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Essays in Children's Time Allocation and Age at Primary School Enrollment

Yared Seid

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ESSAYS IN CHILDREN'S TIME ALLOCATION AND AGE AT PRIMARY SCHOOL ENROLLMENT

BY
YARED SEID

A Dissertation Submitted in Partial Fulfillment
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of
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ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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CONTENTS

LIST OF TABLES

LIST OF FIGURES

ABSTARCT

The essays in this dissertation explore the challenges of primary school attendance and the timing of enrollment in primary school in a typical developing country where child labor is widely practiced and poor households have limited access to school. The first essay assesses if when a child was born relative to his/her siblings affect whether the child attends school or participates in child labor. I investigate this question by estimating the effect of birth order on the probabilities of school attendance and child labor participation. Endogeneity of family size may bias the coefficient estimate of birth order 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. To address the endogeneity of family size, I 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, I estimate unobserved effect bivariate probit instrumental variable model of school attendance and child labor choices. The results suggest that the probability of child labor participation decreases with birth order, but I 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.

The second essay offers empirical evidence on whether access to primary school induces children to enroll in primary school at the legal enrollment age using household survey data from Ethiopia. I exploit the variation in the intensity of the impact of the education reform across districts in Ethiopia to identify the effect of access to school on the timing of enrollment. Using pre-reform enrollment rate in primary school to measure the variation in the intensity of the impact of the reform, I estimate difference-in-differences models. The results suggest that the reform has substantially increased the probability the child enrolls in grade 1 by age 7. It is also found out that the reform has decreased age at enrollment in grade 1 by about 4 months. These estimates highlight an important role that access to school plays in inducing parents to enroll their kids in primary school at the legal enrollment age.

INTRODUCTION

The essays in this dissertation examine barriers to primary education and the timing of enrollment in primary school in the context of a developing country. The prevalence of child labor in most developing countries substantially alters both the costs and benefits of school attendance. However, the effects of child labor on the costs and benefits of school attendance vary across children depending on child and family characteristics. This leads to differences in the educational achievement and labor market earnings of siblings who shared the same family and neighborhood background. This will be explored in essay 1, focusing on the effect of birth order on the child's time allocation between child labor obligations and schooling opportunities. Access to school also interacts with child labor in a number of important ways and influence both school starting age and completed years of schooling. Though the standard human capital models suggest it is optimal to start school as early as possible, this might not be the case in the presence of child labor where longer travel time to school further increases the opportunity cost of school attendance. Thus, the second essay examines the problem of overage primary school enrollment.

Investment in education ensures that a country will have the most productive labor force in the long run. Unfortunately, parents in low income countries cannot afford to adequately invest in their children, rendering the next generation of workers to join the adult labor market with lower productivity. Consider the case of Ethiopia. One in every thirteen new borns does not celebrate her/his first year birthday. Among those who celebrated their first year birthdays and younger than five years, 47% are malnourished (EDHS, 2005). Besides, about 21% of school-age children could not make it to school (MoFED, 2008). Even those who made it, do not have the luxury of being full time students. They are also expected to assist their parents either in household chores, family farm, or work somewhere else for money. Child labor is too crucial for poor families in Ethiopia that some children even start working at the age of 4 (Admassie, 2002), and it remains to be part of the children's daily routine throughout their childhood, regardless of their school attendance status.

Though these types of statistics, aggregated at a national level, are interesting and have their own merits, they conceal important variations in human capital investment in children across households and siblings. They do not tell us, for instance, why siblings who grew up in the same family and shared the same community background may have different educational achievements. The first essay takes on this question and examines the role of birth order on the child's time allocation between child labor and schooling, a decision that eventually affects the child's educational achievement and labor market earnings later in life.

One of the empirical challenges of birth order studies is endogeneity of family size. This is because high birth order children are observed in 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. I attempt to mitigate endogeneity of family size by exploiting the fact that Ethiopian parents prefer boys to girls and estimating unobserved effect bivariate probit instrumental variable (IV) model of child labor and schooling choices using longitudinal household survey data from Ethiopia.

The results reveal that an increase in birth order by one unit decreases the probability of working as child laborer by 5 percentage point, but I 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.

The second essay investigates the effect of access to primary school on the timing of enrollment. Though a large proportion of children in many developing countries enroll in primary school long after the legal enrollment age (Barro & Lee, 2001), the bulk of the literature focuses on enrollment rate, without considering age at enrollment. Delaying enrollment is costly as it, for example, decreases an individual's life time wealth by about 6% (Glewwe & Jacoby, 1995), and it increases grade repetition and school dropout rates (Wils, 2004). Given the high incidence of delayed enrollment in developing countries and the cost associated with it, the topic did not receive the attention it deserves, and we have limited understanding of why many children in developing countries do not enroll in primary school at the prescribed age. The second essay attempts to bridge this gap in the literature by examining the effect of access to school on the timing of enrollment using household survey data from Ethiopia.

To mitigate the potential endogeneity of access to school in the regression framework, I exploit the education reform in Ethiopia as exogenous source of variation in access to school. Between 1997 and 2004, the Ethiopian government implemented Educational Sector Development Program which resulted in the construction of 2,398 primary schools throughout the country. Though the program has increased access to school at a national level, the intensity of the impact of the program vary across districts in Ethiopia. Using the variation in the impact of the intensity of the program and household survey data administered during the years 1996 and 2004 as pre- and post-program data, respectively, I estimate difference-in-differences model.

Overall the results suggest that the education program has increased the probability of enrollment in grade 1 by age 7, the legal enrollment age in Ethiopia, by more than 35%. It is also found out that the program has decreased age at enrollment in grade 1 by 4 months, which is about 3.6% decrease from its pre-program average age (at enrollment in grade 1) of 9.3 years. These estimates highlight an important role that access to school plays in inducing parents to enroll their kids in primary school at the legal enrollment age.

This dissertation enhances our understanding of the educational choices faced by households in low income countries where child labor is an important day to day reality. The two educational choices studied in detail are primary school attendance and the age at which a child enrolls in grade 1. In doing so, the dissertation employs appropriate methods and data sets from a representative low income country to investigate insufficiently explored aspects of human capital acquisition.

Moreover, it provides important policy implications by offering empirical evidence on the potential causes and implications of educational inequalities across siblings and by evaluating the effect of educational policy intervention on the timing of enrollment in primary school.

The results from essay 1 suggest that the probability of participation in child labor and the hours students spend studying vary by birth order. Hence, policies that attempt to narrow down educational inequalities through school expansion may not be effective if parents selectively send specific birth-order children to school. On the other hand, policies that aim at increasing household income decrease the child labor obligation placed on children and may decrease siblings' education inequality in poor households. The second essay, on the other hand, documents that an education intervention in Ethiopia, which was designed to increase enrollment rate, has increased the probability of on-time enrollment. This finding is consistent with the argument that access to school induces households to enroll their children in school at the legal enrollment age. Thus, improving communication networks and public transport may supplement policies that are designed to encourage households to enroll their children in primary school on time.

I. BIRTH ORDER AND CHILDREN'S TIME ALLOCATION

1.1 Introduction

Even if it is 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. The literature that attempts to decompose the sources of economic inequalities into between and within families differences – i.e., sibling correlation studies[1] – shows 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 neighborhood 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

[1] Studies that investigate the effect of family background on children's economic outcomes decompose the sources of economic inequalities into between and within family differences. These studies typically estimate sibling correlation in important economic outcomes such as earnings and schooling using analysis of variance. The idea is that sibling correlation is a summary measure of the effect of shared family and community background, and hence if siblings have more similar economic outcomes than randomly chosen unrelated individuals, then we expect higher sibling correlation.

total variances in completion of elementary school in rural Albania (Picard & Wolff, 2008). 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 & Prtner, 2004).

A potential explanation for differences in educational outcomes of siblings and their labor 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 & 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 labor and the others for school (Horowitz & Wang, 2004).

The widespread practice of child labor in developing countries[2] partly explains differences in the educational achievements of children in developing countries. One important feature of child labor in many developing countries is that it is not a full time activity. Rather, children participate in low intensive child labor 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 ideal (see Basu, 1999, for a survey of the literature on child labor). 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

[2] The report from International Labor Organization reveals that there were 153 million child laborers in the world in 2008 (Diallo, 2010). In Ethiopia, the focus of the present study, 37% of the children below 15 years reported working as their primary activity, while only 14% reported school attendance as their primary activity in 1999. Moreover, 12% of the children has started working by age 4 (Admassie, 2002). Putting aside its moral, psychological, and other non-economic costs, child labor interferes with children's human capital accumulation process. Prior empirical studies have shown that child labor decreases the probability of being in school, and for those who are in school, it hinders their educational achievement and decreases the hours students spend in school (Beegle et al., 2009; Ravallion & Wodon, 2000; Cavalieri, 2002; Boozer & Suri, 2001).

do not work at all. Parents allocate children's time between school attendance and child labor 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 essay, I investigate the effect of birth order on the probabilities of school attendance and participation in child labor. Since parents jointly allocate the child's time between these two activities, estimating a bivariate probit model is appropriate. 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 labor. 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 essay 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 labor, school attendance, and other activities.

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 labor market earnings. In low income countries, on the contrary, most studies suggest that later-born children achieve more years of schooling. 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 5^{th} child is observed only in families with at least 5 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 instrumental variable (IV) for family size and estimating IV models. In this essay, I attempt to mitigate endogeneity of family size by exploiting the fact that Ethiopian parents prefer boys to girls to construct an instrumental variable for family size. Specifically, the proportion of boys in the family is used to instrument family size and unobserved effect bivariate probit IV model of child labor and schooling choices are estimated.

Overall, the results reveal that an increase in birth order by one unit decreases the probability of child labor participation by 5 percentage point, whereas it has no effect whether the child attends school or not. 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 8 child age dummies are included to control for the age of the child, it is not age difference that is driving the results. Comparison of estimates from unobserved effect bivariate probit model and unobserved effect bivariate probit IV model suggest that endogeneity of family size potentially bias birth order estimates in school attendance regressions, but not in child labor regressions.

The remainder of the essay is organized as follows. The following section provides additional background information on the role of birth order, and Section 1.3 presents the theoretical framework. Section 1.4 describes the data, while Section 1.5 discusses the methodology, outlines the empirical approach, and presents the first stage estimates. The main results are reported in Section 1.6, and the last section concludes.

1.2 Literature Review

At first glance it may seem that when a child is born relative to his or her siblings does not matter at all. But for a number of economic and other reasons (discussed in Section 1.3), the birth order of the child has a significant and meaningful effect on important children's outcomes, including educational achievement and labor market earnings. 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 & Glauber, 2006; Booth & 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 labor (Ejrnaes & Prtner, 2004; Emerson & Souza, 2008; Edmonds, 2006).

The wealth model (Becker, 1991; Ejrnaes & Prtner, 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 labor is widely practiced 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 labor is an empirical one, but Edmonds (2006) and Emerson & Souza (2008) argue that it is based on the child's comparative advantage in school and child labor, which, in turn, depends on the child's endowment. Ejrnaes & Prtner (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.

[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 & Van, 1998).

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 instrumental variable model. The occurrence of twin births and siblings sex composition are the two widely used instrumental variables. Twinning is historically the most popular one; recently, however, following Angrist & Evans (1998), use of siblings sex composition is increasing in the literature. This may be partly because using twin births as instrumental variable 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 & Evans (1998) are the first to use siblings sex composition as exogenous variation in family size in their study of the causal effect of family size on the labor supply of mothers in the US. Following Angrist & 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 & 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 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 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.

This essay builds on Angrist et al. (2010) and uses siblings sex composition as exogenous source of variation in family size since Ethiopian parents prefer boys to girls. Given the history of war and less developed police force, particularly in rural areas, Short & Kiros (2002) argue, bravery and physical strength are highly valued in Ethiopian families. Since men supposedly have these essential features, Ethiopian parents prefer boys to girls.

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

1.3 Theoretical Model

Why We Expect Birth Order Effects

There are a number of reasons why we expect children's outcome to vary by birth order. First, children of different birth order face different household environment. Probably the most obvious one is differences in household size; particularly, we expect later-born children to reside in relatively larger families, and this reduces the total parental time they receive. It is also obvious that children of different birth order face a household with different age and sex composition, which in turn have their own effects on children's outcome. Another interesting difference that different birth order children experience in the household is its intellectual environment. Zajonc (1976) argues that earlier born children have an advantage since they grow up in a household with better intellectual environment, i.e., higher average education.

Second, credit constraint induces birth order effects. If parental income increases over their life time, later-born children reside in relatively richer families. On the other hand, imperfect credit market forces families to decrease per child spending with family size. Credit constraint also interacts with child labor. It is not uncommon for credit constrained families to supplement the family income with income from child labor, 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 where, for instance, children are considered as security for old age, parents may favor earlier-born children as they become economically independent earlier (Horton, 1988). Even if parents equally care for their children, birth order effects exist if endowments differ by birth order. For instance, if earlier-born children are well endowed, parents invest more human capital on earlier-born children and compensate later-born, and less endowed, children by investing more nonhuman capital (e.g., bequest) as predicted by Becker & Tomes (1976).

Fourth, later-born children are biologically disadvantaged as they are born with older mothers who are more likely to give low birth weight babies.

Theoretical Model

To sketch the potential effect of birth order on child labor and school attendance choices, consider the simplified version of models developed by Edmonds (2006) and Baland & Robinson (2000). Assume a unitary family where its members live for only two periods, and parents equally care for their children but are too poor to leave bequests. Assume there is no capital market, and transfer from children to parents is not allowed as well. Also, assume there are n children in the household and they are identical except in their birth order and other attributes related to birth order such as age.

In the first period, child i (with birth order b_i) spends e_i hours in school and works for l_i $(= 1 - e_i)$ hours. Parents, on the other hand, supply inelastic labor of L_p hours at a competitive market wage of w_p. Child wage is a function of birth order as $w(b_i)$.[5] The total household consumption in period 1 is:

$$C = w_p L_p + \sum w(b_i)(1 - e_i). \tag{1.1}$$

In period 2, when children leave home and form independent households, their consumption depends on the human capital they accumulated in period 1. The human capital accumulation process is represented by $h(e_i, b_i)$. The birth order enters in the function to capture factors that vary by birth order and also influence

[5] Edmonds (2006) and Emerson & Souza (2008) argue wage decreases with birth order since older children are more productive. This, however, does not necessarily reflect birth order effect. Hence, in this essay, it is only assumed that wage varies by birth order.

the human capital production such as ability, but there is no strong theoretical ground to suggest the direction of the relationship. Thus, the sign of $\frac{\partial h}{\partial b_i}$ is ambiguous. However, h is assumed to be twice differentiable, strictly increasing ($\frac{\partial h}{\partial e_i} > 0$), and concave ($\frac{\partial^2 h}{\partial e_i^2} < 0$) in e.

Assuming parents' labor supply and adult wage are constant across periods, children and parents' consumption in period 2 are respectively,

$$c_i = h(e_i, b_i) \tag{1.2}$$

and

$$c_p = w_p L_p. \tag{1.3}$$

Over the course of their life, parents derive utility from total household consumption in period 1 (C), their consumption in period 2 (c_p), and the sum of their children's consumption in period 2 ($\sum c_i$). It is summarized by the following relations:

$$\begin{aligned} U &= u^1 + u^2 \\ u^1 &= u(C) \\ u^2 &= u(c_p) + u(\sum c_i), \end{aligned}$$

where the superscripts denote the two periods. Therefore, parents maximize

$$U = u(C) + u(c_p) + u(\sum c_i) \tag{1.4}$$

subject to equations (1.1), (1.2), and (1.3). The resulting first order conditions with respect to e_i are

$$-\frac{\partial u}{\partial C}w(b_i) + \frac{\partial u}{\partial \sum c_i}\frac{\partial h}{\partial e_i} = 0; \quad i = 1 \ldots n.$$

Rearranging the first order conditions gives us

$$\frac{\frac{\partial u}{\partial C}}{\frac{\partial u}{\partial \sum c_i}} = \frac{\frac{\partial h}{\partial e_i}}{w(b_i)} \tag{1.5}$$

The left hand side of equation (1.5) does not vary across siblings. Thus, at equilibrium, for any two children in the household, wage adjusted marginal returns to education are equal, i.e., for any two siblings i and j,

$$\frac{\frac{\partial h}{\partial e_i}}{w(b_i)} = \frac{\frac{\partial h}{\partial e_j}}{w(b_j)}. \tag{1.6}$$

Equation (1.6) tells us that parents allocate children's time across labor market obligations and education opportunities based on siblings' comparative advantage. Hence, for time allocation to vary by birth order, marginal returns to education and child labor should vary by birth order. For example, if a first-born child is more talented and has higher returns to education, satisfying the first order condition (equation (1.6)) requires the child to spend more time in school. If, on the other hand, the first-born child commands higher wage, the child spends more time working.

1.4 Data

In this essay, I 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 in 2002, 2,000 one year old children (hereafter "younger" cohort) and 1,000 eight years old children (hereafter "older" cohort) were surveyed in each country. Following up, in 2006 and 2009, the same children were tracked and surveyed when the "younger" cohort children turned to five and eight years old, and the "older" cohort children turned to twelve and fifteen years old, respectively. I 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 eight years old) to go to school at the time of the 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: Addis Ababa, Amhara, Oromia, SNNPR (Southern Nations, Nationalities, and People's Region), and Tigray (see Wilson et al., 2006, for a discussion on the sampling design). The data contain a wealth of information on children, household demographics, and community characteristics.

In 2006 and 2009 survey rounds, eight activities were identified and the number of hours children between the age of 5 and 17 years spend on each of these activities in the last week is reported.[7] This enables me 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

[6] Though the legal school starting age is 7 in Ethiopia, it is not uncommon for most children in developing countries like Ethiopia to delay primary school enrollment by few years beyond the legal school starting age (Barro & Lee, 2000).

[7] The eight activities included in the surveys are: domestic work (fetching water, fetching firewood, cleaning, cooking, washing, shopping, etc), unpaid work (family farm, cattle herding, shepherding and other family businesses), paid work (activities for pay/sale outside of household), caring for others (younger siblings and ill household members), school (including traveling time to school), studying (outside of school time such as at home or extra tuition), playing (including time taken for eating, drinking and bathing) and sleeping.

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 Labor Organization's (ILO's) Convention No. 138 specifies 15 years as the age above which a person may participate in economic activity. I 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. This leaves us with the final sample size of 1,919 children.

The two dependent variables in the bivariate probit model estimated in this essay (see Section 1.5 for detail) are binary indicators for school attendance and child labor participation, where school attendance is 1 if the child attends school, and 0 otherwise. Similarly, child labor participation takes a value of 1 if the child spends more than 14 hours per week on noneconomic activities such as household chores, and 0 otherwise.[8] Table 1.1 presents the fraction of children who attend school and participate in child labor.[9] About 89.8% of the children in the sample attend school, and of those who attend school, 77.7% participate in child labor. On the other hand, 78.6% of children in the sample participates in child labor, and among these children, only 11.3% do not attend school. As mentioned earlier, the table confirms that child labor 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 ideal.

Though child labor is common in Ethiopia, it is important to note that working for pay is not that common. In our sample, only 8% (not reported here) of children work for pay. The remaining 48%, 38%, and 7% of children, respectively, involve in domestic work such as cooking, caring for their younger siblings and/or ill household members, and participate in unpaid family work such as cattle herding.

[8] The 14 hours per week cutoff is chosen to be in line with ILO's definition of "light work" which is working for 14 hours per week or less on noneconomic activities.

[9] Table A.2 in Appendix A.2 provides marginal and joint frequencies for school attendance and child labor.

There is also child labor specialization by gender where girls tend to specialize in domestic work and caring for others while boys specialize in unpaid work (see Table A.3 in Appendix A.2 for a summary of child labor specialization by gender). Haile & Haile (2012) also find out child labor specialization in rural Ethiopia where girls are more likely to participate in domestic chores while boys participate in market work.

Table 1.1: Fraction of Children Who Attend School and Participate in Child Labor

	Child Labor					
School Attendance	**No**		**Yes**		**Total**	
	Row %	Col %	Row %	Col %	Row %	Col %
No	12.8	6.1	87.2	11.3	100.0	10.2
Yes	22.3	93.9	77.7	88.7	100.0	89.8
Total	21.4	100.0	78.6	100.0	100.0	100.0

Note: Figures in the body of the table are conditional probabilities; marginal probabilities are reported under "Total" row and column.

Birth order, the primary independent variable of interest, is constructed as a continuous variable containing the birth order of (resident) children as 1, 2, 3, 4, etc. Thus, the coefficient estimate of birth order tells us the change in the probabilities of school attendance and child labor participation for one unit increase in birth order. The average birth order in the sample is approximately 3 which is expected given the average number of kids in the family is about 5. (see Table 1.2 for descriptive statistics)

The proportions of children attending school and participating in child labor vary by birth order. Generally speaking, the probabilities of school attendance and participation in child labor decreases with birth order (See Figure 1.1). This is expected in non-adjusted relationship between birth order and school attendance/child labor as age decreases with birth order and it is less likely for younger kids either to attend school or participate in child labor.

Figure 1.1: Fraction of Children Who Attend School and Work by Birth Order

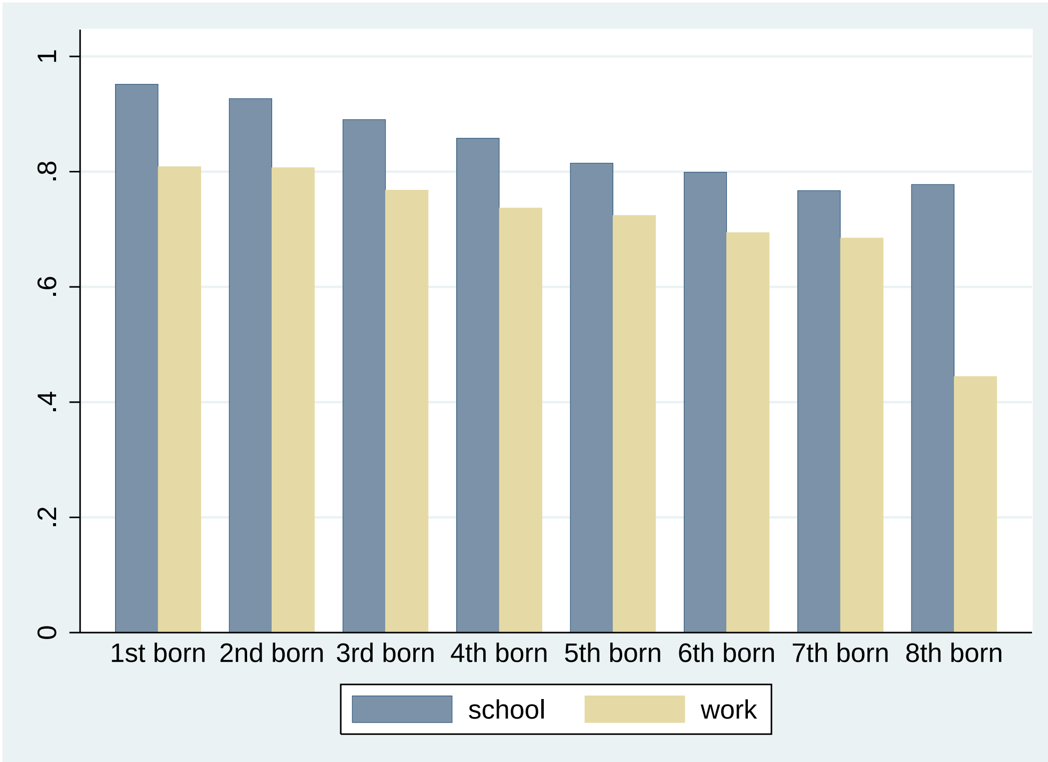


Table 1.2 presents the summary statistics of demographic variables that are included as additional explanatory variables in the regression analysis.[10] Generally speaking, parental years of schooling, which controls for the socioeconomic status of the family, shows that parents in the sample are less educated, with father's and mother's years of schooling of 4 and 2, respectively. A binary indicator for housemaid is also included as control variable since the presence of a housemaid may reduce the child's labor obligation at home. In addition, I control for annual family expenditure, which is a good proxy for permanent family income. Table 1.2 also presents the proportion of children by 8 age dummies, gender, and location (urban versus rural). Finally, note that 19 village dummies are also included as additional control variables in the regression analysis.

[10] See Table A.1 in Appendix A.2 for detailed description of variables used in the regression analysis.

Table 1.2: Summary Statistics of Variables used in the Econometric Analysis

	2006	2009
birth order	3.452	2.978
	(1.634)	(1.419)
number of kids	5.367	5.207
	(1.746)	(1.707)
proportion of boys in the HH	0.507	0.506
	(0.220)	(0.219)
Child's age = 7	0.108	0.000
	(0.310)	(0.000)
Child's age = 8	0.129	0.000
	(0.335)	(0.000)
Child's age = 9	0.125	0.005
	(0.331)	(0.073)
Child's age = 10	0.102	0.109
	(0.303)	(0.312)
Child's age = 11	0.238	0.119
	(0.426)	(0.324)
Child's age = 12	0.297	0.136
	(0.457)	(0.343)
Child's age = 13	0.001	0.102
	(0.032)	(0.303)
Child's age = 14	0.000	0.229
	(0.000)	(0.421)
Child's age = 15	0.000	0.299
	(0.000)	(0.458)
child is a girl (yes=1)	0.482	0.474
	(0.500)	(0.500)
housemaid (yes=1)	0.058	0.080
	(0.234)	(0.272)
father's schooling	3.824	3.881
	(4.020)	(4.034)
mother's schooling	2.278	2.284
	(3.431)	(3.442)
household expenditure	0.974	1.784
	(0.744)	(1.197)
urban (yes=1)	0.307	0.311
	(0.462)	(0.463)
Observations	986	933

Standard deviations in parentheses.

1.5 Empirical Methodology, Identification, and First Stage Estimates

This section is organized as follows: the following subsection outlines the empirical methodology. Subsections 1.5.2 and 1.5.3 respectively discuss the first stage IV results and validity of the instrument.

1.5.1 Empirical Methodology

The empirical objective here 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 labor, and parental utility differs by alternative allocations. This gives us four possible combinations of children's activities: children who are not enrolled in school and not working, those who are in school and not working, those who are not in school but working, and those who are in school and also working.

Since parents jointly allocate the child's time between child labor and school attendance, unobserved effect bivariate probit model is estimated using maximum likelihood procedure. The bivariate probit model consists of two equations: the first equation contains the school attendance (s_{it}) probability, and the second one is the probability of working as child laborer (l_{it}). Following Cameron & Trivedi (2005), let us define the latent parental utility from allocating child $i's$ time on school and child labor in year t, respectively, by

$$s^*_{it} = \delta_s b\_order_{it} + \gamma_s family\_size_{it} + \boldsymbol{\beta}_s \mathbf{X}_{it} + \alpha_{is} + \epsilon_{its}, \tag{1.7}$$

$$l^*_{it} = \delta_l b\_order_{it} + \gamma_l family\_size_{it} + \boldsymbol{\beta}_l \mathbf{X}_{it} + \alpha_{il} + \epsilon_{itl}, \tag{1.8}$$

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 random error terms. Assume that ϵ_{it} are jointly and normally distributed with means zero, variances 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 probit model. If $\rho \neq 0$, bivariate probit model is appropriate.

The four possible outcomes can now be restated as (s_{it}, l_{it}) equals $(0,0)$ for children not enrolled in school and not working, $(1,0)$ for children enrolled in school and not working, $(0,1)$ for children not enrolled in school but working, and $(1,1)$ for children enrolled in school and also working.

I am primarily interested to estimate δ_s and δ_l in equations (1.7) and (1.8), the coefficient estimates of birth order in school attendance and child labor equations, respectively. 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 (1.7) and (1.8). 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 5^{th} child is observed only in families with at least 5 children. Endogeneity of family size can be mitigated by finding appropriate instrumental variable for family size and estimating instrumental variable (IV) models.

In the context of estimating linear models using IV approach, family size is first regressed on instrumental variables, $\mathbf{Z}$ in equation (1.9), and other control variables, then equations (1.7) and (1.8) are estimated after replacing the observed family size in equation (1.7) and (1.8) by its predicted value from equation (1.9).

The first stage equation takes the form:

$$family\_size_{it} = \eta_0 + \boldsymbol{\eta}_1 \mathbf{Z}_{it} + \eta_2 b\_order_{it} + \boldsymbol{\eta}_3 \mathbf{X}_{it} + \psi_i + \mu_{it}, \tag{1.9}$$

where μ_{it} is the random error term and ψ_i is time invariant unobserved individual heterogeneity.

For the IV estimate to mitigate the bias due to endogeneity of family size in equations (1.7) and (1.8), the excluded instruments, **Z** in equation (1.9), should be strongly correlated with the endogenous variable, family size, but not with the residuals of equations (1.7) and (1.8), ϵ_{its} and ϵ_{itl}. Bound et al. (1995) show that if the excluded instrument is weakly correlated with the endogenous variable, then even a weak correlation between the excluded instrument and the residual of the structural equation, ϵ_{its} and ϵ_{itl} in equations (1.7) and (1.8), induces large inconsistency in the IV estimates. Hence, it is crucial to implement an IV methodology where the excluded instruments are both strongly correlated with family size in equation (1.9) but not with the residuals of equations (1.7) and (1.8).

Children's sex composition is a potential candidate to instrument 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) (see, for example, Angrist & 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 (see, for example, Angrist et al., 2010; Short & 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.[11]

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 labor 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.[12]

In the standard two-stage least square (2SLS) regression, the first step is to estimate equation (1.9), the first stage equation. Then, equations (1.7) and (1.8),

[11] Some argue (e.g., Williamson, 1976) 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.

[12] By construction, family size appears on both sides of equation (1.9): as a dependent variable and a denominator of the excluded variable, proportion of boys in the household. Generally, this could lead to a well know bias in labor 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.

the second stage equations, are estimated after replacing the observed family size by its predicted value from equation (1.9). In the context of non-linear second stage equation, Terza et al. (2008) show that IV estimates obtained from 2SLS regression are inconsistent. They, hence, suggest two-stage residual inclusion (2SRI). The procedure in 2SRI and 2SLS are the same except that in 2SRI the endogenous variable is not replaced by its predicted value in the second stage equation. Instead, the predicted residual from the first stage regression is included as an additional variable in the second stage equation.

Since the outcome variables in equations (1.7) and (1.8) are dummy variables and the two equations are modeled as bivariate probit, 2SRI procedure is employed here, i.e., equations (1.7) and (1.8) are estimated where the observed family size is not replaced by its predicted value, instead the predicted residual of equation (1.9) is included as an additional control variable.[13]

1.5.2 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.[14] Table 1.3 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

[13] See the technical note in Appendix A.1 for further description of the specification of the bivariate probit model and the implementation of the IV approach in this specification.

[14] See Table A.4 in Appendix A.2 for the fraction of families that has additional child by parity.

non-government organizations. The latter is used to proxy family planning use, which I 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 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 column 2 and 4 of Table 1.3) to see if 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 have sons only have 2.5 fewer children than those that have daughters only.[15] 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,

[15] Ethiopia is characterized by high fertility rate, with, for example, more than 5 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.

Table 1.3: First Stage Regression Results from the Linear Model
Dependent Variable: Number of Kids

	Pooled OLS		Fixed Effect	
	Reduced IV	Full IV	Reduced IV	Full IV
proportion of boys	-0.235	-0.217	-2.463**	-2.483**
	(0.188)	(0.187)	(0.983)	(0.986)
support		0.356***		0.044
		(0.094)		(0.067)
birth order	0.712***	0.715***	1.014***	1.014***
	(0.024)	(0.024)	(0.030)	(0.030)
child is a girl (yes=1)	0.053	0.054		
	(0.078)	(0.077)		
housemaid (yes=1)	0.618***	0.631***	0.734***	0.733***
	(0.182)	(0.181)	(0.241)	(0.241)
father's schooling	0.039***	0.040***	-0.026	-0.022
	(0.014)	(0.014)	(0.046)	(0.047)
mother's schooling	-0.061***	-0.060***	-0.237	-0.236
	(0.015)	(0.015)	(0.155)	(0.155)
household expenditure	0.243***	0.244***	0.030	0.030
	(0.049)	(0.049)	(0.026)	(0.026)
urban (yes=1)	0.251	0.222	0.257	0.282
	(0.237)	(0.228)	(0.306)	(0.300)
Constant	1.260***	1.244***	3.391***	3.368***
	(0.351)	(0.348)	(0.750)	(0.757)
Child age Dummies	Yes	Yes	Yes	Yes
Village Dummies	Yes	Yes	Yes	Yes
Year Dummy	Yes	Yes	Yes	Yes
Observations	1864	1864	1864	1864
R-sq	0.518	0.522	0.636	0.636

Standard errors in parentheses. *$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
The two IVs presented in column 2 and 4 are jointly significant at 5% level.
Proportion of boys and support variables in the table respectively denote the proportion of boys in the family and a binary indicator for whether a family received support on family planning.

the coefficient estimate of the proportion of boys remains the same whether I 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 1.3) are saved and used as additional control variable in the second stage regressions in Section 1.6.

1.5.3 Validity of the Instrument

Are Boys Better Off?

One important feature of an instrumental variable 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 if the proportion of boys in the household (the instrumental variable) directly affects participation in child labor and/or school attendance (the dependant 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 1.4 presents a simple check whether school attendance and/or participation in child labor 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 1.4, 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 1.4) and more likely to work (lower panel of Table 1.4). However, the differences are not statistically significant.

Table 1.4: 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

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 a serious concern, but a simple check on birth space is conducted to see if there is sex selective abortion in the data. If parents selectively abort female fetuses, the birth space 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 1.5 compares birth space between consecutive children by proportion of boys in the household. The table depicts that the average birth space 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 1.5: Birth Space (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	0.0898
Std. Err.	0.750	0.525	
No. of Obs.	1305	1832	
p-value			0.922

Is there Differential Mortality Rate Across Gender?

If infant (less than 1 year old) and child (less than 5 years old) 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, then results should be interpreted carefully. However, remember that fixed effect model is estimated in the first stage regression. Thus, even if mortality rates are non-random, they do not render our IV invalid as long as they remain constant between the two survey years, i.e., 2006 and 2009.

1.6 Results

Different models are estimated to investigate the effect of birth order on the probabilities of school attendance and child labor, and the summary of the birth order estimates are presented in Table 1.6. The estimated models vary depending on whether it is assumed school attendance and child labor 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 labor decisions are made jointly,[16] I primarily focus on discussing the results from bivariate probit models which are reported in the lower half of Table 1.6. The regression outputs for models

[16] Note that the coefficient estimate of ρ in the bivariate probit model reported in Table A.7 in Appendix A.2 is significant at 1% level, implying bivariate probit model is a better fit than univariate independent probit models.

reported in the last two rows of Table 1.6 are presented in Table 1.7, whereas that of the other models reported in Table 1.6 are presented in Appendix A.2.

Table 1.6: Summary of Estimates of Coefficient and Average Marginal Effect of Birth Order from Different Models

		School	Work
Independent Probit Models			
Pooled Probit	Coef.	-0.028	-0.186
	p-value	(0.552)	(0.000)
	AME	-0.004	-0.037
	LL	-455	-667
Unobserved Effect Probit	Coef.	-0.032	-0.193
	p-value	(0.610)	(0.000)
	AME	-0.003	-0.036
	LL	-449	-666
Unobserved Effect Probit IV	Coef.	0.389	-0.242
	p-value	(0.040)	(0.013)
	AME	0.012	-0.020
	LL	-663	-1146
Bivariate Probit Models			
Pooled Bivariate Probit	Coef.	-0.030	-0.188
	p-value	(0.520)	(0.000)
	AME	-0.004	-0.038
	LL	-1119	–
Unobserved Effect Bivariate Probit	Coef.	-0.026	-0.252
	p-value	(0.672)	(0.000)
	AME	-0.002	-0.049
	LL	-1168	–
Unobserved Effect Bivariate Probit IV	Coef.	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 labor, while LL represents the log likelihood.

The birth order estimates in child labor equations are uniformly negative and significant across models (see Table 1.6), 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 1.6), suggesting that endogeneity of family size is not a serious concern in estimating child labor equation. This is also implied by the insignificant coefficient estimate of the first stage residual in the unobserved effect bivariate probit IV regression which is reported in Table 1.7.

In my preferred model which assumes school attendance and child labor decisions are made jointly and which accounts for endogeneity of family size (i.e., unobserved effect bivariate probit IV model), the average marginal effect of birth order on the probability of child labor 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 labor by about 5 percentage point. The finding that later-born (i.e., younger) children are less likely to participate in child labor than their earlier-born siblings is consistent with prior findings in the literature (see, for example, Emerson & 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 labor, it is important to assess the distribution of the marginal effect since marginal effect is not constant in non-linear models. Figure 1.2, therefore, presents the distribution of the estimated marginal effect of birth order on the probability of child labor participation. As can be seen from the figure, the probabilities are always non-positive, ranging from -10% to 0; besides, it has a bimodal distribution with spikes around -10% and 0. This suggests that there may be differential birth order effect on the probability of child labor participation across different groups of the population.

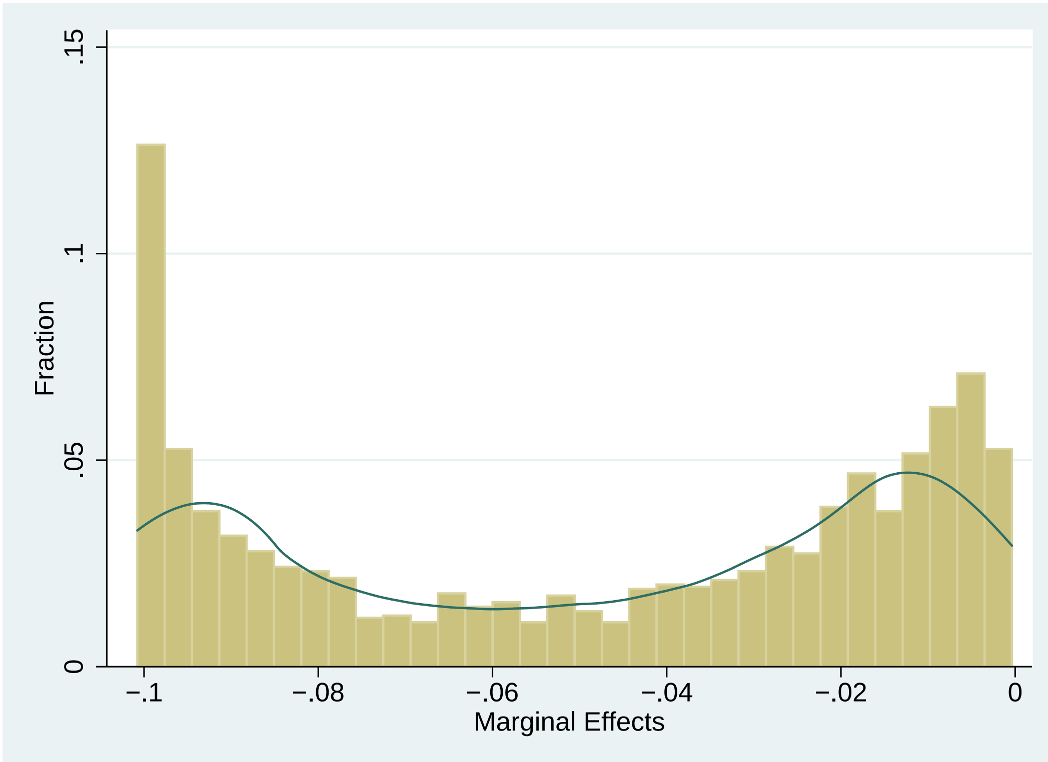
Contrary to the fact that the birth order estimates are uniformly negative and significant across models in child labor regressions, its estimates in the *school attendance* regressions differ both in magnitude and significance across models. Generally, it is negative and insignificant in models which do not control for

Table 1.7: Unobserved Effect Bivariate Probit Estimates of School Attendance and Child Labor Equations

	Bivariate Probit Model			Bivariate Probit IV Model		
	Coef.	AME	SE	Coef.	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)
child is a girl	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)
annual expenditure	0.034	[0.003]	(0.12)	0.022	[0.002]	(0.12)
urban (yes=1)	1.658***	[0.153]	(0.41)	1.781***	[0.163]	(0.43)
1^{st} stage residual				0.191**	[0.018]	(0.10)
Child Labor:						
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)
child is a girl	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)
annual expenditure	-0.042	[-0.008]	(0.05)	-0.040	[-0.008]	(0.05)
urban (yes=1)	-0.703***	[-0.136]	(0.24)	-0.706***	[-0.137]	(0.24)
1^{st} stage residual				-0.010	[-0.002]	(0.06)
Observations	1862			1860		
Log likelihood	-1168.373			-1167.288		

*p < 0.10, ** p < 0.05, *** p < 0.01.
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.

Figure 1.2: Histogram and Kernel Density Estimates of Marginal Effects of Birth Order on Child Labor

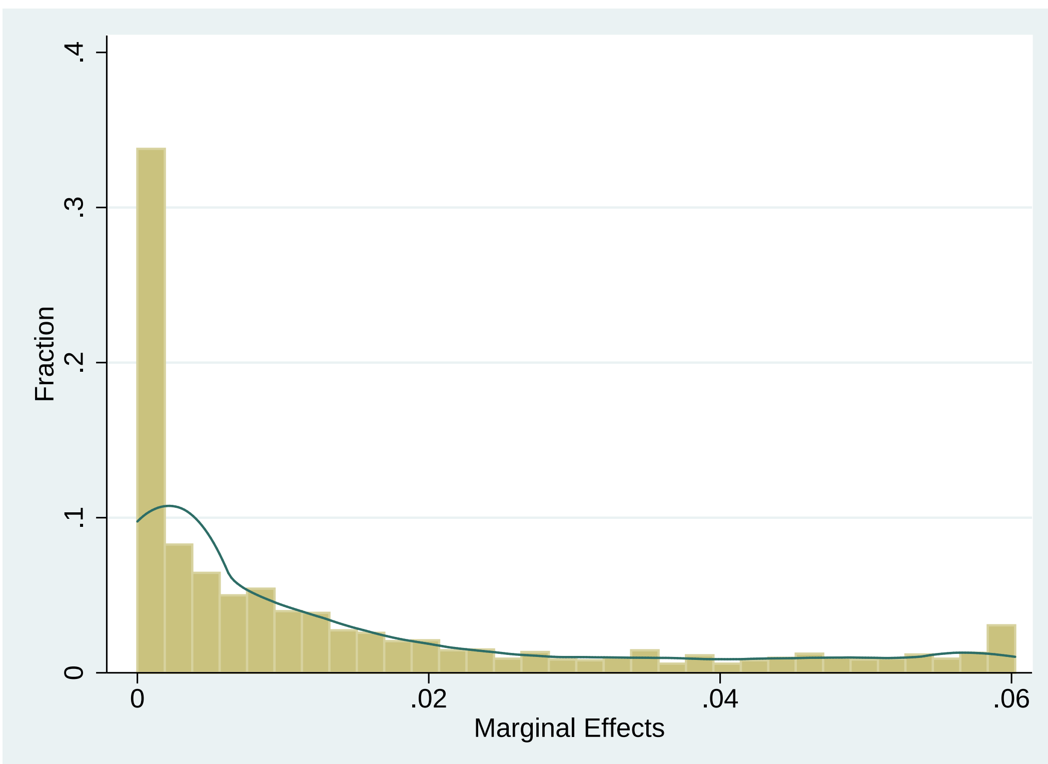


endogeneity of family size. Once endogeneity of family size is controlled for in the IV models, the birth order coefficient has become positive and significant in unobserved effect probit IV model, with estimated average marginal effect of 0.012, implying younger kids are 1.2 percentage point more likely to attend school than their older siblings. However, in my preferred model, unobserved effect bivariate probit IV model, the birth order estimate is positive but not significant ($p - value = 0.124$).

As Table 1.7 depicts 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 unobservables that affect parents' choice of family size seem to affect parents' decision whether to send the 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.

Though my 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 non-negative for each child, ranging from 0 to 6 percentage point (see Figure 1.3 for the distribution of the estimated marginal effect). Remember that only about 10% of children in the sample do not attend school, and this might have contributed in making the coefficient estimate of birth order in school attendance equation insignificant.

Figure 1.3: Histogram and Kernel Density Estimates of Marginal Effects of Birth Order on School Attendance



Note that 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 labor 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 labor.

If school performance measures such as test scores are observed in the data, we can check if the data supports 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, I could have used age adjusted grade to measure school performance as used in prior studies (see, for example, Horowitz & 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 enrollment by few years beyond the legal school starting age (Barro & 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 enrollment on years of schooling. Thus, I resort to assessing if 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 fixed effect model of the effect of birth order on hours students spend studying is estimated, and the results are reported in Table 1.8.[17] Column 1 of Table 1.8, which is estimated by restricting the sample to all children who are going to school, 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 laborer (see column 2 of Table 1.8). But, when I restrict

[17] See Figure A.1 in Appendix A.3 for unadjusted relationship between birth order and the time spent in school and working.

the sample further to children who are going to school but *not working* as child laborer (column 3 of Table 1.8), 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.

Table 1.8: Linear Fixed Effect Estimates of Hours Students Spend Studying

	All Students		Working Students		Non-working Students	
	Coef.	SE	Coef.	SE	Coef.	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 (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 (yes=1)	-0.590	(0.76)	-0.994***	(0.28)	0.223	(0.66)
1^{st} 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-sq	0.052		0.068		0.305	

*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.

The positive relationship found here between birth order and hours spent studying is consistent with the finding that child labor 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 & Prtner (2004) find out that first-borns spend 10 more hours in 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 laborer indicates that child labor crowds out study hours.

Finally, note that 8 child age dummies (with 7 years 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 8 child age dummies are positive and significant in both equations (see Table 1.7). Besides, 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 labor, but father's schooling increases the probability of school attendance. Mother's schooling, nevertheless, has 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 labor. This may be because I controlled for father's and mother's years of schooling, which are proxies for the socioeconomic status of the household.

1.7 Conclusion

It is well known to economists that parental action creates education inequalities among children (Becker & 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 labor is widely practiced. 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 labor obligations and school opportunities. In this essay, I investigate the role the birth order of the child plays whether the child attends school or participates in child labor.

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. I 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 labor 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 labor participation by 5 percentage point, but I 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 8 child age dummies are included to control for the age of the child, it is not age difference that is driving the results. The results obtained here can be generalized to other developing countries which have similar socio-economic environments as that of Ethiopia, i.e., high incidence of child labor, 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 labor participation and hours spent studying across different birth order children, birth order effects tend to work against programs that reduce inequalities in education and income. For example, in developing countries, where child labor is widely practiced and access to school is limited, school expansion may increase the overall level of education. While increasing education levels, child labor 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 labor. Programs that aim to increase household income among resource-constrained households through income transfers or other means may mitigate siblings' educational inequality.

II. THE EFFECT OF ACCESS TO PRIMARY SCHOOL ON THE TIMING OF SCHOOL ENROLLMENT: ANALYSIS OF THE ETHIOPIAN EDUCATION REFORM

2.1 Introduction

One of the main features of the education system in developing countries is that the majority of students enroll in primary school long after the legal enrollment age, which is usually around 6 or 7 years. Barro & Lee (2001) find out that at least 50% of the students enrolled in grade 1 in 31 countries are older than the legal enrollment age. In Ethiopia, a country where the data for this study come from, the 2004 Welfare Monitoring Survey data show that more than 80% of children in rural areas enrolled in grade 1 after the legal enrollment age of 7. A number of other studies documented the presence of delayed primary school enrollment throughout the developing world (Bommier & Lambert, 2000; Glewwe & Jacoby, 1995; Wils, 2004; Moyi, 2010; Todd & Winters, 2011).

The standard human capital investment models fail to explain the widely observed delayed primary school enrollment as they predict that an individual invests in education in the early period of his/her life, and reaps its benefits later in life. Besides, in communities where child labor is a common practice and most of the work children are expected to perform are physically demanding, it is optimal for parents to enroll the child as early as possible since the value of the child's time is lower when the child is younger. There are evidences that suggest delaying primary school enrollment is costly. In Ghana, for example, Glewwe & Jacoby

(1995) calculated that delaying primary school enrollment by 2 years beyond 6 years, the legal enrollment age, costs an individual about 6% of his/her life time wealth. Also, children who enroll in school late have higher grade repetition and school dropout rates, and complete fewer years of schooling than those who enroll at the legal enrollment age (Wils, 2004). Given the high cost associated with delaying primary school enrollment, it is not well understood why most parents in developing countries enroll their children long after the prescribed age.

The bulk of the literature in this area focuses on the probability of enrollment, without considering age at enrollment. However, delayed enrollment cannot be tackled by general policies that are designed to increase enrollment rates since delayed enrollment is not confined to countries that have lower enrollment rates (Moyi, 2010; Lloyd & Blanc, 1996). Very few studies analyzed why students in developing countries delay primary school enrollment. Loosely speaking, the explanations these studies provided can be grouped into three: poor child health, liquidity constraint, and limited (or lack of) access to school.

Poor child health slows down the child's development process and renders the child less ready to attend school at the legal enrollment age. Hence, at legal enrollment age, say, a malnourished child would be too weak to be able to walk the (typically longer) distance to school (Bommier & Lambert, 2000; Partnership, 1999). Besides, poor child health lowers the learning ability of the child and thus it is optimal to delay enrollment until the negative effect of poor child health on mental readiness decreases after a few years when the child gets older (Glewwe & Jacoby, 1995). A liquidity constraint explanation, on the other hand, suggests that resource constrained families might need to employ the child in family activities until the family accumulates sufficient saving to finance the child's schooling (Jacoby, 1994). Finally, if there is limited access to schooling, school officials may ration enrollment in primary school, and the rationing tends to favor older children who are typically on the waiting line for a relatively longer time (Bommier & Lambert, 2000). Note that shortage of schools means schools are widely dispersed and we expect children to walk for a relatively longer distance. Since malnourished children are too weak to walk for longer distance to school, school shortage may interact with child health and have differential impact across children on the health distribution.

Most families in developing countries, particularly those in rural areas, do not have access to primary schools. In recent years, however, many developing countries have made primary schools more accessible. There are evidences that suggest making schools more accessible has increased primary school enrollment, but we know little about the effect of access to school on the timing of enrollment. This essay, thus, attempts to bridge this gap in the literature by offering empirical evidence on the effect of access to school on the timing of primary school enrollment using a household survey data from Ethiopia.

One of the empirical challenges of assessing the effect of access to school on the timing of enrollment is endogeneity of access to school; that is, families that live closer to school may be inherently different and their children may enroll in school on time regardless of their proximity to school. In situations like these, most researchers attempt to mitigate the bias by either finding appropriate instrumental variable or using a "natural experiment" that affects the endogenous variable but not the outcome variable. Some prior studies exploit government programs as exogenous source of variation in economic variables. For example, Todd & Winters (2011) and McEwan (2013), respectively, exploit the government programs in Mexico (called *Oportunidades*) and Chile as exogenous source of variation in child health to investigate the effect of child health on the timing of school enrollment. This study employs a similar approach and uses an education policy shock that happened in Ethiopia between the mid 1990s and mid 2000s as exogenous source of variation in access to primary school.

The Ethiopian government has launched a series of five-year Education Sector Development Programs (ESDPs) since 1997 with a prime objective of achieving universal primary education by 2015. To date, 4 five-year ESDPs have been implemented. During the first two ESDPs that covered 8 academic years between 1997/98 and 2004/05, 2,398 new primary schools were built (World Bank, 2005). The program has substantially decreased distance to primary school at a national level from its average of 2.73 Km in 1996 to that of 1.25 Km in 2004. Though such a large number of primary schools were built in a short period of time and the program has substantially decreased distance to primary school at a national level, the decrease in distance to primary school vary widely across states and zones[1]. For example, distance to primary school has decreased by a 100% in East Wellega zone while the decrease was only 2.81% in South Gondar zone during the same period.

I exploit the variation in the intensity of the impact of the program across states to identify the causal effect of access to primary school on the probability of enrollment in grade 1 by age 7, the legal enrollment age. Narrowing down education inequalities across states by building more schools in rural and under-served areas was at the core of the program's objective. In fact, the program explicitly targeted increasing primary school enrollment from its 30% national average at the beginning of the program to at least 50% by the end of the program. Thus, we should expect more schools to be built in areas that had lower primary school enrollment rate in the pre-program period. Accordingly, states that had pre-program primary school enrollment rate below 30% are assigned into treatment group, whereas those states above 30% enrollment rate are assigned into control group. Then, difference-in-differences models are estimated where the dependent variable is a binary indicator for enrollment in grade 1 by age 7. To estimate the models, I use household survey data - called Welfare Monitoring Survey (WMS) data -

[1] Ethiopia is a federal country with three levels of governments: federal, state (or regional), and local governments. Zones are the lowest level of governments that are equivalent to US counties.

administered by Ethiopia's Central Statistical Agency during the periods 1996 and 2004. The main advantage of using data sets from these survey rounds is that they have information on important variables just before the beginning of the program (i.e., 1996) and around the end of the program (i.e., 2004).

The results from the difference-in-differences models reveal that the education program has increased the probability the child enrolls in grade 1 by age 7 by more than 35%. The results also suggest that the reform has decreased age at enrollment in grade 1 by about 4 months. These estimates highlight an important role that access to school plays in inducing parents to enroll their kids in primary school at the legal enrollment age.

The remainder of the essay is organized as follows. The following section briefly reviews the literature, and Section 2.3 describes the education reform in Ethiopia. Section 2.4 explains the data used in this essay and presents descriptive statistics. The impact of the education program on both access to school and primary school enrollment is discussed in Section 2.5. Section 2.6 presents the evidence on the impact of the education program on the timing of enrollment. While doing so, this section discusses the conceptual framework and the identification strategy, and finally it presents the econometric results. The last section concludes.

2.2 Literature Review

Delayed primary school enrollment is observed in a number of developing countries. For instance, it has been documented in most Sub-Saharan African countries (Barro & Lee, 2001), in Tanzania (Bommier & Lambert, 2000), in Ghana (Glewwe & Jacoby, 1995), in Mozambique (Wils, 2004), in Malawi (Moyi, 2010), and in Mexico (Todd & Winters, 2011). Contrary to the fact that delayed enrollment is common in most developing countries, there are limited studies on the topic.

These prior studies on the topic suggest a number of explanation why parents delay their children's enrollment in primary school. To mention few of them, first, malnutrition could cause delayed primary school enrollment. This is because malnutrition lowers children's learning ability and hence it is optimal to delay enrollment until the negative effect of malnutrition decreases after a few years when the child gets older (Glewwe & Jacoby, 1995). Using a policy intervention that improved child health in Mexico as exogenous source of variation in child health, Todd & Winters (2011) find out that early health and nutrition intervention has increased the probability a child enrolls on time in primary school. On the contrary, McEwan (2013) finds out that a similar policy intervention that made higher calorie meal available to vulnerable children in Chile has no effect on enrollment in grade 1 at the legal enrollment age. The author suggests this could be because the incidence of child malnutrition is lower in Chile, and most children in Chile enroll in school on time. On the other hand, in Ghana and Tanzania, Partnership (1999) found out that malnutrition, measured by height-for-age, delays enrollment in primary school.

Second, De Vreyer et al. (1999) models a household behaviour where households diversify their investment among three assets: physical assets, general human capital acquired through schooling, and specific human capital acquired through child labor. If the return to specific human capital at younger age is higher than that of general human capital, then parents do not send their children to school at the legal school enrollment age.

Third, delayed school enrollment could be the result of liquidity constraints. When households are resource constrained, a child might need to be employed in family activities until the family accumulates sufficient saving to finance the child's schooling (Jacoby, 1994).

Finally, delayed school enrollment could be the result of supply side problems. If there is shortage of school, school officials may ration enrollment in primary school, and the rationing tends to favor older children who are typically on the waiting line for a relatively longer time. On the other hand, shortage of school may mean students have to walk longer distance to school. In this case, delayed enrollment could be due to the fact that children may not be mature enough to walk the distance to school at the legal enrollment age (Bommier & Lambert, 2000). In societies where there is high incidence of child malnutrition, shortage of schools exacerbates the problem of delayed enrollment since developmentally stunted children take relatively longer time to be physically strong and be able to walk the longer distance to school. On the other hand, walking longer distance to school increases the propensity that a child walks through unsafe neighborhoods. Thus, parents that are concerned about the safety of their children may refrain from sending their children, especially their daughters, to school at the legal enrollment age.

Though access to school is one of the most important factors that determine the timing of enrollment, identifying its effect on the timing of enrollment is complicated by the relationship between school proximity, socioeconomic status, parental taste for education, and other characteristics that affect the timing of enrollment. For instance, being economically disadvantaged is correlated with poor taste for education and living further away from schools. All these factors affect the timing of enrollment, but they cannot be perfectly controlled in the regression framework. A credible identification of the effect of access to school on the timing of enrollment, thus, requires exogenous source of variation in access to school that does not affect the timing of enrollment.

In situations like these, government programs can be used as exogenous source of variation in the independent variable. For example, to test the hypothesis that malnutrition delays school enrollment, Todd & Winters (2011) used the government program in Mexico called *Oportunidades* as exogenous source of variation in child health. McEwan (2013) also used a similar intervention in Chile as exogenous source in the amount of calorie intake among children to identify the causal effect of child health on the timing of enrollment. I follow a similar approach and use the education reform that happened in Ethiopia between 1996 and 2004 as exogenous source of variation in proximity to school to identify the causal effect of access to primary school on the timing of enrollment.

2.3 The Education Reform in Ethiopia

Following the change in government in May 1991, Ethiopia has undergone a number of policy changes almost in each sectors of the economy. The education sector is one of the sectors that has gained the attention of the government since then. Consequently, it has undergone many policy changes and received a large and increasing budget share of the government. Among the many changes the sector experienced recently, the implementation of a series of five-year Education Sector Development Programs (ESDPs) is the major one. I exploit the variation in the intensity of the impact of the education program across districts to identify the causal effect of access to primary school on the timing of enrollment in grade 1.

The ESDPs started in 1997 with the objective of achieving universal primary education by 2015. Reducing educational inequalities by increasing access to primary school, mainly in rural and under-served areas, was at the core of the ESDPs. To date, 4 five-year ESDPs have been implemented. I will focus on the first two ESDPs in this essay as their duration align with the survey years of the data used in this essay.

The first ESDP covered five academic years between 1997/98 and 2001/02. Over the five years period of the first ESDP, it was planned to build 2,423 new primary schools, to upgrade 1,814 primary schools, and to renovate 1,220 primary schools in order to accommodate 3.9 million additional students (World Bank, 1998). The expected outcomes were substantial increase in access to primary school specially in rural areas where the majority of newly built schools were to be located. Moreover, it was expected to increase gross primary school enrollment rate from its 30% level by the beginning of the first ESDP to 50% by the end of the first ESDP.

The second ESDP also covered five academic years between 2000/01 to 2004/05. Note that the first two years of the second ESDP overlapped with the last two years of the first ESDP. Thus, in effect, the second ESDP had covered three *unique* academic years between 2002/03 and 2004/05. The reason for the overlap in the duration of the first and second ESDPs is to align the second and consecutive (i.e., third and fourth) ESDPs with the political election cycle and the five year term of the elected government in office. Though it was planned to built 2,423 primary schools during the first ESDP alone, a total of 2,398 new primary schools were built during the first two ESDPs, and, in line with the focus of the program, 86% of the new schools were built in rural areas (World Bank, 2005).

As the first two ESDPs covered 8 academic years between 1997/98 and 2004/05, household survey data collected in 1996 and 2004 are used in this essay so that the 1996 and 2004 data are, respectively, used as pre and post program data. The following section briefly discusses the data used in this essay and presents descriptive statistics.

2.4 Data

The analysis in this essay is based on household survey data called Welfare Monitoring Survey (WMS) data, which was administered by Ethiopia's Central Statistical Agency during the periods 1996 and 2004. The WMS is a cluster-based nationally representative repeated cross section household survey. The 1996 and 2004 WMS covered 11,569 and 36,303 households, respectively, and the surveys contain a wide range of information on household demographics, household assets, availability and use of different facilities (including schools), and other important economic variables.

For each household member aged five and above, I observe whether an individual was attending school during the survey years and a year prior to the survey years. I also observe the grade in which an individual was registered in these two consecutive years. Using this information, I restricted the sample to first time grade 1 enrollees in the two survey years. Since grade repetition is common in Ethiopia as it is in most developing countries, it is important to mention that one of the advantages of these data is that they allow us to observe first time grade 1 enrollees. Hence, bias from measurement error of age at enrollment - that can be caused by grade repetition - is not a serious concern here.

Table 2.1 presents descriptive statistics for a sample of children used in the econometric analysis.[2] The table shows that children in rural areas, on average, enroll in grade 1 at least 2.5 years after the legal enrollment age of 7. The extent of delayed enrollment in rural area is also reflected by the small proportion of children that were enrolled in grade 1 by age 7, which was 11% in 1996 and 18% in 2004. Similarly, a non trivial number of children in urban areas enroll in grade 1 after the legal enrollment age though delayed enrollment in urban areas is not as common as it is in rural areas. For instance, Table 2.1 depicts that only 50% and 53% of children in urban areas were enrolled in grade 1 by age 7 in 1996 and 2004, respectively. Moreover, children in urban areas delay enrollment in grade 1 by about a year in 1996 and 10 months in 2004. To summarize, a sizable proportion of children enroll in grade 1 few years after the legal enrollment age of 7 years. However, children in rural areas are more likely to delay enrollment, and when they do, they delay enrollment by more years than their urban counterparts.

[2] See Table **??** in the Appendix for detailed definition of variables used in the econometric analysis.

Table 2.1: Descriptive Statistics for a Sample of Children Who Were Enrolled in Grade 1 by Year and Location

	1996		2004	
	Rural	Urban	Rural	Urban
Enrolled in grade 1 by age 7 (yes=1)	0.111	0.498	0.176	0.528
	(0.314)	(0.501)	(0.381)	(0.500)
Age at enrollment	10.203	7.967	9.635	7.752
	(2.128)	(1.760)	(2.151)	(1.587)
Girl (yes=1)	0.304	0.474	0.452	0.534
	(0.461)	(0.501)	(0.498)	(0.500)
Birth order	2.538	3.414	2.509	2.859
	(1.334)	(1.936)	(1.414)	(1.646)
Household size	7.184	7.395	7.032	6.662
	(1.895)	(2.409)	(1.896)	(2.110)
Dad's years of schooling	1.108	5.107	1.629	5.041
	(2.165)	(3.687)	(2.580)	(3.679)
Mom's years of schooling	0.149	3.386	0.570	3.248
	(0.768)	(3.642)	(1.635)	(3.756)
Dad's age	45.364	45.558	44.803	43.931
	(9.725)	(11.411)	(10.615)	(10.798)
Mom's age	37.038	36.293	35.867	34.614
	(7.840)	(7.353)	(7.809)	(7.710)

hh has piped water (yes=1)	0.044	0.716	0.151	0.783
	(0.206)	(0.452)	(0.358)	(0.413)
hh has electricity (yes=1)	0.013	0.847	0.020	0.707
	(0.112)	(0.361)	(0.140)	(0.456)
hh has pit latrine (yes=1)	0.092	0.758	0.253	0.741
	(0.289)	(0.429)	(0.435)	(0.439)
hh owns land (yes=1)	0.997	0.521	0.993	0.703
	(0.056)	(0.501)	(0.084)	(0.458)
hh owns farm animal (yes=1)	0.633	0.107	0.972	0.655
	(0.483)	(0.310)	(0.166)	(0.476)
proportion of hhs with piped water	0.035	0.778	0.148	0.819
	(0.137)	(0.318)	(0.286)	(0.301)
proportion of hhs with electricity	0.016	0.811	0.020	0.677
	(0.087)	(0.313)	(0.105)	(0.361)
proportion of hhs with pit latrine	0.083	0.719	0.229	0.705
	(0.204)	(0.262)	(0.295)	(0.267)
proportion of hhs with land	0.974	0.507	0.956	0.547
	(0.048)	(0.252)	(0.072)	(0.269)
proportion of hhs with farm animal	0.493	0.058	0.895	0.515
	(0.300)	(0.138)	(0.101)	(0.295)
Unemployment rate*	2.479	11.013	3.588	5.109

	(4.743)	(14.310)	(3.993)	(6.068)
Observations	316	215	1547	290

Standard deviations are in parentheses.

*Source: The 1994 and 2007 Ethiopian Census.

Proportion of households is defined over the locality of the child's residence which is roughly equivalent to a village or an urban neighborhood. The indicator variable for land ownership (i.e. hh owns land (yes=1)) takes a value of 1 if any member of the household owns any land holdings regardless of how the land is used, and 0 otherwise.

Note that girls' enrollment rate has been disproportionately lower for a long time, particularly in rural Ethiopia. Table 2.1, however, shows that the proportion of girls enrolled in grade 1 has been increasing during the period of analysis, both in rural and urban areas. Given narrowing down gender gap in primary school enrollment was one of the objectives of the program, it is interesting to see increasing proportion of girls was enrolled in grade 1 during this period.

Generally speaking, parents in Ethiopia are less educated, with the highest average years of schooling being 5 years for fathers and 3 years for mothers. As expected, parents in urban area are more educated than their rural counterparts. Parental years of schooling has slightly increased in rural areas between 1996 and 2004. Though it is not clear why this is the case, it could be partly because of ongoing adult education in Ethiopia.

Household assets and amenities variables depicted in Table 2.1 show that families in rural areas have fewer household assets and live in poor housing conditions compared to those in urban areas. However, household assets and housing condition have improved during the period of analysis for households both in rural and urban areas. To control for the economic condition of the locality of the child's residence, I control for the proportion of households that owns different types of household assets and amenities in the locality of the child's residence, i.e., enumeration area which is used as primary sampling unit in the survey design and is roughly equivalent to a village or an urban neighborhood. As expected, the table shows that rural localities are relatively poorer than their urban counterparts. Finally, Table 2.1 depicts that unemployment rate varies by location of residence, with urban unemployment rate higher than rural unemployment rate.

2.5 The Impact of the Education Program on Access to School and Primary School Enrollment

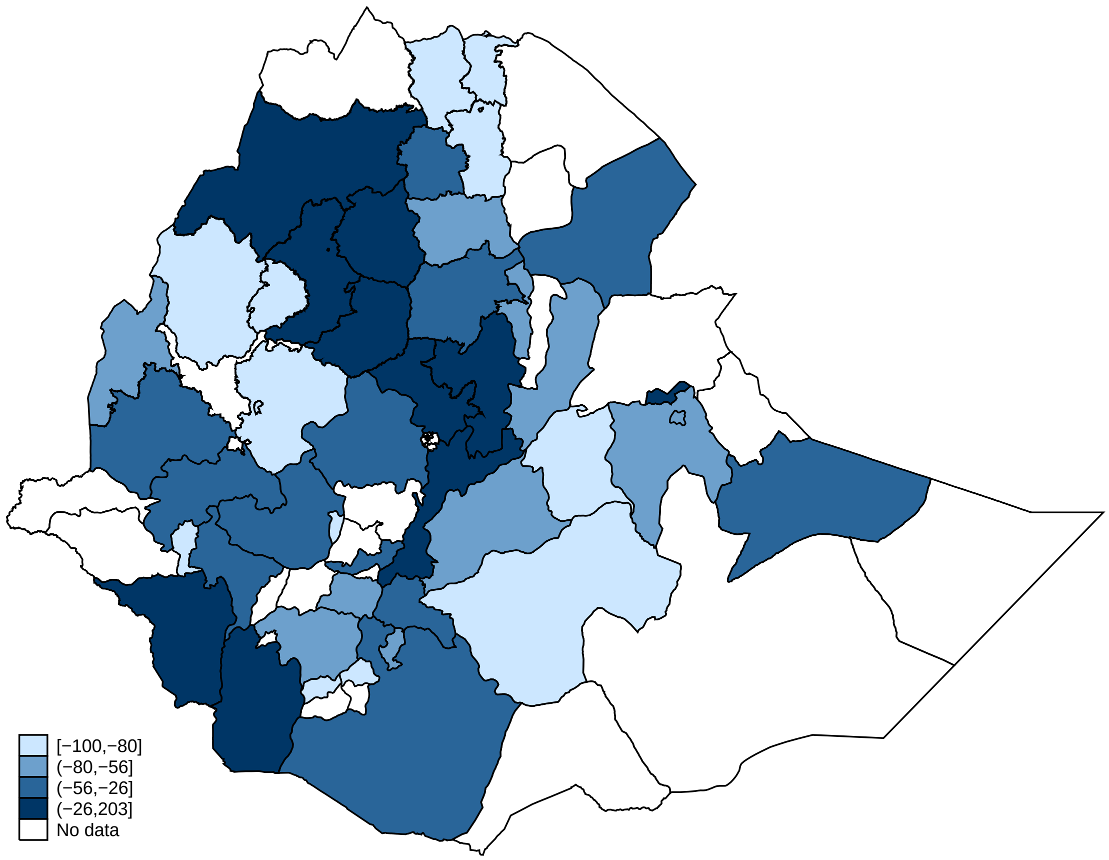
The education program substantially increased access to school in Ethiopia. As mentioned earlier, 2,398 new primary schools were built over a period of 8 years as a results of the program (World Bank, 2005). Besides, data from the 1996 and 2004 Ethiopian Welfare Monitoring Survey show that the average distance to primary school had decreased, at a national level, by 1.48 kilometers between 1996 and 2004, which is more than a 100% decrease from its average of 2.73 Km in 1996 to that of 1.25 Km in 2004.

Though the program has substantially decreased distance at a national level, Figure 2.1 shows that the change in distance to primary school during this period vary widely across zones. Of the total 52 zones surveyed both in 1996 and 2004, distance to primary school decreased in 43 zones, ranging from a 100% decrease to that of 2.81% decrease. On the other hand, distance to primary school increased in 9 zones during the same period, ranging from a 1.13% increase to that of 203% increase.

Similarly, enrollment in primary school has increased substantially in recent years. Figure 2.2 depicts the trend in enrollment rate in primary school in the last three decades using data from the World Bank.[3] For the most part of the 1980s, enrollment rate was stable around 40%, except in the late 1980s where it started to

[3] See Table B.4 in Appendix B.1 for the raw data used to generate Figure 2.2.

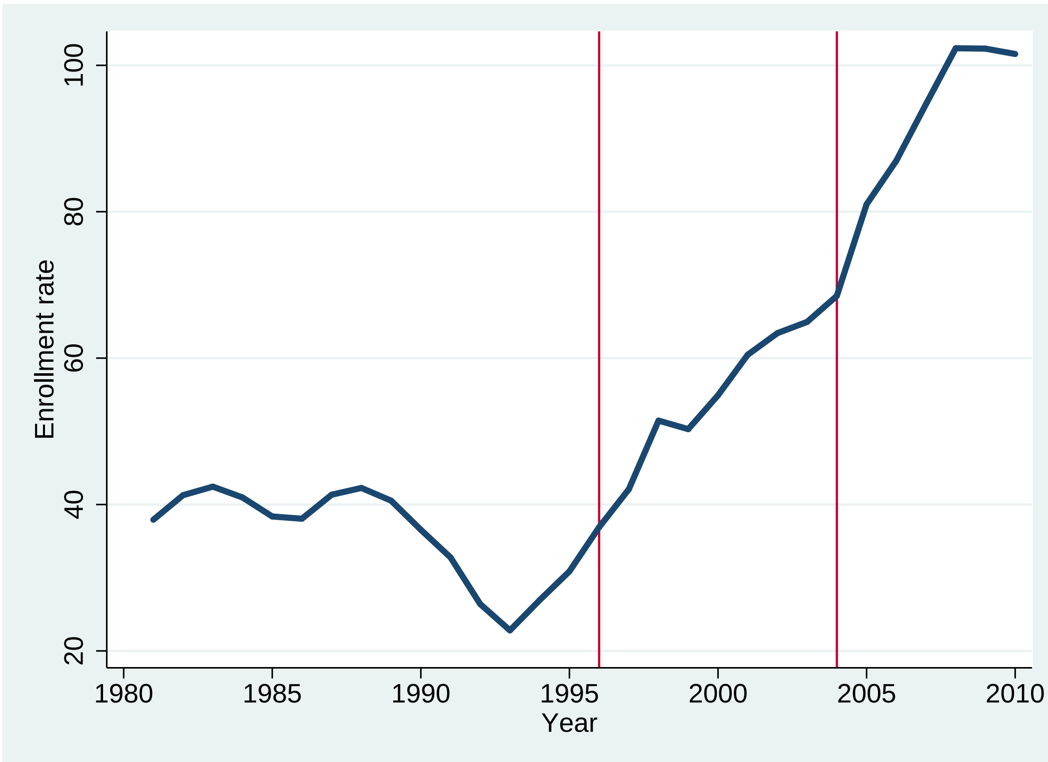
Figure 2.1: Percentage Change in Distance to Primary School Between 1996 and 2004 by Zones in Ethiopia



decline. The decline is mainly because of the aggravated civil war between the military government in power at that time and the rebellion group that finally threw the military government out of power in 1991.

Starting the early 1990s, enrollment rate has started to increase and reached its 1980s level around 1997. Enrollment rate has been continuously increasing since then. The increase in enrollment rate during the period of analysis (which is marked between the two vertical lines in Figure 2.2) is attributed to the education program that has been in place. Remember that even if the focus of this essay is on the education program that was implemented between 1996 and 2004 (more specifically, the first and second Education Sector Development Programs), the next phase of the program (i.e., the third Education Sector Development Program) has been implemented by the end of the second phase of the program. Therefore, we should not expect the growth in enrollment rate to decrease or plateau after 2004. That is why the curve in Figure 2.2 continuously increases even after 2004.

Figure 2.2: Primary School Enrollment Rate Trend in Ethiopia (Source: World Bank)



One feature of the education program is narrowing down educational inequalities across states and between rural and urban residents. This is reflected in the allocation of the newly built schools where 86% of them were built in rural areas (World Bank, 2005). The program also explicitly targeted increasing primary school enrollment from its 30% national average at the beginning of the program to at least 50% by the end of the program. We should, therefore, expect more schools to be built in states that had less than 30% enrollment rate before the program. Accordingly, I assign states with less than 30% enrollment rate before the program in the treated group and those above 30% in the control group.

Using the 1994 Ethiopian census data, Table 2.2 presents the primary school enrollment rate before the program by state and treatment status. Three states had enrollment rate above 30% before the program. These three states are assigned into a control group and all the remaining states are assigned into a treatment group. It is crucial to mention that the three states in the control group are largely urban in nature and leads primarily a non-agrarian economy. But note that all the other states also have major urban areas[4] although the majority of their residents live in rural areas. Given the program focused on building the majority of the schools in rural areas, it is expected states in the control group to be predominantly urban in nature.

Table 2.2: Enrollment Rate in Primary School (Grades 1-8) During the Year Before the Education Program

State/Region	Enrollment Rate	Treated State?
Tigray	15.2	Yes
Afar	2.96	Yes
Amhara	7.64	Yes
Oromiya	9.52	Yes
Somali	2.03	Yes
Benishangul Gumuz	9.94	Yes
SNNP	10.9	Yes
Harari	31	No
Addis Ababa	62	No
Dire Dawa	31.6	No

Source: The 1994 Ethiopian Census.

[4] Central Statistical Agency of Ethiopia defines two types of urban areas: *major urban areas* and *other urban areas*. This classification depends on the nature of economic activity and the number of residents. All state capitals are considered as major urban areas, and they are typically more developed and have larger population size relative to other urban areas.

2.6 The Impact of the Education Program on the Timing of Enrollment

2.6.1 Conceptual Framework

The conceptual framework in this essay draws on Glewwe & Jacoby (1995). Assuming fixed school attendance cost, they show that health status affects a child's readiness to school attendance at the legal enrollment age, and they find out that healthier children enroll in school on time. The model presented here follows their basic structure, but it introduces proximity to school as additional variable cost to the timing of enrollment in primary school.

Assume the child's life is divided into three periods. The first period covers the time between birth and age at enrollment (t_0). During this period, the child works and acquires experience. At t_0, parents decide whether to enroll a child in school. The second period is exclusively allocated to schooling during which the child attends school for s years and accumulates general human capital. In the final period, the child works until retirement year, T. Earnings depend on both the general human capital accumulated and work experience. In the final period, thus, earning depends on years of schooling completed, the experience acquired prior to schooling and after schooling.

The lifetime income (V) of the child is given by the sum of earnings before and after schooling after the cost of schooling is deducted. More precisely,

$$V = \int_0^{t_0} w(s,t)e^{-rt}dt - \int_{t_0}^{t_0+s} c(d)e^{-rt}dt + \int_{t_0+s}^{T} w(s,t)e^{-rt}dt. \qquad (2.1)$$

Wage rate is a function of two arguments: years of schooling (the first argument) and work experience (the second argument). Years of schooling is zero in the first period while it is s in the third period. On the other hand, work experience is t_0 and $(t - s - t_0)$ in periods one and three, respectively. Thus, in period one and three wage rates are given by $w(0, t_0)$ and $w(s, t - s - t_0)$, respectively. Period two is exclusively dedicated to schooling during which the child incurs both direct and indirect costs. The direct costs include tuition fee, purchase of books, etc, which are assumed to be fixed and excluded from the equation. Opportunity cost of school attendance is the indirect cost the child incurs while attending school. This can depend, for example, on distance to school and is denoted by d. Households decide on t_0 (age at school enrollment), i.e.,

$$\max_{t_0} V.$$

Assuming separability between the effect of education and work experience on wage rate, we can write the earning function as:

$$w(s,t) = f(s)g(t),$$

where f and g are increasing functions and concave in their arguments. We expect g to be increasing in its argument since both pre-school experience and readiness to school increases with t (Glewwe & Jacoby, 1995; Bommier & Lambert, 2000).

With a little bit of manipulation and rearrangement, the equilibrium condition of the maximization problem gives us:

$$\frac{\partial t_0}{\partial d} > 0. \tag{2.2}$$

The expression in equation (2.2) suggests that age at enrollment increases with distance.

2.6.2 Econometric Method

According to the conceptual framework developed in Subsection (2.6.1), age at enrollment is a function of distance to primary school. Note that t_0 in equation

(2.2) represents age at primary school enrollment. Let us denote enrollment status of child i in year t by $enroll_{it}$, and the corresponding parental utility by $enroll^{*}_{it}$. We expect parents to enroll the child at the legal age if parental utility from enrolling a child at the legal age is greater than the alternative choice of not enrolling the child in school on time, i.e., $enroll_{it} = 1\ if\ enroll^{*}_{it} > 0,\ and\ 0\ otherwise.$

Taking into account individual differences in observable characteristics, the probability the child enrolls on time is given by:

$$Pr(enroll_{it} = 1|d_{it}, \mathbf{X}_{it}) = G(\theta d_{it} + \boldsymbol{\beta}\mathbf{X}_{it}), \tag{2.3}$$

where d denotes distance to primary school and $\mathbf{X}_{it}$ represents a vector of explanatory variables including a constant. Equation (2.3) is a generic model where G is a function taking on values strictly between zero and one. For the linear probability model G is an identity function so that $Pr(enroll_{it} = 1|d_{it}, \mathbf{X}_{it}) = \theta d_{it} + \boldsymbol{\beta}\mathbf{X}_{it}$. For the probit model, G is the standard normal cumulative distribution function.

If access to primary school (d_{it}) is endogenous in equation (2.3), estimates of equation (2.3) provides biased estimate of θ and hence it cannot be interpreted as the causal effect of access to school on the probability of enrollment on time. There are a number of reasons why we expect access to primary school to be endogenous in equation (2.3), including unobserved parental taste for education. Generally, families that live closer to schools may be inherently different and their children may enroll in school on time regardless of their proximity to school. If there is exogenous source of variation to proximity to school that does not affect the outcome variable, the causal effect of access to school on the timing of enrollment can be identified. I exploit the variation in the intensity of the impact of the education program across states in Ethiopia to identify the causal effect of access to school on the timing of enrollment.

Difference-in-Differences Approach

Ideally, I would compare the probability of enrollment in grade 1 by age 7 ($enroll_i$) for the same set of children when they are exposed to the education program ($enroll_i|education\ program$) and when they are not ($enroll_i|no\ education\ program$). In this ideal case, the average treatment effect would be the differences in the expected values under the two scenarios.

However, the same set of children cannot be observed under both scenarios since the child is either exposed to the program or not. Hence, to estimate the average treatment effect, data on two groups of randomly assigned children where one group is exposed to the program (treatment group) while the other is not exposed to the program (control group) are required. As long as assignment of children to treatment ($Treated = 1$) and control ($Treated = 0$) groups are random, the average treatment effect can be obtained by first difference model.

If, however, children in the two groups differ initially and have different timing of enrollment in the absence of the program, I have to control for the pre-existing difference between the two groups. If I have information on observations both before the education program occurred ($After = 0$) and after the program occurred ($After = 1$), then a difference in differences approach can be used to separate the pre-existing difference from that of the treatment effect. Specifically, I can estimate:

$$Pr(enroll_{it} = 1) = G(\alpha_0 + \eta_0 Treated_{it} + \tau_0 After_{it} + \gamma_0 Treated_{it} * After_{it}) \quad (2.4)$$

In linear probability model, η_0 in equation (2.4) estimates the pre-existing difference between children in the two groups, τ_0 estimates the change in the outcome that occurred over time due to other factors, and γ_0 estimates the impact of the education program. Estimating γ_0 in equation (2.4) assumes children in the two groups would experience the same time trend (τ_0) in the absence of the program, so that once initial difference (η_0) and time trend are controlled for, the remaining difference between children in the treatment and control groups can be attributed to the program.

As mentioned earlier, state level pre-program enrollment rate in primary school is used to group states (and hence students) into treatment and control groups. Specifically, students that live in states that had pre-program primary school enrollment rate below 30% are assigned into treatment group, whereas students that live in states with pre-program primary school enrollment rate above 30% are assigned into control group. The argument is that relatively more schools should be built in areas where the pre-program enrollment rate in primary school is lower since the program explicitly targeted narrowing down education inequalities across states by building more primary schools in areas where primary school enrollment rate was lower before the education program. Hence, if proximity to primary school induces children to enroll on time, in the post-program period, we expect to see children in the treated states to be more likely to enroll in primary school on time relative to those that live in control states.

The basic identification strategy can easily be demonstrated by a simple difference-in-differences table. Table 2.3 presents the difference in differences in age at enrollment in grade 1 between children in the treated and control states before and after the education program. The first column of Table 2.3 displays that, before the program, children in the treated group enrolled in grade 1 at age 9.5 while those in the control group enrolled at age 7.9, a difference of 1.6 years. The difference, however, narrowed down to 1.2 years after the program. Thus, the difference in the differences in age at enrollment in grade 1 is about -0.4 years (i.e., $1.2 - 1.6$).[5]

The difference in differences can be interpreted as the causal effect of the program under the assumption that in the absence of the program the decrease in age at enrollment would not have been systematically different in treated and control states. If this assumption is not satisfied, the difference in differences presented here cannot be interpreted as the "true" treatment effect. In the paragraphs below, I present a difference-in-differences model that adjusts for observable differences between individuals in the treated and control groups in the regression framework.

Table 2.3: Age at Enrollment by Treatment Group Before and After the Program

	Before the Change	After the Change	Time Difference
Treated Group	9.516	9.271	-0.245
	(0.107)	(0.051)	(0.116)
Untreated Group	7.889	8.078	0.189
	(0.198)	(0.143)	(0.255)
Group Difference	1.627	1.193	-0.434
	(0.266)	(0.164)	

Notes: Standard deviations are in parentheses

Using observations sampled from 10 states in Ethiopia and controlling for individual, household, and community-level characteristics; state fixed effects; and state-by-year fixed effects to improve precision, I estimate:

$$Pr(enroll_{ist} = 1) = G(\alpha + \eta Treated_s + \tau After_{it} + \gamma Treated_s * After_{it} + \boldsymbol{\beta}_1 \mathbf{X}_{it} + \boldsymbol{\beta}_2 \mathbf{W}_{ht} + \boldsymbol{\beta}_3 \mathbf{C}_t + \boldsymbol{\beta}_4 \mathbf{S} + \boldsymbol{\beta}_5 \mathbf{S} * \mathbf{Y}) \quad (2.5)$$

[5] A counterpart of Table 2.3 which uses means of enrollment dummy is presented in Table B.2 in Appendix B.1. Table B.2 shows that the unadjusted treatment effect is 0.053, suggesting the program has increased the probability of enrollment in grade 1 by age 7 by 5.3%. Note that the same result can be obtained from OLS regression of equation (2.4). The results from the OLS regression are reported in column 1 of Table B.3 in Appendix B.1. As expected, the coefficient estimate of the interaction term (i.e., $Treated_{it} * After_{it}$) is 0.053

where $enroll_{ist}$ is a dummy variable which takes a value of 1 if child i in state s in year t is enrolled in grade 1 by age 7; $Treated_s$ is a binary indicator for states that had pre-program primary school enrollment rate below 30%; $After_{it}$ is a dummy variable equal to 1 if the child is being observed after the program, and zero otherwise; $\mathbf{X}_{it}$, $\mathbf{W}_{ht}$, and $\mathbf{C}_t$ are vectors of individual, household, and community level characteristics, respectively; $\mathbf{S}$ is a vector of state dummies to control for (time invariant) state fixed effect; and $(\mathbf{S} * \mathbf{Y})$ is a vector of binary indicators for the interaction of state and year dummies to control for state-specific shocks over this period which are correlated with the education program.[6]

The primary (explanatory) variable of interest is the interaction term, $Treated_s * After_{it}$, and γ captures the treatment effect, i.e., the effect on the probability a child enrolls in grade 1 by age 7 due to the child lives in the treated states (relative to those that live in the control states) after the program has occurred. While estimating equation (2.5), the standard errors are clustered by enumeration area, a primary sampling unit, to account for correlation in the error terms within enumeration area over time. For the most part, I assume G is standard normal cumulative distribution function and estimate a probit model, in which case the average marginal effect of the interaction term and its standard error are computed as suggested by Ai & Norton (2003).

2.6.3 Econometric Results

Table 2.4 presents both Linear Probability Model (LPM) and probit estimates of equation (2.5) where the dependent variable is a binary indicator for

[6] A slightly different version of the model presented in equation (2.5) is the one that replaces the dummy variable for treated group, $Treated_s$, by a continuous pre-program state level primary school enrollment rate variable, $EnrolRate_s$, i.e.,

$$\begin{aligned} Pr(enroll_{ist} = 1) = G(\alpha + \eta EnrolRate_s + \tau After_{it} + \gamma EnrolRate_s * After_{it} + \boldsymbol{\beta}_1 \mathbf{X}_{it} \\ + \boldsymbol{\beta}_2 \mathbf{W}_{ht} + \boldsymbol{\beta}_3 \mathbf{C}_t + \boldsymbol{\beta}_4 \mathbf{S} + \boldsymbol{\beta}_5 \mathbf{S} * \mathbf{Y}) \end{aligned}$$

Results from this specification are presented in column 2 of Table 2.6.

enrollment in grade 1 by age 7.[7] The first column shows the results from the LPM, while the second column presents that of the probit model. The probit model indicates that children in the treated states are 31% less likely to enroll in grade 1 by age 7 relative to children in the control states during the pre-program period, and the effect is statistically significant at 2.6 percent level. This evidence supports the argument that there was pre-existing difference in the timing of enrollment in primary school between children in the treated and control states prior to the education program, where children in the treated states were less likely to enroll in primary school at the legal enrollment age relative to those in the control states.

The average marginal effect of the interaction term is 0.35 in LMP and 0.36 in probit model. This suggests children in the treated state are 35% and 36% more likely to enroll on time relative to those who live in the control state after the program has occurred. Note that the specifications control for pre-existing differences in the timing of enrollment between children in the treated and control states; the time trend, i.e., the change in the timing of enrollment overtime due to other factors; observable individual, household, and community-level characteristics; state fixed effect; and state-by-year fixed effect. Hence, this effect is attributed to the education program, and it can be interpreted as the “true” average treatment effect.

Even if Table 2.4 documents positive and significant average treatment effect, it is crucial to examine the distribution of the treatment effect in non-linear models such as probit since marginal effect is not constant in non-linear models. Figure 2.3, hence, presents the histogram and kernel density of the treatment effect. The figure clearly shows that the treatment effect is always non-negative and goes up well above 40%, suggesting large and positive treatment effect. The histogram and kernel density of the treatment effect is also plotted separately for rural and urban

[7] To conserve space, Table 2.4 suppresses the coefficients of the control variables. See Table B.5 in Appendix B.1 for the full version of the regression output.

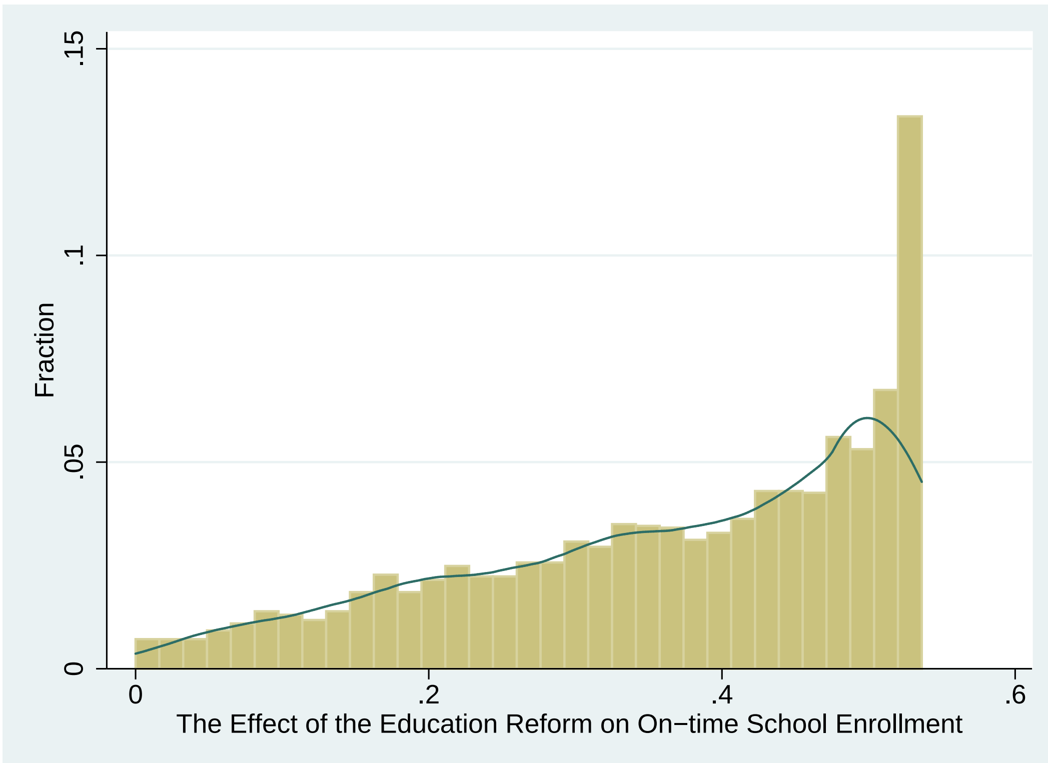
Table 2.4: Difference-in-Differences Estimates of the Effect of the Education Reform on On-time School Enrollment
Dependent Variable: Binary Indicator for Enrollment in Grade 1 by Age 7

	(1) LPM	(2) Probit
Treated (yes=1)	0.037	-0.307**
	(0.120)	(0.138)
After (yes=1)	-0.208	-0.217
	(0.159)	(0.135)
Treated*After	0.352**	0.364**
	(0.173)	(0.189)
Controls	Yes	Yes
State fixed effects	Yes	Yes
State-by-year fixed effects	Yes	Yes
Observations	2372	2372
R-sq	0.243	
Log Likelihood		-1000.591

*p < 0.10, ** p < 0.05, *** p < 0.01.
Reported coefficients are average marginal effects.
Robust standard errors are clustered by enumeration area, the primary sampling unit, and are reported in parentheses. All regressions control for individual-level characteristics (i.e., a binary indicator for gender, birth order, mother's and father's age and years of schooling), household-level characteristics (i.e., household size, binary indicators for whether a household has piped water, electricity, pit latrine, land, and farm animal), locality-level characteristics (i.e., proportion of households with piped water, electricity, pit latrine, land, and farm animal), and location of residence, i.e., urban dummy.

samples (see Figures B.1 and B.2 in Appendix B.2) to see if there is any difference in the treatment effect between rural and urban samples. The figures show strong and positive treatment effect both for urban and rural samples.

Figure 2.3: Histogram and Kernel Density of the Treatment Effect



Alternative Specifications and Robustness Check

Both specifications presented in Table 2.4 do not control for family income. This is because information on family income is not collected in the WMS data. Fortunately, however, detailed information on family income and expenditure is gathered in a supplementary survey called Household Income, Consumption, and Expenditure Survey (HICES), which is also administered by the Ethiopian Central Statistical Agency. HICES collects information on a subset of households that are surveyed in WMS, and it is usually conducted in the same year as the WMS. Using households sampled both in the HICES and WMS, I re-estimate equation (2.5) both by including and excluding household expenditure in the regression model.

The first 3 columns of Table 2.5 present the results from different specifications using the restricted sample (i.e., households observed both in the HICES and WMS), and hence has relatively smaller sample size. To make comparison of results from different specifications (that control for household expenditure and that do not) straight forward, the basic specification reported in column 2 of Table 2.4 is re-estimated for the restricted sample, and the results are presented in column 1 of Table 2.5. Column 2 of Table 2.5 presents the results from a specification that controls for household expenditure. Controlling for household expenditure changes neither the magnitude nor the significance of the average marginal effect of the interaction term. The coefficient estimate of the household expenditure itself, on the other hand, is positive, but not significant. It is insignificant may be because the specification controls for parental years of schooling and household assets and amenities, which are generally good controls for families' socioeconomic status.

If higher income families self select themselves to live at closer proximity to schools and they are more likely to enroll their children in primary school on time regardless of their proximity to school, then household income or expenditure is endogenous and bias the results. The program was explicitly designed to make primary schools more accessible to households in rural areas and underserved localities. In this setting, bias from this type of selection is less likely since the program exogenously allocates new schools across households. If higher income families somehow managed to influence policy makers to build more schools in their locality or higher income families move to areas that received more school construction, then household expenditure is endogenous and biases the results. To mitigate potential endogeneity of household expenditure, I aggregated household expenditure at enumeration area - a primary sampling unit which is typically equivalent to a village or urban neighborhood - level and estimated equation (2.5). The results are depicted in column 3 of Table 2.5. The average treatment effect under this specification is again similar to those presented in columns 1 and 2 both in magnitude and significance. The similarity of the results reinforces the argument that relatively rich communities were less likely to influence policy makers to build more schools in their communities. It also suggests there is no evidence that high income families moved to areas that received more school allocation.

One of the identifying assumptions in the difference-in-differences model is the economic growth rate in the treated and control states do not vary systematically over time. In reality, however, states in the two groups may experience different growth rates. Thus, the estimates could potentially confound the effect of the program with the effect of the differential growth rate on the timing of enrollment that would have been observed even in the absence of the program. Thus, I present a specification that controls for state level unemployment rate in column 5 of Table 2.5. Information on unemployment rate is obtained from the 1994 and 2007 Ethiopian census. In this specification, the average treatment effect has increased by about 9 percentage point relative to the basic specification. Note that column 4 of Table 2.5 simply presents the results of the basic specification reported in column 2 of Table 2.4.

If we expect states with relatively higher growth rate (or lower unemployment rate) make schools relatively more accessible to their residents in the absence of the program, and if we assume the program targets building more schools in states with lower growth rate, then comparison of the average treatment effect in the basic specification and the one that controls for differences in economic growth rate implies that the program help children who live in lower-growth-rate states to

Table 2.5: Difference-in-Differences Estimates of the Effect of the Education Reform on On-time School Enrollment
Dependent Variable: Binary Indicator for Enrollment in Grade 1 by Age 7

	Restricted Sample			Full Sample	
	(1)	(2)	(3)	(4)	(5)
Treated (yes=1)	-0.333**	-0.328**	-0.334**	-0.307**	-0.773*
	(0.141)	(0.140)	(0.140)	(0.138)	(0.413)
After (yes=1)	-0.234*	-0.235*	-0.239*	-0.217	-0.242
	(0.139)	(0.138)	(0.139)	(0.135)	(0.150)
Treated*After	0.399**	0.395**	0.400**	0.364**	0.450**
	(0.158)	(0.157)	(0.158)	(0.189)	(0.215)
Log(exp)		0.016			
		(0.019)			
Log(exp, comm.)			0.013		
			(0.032)		
Unempt rate, state					-0.021
					(0.014)
Controls	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes
State-by-year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1439	1439	1439	2372	2368
Log Likelihood	-631.692	-631.350	-631.610	-1000.591	-1000.591

*p $<$ 0.10, ** p $<$ 0.05, *** p $<$ 0.01.
Reported coefficients are average marginal effects. Robust standard errors are clustered by enumeration area, the primary sampling unit, and are reported in parentheses. All regressions control for individual, household, and locality-level characteristics. The "full sample" contains households that are observed in WMS data and meet the sample restriction criteria of this study, while the "restricted sample" contains a subset of households in the "full sample" which are also observed in a supplementary survey called HICES. See the text for further information. Log(exp), Log(exp, comm.), and Unempt rate, state denote log of household expenditure, log of average household expenditure in the community, and state level unemployment rate, respectively.

catch up with those in high-growth-rate states in terms of enrolling in primary school on time.

Finally, column 2 of Table 2.6 presents results from a model that replaces a binary indicator (for treated states) by a continuous measure of pre-program state level primary school enrollment rate.[8] One advantage of using a continuous primary school enrollment rate variable, rather than a binary indicator, is it makes use of all the available information and hence the treatment effect is more precisely estimated. Besides, it is more robust to the risk of arbitrarily grouping states into treatment and control groups. Prior studies employ a similar strategy to estimate treatment effect. For instance, Miller (2012) used pre-reform insurance rate to investigate the effect of the 2006 Massachusetts health reform on emergency room visits. In this continuous treatment specification, $Treated_s$ in equation (2.5) is replaced by pre-program state level primary school enrollment rate, $EnrolRate_s$.

In this model, the estimate of the average marginal effect of $EnrolRate_s$ can be interpreted as the change in the probability of enrollment in grade 1 by age 7 for a one percent change in the pre-program enrollment rate. I find that children who lived in states with one percent higher pre-program primary school enrollment rate were about 1.9% more likely to enroll in school on time, reaffirming the pre-existing difference on the timing of enrollment across children that live in states with different pre-program primary school enrollment rate. On the other hand, the average treatment effect is estimated to be -0.021. This treatment effect suggests that, on average, children that lived in a state with one percent higher pre-program enrollment rate were 2.1% less likely to enroll in primary school on time. Thus, the program has caused children that live in states with lower pre-program enrollment rate to enroll in school on time.

[8] Again, column 1 of Table 2.6 presents the results of the basic specification reported in column 2 of Table 2.4 for comparison purpose.

Table 2.6: Difference-in-differences Estimates of the Effect of the Education Reform on On-time School Enrollment
Dependent Variable: Binary Indicator for Enrollment in Grade 1 by Age 7

	(1)	(2)
Treated (yes=1)	-0.307**	
	(0.138)	
After (yes=1)	-0.217	0.434**
	(0.135)	(0.193)
Treated*After	0.364**	
	(0.189)	
Pre-program primary school enrollment rate		0.019**
		(0.008)
(Pre-program primary school enrollment rate)*(After)		-0.021**
		(0.009)
Controls	Yes	Yes
State fixed effects	Yes	Yes
State-by-year fixed effects	Yes	Yes
Observations	2372	2372
Log Likelihood	-1000.591	-1000.591

*p < 0.10, ** p < 0.05, *** p < 0.01.
Reported coefficients are average marginal effects.
Robust standard errors are clustered by enumeration area, the primary sampling unit, and are reported in parentheses. All regressions control for individual-level characteristics (i.e., a binary indicator for gender, birth order, mother's and father's age and years of schooling), household-level characteristics (i.e., household size, binary indicators for whether a household has piped water, electricity, pit latrine, land, and farm animal), locality-level characteristics (i.e., proportion of households with piped water, electricity, pit latrine, land, and farm animal), and location of residence, i.e., urban dummy.

By How Much Has the Program Decreased Age at Enrollment?

The results presented above support the argument that the program has increased the probability of enrollment in grade 1 at the legal enrollment age. It is, therefore, interesting to investigate *by how much* the program has decreased age at enrollment. Table 2.7 presents the estimates of this experiment[9] where the dependent variable is the natural logarithm of age at enrollment. Meyer et al. (1995) employed a similar approach to investigate the effect of workers' compensation on time out of work arguing that log duration regression is a special case of exponential, Weibull, and log-logistic hazard models in the absence of censoring and time varying explanatory variables.

Again, for the purpose of comparison, the results of the basic specification reported in column 2 of Table 2.4 is presented in column 1 of Table 2.7. Column 2 of Table 2.7, on the other hand, depicts the results of the log duration regression, where the dependent variable is the natural logarithm of age at enrollment. The results reported in column 2 of Table 2.7 show that the average treatment effect is -0.197, implying that the difference in age at enrollment between children in the treated and control states has decreased by 19.7% as a result of the education program. Remember that children in the treated states, on average, enroll in primary school 1.63 years later than those in the control states before the education program (see Table 2.3). Hence, the program has decreased age at enrollment in grade 1 by 3.85 months or 0.32 $(1.63 * 0.197)$ years.

2.7 Conclusion

In recent years, many governments in developing countries have attempted to achieve universal primary education through a large scale construction of primary

[9] To conserve space, Table 2.7 suppresses the coefficients of the control variables. See Table B.6 in Appendix B.1 for the full version of the regression output.

Table 2.7: Difference-in-Differences Estimates of the Effect of the Education Reform on On-time School Enrollment

	(1) Enrollment Status	(2) Log(Age at Enrollment)
Treated (yes=1)	-0.307**	0.158***
	(0.138)	(0.055)
After (yes=1)	-0.217	0.145**
	(0.135)	(0.060)
Treated*After	0.364**	-0.197***
	(0.189)	(0.068)
Controls	Yes	Yes
State fixed effects	Yes	Yes
State-by-year fixed effects	Yes	Yes
Observations	2372	2372
R-sq		0.384

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

Robust standard errors are clustered by enumeration area, the primary sampling unit, and are reported in parentheses. The regression controls for individual-level characteristics (i.e., a binary indicator for gender, birth order, mother's and father's age and years of schooling), household-level characteristics (i.e., household size, binary indicators for whether a household has piped water, electricity, pit latrine, land, and farm animal), locality-level characteristics (i.e., proportion of households with piped water, electricity, pit latrine, land, and farm animal), and location of residence, i.e., urban dummy.

The dependent variables in column 1 and 2 are binary indicator for enrollment in grade 1 by age 7 and the natural logarithm of age at enrollment in grade 1, respectively.

schools. The majority of the studies on primary education in developing countries focus on enrollment rates, without considering the timing of enrollment. Delaying primary school enrollment beyond the legal enrollment age, however, is more of a norm than an exception in these countries. Prior studies have documented that delaying enrollment is costly as it, for instance, decreases an individual's life time wealth (Glewwe & Jacoby, 1995), and it increases both school dropout and grade repetition rates (Wils, 2004). Though delayed enrollment is widely observed in developing countries and there is a high cost associated with it, the literature on the topic is limited, and we have a limited understanding of why children delay enrollment in primary school. This essay attempts to fill the gap in the literature by investigating the effect of access to primary school on the timing of enrollment in primary school.

Identifying the causal effect of access to primary school on the timing of enrollment is complicated by endogeneity of access to primary school. For instance, parents who choose to live at closer proximity to school may have strong taste for education and enroll their children in school on time regardless of proximity to school. To mitigate biases due to endogeneity of access to school, I exploit the education reform in Ethiopia as exogenous source of variation in access to primary school. Then, I estimated difference-in-differences model where the dependent variable is a binary indicator for enrollment in primary school by age 7, the legal enrollment age in Ethiopia, and the natural logarithm of age at enrollment in primary school.

The average treatment effect is estimated to be between 35% and 45%, suggesting the probability the child enrolls in primary school on time has increased by between 35% and 45% as a result of the education reform. The log duration regression (where the dependent variable is the natural logarithm of age at enrollment), on the other hand, suggests that the reform has decreased age at enrollment in grade 1 by about 4 months. These estimates highlight an important role that access to school plays in inducing parents to enroll their kids in primary school at the legal enrollment age.

The findings reported here are important as they show that, in Ethiopia, education intervention has been effective in decreasing age at enrollment in primary school. The intervention was meant to increase primary school enrollment, but it also induced households to enroll their children in primary school at a relatively younger age. The Ethiopian government provides free primary education. Thus, households do not have to pay for tuition. Households, however, still have to incur other costs related to school attendance, including the child's opportunity cost of time in terms of forgone family income from child labor.

Making schools accessible to poor households would decrease the time the child spends walking to school, and hence decreases the opportunity cost of school attendance. Moreover, accessibility induces physically weaker children to attend school since it decreases the physical strength needed to walk the distance to school. Policy makers, thus, should also consider improving communication networks and public transport as alternative/additional ways to encourage households to enroll their kids in primary school on time.

Appendix A

ESSAY 1

A.1 Technical Notes

Specification of Bivariate Probit Model

Equations (1.7) and (1.8) which are presented in Section 1.5 specify the latent parental utility derived from allocating child $i's$ time on school and child labor in year t as

$$\begin{aligned} s_{it}^{*} &= \delta_s b\_order_{it} + \gamma_s family\_size_{it} + \boldsymbol{\beta}_s \mathbf{X}_{it} + \alpha_{is} + \epsilon_{its}, \\ l_{it}^{*} &= \delta_l b\_order_{it} + \gamma_l family\_size_{it} + \boldsymbol{\beta}_l \mathbf{X}_{it} + \alpha_{il} + \epsilon_{itl}, \end{aligned} \tag{A.1}$$

where ϵ_{its} and ϵ_{itl} are random error terms which are jointly and normally distributed with means zero, variances one, and correlation ρ, and all the other notations are as discussed in Section 1.5. The bivariate probit model specifies the observed outcome as

$$s_{it} = \begin{cases} 1 & \text{if } s_{it}^{*} > 0 \\ 0 & \text{if } s_{it}^{*} \leq 0, \end{cases}$$

$$l_{it} = \begin{cases} 1 & \text{if } l_{it}^{*} > 0 \\ 0 & \text{if } l_{it}^{*} \leq 0, \end{cases}$$

If the two error terms are uncorrelated (i.e., $\rho = 0$), the model collapses to separate probit models. If, on the other hand, $\rho \neq 0$, then bivariate probit model is appropriate. For notational simplicity, let us suppress individual and time subscripts i and t, and also denote the vector of explanatory variables including family size and birth order (and coefficients) in the school attendance and child labor equations, respectively, by $\mathbf{W}_s$ (and $\boldsymbol{\lambda}_s$) and $\mathbf{W}_l$ (and $\boldsymbol{\lambda}_l$). The joint probabilities of, say, $s = 1$ and $l = 1$ (i.e., p_{11}) can now be stated as

$$\begin{aligned} p_{11} &= Pr(s = 1, l = 1), \\ &= Pr(s^* > 0, l^* > 0), \\ &= Pr(\epsilon_s < \mathbf{W}_s\boldsymbol{\lambda}_s, \epsilon_l < \mathbf{W}_l\boldsymbol{\lambda}_l), \\ &= \int_{-\infty}^{\mathbf{W}_s\boldsymbol{\lambda}_s} \int_{-\infty}^{\mathbf{W}_l\boldsymbol{\lambda}_l} \phi(z_s, z_l, \rho) dz_s dz_l, \\ &= \Phi(z_s, z_l, \rho), \end{aligned} \tag{A.2}$$

where $\phi(.)$ and $\Phi(.)$ are, respectively, the standardized bivariate normal density and the cumulative density function for (z_s, z_l). Following Greene (2008), we can state the other three possible outcomes as

$$\begin{aligned} p_{jk} &= Pr(s = j, l = k), \\ &= \Phi(q_s\mathbf{W}_s\boldsymbol{\lambda}_s, q_l\mathbf{W}_l\boldsymbol{\lambda}_l, q_s q_l \rho), \end{aligned} \tag{A.3}$$

where $q_s = 1$ if $s = 1$ and $q_s = -1$ if $s = 0$; similarly, $q_l = 1$ if $l = 1$ and $q_l = -1$ if $l = 0$. The log-likelihood function for the bivariate probit model is, thus:

$$\ln L = \sum_i \Phi(q_s\mathbf{W}_s\boldsymbol{\lambda}_s, q_l\mathbf{W}_l\boldsymbol{\lambda}_l, q_s q_l \rho). \tag{A.4}$$

Equation (A.4) is estimated using maximum likelihood procedure. The marginal effects of a change in birth order on the probabilities of school attendance and child labor are, respectively, given by

$$\frac{\partial \Phi_s(.)}{\partial b\_order_{it}} = \phi_s(.) * \hat{\delta}_s,$$

$$\frac{\partial \Phi_l(.)}{\partial b\_order_{it}} = \phi_1(.) * \hat{\delta}_l,$$

where $\hat{\delta}_s$ and $\hat{\delta}_l$ are the coefficient estimates of birth order in school attendance and child labor equations, respectively, and where $\Phi(.)$ and $\phi(.)$ with subscripts s and l denote the univariate standard normal cumulative distribution function and the marginal standard normal density, respectively. The estimated average marginal effect is simply the average over all observations, evaluated at the maximum likelihood estimates of unknown parameters.

As discussed in Section 1.5, one of the crucial issues that needs to be addressed is estimating this model is potential endogeneity of family size. A variable which records the proportion of boys in the family is used to instrument family size and unobserved effect bivariate probit instrumental variable (IV) model is estimated to mitigate potential endogeneity. The IV approach in the context of the non-linear model discussed above is implemented using, as Terza et al. (2008) called it, two-stage residual inclusion (2SRI) procedure. The procedure in 2SRI and two-stage least square (2SLS) regression are the same except that in 2SRI the endogenous variable is not replaced by its predicted value in the second stage equation. Instead, the predicted residual from the first stage regression is included as an additional variable in the second stage equation. More specifically, the following first stage equation is first estimated:

$$family\_size_{it} = \eta_0 + \eta_1 proportion\_of\_boys_{it} + \eta_2 b\_order_{it} + \boldsymbol{\eta}_3 \mathbf{X}_{it} + \psi_i + \mu_{it},$$

where all notations are as discussed in Section 1.5. Then, the predicted residual from this regression is included as an additional explanatory variable in the second stage equation, i.e., equation (A.4). This is implemented in Stata using Generalized Linear Latent and Mixed Models (GLLAMM) program, which is discussed in detail in Rabe Hesketh (2008)

A.2 Additional Tables

Table A.1: List and Description of Variables Used in Estimation of the Effect of Birth Order on Children's time Allocation

Variable	Description
Dependent variables	
school attendance (yes=1)	=1 if a child attends school; 0 otherwise
working child (yes=1)	=1 if a child works as a child laborer; 0 otherwise
Independent variables	
birth order	=1 for a first-born child, =2 for a second-born child, etc
proportion of boys	Ratio of number of boys to number of kids in the family
support on family planning	=1 if a hh received support on family planning; 0 otherwise
number of kids	Number of children in the household
child is a girl (yes=1)	=1 if a child is girl; 0 otherwise
child's age	child's (aged between 7 and 15) age in completed years
housemaid (yes=1)	=1 if a family hired a housemaid; 0 otherwise
fathers schooling	Highest grade completed by the father
mother's schooling	Highest grade completed by the mother
household expenditure	Annual household expenditure in 2005 prices (in 10,000s)
urban (yes=1)	=1 if the household is located in urban area; 0 otherwise

Table A.2: Marginal and Joint Frequencies for School Attendance and Child Labor

	Child Labor		
School Attendance	**No**	**Yes**	**Total**
	%	%	%
No	1.3	8.9	10.2
Yes	20.1	69.8	89.8
Total	21.4	78.6	100.0

Source: Author calculation

Table A.3: Child Labor Specialization by Gender

	Gender		
Type of Work	Boy	Girl	Total
	%	%	%
Domestic Work	23.2	76.8	100.0
Unpaid Work	80.6	19.4	100.0
Caring for Others	31.0	69.0	100.0
Paid Work	52.3	47.7	100.0

Table A.4: Fraction of Families with Additional Child by Parity and Sex Mix

	Mean	SD	N
Sex mix of the first 3 births in families of 3 or more			
3 boys	0.90	0.29	152
2 boys, 1 girl	0.89	0.31	557
1 boy, 2 girls	0.89	0.32	505
3 girls	0.84	0.37	137
Sex mix of the first 4 births in families of 4 or more			
4 boys	0.74	0.44	80
3 boys, 1 girl	0.80	0.40	275
2 boys, 2 girls	0.80	0.40	464
1 boy, 3 girls	0.78	0.41	260
4 girls	0.72	0.45	60
Sex mix of the first 5 births in families of 5 or more			
5 boys	0.61	0.49	25
4 boys, 1 girl	0.73	0.45	100
3 boys, 2 girls	0.72	0.45	195
2 boys, 3 girls	0.55	0.50	205
1 boy, 4 girls	0.72	0.45	103
5 girls	0.93	0.26	23
Sex mix of the first 6 births in families of 6 or more			
6 boys	0.77	0.44	14
5 boys, 1 girl	0.73	0.45	45
4 boys, 2 girls	0.58	0.49	100
3 boys, 3 girls	0.52	0.50	112
2 boys, 4 girls	0.61	0.49	96
1 boy, 5 girls	0.73	0.44	39
6 girls	0.70	0.47	13

Table A.5: Independent Pooled Probit Estimates of School Attendance and Child Labor Equations

	School Equation		Child Labor Equation	
birth order	-0.028	(0.047)	-0.186***	(0.039)
number of kids	-0.029	(0.043)	0.125***	(0.036)
Child's age = 8	0.577***	(0.185)	0.368**	(0.183)
Child's age = 9	1.184***	(0.203)	0.591***	(0.191)
Child's age = 10	1.190***	(0.194)	0.551***	(0.189)
Child's age = 11	1.958***	(0.226)	0.839***	(0.168)
Child's age = 12	1.659***	(0.197)	0.863***	(0.170)
Child's age = 13	1.324***	(0.302)	0.830***	(0.264)
Child's age = 14	1.753***	(0.292)	0.911***	(0.227)
Child's age = 15	1.366***	(0.264)	0.711***	(0.222)
child is a girl (yes=1)	0.154	(0.097)	0.196**	(0.078)
housemaid (yes=1)	0.120	(0.211)	-0.391**	(0.171)
father's schooling	0.059***	(0.021)	-0.031**	(0.014)
mother's schooling	-0.022	(0.027)	0.001	(0.014)
household expenditure	0.059	(0.091)	-0.075*	(0.041)
urban (yes=1)	1.070	(0.720)	-0.668*	(0.373)
Observations	1790		1860	
Log Likelihood	-455.830		-667.514	

*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 A.6: Independent Unobseved Effect Probit Estimates of School Attendance and Child Labor Equations

	School Equation				Work Equation			
	Probit		Probit IV		Probit		Probit IV	
main								
birth order	-0.032	(0.064)	0.389**	(0.189)	-0.193***	(0.042)	-0.242**	(0.098)
number of kids	-0.047	(0.058)	-0.545***	(0.167)	0.126***	(0.037)	0.032	(0.085)
Child's age = 8	0.770***	(0.259)	1.811***	(0.500)	0.393**	(0.200)	0.832***	(0.310)
Child's age = 9	1.537***	(0.303)	3.478***	(0.542)	0.627***	(0.209)	1.179***	(0.320)
Child's age = 10	1.586***	(0.295)	3.980***	(0.538)	0.586***	(0.198)	1.058***	(0.278)
Child's age = 11	2.533***	(0.354)	5.503***	(0.631)	0.887***	(0.192)	1.508***	(0.331)
Child's age = 12	2.169***	(0.316)	4.863***	(0.624)	0.917***	(0.190)	1.726***	(0.336)
Child's age = 13	1.787***	(0.430)	4.786***	(0.836)	0.866***	(0.291)	1.311***	(0.476)
Child's age = 14	2.250***	(0.428)	5.084***	(0.841)	0.964***	(0.258)	1.515***	(0.472)
Child's age = 15	1.771***	(0.380)	4.216***	(0.829)	0.758***	(0.246)	1.472***	(0.478)
child is a girl (yes=1)	0.184	(0.132)	0.510*	(0.290)	0.202**	(0.086)	0.357*	(0.191)
housemaid (yes=1)	0.144	(0.291)	0.470	(0.586)	-0.414**	(0.177)	-0.360	(0.313)
father's schooling	0.080***	(0.029)	0.135**	(0.060)	-0.033**	(0.015)	-0.070**	(0.032)
mother's schooling	-0.028	(0.035)	-0.161*	(0.083)	0.000	(0.016)	-0.054	(0.040)
annual expenditure	0.081	(0.098)	0.248*	(0.147)	-0.072	(0.046)	0.028	(0.061)
urban (yes=1)	1.322	(0.813)	4.451***	(0.948)	-0.688	(0.521)	-0.636	(0.447)
1^{st} stage residual			0.488***	(0.183)			0.170*	(0.097)
Observations	1862		1862		1860		1860	
Log Likelihood	-449.007		-663.310		-666.841		-1146.119	

*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.

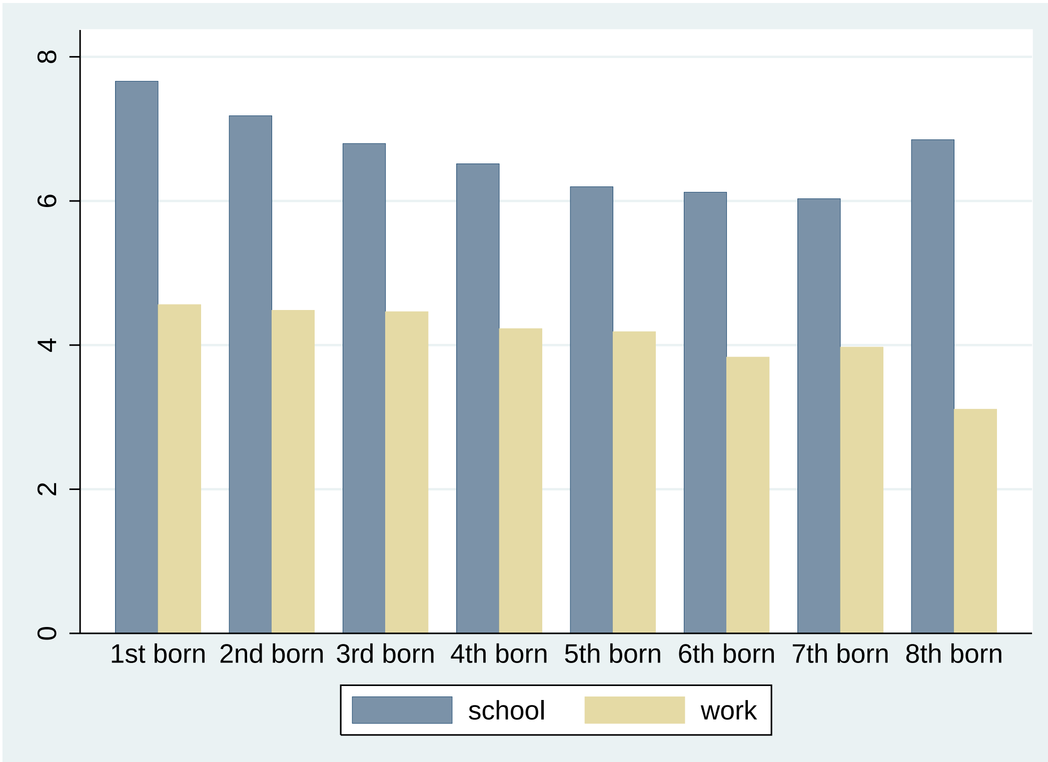
Table A.7: Pooled Bivariate Probit Estimates of School Attendance and Child Labor Equations

	School Equation		Child Labor Equation	
birth order	-0.030	(0.05)	-0.188***	(0.04)
number of kids	-0.031	(0.04)	0.125***	(0.04)
Child's age = 8	0.576***	(0.19)	0.378**	(0.18)
Child's age = 9	1.176***	(0.20)	0.595***	(0.19)
Child's age = 10	1.176***	(0.19)	0.554***	(0.19)
Child's age = 11	1.943***	(0.22)	0.841***	(0.17)
Child's age = 12	1.629***	(0.20)	0.855***	(0.17)
Child's age = 13	1.295***	(0.30)	0.838***	(0.26)
Child's age = 14	1.728***	(0.29)	0.910***	(0.23)
Child's age = 15	1.324***	(0.26)	0.708***	(0.22)
child is a girl (yes=1)	0.138	(0.10)	0.197**	(0.08)
father's schooling	0.060***	(0.02)	-0.031**	(0.01)
mother's schooling	-0.022	(0.03)	0.001	(0.01)
household expenditure	0.066	(0.09)	-0.076*	(0.04)
urban (yes=1)	1.057	(0.71)	-0.687*	(0.38)
Constant	-0.079	(0.89)	-1.124**	(0.50)
athrho				
Constant	-0.230***	(0.09)		
Observations	1860			
Log likelihood	-1119.195			

*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 -.226

A.3 Additional Graphs

Figure A.1: Hours Spent in School and Working by Birth Order



Appendix B

ESSAY 2

B.1 Additional Tables

Table B.1: List and Description of Variables Used in Estimation of the Effect of the Education Reform on the Timing of School Enrollment

Variable	Description
Dependent variables	
Enrolled in grade 1 by age 7 (yes=1)	=1 if a child is enrolled in grade 1 at $age \leq 7$; 0 otherwise
Age at enrollment	Age in years by the time a child enrolled in grade 1
Independent variables	
Treated	=1 if state level primary school enrollment rate is ≤ 30 ; 0 otherwise
After	=1 if year=2004; 0 otherwise
Girl (yes=1)	=1 if a child is a girl; 0 otherwise
Birth order	=1 for a first-born child, =2 for a second-born child, etc
Household size	Total number of people who live in the household
Dad's years of schooling	Highest grade completed by the father
Mom's years of schooling	Highest grade completed by the mother
Dad's age	Father's age in years
Mom's age	Mother's age in years
hh has piped water (yes=1)	=1 if the household has piped water; 0 otherwise
hh has electricity (yes=1)	=1 if the household has electricity; 0 otherwise
hh has pit latrine (yes=1)	=1 if the household has pit latrine; 0 otherwise
hh owns land (yes=1)	=1 if any member of the household owns any land holdings
hh owns farm animal (yes=1)	=1 if the household owns farm animals; 0 otherwise

proportion of hhs with piped water	Proportion of households with piped water
proportion of hhs with electricity	Proportion of households with electricity
proportion of hhs with pit latrine	Proportion of households with pit latrine
proportion of hhs with land	Proportion of households that owns land
proportion of hhs with farm animal	Proportion of households that owns farm animal
Log (Household expenditure)	Log of annual total household expenditure in 2005 prices
Unemployment rate	State level unemployment rate
Urban area (yes=1)	=1 if the household is located in urban area; 0 otherwise

Table B.2: Fraction of Children Enrolled in Grade 1 by Age 7 by Treatment Group Before and After the Program

	Before the Change	After the Change	Time Difference
Treated Group	0.114	0.193	0.079
	(0.018)	(0.016)	(0.026)
Untreated Group	0.505	0.530	0.026
	(0.034)	(0.030)	(0.045)
Group Difference	-0.391	-0.338	0.053
	(0.035)	(0.031)	

Notes: Standard deviations are in parentheses

Table B.3: Difference-in-Differences Estimates of the Effect of the Education Reform on On-time School Enrollment
Dependent Variable: Binary Indicator for Enrollment in Grade 1 by Age 7

	(1) LPM	(2) Probit
Treated (yes=1)	-0.391***	-0.365***
	(0.037)	(0.033)
After (yes=1)	0.026	0.019
	(0.045)	(0.034)
Treated*After	0.053	0.082*
	(0.050)	(0.046)
Constant	0.505***	
	(0.033)	
Observations	2372	2372
R-sq	0.142	

*p < 0.10, ** p < 0.05, *** p < 0.01.
Reported coefficients are average marginal effects.
Robust standard errors are clustered by enumeration area, the primary sampling unit, and are reported in parentheses.

Table B.4: Gross Primary School Enrollment Rate in Ethiopia by Year

Year	%
1981	37.93
1982	41.28
1983	42.45
1984	40.96
1985	38.37
1986	38.06
1987	41.34
1988	42.27
1989	40.53
1990	36.56
1991	32.77
1992	26.40
1993	22.81
1994	26.95
1995	30.87
1996	36.91
1997	42.10
1998	51.47
1999	50.30
2000	54.92
2001	60.46
2002	63.41
2003	64.95
2004	68.51
2005	81.01
2006	86.97
2007	94.71
2008	102.33
2009	102.28
2010	101.55

Source: World Bank

Table B.5: Difference-in-Differences Estimates of the Effect of the Education Reform on On-time School Enrollment
Dependent Variable: Binary Indicator for Enrollment in Grade 1 by Age 7

	(1) LPM	(2) Probit
Treated (yes=1)	0.037	-0.307**
	(0.120)	(0.138)
After (yes=1)	-0.208	-0.217
	(0.159)	(0.135)
Treated*After	0.352**	0.364**
	(0.173)	(0.189)
Girl (yes=1)	-0.018	-0.018
	(0.016)	(0.015)
Birth order	0.096***	0.107***
	(0.008)	(0.008)
Household size	-0.053***	-0.060***
	(0.005)	(0.006)
Dad's years of schooling	0.013***	0.010***
	(0.004)	(0.003)
Mom's years of schooling	0.015***	0.009***
	(0.005)	(0.004)
Dad's age	-0.002	-0.002
	(0.001)	(0.001)

Mom's age	-0.012***	-0.014***
	(0.002)	(0.002)
hh has piped water (yes=1)	0.073*	0.065**
	(0.038)	(0.033)
hh has electricity (yes=1)	0.143**	0.090
	(0.068)	(0.059)
hh has pit latrine (yes=1)	0.066**	0.059**
	(0.028)	(0.024)
hh owns land (yes=1)	-0.012	-0.001
	(0.047)	(0.037)
hh owns farm animal (yes=1)	-0.055*	-0.053*
	(0.033)	(0.030)
proportion of hhs with piped water	-0.068	-0.063
	(0.050)	(0.045)
proportion of hhs with electricity	-0.195**	-0.134*
	(0.089)	(0.077)
proportion of hhs with pit latrine	-0.044	-0.031
	(0.044)	(0.042)
proportion of hhs with land	-0.085	-0.077
	(0.075)	(0.059)
proportion of hhs with farm animal	0.118**	0.118**

	(0.057)	(0.054)
Urban area (yes=1)	0.193***	0.146***
	(0.049)	(0.038)
Constant	0.669***	
	(0.127)	
State fixed effects	Yes	Yes
State-by-year fixed effects	Yes	Yes
Observations	2372	2372
R-sq	0.243	
Log Likelihood		-1000.591

*p < 0.10, ** p < 0.05, *** p < 0.01.

Reported coefficients are average marginal effects.

Robust standard errors are clustered by enumeration area, the primary sampling unit, and are reported in parentheses.

Table B.6: Difference-in-Differences Estimates of the Effect of the Education Reform on On-time School Enrollment

	(1)	(2)
	Enrollment Status	Log(Age at Enrollment)
Treated (yes=1)	-0.307**	0.158***
	(0.138)	(0.055)
After (yes=1)	-0.217	0.145**
	(0.135)	(0.060)
Treated*After	0.364**	-0.197***
	(0.189)	(0.068)
Girl (yes=1)	-0.018	0.002
	(0.015)	(0.008)
Birth order	0.107***	-0.087***
	(0.008)	(0.004)
Household size	-0.060***	0.046***
	(0.006)	(0.003)
Dad's years of schooling	0.010***	-0.006***
	(0.003)	(0.002)
Mom's years of schooling	0.009***	-0.010***
	(0.004)	(0.002)
Dad's age	-0.002	0.002**
	(0.001)	(0.001)

Mom's age	-0.014***	0.010***
	(0.002)	(0.001)
hh has piped water (yes=1)	0.065**	-0.037**
	(0.033)	(0.018)
hh has electricity (yes=1)	0.090	-0.047
	(0.059)	(0.031)
hh has pit latrine (yes=1)	0.059**	-0.013
	(0.024)	(0.014)
hh owns land (yes=1)	-0.001	0.007
	(0.037)	(0.020)
hh owns farm animal (yes=1)	-0.053*	-0.002
	(0.030)	(0.016)
proportion of hhs with piped water	-0.063	0.039
	(0.045)	(0.025)
proportion of hhs with electricity	-0.134*	0.040
	(0.077)	(0.044)
proportion of hhs with pit latrine	-0.031	0.009
	(0.042)	(0.024)
proportion of hhs with land	-0.077	0.049
	(0.059)	(0.035)
proportion of hhs with farm animal	0.118**	-0.013

	(0.054)	(0.030)
Urban area (yes=1)	0.146***	-0.075***
	(0.038)	(0.024)
Constant		1.541***
		(0.061)
State fixed effects	Yes	Yes
State-by-year fixed effects	Yes	Yes
Observations	2372	2372
R-sq		0.384

*p < 0.10, ** p < 0.05, *** p < 0.01.

Robust standard errors are clustered by enumeration area, the primary sampling unit, and are reported in parentheses.

The dependent variables in column 1 and 2 are binary indicator for enrollment in grade 1 by age 7 and age at enrollment in grade 1, respectively.

B.2 Additional Graphs

Figure B.1: Histogram and Kernel Density of the Treatment Effect, Rural Sample

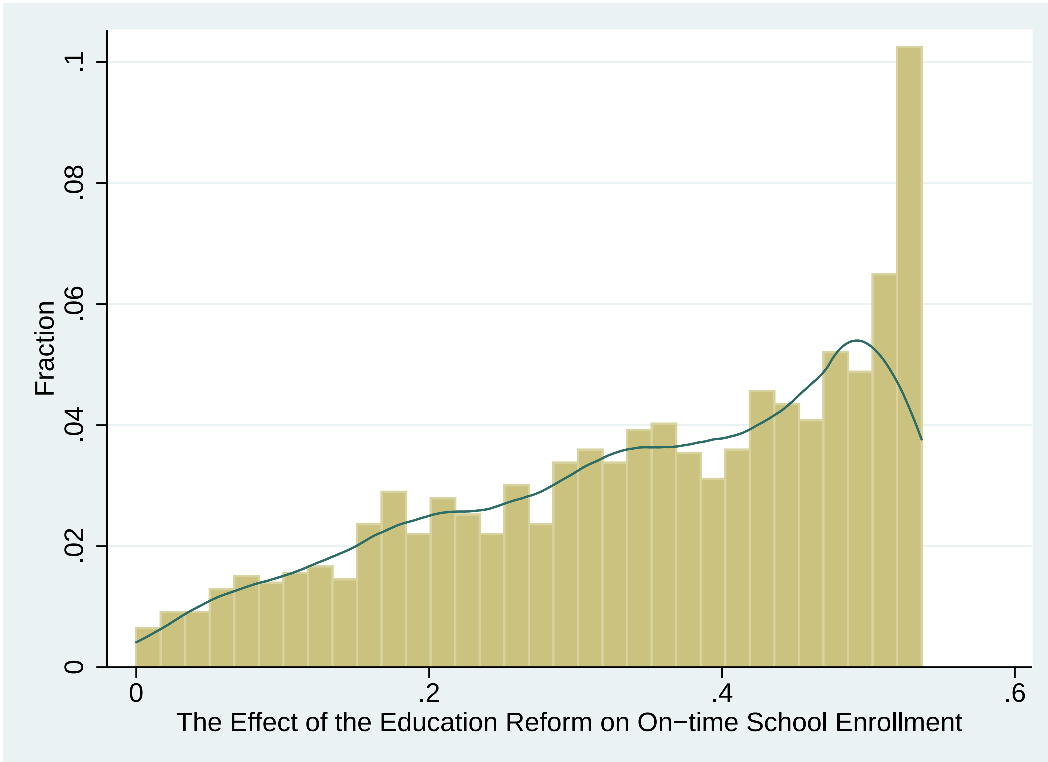
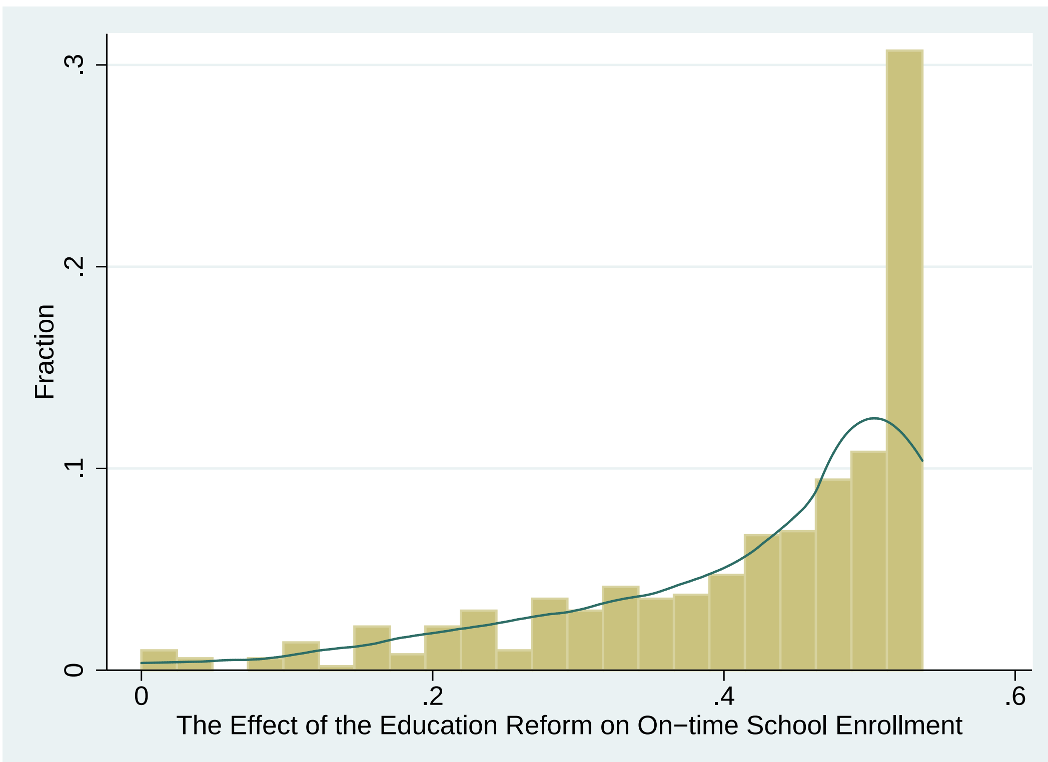


Figure B.2: Histogram and Kernel Density of the Treatment Effect, Urban Sample



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