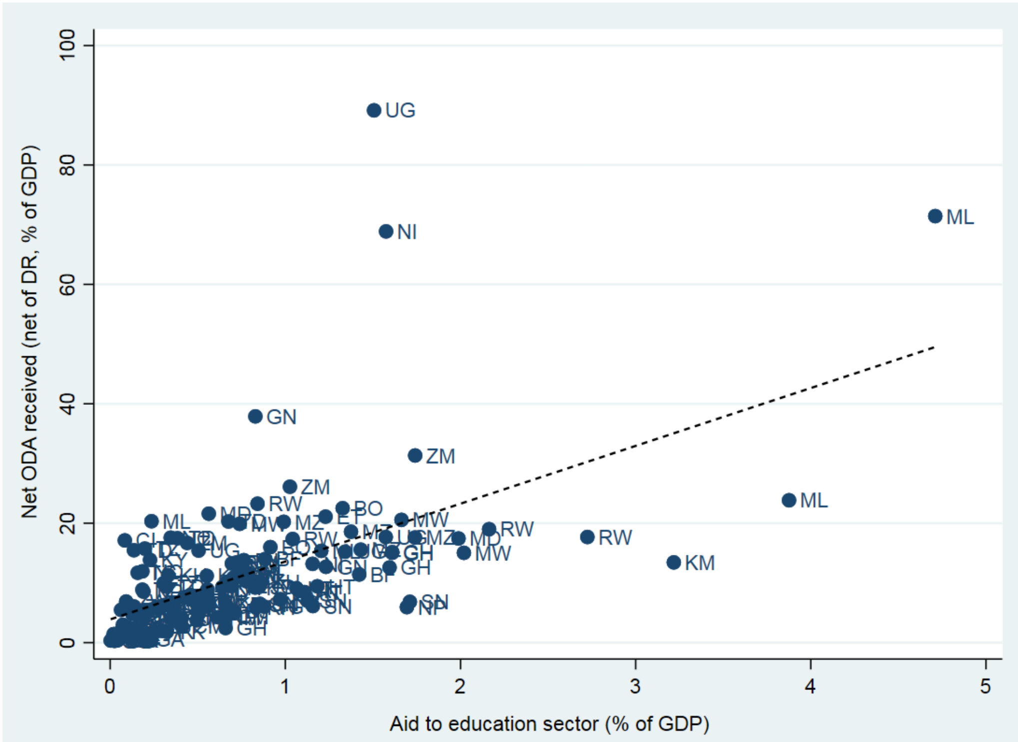
[6] Debt relief provided under the Enhanced HIPC initiative and the MDRI amounted to 77bn of USD as compared to 2.5bn granted under the overall GPE (Tables S.A13 and S.A14).

Table S.A12: Robustness checks - Control for ODA and Other Education Programs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dep. var.:	Primary School Attendance (at least 1 year)								
POST_DP$_{a,c,j}$	0.102***	0.096***	0.100***	0.123***	0.113***	0.112***	0.115***	0.103***	0.102***
	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
GPE exposure$_{c,j}$								-0.005	-0.004
								(0.02)	(0.02)
Gov. Educ. Exp. (% GNI)$_{c,j}$		0.003**							
		(0.00)							
Net ODA receiv. (% GDP)$_{c,j}$			0.001**			0.001*			0.001**
			(0.00)			(0.00)			(0.00)
Aid to education (% GDP)$_{c,j}$				0.008			0.012		
				(0.01)			(0.01)		
Observations	960,010	926,513	960,010	883,019	724,610	724,610	673,191	960,010	960,010
Sample	All	All	All	All	HIPC/GPE	HIPC/GPE	HIPC/GPE	All	All
No. of countries	41	41	41	39	31	31	30	41	41
Indiv. treated (HIPC)	412,972	384,539	412,972	412,972	412,972	412,972	412,972	412,972	412,972
Indiv. treated (GPE)	.	.	.	.	.	.	.	377,041	377,041
Indiv. treated (GPE only)	.	.	.	.	.	.	.	101,791	101,791

Notes: DiD estimates using DHS sampling probability weights. Robust standard-errors clustered at both the country × year-of-birth and country × survey-year levels are shown in parentheses. All regressions include country, survey-year, and year-of-birth fixed effects, as well as country-level and child-level controls as in the baseline estimate (column (4) of Table 1 in the main text). ***, ** and * denote significance at 1%, 5% and 10% levels.

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 survey 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, net from debt relief flows) in percentage of GDP, also retrieved from the OECD-DAC database. Correction for debt relief flows has been conducted in the same way as in Roodman (2006).

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 the figures have been retrieved from the *Global Partnership for Education*'s website.

Table S.A14: 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.

4.3 Sample dependence and sensitivity to clusters

In order to test the sensitivity of the results to the sample composition, we re-run the baseline estimates and drop countries from the sample (both HIPCs and non-HIPCs) one by one. This leads to 41 estimates for which coefficients of the variable of interest are reported in Table S.A15. We observe that, while the number of observations substantially differs with respect to the country excluded, the coefficients of debt relief remain positive and significant, indicating that main results are not driven by outliers in the treatment or control groups.

We repeat this procedure but now 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 columns (2) to (6) of Table S.A16 show that geographical location of control children does not affect the direction or statistical significance of the results. It suggests that our results are not driven by any particular regional trend in terms of primary school attendance which could have artificially generated the improved probability of attending primary school in HIPCs. Column (7) of Table S.A16 reports results when considering only children living in non-HIPCs that a large debt burden over the years preceding the first HIPC initiative. That way, control countries closely match the required eligibility criteria for the 1996 debt relief initiatives and constitute a better counterfactual at the country-level, although selection issues at the country-level may persist (Ferry, 2019). Our main results remain unaffected by such sample restrictions.

We then re-run the baseline specification while including in the sample children who were initially removed to prevent double counting. The sample is consequently made of children of primary age school, observed in each DHS available, although they might have been surveyed twice if DHS were conducted consecutively in a short period of time within the same country. Results reported in Table S.A17 below suggest that our selection process designed to avoid double counting children does not bias our estimates, as the coefficients closely match those reported in Table 1 of the main article.

Lastly, we challenge the robustness of our findings to the level at which standard errors are clustered. In almost every regression, standard errors are clustered at both the country $\times$ year-of-birth and the country $\times$ survey-year levels (multi-way clustering). Yet, one might worry that the unobserved component of primary school attendance likelihood is correlated between children of the same country, belonging to the same age-cohort (i.e. same year-of-birth) and observed the same year (i.e. same survey-year). In such a case, serial correlation would be observed at the treatment group level (Moulton, 1990). Furthermore, in the regressions mobilizing spatial data constructed at the enumeration area level, spatial correlation in error terms might also be an issue. Consequently, we re-run our main specification (i.e. column (4) of Table 1 in the main text) with standards errors clustered at different levels. Table S.A18 presents results when using alternative levels of clustering, intended to control for the several types of auto-correlation and errors contamination discussed above (especially at the spatial level, columns (5) to (8)). Overall, results are steady and do not seem to be affected by the level at which standard errors are clustered.

Table S.A15: Dropping each country one after another

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. var.:	Primary School Attendance (at least 1 year)						
Omitted country	NONE	ARM	BFE	BEN	BOL	CIV	CMR
$POST\_DP_{a,c,j}$	0.102***	0.099***	0.103***	0.101***	0.105***	0.103***	0.105***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Obs.	960,010	955,691	930,873	935,781	936,941	949,585	933,528
Obs. dropped	0.000	4,319	29,137	24,229	23,069	10,425	26,482
Omitted country	COL	DOM	EGY	ETH	GAB	GHA	GIN
$POST\_DP_{a,c,j}$	0.107***	0.115***	0.097***	0.093***	0.102***	0.106***	0.103***
	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
Obs.	930,170	931,557	918,181	935,181	955,331	939,615	940,681
Obs. dropped	29,840	28,453	41,829	24,829	4,679	20,395	19,329
Omitted country	HTI	IDN	JOR	KEN	KHM	COM	KGZ
$POST\_DP_{a,c,j}$	0.089***	0.098***	0.102***	0.085***	0.102***	0.110***	0.102***
	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)
Obs.	940,420	907,002	955,009	922,719	925,123	954,363	955,010
Obs. dropped	19,590	53,008	5,001	37,291	34,887	5,647	5,000
Omitted country	MAR	MDG	MLI	MWI	MOZ	NIC	NGA
$POST\_DP_{a,c,j}$	0.102***	0.097***	0.091***	0.102***	0.104***	0.106***	0.102***
	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Obs.	954,030	935,334	923,752	930,921	936,284	944,969	909,332
Obs. dropped	5,980	24,676	36,258	29,089	23,726	15,041	50,678
Omitted country	NER	NAM	NPL	PER	PAK	RWA	SEN
$POST\_DP_{a,c,j}$	0.105***	0.098***	0.104***	0.097***	0.104***	0.093***	0.099***
	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)
Obs.	928,839	947,533	946,314	910,196	945,155	936,729	939,006
Obs. dropped	31,171	12,477	13,696	49,814	14,855	23,281	21,004
Omitted country	TCD	TOG	TZA	UGA	YEM	ZMB	ZWE
$POST\_DP_{a,c,j}$	0.107***	0.096***	0.107***	0.097***	0.108***	0.120***	0.101***
	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)
Obs.	925,362	944,393	940,976	934,786	940,470	929,671	943,587
Obs. dropped	34,648	15,617	19,034	25,224	19,540	30,339	16,423

Notes: DiD estimates using DHS sampling probability weights. Robust standard-errors clustered at both the country × year-of-birth and country × survey-year levels are shown in parentheses. All regressions include country, survey-year and year-of-birth fixed effects, as well as country-level and child-level controls as in the baseline estimate (column (4) in Table 1 in the main text). ***, ** and * denote significance at 1%, 5% and 10%.

Table S.A16: Dropping each region one after another

	(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$_{a,c,j}$	0.102***	0.105***	0.083***	0.086***	0.113***	0.101***	0.122***
	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
Observations	960,010	926,150	785,006	832,815	851,903	843,564	805,811
No. of countries	41	37	33	35	38	37	33
No. of obs. dropped	.	33,86	175,004	127,195	108,107	116,446	154,199

Notes: DiD estimates using DHS sampling probability weights. 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). Robust standard-errors clustered at both the country $\times$ year-of-birth and the country $\times$ survey-year levels are shown in parentheses. All regressions include country, survey-year, and year-of-birth fixed effects, as well as country-level and child-level controls as in the baseline estimate (column (4) in Table 1 in the main text). ***, ** and * denote significance at 1%, 5% and 10%.

Table S.A17: Baseline results with overlap

	(1)	(2)	(3)	(4)
Dep. var. ($PSA_{i,a,c,j}$):	Primary School Attendance (at least 1 year)			
$POST\_DP_{i,a,c,j}$	0.126***	0.171***	0.161***	0.112***
	(0.02)	(0.02)	(0.02)	(0.02)
$Girl_{i,a,c,j}$		-0.021***	-0.021***	-0.021***
		(0.01)	(0.01)	(0.01)
Parent Educ: $Primary_{i,a,c,j}$		0.188***	0.184***	0.185***
		(0.01)	(0.01)	(0.01)
Parent Educ: Sec. or $tertiary_{i,a,c,j}$		0.216***	0.217***	0.218***
		(0.01)	(0.01)	(0.01)
Head's $child_{i,a,c,j}$		0.006**	0.005	0.004
		(0.00)	(0.00)	(0.00)
Wealth $index_{i,a,c,j}$		0.034***		
		(0.00)		
1st wealth quintile $(Q1)_{i,a,c,j}$			-0.124***	-0.123***
			(0.01)	(0.01)
2nd wealth quintile $(Q2)_{i,a,c,j}$			-0.087***	-0.086***
			(0.01)	(0.01)
3rd wealth quintile $(Q3)_{i,a,c,j}$			-0.063***	-0.064***
			(0.01)	(0.01)
4th wealth quintile $(Q4)_{i,a,c,j}$			-0.038***	-0.038***
			(0.01)	(0.01)
$Rural_{i,a,c,j}$		-0.054***	-0.061***	-0.061***
		(0.01)	(0.01)	(0.01)
GDP per cap. (log, const. $USD)_{c,j}$				-0.017
				(0.04)
Population under 15 $(log)_{c,j}$				0.276***
				(0.08)
Observations	1,989,688	1,224,642	1,224,642	1,221,093
No. of indiv. treated	633,622	480,688	480,688	480,688
No. of countries	44	41	41	41

Notes: DiD estimates using DHS sampling probability weights. Robust standard-errors clustered at both the country × year-of-birth and country × survey-year levels are shown in parentheses. All regressions include country, survey-year and year-of-birth fixed effects. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10% levels.

Table S.A18: Sensitivity to clusters

Dep. var.: $PSA_{i,a,c,j}$	(1)	(2)	(3)	(4)
Post_DP$_{a,c,j}$	0.102***	0.102***	0.102***	0.102***
	(0.02)	(0.02)	(0.02)	(0.02)
Observations	960,01	960,01	960,01	960,01
Clusters	CxS & CxY	CxSxY	CxY	CxS
Dep. var.: $PSA_{i,a,c,j}$	**(5)**	**(6)**	**(7)**	**(8)**
Post_DP$_{a,c,j}$	0.102***	0.102***	0.102***	0.102***
	(0.01)	(0.00)	(0.00)	(0.02)
Observations	960,01	960,01	960,01	960,01
Clusters	CxGPS_id xS	CxGPS_id xY	CxGPS_id xSxY	CxSxY & CxRegion_id

Notes: DiD estimates using DHS sampling probability weights. Robust standard-errors are clustered at different levels: country × survey-year and country × year-of-birth (multi-way clustering: C×S and C×Y) in column (1), country × survey-year × year-of-birth (C×S×Y) in column (2), country × year-of-birth (C×Y) in column (3), country × survey-year (C×S) in column (4), country × DHS GPS id (of enumeration area) × survey-year (C×GPS_id×S) in column (5), country × DHS GPS id × year-of-birth (C×GPS_id×Y) in column (6), country × DHS GPS id × survey-year × year-of-birth (C×GPS_id×S×Y) in column (7), and country × survey-year × year-of-birth and country × Region identifier (multi-way clustering: C×S×Y and C×Region_id) in column (8). All regressions include the controls presented in Table 1 in the main text, country, survey-year and year-of-birth fixed effects. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10% levels.

4.4 Room for improvement and educational trends

Countries that benefited from the HIPC initiative had significantly lower primary school attendance rates before the program began. The average gross primary school attendance rate in 1999 was 80% in HIPC countries and 105% in non-HIPC countries. HIPCs thus had (on average) greater room for improvement in terms of primary school attendance with respect to control group countries, which could partly explain the reported positive effect. The average increase observed in primary school attendance in the post-decision point period might also reflect a catching-up process among HIPCs, which could have taking place anyway, regardless of debt relief. In what follows, we assess the differential effect of debt relief according to countries' room for improvement in primary school attendance. Doing so also enables us to assess whether the contribution of debt relief to primary school attendance identified throughout the paper stems from debt relief initiatives or only captures a catching-up effect of HIPCs, which were initially lagging behind in terms of primary school attendance. To this end, we need to compute the level of primary school attendance before the decision point for HIPCs and before 1999 for non-HIPCs.[7]

The computation of ex-ante levels of education relies on two data sources. We first use the gross primary enrolment rate (GER) in 1999 provided by the World Bank (columns (2) to (6) in Table S.A19).[8] Second, DHS data on primary school attendance are aggregated at the country level (using DHS sample weights) to obtain gross primary school attendance rates (GAR) before 2000 (columns (7) and (8) in Table S.A19). 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 reduces the sample by 26%.

First, we estimate the baseline specification using a reduced sample for non-HIPCs including only non-HIPCs that recorded a low level of primary gross enrolment rate in 1999. This strategy enables us to compare the evolution of primary school attendance in the aftermath of debt relief to the same evolution in non-HIPCs that also had room for improvements in primary schooling. Results in columns (2) to (4) in Table S.A19 show that the coefficient associated with debt relief remains stable. We then interact the variable denoting exposure to debt relief with initial levels of education, using alternately primary school enrolment from the World Bank and DHS attendance rates. Results support the main findings of a positive average contribution of debt relief and also show that the debt relief initiative had a higher (albeit marginal) impact in countries that were initially lagging behind in terms of primary school attendance (columns (5) to (8) in Table S.A19 below).[9]

[7] We chose 1999 as it provides primary school attendance before the Enhanced HIPC initiative was launched.

[8] We use the World Bank primary enrolment rates because time series for primary school attendance are less available and do not allow us to capture the extent of primary school attendance in the early 1990s.

[9] As an example, we can take Niger and Togo, two HIPC countries, with very different initial gross

Lastly, some readers may worry that DiD results simply reflect a temporal trend in education performances, different for HIPCs and non-HIPCs, which should not be attributed to debt relief initiatives. In order to account for potential trends in education, we augment the main specification (with and without controls, apart from fixed-effects) with HIPC-specific year-of-birth trends and its quadratic expression (columns (1) to (4) in Table S.A20). We also add country-specific year-of-birth linear trends (columns (5) and (6)) and their quadratic terms (columns (7) to (8)). Adding specific time trends does not alter the results, as coefficients associated with the $POST\_DP$ variable remain significant and at the same level of magnitude.

primary school attendance rates, with a much higher rate in Togo (120%) than in Niger (31%). Estimates suggest that, everything else being equal, debt relief led to an increase in school attendance of between 9 and 18 percentage points higher in Niger than in Togo.

Table S.A19: Reduced control group and initial level of education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. var.:	Primary School Attendance (at least 1 year)							
POST_DP$_{a,c,j}$	0.102***	0.115***	0.115***	0.111***	0.200***	0.173***	0.236***	0.197***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.04)	(0.05)	(0.04)	(0.05)
POST_DP X					-0.001**	-0.001**	-0.002***	-0.002**
Initial level of education$_{a,c,j}$					(0.00)	(0.00)	(0.00)	(0.00)
Observations	960,010	748,869	697,559	692,559	1,442,405	925,565	1,397,630	889,130
Controls	Yes	Yes	Yes	Yes	No	Yes	No	Yes
Non-HIPC countries selected	All	GER^a ≤105	GER^a ≤100	GER^a ≤95	All	All	All	All
Initial level of education	-	-	-	-	GER in 1999 (WBK)^a	GER in 1999 (WBK)^a	GAR prior to 2000 (DHS)^b	GAR prior to 2000 (DHS)^b

Notes: DiD estimates using DHS sampling probability weights. Robust standard-errors clustered at both the country × year-of-birth and the country × survey-year levels are shown in parentheses. GAR stands for gross attendance rate rate and GER for gross enrollment rate. ^{a}: GER are computed using data from the World Bank in 1999. ^{b} : GAR are computed using, for each country, DHS closest to 1999 (excluding those before 1996). All regressions include country, survey-year, and year-of-birth fixed effects, as well as country-level and child-level controls as in the baseline regression when mentioned in the table. ***, ** and * denote significance at 1%, 5% and 10%.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. var.:	Primary School Attendance (at least 1 year)							
$POST\_DP_{a,c,j}$	0.073**	0.116***	0.136***	0.206***	0.137***	0.175***	0.137***	0.175***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)
Observations	1,548,492	960,010	1,548,492	960,010	1,548,492	960,010	1,548,492	960,010
No. of countries	44	41	44	41	44	41	44	41
HIPC-specific yob trend	Yes	Yes	Yes	Yes	No	No	No	No
HIPC-specific yob trend^2	No	No	Yes	Yes	No	No	No	No
Country-specific yob trend	No	No	No	No	Yes	Yes	Yes	Yes
Country-specific yob trend^2	No	No	No	No	No	No	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: DiD estimates using DHS sampling probability weights. Robust standard-errors clustered at both the country × year-of-birth and the country × survey-year levels are shown in parentheses. All regressions includes country, survey-year, and year-of-birth fixed effects. Macro and micro-level controls are also entered in some regressions (columns (2), (4), (6) and (8)). ***, ** and * denote significance at 1%, 5% and 10%.

4.5 Additional results for the spatial difference-in-discontinuity model

In the core article, we discuss the relevance of the spatial difference-in-discontinuity as an alternative model. Yet, the availability of geocoded DHS, the information that they contain, and the geographic concentration of HIPCs hamper the implementation of such an empirical strategy on our full sample of DHS. As explained in the Section 4.4 of the article, the requirements for conducting a proper spatial difference-in-discontinuity model lead us to drop controls for parents' education and relationship to the head of the household. This may be an issue as parents' education has been shown to be an important determinant of children's educational outcomes. Removing these controls could therefore lead to biased estimates. If parents are more educated in non-HIPCs and assuming an intergenerational human capital transmission, the effect of the HIPC initiative is likely to be underestimated.

An alternative strategy is to rely on a smaller sample of HIPC and non-HIPC border countries with geo-coded information on parents' education and relation with the head of the household for cohorts both before and after the HIPC initiative. This sample includes three pairs of countries: Haiti and the Dominican Republic, Benin and Nigeria, and Cameroon and Nigeria. Table S.A21 presents the results of the spatial difference-in-discontinuity model for this restricted sample with controls for parental education and relation with the head of household. They show a significant positive and strong impact of the HIPC initiative on primary school attendance for individuals living within 20-50 km of the border, regardless of the specification used. The magnitude of the effect is larger than that found with the double difference strategy, with an increase in attendance following debt relief of 13.9 to 22.0 percentage points. Yet given the very small dimension of our sample, these results should only be considered as an illustrative country-case study supporting our main findings.

Table S.A21: Difference-in-Discontinuity - restricted sample estimates

	(1)	(2)	(3)	(4)
Dep. var.:	Primary School Attendance$_{i,a,c,j}$ (at least 1 year)			
Bandwith:	200km	100km	50km	20km
Specification (smooth function for distance)				
Linear				
POST_DP$_{a,c,j}$	-0.002	0.103	0.177**	0.185***
	(0.07)	(0.07)	(0.08)	(0.06)
Linear spline				
POST_DP$_{a,c,j}$	-0.028	0.056	0.189**	0.139**
	(0.07)	(0.07)	(0.08)	(0.07)
Quadratic				
POST_DP$_{a,c,j}$	-0.002	0.104	0.177**	0.183***
	(0.07)	(0.07)	(0.08)	(0.06)
Quadratic spline				
POST_DP$_{a,c,j}$	-0.059	0.095	0.220**	0.144**
	(0.07)	(0.07)	(0.09)	(0.07)
Observations	123,995	72,223	37,355	14,105
No. of indiv. treated	74,797	47,254	24,342	9,164

Notes: Difference-in-discontinuity results stem from estimates using DHS sampling probability weights. Robust standard-errors clustered at both the country × year-of-birth and country × DHS GPS id levels are shown in parentheses. The sample include three pairs of border countries: Haïti and Dominican Republic; Benin and Nigeria; and Cameroon and Nigeria. Each regression includes controls for gender, parents' education, relationship to the head of the household, rural residence and country-level controls presented in Table 1 (in the main text) . Country, survey-year and year-of-birth fixed effects are also included in each regression. Constant term not reported in order to save space. ***, ** and * denote significance at 1%, 5% and 10% levels.

5 Fiscal space heterogeneity: additional results

Table S.A22: Investigating (per capita) fiscal space heterogeneity

Dep. var.: $PSA_{i,a,c,j}$	(1)	(2)	(3)	(4)	(5)
Channel (USD per capita):	Govt.Educ.Exp.	Debt service savings from debt relief (DSS)			
Debtor History (DH:)				Good payers	
$\text{POST-DP}_{a,c,j}$	0.127***				
	(0.02)				
POST-DP X $\text{Channel}_{a,c,j}$	-0.001**	-0.005		-0.017***	
	(0.00)	(0.00)		(0.01)	
POST-DP X $\text{Channel\_HIPC}_{a,c,j}$			-0.011		-0.047***
			(0.01)		(0.01)
POST-DP X $\text{Channel\_MDRI}_{a,c,j}$			-0.001		0.013
			(0.01)		(0.01)
Conditional effect w/r to DH					
POST-DP X Channel X $\text{DH}_{a,c,j}$				0.020***	
				(0.01)	
POST-DP X Channel_HIPC X $\text{DH}_{a,c,j}$					0.055***
					(0.02)
POST-DP X Channel_MDRI X $\text{DH}_{a,c,j}$					-0.010
					(0.01)
Observations	926,513	948,574	948,574	948,574	948,574
No. of countries	41	41	41	41	41

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. The measure of debt service savings initially denominated in percentage of GDP has been then transformed to be expressed in current USD per capita. DiD estimates using DHS sampling probability weights. Robust standard-errors clustered at both the country × year-of-birth and country × survey-year levels are shown in parentheses. Country, survey-year and year-of-birth fixed effects 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% levels.

6 Spatial data

6.1 Sample of geo-coded DHS

Sections 4.4 and 5.4 in the main article discuss results based on geo-spatial data. To compute spatial statistics in the surrounding areas of DHS children, we resort to a subset of DHS recording the longitude and latitude of enumeration areas (hereafter called geocoded DHS). However, the number of surveys with spatial coordinates is much more reduced than our original DHS sample. The limited number of geocoded DHS thus threatens the difference-in-differences structure of our empirical strategy. Indeed, for some HIPCs, only the most recent DHS display longitudes and latitudes, which prevents having the before dimension. Similarly, some control group countries only have geocoded DHS for the most recent years and can no longer be considered a relevant counterfactual for the period preceding the HIPCs' decision point.

Restricting the sample to HIPCs with geocoded DHS both before and after the decision point year and to non-HIPCs with geocoded data before and after the decision point year of at least one HIPC reduces the sample to 21 countries (12 HIPCs and 9 non-HIPCs). Table S.A23 below shows the sample and the survey-year of geocoded DHS.

Table S.A23: Sample of geocoded DHS selected

HIPCs	Geocoded DHS				
Benin	1996	2001	2012		
Burkina Faso	1993	2003	2010		
Cameroon	1991	2004	2011		
Cote d'Ivoire	1994	2012			
Ghana	1993	1998	2003	2008	2014
Guinea	1999	2005	2012		
Haiti	2000	2006	2012		
Madagascar	1997	2008			
Mali	1996	2001	2006	2012	
Senegal	1993	2005	2010	2012	2015
Tanzania	1999	2010			
Togo	1998	2013			
Non-HIPCs					
Cambodia	2000	2005	2010	2014	
Dominican Republic	2007	2013			
Egypt	2000	2005	2008	2014	
Kenya	2003	2014			
Namibia	2000	2013			
Nepal	2001	2011			
Nigeria	2003	2008	2013		
Peru	2000	2004	2009		
Zimbabwe	1999	2005	2010		

Based on this sample, we identify 463,852 children of primary school age for whom individual-level data are available. Results mobilizing spatial data thus need to be interpreted with caution compared to main results, as they are obtained on half of the original sample.

6.2 Distance to the border

One of the main assumptions underlying the validity of spatial difference-in-discontinuity is the relatively exogenous definition of national borders, especially in sub-Saharan Africa, since most of these borders result from the Berlin conference held at the end of the 19^{th} century. We use the geolocation of DHS enumeration areas of sample children and the geocoded national borders from the **GADM** website. For children in HIPCs (resp. non-HIPCs), we compute the distance from their geocoded enumeration area to a non-HIPC (resp. HIPC) neighbor's border. For countries like Nigeria or Zambia that have two potential neighbors, we retrieve the closest distance between each enumeration area and the neighbors' border.

However, readers should consider this measure as a proxy of the distance to the border since it has been computed as the straight distance, which implies for enumeration areas far from the border to cross the sea or the border of other countries, which are not in the sample. However, for enumeration areas near the border, this measure remains accurate. Considering HIPCs and non-HIPCs sharing a common border and being part of the geocoded DHS sample, we compute the distance to the border for 8 pairs of countries: Haïti and the Dominican Republic, Tanzania and Kenya; Uganda and Kenya; Benin and Nigeria; Cameroon and Nigeria; Zambia and Zimbabwe; Zambia and Namibia; and Bolivia and Peru.

6.3 Population density in children's surrounding area

Section 5.4 of the core article investigates the differential effect of debt relief on primary education with respect to children's remoteness. The first indicator considered as a proxy for remoteness and distance to education facilities is the population density in the surrounding area of the DHS children.

To compute population density, we start by mapping the location of the DHS children's area using the latitudes and longitudes of their respective enumeration area. Yet, this does not represent individuals' exact location, as the DHS program deliberately adds a margin of error in the spatial coordinates of enumeration areas of up to 5km and 2km in rural and urban areas, respectively, in order to preserve respondents' location anonymity. We then draw a buffer area with a radius of alternately 20 and 50km around the enumeration area location (which is thus the centroid of the buffer), correcting for the sea surface as some respondents live near the coast and also for homeland surface since some others live near the national borders.

We next resort to the **Gridded Population of the World** (GPW) (v.4.11) from the SEDAC in order to get a raster of population distribution within each sample country. However, GPW data provide population raster in a five-year window, from 1990 to 2015, which differs from the survey-year of the geocoded DHS. Consequently, we decide to match each geocoded DHS survey-year with the closest year available in GPW, as reported in Table S.A24 below. Using the UN population count data, we extract the number of individuals within the buffer area and then divide the total number of individuals by the total surface of the buffer area (defined in square kilometers[10]). We thus obtain the average number of people per square kilometer within the surrounding area of each DHS child.

Table S.A24: Matching Geocoded DHS with GPW datasets

	Gridded Population of the World datasets					
	1990	1995	2000	2005	2010	2015
Geocoded DHS year						
Benin		1996	2001		2012	
Burkina Faso		1993		2003	2010	
Cambodia			2000	2005	2010	2014
Cameroon	1991			2004	2011	
Cote d'Ivoire		1994			2012	
Dominican Republic				2007		2013
Egypt			2000	2005	2008	2014
Ghana		1993	1998	2003	2008	2014
Guinea			1999	2005	2012	
Haiti			2000	2006	2012	
Kenya				2003		2014
Madagascar			1997		2008	
Mali		1996	2001	2006	2012	
Namibia			2000			2013
Nepal			2001		2011	
Nigeria				2003	2008	2013
Peru			2000	2004	2009	
Senegal		1993		2005	2010,12	2015
Tanzania			1999		2010	
Togo			1998			2013
Zimbabwe			1999	2005	2010	

Note that we also try to use a different population count, not UN adjusted, to compute population density. Results do not differ from those reported in the core article and are available upon request to the authors.

[10]Which can differ according to the location of the enumeration area, as some buffer zones are cropped to avoid considering sea or land surface in neighboring countries.

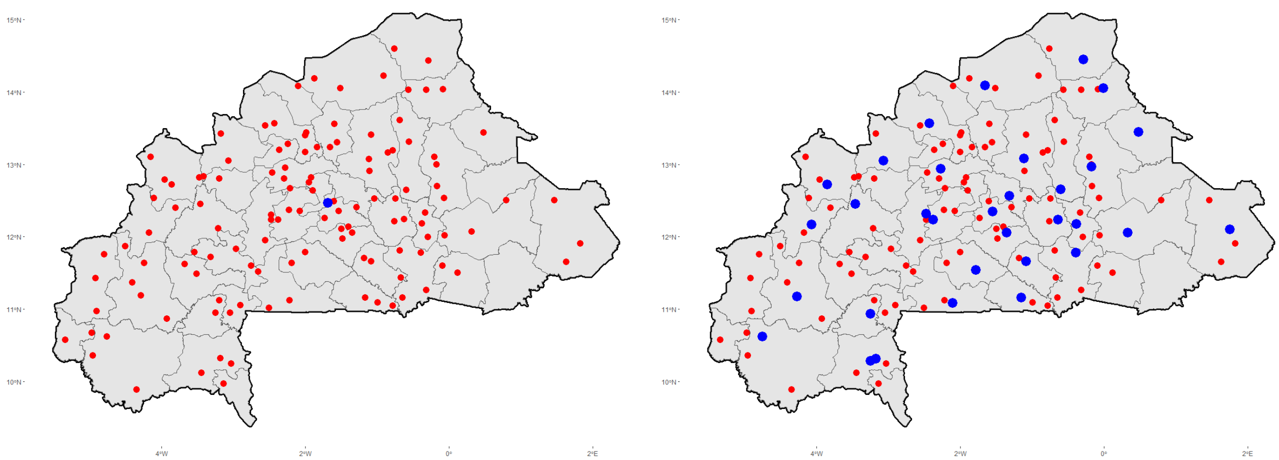
6.4 Distance to large cities and urban areas

As a second indicator of remoteness, we consider the distance of children's enumeration area to the closest large city or large urban areas. Given the temporal dimension of the dataset, finding a list of large cities based on their historical population, is somehow challenging. We first retrieve data from the **World Database of Large Urban Areas** (https://nordpil.com/resources/world-database-of-large-cities/), which records the largest urban areas since 1950. Restricting the dataset down to sample HIPCs and non-HIPCs, we end up with information for urban areas with a population of at least 200,000 in 1990 (the first year of geocoded DHS available in the sample). Unfortunately, this database does not add new large urban areas as the years go by, thus omitting other large urban areas that developed after the early 1990s. Consequently, for most HIPCs and non-HIPCs, these areas only represent the capital cities. For instance, the World Database on Large Urban Areas only reports Ouagadougou as a large urban area for Burkina Faso (Figure S.A5, left graph), whereas in the early 1990s, other cities such as Bobo-Dioulasso (the economic capital) were probably large enough (in terms of population) to host primary schools.

Consequently, using the World Database on Large Urban Areas probably overestimates the distance to the closest large urban area for some children of our sample. For this reason, we mobilize an alternative source of data, the **World Cities Database**, which provides the coordinates of the largest cities (with 10,000 inhabitants or more) by country as of 2020. Compared to the World Database on Large Urban Areas, the World Cities Database records many more large cities (Figure S.A5, right graph), as they are observed more recently. However, given the absence of temporal dimension, resorting to these data for DHS collected in the early 1990s implicitly assumes that the recorded cities in late 2020 were already large at that time. It might not be the case for most developing countries, given their rapid growth and urban expansion. Therefore, and conversely to the World Database on Large Urban Areas, distance from the closest large city stemming from the World Cities Database is probably underestimated for some children observed in older DHS.

Despite these shortcomings in terms of spatial and temporal coverage, results of Section 5.4. in the core article suggest that children located further away from large urban areas or large cities benefit disproportionately from debt relief initiatives, regardless of the database used.

Figure S.A5: Large urban areas vs. Large cities



Notes: Red dots denote 1993 Burkina Faso DHS enumeration areas. Blue dots represent historical large urban areas (retrieved from the World Database on Large Urban Areas) and large cities (as of 2020, and obtained from the World Cities Database) on the left and right map, respectively.

6.5 Distance to primary and secondary roads

We also rely on distance to the nearest primary or secondary road as another remoteness measure. Distance to roads is used to assess the differential impact of debt relief on children with lower connectivity infrastructure since children being located far away from a primary or a secondary road might have less access to school, and therefore a lower probability of attending primary school, compared to children living near a road. The coefficient associated with the interaction term between debt relief exposure and distance to roads reveals whether debt relief contributed to lifting up connectivity constraints (for instance by building schools in remote areas with few accessible roads) or conversely failed to alleviate this kind of geographic poverty trap.

We decide only to consider highways, primary and secondary roads, as other types of roads such as tertiary roads, private roads, or trails might not be accessible for buses, cars, or motorcycles and might not be of good quality enough to allow children to reach distant educational facilities,

Distance to the nearest primary or secondary road is computed by crossing coordinates of DHS enumeration areas with spatial lines from raster data on primary and secondary roads. Yet, as for large urban areas and cities, and to our knowledge, there is no (free) available data on the yearly evolution of roads in developing countries. Reference data come from the **Digital Chart of the World** (DCW), which consists of a digital map of Earth geo-coding a wide range of information, such as country boundaries, public utilities, transportation structure, and roads. However, the information available in the DCW has not been updated for years. It provides a representation of roads (by categories such as highway, primary, secondary roads, or trails) as of 1992. Using these data to calculate the distance between DHS enumeration areas and the nearest primary or secondary road, regardless of the DHS year, implicitly assumes that recorded roads as of 1992 persisted throughout the following years, and more questionably, that no roads have been built since.

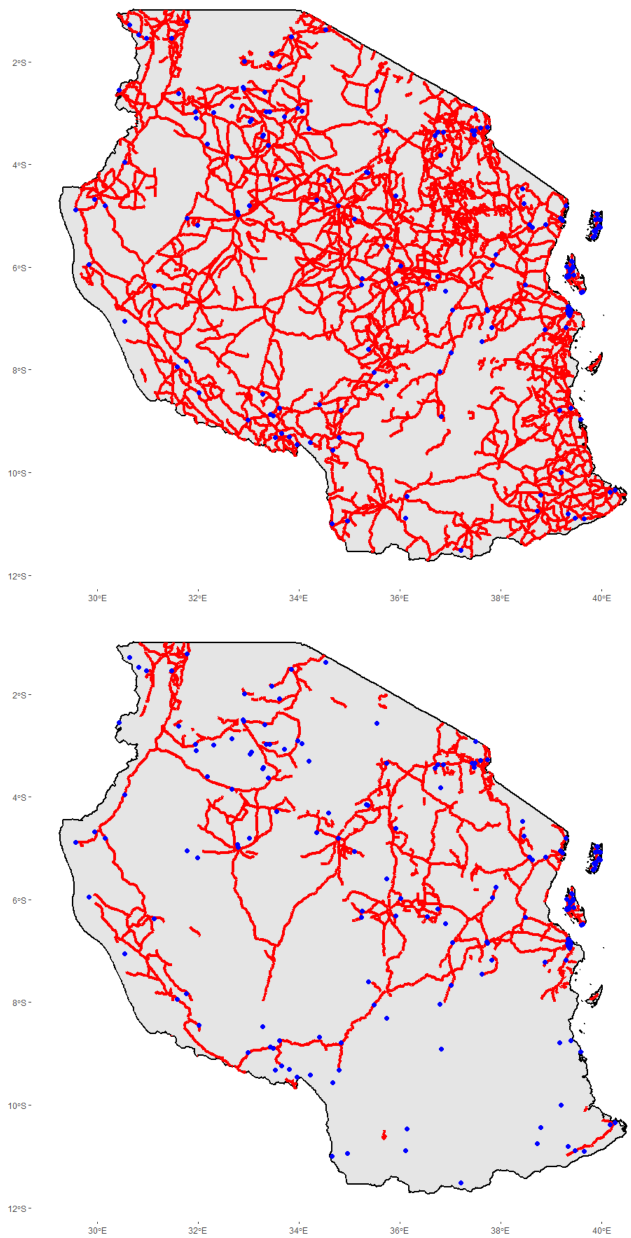
Besides this time limitation, criticisms have also emerged regarding several errors in the DCW database, which led to a major revision in 1997. We therefore also mobilize the **VMAP-0** (a vector-based collection of geographic data), which is a more recent version of the DCW database. However, while there is no clear statement on temporal coverage, some information suggests that the data record geolocation of roads as of 1997.

Lastly, we considered a third data source for geocoded roads: the **Global Roads Open Access Data Set** (gROADS) available from the SEDAC website. It has the advantage of having a wider spatial and temporal coverage than the DCW and the VMAP-0. Yet, when exploring the data, we ended up with no records of primary and secondary roads for sample countries, making it difficult to know whether the observed roads are real transport infrastructure, a private road, or a trail. Most of the roads recorded in gROADS for African countries are considered unspecified, so we chose not use this database to calculate distance to roads.

Consequently, the distance to the nearest primary or secondary roads that have been used relies on roads data stemming from the Digital Chart of the World (available at the Harvard Geospatial Library) and the VMAP-0 retrieved from the following website (`https://gis-lab.info/qa/vmap0-eng.html`). Results of Table A7 in the article are based on the distance to roads obtained thanks to the DCW. Results with the VMAP-0 are not reported in order to save space but are highly similar to those reported with DCW. However, we stress that such results must be interpreted with caution as the record of primary and secondary roads does not seem to be of high accuracy and varies widely from one country to another.

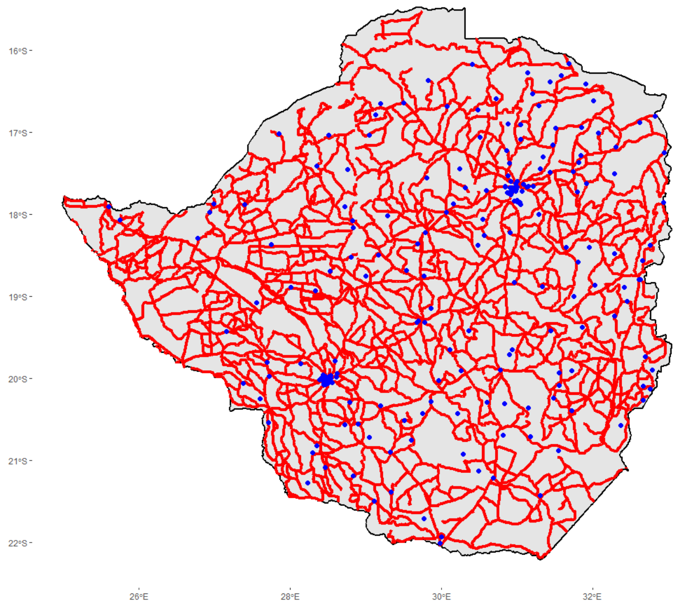
Indeed, for some countries, such as Tanzania, the DWC (and the VMAP-0) database seems to clearly differentiate primary and secondary roads from other types of roads. Figure S.A6 below plots the map of Tanzania along with DHS enumeration area (of the 1999 DHS) and spatial lines denoting roads as recorded in the DCW. The upper map of Figure S.A6 reports all kinds of roads while the lower map displays primary and secondary roads only. One can see that, for Tanzania, there is a pretty good coding of roads' importance, as the second map covers much fewer roads than the upper one. For other countries, such as Zimbabwe, the difference between the two same maps is much thinner. Indeed, Figure S.A7 suggests that most of the country's roads can be considered primary or secondary roads since there is little difference between the two maps (except for northern and southern regions of Zimbabwe). Consequently, and based on these two examples, we do not really know the degree of accuracy of the track record of primary and secondary roads for some countries.

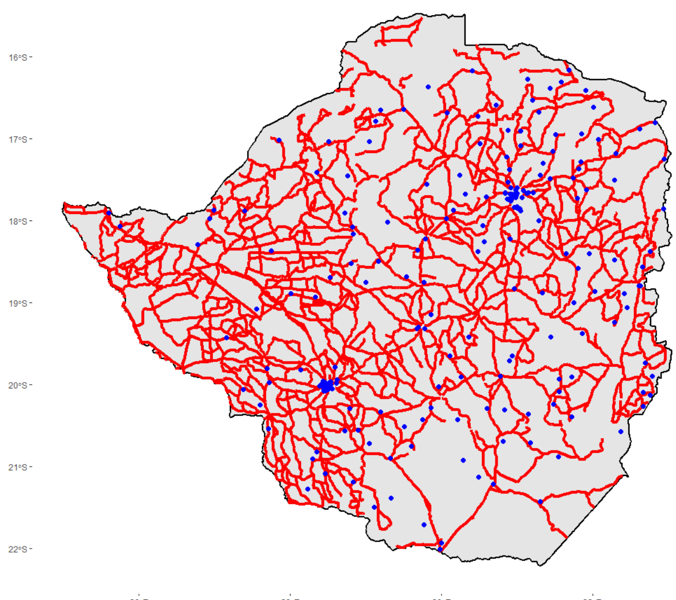
Figure S.A6: Accuracy in assessing primary and secondary roads (DCW) - Tanzania



Notes: Blue dots denote 1999 Tanzania DHS enumeration areas. Red lines in the upper map represent all recorded roads in the DCW, while these of the map below consist in primary and secondary roads as coded in the DCW (based on the modalities recorded in the *RDLNTYPE* variable).

Figure S.A7: Accuracy in assessing primary and secondary roads (DCW) - Zimbabwe





Notes: Blue dots denote 1999 Zimbabwe DHS enumeration areas. Red lines in the upper map represent all recorded roads in the DCW, while these of the below map consist in primary and secondary roads as coded in the DCW (based on the modalities recorded in the *RDLNTYPE* variable).

Considering these limitations in geocoded roads data, we consider our measure of distance to roads as an estimation (albeit incomplete) of children's degree of remoteness and school accessibility and advice interested readers to interpret the results based on the distance from the nearest primary or secondary roads (in Table A6 of the core article) with caution.

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