Tuition fees and the intra-household allocation of schooling: Evidence from Uganda's Free Primary Education reform

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Abstract

Many education policies in low income countries impose eligibility limits, and these can generate household responses that distribute educational investments across children. In this paper, I study the effects of eligibility limits in the context of the Universal Primary Education (UPE) reform in Uganda. The program abolished elementary school fees for up to four children per household, with families paying fees on each additional child. Depending on the composition of children and their age at the onset of the reform, the policy generates costs for primary school that vary both within and across households. Children that are eligible for the tuition waiver but live in households with ineligible siblings complete fewer years of schooling. I estimate that the presence of ineligible siblings wiped out almost half of the overall impact of the UPE reform on educational attainment, with the effects concentrated among the poorest households. Household responses to the subsidy program are similar to those found in other types of schooling programs such as conditional cash transfers.

JEL classification:

J13, I25, O12

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Tuition costs, Schooling attainment, Sibling spillovers, Intra-household Allocation, Education, Universal Primary Education, Uganda, Africa.

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1 Introduction

Policies targeting household investments in children's education often impose eligibility rules that exclude some potential beneficiaries. For example, conditional cash transfer programs (CCTs), voucher programs, and other types of scholarship programs may cap the amount of transfer a household can receive, or may limit the number of children eligible. While such rules are often necessary to ensure the sustainability of such programs, they can lead households to reallocate resources among children. For example, faced with a situation where only some of their children are targeted for a benefit, parents may concentrate household resources towards targeted children; alternatively, they can redistribute those resources to provide equally to all children. These responses can have important implications for policy evaluation, as they can generate spillovers between "treated" and "untreated" siblings (Barrera-Osorio et al., 2011, Lincove and Parker, 2016, Ferreira et al., 2017). While spillovers have been documented in transfer programs, to date there is little evidence from school subsidy programs. Such programs lower tuition and attendance fees, which are an important barrier to student achievement (Sakaue, 2018, Borkum, 2012, Lincove, 2012, Glick and Sahn, 2006).

In this paper, I provide estimates of the effect of eligibility limits in the context of a tuition subsidy program in Uganda. The subsidy was part of the Universal Primary Education (UPE) reform which provided free primary schooling but imposed limits on the number of school fee waivers that a household could receive. Under the program, four children per household could attend school without paying school fees, but those waivers did not apply to additional pupils in the household. Because of large family sizes in Uganda, this cap on tuition waivers was frequently binding, introducing some variation in the cost of education across and within households. The paper thus provides a contribution to the literature of household responses to schooling costs.

I use the tuition waiver eligibility requirement to identify the effect of how families respond to schooling costs along the amount of schooling they provide to their offspring. Using birth histories from the 2000 Demographic and Health Survey (DHS), I reconstruct detailed family structures at the onset of the 1997 policy reform. I then identify households with more than four biological children younger than 16 as being constrained by the policy. With this setup, I consider whether years of schooling for (older) primary school aged children who are otherwise eligible for free education respond to the presence of (younger) siblings whose birth order places them above the eligibility cutoff. Important for the purposes of identification, the UPE eligibility depends on a particular combination of both number and age of siblings. This allows me to control for an extremely rich set of community and household demographic characteristics, and to tightly identify the effect of education costs on schooling choices separately from other implementation aspects of UPE.

The analysis shows that households respond to the presence of young, ineligible children by significantly reducing schooling of older children in the household. Specifically, having more than four siblings in the household reduces the gains in schooling by 0.17 years for the eligible (older) sibling. This is quite substantial in terms of magnitude size, as it is almost half of the estimated increase in years of schooling due to the UPE reform among households that are not constrained by UPE eligibility limits. This negative response to the presence of schooling costs exists for both boys and girls, although the effect is somewhat larger for girls. I also find that the results are larger for households at lower wealth quintiles, possibly suggesting a role for liquidity or credit constraints among poorer households. To explain the results, I use a simple model of intra-household allocation and show that, provided that younger children have more years of primary school ahead of them (due to their age), waivers are more valuable when given to them. Parents may therefore reassign the waiver from older to younger children. This can be achieved by withdrawing the older child from school, or by agreeing with school officials to switch UPE status from enrolled older children to their younger siblings. With the switch, older children "lose out" as parents concentrate investment resources on the schooling of their younger sibling.

A concern with the analysis is that the UPE regulation existed *de jure*, but was not applied *de facto*. If that critique were correct, the estimated effects would be driven by unobserved differences in preferences over schooling that are somewhat correlated with family structure. While hard evidence on policy enforcement is not available, I bring a number of pieces of evidence that cast doubt on the view that the rule was completely ignored. First, I run placebo regressions on DHS data collected before UPE was announced. I find a null effect of placebo UPE eligibility on schooling. I then analyze the period that followed the 2003 expansion of UPE to all children; I find that negative coefficient estimates fade out fairly quickly after repeal. These findings are inconsistent with the view that unobserved demographic factors are driving the main results. Another piece of evidence comes from a separate large-scale household survey carried out in 2001, which indicate that a not insignificant proportion of households paid tuition fees for children enrolled in public primary school. The payment of tuition was particularly common in urban areas and among the wealthiest households; consistent with that, the effects of the policy are concentrated in urban and wealthier areas. Overall, then, the evidence is consistent with the policy influencing household schooling choices while it was in place.

The paper contributes to our understanding of the impact of policies that impose explicit limits on the amount of subsidies received by a household. Such rules are found in a number of relevant situations. For example, in the context of conditional cash transfers programs, limits on the amount of cash provided to a household are found in Mexico (De Janvry and Sadoulet, 2006) and Indonesia, while limits on the number of eligible children are found elsewhere (three children per household in the Philippines, Colombia and Brazil, and five in Argentina, as reported by Raitzer et al., 2022). More generally, many other human capital interventions in poor countries discriminate among siblings: vouchers supporting girls education (Baird et al., 2011), provision of school uniforms to children currently enrolled in secondary school (Evans et al., 2008), or child sponsorship programs (i.e., Wydick et al., 2013 and Wydick et al., 2017). When analyzing how schooling outcomes respond to these policies, the empirical literature typically finds a pattern consistent with "reinforcement"; that is, parents concentrate resources to children who are recipients of the transfer, thereby hurting the educational attainment of the other children (Barrera-Osorio et al., 2011 and Raitzer et al., 2022). This type of response in which parents "pick winners" at the expense of others is found in other settings: for example, Akresh et al., 2012 finds that a child's school enrollment is negatively related to the sibling's measured ability in Mali, and others find that schooling outcomes improve if a sibling suffers some type of negative shock (Parman, 2013, Yi et al., 2015, Alsan, 2017)[1]. My paper extends the analysis to subsidies and its findings are consistent with this prior literature.

The paper also contributes to a large and growing literature focused on the implementation of universal primary education programs and free primary education programs. The Uganda program has been analyzed repeatedly by the education, fertility, and gender literature (e.g., Deininger, 2003, Grogan, 2009, Behrman, 2015, Keats, 2018, Masuda and Yamauchi, 2020, Kan and Klasen, 2021). Most of these studies did not analyze the effect of the eligibility cutoff; an exception is Lincove (2012), whose analysis is focused on attendance but incorporates features of the eligibility limit, and Burlando and Bbaale (2022), who focused on the effects of the eligibility limit on fertility employing a similar strategy as this paper. The focus on eligibility limit allows me to credibly isolate the effects of school fees on student outcomes, something that is otherwise impossible to do given that UPE programs included large scale supply-side interventions (like school construction and teacher hiring) in addition to demand-side interventions (elimination of school fees).

The rest of the paper is organized as follows. Section 2 provides background information

[1] An exception is Lincove and Parker, 2016, which finds positive effects on the schooling of siblings eligible for a CCT program in Nicaragua.

on the Uganda UPE reform and the eligibility cutoff. Regression strategy, data, and results are reported in sections 3, 4, and 5 respectively. Section A lays out a simple model of human capital allocation within the household which can be used to interpret the patterns observed in the data. Section 7 concludes.

2 UPE Policy in Uganda

2.1 Background information

The education system in Uganda is based on seven years of primary education and six years of secondary education. The typical age of entry is seven, and under normal progression the primary cycle is completed by age 14 or 15. According to the World Bank Databank, Gross enrollment rates (GER) through the mid-1990s were around quite low, hovering in the high-sixties. Consequently, the youth literacy rate was a low 70% in 1991. Schooling costs in the pre-reform period were quite high, as public schools relied on parental contributions for almost 60% of their running budget, and 62% of those contributions were assigned to PTA fees (Ablo and Reinikka, 1998).

In this context, the Universal Primary Education policy was announced by President Museveni during the launch of his presidential election manifesto on March 27, 1996, in which he committed to abolish school fees, PTA fees and building fees (Museveni, 1996).[2] Having won the elections in May of that year, president Museveni implemented the campaign promise and abolished fees starting in January 1997. The results were immediate: demand for primary school increased, and hundreds of thousands of new pupils joined the school ranks. Deininger (2003) estimates that primary school attendance increased by 60% in 1999 relative to the pre-reform period, while overall school fees declined 60%. Keats (2018) estimates that completed schooling attainment among girls increased by 0.71 years.

Despite the policy objective of "[providing] Basic Education (Primary Education) to *all* Ugandan children of school-going age" (MoE&S, 1998), the UPE policy restricted fee waiver eligibility to four children in an household, with the limit applied per family "once in a lifetime or until change of policy" (MoE&S, 1998).[3] It appears that the motivation for such

[2]While the eligibility limits discussed in this paper clearly indicate that the policy is not universal, I will use the official title of the reform given by the government, which refers to the policy as such.

[3]The policy was also strongly in favor of the education of girls: "girls have equal opportunities as boys for selection among the four children, and where there are both boys and girls, at least two of the four children shall be girls" (MoE&S, 1998).

a limit was to reduce the costs of the policy.[4] The limit operated through the school budget: the government transferred an annual "capitation grant" to each school equivalent to 5,000 Ugandan Shillings (UGX) per eligible child in grades one to three, and 8,100 shillings per eligible child in grades four through seven (the amounts adjusted over time). Schools did not receive a capitation grant for ineligible children, whose school fees remained a responsibility of the household. To maintain policy compliance, schools categorized children as being "under UPE" or not, and parents transferring pupils from one school to another had to produce letters from previous schools specifying the status of the child (MoE&S, 1998). The policy was controversial. Civil society called the government to eliminate the eligibility requirement during the National Conference on UPE in 1998 (MoE&S (1998)). The requirement was instead reaffirmed and codified. However, in 2003 the government of Uganda expanded UPE to all children, effectively repealing the eligibility cap.

There are indications that the policy was not consistently implemented, with several reports indicating difficulties in registering household members due to the difficulty in defining exactly which children belonged to a family (Bategeka and Okurut, 2005) and schools not charging tuition. Despite this, survey evidence collected in 2001 from a nationally representative sample of households found that 13% of children enrolled in public primary schools were paying tuition; this was particularly common in urban areas and among those with higher wealth levels (UBS, 2001). Among those who paid, average tuition costs were UGX 9,710 per child enrolled in public primary school (USD 5.90), which represents 36% of total schooling expenditures per pupil enrolled in public primary school. For comparison, average tuition costs per child enrolled in private primary school was UGX 26,500 (USD 16.10), or 20% of education expenditures among children enrolled in private school. Taken together, the available evidence supports the idea that the UPE limit policy was widely (if imperfectly) adopted, and that the resulting tuition costs represented a significant household outlay.

2.2 UPE eligibility and household composition

The objective of this paper is to understand the effects of schooling costs on the education of children. To do this, I use the fact that the UPE eligibility rule was binding only within some households. Figure 1 illustrates the eligibility policy for three hypothetical families. The figure represents sibling composition at the onset of the policy in 1997. For simplicity,

[4]In his 1996 speech, Museveni highlighted these costs: "Now, we think we can tackle some of the costs of education with our improved performance in tax collection... If you have more than four children, you will pay only their school fees but not PTA or building fees. In this way we shall be able to send as many children as possible to school."

assume that the observable characteristics of the three households are otherwise the same—they reside in the same community and have similar socioeconomic characteristics—with the only observable variation being the structure of the family (sibling composition). The top row represents a household with four children aged 7 through 14. All are eligible to attend school under UPE tuition waivers, and this is represented in the figure by the dotted box. The middle row represents the same four children, this time with one younger brother, aged 2, and an older sister, aged 18, already aged out of primary school. This household is subject to the eligibility cutoff. The four primary school-aged children are UPE eligible and are able to attend for free. If they do, the household used up all four tuition waivers, which makes the youngest child UPE ineligible. The household would have to pay for primary school once this child reaches schooling age. Note that the four older children are, in principle, just as eligible for UPE as those in the first row. However, because they reside in a household with a UPE ineligible sibling, the amount of schooling they receive under UPE may be dependent on household allocations decisions.

Comparing the first and second household is problematic as the latter household has a higher number of siblings.[5] It is thus possible that the two households also differ along a number of other unobserved characteristics. Consider thus the household in the last row. It includes six children, with a similar age structure and composition to the one in the middle row. The key difference between the two is birth spacing: the two older children in the bottom row are 17 and 19 at the onset of the UPE and are both too old to qualify for primary school. For this reason, the school aged children in that household do not face eligibility constraints, whereas those in the middle row live in a household with those constraints. An additional difference between these two households is that they do potentially face different resource allocation problems, because children of different age and gender have different opportunity cost of their time and different returns from schooling. In the next section I will discuss this issue in detail and explain a strategy to address it.

A final issue is that the UPE policy does not dictate how UPE eligibility should be allocated within the household. A reasonable assumption is that, lacking parental or administrative action, children already in school are registered first and registration continues as younger children become school aged.[6] In this way, UPE eligibility is apportioned by birth order, and tuition costs would fall upon the youngest cohort. To avoid this, parents would need to manipulate the situation. They could choose to pull the older children out of school,

[5] A second difference is that, assuming that the age of the mother is the same, the mother was older at the time of the first birth in the former household.

[6] This allocation rule is explicit in other contexts, e.g. the 4P in the Philippines (Raitzer et al., 2022).

either temporary or permanently. Alternatively, they could decline to register their older children under UPE, continue to pay their schooling fees, and retain the waiver for a younger child. Doing so shifts the costs of schooling from the young to the old. Alternatively, they could avoid facing a cutoff by sending children to foster care in a separate community.[7]

3 Empirical approach

The objective of the empirical strategy is to measure the response of school attainment to the variation in tuition cost induced by UPE. The strategy relies on identifying households that, due to their composition and age structure, include young children who were not UPE eligible at the onset of the policy in 1997. Based on the policy, these households face higher tuition rates because they receive a lower tuition subsidy. We study schooling attainment among their UPE eligible older siblings three years later, using the DHS which was collected between 2000 and 2001. For each child i in household h residing in community c, I define the variable $IneligibleSibling_h$ to indicate whether the child has a younger sibling who would be considered UPE ineligible if eligibility is apportioned by birth order, and use it in the following regression:

$$S_{ihc} = \alpha_0 + \alpha_1 IneligibleSibling_h + X_i\beta^i + X_h\beta^h + D_h + \delta_c + \epsilon_{ihc} \tag{1}$$

where S_{ihc} is years of completed schooling; X_i includes a number of child-level controls (age fixed effects, gender, and birth order); X_h includes household controls (household size, mother's schooling and age cohort fixed effects, mother's age at first birth, age and sex of household head and wealth quintile); and D_h includes a number of additional demographic fixed effects that capture differences in the family structure. The inclusion of community fixed effects δ_c captures any community-level effect of UPE including differences in school access or quality.

The coefficient α_1 measures the change in years of schooling due to the presence of an ineligible sibling. In order for the coefficient α_1 to capture this response, two identification concerns must be addressed. The first concern is that the variable $IneligibleSibling_h$ is correlated with unobserved factors that shift the demand for schooling. Since the treatment is constructed from demographic characteristics, these omitted factors are likely associated with demographics. Fortunately, the construction of $IneligibleSibling_h$ allows for the inclusion of

[7] Of course, a final possibility is that parents either collude with school administrators or with other families that are not constrained in order to avoid the eligibility cutoff entirely. If this were the case, there should be no effect of eligibility cutoffs on schooling.

a demanding set of demographic controls. Two separate sets of controls D_h are sequentially included: indicators for number of boys and girls in the household; and fixed effects that control for each possible boy-girl combination. Note that the variation in the treatment is not completely absorbed by these fixed effects, because UPE eligibility depends on a very particular timing and birth spacing of recent births. Even so, we must assume that, controlling for these demographic and other observable characteristics, UPE eligibility is uncorrelated with other determinants of schooling. I will show some evidence in support of this, including by demonstrating that the cutoff does not predict schooling outcomes in the years prior to the implementation of the policy.

The second concern is measurement error in the treatment variable arising from errors in the measurement of family structure at the onset of the policy. Measurement error comes from the fact that schooling outcomes are available for *current* household members, but household structures could have changed between the onset of the policy and the survey. Family structure changes because of new births, children leaving or joining the household, or death; some of these changes could be driven by the schooling policy. To recover the original household structure in 1997, I thus rely on birth records of the mother, as detailed in the next subsection. This method is not without problems: it excludes foster children or children of mothers older than 45 in the eligibility calculations. Since fostering might be a direct response to the policy, the estimates are best interpreted as "intent to treat".

4 Data and Sample construction

The sample utilized in this study is the 2000/2001 rounds of the DHS, which were collected three years after the implementation of the UPE and before the UPE eligibility limit was lifted in 2003.

To construct an indicator for households with ineligible children, I use detailed birth records of mothers. From the records, I compute the number of children that were (1) already born in 1997, (2) still alive in 2000, and (3) aged 16 or younger in 1997 (19 or younger at the time of the survey).[8] Households with mothers with more than four school-eligible children are then identified as treated. The same child records are used to construct household-level demographic controls. Household structure variables are then merged into the household roster, which contains schooling outcomes for children and adults residing

[8]To avoid endogenous household structure responses to the policy, the strategy relies on household composition in 1996, prior to the implementation of the policy, and restricts the analysis to households with only one mother.

in the household at the time of the survey. Birth records are then used to construct age orderings of children in the household. For example, a child who is aged five in the household roster will be counted as third child if the birth records indicate the presence of a live 14 and 9 year old. A child who has four or more older siblings is labelled as ineligible.

To arrive at the final sample used in this analysis, I impose several sample restrictions. In the schooling regressions, I limit the sample to children aged 7 (when they can enter primary school) to 16, when many children start exiting the household and transition out of primary school. Because households tend to be large and complex, I also restrict the analysis to those households having only one woman aged 15 to 45 with children. This excludes observations from multigenerational households (both mothers and grandmothers), households with mothers older than 45, and households with multiple families living under one roof for which it is unclear how the cap would apply. I drop children whose mothers do not belong to the household. This excludes some categories of children who are always UPE eligible (orphaned children and employees) as well as foster children, who (arguably) need to meet eligibility criteria.[9] What's left is a dataset with 5,731 children. Of these, 411 are UPE ineligible (they have four or more older siblings), and are also dropped, leaving a sample of 5,320.

Construction of $IneligibileSibling_h$ relies on the assumption that children up to the age of 16 in 1997 are eligible for free primary school tuition. Note that the standard age for primary school attendance is 6-13; however, late entry is common.[10] The cutoff adopted in this paper is empirically justified. Appendix table A1 tabulates the proportion of children enrolled in primary school by age in 1995, before the policy was implemented. The likelihood of being enrolled in primary school falls steeply around age 14 and 15, justifying the age cutoff used in the paper.

Table 1 reports the summary statistics of the constructed dataset. 34% of children in the sample live in a household with ineligible siblings (treated). Children in the sample (aged 7-16) have on average 3 years of schooling (88% of them are still in school).

[9]A possible response to the eligibility cap is, indeed, the fostering of children from households facing a cap to those who are not; robustness check which include these children are included in the appendix.

[10]The DHS 2000/2001 report states that UPE eligibility was limited to children up to age 15; while regulations from the Ministry of Education (MoE&S, 1998) do not include this age limit, it is quite possible that a formal age eligibility was specified in other regulations.

5 Results

Estimates of α_1 from regression (1) are provided in Table 2. All regressions focus on the sample of children who are eligible to receive the tuition waiver. The first regression in column 1 includes only child, mother and household controls; column 2 adds village fixed effects; column 3 controls for the number of children and fixed effects for the number of boys and girls. Column 4 includes a more demanding set of 65 family structure fixed effects, each corresponding to a particular combination of boys and girls. In this specification, identification comes from the fact that, within households with a certain sibling combination (for example, four boys and two girls, for a total of six children), some children will have aged out of primary school in some households but not in others. Finally, given that a priori it is not clear which demographic controls should be included in the regression, in column 5 I perform a double-selection Lasso procedure that includes every control in the previous four columns and their interaction with household head age.[11] The procedure allows for a parsimonious selection of covariates, and corrects standard errors to avoid model selection bias. In this procedure, 51 covariates out of the set of 355 are used.

In all regressions, the coefficient estimate α_1 is negative; as controls are included, they become more negative. Coefficient estimates in columns 3, 4 and 5 are very close, varying between -0.156 in column 3 (p-value: 0.044) and -0.18 in the LASSO estimation of column 5 (p-value: 0.053). Moving forward, the specification from column 4 (-0.177) will be used in all subsequent analysis. The interpretation of the point estimate is that schooling for eligible children is lower by 0.177 years if they have any UPE ineligible sibling, relative to the schooling of eligible children in unconstrained households. Importantly, this relative reduction should be understood as "lower gain" in schooling, in the sense that children are increasing their years of schooling on average in this time period due to the overall impact of UPE. To gain a better appreciation of the magnitude of this effect, it is thus helpful to compare it against the overall schooling response to UPE among UPE eligible households. Controlling for child and household characteristics, children who benefited from UPE increased schooling by 0.4 years after three years (see Appendix Table A2 for estimates). Thus, UPE eligible children in households facing an eligibility cutoff experience $1 - 0.177/0.4 = 0.56$ percent of the expected gains from UPE.

One possible explanation for the lower schooling gains the years of schooling is that parents "pick winners" and choose to keep some children from ever enrolling in school. Table

[11] The procedure utilizes the Stata command dsregress and allows for the optimal selection of the penalizing factor λ.

3, column 1, reports the result of a regression where the main outcome variable is whether the child ever attended school. The point estimate is very small and statistically insignificant. Thus, the reduction in years of school are not due to the extensive margin. At the intensive margin, parents may be pulling children out of school; one should expect that variables associated with school dropout are worse for exposed children.[12] In column 2, I consider whether the child is currently in school (column 2). Interestingly, there is a marginally significant 3.4 p.p. increase in the likelihood of currently attending school (p-value: 0.08, column 2). One explanation is that, in response to the policy, some households withdrew their children temporarily, or delayed their school entrance. It is, however, important to keep in mind that this effect is small given that 88% of children in the sample are in school. I do not observe that children are less likely to complete primary school (column 3) or start secondary school (column 4).

Treatment effects over time It is possible that households with UPE ineligible children are associated with low schooling for reasons unrelated to the policy. One way to establish whether this is the case is to run regressions (1) before, during and after the eligibility limits were in place, and verify that the estimate is present only in the policy period. Figure 2 plots the point estimates of the regression estimated with data from other DHS rounds, including 1995, 2000, 2006 and 2011. In each estimation, household structure from birth records three years prior were used to determine household eligibility. The estimate from two years prior to UPE (1995) is positive and close to zero: UPE eligibility does not predict schooling outcomes. The coefficient estimate three years after the eligibility rule was lifted (2006) is similar to the coefficient in 2000/2001, but standard errors are noticeably larger and include the null hypothesis estimate. This is not surprising, given that many children 2006 would have been affected by the cutoff when that was still in place. By 2011, however, the estimate has returned to the 1995 level.[13] It is also possible to combine the rounds together and estimate the main effects using a difference in difference strategy. Point estimates are very similar to the results presented so far (see Appendix Table A4).

An alternative strategy to verify the validity of the identification strategy is to regress household characteristics on the ineligible indicator, controlling for other factors (see Table

[12]Pulling children from school may be the consequence of the substitution effect caused by the change in *relative* prices of schooling.

[13]It is important to recognize that, since family structures responded to the eligibility rule (**?**), the treatment effects in 2006 and 2011 are likely influenced by it. If, moreover, eligibility rules caused a quantity-quality response among households that value education more heavily, those with UPE ineligible children would have lower schooling levels. This would introduce a negative bias of the estimated effect in 2006 and 2011.

4). None of the coefficient estimates are statistically significant, validating the approach taken in the paper.

Heterogeneity by characteristics of the child To better understand the mechanism through which the UPE eligibility policy influences school attainment, I next show how treatment effects vary by characteristics of children in the household.

To begin, I show whether the impact of eligibility depends on the number of ineligible siblings. Appendix Figure A1 shows the distribution of the number of UPE ineligible siblings in the data. Children have between zero and four ineligible siblings, and a non-trivial fraction of the data (approximately 15%) includes children with more than one ineligible sibling. I estimate a model where the treatment variable is replaced with four indicators associated with the number of ineligible siblings and plot the coefficients in Figure 3, panel A. All estimates are negative and (with the exclusion of the last one) are statistically significant at the five or ten percent levels. Moreover, all of them fall within the confidence interval of the other ones, indicating an absence of a dosage effect.

Next, I show that treatment effects depend on the age of the (UPE eligible) child. Figure 3, panel B, plots the coefficients of the interaction between the treatment and the age of the child in 2000. While the coefficient estimates are not always individually significant, the pattern they provide is clear: estimates are close to zero for young children below the age of 10, and become negative at higher ages. Point estimates are especially negative for 14 and 16 year olds. I test the negative relationship between age interactions and schooling in Table 5, column 1: the interaction term between age of the child and the eligibility indicator is negative and statistically significant, with each additional year of the child reducing schooling attainment by 0.05 years.

The test carried out above would suggest that household responses are concentrated among the oldest set of school-aged children. To formally shed light on this, in Table 5, column 2, I interact UPE eligibility with an indicator that identifies children with older (eligible) siblings. If the penalty from the eligibility cap is borne entirely by older children, we should expect to see a negative coefficient on "UPE Ineligible sibling", and a positive coefficient of a similar magnitude for the interaction. What we see instead is a negative coefficient on the treatment indicator, and a close-to-zero coefficient on the interaction. In Panel C of Figure 3, I interact the treatment with dummies for the number of ineligible younger children to confirm this finding. While all point estimates are negative and of similar magnitude, an intriguing finding from the figure is that the estimate on the second oldest child is the most negative and the only one that is statistically different from zero.

This pattern is not completely unexpected, since the oldest eligible sibling is very often the first born, whose schooling is somewhat protected by birth order preferences. The burden thus seems to fall more strongly onto the "next in line" child.

In column 3 of Table 5, I consider whether it matters whether children have siblings that are UPE ineligible but not old enough to attend school. The reason why it might make a difference is that parents could easily decide the allocation of the UPE eligibility if both types of children are attending school, while this is practically harder if the ineligible child has yet to enter primary school. We would thus expect that the effect of having UPE ineligible siblings should be less negative for those with siblings not yet in school. The regression estimate on the interaction term is positive (coeff. 0.083) and consistent with this story. However, this interaction term is measured very noisily, possibly due to the fact that almost 90% of children have young siblings not yet in school and thus the estimation is underpowered.

I next study whether the effects are different between boys and girls by interacting the ineligible status with an indicator for girls. As mentioned before, the policy required at least two of the girls in the household to be UPE eligible. We would thus expect that girls' education is less responsive than boys to the presence of an ineligible sibling, and the interaction term would thus be positive. On the other hand, any preference for boys' education over girls would counteract this effect. In column 4, we see that the estimated impacts are negative for both boys and girls, and more strongly so for girls (coeff. on interaction is 0.179, p-value: 0.07). However, the difference is only weakly significant; moreover, when running the regression with household fixed effects, the estimate on the interaction is reduced to -0.083 (p-value: 0.54). Finally, I study the gender composition of siblings. In the final column of the table I regress school attainment on the interaction between the ineligible indicator and the fraction of siblings that are girls. One would expect that schooling should decrease with the number of girls in the household. This does not appear to be the case.[14] Overall, the evidence that gender matters is weak.

Finally, I re-run the analysis on a larger sample that include foster children. As shown in Appendix Table A3, point estimates are somewhat noisier and the preferred estimate falls from -0.177 to -0.159 (p-value: 0.087).

Heterogeneity along household and community characteristics As a final exercise I break down the data in various subsamples of households to understand whether the effects

[14] The lack of significance of the interaction term remains the same even when considering boys and girls in separate regressions.

differ along observable dimensions of household and community heterogeneity (Table 6). The first two columns report results for the urban and rural subsample respectively. The response to the policy is coming entirely from the urban sample, where the coefficient estimate is large (-0.48) and highly significant. While the coefficient estimate for the rural sample remains negative, it is much smaller in size (-0.07) and insignificant. A related result comes from breaking down the results by the wealth level of the community (columns 3, 4 and 5). Estimated coefficients become more and more negative at higher wealth quantiles. These differences in responses by urban and community wealth status correlate with education spending data indicating that tuition fees are more likely to be spent in urban areas rather than in rural areas, which is what one would expect if the policy was enforced more strongly in cities and in wealthier communities.

Interestingly, the fact that policy responses are stronger in better-off communities does not imply that the effects are driven by better-off families. Figure 3 Panel D indicates quite the opposite: estimated effects are largest in households at the lowest wealth quintile, and are indistinguishable from zero at the top two quintiles of the wealth distribution. Similarly, coefficient estimates are more statistically significant for less educated mothers (columns 6 and 7 of Table 6), although the hypothesis that the response is the same as educated mothers cannot be rejected.

One possible explanation for this lack of response by high SES households is due to their better ability to avoid the policy through a variety of mechanisms (i.e., collusion, fostering, higher reliance on private education). External survey evidence is inconsistent with this view: the likelihood of paying tuition is correlated with the wealth level (UBS, 2001). The alternative explanation is that the demand for schooling decreases with wealth, either due to differences in preferences or due to binding credit or liquidity constraints among the poor. While pinning down the exact mechanism at work is not possible, these results are consistent with this schooling policy being unevenly enforced and differentially impacting the poor.

6 Concentrate or redistribute resources?

To understand the implications of eligibility limits, it is necessary to deduce whether the effect of these limits found in the preceding section were due to reinforcing behaviors (parents concentrate resources to targeted children) or compensating behaviors (meaning that parents redistribute resources to all children). As mentioned in the introduction, most of the evidence from the transfers literature and other types of analyses indicate that parents treat children's schooling as substitutable, which means that their response to a transfer targeted towards one child is to move resources away from other siblings towards that child. Does the same hold true for school subsidies?

In our context, the results presented so far provide only a partial answer to the question; a definitive answer would require looking at the completed schooling outcomes of the younger siblings. Unfortunately, doing this is not possible: with the 2003 expansion of UPE to all children, every child in the sample is (eventually) treated by FPE. Since this strategy is not available, I adopt an alternative method, which is to create a simple theoretical model of the household decision-making process under the eligibility limits of UPE in which parental preferences are either compensatory or reinforcing.[15] The model, described in detail in the appendix, generates a number of qualitative predictions under both scenarios, and this can be compared to my empirical findings then compare the qualitative predictions of the model with the empirical results of the study. The model delivers reductions in schooling of older (UPE eligible) children under two scenarios. The first scenario is that parents act in a compensatory way: facing with tuition payments for younger children, they reduce everyone's schooling due to the income effect from higher costs. While possible, this scenario is somewhat unlikely: it requires us to believe against most existing evidence that households have strong equitable motives, *and* that wealth effects from tuition cost are very large. The second scenario is that parents prefer to concentrate resources on fewer children, and have the ability to transfer the fee subsidy from older children to their younger siblings. Intuitively, parents will choose to transfer because the subsidy is more valuable when given to them (they have fewer years of completed schooling). As parents shift resources to the young child, older children end up with lower levels of schooling. In this scenario, substitution and income effects operate in the same direction, explaining the relatively large drop in schooling.

The take-aways from the exercise are two. First, the results in Uganda are entirely consistent with the empirical literature which finds that parents concentrate resources (Barrera-Osorio et al., 2011, Yi et al., 2015, Raitzer et al., 2022). It extends those findings to the case of tuition subsidies, which remain an important policy tool. Second, it highlights the possibility that parents may manipulate eligibility, and that this leads to more unequal outcomes when parents' preferences are reinforcing. This is relevant in other contexts as well. For example, the CCT program in the Philippines had clear guidelines for the assignment of the CCT: eligibility was given in ascending order of child age above age six. This policy was eventually replaced by one where parents could choose the identity of the recipient (Raitzer

[15] In other words, when preferences are compensatory parents treat the educational attainment of their children as complements; substitution effects are small. When preferences are reinforcing, they treat educational attainment as substitutes.

et al., 2022).

7 Conclusion

Many education programs impose eligibility criteria that can be binding within a household, thereby creating potential inequities between siblings. In this paper, I study the effect of a limit on the number of siblings that can receive free schooling in Uganda. The policy created different prices of schooling for different children. I show that households respond to relative price changes by reducing schooling investments on older children. Three years after the reform, UPE eligible children had 0.17 fewer years of schooling for each ineligible sibling in the household, cutting the expected gains from the UPE reform by 44%. The results confirm that eligibility limits can generate powerful responses within families and introduce significant inequalities as parents focus resources to the eligible.

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A A simple household allocation model

The model is derived from Ejrnæs and Pörtner (2004), modified to account for the presence of child-specific prices.[16] A unitary household derives utility from consumption c and from providing their n children (indexed by parity $i = 1, 2, ..., n$) with human capital w_i. To keep the model simple, I assume that the number of children is limited to 2, with the youngest child being possibly UPE ineligible. Like Ejrnæs and Pörtner (2004), parental consumption and human capital investments are separable in the utility function and the amount allocated to children is a constant fraction of income, and schooling choices is modeled with a CES sub-utility function:

$$U(w_1, w_2) = (w_1^\rho + w_2^\rho)^{\frac{1}{\rho}}$$

Let $\sigma = \frac{1}{1-\rho}$ be the elasticity of substitution. This is a measure of tolerance for inequality; parents reinforce differences when $\sigma > 1$ and compensate when $\sigma < 1$.

A child's human capital is a simple function of schooling as in Becker and Tomes (1976). For simplicity, I assume that the function is given by

$$w_i = a_i s_i^\alpha, \tag{2}$$

where a_i can be interpreted as a measure of the weight parents give to the schooling of child i. The choice of schooling (and therefore human capital) is decided once, and there is no human capital accumulation. However, I do assume that, at the time of the choice, children have already accumulated some schooling so that the choice of s_i is bound below by the following constraint:

$$s_i \geq \underline{s_i}, \tag{3}$$

with $\underline{s_1} > \underline{s_2}$ due to the different ages of child 1 and 2. The motivation for this deviation of the Ejrnæs and Pörtner (2004) model is directly tied to the UPE setting. At the time of the reform, all households had the opportunity to reoptimize the allocation of schooling across existing children. The assumption makes explicit the fact that these reallocations had to account for investments that had already taken place, and that those investments are higher for older children.

Finally, following Ejrnæs and Pörtner (2004) the budget set available to the household

[16] The model is designed to fit the features of the UPE policy in Uganda, and it therefore strips many of the features of a more general model such as Glick (2008).

for schooling expenses is given by:

$$p_1(s_1 - \underline{s_1}) + p_2(s_2 - \underline{s_2}) = I, \tag{4}$$

where I is the fixed income devoted to schooling. To account for the fact that UPE eligibility varies within the household, the price of schooling p_i is indexed by i. If there are no differences in tuition costs, then $p_i = p_j$, and the budget set simplifies to the one presented by Becker and Tomes.

The household maximizes the sub-utility function with respect to s_1, s_2, subject to (3) and (4). The demand functions for schooling for child 1 and child 2 respectively are:

$$s_1 = \frac{I}{P}(p_1^{\sigma} a_2^{\sigma-1})^{\omega}, \qquad s_2 = \frac{I}{P}(p_2^{\sigma} a_1^{\sigma-1})^{\omega}.$$

Where the price index $P = (p_1^{\alpha} a_1)^{\omega} + (p_2^{\alpha} a_2)^{\omega}$ with $\omega = \frac{\sigma-1}{\alpha(\sigma-1)-\sigma}$. Here, ω captures the degree of inequality aversion and it is directly linked to the elasticity of substitution: $\omega > 0$ if $\frac{\alpha}{\alpha-1} < \sigma < 1$, and $\omega < 0$ if $\sigma > 1$. For simplicity, assume that $\frac{\alpha}{\alpha-1} < \sigma$. This assumption is not restrictive as the production function for human capital approaches linearity. The associated value function V is

$$V = a_1 a_2 I^{\alpha}(P)^{-\frac{1}{\omega}}. \tag{5}$$

Allocation under unconstrained UPE When there are no eligibility limits to UPE, all children pay the same schooling cost: $p_1 = p_2 \equiv \tilde{p}$. Child 1 (oldest child) schooling is defined by

$$s_1^u = \frac{I^u}{\tilde{p}} \frac{a_2^{\omega}}{a_1^{\omega} + a_2^{\omega}},$$

With associated value function $V^u = a_1 a_2 \frac{I^u}{\tilde{p}}(a_1^{\omega} + a_2^{\omega})^{\frac{-1}{\omega}}$, and $I^u = I + \tilde{p}(\underline{s}_1 + \underline{s}_2)$.[17]

Allocation under UPE eligibility rules Under UPE eligibility rules, child 1 is registered for the low tuition cost $\tilde{p}$ which makes child 2 ineligible. She must therefore pay the full tuition $p > \tilde{p}$. If parents are compliers, they do not attempt to shift UPE eligibility from the oldest to the youngest child. Whether schooling for the older child increases relative to full UPE depends on parental preferences (i.e., taste for equality). This is captured by the elasticity of substitution in the sub-utility function, according to the following proposition:

[17] I will not keep track of s_2, due to the fact that the empirical analysis focuses on older children.

Proposition 1. *If parents do not shift eligibility to a younger ineligible sibling, schooling for the older (eligible) child increases if parents reinforce differences ($\sigma > 1$). Schooling may decrease if parents dislike inequality ($\sigma < 1$) and the negative income effect from the higher schooling costs is sufficiently large.*

Proposition 1 provides a potential explanation for the empirical results: faced with tuition costs and an inability to apply the UPE eligibility to the younger children, parents reduce schooling to all siblings as a result of the income effect from the price change and preferences that are reinforcing.

Allocation with UPE eligibility choice Next, consider the case where parents are able to choose who is UPE eligible, and may choose to transfer eligibility from child 1 to child 2. Per unit costs for schooling s_1 and s_2 are thus p and $\tilde{p}$ respectively. Assuming that the switch does not incur any fixed costs, parents will choose to switch if they gain a higher utility. The following proposition provides conditions under which they will do so:

Proposition 2. *Households transfer eligibility from the older to the younger sibling if:*

- $a_1 = a_2$ *(parents value all children equally);*
- $a_1 < a_2$ *and* $\sigma \geq 1$ *(parents favor the young child and tolerate inequality);*
- $a_1 > a_2$ *and* $\sigma \leq 1$ *(parents favor the older child and are inequality averse).*

In all other cases households will transfer eligibility if the cost reduction from the switch is sufficiently large.

When parents value all children equally, schooling investments are more productive when invested on child 2, because she has fewer years of completed schooling. At a given price, then, relatively more schooling goes towards child 2, and therefore total costs are minimized by shifting the UPE price to that child. This cost reduction effect from shifting UPE to the least-educated child may induce a transfer of eligibility even if the older child is favored.

The final relevant result relates to the optimal choice of schooling for child 1 if the parents choose to manipulate eligibility.

Proposition 3. *When households transfer UPE eligibility, the older child accumulates fewer years of schooling relative to the full UPE case if parents tolerate inequality ($\sigma > 1$). The effect on schooling for the older child is ambiguous if parents are inequality averse.*

The above proposition demonstrates that the negative effects of eligibility on the education of older children could be the result of reinforcement behavior, as parents switch schooling investments to younger children. In summary, the empirical evidence could be explained by compensation if parents are unable to switch eligibility (by proposition 1); in the more realistic case where parents have a say on who receives free schooling, the better explanation is that parents assign UPE to younger children (proposition 2) and then switch resources away from older children (proposition 3).

B Proofs

B.1 Proof of proposition 1

When child 2 is UPE ineligible, schooling for child 1 is defined by

$$s_1^n = I^n(\frac{(\tilde{p}^\sigma a_2^{\sigma-1})^\omega}{(p^\alpha a_1)^\omega + (\tilde{p}^\alpha a_2)^\omega}) = \frac{I^n}{P^n}(\tilde{p}^\sigma a_2^{\sigma-1})^\omega,$$

where $I^n = I + p\underline{s}_2 + \tilde{p}\underline{s}_1$. Consider now the ratio $\frac{s_1^n}{s_1^u}$:

$$\frac{s_1^n}{s_1^u} = \frac{I^n}{I^u}\frac{P^u}{P^n} = \frac{I^n}{I^u}\frac{(\tilde{p}^\alpha a_1)^\omega + (\tilde{p}^\alpha a_2)^\omega}{(p^\alpha a_1)^\omega + (\tilde{p}^\alpha a_2)^\omega}$$

Whether the ratio is greater or smaller than 1 depends on the sizes of $\frac{I^n}{I^u}$ and $\frac{P^u}{P^n}$. The first term is always greater than 1: $I^u = I + \tilde{p}(\underline{s}_1 + \underline{s}_2) < I + p\underline{s}_2 + \tilde{p}\underline{s}_1 = I^n$ since $\tilde{p} < p$. Whether the second term is greater or smaller than 1 depends on σ. With $\sigma > 1$ (i.e., parents reinforce inequality), $\omega < 0$, $P^u > P^n$, and the ratio is greater than 1. This implies that child 1 increases schooling (i.e., inequalities are reinforced). With $\sigma < 1$, the ratio is less than 1 and $\frac{s_1^n}{s_1^u} < 1$ if $\frac{I^n}{I^u} < \frac{P^n}{P^u}$, i.e., provided that the income effect from the higher price of schooling is sufficiently small.

B.2 Proof of proposition 2

Households will manipulate elgibility of UPE if $V^n < V^m$, or if $\frac{V^n}{V^m} < 1$. This ratio is

$$\frac{V^n}{V^m} = \left(\frac{I^n}{I^m}\right)^\alpha \left(\frac{P^n}{P^m}\right)^{-\frac{1}{\omega}}. \tag{6}$$

The first term is $\left(\frac{I^n}{I^m}\right)^\alpha < 1$, owing to the fact that $I^n = I + \tilde{p}\underline{s}_2 + p\underline{s}_1 < I + p\underline{s}_2 + \tilde{p}\underline{s}_1 = I^m$ since $\underline{s}_1 > \underline{s}_2$. The size of the second term depends on σ and preference parameters a_1, a_2.

Consider first the case where $a_1 = a_2 = a$. Then,

$$\frac{P^n}{P^m} = \frac{(\tilde{p}^\alpha a)^\omega + (p^\alpha a)^\omega}{(p^\alpha a)^\omega + (\tilde{p}^\alpha a)^\omega} = 1,$$

and $V^n < V^m$ for any value of ω.

Next, consider the case of $a_2 \neq a_1$, i.e., parents value the human capital of their children differently. Then,

$$\frac{P^n}{P^m} = \frac{(\tilde{p}^\alpha a_2)^\omega + (p^\alpha a_1)^\omega}{(p^\alpha a_2)^\omega + (\tilde{p}^\alpha a_1)^\omega}.$$

This term is ≤ 1 if $(\tilde{p}^\alpha a_2)^\omega + (p^\alpha a_1)^\omega \leq (p^\alpha a_2)^\omega + (\tilde{p}^\alpha a_1)^\omega$; rearranging the terms, the condition is $\tilde{p}^{\alpha\omega}(a_2^\omega - a_1^\omega) \leq p^{\alpha\omega}(a_2^\omega - a_1^\omega)$. Whether this condition is met depends on ω and the relative values of a_1 and a_2.

- $a_2 > a_1$ and $\omega \leq 0$ (young child is preferred to older sibling and parents tolerate inequality). The condition above simplifies to $\tilde{p}^{\alpha\omega} \geq p^{\alpha\omega}$. This is always the case given that $\omega \leq 0$ (the term is equal to 1 if $\omega = 0$). Moreover, since $(P^n/P^m) < 1$, $(P^n/P^m)^{-\frac{1}{\omega}} < 1$. Thus, the household manipulate eligibility.

- $a_2 > a_1$ and $\omega > 0$. In this case, we have that $\tilde{p}^{\alpha\omega} < p^{\alpha\omega}$ and $\tilde{p}^{\alpha\omega}(a_2^\omega - a_1^\omega) < p^{\alpha\omega}(a_2^\omega - a_1^\omega)$. This implies that $(P^n/P^m) < 1$, and $(P^n/P^m)^{-\frac{1}{\omega}} > 1$. Thus, equation (6) is negative and $V^n/V^m < 1$ if $\left(\frac{P^n}{P^m}\right)^{-\frac{1}{\omega}} > \left(\frac{I^m}{I^n}\right)^\alpha$.

- $a_1 > a_2$ and $\omega < 0$. By the same arguments as above, $(P^n/P^m) > 1$, and $(P^n/P^m)^{-\frac{1}{\omega}} > 1$. Thus, $V^n/V^m < 1$ if $\left(\frac{P^n}{P^m}\right)^{-\frac{1}{\omega}} > \left(\frac{I^m}{I^n}\right)^\alpha$.

- $a_1 > a_2$ and $\omega \geq 0$. By the same arguments as above, $(P^n/P^m) > 1$, and $(P^n/P^m)^{-\frac{1}{\omega}} < 1$. Thus, $V^n/V^m < 1$ and households manipulate eligibility.

B.3 Proof of proposition 3

If parents manipulate eligibility, schooling for child 1 is given by s_1^m:

$$s_1^m = I^m \left(\frac{(p^\sigma a_2^{\sigma-1})^\omega}{(\tilde{p}^\alpha a_1)^\omega + (p^\alpha a_2)^\omega} \right)$$

The ratio $\frac{s_1^m}{s_1^u}$ describes the schooling choice relative to the case where UPE limits are not binding:

$$\frac{s_1^m}{s_1^u} = \frac{I^m}{I^u}\frac{P^u}{P^m} = \frac{I^m}{I^u}\frac{\tilde{p}}{p}\left(\frac{a_1^\omega + a_2^\omega}{(\frac{\tilde{p}}{p})^{\alpha\omega}a_1^\omega + a_2^\omega}\right)$$

There are three terms to this function. First, I demonstrate that the product of the first two terms is always less than 1. Note that $\frac{I^m}{I^u}\frac{\tilde{p}}{p} < 1$ if $I^m\tilde{p} < I^u p$. This condition simplifies to $\tilde{p}I + \tilde{p}^2\underline{s}_2 < pI + p\tilde{p}\underline{s}_2$, which is always met.

The last term is ≤ 1 if $a_1^\omega + a_2^\omega \leq (\frac{\tilde{p}}{p})^{\alpha\omega}a_1^\omega + a_2^\omega$. This is the case if $\tilde{p}^{\alpha\omega} < p^{\alpha\omega}$, which occurs whenever $\omega < 0$. Under that condition, $s_1^m < s_1^u$. When $\omega > 0$, the third term is positive. This implies that the ratio s_1^m/s_1^u is ambiguous. It also implies that the difference in schooling between child 1 and child 2, $s_1^m - s_2^m$, is larger when $\omega < 0$.

C Tables and figures

Table 1: Summary statistics

VARIABLES	(1) N	(2) mean	(3) sd
Child characteristics			
born after cutoff	5,731	0.0717	0.258
UPE Ineligible sibling	5,320	0.342	0.474
age	5,320	11.25	2.893
female	5,320	1.504	0.500
child ordering	5,320	2.157	1.043
years of schooling	5,315	2.935	2.314
child still in school	5,312	0.880	0.325
no schooling	5,320	0.0904	0.287
completed primary	5,320	0.0786	0.269
started secondary	5,320	0.0453	0.208
Household characteristics			
number of household members	5,320	7.321	2.527
male head of household	5,320	0.729	0.444
age of head of household	5,320	41.58	10.32
urban	5,320	0.270	0.444
mother: no schooling	5,320	0.292	0.455
mother: primary	5,320	0.545	0.498
mother: secondary	5,320	0.119	0.324
mother: higher ed.	5,320	0.0440	0.205
mother: age at first birth	5,320	18.17	3.183
mother: age	5,320	34.83	6.728

Data from 2000/2001 Uganda DHS. Sample of children aged aged 7-19. Born after cutoff is equal to one if the child has at least four older siblings. Remaining rows are only for children born before cutoff.

Table 2: Benchmark: Effect of sibling UPE ineligibility on number of years of schooling completed

VARIABLES	(1)	(2)	(3)	(4)	(5) LASSO
UPE Ineligible sibling	-0.097	-0.119*	-0.156**	-0.177*	-0.182*
	(0.072)	(0.071)	(0.077)	(0.096)	(0.094)
p-value	0.180	0.097	0.044	0.067	0.054
Observations	5,315	5,315	5,315	5,315	5,315
R-squared	0.632	0.691	0.694	0.698	
Controls	yes	yes	yes	yes	LASSO
Village f.e.	no	yes	yes	yes	yes
Demog. f.e. 1	no	no	yes	no	
Demog. f.e. 2	no	no	no	yes	
Mean years of schooling	2.935	2.935	2.935	2.935	2.935

Estimates of regression 1. Individual child controls include: birth order fixed effect, age fixed effect, sex. Household controls include: age of household head, number of women, household size, sex and age of household head, wealth quantile. Double selection Lasso procedure includes interactions between head of household and all other categorical variables from previous columns (355 controls), with 51 selected controls, and estimated through the STATA command `dsregress`. Errors clustered at the village level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 3: Other schooling outcomes

VARIABLES	(1) never schooled	(2) still in school	(3) completed primary	(4) started secondary
UPE Ineligible sibling	0.003	0.034*	-0.045	0.030
	(0.016)	(0.019)	(0.038)	(0.041)
Observations	5,320	5,312	1,916	1,446
R-squared	0.256	0.234	0.430	0.476
Controls	yes	yes	yes	yes
Village f.e.	yes	yes	yes	yes
Demog. f.e. 2	yes	yes	yes	yes
Sample	full	full	age > 12	age > 13
Mean outcome	0.0904	0.880	0.212	0.158

Regression equation 1 on the dependent variable indicators referred in the column title. See notes under Table 2 for description of variables used as control. Errors clustered at the village level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 4: Household level placebo regressions: household characteristics and UPE ineligibility

Dep Var:	(1) Female hhld head	(2) Bottom wealth quintile	(3) Middle wealth quintiles	(4) Top wealth quintile	(5) Mother prim. educ or less	(6) Mother sec. educ or more
UPE Ineligible	-0.005	-0.021	0.051	-0.030	0.019	-0.019
	(0.036)	(0.021)	(0.035)	(0.021)	(0.029)	(0.029)
Observations	2,534	2,534	2,534	2,534	2,534	2,534
R-squared	0.256	0.633	0.436	0.763	0.406	0.406
Controls	yes	yes	yes	yes	yes	yes
Village f.e.	yes	yes	yes	yes	yes	yes
Demog. f.e. 2	yes	yes	yes	yes	yes	yes
Mean outcome	1.286	0.167	0.555	0.278	0.835	0.165

Regressions at the level of the mother. Column titles indicates the dependent variable. Each regression includes household level controls excluding the outcome variable. Errors clustered at the village level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 5: Heterogeneity: interactions of UPE ineligible sibling with child characteristics

VARIABLES	(1)	(2)	(3)	(4)	(5)
UPE Ineligible sibling	0.396	-0.163	-0.295	-0.087	-0.146
	(0.258)	(0.171)	(0.745)	(0.106)	(0.131)
Age × Ineligible	-0.047**				
	(0.022)				
Has older sibling × Ineligible		-0.017			
		(0.142)			
Siblings not yet school aged × Ineligible			0.083		
			(0.752)		
Girl × Ineligible				-0.179*	
				(0.098)	
Fraction girl siblings × ineligible					0.040
					(0.196)
Observations	5,315	5,315	5,315	5,315	4,933
R-squared	0.698	0.698	0.698	0.698	0.702
Controls	yes	yes	yes	yes	yes
Village f.e.	yes	yes	yes	yes	yes
Demog. f.e. 2	yes	yes	yes	yes	no*

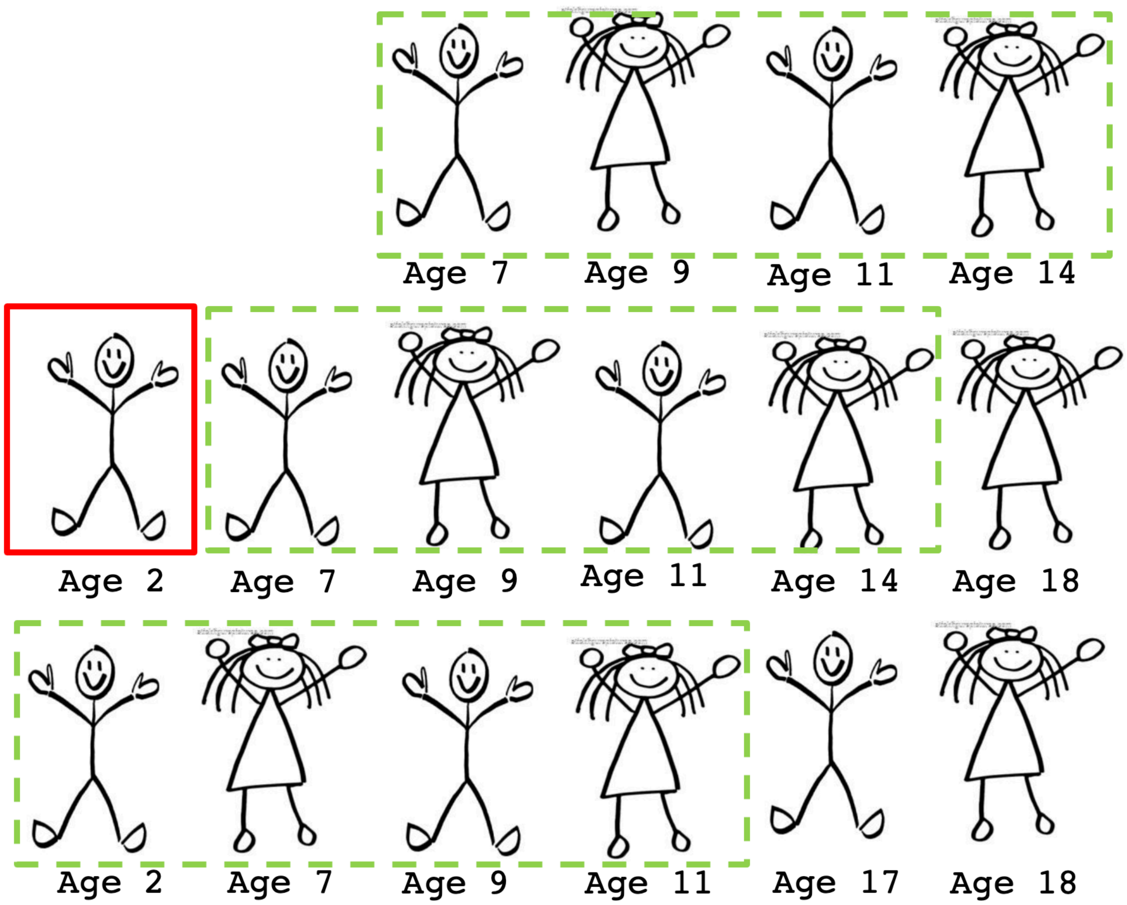
See notes under Table 2 for description of variables used as control. Interacted variables correspond to the characteristics of the older (eligible) child. Each regression includes the interacted variable as an additional control. Column 5 controls for number of children only. This is because the demographic fixed effects implicitly control for sibling gender composition. Errors clustered at the village level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 6: Heterogeneous effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Urban	Rural	Community in bottom wealth quintile	Community in middle wealth quintiles	Community in top wealth quintile	Mother prim. educ or less	Mother sec. educ or more	Exclude Northern Uganda
UPE Ineligible sibling	-0.485**	-0.068	0.041	-0.255*	-0.328*	-0.203**	-0.283	-0.222**
	(0.244)	(0.098)	(0.171)	(0.149)	(0.165)	(0.103)	(0.386)	(0.110)
Observations	1,436	3,879	1,005	2,768	1,542	4,449	866	4,439
R-squared	0.725	0.686	0.710	0.695	0.738	0.672	0.819	0.706
Controls	yes	yes	yes	yes	yes	yes	yes	yes
Village f.e.	yes	yes	yes	yes	yes	yes	yes	yes
Demog. f.e. 2	yes	yes	yes	yes	yes	yes	yes	yes
Mean outcome	3.788	2.619	2.860	2.863	3.113	2.672	4.289	3.052

Each column represents a different estimation of regression 1 on the sub-sample indicated by the column title. All household and demographic controls from table 2 included. Errors clustered at the village level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Figure 1: UPE eligibility scheme



Example of three different households with varying sibling composition and age structures. Children in dashed boxes are primary school-aged or younger, and eligible for UPE tuition fee waiver based on birth order. Child in red box is UPE ineligible based on birth order and presence of four older, eligible siblings. Children outside of boxes are aged out of primary school. See section 2.2 for more detailed discussion.

Figure 2: Years of schooling, cross-sectional estimates over the years

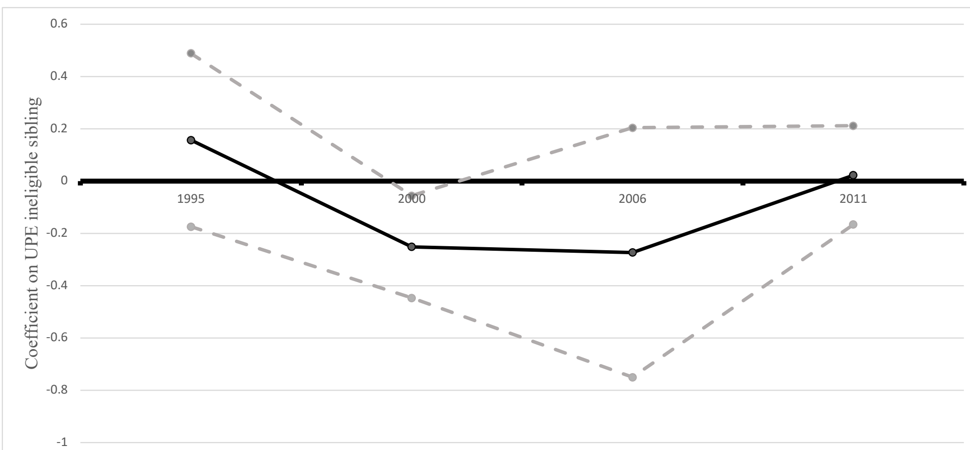
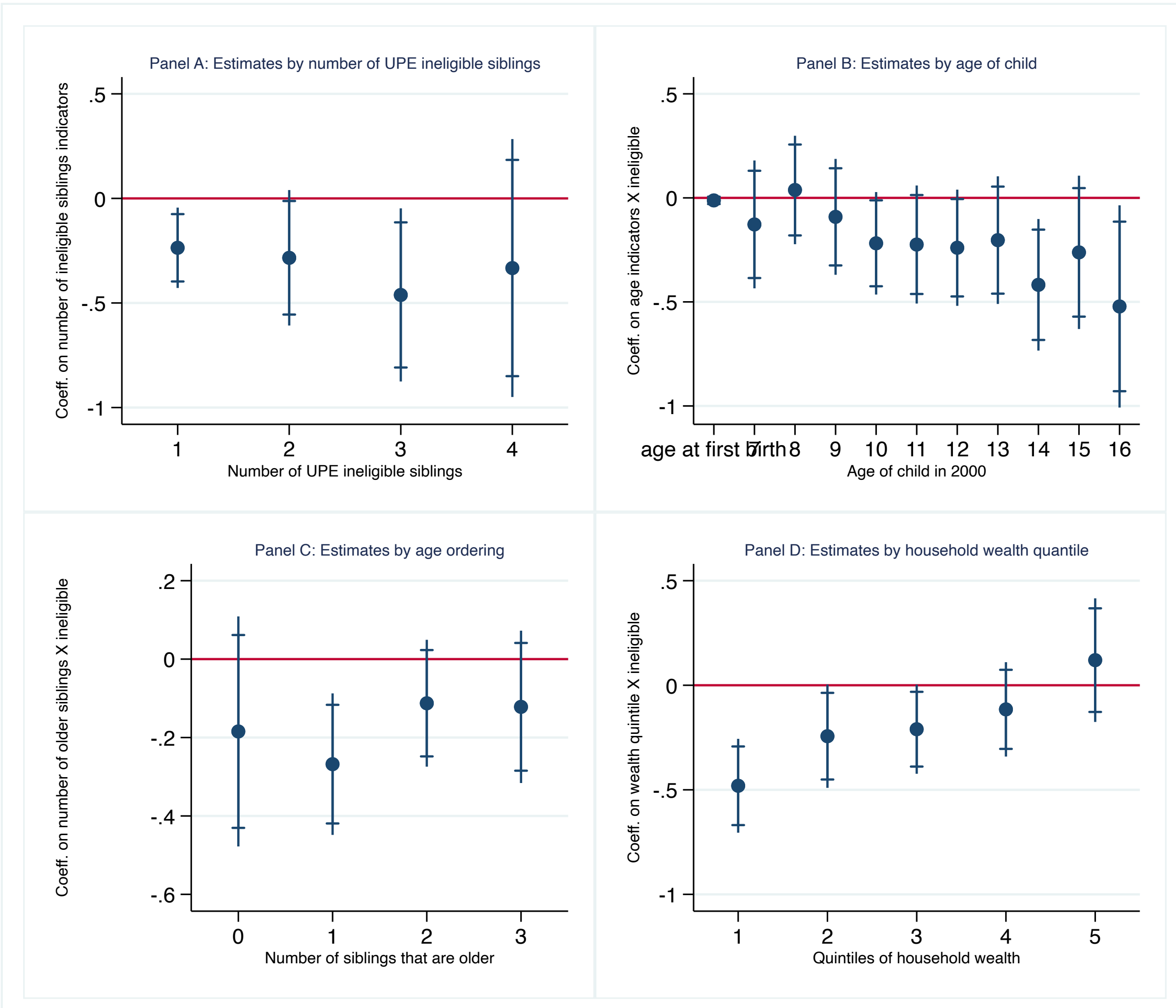


Figure reports point estimates of indicators for whether ineligible siblings are present in the household, using DHS rounds 1995, 2000/01, 2006 and 2011. All controls from table 2 are included except for wealth, which is omitted everywhere since it is missing in some years. Demographic controls 2 are used. 95% confidence intervals depicted with dashed lines.

Figure 3: Years of schooling estimate heterogeneity



Panel A: Point estimates of the interaction of UPE ineligible and number of children in the household in excess of the eligibility cutoff. Panel B: interactions with child age (in completed years) at the time of the interview. Panel C: Interactions with indicators of the number of eligible children that are older. Panel D: interactions with quintiles of household wealth. Regressions underlying each figure include the full set of controls, and demographic controls 3. Vertical lines indicate the 90th and 95th percent confidence intervals.

D Appendix figures and tables

Table A1: Enrollments by grade and age in 1995

Age	Enrolled	Years of schooling if in school							
of child	in primary	0	1	2	3	4	5	6	7
6	44.8	77.0	20.3	2.4	0.3	0.0	0.0	0.0	0.0
7	63.0	61.3	29.7	7.9	0.7	0.1	0.3	0.0	0.0
8	72.5	42.7	33.7	16.4	6.2	0.9	0.1	0.0	0.0
9	77.8	28.1	26.0	26.6	13.7	4.8	0.6	0.1	0.0
10	78.9	15.8	24.2	27.1	19.0	10.4	2.4	0.5	0.2
11	82.3	9.6	18.2	22.8	23.8	17.1	6.5	1.4	0.3
12	77.0	5.1	13.5	19.5	23.9	18.7	13.4	3.9	1.4
13	69.2	2.7	5.6	13.5	20.2	22.9	17.6	12.0	3.3
14	61.9	1.1	3.9	10.2	12.3	20.5	19.1	19.4	8.4
15	46.0	1.9	1.6	5.2	9.8	13.3	19.0	25.5	13.6
16	24.7	1.0	1.0	2.9	6.0	11.4	14.2	18.0	15.2
17	14.9	0.5	0.5	0.5	2.3	6.9	8.7	19.3	17.4
18	8.2	0.0	0.5	1.9	4.2	4.2	7.0	10.3	11.7
19	6.1	0.0	1.6	0.0	1.6	4.8	7.2	7.2	12.0

Data from the 1995 DHS. The column "enrolled in primary" presents the fraction of children who are currently enrolled in school and whose schooling attainment is "incomplete primary" or less, by child age. The remaining columns report the proportion of children by age and years of school completed, conditional on being currently enrolled in school (including secondary school.)

Table A2: Schooling gains following UPE reform among UPE eligible households

VARIABLES	(1)	(2)	(3)
post	0.316**	0.404***	0.404***
	(0.138)	(0.082)	(0.082)
Observations	5,552	5,549	5,549
R-squared	0.003	0.410	0.410
Controls	no	yes	yes
Village f.e.	no	no	no
Demog. f.e. 3	no	no	yes

Outcome variable: years of completed schooling for children 7-16. Data from the 1995 and 2000-20001 DHS include children living in households that do not face an eligibility cutoff. Errors are clustered at the village level.

Table A3: Estimates including children of mothers not in the household

VARIABLES	(1)	(2)	(3)	(4)	(5) LASSO
UPE Ineligible sibling	-0.080	-0.091	-0.120	-0.159*	-0.163*
	(0.071)	(0.067)	(0.073)	(0.093)	(0.084)
Observations	6,869	6,869	6,869	6,869	6,869
R-squared	0.603	0.662	0.664	0.667	
Controls	yes	yes	yes	yes	LASSO
Village f.e.	no	yes	yes	yes	yes
Demog. f.e. 1	no	no	yes	no	
Demog. f.e. 2	no	no	no	yes	
p-value	0.264	0.178	0.102	0.0865	0.0541
Mean years of schooling	2.861	2.861	2.861	2.861	2.861

Table reproduces benchmark regressions from Table 2. Sample includes children who reside in the household but whose mothers were not part of the household (i.e., the line number for the mother is entered in the dataset as missing). UPE Ineligible indicator and demographic fixed effects are calculated from birth records, and are therefore not affected by the inclusion of foster or orphaned children. The indicator for "mother does not reside in household" is included in the sets of individual controls.

Table A4: Difference in difference estimates of main schooling effects

Survey years	(1) 1995 and 2001	(2) 1995 and 2006
UPE Ineligible sibling x Post reform	-0.160*	-0.100
	(0.089)	(0.149)
P-value	0.073	0.501
Observations	9,736	10,991
Controls	yes	yes
Village f.e.	yes	yes
Demog. f.e. 2	yes	yes

Table reports the coefficient estimates of the interaction between the treatment indicator and an indicator for observations from the 2000 DHS (column 1) or from the 2006 DHS (column 2). Outcome variable is years of schooling. All controls from table 2 are included except for wealth, which is omitted everywhere since it is missing in some years. Demographic controls 2 are used. 95% confidence intervals depicted with dashed lines.

Figure A1: Histogram of the number of UPE ineligible siblings