Family size and schooling in Sub Saharan Africa:

Testing the quantity-quality trade-off

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Abstract

Many family planning programs start from the idea that "a small family is a happy family". Because of endogeneity issues, this idea is however difficult to verify empirically. In a compilation of DHS rounds from 35 SSA countries over the period 1990-2014, we exploit the birth of twins to study the effect of an exogenous increase in family size on children's schooling. Our findings do not provide any support for the generally assumed negative effect of family size on schooling attainments. Moreover, in the subsample of relatively wealthy households, we find a positive effect of family size on schooling. A tentative exploration of the underlying mechanism suggests that this positive effect is driven by the desire of parents to benefit from economies of scale by sending children early on to school, together with their siblings. In contrast to the relatively well-off, poor households do not have the room of manoeuvre to fulfil this desire. However, even in these latter households, we do not find empirical support for the quantity-quality trade-off.

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1. Introduction

Countries worldwide have devoted much effort and resources to family planning programs (Bongaarts, 2009). Most of these programs have been voluntary, but some have left little choice to parents, such as China's one-child program or India's notorious sterilization camps. Underlying these programs is, among others, the assumption that "a small family is a happy family" [3] and that small families lead to a stronger economy, because a reduction in family size enables families to raise investments in human capital, one of the key drivers of economic growth (Bongaarts, 2009). Intuitively the assumed causality between small family sizes and high schooling attainments makes sense: dividing scarce resources among less children, leaves each child with more resources.

This intuition found support in the first generation of social science theories. Judith Blake (1989), studying U.S. families, famously concluded that children from one- and two-child families are better educated and more successful than children in larger families because their parents have more time and money to invest in them. This 'resource dilution model' is also supported by economic theory, in particular in a pioneering paper written by Nobel Prize Laureate Gary S. Becker (1960), in which the quantity and quality of children are modelled as substitutes from the parents' point of view.

However, there also exist theories that support a *positive* causal effect of family size on children's schooling. These theories break with Blake's and Becker's assumptions that children – as long as they have not reached adulthood - only imply a cost to parents, and that more children imply higher costs. The quantity-quality trade-off needs no longer hold when, for instance, allowing for part-time child work (outside or inside the household) (Mueller, 1984; Marteleto and de Souza, 2013), or for economies of scale in raising children, with children sharing clothes, transport to school and text books (Rosenzweig and Wolpin, 2000; Qian, 2009). Economies of scale can also be present in household chores, such that the time each child spends on chores reduces with its number of siblings, thus freeing up time for school.

Despite the diversity of theoretical predictions, it is hard to shake off the intuitive interpretation of a negative causal relation. An important reason for its stickiness lies in the strong negative correlation between family size and children's schooling, and the difficulty to empirically distinguish this correlation from the actual causal effect of family size on schooling. To make this distinction, one needs to purge the correlation of important confounding factors. Most importantly, parents' characteristics may determine preferences both for the number of children

[3] This text featured on a 2 Rupee Coin issued by India in 1993, to promote family planning.

and their years of schooling. For instance, mothers who enjoyed more years of schooling may prefer smaller families and at the same time may attach more importance to their children's schooling. Other confounding factors include wealth, innate intelligence, social norms regarding fertility and child labor, labor market opportunities for adults and children, and the availability and quality of old-age security schemes and education policies (Rosenzweig and Wolpin, 1980; Angrist et al., 2010; Black et al., 2010). To the extent that these confounding factors are not perfectly observed and controlled for, the estimated relation between family size and children's schooling is plagued by endogeneity issues.

In this paper, we remedy this endogeneity problem in a sample of children from 89,531 households across 35 SSA countries. In 1,852 of these households twins were born, causing a quasi-exogenous increase in household size. Provided controls for certain mothers' characteristics (Smits and Monden, 2011)[4], and for the health condition of twins (Rosenzweig and Zhang, 2009)[5], it is generally accepted that the use of twinning in an instrumental variable model can isolate the causal effect of family size on the educational outcomes of children born *prior* to the twin birth (Angrist et al., 2010). Concretely, in our IV approach, we look at the outcomes of first- and second- born children in families of three or more children, using the birth of twins at the third birth order as the instrumental variable.

Our empirical investigation adds to the body of literature that has tried to empirically unearth the quantity-quality trade-off by relying on a number of techniques, such as exploiting the gradual roll-out of family planning programs (Qian, 2009), a randomized controlled trial in family planning (Sinha, 2005; Joshi and Schultz, 2007), and instrumental variable approaches based on reported miscarriages (Maralani, 2008), siblings' sex- composition (Angrist et al., 2010; Conley and Glauber, 2006; Black et al., 2010), and – as in our approach - twin birth (Rosenzweig and Wolpin, 1980; Rosenzweig and Zhang, 2009; Marteleto and de Souza, 2012, 2013; Angrist et al., 2010; Black et al., 2005, 2010). In contrast to the traditional OLS estimates, these exercises in causal identification have not uniformly yielded negative estimates of the effect of sibship on schooling. Instead, the

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[4] According to Smits and Monden (2011: pp. 1-2), "The most important factor associated with (dizygotic) twinning is maternal age. The number of twin pregnancies increases substantially with maternal age, until age 38, and then decreases again . Other factors associated with twinning are parity, maternal height, smoking, oral contraceptive use and race/ethnicity."

[5] Rosenzweig and Zhang (2009) point out that twins have lower average birth weight than singletons, and perhaps worse health or cognitive achievement later on, and this may threaten the exclusion restriction if parents therefore allocate resources away from twins, towards older singleton-birth children.

effect turns out to vary over time, across regions and subpopulations, and across the exact outcome of interest studied (e.g. private schooling, enrolment, attainment, IQ).[6]

None of these exercises in causal identification has however looked at Sub Saharan African countries. Our paper's main contribution is to fill this gap. There are several reasons why SSA provides an interesting setting for such analysis. First, in SSA, a majority of households face tight budget constraints, schooling is barely compulsory, and children's participation in the labor market and in time-consuming household chores is socially still largely accepted (Bass, 2004). Combined, these features make it very likely that a household's decision to invest in children's formal education involves important trade-offs, that may or may not be in line with the quantity-quality trade-off. Second, family members are culturally bound to act for the benefit of the collective, be it the nuclear family or the extended family, the clan or ethnic group (Lloyd and Blanc, 1996). Regarding the decision to invest in schooling, this implies that the cost of schooling may be born not by the household but by the extended family; and that the benefits of schooling are also expected to be shared – giving for instance way to the so-called 'chain arrangement' in which earlier-born children are sent to school and use their wage earnings to invest in their younger siblings later on, rendering a quantity-quality trade-off superfluous. Third, SSA still is the region with the highest fertility and lowest educational enrolments and attainments, increasing the relevance of research on these issues; and if and how they inter-relate. Fourth, given our reliance on a large dataset over the period 1990-2014, we can also tentatively explore whether the "Education For All" initiative launched in 2000 has altered the relation between children's educational outcomes and its determinants in SSA.

Our use of DHS data comes with both pros and cons. Among the pros, we count the availability of demographic and health data of mothers, allowing us to be the first to explicitly control for factors such as ethnicity and mother height that are likely to affect twinning. In doing so, we further purge this instrument of any potential source of endogeneity. Second, the detailed information on

[6] Studying Bangladesh' Matlab Program, Foster and Roy (1997) find that the fertility decrease caused by the family planning program has no effect on school enrollment. This non-result is confirmed by Sinha (2005) who looks at school enrollment in a later phase of the program. In the first study using twins as an instrument, Rosenzweig and Wolpin (1980) find that schooling levels of Indian children decrease with exogenous increases in fertility. Consecutive studies using twins find mixed results. Relying on US Census data, Caceres (2006) finds a negative effect of family size on the probability of enrollment in private schooling. Family size is found to negatively affects the IQ of younger cohorts in Norway (Black et al. 2010). Relying on Chinese twins for identification, Li et al. (2008) find a negative effect of sibship on schooling attainments. However, family size is found to have no effect on children's educational attainment in Norway (Black et al. 2005) and in Israel (Angrist et al. 2010). Furthermore, Marteleto and de Souza (2012) study the effect of family size on adolescents' schooling in Brazil, and find a positive effect in periods and regions in the earlier stages of socioeconomic development and with high fertility; but this effects disappears for recent periods "when the opportunities for child farm work have declined, education has expanded, and fertility has declined to below-replacement levels" (p. 1473). Qian (2009), relying on the roll-out of relaxations in China's One Child Policy, finds that an additional child significantly increased school enrollment of first-born children.

children's health allows us to verify that parents do not allocate resources away from twins – who may suffer from poorer health at birth - toward older singleton-birth children, thus further providing confidence in our instrument. Among the cons, we face the constraint that, beyond the third birth order, there are not sufficient observations on twin births in the DHS to provide enough power in the first stage. Our analysis therefore only focuses on the effect of family size on outcomes of the first- and second- born children, and its findings cannot readily be generalized to siblings with higher birth orders (Qian, 2009). Another point of attention, is that the DHS focuses on mothers of childbearing age, 15-49, such that the observed number of children may be below the eventual number of children. Consequently, the relation that we observe between sibship and schooling captures a process in motion, not in equilibrium. We will take care to interpret our results in this light. And, as will be explained further on, we also need to address the complexity of family structure in SSA, which includes polygamy and a non-negligible number of children living outside the household with extended family.

The next section sets the stage with a brief overview of fertility and schooling in SSA. We then describe our data; detail the empirical strategy; and present the results. In a nutshell: we find a positive relationship between family size and children's schooling, that turns significant for the relatively wealthy households. In the discussion that follows, we tentatively explore the mechanism underlying this finding.

2. Fertility and schooling in SSA

Figure 1 shows the total fertility rate (TFR[7]) for 46 SSA countries, for the year 1985 on the horizontal axis, and for the year 2014 on the vertical axis. On average, TFR reached 6.62 in 1985 but declined to 4.97 in 2014, thus corresponding to an average decline of 0.55 child per decade. Despite the decline, SSA remains the region with the highest TFR, compared to for instance 2.56 TFR in South-Asia and 2.11 in Latin-America in 2014 (World Development Indicators, World Bank, 2016). The averages hide important heterogeneity. Looking at the four quadrants in Figure 1, we note that countries with the lowest TFR in both periods are mainly located in Southern Africa (South Africa, Lesotho, Botswana, Namibia, Zimbabwe, Swaziland) while most countries with a marked decline over the 30-year period are located in East Africa (Rwanda, Ethiopia, Kenya, Eritrea). In contrast, most countries of West and Central Africa (at the notable exception of Ghana

[7] TFR is the sum of the age-specific fertility rates in a given year. In layman term the average number of children that would be born to a woman over her lifetime if she were to experience the exact current age-specific fertility rates through her lifetime, and she were to survive from birth through the end of her reproductive life (WDI, World Bank).

and Gabon) belong to quadrant 4, that features countries with persistently high TFR (compared to the other SSA countries).

Figure 2 shows gross school enrolment rates (GSE[8]) in primary education for 46 SSA countries, for the year 1985 on the horizontal axis and 2013 on the vertical axis. On average, GSE reached 79.24 in 1985 but increased to 98.96 in 2013, thus corresponding to an average increase of 6.80 percentage points per decade. Despite the strong increase, SSA remains the region with the lowest GSE, compared to for instance 109.06 GSE in Southern Asia and 109.03 in Latin-America and the Caribbean in 2013 (UIS UNESCO's database, 2016). The averages hide important heterogeneity, as depicted in the four quadrants of Figure 2. The countries in quadrant 4 are countries with higher than average GSE in both years (Swaziland, Zambia, Zimbabwe, Mozambique). The countries in quadrant 3 are countries where the increase was strongest over the 30-year period (Malawi, Rwanda, Benin, Ghana). Quadrant 1 features countries with persistently low GSE (Niger, Burkina-Faso, Mali, Guinea, Gambia), which are – with the exception of Djibouti - all located in West Africa.

As shown in Figure 3, GSE is negatively correlated with TFR across the 46 SSA countries both in 1985 and 2013. However, the strength of the correlation has diminished over time.

3. Data

In our empirical analysis, we rely on 93 DHS survey rounds from 35 SSA countries over the period 1990-2014. Appendix 10 gives an overview of the survey rounds by country and year.

We focus on the educational attainments of first- and second-born children in the age group 6 to 18 living with their parents. The lower-limit of 6 is the age at which many children in SSA start primary schooling. The upper-limit of 18 is the age of secondary schooling completion, provided a swift grade progression. We do not extend the upper-limit beyond 18 because post-secondary education still is relatively less developed in SSA, facing important supply side constraints, and because a considerable proportion of children above 18 live outside the household such that their schooling attainment goes unrecorded in DHS survey.

In our baseline approach, we restrict the sample to children whose siblings of schooling age (6-18) all reside within the household. This yields a sample A0 of 140,987 children of first and second

[8] The GSE gives the number of students enrolled in a given level of education, regardless of age, expressed as a percentage of the official school-age population corresponding to that level of education (UNESCO).

birth order of families with three or more children. To measure the educational attainment of these children, we look at their completed years of schooling at the time of the survey. Summary statistics in Table 1 show that there is an extensive variation in completed years of schooling at each age. For instance, the years of schooling attained at age 12 vary from 0 to 9 years with a mean of 3.12 and a standard deviation of 2.19. To disentangle the channels underlying the variation in the completed years of schooling, we additionally look at enrolment. In our sample, 25.92% of children never enrolled in school, varying from a low of 3.14% in Swaziland in the year 2007 to a high of 68.59% in Mali in 1996.

Our explanatory variable of interest is the number of children in a household. In our baseline results, we define this variable as the total number of sons and daughters of the head of the household residing in the household. On average, sibship counts 4.35 children, varying from 3 to 26. Most children in our baseline sample (63.43%) are from families of less than 5 children and 16.76% have more than 5 siblings.

Our main instrumental variable is the birth of twins, at second or third birth order. In the DHS birth records, there is a specific variable indicating whether a child is part of a twinship or not. To determine birth order, we consider all children of the household head including those who do not have their mother in the household. We match the birth records dataset (containing twinship and birth order) with the household dataset using a key based on household identification, mother line and age or identifier of household member. In our sample, the average percentage of families having twins at 3rd birth is 1.57%.

To check the robustness of our findings, we also perform estimations in two alternative samples. First, instead of looking at the educational attainments of first and second born children in families with three or more children, we restrict the sample of children to 92,311 first born children of families of 2+ children (Sample A1). Doing so, allows us to verify whether there is a difference of the impact of a twin birth at second and third birth order. Second, instead of restricting the sample to those children that are part of households where all children reside within the household, we expand the sample to include also children that live in households where one or more school-aged siblings reside outside the household. This approach yields a sample of 157,563 children of first or second birth order of families of 3+ children (Sample A2). While Sample A2 is arguably a more representative sample of the population, the empirical results obtained from it must be interpret more cautiously because of endogeneity issues surrounding the decision to 'outsource' children.

The summary statistics of the principal variables in our analysis can be consulted in Table 2. The summary statistics of the control variables, which we briefly explain in the next section, can be consulted in Appendix 9.

4. Empirical strategy

4.1. Baseline

We first examine the relationship between family size and completed years of schooling using ordinary least squares (OLS). Then, we use twin birth to instrument family size. To do so, we restrict the sample to families with at least three children and examine the completed years of education of the first- and second-born children (with twinning at third birth as instrumental variable). Focusing on the sample of n children born prior to twins at n+1th birth, avoids selection problems that arise because "families who choose to have another child after a twin birth may differ from families who choose to have another child after a singleton birth" (Black et al. 2005).

Concretely, the OLS specification takes the following form:

$$(Eq. 1)\quad Education_{hmfi} = \beta_0 + \beta_1\ Number\ of\ children_h + \beta_2 X_{hmfi} + \beta_3 X_{hm} + \beta_4 X_{hf} + \beta_5 X_h + \varepsilon_{hmfi}$$

where *h* indicates household, *m* mother, *f* father and *i* the individual child. $Education_{hmfi}$ alternatively equals the child's completed years of schooling and its enrolment defined as a dummy variable. We capture family size through $Number\ of\ children_h$ which is a count variable equals to total number of sons and daughters of the head of the household living in the household; X_{hmfi} comprises child-level characteristics such as its sex, an indicator variable for its age, succeeding birth interval and birth-year; X_{hm} is the set of mother-level characteristics including her education, age, age squared, height, ethnicity[9] and the number of her children who have died; X_{hf} is the set of father-level characteristics comprising his age and education; X_h includes household's residence area, wealth quintile, and the total number of women in the household. We cluster all error terms at household level.

The second stage of the IV specification is captured by the following equation:

$$(Eq. 2)\qquad Education_{hmfi} = \delta_0 + \delta_1\ \widehat{Number\ of\ children_h} + \delta_2 X_{hmfi} + \delta_3 X_{hm} + \delta_4 X_{hf} + \delta_5 X_h + \omega_{hmfi}$$

in which family size is instrumented in the first stage equation:

$$(Eq. 3)\qquad Number\ of\ children_h = \alpha_0 + \alpha_1\ Twin_h + \alpha_2 X_{hm} + \alpha_3 X_{hf} + \alpha_4 X_h + \vartheta_h.$$

[9] Mother's ethnicity is country specific and generated as follow : (country code x 1000) + ethnic group code. The insertion of these ethnicity fixed effects makes country fixed effect superfluous. In some settings, ethnicity is a sensitive variable and as such is not included in the DHS for a number of countries (e.g. Rwanda, Burundi). We use region of residence of the household as a proxy for ethnicity in a robustness check to account for that.

The indicator variable $Twin_h$ equals 1 if the n+1 birth is a multiple birth and 0 otherwise and X are control variables previously defined. ε_{hmfi}, ϑ_h and ω_{hmfi} are error terms.

Following Angrist et al. (2010), we allow for heterogeneity of family size in response to twin birth across subsamples[10]. We do so by including interaction terms in our first stage regressions between twin birth and a set of indicator variables (rural, mother is Muslim, rural West & Central Africa), thus adding the following regressors: $Twin_h * Rural$; $Twin_h * Mother\_Islam$ and $Twin_h * Rural\ West\&Central\ Africa$. The rationale for including these regressors is that fertility is higher in rural areas (compared to urban areas), in Muslim families and in West and Central Africa (compare to East and Southern Africa), and the twin instrument tends to perform less well in larger families (Angrist et al., 2010).

4.2. Robustness checks

Among our 89,531 DHS sample household, we count 19,848 polygamous units. In such units, decision making is partly decentralized as each mother will tend to care more about her own children. We therefore adopt two approaches: our baseline approach which considering households as unit of decision and a decentralized approach where decisions are assumed to be taken at the level of each mother. In the decentralized approach, the number of children is defined for each of the household head's wives as her total number of sons and daughters living in the household. Birth order is also defined at each of the household head's wives level. In that case, we cluster all errors terms at the level of the mother (instead of the household level).

In a second type of robustness check, we use alternative definitions of our key variables. First, we alternatively define number of children as total number of births given by the household head's wives[11]. Second, to rule out possible measurement errors in our schooling variable (for instance parents including years in kindergarten in completed years of education), we censor the completed years of education to the child's age minus 6. For instance, if a 12-year old in our sample report 9 years of completed education, we censor it at 6. Third, following Angrist et al. (2010), we try out a second instrument, i.e. sibship sex-composition. This alternative instrument relies on the idea that parents prefer to have both boys and girls rather than only children of the same sex. Hence, in the latter case, they may be more likely to have an additional child.

[10] For a complete discussion on this, see Angrist et al (2010), pp. 786-790.
[11] If the household head is female, we consider her total number of births.

In all cases, we use the Stata command ivreg2[12] to get statistics that are robust to heteroskedasticity and to get post-estimation tests (underidentification test, weak identification test and overidentification test).

5. Results

5.1 Baseline estimation

The estimation of Eq.1 yields a negative and significant relationship between family size and children schooling. However, as shown in Table 3, the coefficients are small in magnitude: an additional child decreases average completed years of schooling by 0.068 years which is only 2.98% of the average completed years of schooling. As this result does not isolate a causal link between our variables of interest, we turn to our IV estimations.

The estimates of the first stage Eq.3 are shown in Table 4. They indicate that twins at 3rd birth increase average family size by 0.595. The coefficient is estimated precisely, is significant at 1%, and is consistent with the range of coefficients found in previous research[13]. Twins' first stage effect on family size is homogeneous across rural and urban areas, but is significantly lower (by 0.122) for families living in rural West and Central Africa and in families where the mother is Muslim (by 0.136). In all cases, the twin instrument has reassuring first stage post-estimation statistics, with for instance the F-statistic well above 100.

In contrast to the OLS estimates, the IV second-stage estimates indicate a positive, and slightly significant effect (at 10%), of family size on completed years of schooling. In particular, an exogenous increase in family size increases educational attainment on average by 0.144 years which is about 6.32% of average completed years of schooling. This is similar to the effect size of 0.131 years found by Marteleto and de Souza (2012), in the Brazilian case. We find no effect on children's school enrolment (see Table 7).

5.2. On instrument validity

A concern when implementing the IV estimation is the violation of the exclusion restriction. The exclusion restriction may be threatened because of the presence of confounding factors, e.g. mothers' characteristics such as maternal age, maternal height, or ethnicity that may affect the probability of twinning (Smits and Monden, 2011). We control for these factors in our IV strategy

[12] To assess the impact on enrollment, we make use of ivprobit.

[13] For instance Marteleto and de Souza (2012) find an effect size of 0.61 for twins at 2nd birth and 0.77 for twins at 3rd birth; Angrist et al. (2010) find 0.44 and 0.59, respectively; compared to 0.68 and 0.76 in Black et al. (2005)

to purge the twin instrument of potential bias. In theory, multiple-birth-enhancing fertility treatments could pose another threat to our instrument's validity, but in practise this threat is neutralized in SSA due to its prohibitively high cost and because of cultural impediments (Inhorn, 2002).

Another potential violation of the exclusion restrictions stems from parental behaviour towards twins due to twins' lower average birth weight and its potential consequences for their health or cognitive achievements later on (Rosenzweig and Zhang, 2009). If the future earning potential of twins is thought to be lower, parents may divert resources from twins to singletons. A straightforward solution to control for this potential bias, would be to include birth weight in the regression, were it not that the DHS only includes this information for under-five year olds. As a second best, we therefore verify whether this 'diversion of resources' hypothesis is supported in the sample of under-five year olds (see Table 5).

Using our 93 DHS waves, we regress birth weight (in log terms) on a twin indicator[14] and find that twins indeed have a significantly lower average birth weight than singletons. Looking at children's body mass index percentile at the time of the survey[15], we find that twins and singletons belong to the same percentile[16] on average, suggesting no significant difference between the two groups. The closing of the gap suggests that twins are not treated differently than singletons. Alternatively, the apparent convergence could be driven by selective mortality of twins of lower birth weight. However, there is no clear evidence in the literature about the link between birth weight and the risk of high mortality, nor educational attainment for that matter (Conley et al., 2006, p.182)[17].

[14] Here the twin indicator equals 1 if the child is part of twinship and 0 otherwise. We control for several characteristics likely to affect birth weight such as mother age, mother education, mother's body mass, mother age at first birth, mother ethnicity, child sex, preceding birth interval, year of birth, household's residence and wealth quintile. All error terms are clustered at household level.

[15] We regress current body mass index and body mass index percentile on twin indicator, controlling for the factors in footnote 17, as well as for birth weight, child age, number of months of breastfeeding, number of under-5 year olds in the households and whether or not the mother lives with her husband.

[16] The difference in body mass index (BMI) is measured in terms of percentile for children. A child is consider under weighted if his BMI is below the 2nd percentile and over-weighted if his BMI is above the 85th percentile of BMI-for-age (see Must and Anderson, Body mass index in children and adolescents: considerations for population-based applications, International Journal of Obesity (2006) 30, p.591). Here, we use 20-tiles and consider a difference is significant if of at least one 20-tile.

[17] For instance, birth weight can affect neo-natal mortality in unexpected ways because of genes, fetus developmental problem and varying conditions across different situations. And we cannot control for all these variables.

Overall, there is little to no evidence that points to lower resource allocation towards twins. A simple mean t-test provided in Table 6 even shows that on average, twins enjoy higher education than singletons.

5.3. Heterogeneity

As mentioned in the introduction, the studies that have set out to identify the causal relationship between family size and schooling have produced mixed results, suggesting that the relation is context dependent, thus varying across space and time.

Hence, we explore heterogeneity in our dataset. First, we ran separate regression across subsamples with respect to gender, region and poverty status. To explore regional variation in the estimated relation, we contrast West and Central Africa with East and Southern Africa. To compare across poor and non-poor, we define families in the 1st and the 2nd wealth quintiles as poor and those in the 4th and the 5th quintiles as non-poor. We discard the 3th quintile to achieve a starker contrast between poor and non-poor. We found no differential effect across gender. In the regional subsamples, the estimated coefficient on family is not significantly different from zero in East and Southern Africa, but it is positive and slightly significant in West and Central Africa (the completed years of education increase by 0.166 years which represent 7.22% of the average in this region). In the poor and non-poor subsamples, we observe the positive effect of family size only in non-poor families (0.255 years or 7.86% of the average completed years of education for this category of households) (see details in Table 8).

Second, we compare the causal effect of family size on schooling across countries with persistently high fertility[18] and countries with either a low TFR or downward fertility trend[19]. In the high-fertility countries, we found a positive and significant effect (of 0.225 or 10.4% of the average in this group), particularly for non-poor households. The effect is insignificant for the low-fertility countries (see Table 9).

Finally, we explore whether the relation between family size and schooling has changed over time. To do so, we focus on the year 2000, which marked the launch of the Education For All initiative. Hence, we compare the effect of family size on schooling across children born prior to 2000 and children born from 2000 onwards. We do not find a differential effect of family size, but it is

[18] Burkina Faso, Burundi, Congo DR, Guinea, Malawi, Mali, Niger, Nigeria, Chad (see Lesthaeghe, R., 2014, p. 2)
[19] Ghana, Lesotho, Liberia, Madagascar, Namibia, Rwanda, Senegal, South Africa, Swaziland, Uganda (see Lesthaeghe, R., 2014, p. 2)

noteworthy that the association of wealth, gender and residence area with children's educational outcomes has much weakened over time, suggesting a democratisation of schooling (see Table 10).

5.4. Robustness

In the first of a series of eight robustness checks, we restrict our sample to first born children of families of 2+ children (sample A1), using twinning at 2^{nd} birth as an instrument. The first stage results suggest that twins at 2^{nd} birth increase average family size by 0.608 and perform less well as an instrument in families living in rural West and Central Africa. The second-stage results show no effect of family size on completed years of schooling, neither in richer households nor in poor ones (see Appendix 1).

Second, we turn to sample A2, which expands our baseline sample with households in which some school-aged children reside outside the household. The coefficient on family size is still positive but no longer significant. But again, we find a positive effect of family size on children's schooling in non-poor families (effect of 0.212 years for an average of 3.306 years). However, these coefficients should be interpreted with caution because of endogeneity issues surrounding the decision to 'outsource' children (see Appendix 2).

Third, we use region of residence of the household instead of mother's ethnicity to take into account countries in which ethnicity is a sensitive variable and as such is not included in the DHS (e.g. Rwanda, Burundi…). This has no influence on our results both in term of sign, significance and magnitude (see Appendix 3).

Fourth, we use an alternative definition to discriminate poor and non-poor families, defining the poverty line as the average value of the wealth index in each DHS round. This approach confirms the positive effect observed only in non-poor families (see results in Appendix 4).

Fifth, when using the alternative definition of family size (define as total births given by household head's wives) our results are confirmed (see Appendix 5) .

Sixth, given that children are admitted to primary school at 6 in most of the countries[20], we censored completed years of schooling to be at most equal to the child's age minus 6. Doing so, allows us to verify whether our findings are driven by possible measurement errors in our outcome variable (for

[20] World Bank data on Official entrance age to primary school (from http://data.worldbank.org/indicator)

instance years in kindergarten counted as years of schooling or children starting school very soon). Our findings remain similar (see Appendix 6).

Seventh, in the decentralized approach (mother as decision units), family size has no effect on completed years of education. However, we still observe a positive effect of family size in non-poor families (see Appendix 7).

Finally, we replace our twinning instrument with the sex-composition instrument[21]. Unlike Angrist et al. (2010), in our case, only twinning performs well in the first stage. For instance, where the F-statistic increases 100 for twinning, it barely reaches 20 for the sex composition instrument. We therefore focus on twinning in the main text, and refer the discussion on sex-composition as an instrument to the Appendix 8.

5.5 Mechanisms

To have a better understanding of the causal effect between family size and children education, in particular the striking positive effect observed only in rich families, we investigate the following three mechanisms: child labor, chain arrangement and economies of scale.

5.5.1. Child labor

Although declining, children's employment's still is common in many SSA countries. However, the group of children who are working is increasingly made up of children combining employment and schooling (Guarcello et al., 2015).

The combination of work and schooling may allow for child labor to contribute to schooling rather than crowding it out, e.g. by providing resources for schooling fees. Moreover, if children are working, the effect of family size on schooling could be positive because having more siblings could mean that children can share household chores such as fetching wood and water and divide labor market work, e.g. to make the combination of work and schooling possible rather than working full-time.

Should child labor explain the positive effect of family size, the effect would however be larger in poor households and lower in non-poor ones, given that the latter rely less on resources provided by children[22]. Instead, our subsample regression results (Table 8), indicate the reverse, i.e. that

[21] In our baseline sample, the two firstborns are boys in 25.36% of households while they are girls in 22.75%. Furthermore, 12.75% of families have only boys at the first three births and 10.89% have three girls.

[22] Indeed, in our sample, average total hours worked by children is significantly larger in poor families: 3.685 hours higher in a sample mean t-test.

family size has no effect on children schooling in poor household while the effect is positive, sizeable and significant at 5% in non-poor household.

To test more formally for the child labor mechanism, we exploit the available child labor information in a subset of 20 countries in our sample (the countries are listed in Appendix 10). If child labor contributes to schooling and explains the positive effect of family size, the estimated coefficient of child labor would be positive and that of family size would be reduced after controlling for child labor in our model. Results in Table 11 show that child labor exerts a small but insignificant positive effect. However, when exceeding 21 hours per week, the estimated effect of child labor turns negative and significant. This negative effect is confirmed when including siblings' labor instead of own child labor. Moreover, rather than attenuating the positive effect of family size, the inclusion of child labor reinforces the effect.

These results indicate that child labor is not the mechanism underlying the positive effect of family size on children schooling.

5.5.2 Chain arrangement

To maximize schooling of all children in their household, parents may turn to the so-called chain arrangement, whereby they invest in the schooling of the earlier-born children who are then expected to contribute in their turn to the education of their younger siblings. Compared to a resource dilution logic where parents' scarce resources are divided equally across their children, this strategy may result in higher schooling for all children (Marteleto and de Souza, 2012). However, the strategy is only feasible when there is a relatively large age difference between the earlier-born and later-born, enabling the earlier-born to enter the labor market before its younger siblings leave school.

If the positive effect of family size is driven by chain arrangement, we expect the effect to be larger in poor families, assuming that the cash constraint is not binding for non-poor families. However, our subsample regression results (see Table 8), suggest the reverse : family size only has a significant positive effect in non-poor households, not in poor households.

To test more directly for the chain arrangement mechanism, we include in our model the difference in age between the first born and last born child. We find that this difference takes on a negative and significant coefficient, contradicting the chain arrangement (see Table 12). Thus, we conclude that the chain arrangement is not supported by our analysis.

5.5.3. Economies of scale

Most economies of scale in household consumption come from shared goods and services like housing, child care and food preparation (Deaton and Paxson, 1998). Household economies of scale in food are larger than those in clothing and in transportation and roughly the same as those in household care, but the largest economies of scale are by far, in shelter (Nelson, 1988)[23].

Qian (2009, p. 9) argues that "economies of scale translate into positive effect of family size, if cost of an additional year of schooling is less than the savings parents gain from having an additional child". Economies of scale are more likely to kick in when children are close in age. This is in line with our previous result (in Table 12) that the smaller the age difference, the larger the educational attainment.

Economies of scale may be more important in the relatively wealthy households because their relatively higher and more diverse consumption pattern gives way to more scope for economies of scale. These expenditures include expenditures on schooling inputs, such as private tutoring, additional books, and transport to school. Also non-monetary inputs, such as time spent by parents in helping children with school homework may be subject to economies of scale (Rosenzweig and Zhang, 2009). Furthermore, children may also help each other with their homework and may be stimulated to do well at school be the presence of other school-going children in the household.
In so far these expenditures do not take place in poorer families, there is less scope for economies of scale. Furthermore, because of resource constraints, poor families may be unable to send their children to school, therefore unable to take advantage of any economies of scale this would entail. Thus, in response to an increase in family size, richer households may decide to send children to school earlier in order to take advantage of economies of scale in education, in our case particularly the second born. Instead, poor families might wait or delay entrance to school in order to spread the burden of education cost over time (nearly significant effect for non-poor families, see Table 14). This is also consistent with the fact that we do not observe any effect of family size on first born' completed years of schooling (see first paragraph of robustness checks).

We can further verify the economies of scale effect, by testing whether the data are consistent with the following two predictions: (i) At constant family size, the difference between the first born and last born ages negatively affects children schooling because of economies of scales and this effect is stronger in rich families; (ii) At constant family size, the two first children being of the same sex

[23] Cited in Li and Vernon, Journal of Political Economy (Dec. 2003, p. 1366).

positively affects children schooling because of higher economies of scales and this effect is stronger in rich families. The results in Table 13 confirm (i) but not (ii), as we found no effect of the two first children being of the same sex on completed years of education. Lower economies of scale in clothing (Nelson, 1988[24]) may explain the latter.

6. Conclusion

The aim of this study was to test the quantity-quality trade-off in SSA. To do so, we investigated the extent to which an increase in family size affects children's schooling, using twin birth as an instrumental variable to deal with endogeneity issues. Overall, our findings cast strong doubts on the generally assumed negative relation between family size and schooling. Moreover, in several subsamples – notably the richer households, we find a positive effect of family size on schooling and these results are confirmed by various robustness checks.

We tentatively explore child labor, chain arrangement and economies of scale in household consumption as potential mechanisms at work behind the positive effect in richer households. Our findings suggest that among the three, economies of scale in household consumption is more likely to be driving our results. Its effect dominates in richer families. In contrast, because of resource constraints, poor families may not be equipped to reap the economics of scale.

Exploring heterogeneity in our results, we found no evidence of a different relationship between family size and children schooling across West & Central Africa and East & Southern Africa. Also, there is no evidence of differential treatment between boys and girls. However, we found a larger effect in non-poor households (compared to poor), and also a larger effect in countries with persistently high fertility (compared to countries with low fertility).

While among the significant determinants of children's schooling, wealth, parental education, gender, residence area (urban/rural) and household wealth, remain important, their importance has decreased over time, i.e. for children born after 2000.

Our research suffers from a number of limitations. The first one is a classic drawback of using twin birth to instrument for family size in testing quantity-quality trade-off. Indeed, focusing on educational outcomes of the first- and second-born limits the scope for generalisation of our findings to higher parity children (Qian, 2009).

[24] Cited in Li and Vernon, Journal of Political Economy (Dec. 2003, p. 1366).

The other limitation lies in the nature of data used in our study. The DHS provides a snapshot of households in which women are of childbearing age (15-49). The number of children observed is therefore not equal to the eventual number of siblings. Hence, our results capture only short-term considerations in the quantity-quality trade-off, not equilibrium. Moreover, we lack data on household consumption to explicitly test the economies of scale mechanism that we put forward here to explain the positive effect of family size on children education. These gaps should be filled by future research, relying on other types of data.

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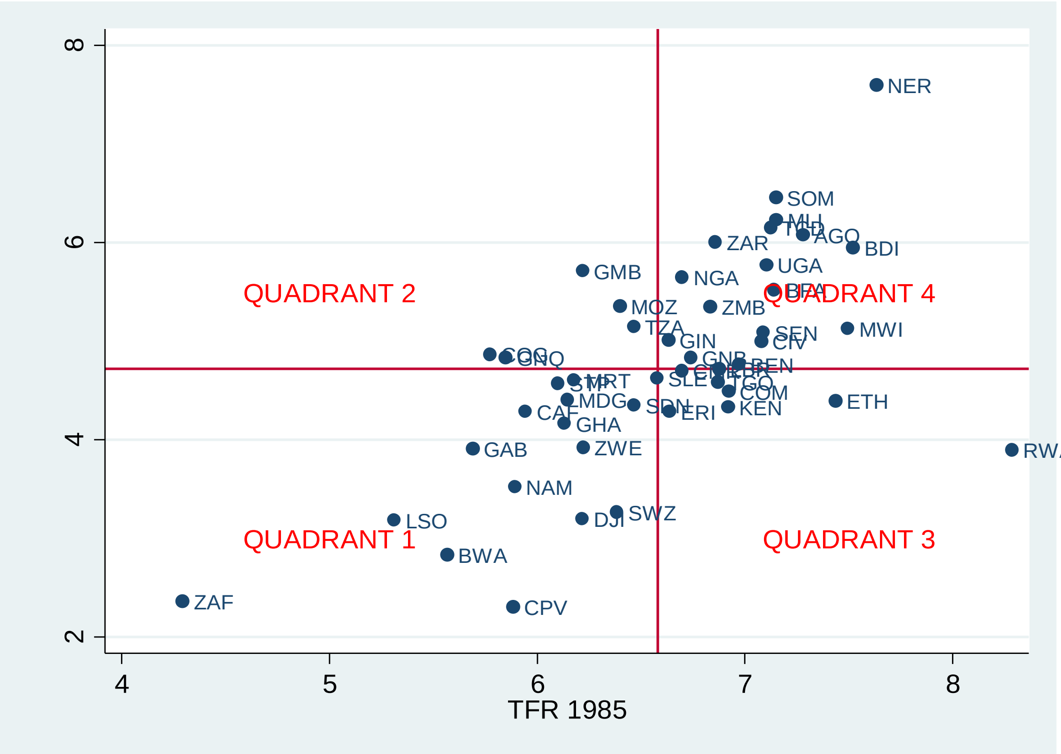
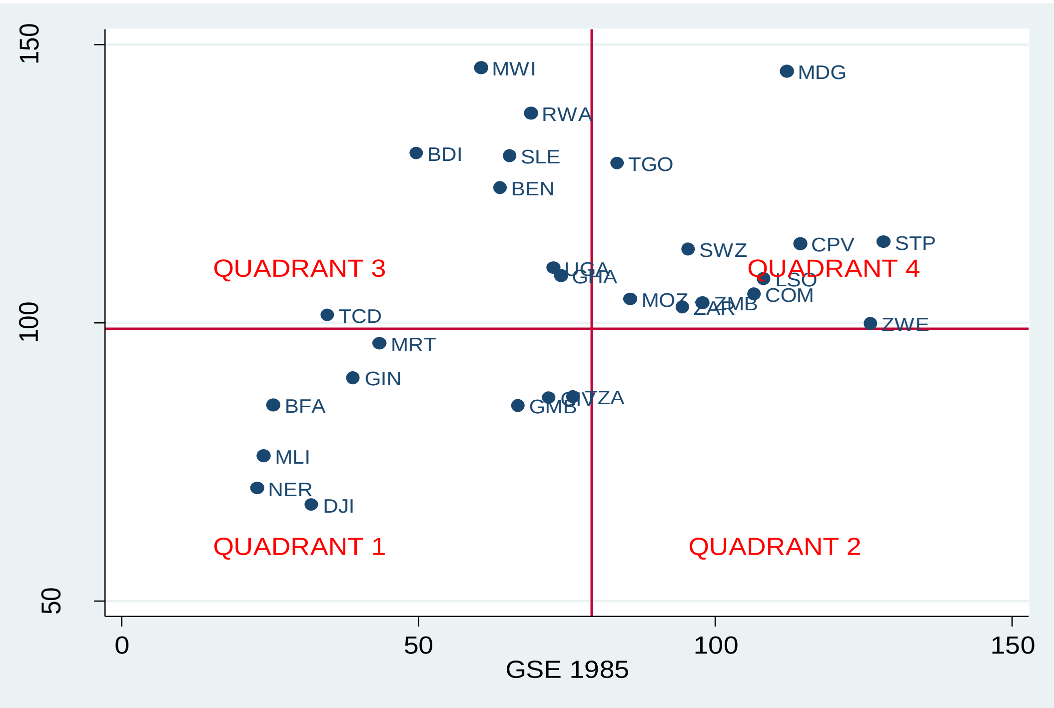
Figures and Tables

Figure 1 : Total Fertility rate in SSA in 1985 and 2014



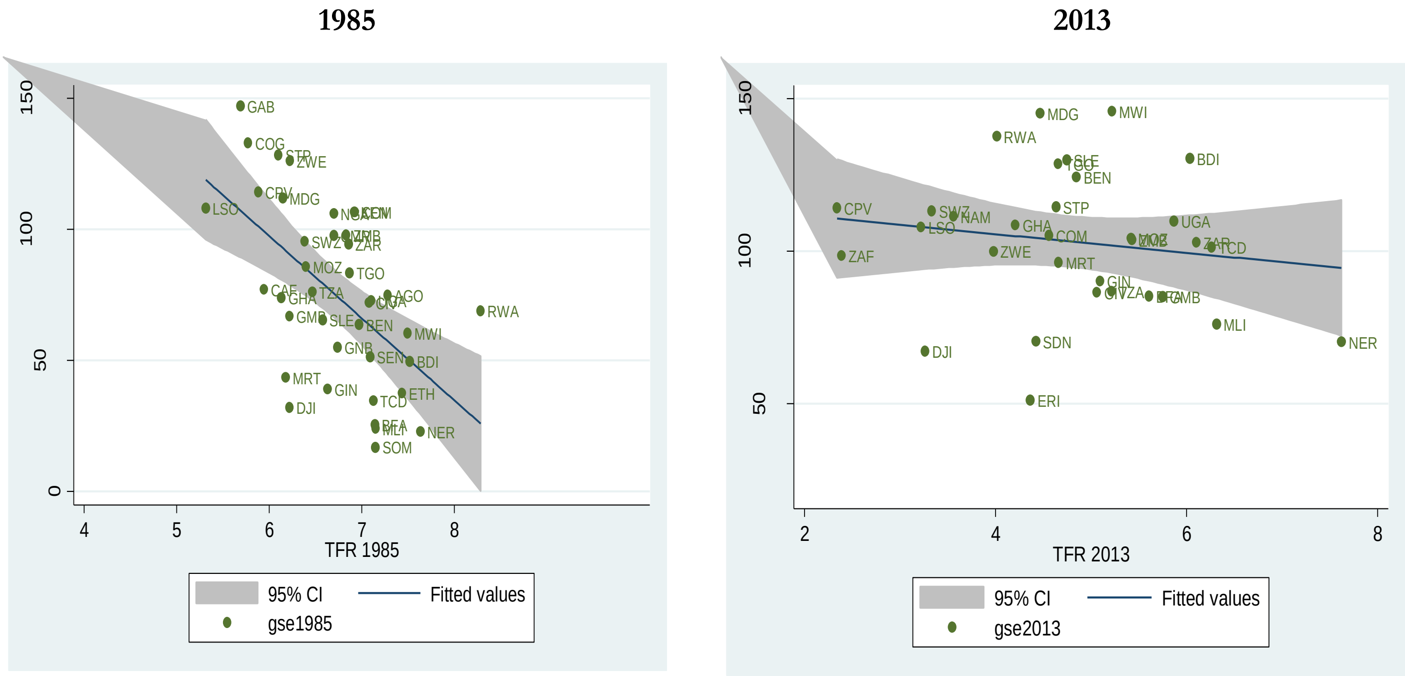
Source : Authors, based on data from World Development Indicators, World Bank, 2016

Figure 2: Gross primary school enrolment rate in SSA in 1985 and 2013



Source : Authors, based on data from UIS UNESCO. 2016

Figure 3 : Cross-country correlation between total fertility rate (TFR) and gross school enrolment (GSE) rate in 1985 and 2013



Source : Authors, based on data from World Development Indicators and UIS UNESCO, 2016

Table 1 : Enrolment and completed years of education at each age

Child age	Enrolment (%)	Completed year of education			
		Mean	**St. Dev.**	**Min**	**Max**
6	40.16	0.21	0.49	0	3
7	61.94	0.55	0.77	0	4
8	71.79	0.98	1.07	0	5
9	80.16	1.56	1.34	0	6
10	78.22	1.99	1.64	0	7
11	84.40	2.72	1.88	0	8
12	81.80	3.12	2.19	0	9
13	83.33	3.79	2.46	0	10
14	83.15	4.34	2.74	0	11
15	85.99	5.14	2.97	0	12
16	88.34	5.80	3.21	0	13
17	86.59	6.25	3.49	0	13

Source : Authors, based on Sample A0

Table 2: Sample means and proportions

Variables	Sample A0	Sample A1	Sample A2
Number of children	4.35	3.80	4.35
Completed years of schooling	2.28	2.48	2.34
Children age	10.21	10.40	10.40
Mother education (single years)	3.55	4.02	3.47
Mother age	31.10	30.38	31.33
Father education (single years)	4.97	5.40	4.88
Father age	38.92	37.89	39.23
Children Sex			
Male	51.12	50.94	51.04
Female	48.88	49.06	48.96
Number of children			
2	*n.a.*	*18.33*	*n.a.*
3	*31.99*	*30.44*	*32.01*
4	*31.44*	*24.76*	*31.27*
5	*19.82*	*14.54*	*19.85*
6 and +	*16.76*	*11.93*	*16.87*
Highest education level			
No education	*25.92*	*24.73*	*25.87*
Primary	*66.40*	*65.79*	*66.14*
Secondary	*7.67*	*9.46*	*7.98*
Tertiary	*0.01*	*0.02*	*0.01*
Residence			
Urban	*28.32*	*31.50*	*27.56*
Rural	*71.68*	*68.50*	*72.44*
Wealth quintile			
Poorest	*23.21*	*21.60*	*23.71*
Poorer	*19.62*	*18.89*	*19.99*
Middle	*19.10*	*18.34*	*19.23*
Richer	*18.58*	*18.77*	*18.47*
Richest	*19.49*	*22.40*	*18.60*
Twin birth			
Twin at 2nd birth	*1.20*	*1.30*	*1.09*
Twin at 3rd birth	*1.57*	*1.57*	*1.63*
Sex-composition			
First two born are boys	*25.28*	*26.01*	*24.83*
First two born are girls	*22.86*	*23.62*	*23.36*
First three born are boys	*12.66*	*13.15*	*12.47*
First three born are girls	*10.96*	*11.37*	*11.29*
Mother religion			
Islam	*34.32*	*32.42*	*34.93*
Not Islam	*65.68*	*67.58*	*65.07*
Region			
West and Central Africa	*54.49*	*53.28*	*55.41*
East and Southern Africa	*45.51*	*46.72*	*44.59*

Child labor			
less than 8 hrs/week	*60.90*	*62.90*	*60.43*
8 to 21 hrs/week	*23.37*	*22.12*	*23.43*
22 to 35hrs/week	*8.06*	*7.55*	*8.16*
35 hrs/week and+	*7.67*	*7.43*	*7.98*
N (number of observations)	**140,987**	**92,311**	**157,563**

Source : Authors, based on DHS data. Twin and sex-composition indicators are in terms of percentage of total number of households.

Table 3 : OLS estimates

	DEPENDENT VARIABLE : COMPLETED YEARS OF EDUCATION
Number of children	-0.067***
Observations	*100,702*
R-squared	*0.620*

*** p<0.01, ** p<0.05, * p<0.1

Note.—This table shows OLS estimates of number of children effect on children schooling. The sample includes first and second-born non twins from families with three or more children (3+). Control variables include child-level characteristics (sex, age and birth-year), mother-level characteristics (mother's education, mother's age, mother-age squared, mother height, mother ethnicity and the number of her children who have died), father-level characteristics (his age and education) and household-level characteristics (residence area, wealth quintile, and the total number of women in the household). Standard errors are clustered at household level. Sample is A0.

Table 4 : First stage estimates of twins birth effect using twin at 3rd birth instrument

	DEPENDENT VARIABLE : NUMBER OF CHILDREN			
	(1)	(2)	(3)	(4)
Twin3	0.595***	0.666***	0.642***	0.651***
Twin3 x Rural		-0.103		
Twin3 x Mother islam			-0.136*	
Twin3 x Rural West & Central Africa				-0.122*
Observations	*100,702*	*100,702*	*99,879*	*100,702*
F-statistic (excluded instrument)	*296.49*	*149.05*	*171.42*	*154.83*
Under identification test p-value	*0.000*	*0.000*	*0.000*	*0.000*
	491.19	*247.18*	*247.74*	*248.20*
Weak identification test: Cragg-Donald Wald F (Stock-Yogo critical values)	*(16.38)*	*(19.93)*	*(19.93)*	*(19.93)*
Ho: equation is weakly identified				

*** p<0.01, ** p<0.05, * p<0.1

Note.—This table shows first stage effects of twin at 3rd birth on number of children. The sample includes first and second-born non twins from families with three or more children (3+). In Col. (2), we interact twin at 3rd instrument with residence in Rural area; in Col. (3), we interact with mother's religion and in Col. (4) we interact with residence in Rural West & Central Africa. Control variables are ones specified in note of table 3. Standard errors are clustered at household level. Sample is A0.

Table 5 : Testing twin instrument validity using data on under-five children

	DEPENDENT VARIABLES :		
	(1) Log birth weight	(2) Log current weight	(4) Child's current body mass index 20-tile
Twin	-0.172***	-0.069***	-0.615***
Log birth weight		0.129***	2.59***
Observations	*60,067*	*40,614*	*40,251*
R-squared	*0.102*	*0.725*	*0.115*

*** p<0.01, ** p<0.05, * p<0.1

Note : In Col.1 we control for mother characteristics (age, education, body mass index, age at first birth, ethnicity) and child characteristics (sex, preceding birth interval, birth year) and household characteristics (residence, wealth). In Cols. (2) and (3) , we additionally control for number of under-five children living in household, months of breastfeeding, and whether mother lives or not with husband. All errors terms are clustered at household level. This table is based on data on under-five year children from our 93 DHS rounds.

Table 6 : Two-sample t test for completed years of education (mean) with equal variances

Group	Obs	Mean	Std. Err.	Std. Dev.	[95% Conf. Interval]	
Singletons	525,716	2.057	0.003	2.443	2.050	2.063
Twins	12,990	2.123	0.021	2.443	2.081	2.165
mean(Singletons) - mean(Twins)		-0.066	0.022		-0.108	-0.023

t =-3.029. Degrees of freedom = 538,704. p-value=0.001

Note: This table is based on completed years of education of all children in the 93 DHS round compiled.

Table 7 : Second stage IV estimates using twin at 3rd birth

	DEPENDENT VARIABLE: COMPLETED YEARS OF EDUCATION			DEPENDENT VARIABLE: ENROLLMENT		
	(1)	(2)	(3)	(4)	(5)	(6)
Number of children	0.144*	0.136*	0.134*	0.081	0.092	0.079
Observations	*100,702*	*99,879*	*100,702*	*100,571*	*99,748*	*100,571*
R-squared/p-value Wald chi2	*0.489*	*0.488*	*0.490*	*0.000*	*0.000*	*0.000*

*** p<0.01, ** p<0.05, * p<0.1

Note.—This table shows second stage estimates of number of children effect on children education. The sample includes firstborn and second-born non twins from families with three or more children (3+). For Cols. (1) to (3) dependent variable is completed years of education; for Cols. (4) to (6) dependent variable is school enrolment. In Cols (1) and (4) the instrument is twin at 3rd birth. In Cols.(2) and (5) twin at 3rd birth is interacted with mother's religion. In Cols. (3) and (6), twin at 3rd birth is interacted with residence in Rural West & Central Africa. Control variables are ones specified in note of table 3. Standard errors are clustered at household level. Sample is A0.

Table 8: Separate regressions to analyse heterogeneity

	Boys	Girls	Poor	Non poor	West & Central	East & Southern	Poor West & Central	Non poor West & Central	Poor East & Southern	Non poor East & Southern
2ND STAGE DEPENDENT VARIABLE: COMPLETED YEARS OF EDUCATION										
Number of children	0.132	0.137	0.033	0.255***	0.166*	0.117	0.096	0.199 [25]	-0.075	0.411***
Observations	*51,720*	*48,982*	*44,175*	*37,024*	*69,353*	*31,349*	*30,576*	*25,498*	*13,599*	*11,526*
R-squared	*0.484*	*0.498*	*0.333*	*0.593*	*0.443*	*0.524*	*0.272*	*0.556*	*0.485*	*0.663*
1ST STAGE DEPENDENT VARIABLE : NUMBER OF CHILDREN										
Twin3	0.609***	0.577***	0.509***	0.652***	0.580***	0.643***	0.481***	0.660***	0.589***	0.647***
F-statistic (excluded instrument)	*159.94*	*191.39*	*91.09*	*196.03*	*180.79*	*137.00*	*51.19*	*137.96*	*56.98*	*61.37*
Under identification test p-value	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*
Weak identification test: Cragg-Donald Wald F (Stock-Yogo critical values)	*262.02 (16.38)*	*224.43 (16.38)*	*138.40 (16.38)*	*279.13 (16.38)*	*294.76 (16.38)*	*231.31 (16.38)*	*78.65 (16.38)*	*179.36 (16.38)*	*74.64 (16.38)*	*109.70 (16.38)*
Ho: equation is weakly identified										

*** p<0.01, ** p<0.05, * p<0.1

Note.—This table shows second stage estimates of number of children on children education across subsamples of sample A0 using separate regressions. Control variables are ones specified in note of table 3. Standard errors are clustered at household level.

[25] p-value=0.101

Table 9: Effect of family size in various demographic stages

	High fertility	High fertility Poor	High fertility Non poor	Low Fertility	Low Fertility Poor	Low Fertility Non poor
	2ND STAGE DEPENDENT VARIABLE: COMPLETED YEARS OF EDUCATION					
Number of children	0.225*	-0.182	0.374**	0.062	0.050	0.037
Observations	*55,360*	*22,139*	*22,456*	*29,615*	*12,959*	*11,203*
R-squared	*0.439*	*0.270*	*0.535*	*0.566*	*0.426*	*0.662*
	1ST STAGE DEPENDENT VARIABLE : NUMBER OF CHILDREN					
Twin3	0.520***	0.470***	0.571***	0.560***	0.555***	0.599***
F-statistic (excluded instrument)	*116.89*	*27.25*	*94.76*	*83.35*	*40.09*	*37.70*
Under identification test p-value	*0.000*	*0.000*	*0.000*		*0.000*	*0.000*
Weak identification test: Cragg-Donald Wald F	*195.39*	*54.19*	*120.24*	*121.54*	*49.24*	*60.82*
(Stock-Yogo critical values)	*(16.38)*	*(16.38)*	*(16.38)*	*(16.38)*	*(16.38)*	*(16.38)*
Ho: equation is weakly identified						

*** p<0.01, ** p<0.05, * p<0.1

Note.—This table shows effects of number of children on education on specific group of countries. Control variables are ones specified in note of table 3. Standard errors are clustered at household level.

Table 10 : Family size effect on younger cohorts (children born after 2000)

	2ND STAGE DEPENDENT VARIABLE: COMPLETED YEARS OF EDUCATION	
Number of children	0.129	
*Number of children * young cohort*	*-0.051*	
Child sex (*Male*)		
Female	*-0.052****	
*Female * young cohort*	*0.068****	
Mother's completed years of education	0.097***	
*Mother's completed years of education * young cohort*	-0.067***	
Father's completed years of education	0.087***	
Father's completed years of education young cohort*	-0.044***	
Residence (*Urban*)		
Rural	*-0.433****	
*Rural * young cohort*	*0.274****	
Wealth quintile	*0.286****	
*Wealth quintile * young cohort*	*-0.099*	
Observations	*100,702*	
R-squared	*0.510*	
	1ST STAGE DEPENDENT VARIABLES	
	NUMBER OF CHILDREN	**NUMBER OF CHILDREN * YOUNG COHORT**
Twin3	0.579***	0.088***
Twin3 * young cohort	0.045	0.396***
Sanderson-Windmeijer multivariate F-statistic (excluded instrument)	*77.23*	*78.31*
Under identification test p-value	*0.000*	
Weak identification test: Cragg-Donald Wald F	*65.28*	
(Stock-Yogo critical values)	*(7.03)*	
Ho: equation is weakly identified		

*** p<0.01, ** p<0.05, * p<0.1

Note : This table shows effect of number of children and other significant variables on children education in younger cohort in sample A0. Control variables are ones specified in note of table 3. Standard errors are clustered at household level.

Table 11: Formal test of child labor mechanism

		2ND STAGE DEPENDENT VARIABLE: COMPLETED YEARS OF EDUCATION		
		(1)	(2)	(3)
Number of children		0.341**	0.362**	0.387**
Child labor	*(less than 8 hrs/week)*		-	
	8 to 21 hrs/week		*0.034*	
	22 to 35hrs/week		*-0.113****	
	35 hrs/week and+		*-0.325****	
Siblings labor (total hours)				*-0.008****
Observations		*35,217*	*35,217*	*35,217*
R-squared		*0.320*	*0.317*	*0.317*
		1ST STAGE DEPENDENT VARIABLE : NUMBER OF CHILDREN		
Twin3		0.534***	0.532***	0.505***
F-statistic (excluded instrument)		*104.10*	*102.68*	*90.62*
Under identification test p-value		*0.000*	*0.000*	*0.000*
Weak identification test:				
Cragg-Donald Wald F		*137.73*	*136.55*	*128.68*
(Stock-Yogo critical values)		*(16.38)*	*(16.38)*	*(16.38)*
Ho: equation is weakly identified				

*** p<0.01, ** p<0.05, * p<0.1

Note: Col. (1) does not includes child labor; Col. (2) includes own child labor and Col.(3) includes siblings child labor. Control variables are ones specified in note of table 3. Standard errors are clustered at household level. Sample A0 is here restricted to countries for which data on child labor is available.

Table 12: Formal test of chain arrangement mechanism

	2ND STAGE DEPENDENT VARIABLE: COMPLETED YEARS OF EDUCATION	
	(1)	(2)
Number of children	0.144*	0.099*
Difference in age between first born and last born		-0.042***
Observations	*100,702*	*99,709*
R-squared	*0.489*	*0.496*
	1ST STAGE DEPENDENT VARIABLE : NUMBER OF CHILDREN	
Twin3	0.595***	0.751***
F-statistic (excluded instrument)	*296.49*	*683.10*
Under identification test p-value	*0.000*	*0.000*
Weak identification test: Cragg-Donald Wald F	*491.19*	*1061.39*
(Stock-Yogo critical values)	*(16.38)*	*(16.38)*
Ho: equation is weakly identified		

*** p<0.01, ** p<0.05, * p<0.1

Note: Difference in age = Age (First born) – Age (Last born). Col. (1) does not include difference in age between first born and last born; Col. (2) includes difference in age between first born and last born. Control variables are ones specified in note of table 3. Standard errors are clustered at household level. Sample is A0.

Table 13: Formal tests of economies of scale channel

	Poor (1)	Non poor (2)	Poor (3)	Non poor (4)
	2ND STAGE DEPENDENT VARIABLE: COMPLETED YEARS OF EDUCATION			
Number of children	0.029	0.193**	0.035	0. 254***
Difference in first born and last born ages	-0.013	-0.062***		
Two first children being the same sex			-0.018	-0.023
Observations	*43,720*	*36,697*	*44,175*	*37,024*
R-squared	*0.334*	*0.605*	*0.333*	*0.593*
	1ST STAGE DEPENDENT VARIABLE : NUMBER OF CHILDREN			
Twin3	0.665***	*0.797****	*0.511****	*0.652****
F-statistic (excluded instrument)	*210.38*	*470.20*	*91.72*	*196.06*
Under identification test p-value	*0.000*	*0.000*	*0.000*	*0.000*
Weak identification test: Cragg-Donald Wald F (Stock-Yogo critical values)	*311.16 (16.38)*	*585.35 (16.38)*	*139.46 (16.38)*	*278.78 (16.38)*
Ho: equation is weakly identified				

*** p<0.01, ** p<0.05, * p<0.1

Note: In Cols. (1) and (2) we include difference in age between first born and last born while in Cols (3) and (4) we include a dummy for two first children being the same sex to capture economies of scale. Control variables are ones specified in note of table 3. Standard errors are clustered at household level. Sample is A0.

Table 14 : School starting age of second born

	2ND STAGE DEPENDENT VARIABLE: SCHOOL STARTING AGE OF SECOND BORN		
	All (1)	Poor (2)	Non Poor (3)
Number of children	-0.053	0.279	-0.315[26]
Observations	*9097*	*3,041*	*4,331*
R-squared	*0.380*	*0.357*	*0.254*
	1ST STAGE DEPENDENT VARIABLE : NUMBER OF CHILDREN		
Twin3	*0.684****	*0.662****	*0.704****
F-statistic (excluded instrument)	*86.09*	*26.50*	*47.85*
Under identification test p-value	*0.000*	*0.000*	*0.000*
Weak identification test: Cragg-Donald Wald F (Stock-Yogo critical values)	*74.85 (16.38)*	*21.10 (16.38)*	*44.61 (16.38)*
Ho: equation is weakly identified			

*** p<0.01, ** p<0.05, * p<0.1

Note: This table shows the effect of an exogenous increase in number of children on school starting age for second born. Control variables are ones specified in note of table 3. Standard errors are clustered at household level. Sample is A0 restricted to children enrolled to school at the time of the survey.

[26] p-value=0.103

APPENDIXES

Appendix 1 : Robustness check using sample of first born in 2+ families with no children living outside

	2ND STAGE DEPENDENT VARIABLE: COMPLETED YEARS OF EDUCATION			
	(1)	(2)	(3)	(4)
Number of children	0.084	0.084	0.076	0.097
Observations	*65,130*	*65,130*	*64,343*	*65,130*
R-squared	*0.510*	*0.510*	*0.509*	*0.570*
	1ST STAGE DEPENDENT VARIABLE : NUMBER OF CHILDREN			
Twin2	0.608***	*0.676****	*0.629****	*0.703****
Twin2 x Rural		-0.107		
Twin2 x Mother islam			-0.063	
Twin2 x Rural West & Central Africa				-0.209***
F-statistic (excluded instrument)	*315.72*	*165.70*	*157.57*	*180.97*
Under identification test p-value	*0.000*	*0.000*	*0.000*	*0.000*
Weak identification test: Cragg-Donald Wald F (Stock-Yogo critical values)	*299.66 (16.38)*	*150.91 (19.93)*	*149.32 (19.93)*	*154.30 (19.93)*
Ho: equation is weakly identified				

*** p<0.01, ** p<0.05, * p<0.1

Note.—This table shows IV estimates using twin at 2nd birth instrument. The sample includes firstborn non twins from families with two or more children (2+). In Col.(2) we interact the instrument with residence in Rural area; in Col. (3) , we interact with mother's religion and in Col. (4) we interact with residence in Rural West & Central Africa. Control variables are ones specified in note of table 3. Standard errors are clustered at household level. Sample is A1.

Appendix 2: Robustness check using baseline sample extended to children with schooling age siblings living outside (Sample A2)

	All	Poor	Non poor
	2ND STAGE DEPENDENT VARIABLE: COMPLETED YEARS OF EDUCATION		
Number of children	0.124	0.076	0.212**
Observations	*113,192*	*50,733*	*40,441*
R-squared	*0.470*	*0.315*	*0.468*
	1ST STAGE DEPENDENT VARIABLE : NUMBER OF CHILDREN		
Twin3	0.551***	0.473***	0.608***
F-statistic (excluded instrument)	*287.61*	*90.86*	*177.31*
Under identification test p-value	*0.000*	*0.000*	*0.000*
Weak identification test: Cragg-Donald Wald F	*461.84*	*135.65*	*255.02*
(Stock-Yogo critical values)	*(16.38)*	*(16.38)*	*(16.38)*
Ho: equation is weakly identified			

*** p<0.01, ** p<0.05, * p<0.1

Note.—This table shows IV estimates of number of children on children education using sample A2. IV is twin at 3rd birth. The sample includes firstborn and second born non twins from families with three or more children (3+) without child living outside + with school age children living elsewhere. Control variables are ones specified in note of table 3. Standard errors are clustered at household level.

Appendix 3: Robustness check with region of residence instead of mother's ethnicity

	2ND STAGE DEPENDENT VARIABLE: COMPLETED YEARS OF EDUCATION		
	All	Poor	Non poor
Number of children	0.113*	0.008	0.232***
Observations	*138,755*	*59,509*	*52,662*
R-squared	*0.509*	*0.367*	*0.604*
	1ST STAGE DEPENDENT VARIABLE : NUMBER OF CHILDREN		
Twin3	0.585***	0.513***	0.633***
F-statistic (excluded instrument)	*406.42*	*122.91*	*250.63*
Under identification test p-value	*0.000*	*0.000*	*0.000*
Weak identification test:			
Cragg-Donald Wald F	*655.40*	*185.94*	*367.62*
(Stock-Yogo critical values)	*(16.38)*	*(16.38)*	*(16.38)*
Ho: equation is weakly identified			

*** p<0.01, ** p<0.05, * p<0.1

Note.—This table shows effects of number of children on education using sample A0. IV is twin at 3rd birth. Region of residence is used instead of mother's ethnicity. Control variables are ones specified in note of table 3. Standard errors are clustered at household level.

Appendix 4 : Robustness check with alternative definition of poverty

	2ND STAGE DEPENDENT VARIABLE: COMPLETED YEARS OF EDUCATION	
	Poor	Non poor
Number of children	0.049	0.198**
Observations	*59,951*	*40,751*
R-squared	*0.366*	*0.570*
	1ST STAGE DEPENDENT VARIABLE : NUMBER OF CHILDREN	
Twin3	0.515***	0.687***
F-statistic (excluded instrument)	*135.21*	*162.44*
Under identification test p-value	*0.000*	*0.000*
Weak identification test: Cragg-Donald Wald F	*194.12*	*325.98*
(Stock-Yogo critical values)	*(16.38)*	*(16.38)*
Ho: equation is weakly identified		

*** p<0.01, ** p<0.05, * p<0.1

Note.—This table shows effects of number of children on education using sample A0. IV is twin at 3rd birth. Poverty line is alternatively defined as average wealth index in each DHS round. Control variables are ones specified in note of table 3. Standard errors are clustered at household level.

Appendix 5: Robustness check with number of children defined as total number of birth (centralized decision at household level)

	2ND STAGE DEPENDENT VARIABLE: COMPLETED YEARS OF EDUCATION		
	All	Poor	Non poor
Number of children	0.135**	0.030	0.249***
Observations	*100,702*	*44,175*	*37,024*
R-squared	*0.490*	*0.333*	*0.595*
	1ST STAGE DEPENDENT VARIABLE : NUMBER OF CHILDREN		
Twin3	*0.631****	*0.570****	*0.669****
F-statistic (excluded instrument)	*366.34*	*118.29*	*205.60*
Under identification test p-value	*0.000*	*0.000*	*0.000*
Weak identification test:			
Cragg-Donald Wald F	*628.24*	*200.58*	*329.13*
(Stock-Yogo critical values)	*(16.38)*	*(16.38)*	*(16.38)*
Ho: equation is weakly identified			

*** p<0.01, ** p<0.05, * p<0.1

Note.—This table shows effects of number of children on education using sample A0. IV is twin at 3rd birth. Number of children is alternatively defined as total number of births given by the household head's wives. Control variables are ones specified in note of table 3. Standard errors are clustered at household level.

Appendix 6 : Robustness check using censored completed years of schooling.

	2ND STAGE DEPENDENT VARIABLE: CENSORED COMPLETED YEARS OF EDUCATION		
	All	Poor	Non poor
Number of children	0.149**	0.054	0.243***
Observations	*100,702*	*44,175*	*37,024*
R-squared	*0.509*	*0.350*	*0.646*
	1ST STAGE DEPENDENT VARIABLE : NUMBER OF CHILDREN		
Twin3	*0.595****	*0.509****	*0.652****
F-statistic (excluded instrument)	*296.49*	*91.09*	*196.03*
Under identification test p-value	*0.000*	*0.000*	*0.000*
Weak identification test:			
Cragg-Donald Wald F	*491.19*	*138.40*	*279.13*
(Stock-Yogo critical values)	*(16.38)*	*(16.38)*	*(16.38)*
Ho: equation is weakly identified			

*** p<0.01, ** p<0.05, * p<0.1

Note.—This table shows effects of number of children on education using sample A0. IV is twin at 3rd birth. Censored completed years of schooling is at most equal to child's age minus 6. Control variables are ones specified in note of table 3. Standard errors are clustered at household level.

Appendix 7: Regressions results for decentralized level of decision (mother level analysis)

	Sample A0'	Sample A0' Poor	Sample A0' Non poor	Sample A2'	Sample A2' Poor	Sample A2' Non poor
	2ND STAGE DEPENDENT VARIABLE: COMPLETED YEARS OF EDUCATION					
Number of children	0.106	0.020	0.292***	0.080	0.030	0.258**
Observations	*118,213*	*52,071*	*43,118*	*133,351*	*59,946*	*47,241*
R-squared	*0.486*	*0.322*	*0.586*	*0.466*	*0.306*	*0.570*
	1ST STAGE DEPENDENT VARIABLE : NUMBER OF CHILDREN					
Twin3	0.571***	0.533***	0.598***	0.535***	0.503***	0.557***
F-statistic (excluded instrument)	*479.95*	*181.92*	*235.84*	470.19	*180.08*	*215.79*
Under identification test p-value	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*
Weak identification test:						
Cragg-Donald Wald F	824.94	304.73	371.47	802.69	303.50	345.94
(Stock-Yogo critical values)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)	(16.38)
Ho: equation is weakly identified						

*** p<0.01, ** p<0.05, * p<0.1

Note.—This table shows effects of number of children on education using baseline sample A0' and robustness check sample A2' (equivalent to samples A0 and A2 but with mother as unit of decision). IV is twin at 3rd birth. Unit of decision here is mother (decentralized approach). Control variables are ones specified in note of table 3. Standard errors are clustered at mother level.

Appendix 8 : First stage results with sex-composition as IV

The first stage equation with sex-composition is as follow:

$$Famsize_h = \gamma_0 + \gamma_1 b_{hmfi} + \gamma_2 b_{12h} + \gamma_3 g_{12h} + \gamma_4(1 - s_{12h})b_{3h} + \gamma_5 b_{123h} + \gamma_6 g_{123h} + \gamma_7 X_{hm} + \gamma_8 X_{hf} + \gamma_9 X_h + \theta_i$$

With b_{hmfi} : child sex

b_{12h} : dummy for boys at first and second births

b_{123h} : dummy for all-boy triple (boys at first, second and third births)

g_{12h} : dummy for girls at first and second births

g_{123h} : dummy for all-girl triple

s_{12h} : dummy for same sex at first and second births ($b_{12} + g_{12}$)

b_{3h} : boy at third birth.

X are control variables previously defined (kindly refer to p9).

Table 15 : First stage estimates of sex-composition

DEPENDENT VARIABLE : NUMBER OF CHILDREN				
	Sex-composition in 3+ families			
	(1)	(2)	(3)	(4)
All girls triple	0.021	0.034	0.040**	0.074***
All boys triple	0.015	0.027	-0.002	0.060***
Boy3 x (1 - samesex12)	-0.108***	-0.140***	-0.099***	-0.084***
All girls x Rural		-0.018		
All boys x Rural		-0.017		
Boy3 x (1 - samesex12) x Rural		0.046**		
All girls x Mother islam			-0.046*	
All boys x Mother islam			0.042	
Boy3 x (1 - samesex12) x Mother islam			-0.025	
All girls x Rural West & Central Africa				-0.122***
All boys x Rural West & Central Africa				-0.098***
Boy3 x (1 - samesex12) x West & Central Africa				-0.052***
Observations	*100,702*	*100,702*	*99,879*	*100,702*
F-statistic (excluded instrument)	*21.05*	*14.33*	*14.28*	*16.33*
Under identification test p-value	*0.000*	*0.000*	*0.000*	*0.000*
Weak identification test: Cragg-Donald Wald F	*33.04*	*21.89*	*22.46*	*26.76*
(Stock-Yogo critical values)	*(10.83)*	*(11.39)*	*(11.39)*	*(11.39)*
Ho: equation is weakly identified				

*** p<0.01, ** p<0.05, * p<0.1

Note.—This table shows first stage effects of sex-composition instrument on number of children using sample A0. In Col (1) IV are sex-composition dummies, in Col. (2), we interact with residence in residence in Rural area; in Col. (3) we interact with mother's religion and in Col. (4) we interact with residence in Rural West & Central Africa. Control variables are ones specified in note of table 3. Standard errors are clustered at household level.

Appendix 9 : Summary statistics of control variables

Variables	Sample A0	Sample A1	Sample A2
Number of 15-49 women in the household (wives or household's head)	1.06	1.05	1.07
Succeding birth interval (months)	34.24	36.24	33.94
Mother age square	989.52	947.18	1004.51
Mother height (cms)	158.71	158.67	158.73
Number of dead children of mother	0.33	0.26	0.34
Child birth year			
1972	0.00		0.00
1975	0.02	0.02	0.02
1976	0.07	0.09	0.07
1977	0.14	0.16	0.15
1978	0.43	0.50	0.43
1979	0.47	0.49	0.46
1980	0.74	0.67	0.69
1981	0.74	0.71	0.71
1982	0.99	0.92	0.95
1983	1.31	1.32	1.28
1984	1.44	1.49	1.44
1985	1.59	1.60	1.58
1986	1.77	1.86	1.77
1987	1.84	1.99	1.88
1988	2.01	2.11	2.07
1989	2.54	2.71	2.61
1990	2.95	3.13	3.06
1991	3.20	3.37	3.32
1992	3.46	3.65	3.61
1993	4.07	4.32	4.20
1994	4.63	4.91	4.77
1995	5.15	5.31	5.29
1996	6.07	6.34	6.21
1997	6.36	6.50	6.48
1998	6.43	6.34	6.52
1999	6.13	6.03	6.16
2000	6.92	6.57	6.90
2001	5.41	5.04	5.33
2002	5.77	5.33	5.60
2003	5.16	4.79	4.97
2004	4.59	4.37	4.39
2005	3.48	3.31	3.28
2006	2.47	2.38	2.30
2007	1.37	1.33	1.26

2008	0.28	0.34	0.26
2009	0.00	0.00	0.00
N (number of observations)	**140,987**	**92,311**	**157,563**

Source : Authors, based on DHS data

Appendix 10: Cross-country summary statistics based on Sample A0

Country	Obs.	Percentage	DHS waves	Percentage of children not enrolled to school	Number of children	Completed years of education	Exploitable data on child labor
Benin	8,839	6.27	1996	45.35	4.55	1.27	No
			2001	35.74	4.42	1.82	No
			2006	26.19	4.51	2.68	No
			2012	20.81	4.65	3.15	No
Burkina	5,629	3.99	1993	48.40	4.47	1.41	No
			2003	58.66	4.33	1.33	Yes
			2010	40.89	4.56	1.92	Yes
Burundi	2,271	1.61	2010	15.10	4.43	2.27	Yes
Cameroon	3,009	2.13	2004	15.62	4.36	2.09	No
			2011	11.48	4.52	3.10	Yes
Centrafrica	494	0.35	1995	37.45	4.35	1.02	No
Chad	1,172	0.83	2004	54.52	4.70	1.41	Yes
Comoros	1,207	0.86	1996	40.47	4.64	1.18	No
			2012	8.48	4.50	3.63	Yes
Congo	2,112	1.50	2005	6.25	3.84	3.26	Yes
			2012	4.26	4.08	3.17	Yes
Congo Dem.	6,610	4.68	2007	18.82	4.57	2.30	Yes
			2014	9.07	4.72	2.68	Yes
Cote d'Ivoire	1,002	0.71	2005	41.02	3.87	2.10	No
			2012	24.92	4.18	2.50	Yes
Ethiopia	7,642	5.42	2000	65.34	4.06	0.58	No
			2005	58.25	4.20	0.78	No
			2011	27.77	4.35	1.82	Yes
Gabon	849	0.60	2000	4.64	4.21	2.56	No
			2012	5.62	4.20	3.36	Yes
Gambia	1,097	0.78	2013	32.97	4.76	2.32	No
Ghana	3,525	2.50	1993	10.09	3.82	2.59	No
			1998	26.16	4.03	2.11	No
			2003	31.00	4.04	1.79	No
			2008	24.93	4.10	2.97	No
			2014	18.04	4.00	3.17	No
Guinea	3,103	2.20	1999	54.63	4.43	1.08	No
			2005	49.73	4.24	1.06	No
			2012	39.30	4.47	1.96	Yes
Kenya	7,430	5.27	1993	16.75	4.39	2.14	No
			1998	6.66	4.16	2.48	No
			2003	27.92	4.05	1.98	No
			2014	16.89	4.24	3.33	No
Lesotho	1,604	1.14	2004	6.27	3.67	3.12	No
			2009	4.68	3.69	3.02	No

Liberia	1,763	1.25	2007	51.81	4.01	1.37	Yes
			2013	51.24	4.09	1.06	No
Madagascar	6,605	4.69	1992	21.48	4.27	0.13	No
			1997	22.28	4.15	1.54	No
			2004	10.00	3.92	2.22	No
			2009	12.01	4.21	2.55	No
Malawi	10,552	7.48	1992	21.45	4.18	1.91	No
			2000	13.00	3.97	1.68	Yes
			2004	9.58	4.12	2.30	Yes
			2010	6.71	4.13	2.73	No
Mali	6,662	4.73	1996	68.59	4.62	0.77	No
			2006	52.36	4.31	1.12	Yes
			2013	40.44	4.71	2.19	Yes
Mozambique	5,243	3.72	1997	27.86	3.93	1.27	No
			2003	24.15	4.24	1.39	No
			2011	15.78	4.20	2.51	No
Namibia	1,435	1.02	1992	12.72	3.97	2.16	No
			2000	25.08	3.79	2.59	No
			2007	14.01	3.78	3.72	No
			2013	11.26	3.64	3.58	No
Niger	4,806	3.41	1998	61.93	4.32	1.09	No
			2006	53.43	4.45	1.11	Yes
			2012	47.96	4.92	1.64	Yes
Nigeria	16,100	11.42	1990	39.06	4.49	1.81	No
			2003	31.36	4.35	1.86	No
			2008	32.82	4.50	2.61	No
			2013	29.52	4.58	2.83	No
Rwanda	6,013	4.27	2000	24.98	4.15	1.39	No
			2005	18.81	4.34	1.36	No
			2010	10.44	4.22	2.23	Yes
Sao Tome	563	0.40	2009	3.20	3.74	3.64	No
Senegal	3,954	2.80	1993	58.64	4.79	1.22	No
			2005	40.45	4.57	1.38	Yes
			2011	33.89	4.68	2.24	Yes
			2013	33.20	4.67	2.34	Yes
			2014	37.17	4.77	2.11	No
Sierra Leone	3,099	2.20	2008	24.89	4.04	2.43	Yes
			2013	19.34	4.03	2.99	Yes
South Africa	629	0.45	1998	7.31	3.60	3.92	No
Swaziland	349	0.25	2007	3.15	4.12	4.10	No
Tanzania	5,461	3.87	1992	49.15	4.27	1.34	No
			1996	51.10	4.18	0.98	No
			2005	26.64	4.42	2.16	No
			2010	17.64	4.42	2.95	No
Togo	2,451	1.74	1998	26.88	4.61	1.42	No
			2014	9.95	4.40	3.28	Yes

Uganda	4,394	3.12	2001	15.92	4.43	1.89	Yes
			2006	15.94	4.64	2.06	No
			2011	19.37	4.72	2.02	No
Zimbabwe	3,324	2.36	1994	7.95	4.02	2.81	No
			1999	10.63	3.79	2.78	No
			2006	11.31	3.82	3.74	No
			2011	5.18	3.65	4.19	No
All Together	**140,987**	**100.00**	**93**	**25.92**	**4.35**	**2.28**	

Source : Authors, based on DHS data