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Abstract

Does when a child was born relative to his or her siblings affect whether the child attends school or participates in child labour? We investigate this question by estimating the causal effect of birth order on the probabilities of school attendance and child labour participation. To address the potential endogeneity of family size, we use instrumental variable approach where the proportion of boys in the family is used to instrument family size. Using a longitudinal household survey data from Ethiopia, we estimate unobserved effects bivariate probit instrumental variable model of school attendance and child labour choices. The results suggest that the probability of child labour participation decreases with birth order, but we find no evidence that suggests birth order affects the probability of school attendance. However, among children who are going to school, hours spent studying increases with birth order. Results from complementary time-use analysis reveal that there is no birth order effect on hours spent on household chore. However, hours spent on school increases with birth order, where the increase in hours spent on school seems to come from a decrease in hours spent on market work.

Keywords: child education, child labour, birth order, unobserved effects instrumental variables model, Ethiopia
JEL: J13, I25, I24, O10

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1. Introduction

While it may be relatively easier to understand why two randomly chosen unrelated individuals may differ in their educational achievements, it is not clear why siblings who grew up in the same family and shared the same community background have different educational achievement. Studies that attempt to decompose the sources of economic inequalities into between and within families differences show that there is a considerable variation in the educational achievement and other important economic aspects of siblings.

In the US, for instance, the variance in the permanent component of siblings' log earnings is estimated to be somewhere around 40% (see, Solon, 1999, for a review of the literature on siblings correlation). This suggests that 40% of earning inequalities are attributed to shared family and community background such as neighbourhood and school qualities, while the remaining 60% is due to factors which are not shared by siblings, including, but not limited to, genetic traits, gender, birth order and sibling-specific parenting.

Studies from developing countries also arrived at a more or less similar conclusion. For instance, within families difference account for about 37% of the total variances in completion of elementary school in rural Albania (Picard and Wolff, 2010). Similarly, a simple variance analysis shows that only about half of the total variation in completed education in Laguna Province, Philippines is explained by between families difference (Ejrnaes and Pörtner, 2004).

A potential explanation for differences in educational outcomes of siblings and their labour market earnings later in life is the role of parental action. Even parents who are equally concerned about their children may invest more in the education of the more endowed child and compensate the less endowed one by leaving more bequests (Becker and Tomes, 1976). In low-income countries, however, poor parents do not have the resource to make such compensation, but they create a sizable difference in the educational achievement of siblings, primarily through specializing some of their children for child labour and the others for school (Horowitz and Wang, 2004).

The widespread practice of child labour in developing countries[2] partly ex-

[2]The report from International Labour Organisation reveals that there were 153 million child labourers in the world in 2008 (Diallo, 2010). In Ethiopia, the country which is also the focus of the present study, data from the 2011 Ethiopian Demographic and Health Survey have showed that about 27% of children between the age of 5 and 14 involved in child labour (EDHS, 2011).

plains differences in the educational achievements of children in developing countries. One important feature of child labour in many developing countries is that it is not a full-time activity. Rather, children participate in less-intensive child labour such as helping their mothers in household chores or their fathers on family farm for few hours per day, leaving the children with few more hours either to attend school or remain idle (see Basu, 1999, for a survey of the literature on child labour). Siblings in a given family also do not necessarily participate in equally demanding work; some may work full time, others work on a part-time basis, and some others do not work at all. Parents allocate children's time between school attendance and child labour based on siblings' comparative advantage in these two activities (Edmonds, 2006), which in turn depends on a number of child attributes such as birth order, health, ability, age and gender.

In this article, we investigate the effect of birth order on the probabilities of school attendance and participation in child labour. Since parents jointly allocate the child's time between these two activities, we estimate a bivariate probit regression model. The bivariate probit model consists of two equations: the first equation contains the school attendance probability, and the second one is the probability of participating in child labour. The bivariate probit model is estimated using longitudinal household survey data from Ethiopia. Unlike most studies from low-income countries, the longitudinal data used in this article report the actual number of hours children spend on different activities. This reduces bias from measurement error relative to using data that only have binary indicators for child labour, school attendance and other activities.

Since we observe how children allocate their time between various activities, we also provide complementary time-use analysis by estimating a system of three equations consisting of hours spent on school, market work and household chore. We employ both seemingly unrelated regression equation (SURE) and fixed-effect three-stage least-squares (3SLS) instrumental variable (IV) regressions that take into account the correlations among time-use equations, endogeneity of family size and unobserved heterogeneity.

The role of birth order in children's outcome is widely documented in the literature. In developed countries, the vast majority of these studies conclude that first-born children have better outcomes in a number of aspects including educational achievement and labour market earnings (e.g. see Black *et al.*, 2005). In low-income countries, on the contrary, most studies suggest that later-born children achieve more years of schooling (e.g. see Emerson and Souza, 2008). Most of the birth order studies, particularly those that use data from low-income countries, however, did not convincingly treat endogeneity of family size. This is a serious problem as high birth order children are observed only in large families. For instance, a fifth child is observed only in families with at least five children. If parents who choose to have more kids are inherently different and children in these families have worse outcome regardless of family size and birth order, then the coefficient estimate of birth order is biased.

Endogeneity of family size can be mitigated by finding appropriate IV for family size and estimating IV models. In this article, we attempt to mitigate endogeneity of family size by exploiting the fact that Ethiopian parents prefer boys to girls to construct an IV for family size. Specifically, the proportion of boys in the family is used to instrument family size, and then unobserved effect bivariate probit IV model of child labour and schooling choices are estimated.

Overall, the results reveal that an increase in birth order by one unit decreases the probability of child labour participation by 5 percentage points, whereas it has no effect whether the child attends school or not. Similar patterns in birth order effects emerge using ordered outcomes models; we find negative and significant birth order effect of time spent on child labour, but insignificant positive effect on time spent in school. However, among children who are going to school, a one-unit increase in birth order increases the time the child spends studying by 1.9 hours per day. Since eight child age dummies are included to control for the age of the child, it is not age difference that is driving the results. A comparison of estimates from unobserved effect bivariate probit model and unobserved effect bivariate probit IV model suggests that endogeneity of family size potentially bias birth order estimates in school attendance regressions, but not in child labour regressions.

Results from time-use analysis, on the other hand, suggest that younger students spend more hours on school, probably because younger kids, who are less likely to participate in child labour, skip classes less frequently relative to their older siblings. The increase in hours spent on school among younger kids seems to come from the decrease in hours spent on market work. On the contrary, there is no statistical difference on hours spent on household chore by birth order.

The remainder of the article is organized as follows. The next section reviews the relevant literature, and Section 3 describes the data. Section 4 discusses the methodology, outlines the empirical approach and presents the first-stage estimates. The main results are reported in Section 5, and the last section concludes.

2. Background

At first glance, it may seem that when a child is born relative to his or her siblings does not matter at all. But there are a number of reasons why we expect children's outcome to vary by birth order. First, children of different birth order face a different household environment. For example, household size and the intellectual environment in the household differ by birth order (Zajonc, 1976). Second, credit constraint induces birth order effects. If parental income increases over their life time, later-born children reside in relatively richer families. Credit constraint also interacts with child labour: credit-constrained families supplement the family income with income from child labour and this may involve sending the most productive child to work. If, say, earlier-born children are more productive, then we expect them to spend more time working. Third, birth order effects can be a result of parents' preferences. In communities, for instance, where children are considered as security for old age, parents may favour earlier-born children as they become economically independent earlier (Horton, 1988). Fourth, later-born children are biologically disadvantaged as they are born with older mothers who are more likely to give low-birth-weight babies.

The literature that links birth order with children's outcome is well developed. Studies from developed countries have documented that first-born children achieve more years of education, earn more, are more likely to attend private schools, are less likely to held back in school, are more likely to have full-time employment, and, for girls, are less likely to give birth while teenagers (Conley and Glauber, 2006; Booth and Kee, 2008; Gary-Bobo *et al.*, 2006; Iacovou, 2001; Black *et al.*, 2005). On the other hand, studies that use data from low-income countries tell a different story: later-born children complete more years of schooling and are less likely to participate in child labour (Ejrnaes and Pörtner, 2004; Emerson and Souza, 2008; Edmonds, 2006).

The wealth model (Becker, 1991; Ejrnaes and Pörtner, 2004) suggests that parents invest in the child's human capital until the marginal return to education equals the market rate of return. In developing countries, where child labour is widely practised and parents are too poor to send all their children to school at the same time, this may mean that parents send some of their children to school and the others to work.[3] How the child's time is allocated between school and child labour is an empirical one, but Baland and Robinson (2000), Edmonds (2006) and Emerson and Souza (2008) argue that it is based on the child's comparative advantage in school and child labour, which, in turn, depends on the child's endowment. Ejrnaes and Pörtner (2004) explicitly consider birth order as one type of endowment and show that birth order affects investment in children even without assuming parental preference for specific birth order children and genetic endowments vary by birth order.

On methodological side, endogeneity of family size is one of the empirical challenges of birth order studies. Obviously, high birth order children are observed in relatively larger families, and larger families may be inherently different and children in these families would have worse outcome regardless of family size and birth order. Thus, it is crucial to address the endogeneity of family size. One possible solution is to estimate separate outcome equation by restricting the sample to each observed family size in the data. Generally speaking, this is not practical since most surveys to date have small number of observations to allow precise estimate by family size. However, Black *et al.* (2005) could do so using a unique data set on the *entire* population of Norway.

A more common and practical approach is to look for exogenous variation in family size and estimate IV model. The occurrence of twin births and siblings sex composition are the two widely used IVs. Twinning is historically the most popular one; recently, however, following Angrist and Evans (1998), use of siblings' sex composition is also increasing in the literature. This may be partly because using twin births as IV demands large data sets since twin births occur rarely.

The basic idea in using siblings sex composition as exogenous variation in family size is that parents in a two-child family prefer to have mixed-sex children (a girl and a boy) to same-sex children (two boys or two girls). Hence, families with same-sex siblings in the first two births are more likely to have an additional child. The data from developed countries support this argument, and a number of researchers have used it to instrument family size. Angrist and Evans (1998) are the first to use siblings' sex composition as exogenous variation in family size in their

[3] It is important to note that parents send their kids to work not because parents are selfish; it is because, for poor families, sending their kids to work is crucial for the households' survival (Basu and Van, 1998).

study of the causal effect of family size on the labour supply of mothers in the US. Following Angrist and Evans (1998), a number of birth order studies in developed countries use siblings sex composition to instrument family size in their attempt to estimate the causal effect of birth order on children's outcome (Conley and Glauber, 2006; Black *et al.*, 2005; de Haan, 2010).[4]

Unfortunately, birth order studies that use data from developing countries have not yet convincingly disentangled the effect of family size and birth order. Thus, it is not clear whether the documented birth order effect on children's outcome is causal. This could be partly due to data limitation. Besides, families in developing countries are early in their fertility transition with high fertility rate which makes it unreasonable to consider twin births as major shocks in family size. Angrist *et al.* (2010) employ both the occurrence of twin births and siblings sex composition to instrument family size in their study of quality-quantity trade-off among children in Israel, a country that somehow falls between developed and developing countries with respect to its fertility rate. They also exploit preference for boys by traditional Israeli families to instrument family size, and they find out that, among Asian and African Jew families in Israel that have mixed-sex siblings in the first two births, having a boy in the third birth decreases the probability of having an additional child, implying parents prefer boys to girls.

The empirical strategy of this article is motivated by Angrist *et al.* (2010), but innovates upon their approach. Note that use of twin births as instrument is appropriate in countries where fertility rate is too low to consider twin births as sizable exogenous shocks in family size. The common limitation of instrumenting family size by twin births and gender of the first two births is that they only allow estimating the marginal effect of the third child. This makes them less applicable in societies where fertility rates are either extremely low as in China and South Korea or high as in many developing countries. That is why studies from countries with relatively lower fertility rates and with strong preference for boys over girls tend to use gender of first birth to instrument family size as this would allow estimating the marginal effect of the second child (e.g. see Lee (2008) and Kugler and Kumar (2014) for studies that instrument family size by gender of the first child).

However, for countries like Ethiopia where fertility rate is high (even by Sub-

[4]Goux and Maurin (2005) also employ similar instrumental variable for family size when they assess the effect of overcrowded housing on children's performance at school.

Saharan Africa standard) and also where preference for boys over girls is widely observed,[5]use of siblings' sex composition as exogenous source of variation in family size is more appropriate.

This article, thus, contributes to the literature in two important ways. First, it extends use of siblings' sex composition as instrument for family size to a typical low-income country. Second, it attempts to document the causal effect of birth order on schooling and child labour. We are not aware of similar studies done on Ethiopia and much of Sub-Saharan Africa that convincingly disentangle the effect of family size and birth order.

3. Data

We use longitudinal household survey data from Ethiopia which was administered by Young Lives, an international research project based in the University of Oxford. As part of the project, data on children from four low-income countries – Ethiopia, India (in the Andhra Pradesh state), Peru and Vietnam – have been collected. During the first survey round of data collection of 2002, 2000 one-year-old children (hereafter 'younger' cohort) and 1000 eight-year-old children (hereafter 'older' cohort) were surveyed in each country. In a follow-up survey conducted in 2006 and 2009, the same children were tracked and surveyed when the 'younger' cohort children turned to 5 and 8 years old, and the 'older' cohort children turned to 12 and 15 years old, respectively. We specifically use the Ethiopian part of the data from the 2006 and 2009 survey rounds of 'older' cohort children. Data from the 'younger' cohort surveys are not used in the analysis as most of the children in this cohort were too young (around 8 years old) to go to school at the time of the most recent survey.[6]

In the Ethiopian part of the survey, children were randomly sampled from 20 semi-purposively selected sentinel sites in the five largest regions of the country (see Wilson *et al.*, 2006, for a discussion on the sampling design). In 2006 and 2009 survey rounds, eight activities were identified and the number of hours children between the age of 5 and 17 years spent on each of these activities in the last week is reported.

[5]Given the history of war and less-developed police force, particularly in rural areas, Short and Kiros (2002) argue that bravery and physical strength are highly valued in Ethiopian families. Since men supposedly have these essential features, Ethiopian parents prefer boys to girls.

[6]Though the legal school starting age is seven in Ethiopia, it is not uncommon for most children in developing countries like Ethiopia to delay primary school enrolment by few years beyond the legal school starting age (Barro and Lee, 2000).

This enables us to observe how children spend their time more accurately. Though information on time use was collected on children between the age of 5 and 17 years, only children between the age of 7 and 15 years are included in the analysis. Children below 7 and above 15 years old are excluded, respectively, because compulsory school starting age in Ethiopia is 7 years and the International Labour Organisation's (ILO's) Convention No. 138 specifies 15 years as the age above which a person may participate in economic activity. We further restrict the original sample of households to those with at least two resident children between the age of 7 and 15 at the time of the surveys.[7] This leaves us with the final sample size of 1866 children.

The two dependent variables used in estimation are binary indicators for school attendance and child labour participation, where school attendance is 1 if the child attends school, and 0 otherwise. Similarly, child labour participation takes a value of 1 if the child spends more than 14 hours per week on activities such as household chores, and 0 otherwise.[8] Table 1 provides marginal and joint (cell) frequencies for school attendance and child labour. As mentioned earlier, this table confirms that child labour in Ethiopia is not a full-time activity for most children. Rather, children work for few hours per day, leaving the children with few more hours either to attend school or remain idle. Table 1, for instance, shows that the majority of the children (69.7%) in the sample do both, i.e. attend school and participate in child labour, while only 1.3% of the children remain idle.

Though child labour is common in Ethiopia, it is important to note that working for pay is not that common. In our sample, only 8% of children work for pay. The rest are involved in domestic work such as cooking (48%), caring for their younger siblings and/or ill household members (38%), and participating in unpaid family work such as cattle herding (7%). There is also child labour specialization by gender where girls tend to specialize in domestic work and caring for others while boys specialize in unpaid work (see Table A.1 for a summary of child labour specialization by gender). Haile and Haile (2012) also find out child labour specialization in rural Ethiopia where girls are more likely to participate in domestic chores while boys participate in market work.

[7] Since we are interested in exploring birth order effect, it is crucial to observe at least two kids in a given household. In countries like Ethiopia where parents have an average of five kids, this type of sample restriction does not create a serious selection issue.

[8] The 14 hours per week cut-off is chosen to be in line with ILO's definition of 'light work' which is working for 14 hours per week or less.

Table 1: Marginal and joint percentage distributions for school attendance and child labour (N = 1866)

School attendance	**Child labour** No %	Yes %	**Total** %
No	1.3	8.8	10.1
Yes	20.1	69.7	89.9
Total	21.4	78.6	100.0

Source: Authors' calculation based on household survey data from Young Lives.

Birth order, the primary independent variable of interest, is constructed as a variable containing the birth order of (resident) children as 1, 2, 3, 4, etc. Thus, the estimate of the marginal effect of birth order tells us the approximate change in the probability of school attendance or child labour participation for one-unit increase in birth order. The average birth order in the sample is approximately three which is expected given the average number of kids in the family is about five (see Table 2). The proportions of children attending school and participating in child labour vary by birth order. Generally speaking, the probabilities of school attendance and participation in child labour decrease with birth order (see Figure 1). This is expected in nonadjusted relationship between birth order and school attendance/child labour as age decreases with birth order, and it is less likely for younger kids either to attend school or participate in child labour.

Table 2 presents the summary statistics of variables employed in the regression analysis. Generally speaking, parental years of schooling, which controls for the socio-economic status of the family, shows that parents in the sample are less educated, with father's and mother's years of schooling of four and two, respectively. A female dummy takes on a value of 1 if the child is a girl and 0 otherwise. Likewise, urban dummy assumes a value 1 if the place of residence is urban, and 0 if rural. A binary indicator for housemaid is also included as control variable since the presence of a housemaid may reduce the child's labour obligation at home. In addition, we control for annual family expenditure, which is a good proxy for permanent family income. Table 2 also presents the proportion of girls in the sample and children who live in urban area. Finally, 19 village dummies are also included as additional control variables in the regression analysis.

Table 2: Means and SDs of explanatory variables

	2006	2009
Birth order	3.445	2.978
	(1.62)	(1.42)
Number of kids	5.327	5.207
	(1.72)	(1.71)
Proportion of boys in the HH	0.511	0.506
	(0.22)	(0.22)
Support on family planning (yes=1)	0.084	0.118
	(0.28)	(0.10)
Child age (in years)	10.123	13.104
	(1.74)	(1.76)
Female dummy (yes=1)	0.474	0.474
	(0.50)	(0.50)
Housemaid dummy (yes=1)	0.060	0.080
	(0.24)	(0.27)
Father's schooling	3.860	3.881
	(4.04)	(4.03)
Mother's schooling	2.282	2.284
	(3.44)	(3.44)
Household expenditure	0.978	1.784
	(0.74)	(1.20)
Urban dummy (yes=1)	0.303	0.311
	(0.46)	(0.46)
Observations	933	933

Notes: SDs in parentheses

We control for village effects as well as year effects in regressions. Summary statistics for other variables are provided under Results section, when robustness checks and alternative specifications are considered.

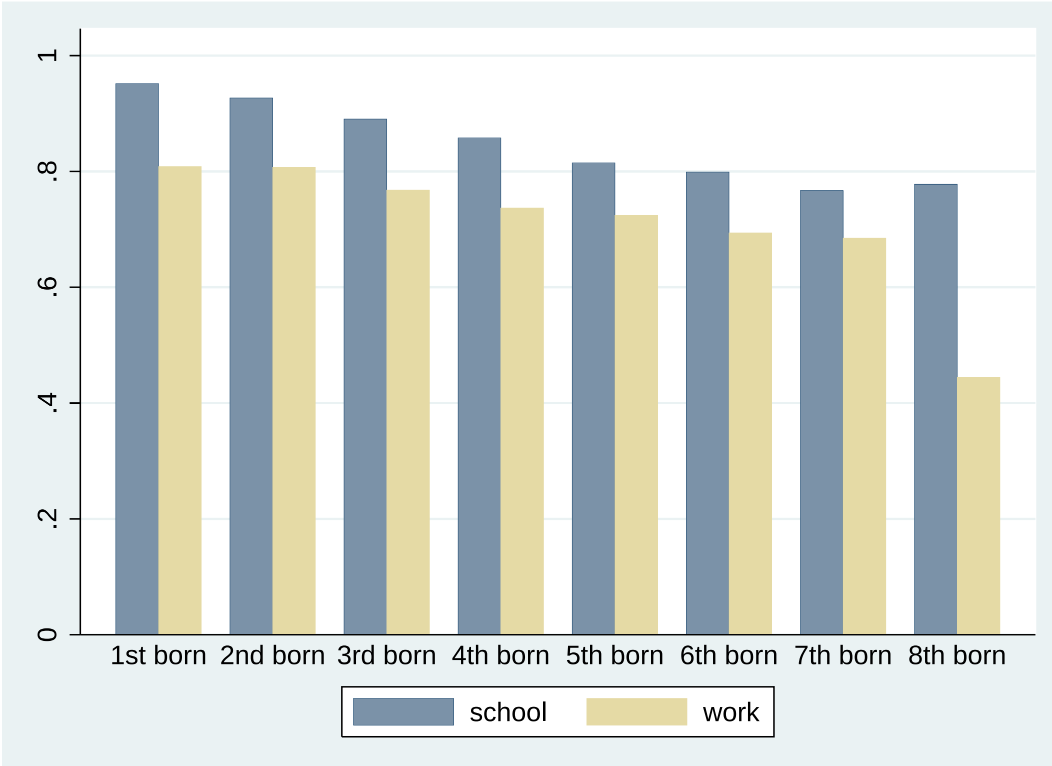

Figure 1: Fraction of children who attend school and work by birth order

4. Empirical Methodology, Identification and First-Stage Estimates

4.1. Empirical methodology

Our main goal is to estimate the causal effect of birth order on children's time allocation. It is assumed that parents are responsible to allocate children's time between schooling and child labour, and parental utility differs by alternative allocations. Since parents jointly allocate the child's time between child labour and school attendance, unobserved effect bivariate probit model is estimated using maximum likelihood procedure. The bivariate probit model consists of two equations: school attendance (s_{it}) and child labour (l_{it}) equations. Define the latent parental utility from allocating child $i's$ time on school and child labour in year t, respectively, by

$$s^*_{it} = \delta_s b\_order_{it} + \gamma_s family\_size_{it} + \mathbf{X}_{it}\boldsymbol{\beta}_s + \alpha_{is} + \epsilon_{its} \quad (1)$$

$$l^*_{it} = \delta_l b\_order_{it} + \gamma_l family\_size_{it} + \mathbf{X}_{it}\boldsymbol{\beta}_l + \alpha_{il} + \epsilon_{itl}, \quad (2)$$

where s_{it} and l_{it} are the corresponding observed dependent variables such that $s_{it} = 1[s^*_{it} > 0]$ and $l_{it} = 1[l^*_{it} > 0]$, where $1[.]$ is an indicator function and is unity whenever the statement in brackets is true, and zero otherwise. Here, b_order_{it} represents the birth order of child i in year t, $family\_size_{it}$ denotes the number of children in child $i's$ household in year t and $\mathbf{X}_{it}$ is a vector of observable control variables including a constant. $\alpha_i = \{\alpha_{is}, \alpha_{il}\}$ are random variables representing time-invariant unobserved individual heterogeneity and $\epsilon_{it} = \{\epsilon_{its}, \epsilon_{itl}\}$ are the idiosyncratic error terms. Assume that ϵ_{it} are jointly and normally distributed each with mean zero and variance one, and correlation ρ. If the error terms ϵ_{its} and ϵ_{itl} are uncorrelated, i.e. $\rho = 0$, the two equations can be estimated separately using unobserved effects probit model. For further details about random-effects bivariate probit, see Cameron and Trivedi (2005).

We are primarily interested in estimating δ_s and δ_l, the coefficient estimates of birth order in school attendance and child labour equations in 1 and 2. However, as mentioned earlier, the birth order coefficients may pick up the effect of family size on the outcome variables as family size is endogenous in Equations 1 and 2. A potential source of endogeneity in our case arises from the fact that high birth order children are observed only in larger families. For instance, a fifth child is observed only in families with at least five children. Endogeneity of family size can be mitigated by finding appropriate IV for family size and estimating IV models.

The reduced form equation for $family\_size$ takes the form

$$family\_size_{it} = \eta_0 + \mathbf{Z}_{it}\boldsymbol{\eta}_1 + \eta_2 b\_order_{it} + \mathbf{X}_{it}\boldsymbol{\eta}_3 + \psi_i + \mu_{it}, \tag{3}$$

where $\mathbf{Z}_{it}$ is a vector of identifying instruments for family size, μ_{it} is the idiosyncratic error term and ψ_i is time-invariant unobserved individual heterogeneity.

In implementation, we use children's sex composition as an instrument for family size. The argument is that if parents prefer to have mixed gender children (i.e. boys and girls) to same gender children (i.e. all boys or all girls), then siblings' sex composition is correlated with the number of kids parents have. In the US, for instance, parents in a two-child family are more likely to bear an additional child if they have the same-sex children (two boys or two girls) than those who have mixed-sex children (a boy and a girl) (e.g. see Angrist and Evans, 1998; Price, 2008).

In developing countries, high fertility rate and parents' preference for boys to girls provide additional dimensions to the preference for mixed gender children. Many studies from developing countries, including Ethiopia, have documented the presence of strong sons preference (e.g. see Angrist *et al.*, 2010; Short and Kiros, 2002). If parents have preference for boys to girls, then the proportion of boys in the household affects parents' fertility decision; that is, the higher the proportion of boys, the lower the probability for parents to bear an additional child, and hence they will end up with a relatively smaller family size.

To fix ideas, consider the case where parents care only about having two sons. If parents are 'lucky' enough to give birth to two boys in their first two births, then we expect them to stop child bearing, and hence the proportion of boys in this family is 100%. If, on the other hand, they are not that 'lucky' and have to wait until, say, the tenth birth to give birth to the second boy, then the two boys account for 20% of the children for this family. Obviously, the example is a bit extreme where parents are considered as if they only care about having two sons, but it demonstrates the possibility for a negative relationship between the proportion of boys and the number of children in the family in the presence of sons preference.[9]

The negative correlation between the proportion of boys and family size can be exploited to disentangle the effect of birth order and family size on children's outcome - i.e. school attendance and child labour participation - as long as the proportion of boys in the household does not affect children's outcome, except indirectly through its effect on family size.[10]

Our basic model consists of unobserved effects bivariate probit Equations 1 and 2 for binary responses and a linear equation for IV as specified in Equation 3. Due to nonlinearity of the unobserved effects bivariate probit model, the usual approach of using predicted values of the endogenous variable (family size) in the second stage would not provide consistent estimates. Instead, we use the Terza *et al.* (2008) two-stage residual inclusion (2SRI) procedure, which is shown to be consistent for a general class of nonlinear models such as the model used in this article. Basically, Equations 1 and 2 are estimated, where the observed family size is not replaced by its predicted value, but instead the predicted residual from Equation 3 is included as

[9] Some argue (e.g. Williamson, 1976) that the relationship between the proportion of boys and family size holds if parents have a taste for small or moderate family size since in large families a mix of both genders is more likely to happen due to mere biological probability. This argument is valid if parents care only about having at least one child of each gender. However, if parents prefer a specific proportion of boys - say, more boys than girls - then preference for sons affect fertility even if parents have a taste for larger family.

[10] By construction, family size appears on both sides of Equation 3: as a dependent variable and a denominator of the excluded variable, proportion of boys in the household. Generally, this could lead to a well-known bias in labour economics called Borjas' division bias (Borjas, 1980) if there is measurement error in family size. As in most household survey data, measurement error in family size is not a serious problem in our data to make Borjas' division bias a serious concern.

an additional control variable in estimation of unobserved effects bivariate probit.

We use marginal effects obtained from Equations 1 and 2 to estimate and interpret the birth order effects. The average marginal effects (AMEs) of birth order on likelihoods of school attendance and child labour are estimated by averaging the underlying partial effects over the distributions of the explanatory variables and the unobserved effects, evaluated at the maximum likelihood estimates of the unknown parameters.

4.2. First-stage IV results

As discussed above, we expect a negative relationship between the proportion of boys and the number of kids in the family in the presence of son preference, i.e. where parents prefer boys to girls. Table 3 presents the first-stage results that depict this relationship. The first two columns display results from OLS regressions while the last two columns display that of household fixed-effect regressions. Under both OLS and fixed-effect regressions, two equations are estimated: one with only one excluded instrument, proportion of boys, and the other with two excluded instruments, proportion of boys and an indicator variable whether a family received support on family planning either from government or nongovernment organizations. The latter is used to proxy family planning use, which we do not observe.

For son preference to affect the number of kids in the family, parents should be able to stop child bearing once they achieved the desired gender mix. That is why controlling for family planning use is important in the first-stage regressions. Admittedly, however, support on family planning may not be a good proxy for use of family planning since *access* does not necessarily guarantee *use*. Moreover, the support could target some group of the population, say poor or high fertility households, and this may create selection bias. Given information on family planning use is not collected and considering part of the problem is mitigated by estimating a fixed-effect model that accounts for individual heterogeneity, support on family planning is used as a proxy for family planning use, and hence as an additional excluded instrument (in columns 2 and 4 of Table 3) to see whether results are sensitive to controlling family planning use.

In the OLS regressions, the coefficient estimates of the proportion of boys in the family are insignificant in both specifications. On the contrary, it is negative and significant in the fixed-effect regressions. The coefficient estimate of the proportion of boys in the family is about -2.5 in the fixed-effect regressions, implying parents that

Table 3: First-stage regression results from linear models (Dependent variable: number of kids)

	Pooled OLS		Fixed effect	
	Reduced IV	Full IV	Reduced IV	Full IV
Proportion of boys in the family	-0.235	-0.217	-2.463**	-2.483**
	(0.19)	(0.19)	(0.98)	(0.99)
Support on family planning (yes=1)		0.356***		0.044
		(0.09)		(0.07)
Birth order	0.712***	0.715***	1.014***	1.014***
	(0.02)	(0.02)	(0.03)	(0.03)
Female dummy (yes=1)	0.053	0.054		
	(0.08)	(0.08)		
Housemaid dummy (yes=1)	0.618***	0.631***	0.734***	0.733***
	(0.18)	(0.18)	(0.24)	(0.24)
Father's schooling	0.039***	0.040***	-0.026	-0.022
	(0.01)	(0.01)	(0.05)	(0.05)
Mother's schooling	-0.061***	-0.060***	-0.237	-0.236
	(0.02)	(0.02)	(0.16)	(0.16)
Household expenditure	0.243***	0.244***	0.030	0.030
	(0.05)	(0.05)	(0.03)	(0.03)
Urban dummy (yes=1)	0.251	0.222	0.257	0.282
	(0.24)	(0.23)	(0.31)	(0.30)
Constant	1.260***	1.244***	3.391***	3.368***
	(0.35)	(0.35)	(0.75)	(0.76)
Child age Dummies	Yes	Yes	Yes	Yes
Village Dummies	Yes	Yes	Yes	Yes
Observations	1864	1864	1864	1864
R^2	0.518	0.522	0.636	0.636

Notes: SEs in parentheses. *p < 0.10; ** p < 0.05; *** p < 0.01.

Time-constant variables such as female dummy are dropped for fixed-effects specifications.

The two IVs presented in columns 2 and 4 are jointly significant at 5% level.

have sons only have 2.5 fewer children than those that have daughters only.[11] This suggests parents prefer sons to daughters. The fact that the coefficient estimates of the proportion of boys in the fixed-effect regressions are negative and significant unlike that of in the OLS regressions suggests the presence of individual heterogeneity in son preference. Though the proxy variable for family planning use, support on family planning, is significant in the OLS regressions, it is insignificant in the fixed-effect regressions. Moreover, in the fixed-effect regressions, the coefficient estimate of the proportion of boys remains the same whether we control for family planning use or not. Thus, the predicted residuals from the fixed-effect regression which include proportion of boys as the only excluded instrument (column 3 of Table 3) are saved and used as additional control variable in the second-stage regressions in Section 5.

4.3. Validity of the instrument

Do Ethiopian parents prefer sons to daughters?

The argument that parents in traditional societies like Ethiopia prefer sons to daughters has been made in the previous sections. In this subsection, we want to document whether data from Ethiopia support this argument. Following Dahl and Moretti (2008), we have presented the probability of parental divorce by gender of first-born child (see Table 4). If there is preference for sons, we expect that having a girl in the first birth increases the probability of parental divorce (relative to that of having a boy in the first birth). Since divorce rate is low in Ethiopia, small household survey data such as Young Lives survey data are not well suited to assess divorce rate by gender of first-born child. Thus, we employ the 2% public-use microdata samples from the 2007 Ethiopian population census. As can be seen from Table 4, having a girl in the first birth increases the probability of parental divorce by 0.7 percentage points, and the difference is significant at 1%. This supports the argument that Ethiopian parents prefer sons to daughters.

The results from Table 4 imply that parents that have a second child are a 'selected' group, i.e. those that remained together (after having had the first child) have a relatively weaker preference for son. Since we have restricted our sample to parents with at least two kids, differential divorce rate by gender of first-born child

[11]Ethiopia is characterized by high fertility rate, with, for example, more than five kids per woman in our sample. Given the high fertility rate and the presence of son preference, the magnitude of the coefficient estimate of the 'proportion of boys in the family' variable (i.e. having 2.5 fewer children) is not surprising.

could potentially bias our estimates downward. However, divorce rate in Ethiopia is too low[12] to cause a major concern of this type of selection bias in our data.

Table 4: Fraction of parents divorced by gender of first-born child

	Gender of first-born			
	Boy	Girl	Mean difference	p-value
Divorced	0.041	0.047	-0.007	0.000
Observations	66508	58942		

Source: Authors' calculation based on the 2% public-use microdata samples from the 2007 Ethiopian population census.

Are boys better off?

One important feature of an IV is that it should not affect the dependent variable, except indirectly through the endogenous variable it is supposed to instrument. Thus, it is important to assess whether the proportion of boys in the household (i.e. the IV) directly affects participation in child labour and/or school attendance (i.e. the dependent variables). This assessment is crucial, but it is impossible to empirically test whether the correlation exists as it involves the error term in the second-stage equation.

Table 5 presents a simple check whether school attendance and/or participation in child labour systematically varies for boys by the number of sisters they have. If, say, boys who live with more sisters are more likely to attend school than those who live with fewer sisters, then we expect boys who live with more sisters to have a higher probability of school attendance, an indication of direct relationship between proportion of boys and school attendance. Table 5, however, suggests this is not the case in our data. In fact, it depicts that boys who live with more sisters are less likely to attend school (upper panel of Table 5) and more likely to work (lower panel of Table 5). However, the differences are not statistically significant.

Is there sex selective abortion?

If parents selectively abort female fetuses, then the proportion of boys in the household is endogenous, and hence not a valid instrument. However, sex determining technologies of fetuses such as ultrasound are not widely used in Ethiopia to cause

[12]Data from the 2011 Ethiopian Demographic and Health Survey show that divorce rate in Ethiopia is about 2.5% for men and 7.4% for women.

Table 5: Fraction of boys who attend school and work by number of sisters

	Mean	SD	N	p-value
School				
HHs with more daughters	0.806	0.396	506	
HHs with fewer daughters	0.824	0.381	721	
Mean difference	-0.018			0.435
Work				
HHs with more daughters	0.903	0.296	506	
HHs with fewer daughters	0.875	0.331	720	
Mean difference	0.028			0.126

a serious concern, but a simple check on birth spacing is conducted to see whether there is sex-selective abortion in the data. If parents selectively abort female fetuses, the birth spacing is expected to be higher for families with higher proportion of boys since the higher proportion of boys is partly driven by sex-selective abortion.

Table 6 compares birth spacing between consecutive children by proportion of boys in the household. The table shows that the average length between births is about 38 months regardless of the sex composition in the household, implying sex-selective abortion is not a serious concern in the data to make proportion of boys in the household an invalid instrument.

Table 6: Birth spacing (in months) by proportion of boys in the household

	Proportion of boys in the household		
	Less than half	At least half	Mean difference
Mean	37.88	37.79	0.090
SE	0.750	0.525	
Observations	1305	1832	
p-value			0.922

Is there differential mortality rate across gender?

If infant and child mortality rates are random across gender, then they do not affect the relationship between the proportion of boys and the number of kids in the household. However, if they systematically vary across gender, the observed gender mix in the household not only reflects parents' deliberate effort to achieve their desired gender mix but also the differential mortality rates across gender.

Since information on mortality rates is not recorded in the data, the presence of differential mortality rates (or their absence) cannot be empirically tested. If mortality rates are not random across gender, then results should be interpreted carefully. However, since fixed-effect model is estimated in the first-stage regression, even if mortality rates are nonrandom across gender, they do not render our IV invalid as long as they remain constant between the two survey years, 2006 and 2009.

5. Results

Various probit models are estimated to investigate the effect of birth order on the probabilities of school attendance and child labour. Focusing on key results, Table 7 provides estimates of the coefficients and ensuing AMEs of birth order on probabilities of schooling and work. The estimated models vary depending on whether it is assumed school attendance and child labour decisions are made jointly or independently (probit versus bivariate probit models), household heterogeneity is accounted for (pooled versus unobserved or random effect models) and endogeneity of family size is addressed (IV models versus the rest of the models). Since it is reasonable to assume that school attendance and child labour decisions are made jointly,[13] we primarily focus on discussing the results from bivariate probit models which are reported in the lower half of Table 7. The regression results for models reported in the last two rows of Table 7 are presented in Table 8.[14]

The birth order estimates in child labour equations are uniformly negative and significant across models (see Table 7), though their magnitudes differ. The coefficient estimates are particularly similar in unobserved effect bivariate probit and unobserved effect bivariate probit IV models (the last two models reported in Table 7), suggesting that endogeneity of family size is not a serious concern in estimating child labour equation. This is also confirmed by the insignificant coefficient estimate associated with the first-stage residual in the unobserved effect bivariate probit IV regression model, which is reported in Table 8.

In our preferred model which assumes school attendance and child labour decisions are made jointly and which accounts for endogeneity of family size (i.e.

[13]The coefficient estimate of ρ in the bivariate probit model is -0.111 and is statistically significant at 1% level, implying bivariate probit model is a better fit than univariate independent probit models.

[14]The compete regression results from the other models presented in Table 7 are available upon request or at the web link at http://www2.gsu.edu/ ecosgg/research/pdf/Seid&Gurmu_AE2015.pdf.

Table 7: Summary of estimates of coefficient and average marginal effect of birth order from different models

		School	Work
Independent probit models			
Pooled probit	Coeff.	-0.027	-0.186
	p-Value	(0.549)	(0.000)
	AME	-0.004	-0.037
	LL	-457	-667
Unobserved effect probit	Coeff.	-0.032	-0.193
	p-Value	(0.610)	(0.000)
	AME	-0.003	-0.036
	LL	-449	-666
Unobserved effect probit IV	Coeff.	0.389	-0.242
	p-Value	(0.040)	(0.013)
	AME	0.012	-0.020
	LL	-663	-1146
Bivariate probit models			
Pooled bivariate probit	Coeff.	-0.030	-0.188
	p-Value	(0.520)	(0.000)
	AME	-0.004	-0.038
	LL	-1119	–
Unobserved effect bivariate probit	Coeff.	-0.026	-0.252
	p-Value	(0.672)	(0.000)
	AME	-0.002	-0.049
	LL	-1168	–
Unobserved effect bivariate probit IV	Coeff.	0.151	-0.253
	p-Value	(0.124)	(0.000)
	AME	0.014	-0.049
	LL	-1167	–

Note: AME denotes the estimated average marginal effect of birth order on the probabilities of school attendance and child labour, while LL represents the maximized value of the log-likelihood function.

Table 8: Unobserved effect bivariate probit estimates of school attendance and child labour equations

	Bivariate probit model			Bivariate probit IV model		
	Coeff.	AME	SE	Coeff.	AME	SE
School attendance						
Birth order	-0.026	[-0.002]	(0.06)	0.151	[0.014]	(0.10)
Number of kids	-0.036	[-0.003]	(0.05)	-0.205**	[-0.019]	(0.09)
Child's age = 8	0.742***	[0.068]	(0.24)	0.768***	[0.070]	(0.24)
Child's age = 9	1.501***	[0.138]	(0.28)	1.447***	[0.132]	(0.28)
Child's age = 10	1.550***	[0.143]	(0.28)	1.508***	[0.138]	(0.28)
Child's age = 11	2.519***	[0.232]	(0.35)	2.514***	[0.230]	(0.35)
Child's age = 12	2.090***	[0.193]	(0.31)	2.018***	[0.185]	(0.31)
Child's age = 13	1.739***	[0.160]	(0.42)	1.658***	[0.152]	(0.42)
Child's age = 14	2.215***	[0.204]	(0.41)	2.180***	[0.200]	(0.41)
Child's age = 15	1.706***	[0.157]	(0.37)	1.589***	[0.145]	(0.37)
Female dummy (yes=1)	0.140	[0.013]	(0.13)	0.206	[0.019]	(0.13)
Father's schooling	0.066**	[0.006]	(0.03)	0.059**	[0.005]	(0.03)
Mother's schooling	-0.026	[-0.002]	(0.03)	-0.068*	[-0.006]	(0.04)
Household expenditure	0.034	[0.003]	(0.12)	0.022	[0.002]	(0.12)
Urban dummy (yes=1)	1.658***	[0.153]	(0.41)	1.781***	[0.163]	(0.43)
First-stage residual				0.191**	[0.018]	(0.10)
Child labour						
Birth order	-0.252***	[-0.049]	(0.05)	-0.253***	[-0.049]	(0.07)
Number of kids	0.151***	[0.029]	(0.04)	0.151**	[0.029]	(0.06)
Child's age = 8	0.471**	[0.091]	(0.21)	0.473**	[0.092]	(0.21)
Child's age = 9	0.629***	[0.122]	(0.22)	0.634***	[0.123]	(0.22)
Child's age = 10	0.588***	[0.114]	(0.22)	0.596***	[0.115]	(0.22)
Child's age = 11	0.862***	[0.167]	(0.20)	0.870***	[0.169]	(0.20)
Child's age = 12	0.903***	[0.175]	(0.20)	0.917***	[0.178]	(0.20)
Child's age = 13	0.837***	[0.162]	(0.31)	0.852***	[0.165]	(0.31)
Child's age = 14	0.814***	[0.158]	(0.26)	0.833***	[0.161]	(0.26)
Child's age = 15	0.679***	[0.131]	(0.26)	0.700***	[0.136]	(0.26)
Female dummy (yes=1)	0.176*	[0.034]	(0.09)	0.172*	[0.033]	(0.09)
Father's schooling	-0.021	[-0.004]	(0.02)	-0.020	[-0.004]	(0.02)
Mother's schooling	-0.003	[-0.000]	(0.02)	-0.000	[-0.000]	(0.02)
Household expenditure	-0.042	[-0.008]	(0.05)	-0.040	[-0.008]	(0.05)
Urban dummy (yes=1)	-0.703***	[-0.136]	(0.24)	-0.706***	[-0.137]	(0.24)
First-stage residual				-0.010	[-0.002]	(0.06)
Observations	1862			1860		
Log likelihood	-1168.373			-1167.288		

Notes: *p < 0.10; ** p < 0.05; *** p < 0.01.
Reported coefficients are average marginal effects.
Average marginal effects (AMEs) and SEs are reported in brackets and parentheses, respectively.
Village dummies, a year dummy and a dummy variable for the presence of housemaid are included as additional control variables.
In the bivariate probit IV model, the estimated correlation between the two individual effects, $Corr(\alpha_{is}, \alpha_{il})$, is -0.37, with variances of unobserved effects of 0.71 for school attendance and 0.33 for child labour.

unobserved effect bivariate probit IV model), the average marginal effect of birth order on the probability of child labour is -0.049. This suggests that a one-unit increase in the birth order of the child, on average, decreases the probability of participation in child labour by about 5 percentage points. The finding that later-born (i.e. younger) children are less likely to participate in child labour than their earlier-born siblings is consistent with prior findings in the literature (e.g. see Emerson and Souza, 2008; Edmonds, 2006).

Even if the results discussed above suggest the presence of a negative and significant birth order effect on the probability of participation in child labour, it is important to assess the distribution of the marginal effect since marginal effect is not constant in nonlinear models. Figure 2, therefore, presents the distribution of the estimated marginal effect of birth order on the probability of child labour participation, given observed characteristics and estimated values of the unobserved effects. As can be seen from the figure, the probabilities are always nonpositive, ranging from -10 to 0 percentage points; besides, it has a bimodal distribution with spikes around -9 and -1 percentage points. This suggests that there may be differential birth order effect on the probability of child labour participation across different groups of the population.

Contrary to the fact that the birth order estimates are uniformly negative and significant across models in child labour regressions, its estimates in the *school attendance* regressions differ both in magnitude and significance across models. Generally, it is negative and insignificant in models which do not control for endogeneity of family size. Once endogeneity of family size is controlled for in the IV models, the birth order coefficient has become positive and significant in unobserved effect probit IV model, with estimated average marginal effect of 0.012, implying younger kids are 1.2 percentage points more likely to attend school than their older siblings. However, in our preferred model, unobserved effect bivariate probit IV model, the birth order estimate is positive (i.e. 0.014) but not significant ($p-value = 0.124$).

As Table 8 shows, the average marginal effects of birth order on school attendance are 0.014 and -0.002 in the IV and non-IV models, respectively; besides, the coefficient estimate of the first-stage residual in the (school attendance) IV regression is significant. This suggests that endogeneity of family size is an issue in the school attendance equation. Hence, the same set of unobservable characteristics that affect parents' choice of family size seem to affect parents' decision whether to send the

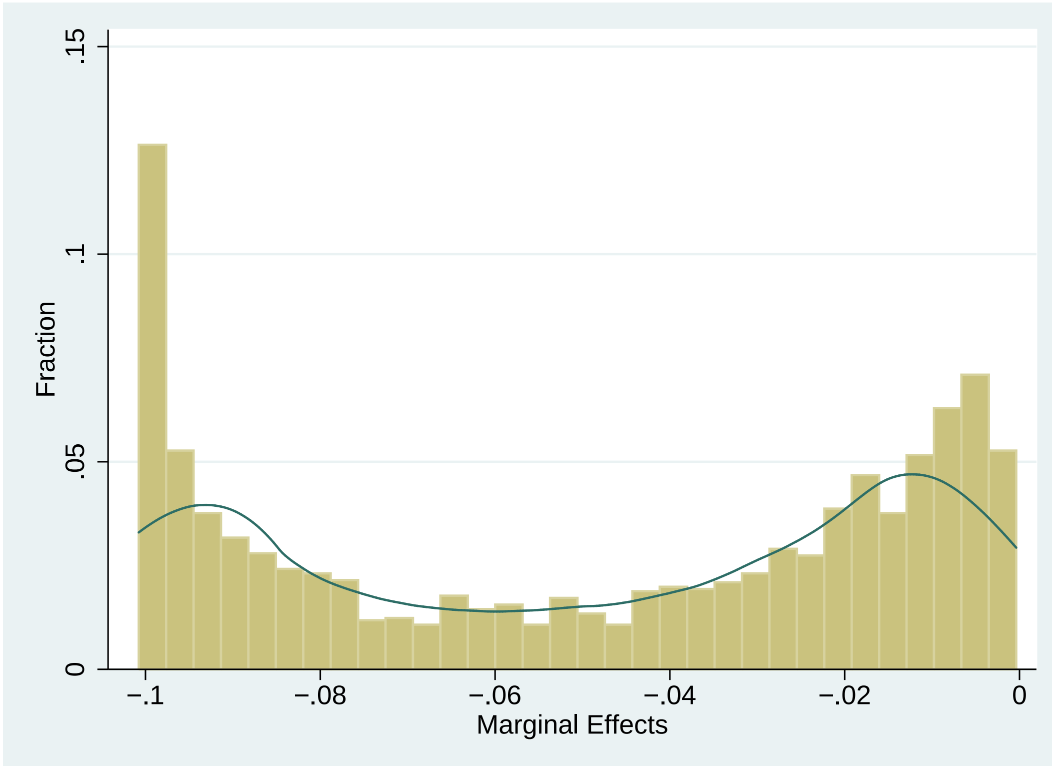

Figure 2: Histogram and kernel density estimates of marginal effects of birth order on child labour

child to school. For example, parents who have strong taste for education and care more for their children's education may decide to have fewer kids and send them to school regardless of the birth order of the child.

Although our preferred model implies that there is no birth order effect in the probability of school attendance, the estimated marginal effect of birth order on the probability of school attendance is always nonnegative for each child, ranging from 0 to 6 percentage points (see Figure 3 for the distribution of the estimated marginal effect). We emphasize that only about 10% of children in the sample do not attend school; this might have contributed in making the coefficient estimate of birth order in school attendance equation insignificant.

Though birth order has no effect on school attendance, the estimated correlation parameter between the individual effects in school attendance and child labour equations, $Corr(\alpha_{is}, \alpha_{il})$, is -0.37 in the bivariate probit IV model which is presented in Table 8. This suggests that unobservable characteristics may have opposite effects on the probabilities of school attendance and child labour participation. This could happen, for instance, if parents are more likely to send their more-able child to school relative to the child's (less-able) sibling.

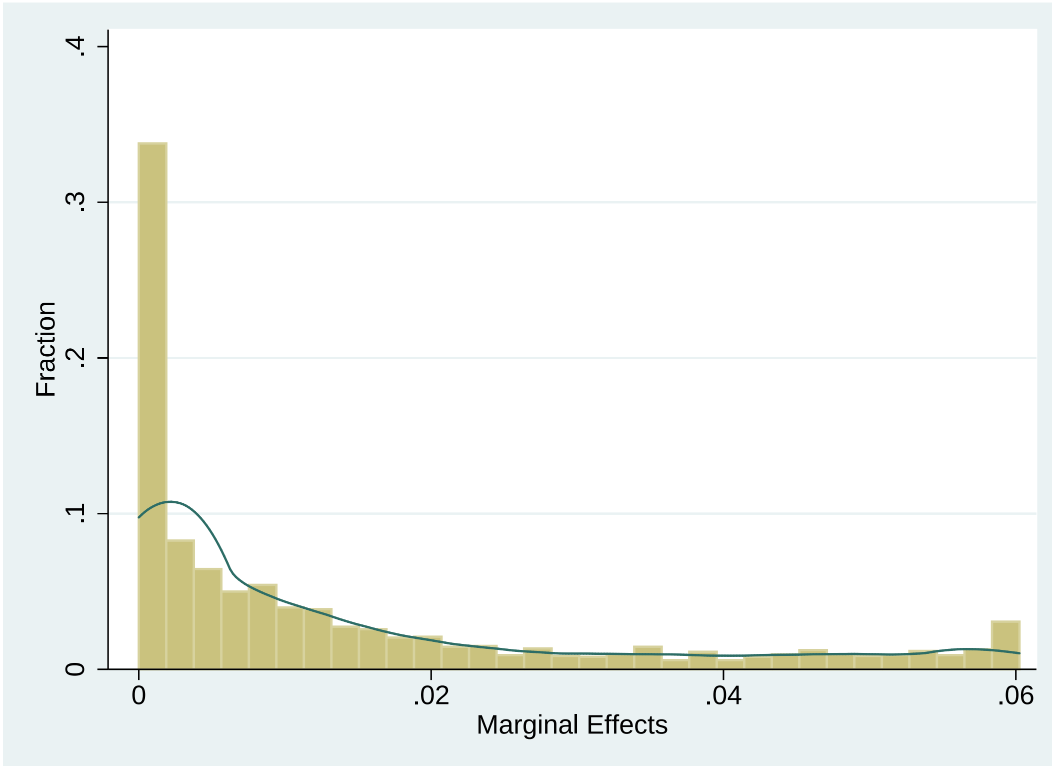

Figure 3: Histogram and kernel density estimates of marginal effects of birth order on school attendance

On the other hand, it is possible for birth order to affect the school performance of children who are going to school even if it does not affect the probability of school attendance. Cavalieri (2002), for instance, has shown that child labour negatively affects school performance. If this is true in our data too, we expect high birth order (i.e. younger) children to outperform their low birth order siblings in school since the former are less likely to participate in child labour.

If school performance measures such as test scores are observed in the data, we can check if the data support this argument by regressing the school performance measure on birth order and a host of control variables. Unfortunately, however, students' test score or other relevant school performance measures are not recorded in the data. But information on the child's current grade and his or her age are available in the data; thus, we could have used age-adjusted grade to measure school performance as used in prior studies (e.g. see Horowitz and Souza, 2011). The problem of using this measure in our data is that school starting age is not observable, and given most children in developing countries delay primary school enrolment by few years beyond the legal school starting age (Barro and Lee, 2000), using age-adjusted grade would create an additional problem of identification; namely, identifying the separate effects of birth order and delayed primary school enrolment on years of schooling. Thus, we resort to assessing whether birth order affects the number of hours the child spends studying. It is inaccurate to argue that hours spent studying is directly translated to better school performance since study time is only one of the inputs that affect performance at school. However, it is plausible to assume that the hours spent studying help students understand the subjects better and perform well in school, other things being equal.

A linear fixed-effect model of the effect of birth order on hours students spend studying is estimated, and the results are reported in Table A.2.[15] Column 1 of Table A.2, which is estimated by restricting the sample to students, suggests that there is no birth order effect on the number of hours students spend studying. The same is true for a sample of children who are going to school but *working* as child labourer (see column 2 of Table A.2). But when we restrict the sample further to children who are going to school but *not working* as child labourer (column 3 of Table A.2), the coefficient estimate of birth order is positive and significant, suggesting that a one-unit increase in birth order increases hours the child spends studying by 1.9 hours per day.

We caution that the positive effect of birth order on study hours documented here is based on a small sample of 365 nonworking students and the estimated coefficient is significant at 8%. However, our finding is consistent with the finding that child labour negatively affects school performance (Cavalieri, 2002) since high birth order children are less likely to work. Though their result and the one found here are not directly comparable, it is interesting to note that Ejrnaes and Pörtner (2004) find out that first-borns spend 10 more hours on school per week than last-borns. The presence of birth order effect (on study hours) only among children who are going to school but not working as child labourer indicates that child labour crowds out study hours.

Finally, note that eight child age dummies (with 7-year-old children as excluded group) are included to control for the age of the child; hence, it is not age difference that is driving the results. The coefficient estimates of all the eight child age dummies are positive and significant in both equations (see Table 8). Besides,

[15] Mean and SDs of study hours are 1.75 and 1.01, respectively.

their magnitude increases somehow progressively with age, suggesting the probability that the child attends school and works increases with age. The other control variables, in general, have the expected signs. Children who live in urban areas are more likely to attend school and less likely to work than their rural counterparts. Compared to boys, girls are more likely to work, but there is no difference in the probability of school attendance by gender. Parental years of schooling have no effect on participation in child labour, but father's schooling increases the probability of school attendance. Mother's schooling, nevertheless, has a negative effect on school attendance, which is not consistent with what we expect. Household expenditure, a proxy to the family's permanent income, plays no role in school attendance and participation in child labour. This may be because we controlled for father's and mother's years of schooling, which are proxies for the socioeconomic status of the household.

5.1. Alternative specifications

Time-use analysis

As mentioned above, one advantage of the data used in this article is that we observe how children spend their time on a number of selected activities. This allows us to complement the estimates reported above by estimating a richer model of time-use equations. We now, thus, report results from estimating system equations for a sample of children who attend school[16] where the dependent variables are hours spent on school, market work and household chore.[17]

SURE model is typically well suited to estimate such type of time-use equations. For the case at hand, however, estimating the standard SURE model alone is not appropriate because of potential endogeneity of family size. Since we want to employ a model that controls for both potential endogeneity of family size and unobserved heterogeneity in the multiequation system, we estimated fixed-effect 3SLS IV model[18] as well.

[16] The time-use models are also estimated by including those children that are *not* going to school. The results are comparable to those reported here. These results are available upon request or at the web link given in footnote 14.

[17] Mean (and SDs) of hours students in our sample spend on school, market work and household chore are 4.98 (0.19), 3.94 (2.02) and 1.31 (1.74), respectively.

[18] In the fixed-effect 3SLS IV model, Equation 3, as before, denotes the first-stage equation where family size is the dependent variable and proportion of boys is the exclusion restriction. Results reported in the fixed-effect 3SLS IV model are obtained by running 3SLS IV regression on time-demeaned data.

Table 9: Summary of estimates of coefficient of birth order from different time use models (N = 1670)

		Dependent variables: hours spent on		Dependent variables: hours spent on		
		School	Total work	School	Market work	Household chore
Pooled SURE model	Coeff.	0.091***	-0.332***	0.091***	-0.271***	-0.061*
	p-value	(0.003)	(0.000)	(0.003)	(0.000)	(0.054)
	R^2	0.184	0.335	0.184	0.274	0.400
Unobserved effect SURE model	Coeff.	0.180***	-0.172***	0.281***	-0.144***	0.004
	p-value	(0.000)	(0.000)	(0.000)	(0.000)	(0.785)
Fixed-effect 3SLS IV model	Coeff.	1.342**	-1.110	1.342**	-1.502*	0.392
	p-value	(0.048)	(0.305)	(0.048)	(0.081)	(0.515)
	R^2	0.411	0.064	0.411	0.084	0.013

Notes: *p < 0.10; ** p < 0.05; *** p < 0.01.
Hours spent on total work is the sum of hours spent on market work and household chore.

More specifically, we estimated pooled SURE, unobserved effect SURE and fixed-effect 3SLS IV models. In each of these models, two types of equations are estimated where the dependent variables are (1) hours spent on school and total work (which is the sum of hours spent on market work and household chore) and (2) hours spent on school, market work and household chore. The summary of the estimates of coefficient of birth order from these different time-use models is presented in Table 9.[19]

The estimates of the coefficient of birth order in all time use models reported in Table 9 show that birth order has positive and statistically significant effect on hours students spend on school, suggesting younger kids on average spend more hours on school relative to their older siblings. This could be because younger siblings, who are less likely to participate in child labour, skip classes less frequently.

When it comes to the effect of birth order on hours students spent on child labour, it seems that birth order has a negative effect on hours spent on market work, but it has no effect on hours spent on household chore. In the fixed-effect 3SLS IV model, for instance, younger kids spend 1.5 less hours on market work relative to their older siblings. However, the coefficient estimate of birth order is insignificant in the household chore equation. Interestingly, the estimates from this model show that younger kids spend about 1.3 more hours on school, indicating that the increase in school hours come from a decrease in hours spent on market work.

Unlike the probability models, the dependent variables in the time-use analysis are continuous number of hours spent on different activities. A closer look at the joint distribution of hours spent on school and work, however, reveals that some of the cells in the joint distribution are sparse and empty. This problem gets worse when we split hours spent on work into two: hours spent on market work and household chore. As a result, the time-use models are not tightly estimated, and, thus, we have more confidence in the results from the probability models than those from the time-use models. To mitigate this problem and as an additional complementary analysis, we estimated different ordered probit models.[20] The results from the ordered probit models suggest positive birth order effect on time spent on school, but the effect is insignificant in our preferred specification. These results also show a significant

[19]The compete regression results from the three time-use models reported in Table 9 are available upon request or at the web link given in footnote 14.

[20]The regression results from the various ordered probit models are available upon request or at the web link given in footnote 14.

negative effect of birth order on time spent on child labour across all specifications.

Exponential mean regression for family size

In the IV approach implemented in the previous sections, the first-stage equation is denoted by Equation 3 where the dependent variable is family size and the equation is estimated by OLS and fixed-effect regressions (see Table 3 for results from these regressions). Since family size is a dependent variable with a limited number of integer outcomes, we re-estimated the bivariate probit IV model where the first-stage equation is modelled as count data model with exponential mean using nonlinear least squares; see, for example, Cameron and Trivedi (2005), Section 20.5.2.

A comparison of results reported in Table 8 with those from where the first-stage equation is modelled as count data model using exponential mean[21] reveals that the coefficient estimates of variables in the bivariate probit IV models are not sensitive to how we model the first-stage equation. For instance, the regression results from unobserved effect bivariate probit IV model where the first-stage equation is modelled as count data model using exponential mean show that birth order does not affect the probability of school attendance whereas it decreases the probability of participation in child labour by about 4.4%, which is comparable to what has been reported in Table 8.

6. Conclusion

It is well known to economists that parental action creates education inequalities among children (Becker and Tomes, 1976). The role parental action plays in creating education inequalities is more pronounced in developing countries where parents are too poor to send all their children to school at the same time and when child labour is widely practised. It is not uncommon for poor parents in developing countries to send some of their children to school and the others to work. Parents consider child characteristics and a whole lot of other factors when they allocate the child's time between child labour obligations and school opportunities. In this article, we investigate the role the birth order of the child plays in whether the child attends school or participates in child labour.

[21] Regression results from unobserved effect bivariate probit IV model where the first-stage equation is modelled as count data model using exponential mean are available upon request or at the web link given in footnote 14.

One of the methodological challenges in birth order studies is endogeneity of family size. Endogeneity of family size arises in birth order studies since high birth order children are observed only in larger families, and parents who choose to have more kids may be inherently different and children in these families would have worse outcome regardless of family size and birth order. We exploit the fact that Ethiopian parents prefer boys to girls and use proportion of boys in the family to instrument family size and estimated unobserved effect bivariate probit IV model of school attendance and child labour choices using longitudinal household survey data from Ethiopia.

The results reveal that an increase in birth order by one unit decreases the probability of child labour participation by 5 percentage points, but we find no evidence that suggests birth order affects the probability of school attendance. However, among children who are going to school, a one-unit increase in birth order increases the time the child spends studying by 1.9 hours per day. Since eight child age dummies are included to control for the age of the child, it is not age difference that is driving the results.

On the other hand, the results from time-use analysis, which explores time allocation across school, market work and household chore by birth order, reveal that younger students spend more hours on school relative to their older siblings. The increase in hours spent on school seems to come from a decrease in hours spent on market work. However, there is no birth order effect on hours spent on household chore.

The results obtained here can be generalized to other developing countries which have similar socio-economic environments as that of Ethiopia, such as high incidence of child labour, limited access to school and strong preference for boys.

The birth order effects documented here have important policy implications for inequalities in education and income. Given differences in the probability of child labour participation and hours spent studying across different birth order children, birth order effects tend to work against programmes that reduce inequalities in education and income. For example, in developing countries, where child labour is widely practised and access to school is limited, school expansion may increase the overall level of education. While increasing education levels, child labour may exacerbate inequality in education within households if parents, based on birth order, increase schooling for some of their children while relegating others to child labour. On the other hand, programmes that aim to increase household income among resource-constrained households through income transfers or other means may mitigate siblings' educational inequality.

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Appendix: Additional tables

Table A.1: Child labour specialization by gender (N = 1866)

Type of work	Gender Boy %	 Girl %	 Total %
Domestic work	23.6	76.4	100.0
Unpaid work	81.7	18.3	100.0
Caring for others	31.6	68.4	100.0
Paid work	50.7	49.3	100.0

Note: Authors' calculation based on household survey data from Young Lives.

Table A.2: Fixed-effect estimates of hours students spend studying

	All students		Working students		Nonworking students	
	Coeff.	SE	Coeff.	SE	Coeff.	SE
Birth order	0.750	(0.62)	0.217	(1.01)	1.946*	(1.11)
Number of kids	-0.709	(0.60)	-0.263	(0.99)	-1.537	(1.05)
Child's age = 8	0.669	(0.69)	-1.306***	(0.21)	1.716**	(0.85)
Child's age = 9	0.811	(0.79)	-0.942**	(0.40)	1.805	(1.34)
Child's age = 10	0.858	(0.84)	-0.824	(0.56)	0.376	(1.30)
Child's age = 11	1.859	(1.42)	-1.756***	(0.57)	2.130	(1.91)
Child's age = 12	1.832	(1.53)	-1.436*	(0.79)	1.905	(2.45)
Child's age = 13	1.727	(1.65)	-1.459	(1.09)	0.631	(2.63)
Child's age = 14	2.701	(2.20)	-2.498**	(1.13)	2.690	(3.10)
Child's age = 15	2.851	(2.32)	-1.885	(1.35)	2.275	(3.63)
Housemaid duumy (yes=1)	0.919*	(0.50)	0.499	(0.78)	1.561	(1.13)
Father's schooling	-0.089	(0.10)	-0.117	(0.10)		
Mother's schooling	-0.383	(0.25)	-0.018	(0.38)	-0.551	(0.59)
Household expenditure	0.059	(0.04)	0.097	(0.08)	0.138**	(0.07)
Urban dummy (yes=1)	-0.590	(0.76)	-0.994***	(0.28)	0.223	(0.66)
First-stage residual	0.650	(0.60)	0.305	(0.99)	1.275	(1.03)
Working child (yes=1)	-0.134	(0.10)				
Constant	2.657	(1.98)	3.635	(2.22)	2.543	(3.74)
Observations	1670		1305		365	
R^2	0.052		0.068		0.305	

Notes: *p < 0.10; ** p < 0.05; *** p < 0.01.
SEs are reported in parentheses. Village dummies are included as additional control variables.

Supplementary material for "The role of birth order in child labour and schooling"

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Appendix B: Supplementary Appendix

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I. Supplementary Appendix

Additional Tables

Binary response models The three tables below (i.e., Tables B.1, B.2, and B.3) report regression results that are summarised in the Results section of the article in Table 7. More specifically, Tables B.1 and B.2 report results from independent pooled probit, independent unobserved effect probit, and independent unobserved effect probit IV models which are summarised in the upper panel of Table 7. Table B.3, on the other hand, report results from pooled bivariate probit model which is summarised in the first row of the lower panel of Table 7.

Table B.1. Independent pooled probit estimates of school attendance and child labour equations

	School Equation	Child Labour Equation
Birth order	-0.027	-0.186***
	(0.05)	(0.04)
Number of kids	-0.031	0.125***
	(0.04)	(0.04)
Child's age = 8	0.579***	0.368**
	(0.20)	(0.18)
Child's age = 9	1.184***	0.591***
	(0.22)	(0.19)
Child's age = 10	1.196***	0.551***
	(0.22)	(0.19)
Child's age = 11	1.958***	0.839***
	(0.23)	(0.17)
Child's age = 12	1.657***	0.863***
	(0.18)	(0.17)
Child's age = 13	1.326***	0.830***
	(0.30)	(0.26)
Child's age = 14	1.749***	0.911***
	(0.23)	(0.23)
Child's age = 15	1.365***	0.711***
	(0.26)	(0.22)
Female dummy (yes=1)	0.153	0.196**
	(0.12)	(0.08)
Housemaid dummy (yes=1)	0.122	-0.391**
	(0.24)	(0.17)
Father's schooling	0.060***	-0.031**
	(0.02)	(0.01)
Mother's schooling	-0.024	0.001
	(0.03)	(0.01)
Household expenditure	0.057	-0.075*
	(0.09)	(0.04)
Urban dummy (yes=1)	1.072	-0.668*
	(0.73)	(0.37)
Observations	1860	1860
Log Likelihood	-457.828	-667.514

Notes: *$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Standard errors (SE) are reported in parentheses. Village and year dummies are included as additional control variables.

Table B.2. Independent unobserved effect probit estimates of school attendance and child labour equations

	School Equation				Work Equation			
	Probit		Probit IV		Probit		Probit IV	
Birth order	-0.032	(0.06)	0.389**	(0.19)	-0.193***	(0.04)	-0.242**	(0.10)
Number of kids	-0.047	(0.06)	-0.545***	(0.17)	0.126***	(0.04)	0.032	(0.09)
Child's age = 8	0.770***	(0.26)	1.811***	(0.50)	0.393**	(0.20)	0.832***	(0.31)
Child's age = 9	1.537***	(0.30)	3.478***	(0.54)	0.627***	(0.21)	1.179***	(0.32)
Child's age = 10	1.586***	(0.30)	3.980***	(0.54)	0.586***	(0.20)	1.058***	(0.28)
Child's age = 11	2.533***	(0.35)	5.503***	(0.63)	0.887***	(0.19)	1.508***	(0.33)
Child's age = 12	2.169***	(0.32)	4.863***	(0.62)	0.917***	(0.19)	1.726***	(0.34)
Child's age = 13	1.787***	(0.43)	4.786***	(0.84)	0.866***	(0.29)	1.311***	(0.48)
Child's age = 14	2.250***	(0.43)	5.084***	(0.84)	0.964***	(0.26)	1.515***	(0.47)
Child's age = 15	1.771***	(0.38)	4.216***	(0.83)	0.758***	(0.25)	1.472***	(0.48)
Female dummy (yes=1)	0.184	(0.13)	0.510*	(0.29)	0.202**	(0.09)	0.357*	(0.19)
Housemaid dummy (yes=1)	0.144	(0.29)	0.470	(0.59)	-0.414**	(0.18)	-0.360	(0.31)
Father's schooling	0.080***	(0.03)	0.135**	(0.06)	-0.033**	(0.02)	-0.070**	(0.03)
Father's schooling	-0.028	(0.04)	-0.161*	(0.08)	0.000	(0.02)	-0.054	(0.04)
Household expenditure	0.081	(0.10)	0.248*	(0.15)	-0.072	(0.05)	0.028	(0.06)
Urban dummy (yes=1)	1.322	(0.81)	4.451***	(0.95)	-0.688	(0.52)	-0.636	(0.45)
First-stage residual			0.488***	(0.18)			0.170*	(0.10)
Observations	1862		1862		1860		1860	
Log Likelihood	-449.007		-663.310		-666.841		-1146.119	

Notes: *$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors are reported in parentheses. Village and year dummies are included as additional control variables.

Table B.3. Pooled bivariate probit estimates of school attendance and child labour equations

	Coeff.	SE
School attendance		
Birth order	-0.030	(0.05)
Number of kids	-0.031	(0.04)
Child's age = 8	0.576***	(0.19)
Child's age = 9	1.176***	(0.20)
Child's age = 10	1.176***	(0.19)
Child's age = 11	1.943***	(0.22)
Child's age = 12	1.629***	(0.20)
Child's age = 13	1.295***	(0.30)
Child's age = 14	1.728***	(0.29)
Child's age = 15	1.324***	(0.26)
Female dummy (yes=1)	0.138	(0.10)
Father's schooling	0.060***	(0.02)
Mother's schooling	-0.022	(0.03)
Household expenditure	0.066	(0.09)
Urban dummy (yes=1)	1.057	(0.71)
Constant	-0.079	(0.89)
Child Labour		
Birth order	-0.188***	(0.04)
Number of kids	0.125***	(0.04)
Child's age = 8	0.378**	(0.18)
Child's age = 9	0.595***	(0.19)
Child's age = 10	0.554***	(0.19)
Child's age = 11	0.841***	(0.17)
Child's age = 12	0.855***	(0.17)
Child's age = 13	0.838***	(0.26)
Child's age = 14	0.910***	(0.23)
Child's age = 15	0.708***	(0.22)
Female dummy (yes=1)	0.197**	(0.08)
Father's schooling	-0.031**	(0.01)
Mother's schooling	0.001	(0.01)
Household expenditure	-0.076*	(0.04)
Urban dummy (yes=1)	-0.687*	(0.38)
Constant	-1.124**	(0.50)
athrho		
Constant	-0.230***	(0.09)
Observations	1860	
Log likelihood	-1119.195	

Notes: *$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Standard errors (SE) are reported in parentheses. Village dummies, a year dummy, and a dummy variable for the presence of housemaid are included as additional control variables.
Note: As stated in Stata documentation, in the maximum likelihood estimation, ρ is not directly estimated, but atanh ρ (i.e., athrho constant in the Table) is, where $atanh\ \rho = \frac{1}{2}\ ln\ (\frac{1+\rho}{1-\rho})$. If $atanh\ \rho$ is statistically significantly different from zero, then bivariate probit model is a better fit than univariate independent probit models. The estimate of the untransformed ρ is -0.111.

Time use analysis The three tables below (i.e., Tables B.4, B.5, and B.6) report regression results that are summarised in the Results section of the article in Table 9, where Tables B.4, B.5, and B.6 respectively report results summarised in rows 1-3, 4-5, and 6-8 of Table 9.

Table B.7, on the other hand, is a counterpart of Table 9 and summarises results from the same specifications as those reported in Table 9 but for a larger sample that includes both children who are attending school and those who are not.

Table B.4. Estimates from pooled seemingly unrelated regression equation (SURE) of hours spent on school, total work, market work, and household chore (N = 1670)

	Dependent Variables: hours spent on				Dependent Variables: hours spent on					
	School		Total Work		School		Market Work		Household Chore	
Birth order	0.091***	(0.03)	-0.332***	(0.06)	0.091***	(0.03)	-0.271***	(0.04)	-0.061*	(0.03)
Number of kids	-0.083***	(0.03)	0.262***	(0.05)	-0.083***	(0.03)	0.179***	(0.03)	0.083***	(0.03)
Child's age = 8	-0.114	(0.20)	1.041***	(0.40)	-0.114**	(0.20)	0.789***	(0.26)	0.253	(0.21)
Child's age = 9	0.233	(0.19)	1.317***	(0.39)	0.233	(0.19)	0.826***	(0.25)	0.490**	(0.20)
Child's age = 10	0.162	(0.18)	1.230***	(0.36)	0.162	(0.18)	1.116***	(0.23)	0.114	(0.19)
Child's age = 11	0.467***	(0.16)	2.098***	(0.33)	0.467***	(0.16)	1.624***	(0.21)	0.474***	(0.17)
Child's age = 12	0.590***	(0.16)	1.991***	(0.33)	0.590***	(0.16)	1.516***	(0.21)	0.475***	(0.17)
Child's age = 13	0.399*	(0.21)	2.376***	(0.42)	0.399*	(0.21)	1.814***	(0.27)	0.562***	(0.22)
Child's age = 14	0.843***	(0.18)	1.796***	(0.35)	0.843***	(0.18)	1.642***	(0.23)	0.154	(0.18)
Child's age = 15	0.995***	(0.17)	1.661***	(0.35)	0.995***	(0.17)	1.513***	(0.22)	0.148	(0.18)
Female dummy (yes=1)	0.087	(0.06)	-1.058*	(0.13)	0.087	(0.06)	0.334***	(0.08)	-1.392***	(0.07)
Housemaid dummy (yes=1)	0.064	(0.15)	-0.378	(0.31)	0.064	(0.15)	-0.238	(0.20)	-0.140	(0.16)
Father's schooling	0.027**	(0.01)	-0.025	(0.02)	0.027**	(0.01)	-0.030*	(0.02)	0.006	(0.01)
Mother's schooling	0.023*	(0.01)	-0.047*	(0.03)	0.023*	(0.01)	-0.008	(0.02)	-0.039***	(0.01)
Household expenditure	0.072**	(0.04)	0.003	(0.07)	0.072**	(0.04)	0.006	(0.05)	-0.003	(0.04)
Urban dummy (yes=1)	1.186***	(0.16)	-0.991***	(0.33)	1.186***	(0.16)	-0.732***	(0.21)	-0.259	(0.17)

Notes: *$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Standard errors are reported in parentheses. Village and year dummies are included as additional control variables.
Hours spent on total work is the sum of hours spent on market work and household chore.

Table B.5. Estimates from unobserved effect seemingly unrelated regression equation (SURE) of hours spent on school, total work, market work, and household chore (N = 1670)

	Dependent Variables: hours spent on				Dependent Variables: hours spent on					
	School		Total Work		School		Market Work		Household Chore	
Birth order	0.180***	(0.05)	-0.172***	(0.03)	0.281***	(0.02)	-0.144***	(0.03)	0.004	(0.03)
Number of kids	0.088**	(0.04)	0.232***	(0.02)	-0.016	(0.02)	0.180***	(0.02)	0.056**	(0.02)
Child's age = 8	2.378***	(0.27)	2.152***	(0.15)	2.185***	(0.13)	1.347***	(0.15)	0.337**	(0.16)
Child's age = 9	2.547***	(0.26)	2.688***	(0.15)	2.316***	(0.12)	1.554***	(0.14)	0.531***	(0.15)
Child's age = 10	2.502***	(0.23)	2.580***	(0.13)	2.229***	(0.11)	2.002***	(0.13)	-0.007	(0.14)
Child's age = 11	2.958***	(0.20)	3.583***	(0.12)	2.791***	(0.10)	2.414***	(0.12)	0.416***	(0.12)
Child's age = 12	2.991***	(0.20)	3.487***	(0.11)	2.982***	(0.10)	2.399***	(0.11)	0.327***	(0.12)
Child's age = 13	3.063***	(0.29)	3.812***	(0.16)	2.696***	(0.14)	2.538***	(0.16)	0.729***	(0.16)
Child's age = 14	3.162***	(0.22)	3.505***	(0.13)	3.293***	(0.11)	2.577***	(0.12)	0.036	(0.13)
Child's age = 15	3.338***	(0.22)	3.306***	(0.12)	3.410***	(0.11)	2.424***	(0.12)	-0.017	(0.13)
Female dummy (yes=1)	0.424***	(0.10)	-0.547***	(0.06)	0.311***	(0.05)	0.424***	(0.06)	-1.465***	(0.06)
Housemaid dummy (yes=1)	-0.001	(0.23)	-0.718***	(0.14)	-0.108	(0.11)	-0.334**	(0.13)	-0.029	(0.13)
Father's schooling	0.037*	(0.02)	-0.014	(0.01)	0.080***	(0.01)	-0.020*	(0.01)	0.010	(0.01)
Mother's schooling	0.045**	(0.02)	-0.046***	(0.01)	0.042***	(0.01)	-0.011	(0.01)	-0.012	(0.01)
Household expenditure	0.028	(0.05)	0.117***	(0.03)	0.064**	(0.03)	0.030	(0.03)	-0.025	(0.03)
Urban dummy (yes=1)	3.775***	(0.23)	0.351***	(0.14)	3.616***	(0.12)	0.494***	(0.14)	0.673***	(0.14)

Notes: *$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Standard errors are reported in parentheses. Village and year dummies are included as additional control variables.
Hours spent on total work is the sum of hours spent on market work and household chore.

Table B.6. Three-stage least-squares estimates of hours students spent on school, market work, household chores (N = 1670)

	Dependent Variables: hours spent on				Dependent Variables: hours spent on					
	School		Total Work		School		Market Work		Household Chore	
Birth order	1.342**	(0.68)	-1.110	(1.18)	1.342**	(0.68)	-1.502*	(0.86)	0.392	(0.60)
Number of kids	-1.266*	(0.65)	0.770	(1.13)	-1.266*	(0.65)	1.223	(0.83)	-0.452	(0.58)
Child's age = 8	0.095	(0.36)	1.203*	(0.62)	0.095	(0.36)	0.797*	(0.45)	0.407	(0.32)
Child's age = 9	-0.293	(0.31)	1.477***	(0.53)	-0.293	(0.31)	1.020***	(0.39)	0.457*	(0.27)
Child's age = 10	0.347	(0.22)	1.255***	(0.37)	0.347	(0.22)	0.915***	(0.27)	0.340*	(0.19)
Child's age = 11	1.024**	(0.48)	2.551***	(0.83)	1.024**	(0.48)	1.486**	(0.61)	1.065**	(0.43)
Child's age = 12	0.380	(0.38)	2.087***	(0.65)	0.380	(0.38)	1.360***	(0.48)	0.726**	(0.34)
Child's age = 13	0.860**	(0.37)	2.338***	(0.64)	0.860**	(0.37)	1.434***	(0.47)	0.904***	(0.33)
Child's age = 14	1.751***	(0.62)	2.227**	(1.07)	1.751***	(0.62)	1.238	(0.78)	0.989*	(0.55)
Child's age = 15	1.259**	(0.54)	1.525	(0.94)	1.259**	(0.54)	0.990	(0.69)	0.535	(0.48)
Housemaid dummy (yes=1)	1.574**	(0.77)	-2.233*	(1.34)	1.574**	(0.77)	-2.301**	(0.98)	0.068	(0.69)
Father's schooling	-0.011	(0.07)	0.076	(0.12)	-0.011	(0.07)	-0.054	(0.09)	0.130**	(0.06)
Mother's schooling	-0.190**	(0.09)	0.306**	(0.15)	-0.190**	(0.09)	0.231**	(0.11)	0.074	(0.08)
Household expenditure	-0.016	(0.07)	0.166	(0.12)	-0.016	(0.07)	0.092	(0.09)	0.074	(0.06)
Urban dummy (yes=1)	-0.135	(0.38)	-0.015	(0.67)	-0.135	(0.38)	0.030	(0.49)	-0.045	(0.34)

Notes: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.
Standard errors (SE) are reported in parentheses. Village dummies are included as additional control variables.

Table B.7. Summary of estimates of coefficient of birth order from different time use models (N = 1862)

		Dependent Variables: hours spent on		Dependent Variables: hours spent on		
		School	Total Work	School	Market Work	Household Chore
Pooled SURE Model	Coeff.	0.087*	-0.272***	0.087*	-0.228***	-0.037
	p-Value	(0.082)	(0.000)	(0.082)	(0.000)	(0.240)
	R^2	0.220	0.310	0.220	0.222	0.391
Unobserved Effect SURE Model	Coeff.	0.269***	-0.044	0.264***	-0.114***	0.035
	p-Value	(0.000)	(0.387)	(0.000)	(0.001)	(0.242)
Fixed-effect 3SLS IV Model	Coeff.	2.335*	-2.257	2.335*	-2.562**	-0.503
	p-Value	(0.056)	(0.118)	(0.056)	(0.036)	(0.446)
	R^2	0.430	0.180	0.430	0.458	0.037

Notes: *$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Hours spent on total work is the sum of hours spent on market work and household chore.

Ordered probit model For the purpose of estimating various ordered probit models, we grouped hours spent on school into 4 classes: 0 hour, 1-6 hours, 7-8 hours, and 9-11 hours. Similarly, we grouped hours spent on total work into 4 classes: 0 hour, 1-3 hours, 4-7 hours, and 8-12 hours. See Table B.8 for joint percentage distribution of hours spent on school and work under these groupings. After we grouped time use in hours into class intervals, we estimated various ordered probit models with full set of controls, including year effects. Also, three threshold parameters were estimated for each response variable. Summary of estimates of coefficient of birth order from different ordered probit models are presented in Table B.9.

Table B.8. Joint percentage distributions for (grouped) hours spent on school and work (N = 1862)

	Hours spent on total work				
Hours spent on school	0 hr	1-3 hrs	4-7 hrs	8-12 hrs	Total
0 hr	0.70	0.75	1.66	5.75	8.86
1-6 hrs	0.54	3.06	6.12	8.06	17.78
7-8 hrs	2.04	9.77	20.95	17.08	49.84
9-11 hrs	2.26	10.04	8.11	3.11	23.52
Total	5.53	23.63	36.84	34.00	100.00

Note: Authors' calculation based on household survey data from Young Lives.

Table B.9. Summary of estimates of coefficient of birth order from different ordered probit models for all children in the sample (N = 1862)

		Dependent Variables: hours spent on	
		School	Total Work
Independent Ordered Probit (OP) Models			
Pooled OP Model	Coeff.	0.067***	-0.095***
	p-Value	(0.006)	(0.000)
	R^2	0.099	0.142
Unobserved Effect OP Model	Coeff.	0.074**	-0.096***
	p-Value	(0.015)	(0.002)
Unobserved Effect OP IV Model	Coeff.	0.068	-0.150**
	p-Value	(0.261)	(0.015)

Notes: *p < 0.10, ** p < 0.05, *** p < 0.01.
Hours spent on total work is the sum of hours spent on market work and household chore.

Exponential mean regression for family size Family size is a dependent variable with a limited number of integer outcomes. Hence, we re-estimated the unobserved effect bivariate probit IV model where the first-stage equation is modelled as count data model with exponential mean using nonlinear least squares (NLS). See Table B.10 for regression results from unobserved effect bivariate probit IV model where the first-stage equation is modelled as count data model.

Table B.10. Unobserved effect bivariate probit estimates of school attendance and child labour equations

	Bivariate Probit IV Model		
	Coeff.	AME	SE
School attendance			
Birth order	-0.005	[-0.004]	(0.10)
Number of kids	-0.104**	[-0.009]	(0.05)
Child's age = 8	0.922***	[0.082]	(0.27)
Child's age = 9	1.161***	[0.103]	(0.28)
Child's age = 10	1.186***	[0.105]	(0.26)
Child's age = 11	2.066***	[0.183]	(0.32)
Child's age = 12	2.066***	[0.152]	(0.32)
Child's age = 13	1.389***	[0.123]	(0.34)
Child's age = 14	1.547***	[0.137]	(0.31)
Child's age = 15	1.070***	[0.095]	(0.28)
Female dummy (yes=1)	0.239*	[0.021]	(0.14)
Father's schooling	0.018	[0.001]	(0.27)
Mother's schooling	-0.043	[-0.004]	(0.03)
Household expenditure	0.845	[0.075]	(0.73)
Urban dummy (yes=1)	1.508***	[0.134]	(0.35)
First-stage residual^{+}	0.270	[0.240]	(0.42)
Child Labour			
Birth order	-0.223***	[-0.044]	(0.07)
Number of kids	0.098***	[0.019]	(0.04)
Child's age = 8	0.497**	[0.099]	(0.22)
Child's age = 9	0.654***	[0.129]	(0.22)
Child's age = 10	0.900***	[0.178]	(0.21)
Child's age = 11	1.044***	[0.207]	(0.20)
Child's age = 12	1.320***	[0.261]	(0.21)
Child's age = 13	1.576***	[0.312]	(0.30)
Child's age = 14	1.130***	[0.224]	(0.23)
Child's age = 15	1.242***	[0.246]	(0.23)
Female dummy (yes=1)	0.326***	[0.065]	(0.10)
Father's schooling	-0.008	[-0.002]	(0.02)
Mother's schooling	-0.009	[-0.002]	(0.02)
Household expenditure	-0.002	[-0.000]	(0.34)
Urban dummy (yes=1)	-0.962***	[-0.190]	(0.22)
First-stage residual^{+}	0.181	[0.036]	(0.25)
Observations	1862		
Log likelihood	-1276.309		

Notes: *$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Reported coefficients are average marginal effects.
Average marginal effects [AME] and standard errors (SE) are reported in brackets and parentheses, respectively. Village dummies, a year dummy, and a dummy variable for the presence of housemaid are included as additional control variables.
$^{+}$ denotes that first-stage residuals come from the first-stage equation which is modelled as count data model.