EMPIRICAL ESSAYS ON CHILDHOOD HUMAN CAPITAL IN ETHIOPIA

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Declaration

This thesis is my own work.

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Abstract

This thesis contains three empirical papers on childhood human capital accumulation in Ethiopia. The first paper examines the long-term education impacts of exposure to the 1998–2000 Ethiopia–Eritrea conflict. I exploit exogenous variation on regional and birth-year intensity of the conflict, using cross-sectional school survey data. The empirical findings indicate that exposure to the conflict during *early* childhood decreases student achievement in mathematics and language scores a decade later (mainly for girls). In addition, exposure to the conflict increases the probability of grade repetition (for boys and girls) and school dropout (for boys only). The paper provides first estimates on the long-term effect of exposure to conflict on human capital accumulation. It further contributes to the study of gender differences with regard to exposure to conflict.

The second paper extends the analysis of the first paper by investigating the effect of conflict on childhood health and education using unique child-level panel data from Ethiopia. It also examines to what extent child health operates as a mechanism through which conflict affects childhood education outcomes. Identification is based on a difference-in-difference approach, using two points in time at which older and younger children have the same average age and controlling for observable household and child-level time-variant characteristics. The paper contributes to an empirical literature that relies predominantly on cross-sectional comparisons of child cohorts born *before* and *after* the war in war-affected and unaffected regions. The results show that war-exposed children have a one-third of a standard deviation lower height-for-age and higher incidence of stunting. In addition, exposed children are less likely to be enrolled in school, complete fewer grades (given enrollment), and are more likely to exhibit reading problems (given

enrollment). Suggestive evidence indicates that the conflict reduces child education directly as well as through its effect on child health.

The final paper examines whether student retention improves achievement later and to what extent the former is correlated with school dropout. The relationship between grade repetition and subsequent achievements as well as school dropout remains entirely an empirical question as theoretical predictions are inconclusive. I apply bivariate probit and endogenous treatment regression models to cross-sectional student-level survey data from Ethiopia to examine this relationship. The results indicate that student retention does not improve achievement in mathematics and evidence on the improvement in verbal test scores is relatively weak. In addition, grade repetition is highly correlated with school dropout. The findings have important policy implications because they suggest that grade repetition may be viewed as a waste of resources in the absence of higher test scores, indicating that students in Ethiopia should not be retained and that an investigation of alternative policies is needed.

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Chapter 1

Introduction

Prominent economists such as Alfred Marshall and others argue that human capital is one of the key drivers of economic growth (Becker, 1994; Grossman, 1972; Mincer, 1970; Schultz, 1961). In addition, a substantial body of empirical work demonstrates the role of human capital for economic growth (see for instance, Barro, 2001; Bhargava et al., 2001; Bloom et al., 2001; Cohen and Soto, 2007; Eicher and Garcıa-Peñalosa, 2001; Hanushek, 2013; Strauss and Thomas, 1998). Currie and Almond (2011) argue that "in recent economics, the focus is on how human capital accumulation responds to the early childhood environment." And most developing countries are facing natural and human-induced shocks that affect childhood environment negatively, one of which is armed conflict.

Exposure to armed conflict or war can have severe implications for development (World Bank, 2011; Gates et al., 2012). Conflict may severely affect the human capital of children, leading to poor health (including malnutrition) and lower education (Akresh, 2016). The young are usually vulnerable to adverse shocks, such as those resulting from war, because attachment to their parents could be disrupted (Barbara, 2006). They are also at an early stage of growth and many human capital investments are age-specific (Justino et al., 2013).

Conflict can have a lasting impact through its effect on childhood human capital. A substantial literature documents that poor health (nutrition) and education in early-life can have lasting effects on adulthood health, education, and labor market outcomes (Alderman et al., 2006; Currie, 2008; Currie and Vogl, 2012; Duflo, 2001; Lucas, 1998; Lucas et al., 1999; Martorell, 1999; Silventoinen, 2003). For instance, Grantham-McGregor et al. (2007) argue that disadvantaged children in developing countries who do not reach their developmental potential are less likely to be productive adults. However, there is some evidence that suggests that some of early-life damages can be remediated (Currie and Almond, 2011).

In the next two chapters of this thesis, I focus on the potential channels – childhood education and health outcomes – through which the Ethiopia–Eritrea conflict could have affected long-term human capital accumulation in Ethiopia. In the first paper, I go beyond the fairly common analysis of the effect of conflict on school enrollment or years of schooling of school-age children at times of conflict. I contribute to the literature by studying the effects of exposure to conflict in *early* childhood (before school-age) on test scores, the probability of grade repetition and school dropout. I also contribute to the mixed empirical evidence on gender differences in the effect of exposure to conflict.

In the second paper, I examine the effect of conflict on childhood health and to what extent childhood health plays as a mechanism through which conflict affects education. The empirical evidence is typically based on cross-sectional data and involves a comparison of cohorts that were born $before$ and $after$ war (conflict), using a difference-in-difference (DID) approach that exploits temporal and spatial variation. The key implicit assumption of these studies is that in the absence of war, changes in outcomes between those children born $after$ (younger) and $before$ (older) the war would have been the same for war-affected and unaffected areas. However, if poverty caused war or if war-affected areas tend to be economically poor or drought prone ex-ante, the key assumption may not hold because older cohorts may accumulate larger deficits than younger cohorts, and as a result the effect of conflict may be overestimated (Duflo, 2003; Martorell and Habicht, 1986). The opposite holds if war-affected areas tend to be economically rich or less drought prone ex-ante. However, I argue that the former holds in the Ethiopian context and in most other circumstances because areas of civil war tend to be economically poor ex-ante, making it harder to identify causal effects (Blattman and Miguel, 2010).

In the third paper, I investigate the effect of grade repetition on subsequent performance in mathematics and language test scores and the correlation between grade repetition and school dropout. According to a report by the UNESCO Global Education Digest (2012), 32.2 million pupils worldwide repeated a grade in primary education in 2010. The report also states that "pupils who are over-age for their grade due to late entry and/or repetition are at greater risk of leaving school early and children with the least opportunities arising from poverty and compounding disadvantages are most likely to repeat grades and leave school early"(UNESCO, 2012). Furthermore, Manacorda (2012) argues that "grade repetition is common in less developed countries and often accompanied by *low* enrollment and *high* dropout rates, the combination of the two often referred to as '*wastage*'."

The cost and benefit of student retention has been a matter of intense debate in the economics and psychology literature. Eide and Showalter (2001) and Alet et al. (2013) argue that economists have not contributed much to this lively and important debate although the heart of the problem is a resource allocation decision. The opponents of student retention argue that it harms students through stigma and lower self-esteem as a result of disturbing existing peer relationships and alienating students from school. This potentially leads to lower performance and school dropout (Bowman, 2005; Brophy, 2006; Jimerson and Ferguson, 2007). In addition, grade repetition delays labor market entry, imposing substantial monetary cost. Furthermore, every repeater affects school resources in the same way as a new student, resulting in a compromise of per pupil school inputs (Koppensteiner, 2014).

On the other hand, proponents argue that grade repetition raises the expectations of schools and so may provide an incentive to study hard (Gary-Bobo et al., 2016), consequently, increasing students' achievement. In addition, it can increase students' achievement by allowing low achieving students to catch-up during an extra year of schooling, thereby mastering the relevant content before proceeding to the next grade. Furthermore, Koppensteiner (2014) argues that student retention "may help to make classes more homogeneous in achievement and therefore easier to teach by improving the match between peers in the classroom."

Some schools or countries allow automatic grade promotion despite low performance of a student (e.g, most Scandinavian countries) while others such as France, Portugal, and Spain have strict retention rules (EACEA and Eurydice, 2011). In Ethiopia, on average, nearly 10 percent of primary school students repeated a grade every year in the past decade (UNESCO, 2016). This is above the average of developing countries, which is about 5 percent, but equivalent to the Sub-Saharan average which is also around 10 percent. However, some schools allow automatic grade promotion despite low performance, yet, there is no empirical evidence to support such a policy. We do not know, at least in the context of Ethiopia, whether or not repeating a grade would help to improve subsequent student performance. Empirical evidence on grade repetition has important policy implications because grade repetition may be viewed as a waste of resources if it does not improve student performance.

1.1 Conflict Exposure and Human Capital Accumulation

Despite the importance of the effects of conflict on economic welfare and human capital, there is only a small yet recently growing body of empirical literature. Moreover, theoretically, the impact of war (conflict) on long-term economic performance of a country is not clear. Neoclassical economic theory predicts rapid recovery (convergence) in the aftermath of a war. Some empirical evidence that supports this argument is found in Sierra-Leone (Bellows and Miguel, 2006), Germany (Brakman et al., 2004), Japan (Davis and Weinstein, 2002), and Vietnam (Miguel and Roland, 2011). Chen and his co-authors perform a cross-country analysis and conclude that "when the end of war marks the beginning of lasting peace, recovery and improvement are achieved" (Chen et al., 2008). However, conflict may still have detrimental consequences on health, education, and labour market outcomes of individuals and households at the microeconomic level even if economic growth converges at the aggregate level (Akbulut-Yuksel, 2014; Blattman and Miguel, 2010; Justino et al., 2013). I review the empirical literature on microeconomic impacts of exposure to conflict on education and health in the following two subsections.

1.1.1 Education

The empirical evidence on the effect of exposure to conflict on education varies considerably depending on the context of the analysis, with effects ranging from negative to positive. Of the studies that find a negative effect of conflict on schooling outcomes, Leon (2012) documents a persistent effect of exposure to violence before school age (in utero, early childhood, and preschool age). Children affected before school-age accumulated 0.31 fewer years of schooling upon reaching adulthood in contrast to those exposed after starting school who fully catch up. Similarly, Akresh and de Walque (2010) find that children exposed to the Rwandan genocide accumulate 0.5 fewer years of primary education.

Verwimp and Van Bavel (2013) find that boys exposed to violent conflict in Burundi are less likely to complete primary schooling. Chamarbagwala and Morán (2011) also find that rural Mayan school-age males and females exposed to the three periods of civil war in Guatemala completed fewer years of schooling. Shemyakina (2011a) exploits regional and temporal variation of the 1992–1998 armed conflict in Tajikistan and finds that exposure to conflict decreases the probability of completing mandatory schooling of girls but does not affect boys. Kibris (2015) also finds a significant association between Turkish-Kurdish conflict and university entrance exam scores of Turkish students. However, this study does not control for region-specific time trends and therefore assumes omitted time-varying effects are not correlated with conflict, as pointed out by Shemyakina (2011a), making it difficult to isolate the effect of the conflict from the effect of other factors such as economic conditions.

In contrast, a recent study by Valente (2013), based on data from Nepal finds that conflict intensity is associated with an increase in schooling attainment especially for females (although the abductions by Maoists have a negative effect). This study is able to measure conflict in great detail of intensity but it does not control for region-specific time trends. A more recent study from the same country using individual fixed effects by Pivovarova and Swee (2015) concludes that there is no effect of the intensity of war on schooling attainment. The authors argue that "while the conventional difference-in-differences estimation yields a positive effect of war intensity on schooling attainment the effect diminishes completely when we augment difference-in-differences with individual fixed effects, suggesting that unobserved individual heterogeneity may play an important role."

In addition, Justino et al. (2013) use two waves of cross-sectional survey data from Timor-Leste and find a rapid recovery for girls and negative long-term effects of exposure to the conflict on primary school attendance and completion among boys. Furthermore, Arcand and Wouabe (2009) find that exposure to the 27-year-long Angolan civil conflict does not significantly affect household expenditures, increases school enrollment and decreases fertility and child health. Shemyakina (2011b) finds that school-age women who were exposed to the 1992–1998 Tajik civil war had a lower likelihood of completing primary schooling but surprisingly, better labor market outcomes (Shemyakina, 2015).

In summary, with the exception of Leon (2012), most of the literature focuses on the effect of exposure to conflict on school-age children at times of conflict in contrast to exposure at *early* childhood (before school-age). Moreover, with the exception of Kibris (2015), the focus is, unlike what is investigated in my thesis, on school enrollment or years of schooling. Thus, the second chapter of this thesis contributes to the empirical literature by studying the effects of exposure to conflict at *early* childhood (before school-age children) on achievement in test scores, the probability of grade repetition, and school dropout.

1.1.2 Health

The effect of early-life exposure to conflict on child and adult health is largely negative. Akresh et al. (2012a) study adults with and without exposure to the Nigerian civil war, which took place during a time when these adults were children and adolescents. They find that adults who were exposed to the war during childhood and adolescence (and who survived) exhibit reduced stature relative to unexposed adults 30 to 40 years later. Surprisingly, they find that exposure during adolescence has a larger effect than exposure during childhood.

Akresh et al. (2011) look at the effect of civil war and crop failure on child stunting using cross-sectional household data from Rwanda. They find that the height-for-age z-score of children (both boys and girls) who were exposed to conflict is about one standard deviation lower. Similarly, Bundervoet et al. (2009) find a 0.35 standard deviation (and a 0.047 standard deviation for each additional month of conflict exposure) lower height-for-age z-score for those exposed to civil war in rural Burundi. Minoiu and Shemyakina (2012) find 0.489 standard deviations lower in height-for-age z-scores for those exposed to armed conflict in Ivory Coast.

A closely related study by Akresh et al. (2012b) uses the 2002 cross-sectional Eritrean Development and Health Survey (DHS) to investigate the impact of the Ethiopia–Eritrea war on child height-for-age z-score, exploiting exogenous variation in geographic extent and timing of conflict. They find that exposure to the war decreases the child height-for-age z-scores by about 0.45 standard deviations.

In sum, empirical evidence is typically based on cross-sectional data and involves a comparison of cohorts that were born $before$ and $after$ war (conflict), using a DID approach and exploiting temporal and spatial variation. The key implicit assumption of these studies is that in the absence of war, changes in outcomes between those children born $after$ (younger) and $before$ (older) the war would have been the same for war-affected and unaffected areas. However, if poverty caused war or if war-affected areas tend to be economically poorer or drought prone ex-ante, the key assumption may not hold because older cohorts may accumulate larger deficits than younger cohorts. As a result, the effect of the conflict may be overestimated (Martorell and Habicht, 1986; Duflo, 2003). The opposite holds if war-affected areas tend to be economically richer or less drought prone ex-ante. However, I argue that the former holds in the Ethiopian context and in most other circumstances because areas of civil war tend to be economically poor ex-ante, making it harder to identify causal effects (Blattman and Miguel, 2010).

1.2 Grade Repetition and Education Outcomes

The empirical evidence on the effect of grade repetition is rather mixed. On the negative side, student retention is found to be one of the most significant predictors of school dropouts (Bowman, 2005; Jacob and Lefgren, 2004, 2009; Stearns et al., 2007). For instance, Jimerson et al. (2002) conduct a systematic review of 17 studies and conclude that grade retention is one of the most powerful predictors of dropout status. Using data from junior high school in Uruguay, Manacorda (2012) finds that student retention causes substantial dropout and lower achievement even 4-5 years later. In addition, Bowman (2005) argues that the financial cost and the pronounced cost to children's self-esteem could outweigh the benefit. Furthermore, grade retention may also have negative effects on adolescence behavior (Jimerson and Ferguson, 2007; Pagani et al., 2001).

Hong and Yu (2007) conclude that "there is no evidence that early grade retention brings benefits to the retainee's reading and maths learning toward the end of the elementary years." Similarly, McCoy and Reynolds (1999) find an association of grade retention with lower achievement and suggest that "intervention approaches other than grade retention are needed to better promote school achievement and adjustment." Silberglitt et al. (2006) also find that retained students did not experience a benefit in their growth rate, even compared to similar but promoted students, and perform less compared to a random sample of students.

In addition, Alet (2010) and Alet et al. (2013) use data from France and instrument initial student achievement by kindergarten schooling duration and type as well as grade repetition by quarter of birth. The main result indicates that grade repetition has only a transitory positive effect on achievement as these initial associated gains in achievement translate into losses a few years later. Similarly, Chen et al. (2010) use data from Shaanxi province in China and find a negative effect of grade retention on school performance of students.

Another line of research suggests that there is a negative externality effect of student retention. A recent study by Hill (2014) uses course fixed effects from US high schools and shows that "5 to 10 percent of share of repeaters in a given course results in a moderate to significant increase in the probability of course failure for first-time course-takers." He also provides evidence that distinguishes course repetition externalities from low ability peer effects (not related to grade repetition).

Do the studies that largely document negative effects of grade repetition imply that automatic grade promotion helps to improve academic achievement? A recent study using a difference-in-difference approach by Koppensteiner (2014) finds that automatic grade promotion reduces maths test scores by 7 percent in Brazil. The author interprets this as a disincentive effect. Fertig (2004) finds a significantly positive effect of class repetition on educational outcomes in Germany. Using a nationally representative dataset in the US, Dong (2010) provides evidence that holding children back in kindergarten has positive but diminishing effects on the academic performance up to third grade. These findings suggest that automatic grade promotion may not improve student outcomes, and therefore, provide evidence in favor of retention policies.

In the African context, the empirical evidence is quite limited despite higher occurrence of grade repetition in the continent. Glick and Sahn (2010) use data from Senegal and exploit variation across schools in test score thresholds for promotion to compare retained and promoted students with the same level of performance. They conclude that repeating students are more likely to leave school before completing primary school than students with similar ability who are not held back, pointing to the need for alternative measures to improve the skills of lagging children. However, they can not control for unobserved school fixed effects. In contrast, a more recent empirical study on Uganda that exploits variation across schools and time with regard to the adoption of automatic promotion policy (i.e., the new policy affected only the public schools) finds that this policy translates into an increase in learning outcomes in reading and mathematics at primary three and primary six levels (Okurut, 2015). However, this study assumes that public and private schools will have the same trend with regard to factors affecting learning outcomes.

1.3 Thesis Purpose, Approach, and Contribution

This thesis presents three self-contained empirical research papers on how early life exposure to conflict can have long–term effects on human capital accumulation through its effect on childhood education and health; and whether or not student retention (grade repetition) improves test score outcomes in Ethiopia.

I focus on health and education and apply careful econometric techniques to identify the impacts of exposure to conflict on (a) childhood education using cross-sectional school survey data; (b) childhood health and education outcomes using panel data of younger and older cohorts in Ethiopia. In addition, I examine the effect of grade repetition on schooling outcomes of children using the same cross-sectional school survey data.

In the first two papers I use a difference-in-difference approach and exploit exogenous variation on timing and regional exposure to the 1998–2000 Ethiopia–Eritrea conflict to identify causal effects. In the third paper, I use endogenous treatment regression and bivariate probit models to deal with the endogniety concerns associated with the selection effect of grade repetition. All these papers use observational data and I exploit quasi-experimental settings for causal inference by carefully assessing the sources of potential biases and providing the best possible remedy to such biases given the available data.

The primary contributions of this thesis are the new empirical results addressing the three questions mentioned above. In addition, I introduce several methodological innovations. The empirical method used in Chapters 2 and 3 is essentially a difference-in-difference approach. In chapter three, I extend this approach to focus on the same-age cohorts of younger and older children at two points in time while controlling for time-variant characteristics. I first replicate the results from the literature that is not restricted to cohorts of the same age using the entire (pooled) sample and cross-sectional sub-samples at each survey round. Afterwards, I restrict the analysis to cohorts that have exactly the same average age. The effect of exposure to the 1998–2000 Ethiopia–Eritrea conflict declines by nearly 40 percent ranging from 0.30 to 0.38 standard deviations in height-for age z-score, which corresponds to a 12- to 15-percentage points higher likelihood of being stunted.

In Chapter 4, I apply endogenous treatment regression and bivariate probit models to deal with endogniety problems associated with selection effect of grade repetition. The former allows me to estimate a simultaneous recursive maximum likelihood such that plausible assumptions are made regarding the correlation structure between the unobservables that affect the treatment and the unobservables that affect the potential outcomes. In the second, following Altonji et al. (2005), I use a recursive bivariate probit model that examines the sensitivity of the estimates to the correlation between the unobserved factors that determine grade repetition and the outcome – school dropout. I show estimates of the grade repetition effect for different values of the correlation between the unobservable determinants of grade repetition and school dropout.

1.4 Key Research Questions and Results

Chapter 2—The Persistent Effect of Conflict on Childhood Education

The first research paper asks the questions: What is the effect of exposure at *early* childhood to the 1998–2000 Ethiopia–Eritrea conflict on achievement in mathematics and language test scores for year 4 and 5 primary school students 12 years after the conflict? Does exposure to the conflict increase the likelihood of grade repetition and school dropout? Do these effects vary by gender?

The findings suggest that exposure to conflict during *early* childhood, in contrast to *late* childhood, increases the probability of grade repetition (for boys and girls) and school dropout (for boys only), and decreases student achievement in mathematics and language scores (mainly for girls) a decade later.

Chapter 3—The Effect of Conflict on Childhood Health and Education: Panel Data Evidence

Chapter 3 turns to the research question raised in Chapter 2 and further examines in detail using panel data of older and younger cohorts using two points in time at which older and younger children have the same average age and controlling for observable household and child-level time-variant characteristics. The specific research questions are: What is the effect of exposure to the 1998–2000 Ethiopia–Eritrea conflict on height-for-age z-score of 8 year old children? Does exposure to conflict have an effect on school enrollment, grade completion, and reading proficiency? To what extent does child health plays a mediating role in the effect of conflict on child education?

The key findings are that war-exposed children have, on average, a one-third of a standard deviation lower height-for-age z-score and a 12-percentage point higher incidence of childhood stunting. In addition, exposed children are less likely to be enrolled in school, complete fewer grades (given enrollment), and are more likely to exhibit reading problems (given enrollment). While analyzing the exact mechanisms is challenging, suggestive evidence indicates that child health reduces child education, in particular the probability of child enrollment at school.

Chapter 4—Grade Repetition, School Dropout, and Student Achievement in Ethiopia: A Waste of Resources?

The final research paper asks: Does student retention (grade repetition) improve learning outcomes later? Is student retention correlated with school dropout? Schools or parents make students repeat a grade under the rationale that student retention will improve learning outcomes later. However, whether retention improves student achievement remains entirely an empirical question as theoretical predictions are inconclusive.

The findings suggest that student retention does not improve achievement in maths and evidence on the improvement in verbal test scores is relatively weak. In addition, grade repetition is highly correlated with school dropout. However, the evidence suggests that the correlation between grade repetition and school dropout is explained by unobserved heterogeneity.

1.5 Organisation

This thesis has five chapters. Chapters 2–4 present the core research. Chapter 2 examines the persistent impacts of exposure to conflict during early life on education. Chapter 3 extends the analysis of Chapter 2 and examines the effect of conflict on childhood health and education using panel data and examines the possibility of child health as a mechanism through which exposure to conflict affects child education. Chapter 4 presents findings from the analysis on whether or not student retention will improve learning outcomes later. Chapter 5 concludes by summarizing the key findings and by discussing their implications and outlines of future directions of research in this arena.

Chapter 2

The Persistent Effect of Conflict on Childhood Education

Abstract

This paper examines the persistent effect of the 1998–2000 Ethiopia–Eritrea conflict on human capital accumulation. The empirical findings indicate that exposure to conflict during *early* childhood increases the probability of grade repetition (for boys and girls) and school dropout (for boys only), and decreases student achievement in mathematics and language scores (mainly for girls) a decade later. Identification of the effect is based on a difference-in-difference approach that exploits temporal and regional variation of the conflict. These effects are robust when including region-specific trends, school, grade, class, and teacher level fixed effects, and other student and family characteristics. The paper provides the first estimates on the long-term effect of exposure to conflict at *early* (before school-age) childhood on test scores of primary school students.

2.1 Introduction

Heckman (2006) argues that "life cycle skill formation is a dynamic process in which early inputs strongly affect the productivity of later inputs." It is well-documented that early-life health and education can have lasting impacts into adulthood (Alderman et al., 2006; Currie, 2008; Currie and Vogl, 2012; Duflo, 2001; Heckman et al., 2013) and poor childhood health and exposure to other risks before age five can have detrimental effects on cognitive, motor, and social-emotional development (Grantham-McGregor et al., 2007). However, there is evidence that suggests that some early-life damages can be remediated (Currie and Almond, 2011).

Despite strong and conclusive evidence on the causal effect of conflict on childhood health (Akresh et al., 2012b, 2011; Bundervoet et al., 2009; Weldeegzie, 2017), little is known about the causal effect of conflict on the learning outcomes of children and the evidence to date is inconclusive (Pivovarova and Swee, 2015; Valente, 2013). Furthermore, most studies limit their analysis to enrollment and years of education (Leon, 2012; Shemyakina, 2011a, 2015) or study exposure of conflict for those who were school-age children during times of conflict (Akresh and de Walque, 2010; Chamarbagwala and Morán, 2011; Shemyakina, 2011a, 2015).

However, these outcomes are imperfect because they may indicate how much time students spent at school instead of how much they have learned at school.[1] This implies that increases in the number of years of schooling does not necessarily imply increases in learning and acquiring the required skills and knowledge. Hanushek and Woessmann (2007) conclude that "the cognitive skills of the population – rather than mere school attainment – are powerfully related to individual earnings, to the distribution of income, and to economic growth."

[1] Pritchett (2001) points out that increases in low quality schooling does not raise cognitive skills and productivity, which may explain why schooling does not increase growth in some countries.

This study goes beyond the fairly common analysis of the effect of conflict on school enrollment or years of schooling and contributes to the literature by studying the effects on achievement in test scores and the probability of grade repetition and school dropout. I use unique school survey data from a sample of more than 10,000 children in 94 schools spread across seven regions in Ethiopia to examine the long-term causal effect of exposure to the 1998–2000 Ethiopia–Eritrea conflict on education outcomes. Identification of the effect is based on a difference-in-difference (DID) approach. The reduced DID model controls for region-specific time trends, and including school, grade, class, and teacher level fixed effects as well as other student and household characteristics. Furthermore, I do not only compare the outcomes of those born *before* and *after* the conflict in conflict-affected and unaffected areas but also of those born *during* and *after* the conflict in conflict-affected and unaffected areas.

The empirical findings reveal that exposure to the conflict persists a decade later and is manifested through reduced achievement in mathematics and language scores by about 4 percentage points, on average. The effect is larger and statistically significant for girls in contrast to boys. In addition, exposure to the conflict increases the probability of repeating a grade and school dropout by about 10 and 6 percentage points respectively, but no statistically significant effect was detected on school dropout of boys. These effects are relatively large and consistently significant for younger children (born *during* the conflict) relative to older children (born *before* the conflict). This is consistent with studies that suggest that childhood exposure to disadvantage matters more in *early* than in *late* childhood (Leon, 2012).

This paper contributes to the literature on the link between violent conflict and human welfare in several respects. First, I provide the first estimates of the long-term effects of exposure to conflict at *early* (before school-age) childhood on test scores of primary school students, showing that the effects of violence at *early* childhood is persistent in contrast to exposure at *late* childhood. Second, the analysis extends the effect of conflict on the likelihood of school dropout and grade repetition. Third, I use a high-quality large-scale dataset which permits the consideration of region-specific trends and student and family characteristics. Fourth, this is the first empirical evidence to explore these outcomes in the literature using a sample of children from Ethiopia. Finally, it contributes to the inconclusive findings of the literature on the gender differential effect of exposure to the conflict.

The remainder of the chapter is organized as follows. Section 2.2 provides a brief historical explanation of the Ethiopia–Eritrea conflict. Section 2.3 reviews the literature. Section 2.4 describes the dataset and provides a preliminary analysis. Section 2.5 presents the theoretical framework, empirical strategy, and outlines sources of potential bias. Section 2.6 presents and discusses the results. Section 2.7 presents additional results from a set of sensitivity analysis. The final section concludes.

2.2 The History of Ethiopia–Eritrea War

Eritrea was one of the regional states under the umbrella of Ethiopia and became an independent nation after a referendum in 1993 (Tronvoll, 1999). Five years later, a border conflict between Ethiopia and Eritrea led to a war lasting from May 1998 until June 2000 (Abbink, 1998). The Ethiopia–Eritrea War began as a territorial dispute on May 6, 1988, at Badme (a district in Tigray region), a small village on the western side of the international border (Barnes and Tareke, 2010). Later on the war took place in two other locations, Tsorona-Zalambessa and Bure of the Tigray and Afar regions, respectively. Most of the battles took place in the Tigray region. Akresh et al. (2012b) argue that both countries claimed sovereignty over these three areas of war due to the confusion over the border demarcation between the two countries.

The war led to a significant loss of life and material damage for both nations. As documented in a report from Addis Ababa University (2012), both countries committed to large spending to mobilize military forces. It is estimated that the total cost of the conflict was about USD 280 million for Eritrea and USD 397 million for Ethiopia, in addition to nearly 50,000 Eritrean and 75,000 Ethiopian troops lost (Addis Ababa University, 2012). Furthermore, a large number of people was internally displaced. As documented in the Internally Displaced Persons (IDP) global database of the Norwegian Refugee Council, about 315,000 Ethiopians were displaced by December 1998 and this number grew to more than 360,000 on May 2000. 90 percent of these were in the Tigray region and about 30,000 in the Afar region (Global IDP, 2004a,b). The foregone GDP growth and the non-monetary human cost imply a significant impact on the overall economy of both nations.

Due to geographic proximity, it appears likely that children who reside close to the war-affected region were affected more than those in regions farther away from the battlefield. Moreover, after the war ended formally in June 2000, Ethiopia went through a relatively peaceful decade. As a result, children born after the war in both war-affected and unaffected regions were not directly exposed to war. Consequently, causal effects can be identified by comparing changes in outcomes for children born before and after the war in war-affected and unaffected regions. This identification strategy relies on the assumption that the war is exogenous to child health and schooling outcomes in Ethiopia, which seems plausible, given that the war was the result of border dispute between the two countries.[2]

[2] Woldemariam (2015) provides a detailed explanation on the causes of the war.

2.3 Related Literature

Theoretical predictions of the long-term effect of conflict on human capital accumulation are ambiguous (Bellows and Miguel, 2006). At a macroeconomic level, neoclassical economics predicts conflict–affected areas have the possibility of catching up to unaffected areas (Chen et al., 2008). However, Blattman and Miguel (2010) argue that war or violent conflict may still have detrimental consequence on health, education, and labour market outcomes of individuals and households at the microeconomic level even if economic growth converges at the aggregate level (Akbulut-Yuksel, 2014; Justino et al., 2013).

Empirical evidence varies considerably depending on the context of the analysis, ranging from a negative to a positive effect on education. Of the studies that find a negative effect of conflict on schooling outcomes of children, Leon (2012) documents a persistent effect of exposure to violence before school-age (in utero, early childhood, and pre-school age) in Peru. Children affected before school-age accumulated 0.31 fewer years of schooling upon reaching adulthood in contrast to those exposed after starting school who fully catch up. Similarly, Akresh and de Walque (2010) find that children exposed to the Rwandan genocide accumulate 0.5 fewer years of primary education.

Verwimp and Van Bavel (2013) find that boys exposed to violent conflict in Burundi are less likely to complete primary schooling. Chamarbagwala and Morán (2011) also find that rural Mayan school-age males and females exposed to the three periods of civil war in Guatemala completed fewer years of schooling. Shemyakina (2011a) exploits regional and temporal variation of the 1992–1998 armed conflict in Tajikistan and finds that exposure to the conflict decreases the probability of completing mandatory schooling of girls but does not affect boys, but surprisingly, young women exposed to the conflict had better labor market outcomes (Shemyakina, 2015). Kibris (2015) also finds a significant association between Turkish-Kurdish conflict and university entrance exam scores of Turkish students. However, this study does not control for region-specific time trends and therefore assumes that omitted time-varying effects are not correlated with conflict, as pointed out by Shemyakina (2011a), making it difficult to isolate the effect of the conflict from the effect of other factors such as economic conditions.

In contrast, a recent study by Valente (2013), based on data from Nepal finds that conflict intensity is associated with an increase in schooling attainment especially for females (although the abductions by Maoists have a negative effect). This study is able to measure conflict intensity in great detail but it does not control for region-specific time trends. A more recent study from the same country using individual fixed effects by Pivovarova and Swee (2015) concludes that there is no effect of war intensity on schooling attainment. The authors argue that "while the conventional difference-in-differences estimation yields a positive effect of war intensity on schooling attainment the effect diminishes completely when we augment difference-in-differences with individual fixed effects, suggesting that unobserved individual heterogeneity may play an important role."

In addition, Justino et al. (2013) use two waves of cross-sectional survey data from Timor-Leste and find a rapid recovery for girls and negative long-term effects of exposure to the conflict on primary school attendance and completion among boys. Furthermore, Arcand and Wouabe (2009) find that exposure to the 27-year-long Angolan civil conflict does not significantly affect household expenditures, increases school enrollment and decreases fertility and child health.

In summary, with the exception of Leon (2012), most of the literature focus on two of these exposure and outcome aspects – the effect of exposure to conflict on school-age children at times of conflict in contrast to before school-age children and outcomes such as enrollment and years of education. Thus, this paper particularly contributes to the evidence on the long-term effect of conflict on pre-school age children. In addition, in this chapter, I focus on test score outcomes instead of enrollment and years of education.

2.4 Data and Preliminary Analysis

2.4.1 Data Description

I use cross-sectional schools survey data collected by the Young Lives team in Ethiopia and Oxford University. This is a separate dataset from the Young Lives longitudinal childhood poverty study, which tracks a sample of older and younger cohorts in four developing countries, including Ethiopia, Peru, Vietnam, and India. The school survey data consists of a sample of all pupils studying in grades 4 and 5 during the 2012/13 academic year in all schools located within the geographic boundaries of each of the 30 sentinel sites spread across seven regions in Ethiopia – Tigray, Afar, Somalia, Amhara, SNNP, Addis Ababa, and Oromia (Aurino et al., 2014).

I combine the student level data with teacher, class, and school level information. This unique data set gives a wealth of information to construct birth-year and geographic exposure to the conflict. It also provides a number of student and family covariates such as age of starting school, attendance of pre-school, long-term health problems of a student as well as the number of family members, the extent of help a student receives with school work, parental education, the number of meals a child usually eats per day and so on.

Table 2.1: Summary Statistics

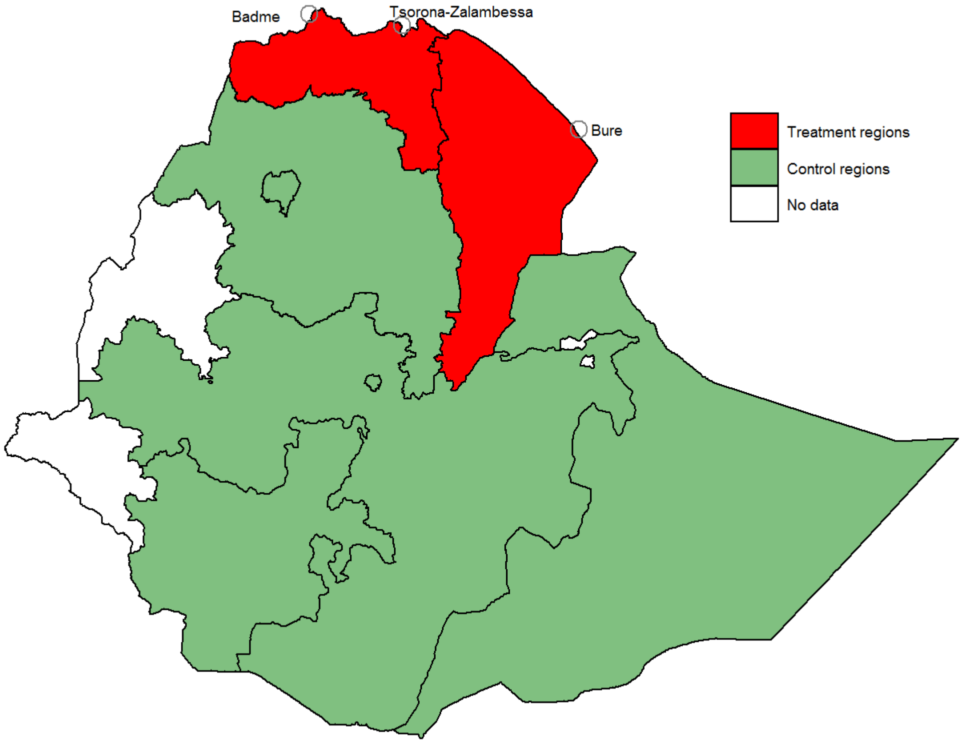
Variable	Mean	Std. Dev.	Min.	Max.	N
Region=Addis Ababa	0.192	0.394	0	1	11641
Region=Amhara	0.101	0.302	0	1	11641
Region=Oromiya	0.093	0.291	0	1	11641
Region=SNNP	0.241	0.428	0	1	11641
Region=Tigray	0.154	0.361	0	1	11641
Region=Somali	0.12	0.325	0	1	11641
Region=Afar	0.099	0.298	0	1	11641
Urban=1	0.712	0.453	0	1	11641
School identification	40.135	25.602	1	94	11641
Class identification	2.996	2.059	1	10	11641
Teacher identification	6.281	4.1	2	16	11641
Grade=4	0.512	0.5	0	1	11641
Maths grade	63.919	15.725	7	100	10945
Language grade	68.219	16.071	8	100	10947
Dropped out of school	0.177	0.382	0	1	11549
Repeated grade	0.243	0.429	0	1	11610
Sex=Girl	0.503	0.5	0	1	11641
Age in yrs	11.584	1.831	8	34	11641
Age starting school yrs	6.867	1.816	4	29	11641
Absence days since Oct 1	1.111	2.321	0	26	11641
Absence days since wave 1	5.887	7.717	0	72	10951
Attended pre-school	0.516	0.5	0	1	11573
Longterm health problem	0.228	0.42	0	1	11641
No of meals/day=1	0.044	0.205	0	1	11615
No of meals/day=2	0.148	0.355	0	1	11615
No of meals/day>=3	0.808	0.394	0	1	11615
No of other people	6.963	3.572	1	30	11641
Mother alive	0.935	0.247	0	1	11618
Father alive	0.829	0.377	0	1	11556
Mother read & write	0.479	0.5	0	1	11641
Father read & write	0.59	0.492	0	1	11641
No one read & write	0.071	0.257	0	1	11641
Help at home=never	0.182	0.386	0	1	11606
Help at home=always	0.459	0.498	0	1	11606
Help at home=sometimes	0.359	0.48	0	1	11606

Source: Young Lives school survey data from Ethiopia, 2012/13.

The summary statistics above show that the age of students in the sample ranges from 8–34 at the end of 2012. This gives ample variation in terms of year of birth (some born *before*, *during* and *after* the conflict). Nearly 72 percent of students are from urban areas. About 50 percent of the students in the sample are in grade four and 50 percent of these students are boys. On average, a student starts school at age seven. About 80 percent of students eat at least three times a day and more than half of the students get support with school work from family members. In addition, about 50 percent of parents can read and write.

Figure 2.1 shows the regions of the sentinel sites of the school survey. The geographic proximity to the conflict makes Tigray and Afar regions war-intense regions because the war was concentrated in the Northern and Northeastern part of the country: in Badme, Tsorona-Zalambessa and Bure of the Tigray and Afar regions. The remaining regions serve as a control group.

Figure 2.1: Treatment and Control Regions and District of War Locations



Source: Author's own work based on from Young Lives school survey data 2012/13, Ethiopia.

Because the war lasted from May 1998–June 2000, I call cohorts that belong to the pre-1998 birth year as the "before" [3] whereas those born between 1998 and 2000 (inclusive) as "during". Those who are born from 2001 onwards are "after" cohorts. The variations across birth year cohorts and geographic proximity permits a comparison of outcomes for those born before and after the conflict (and during and after the conflict) from conflict-affected and unaffected regions in a DID setting. Table 2.2 below shows the mean differences between these cohorts by war-affected and unaffected regions for each gender.

[3]Note: the "before" cohorts include those born in 1994, 1995, 1996, and 1997 only so that the outliers above 18 years of age at the time of the survey but still studying in year 4 and 5 are removed. As a result, 23 observations are deleted from the analysis.

Table 2.2: Mean Differences of Outcome Variables by Cohort and Region

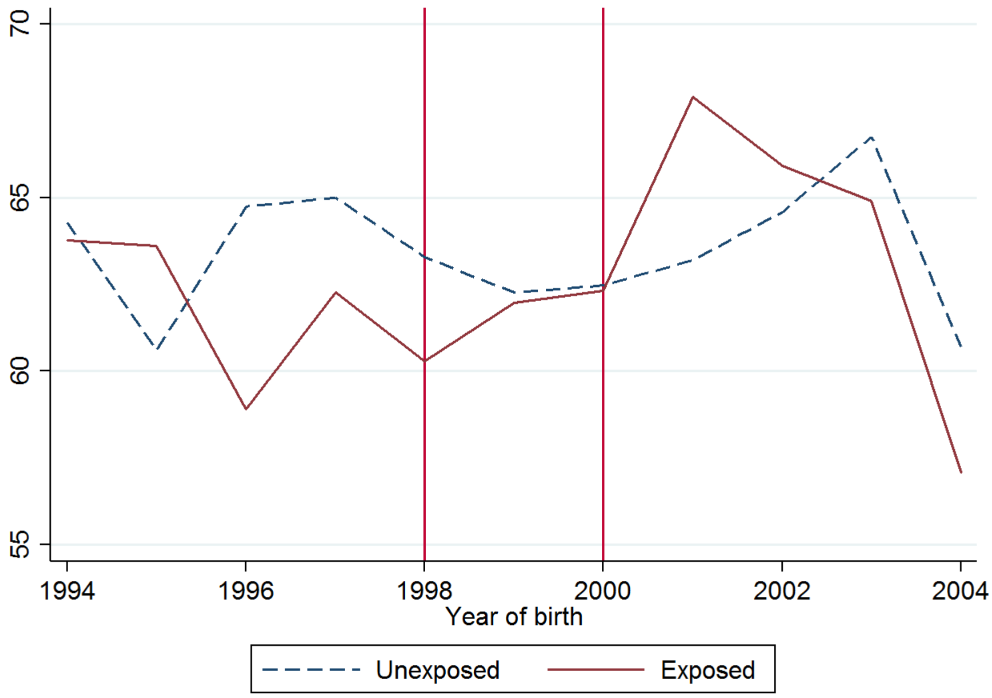
Boys sample						
	War			Non-war		
	Before	After	Diff.	Before	After	Diff.
Maths grade (max.100)	64.88	66.68	-1.80	65.03	64.19	0.84
Language grade (max.100)	61.13	68.92	-7.80***	68.18	69.46	-1.28
Repeated grade (%)	0.35	0.18	0.17***	0.26	0.22	0.04
Dropped out of school (%)	0.34	0.11	0.22***	0.35	0.12	0.23***
N	104	939		253	1960	
	During	After	Diff.	During	After	Diff.
Maths grade (max.100)	63.14	66.68	-3.54***	62.86	64.19	-1.33*
Language grade (max.100)	62.06	68.92	-6.86***	67.43	69.46	-2.03***
Repeated grade (%)	0.44	0.18	0.26***	0.29	0.22	0.08***
Dropped out of school (%)	0.32	0.11	0.21***	0.22	0.12	0.09***
N	401	939		1578	1960	
Girls sample						
	Before	After	Diff.	Before	After	Diff.
Maths grade (max.100)	55.85	66.80	-10.95***	65.31	64.07	1.24
Language grade (max.100)	53.13	69.49	-16.36***	69.80	70.25	-0.45
Repeated grade (%)	0.37	0.16	0.21**	0.20	0.20	-0.01
Dropped out of school (%)	0.44	0.08	0.36***	0.37	0.10	0.27***
N	54	989		204	2191	
	During	After	Diff.	During	After	Diff.
Maths grade (max.100)	60.45	66.80	-6.35***	62.55	64.07	-1.53**
Language grade (max.100)	61.05	69.49	-8.44***	68.11	70.25	-2.14***
Repeated grade (%)	0.43	0.16	0.27***	0.27	0.20	0.06***
Dropped out of school (%)	0.27	0.08	0.19***	0.21	0.10	0.11***
N	319	989		1624	2191	

Note: War refers to Tigray and Afar regions, Non-war refers to the rest of regions. $*p < 0.10, **p < 0.05, ***p < 0.01$. Source: Author's descriptive analysis based on Young Lives data.

2.4.2 Preliminary Analysis

I begin by plotting mean scores of mathematics and language tests, grade repetition rates, and school dropout rates by birth year and region of exposure to the conflict. These provide a preliminary indication of the differences in schooling outcomes within each birth-year cohort between conflict-affected and unaffected regions.

Figure 2.2: Mean Maths Score by Year of Birth and Regional Exposure



Source: Author's analysis based on Young Lives school survey data 2012/13, Ethiopia.

Figure 2.2 shows that children from war-affected regions who belong to the 1995-1999 birth cohorts have lower mathematics achievement, on average, compared to the same birth year cohorts from unaffected regions. This gap in education achievement between war-affected and unaffected children for those cohorts born before and during the conflict is much more pronounced in language scores as shown in Figure 2.3. However, this is not the case for post-conflict cohorts.

Figure 2.3: Mean Language Score by Year of Birth and Regional Exposure

Source: Author's analysis based on Young Lives school survey data 2012/13, Ethiopia.

A similar trend is observed in grade repetition and school dropout rates (Figures 2.4 and 2.5).

Figure 2.4: Mean Grade Repetition Rate by Year of Birth and Regional Exposure

Source: Author's analysis based on Young Lives school survey data 2012/13, Ethiopia.

Figure 2.5: Mean School Dropout Rate by Year of Birth and Regional Exposure

Source: Author's analysis based on Young Lives school survey data 2012/13, Ethiopia.

In sum, Figures 2.2–2.5 show that, on average, the cohorts of children born either $before$ or $during$ the conflict (born in 2000 and earlier) from conflict-affected regions tend have lower mathematics and language scores and a higher grade repetition and school dropout rates compared to those that come from the unaffected regions. However, for those cohorts born after the conflict (from 2001 onwards), there is no significant difference in outcome measures between conflict-affected and unaffected regions, providing preliminary evidence of the effect of the conflict. But, these differences could also reflect regional differences during the pre-conflict period in other factors such as poverty and convergence of these regions during the post-conflict era.

2.5 Theoretical Framework and Empirical Strategy

Suppose a typical education production function model, in line with those discussed in Hanushek (1979) and following Leon (2012) , where the stock of education (S_t) of an individual at time t is a function of her endowments in each period $(E_1, ..., E_t)$, the history of educational inputs to which she had access $(N_1, ..., N_t)$, factors related to the (time-invariant) demographic characteristics (X), and community characteristics $(C_1, ..., C_t)$ such as the availability of schools and teachers.

$$S_t = s(E_1, ..., E_t, N_1, ..., N_t, X, C_1, ..., C_t) \tag{2.1}$$

The endowment in each time period, $E_1, ..., E_t$, is determined by both demand and supply-side factors. From a demand side perspective, there are genetic factors (G), household's endowments (E_{h0}), and environmental experiences and conditions at the start of each period (U). The supply side factors are denoted by C_t.

$$E_t = g(G, E_{h0}, U_t, C_t) \tag{2.2}$$

The temporal and regional location of residence jointly determine the exposure to the conflict. I estimate a reduced form linear DID model that allows me to identify the effect of the conflict by estimating changes in education outcomes for cohorts born *before* or *during* and *after* the conflict from conflict-affected areas relative to the changes between these cohorts in unaffected areas. This setting provides exogenous variation induced by the time when the conflict started in a specific geographic location.

$$\begin{aligned} Y_{ij} = c + \sum R_j + \alpha * before_i + \beta * during_i + \pi * ExposedR_j \\ + \theta * before_i * ExposedR_j + \gamma * during_i * ExposedR_j + \\ \delta * Age_i * \sum R_j + X_i + FE + \epsilon_{ij} \end{aligned} \tag{2.3}$$

where Y_{ij} is the outcome of interest, c is a constant and $\sum R_j$ indicates a set of four region dummies, $before_i$ takes on the value 1 if a child is born *before* the war (before 1998) and 0 otherwise while $during_i$ takes on the value 1 if a child is born *during* the war (from 1998–2000 inclusive) and 0 otherwise. $ExposedR_j$ takes on the value 1 for children from the war-affected regions and 0 otherwise. $Age_i * \sum R_j$ is the interaction of region dummies by age of a child capturing region-specific time trends. X_i includes a vector of child and family level characteristics and FE includes school, grade, class, and teacher level fixed effects. ϵ_{ij} is the unobserved error term.

While α and β are the cohort effects for those born *before* and *during* the conflict respectively, π captures the selection effect. The relevant parameters of interest are θ and γ. θ is the DID coefficient on the effect of the war when comparing those cohorts born *before* the war in war-affected and unaffected regions relative to those born *after* the war in war-affected and unaffected regions. Similarly, γ denotes the effect of the conflict for those cohorts born *during* the war relative to those born *after* the war in war-affected and unaffected regions. If exposure to conflict matters more during *early* childhood than *late* childhood, I expect γ to be larger than θ both in magnitude and statistical significance.

Exposure to conflict or war can affect both demand- and supply-side factors in different ways. I limit the discussion of such pathways relevant to this context and based on the findings of the study. From the demand side perspective, war can affect individual endowments through many channels. First, war can affect a member of the household, for example, by killing or wounding, affecting household endowments as it brings an income shock for the household. This can then be manifested in terms of reduction of food availability to the household. This is particularly severe in case of poor country households such as Ethiopia where there is binding income constraint and high incidence of vulnerability to malnutrition.

This reduction of food availability could affect indirectly child's health through it's effect on maternal nutrition and mental health. Mulder et al. (2002) conclude that maternal psychological factors may significantly contribute to pregnancy complications and unfavorable development of the (unborn) child. Such effects may for instance lead to a low birth weight (Camacho, 2008). In addition, a reduction in the availability of food could directly affect child health or malnutrition. There is a strong link between child health and cognitive development (Grantham-McGregor et al., 2007) .

Second, violence exposure could affect a child's mental health as a result of traumatic experiences, thereby psychologically affecting children exposed to conflict (Dyregrov et al., 2000; Papageorgiou et al., 2000), which will in turn affect cognitive ability (Currie and Stabile, 2006; Grimard and Laszlo, 2014) .

Violence can also affect the supply side factors by destroying a number of community educational resources and other relevant infrastructure directly. Furthermore it can limit the physical movement of people, including students and teachers. However, such effects seem negligible in this context as in this study (1) the effect of the conflict is not analyzed on school-age children during the conflict and (2) the effect is found on younger children instead of older children, suggesting that the above two pathways are more likely.

Using this DID model, it is possible to identify the causal effect of the conflict on education outcomes under certain assumptions. First, the key identification assumption for this model is the parallel trend assumption – in the absence of the war, changes in education outcomes are the same across regions. However, if there are region-specific differentials over time with regard to the factors affecting child education, then θ and γ will be biased. Therefore, the extended DID model in equation three above controls for region-specific time trends which are captured by the age with region dummies.

Second, another source of potential bias could come from the systematic migration of people. It is more likely that people were displaced from their initial settlement as a consequence of the conflict. However, this is not a threat to the identification strategy because most of (more than 90 percent) such displacements took place within a region (Global IDP, 2004a,b). Furthermore, I conduct two placebo tests. Primarily, I use the younger cohorts only (those born $after$ the conflict) and conduct the same DID analysis. The hypothesis is that I should not find significant effects as these cohorts are control groups. Next, I use both older and younger cohorts in the remaining five regions (excluding the war-affected regions) and assign each region at a time to a treatment group and the rest to control groups, to test the sensitivity of the results. In most of these cases, statistically insignificant results should be expected as this is a falsification test

(Section 2.7).

Third, systematic sample selection bias due to differences in mortality rates across regions over time as a result of the war could potentially lead to an underestimate of the true effect. For this reason, the estimated effect should be interpreted as an effect of the war on survivors, that may potentially underestimate the effect on the average cohorts.

While it is not possible to test the common trend assumption directly, the data set contains a rich set of information on child and household specific co-variates, which help to control for several observables. Thus I control for school, grade, class, and teacher level fixed effects and a number of other child and family level characteristics. Controlling for these factors does not change the results qualitatively. Finally, I show that the results pass several falsification tests.

2.6 Results and Discussion

The first and fourth columns of all the tables provides the estimates after controlling for region and cohort fixed effects only providing results from the basic DID model. From the simple DID model (Columns 1 and 4 of Table 2.3), results show that both cohorts born $before$ and *during* the conflict have lower achievement in both mathematics and language scores.

Table 2.3: The Effect of Conflict on Human Capital Accumulation

VARIABLES	(1) Maths	(2) Maths	(3) Maths	(4) Language	(5) Language	(6) Language
Panel A: All sample						
1.before#1.ExposedR	-5.898***	-2.008	-1.736	-9.959***	-3.287*	-3.002*
	(1.450)	(1.710)	(1.683)	(1.656)	(1.837)	(1.717)
1.during#1.ExposedR	-3.370***	-1.939**	-1.529*	-5.765***	-2.833***	-2.410***
	(0.740)	(0.818)	(0.797)	(0.806)	(0.841)	(0.824)
Observations	10,616	10,616	10,616	10,616	10,616	10,616
R-squared	0.036	0.226	0.282	0.041	0.222	0.277
Panel B: Male sample						
1.before#1.ExposedR	-2.852	-0.149	-0.335	-6.868***	-1.633	-1.958
	(1.939)	(2.460)	(2.381)	(2.245)	(2.608)	(2.442)
1.during#1.ExposedR	-2.113**	-0.455	0.0719	-4.964***	-2.042*	-1.489
	(1.056)	(1.184)	(1.154)	(1.138)	(1.197)	(1.168)
Observations	5,235	5,235	5,235	5,235	5,235	5,235
R-squared	0.025	0.222	0.277	0.026	0.217	0.274
Panel C: Female sample						
1.before#1.ExposedR	-11.22***	-4.896**	-3.023	-15.19***	-5.689**	-4.090*
	(1.896)	(2.226)	(2.309)	(2.170)	(2.498)	(2.377)
1.during#1.ExposedR	-4.830***	-3.907***	-3.463***	-6.589***	-3.906***	-3.565***
	(1.029)	(1.116)	(1.087)	(1.137)	(1.195)	(1.177)
Observations	5,381	5,381	5,381	5,381	5,381	5,381
R-squared	0.056	0.267	0.324	0.061	0.263	0.313
Region FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
Gender dummy	N	Y	Y	N	Y	Y
Age in yrs	N	Y	Y	N	Y	Y
Age#Region dummies	N	Y	Y	N	Y	Y
Urban dummy	N	Y	Y	N	Y	Y
School FE	N	Y	Y	N	Y	Y
Grade FE	N	Y	Y	N	Y	Y
Class FE	N	Y	Y	N	Y	Y
Teacher FE	N	Y	Y	N	Y	Y
Add.controls	N	N	Y	N	N	Y

Note: The dependent variable in Columns 1-3 is Semester I maths grade out of 100 while it is Semester I language grade in Columns 4-6. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Additional covariates include age at starting school, number of days absent from school, number of other people, and indicators for attendance of preschool, having long-term health problems, mother and father alive, mother and father can read and write, no one reads in the family, number of meals usually eaten per day, frequency of help with study. These same set of controls are included throughout all tables and so the same notes apply unless otherwise mentioned. Source: Author's analysis based on Young Lives school survey data from Ethiopia. See appendix for complete results.

However, the effect on mathematics achievement for those born *before* the conflict goes away once I control for region-specific time trends and other covariates. But the effect on language achievement remains significant. Looking at Columns 3 and 6 of Table 2.3 (in panel A) reveals that the effect of the conflict on maths and language scores for those born *during* the conflict is about 1.5 and 2.5 percentage points lower compared to those not exposed. This is a large effect given that it is a long-term effect, which persists 12 years after the event. Panels B and C reveal that the effect is large and highly significant for girls in both mathematics and language scores. The results indicate that girls are severely affected by the conflict, whereas the effect on boys is statistically insignificant. This could be due to household behavior at times of shocks that may result in favoring sons over daughters.

Looking at the literature, the impact of conflict on gender differentials in schooling appears to vary contextually (Buvinič et al., 2013). For instance, armed conflict reduces boys' educational attainment more than that of girls' in cases where boys participate in the military (Swee et al., 2009) or in contexts where boys are likely to work to compensate conflict-induced shocks (Justino et al., 2013; Rodriguez and Sanchez, 2012). In addition, in cases where girls are less likely to be in school during the pre-conflict period, boys' educational attainment may decline more than that of girls' because girls are already less educated (De Walque, 2006; Verwimp and Van Bavel, 2013).[4]

However, the effect of conflict on girls' schooling could be more severe than boys' if parents seek to protect their girls from rape and other threats (Shemyakina, 2011a) or when households' resource allocation decisions favor boys over girls (Singh and Shemyakina, 2016). In Ethiopia, Hadley et al. (2008) find evidence that support their hypothesis that "where girls have historically experienced discrimination, buffering is preferentially aimed at boys, especially as the household

[4]See Figure 2A in the appendix for suggestive evidence in favor of this hypothesis.

experiences greater levels of food stress."

Table 2.4: The Effect of Conflict on Human Capital Accumulation (2)

	(1) Repeat	(2) Repeat	(3) Repeat	(4) Dropout	(5) Dropout	(6) Dropout
Panel A: All sample						
1.before#1.ExposedR	0.155***	-0.0626	-0.0848	0.0230	-0.109**	-0.0999*
	(0.0441)	(0.0535)	(0.0529)	(0.0453)	(0.0540)	(0.0537)
1.during#1.ExposedR	0.182***	0.115***	0.102***	0.0957***	0.0468**	0.0477**
	(0.0227)	(0.0261)	(0.0256)	(0.0204)	(0.0230)	(0.0229)
Observations	10,616	10,616	10,616	10,616	10,616	10,616
R-squared	0.049	0.102	0.132	0.051	0.093	0.105
Panel B: Male sample						
1.before#1.ExposedR	0.114**	-0.119	-0.132*	-0.00478	-0.0748	-0.0680
	(0.0563)	(0.0756)	(0.0737)	(0.0565)	(0.0718)	(0.0716)
1.during#1.ExposedR	0.163***	0.0841**	0.0747**	0.111***	0.0768**	0.0731**
	(0.0314)	(0.0378)	(0.0371)	(0.0285)	(0.0329)	(0.0326)
Observations	5,235	5,235	5,235	5,235	5,235	5,235
R-squared	0.050	0.111	0.144	0.053	0.108	0.119
Panel C: Female sample						
1.before#1.ExposedR	0.197***	-0.0316	-0.0600	0.0863	-0.106	-0.0942
	(0.0727)	(0.0821)	(0.0847)	(0.0764)	(0.0904)	(0.0898)
1.during#1.ExposedR	0.198***	0.146***	0.131***	0.0767***	0.0150	0.0208
	(0.0331)	(0.0371)	(0.0365)	(0.0292)	(0.0329)	(0.0328)
Observations	5,381	5,381	5,381	5,381	5,381	5,381
R-squared	0.053	0.121	0.149	0.053	0.099	0.114
Region FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
Gender dummy	N	Y	Y	N	Y	Y
Age in yrs	N	Y	Y	N	Y	Y
Age#Region dummies	N	Y	Y	N	Y	Y
Urban dummy	N	Y	Y	N	Y	Y
School FE	N	Y	Y	N	Y	Y
Grade FE	N	Y	Y	N	Y	Y
Class FE	N	Y	Y	N	Y	Y
Teacher FE	N	Y	Y	N	Y	Y
Add.controls	N	N	Y	N	N	Y

Note: The dependent variable in Columns 1-3 takes on the value 1 if a child has repeated a grade, and 0 otherwise while it takes the same value if a child has dropped out of school in columns 4-6. Results reported are from linear probability model but coefficients from the marginal effects of the probit model are very similar. See Table 2.3 for the remaining notes.

The estimates in Table 2.4 suggest that xposure to the conflict increases the likelihood of repeating a grade and school dropout, on average, by about 10 and 5 percentage points, respectively (Columns 3 and 6 of Table 2.4). Consistent with previous test score results, the effect of conflict on repeating a grade is larger for girls compared to boys, 13 and 7.5 percentage points, respectively. However, exposure to the conflict increases school dropout of boys only. This may be due to higher school dropout rates of girls during the pre-conflict period (De Walque, 2006; Verwimp and Van Bavel, 2013).

Similar to the test score analysis, this chapter also ascertains the effect on grade repetition and school dropout for cohorts born *during* the conflict (in contrast to those born $before$ the conflict) compared to those born $after$ the conflict. These effects are large in magnitude, given that the mean rate of grade repetition and school dropout across the entire sample is 24 and 17 percent, respectively. Surprisingly, older cohorts born in conflict-affected areas $before$ the conflict exhibit lower school dropout rates compared to younger cohorts born in conflict-unaffected areas $after$ the conflict (Columns 5 and 6, Panel A) and these numbers are driven by the male sample (Column 3, Panel B).

2.7 Further Sensitivity Analysis

While the previous results are robust to including region-specific time trends, a range of fixed effects and a number of other student and family level characteristics, I perform two additional falsification tests in this section. First, I conduct a DID analysis based on sub-dividing the sample of cohorts of children born $after$ the war into two groups: older children (those born in 2002) and younger children (those born from 2003 onwards). The hypothesis tested here is that there is no effect if these cohorts constitute a suitable control group because these cohorts were born $after$ the war, and were not exposed to the conflict. The results are summarized in Table 2.5 below.

Indeed the results in Tables 2.5 support this hypothesis and all the parameters are statistically insignificant even at a 10 percent level of significance. This suggests that the younger cohorts constitute suitable counterfactual for the older cohorts after controlling for cohort and region fixed effects.

Table 2.5: Falsification Test: The Effect of Conflict on Human Capital Accumulation

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A	Maths	Maths	Maths	Language	Language	Language
1.Older#1.ExposedR	2.043	-1.110	-0.769	0.845	-0.793	-0.315
	(1.486)	(1.676)	(1.614)	(1.571)	(1.635)	(1.566)
Observations	3,376	3,376	3,376	3,376	3,376	3,376
R-squared	0.053	0.303	0.350	0.051	0.309	0.362
Panel B	Repeat	Repeat	Repeat	Dropout	Dropout	Dropout
1.Older#1.ExposedR	-0.0458	0.0248	0.0238	-0.0302	-0.00730	-0.0138
	(0.0380)	(0.0458)	(0.0459)	(0.0315)	(0.0350)	(0.0352)
Observations	3,376	3,376	3,376	3,376	3,376	3,376
R-squared	0.013	0.119	0.137	0.003	0.081	0.090
Region FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
Gender dummy	N	Y	Y	N	Y	Y
Age in yrs	N	Y	Y	N	Y	Y
Age#Region dummies	N	Y	Y	N	Y	Y
Urban dummy	N	Y	Y	N	Y	Y
School FE	N	Y	Y	N	Y	Y
Grade FE	N	Y	Y	N	Y	Y
Class FE	N	Y	Y	N	Y	Y
Teacher FE	N	Y	Y	N	Y	Y
Add.controls	N	N	Y	N	N	Y

Note: In Panel A, the dependent variable in Columns 1-3 is Semester I maths grade out of 100 and the dependent variable in Columns 4-6 is Semester I language grade out of 100. In Panel B, the dependent variable in Columns 1-3 takes on a value 1 if a child has repeated a grade, and 0 otherwise and the dependent variable in Columns 4-6 takes on a value 1 if a child has dropped out of school, and 0 otherwise Results reported are from linear probability model but coefficients from the marginal effects of the probit model are very similar. See notes to Table 2.2.

Second, I exclude the main war-affected regions (Tigray and Afar) and repeat the DID analysis using the remaining five regions as follows. I choose each region at a time and assign it to a treatment group while the remaining regions are being assigned to a control group. This approach allows me to use five alternative treatment/control groups to perform the DID analysis using only the Placebo regions. The resulting estimates are expected to be insignificant because the main war-affected regions are excluded from the analysis. That is, these regions are effectively control groups to the war-affected regions. I present a summary of the results obtained from these Placebo regions in Tables 2.6 and 2.7 below.

Table 2.6: Falsification Test II: The Effect of Conflict on Human Capital Accumulation

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Maths	Maths	Maths	Language	Language	Language
1.before#1.Addis Ababa	-1.788	1.593	2.133	-0.342	-0.0203	0.278
	(2.032)	(3.358)	(3.197)	(1.849)	(3.219)	(3.079)
1.during#1.Addis Ababa	-3.990***	-1.515	-1.345	-3.494***	-2.034	-1.880
	(0.900)	(1.408)	(1.358)	(0.823)	(1.326)	(1.276)
1.before#1.Amhara	5.250*	-0.724	1.471	4.379*	0.336	2.019
	(3.058)	(4.698)	(4.181)	(2.658)	(4.523)	(4.062)
1.during#1.Amhara	1.159	-2.143	-0.602	1.456	-1.376	0.118
	(1.192)	(1.911)	(1.748)	(1.125)	(1.860)	(1.746)
1.before#1.Oromia	2.553	-6.308	-7.828*	3.561	2.455	1.207
	(2.390)	(4.346)	(4.063)	(2.559)	(4.930)	(4.615)
1.during#1.Oromia	2.188*	-1.858	-2.563	1.109	0.379	-0.245
	(1.121)	(1.968)	(1.840)	(1.206)	(2.069)	(1.943)
1.before#1.SNNP	-3.767**	-0.00412	0.0228	-2.439	-5.421*	-5.188*
	(1.663)	(2.859)	(2.719)	(1.710)	(3.022)	(2.880)
1.during#1.SNNP	0.847	3.031**	2.674**	2.028***	0.822	0.468
	(0.766)	(1.268)	(1.213)	(0.766)	(1.306)	(1.258)
1.before#1.Somalia	2.430	2.963	1.476	-1.786	6.816*	5.516
	(2.026)	(3.646)	(3.533)	(2.488)	(3.899)	(3.784)
1.during#1.Somalia	1.425	0.370	-0.0819	-0.726	2.568	2.116
	(0.973)	(1.722)	(1.683)	(1.046)	(1.747)	(1.721)
Observations	7,810	7,810	7,810	7,810	7,810	7,810
Region FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
Gender dummy	N	Y	Y	N	Y	Y
Age in yrs	N	Y	Y	N	Y	Y
Age#Region dummies	N	Y	Y	N	Y	Y
Urban dummy	N	Y	Y	N	Y	Y
School FE	N	Y	Y	N	Y	Y
Grade FE	N	Y	Y	N	Y	Y
Class FE	N	Y	Y	N	Y	Y
Teacher FE	N	Y	Y	N	Y	Y
Add.controls	N	N	Y	N	N	Y

Note: The dependent variable in Columns 1-3 is Semester I maths grade out of 100. The dependent variable in Columns 4-6 is Semester I language grade out of 100. See notes for Table 2.3.

The results in Table 2.6 show that there is no effect in the five Placebo regions in terms of both mathematics and language achievement (except in mathematics achievement for SNNP and Oromia regions). The results in Table 2.7 show that, after controlling for all observables, there is usually no effect on grade repetition and school dropout (except in the Addis Ababa and Oromia regions) when the Placebo regions are being compared. Although I observe a few significant results, this does not mean that the former estimates of the effect of conflict are biased. That is, the identification strategy requires a parallel trend assumption of treatment and control regions on average but it does not necessarily require a parallel trend assumption of the treatment regions compared to each of the control regions.[5]

[5] Observing a few significant estimates should not be a serious concern. I repeat the main analysis after excluding these regions and the results remain largely unchanged. Complete results are reported in Appendix Tables 2C and 2D.

Table 2.7: Falsification Test II: The Effect of Conflict on Human Capital Accumulation (2)

	(1) Repeat	(2) Repeat	(3) Repeat	(4) Dropout	(5) Dropout	(6) Dropout
1.before#1.Addis Ababa	-0.00690	-0.180**	-0.165*	0.119**	0.138	0.127
	(0.0464)	(0.0902)	(0.0883)	(0.0535)	(0.0887)	(0.0876)
1.during#1.Addis Ababa	0.0595**	0.00359	0.00146	0.0224	0.0282	0.0203
	(0.0237)	(0.0412)	(0.0404)	(0.0202)	(0.0356)	(0.0355)
1.before#1.Amhara	-0.0659	-0.177	-0.167	-0.0567	-0.222*	-0.210*
	(0.0662)	(0.132)	(0.129)	(0.0685)	(0.121)	(0.120)
1.during#1.Amhara	0.0374	-0.0207	-0.0419	0.0351	-0.0399	-0.0458
	(0.0330)	(0.0600)	(0.0592)	(0.0282)	(0.0515)	(0.0509)
1.before#1.Oromia	0.0265	0.138	0.149	0.0111	0.0890	0.0989
	(0.0572)	(0.107)	(0.104)	(0.0722)	(0.127)	(0.127)
1.during#1.Oromia	-0.0455*	0.00126	0.0142	0.0721**	0.107**	0.109**
	(0.0253)	(0.0461)	(0.0452)	(0.0286)	(0.0534)	(0.0532)
1.before#1.SNNP	0.0473	0.102	0.0769	0.0797	-0.00795	-0.0125
	(0.0461)	(0.0880)	(0.0869)	(0.0499)	(0.0845)	(0.0838)
1.during#1.SNNP	0.0258	0.0328	0.0397	0.00608	-0.0280	-0.0249
	(0.0220)	(0.0400)	(0.0393)	(0.0189)	(0.0345)	(0.0343)
1.before#1.Somalia	-0.0502	0.101	0.105	-0.260***	-0.0731	-0.0628
	(0.0466)	(0.0861)	(0.0846)	(0.0486)	(0.0854)	(0.0852)
1.during#1.Somalia	-0.123***	-0.0428	-0.0445	-0.140***	-0.0521	-0.0411
	(0.0236)	(0.0431)	(0.0429)	(0.0208)	(0.0380)	(0.0379)
Observations	7,810	7,810	7,810	7,810	7,810	7,810
Region FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
Gender dummy	N	Y	Y	N	Y	Y
Age in yrs	N	Y	Y	N	Y	Y
Age#Region dummies	N	Y	Y	N	Y	Y
Urban dummy	N	Y	Y	N	Y	Y
School FE	N	Y	Y	N	Y	Y
Grade FE	N	Y	Y	N	Y	Y
Class FE	N	Y	Y	N	Y	Y
Teacher FE	N	Y	Y	N	Y	Y
Add.controls	N	N	Y	N	N	Y

Note: The dependent variable in Columns 1-3 takes on the value 1 if a child has repeated a grade, and 0 otherwise. The dependent variable in Columns 4-6 takes on the value 1 if a child has dropped out of school, and 0 otherwise. Results reported are from linear probability model but coefficients from the marginal effects of the probit model are very similar. See notes for Table 2.3.

2.8 Conclusion

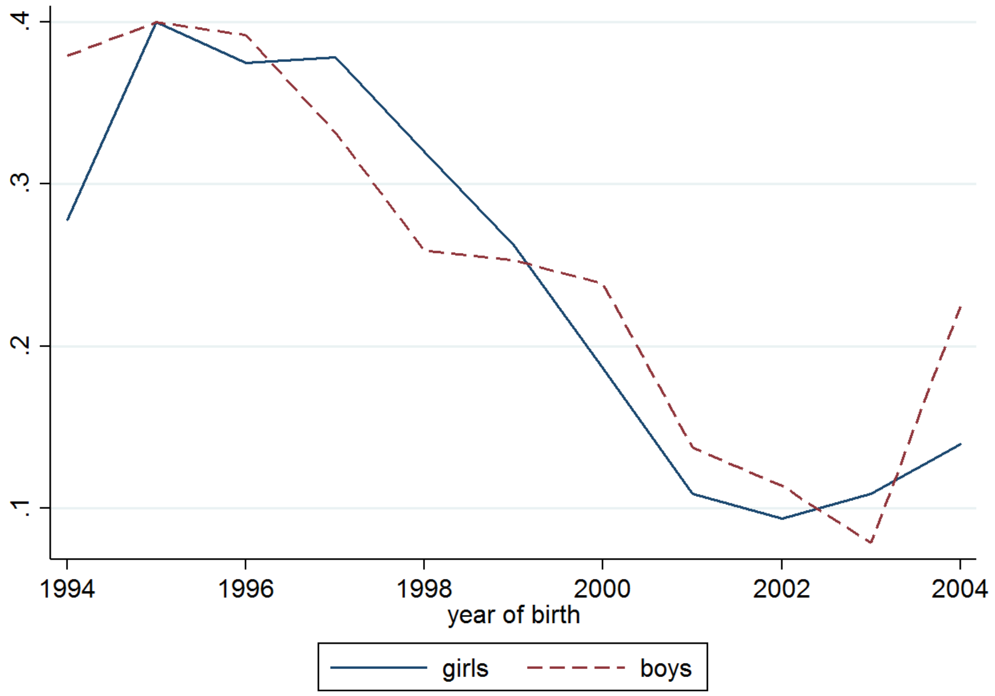
This paper examines the long-term casual effect of the 1998–2000 Ethiopia–Eritrea conflict on human capital accumulation in Ethiopia. I find that, on average, exposure to the conflict during early childhood decreases girls' achievement in mathematics and language scores by about 3.5 percentage points 12 years later. One explanation for this result could be discriminatory behavior of households in ways that favor boys at times of conflict-induced shocks. In addition, exposure to the conflict increases the likelihood of grade repetition among both boys and girls. It also increases the likelihood of school dropout of boys. This finding may be due to initial higher school dropout rates among girls during the pre-conflict period. Given strong evidence of the effect of early-life education on outcomes during adulthood, these results highlight that the long-term consequences of the 1998–2000 Ethiopia–Eritrea conflict are far reaching.

This chapter contributes to the literature by extending the fairly common analysis of the effect of conflict on school enrollment or the number of years of schooling to test scores and to grade repetition and school dropout. It also generates empirical evidence on gender differences in the effect of exposure to conflict. Interestingly, consistent with the medical science literature (Heckman, 2007; Lucas, 1998; Lucas et al., 1999; Martorell, 1999), the effect was observed only for those who were born *during* the conflict in contrast to those who were born *before* the conflict, suggesting that exposure to the conflict in *early* childhood has a persistent effect in contrast to *late* childhood exposure. This evidence implies that, unlike young children, children who are exposed to the conflict at relatively older stages of their development have the possibility to catch up in terms of educational outcomes.

The evidence implies that interventions that target pregnant mothers and young children at the time of conflict can reduce negative long-term welfare effects at least in terms of educational outcomes. These may include, among others, physical protection or evacuation of mothers and young children from conflict areas and appropriate psychological interventions to treat post-war trauma.

2.9 Appendix

Figure 2.A: Mean School Dropout Rate by Year of Birth and Gender



Note: This figure shows mean school dropout rates for each birth-year cohort across the entire sample. Source: Author's own calculations based on Young Lives school survey data 2012/13, Ethiopia.

Table 2.A: The Effect of Conflict on Human Capital Accumulation: Extended Results

VARIABLES	(1) Maths	(2) Maths	(3) Maths	(4) Language	(5) Language	(6) Language
Panel A: All sample						
before = 1	0.952	1.429	1.832	-0.865	0.544	0.961
	(0.838)	(1.293)	(1.237)	(0.840)	(1.328)	(1.270)
ExposedR = 1	-0.458	1.432	2.040	-4.335***	-2.738	-2.096
	(0.513)	(2.191)	(2.142)	(0.505)	(2.404)	(2.301)
1.before#1.ExposedR	-5.898***	-2.008	-1.736	-9.959***	-3.287*	-3.002*
	(1.450)	(1.710)	(1.683)	(1.656)	(1.837)	(1.717)
during = 1	-1.421***	-0.164	0.127	-1.839***	-0.621	-0.264
	(0.374)	(0.564)	(0.544)	(0.369)	(0.567)	(0.548)
1.during#1.ExposedR	-3.370***	-1.939**	-1.529*	-5.765***	-2.833***	-2.410***
	(0.740)	(0.818)	(0.797)	(0.806)	(0.841)	(0.824)
Observations	10,616	10,616	10,616	10,616	10,616	10,616
R-squared	0.036	0.226	0.282	0.041	0.222	0.277
Panel B: Male sample						
before = 1	1.055	0.509	0.755	-0.932	1.304	1.574
	(1.153)	(1.872)	(1.772)	(1.187)	(1.926)	(1.844)
ExposedR = 1	1.097	6.602**	6.304**	-2.775***	3.923	3.271
	(0.766)	(3.035)	(2.826)	(0.749)	(3.245)	(3.106)
1.before#1.ExposedR	-2.852	-0.149	-0.335	-6.868***	-1.633	-1.958
	(1.939)	(2.460)	(2.381)	(2.245)	(2.608)	(2.442)
during = 1	-1.424***	-0.783	-0.611	-1.901***	-0.474	-0.213
	(0.547)	(0.820)	(0.790)	(0.533)	(0.818)	(0.794)
1.during#1.ExposedR	-2.113**	-0.455	0.0719	-4.964***	-2.042*	-1.489
	(1.056)	(1.184)	(1.154)	(1.138)	(1.197)	(1.168)
Observations	5,235	5,235	5,235	5,235	5,235	5,235
R-squared	0.025	0.222	0.277	0.026	0.217	0.274
Panel C: Female sample						
before = 1	0.265	2.034	2.573	-1.170	-0.377	0.183
	(1.203)	(1.779)	(1.721)	(1.172)	(1.845)	(1.769)
ExposedR = 1	-1.779***	-5.302*	-3.778	-5.597***	-10.79***	-9.273**
	(0.689)	(2.991)	(3.223)	(0.683)	(3.847)	(3.617)
1.before#1.ExposedR	-11.22***	-4.896**	-3.023	-15.19***	-5.689**	-4.090*
	(1.896)	(2.226)	(2.309)	(2.170)	(2.498)	(2.377)
during = 1	-1.519***	0.618	0.898	-1.855***	-0.587	-0.227
	(0.512)	(0.781)	(0.753)	(0.511)	(0.790)	(0.762)
1.during#1.ExposedR	-4.830***	-3.907***	-3.463***	-6.589***	-3.906***	-3.565***
	(1.029)	(1.116)	(1.087)	(1.137)	(1.195)	(1.177)
Observations	5,381	5,381	5,381	5,381	5,381	5,381
R-squared	0.056	0.267	0.324	0.061	0.263	0.313
Region FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
Gender dummy	N	Y	Y	N	Y	Y
Age in yrs	N	Y	Y	N	Y	Y
Age#Region dummies	N	Y	Y	N	Y	Y
Urban dummy	N	Y	Y	N	Y	Y
School FE	N	Y	Y	N	Y	Y
Grade FE	N	Y	Y	N	Y	Y
Class FE	N	Y	Y	N	Y	Y
Teacher FE	N	Y	Y	N	Y	Y
Add.controls	N	N	Y	N	N	Y

Note: See notes to Table 2.3.

Table 2.B: The Effect of Conflict on Human Capital Accumulation (2): Extended Results

	(1) Repeat	(2) Repeat	(3) Repeat	(4) Dropout	(5) Dropout	(6) Dropout
Panel A: All sample						
before = 1	0.0312	-0.0962***	-0.106***	0.253***	0.112***	0.105***
	(0.0205)	(0.0365)	(0.0358)	(0.0228)	(0.0364)	(0.0361)
ExposedR = 1	-0.0369***	0.264***	0.294***	-0.00299	-0.0636	-0.0626
	(0.0130)	(0.0780)	(0.0759)	(0.0103)	(0.0682)	(0.0672)
1.before#1.ExposedR	0.155***	-0.0626	-0.0848	0.0230	-0.109**	-0.0999*
	(0.0441)	(0.0535)	(0.0529)	(0.0453)	(0.0540)	(0.0537)
during = 1	0.0823***	0.0260	0.0159	0.105***	0.0543***	0.0510***
	(0.0101)	(0.0169)	(0.0166)	(0.00886)	(0.0150)	(0.0149)
1.during#1.ExposedR	0.182***	0.115***	0.102***	0.0957***	0.0468**	0.0477**
	(0.0227)	(0.0261)	(0.0256)	(0.0204)	(0.0230)	(0.0229)
Observations	10,616	10,616	10,616	10,616	10,616	10,616
R-squared	0.049	0.102	0.132	0.051	0.093	0.105
Panel B: Male sample						
before = 1	0.0513*	-0.0269	-0.0327	0.227***	0.0843	0.0776
	(0.0289)	(0.0534)	(0.0526)	(0.0306)	(0.0513)	(0.0509)
ExposedR = 1	-0.0773***	0.115	0.184*	0.00764	-0.122	-0.104
	(0.0199)	(0.106)	(0.103)	(0.0160)	(0.0832)	(0.0830)
1.before#1.ExposedR	0.114**	-0.119	-0.132*	-0.00478	-0.0748	-0.0680
	(0.0563)	(0.0756)	(0.0737)	(0.0565)	(0.0718)	(0.0716)
during = 1	0.0920***	0.0554**	0.0431*	0.0941***	0.0419*	0.0368*
	(0.0147)	(0.0242)	(0.0238)	(0.0128)	(0.0217)	(0.0216)
1.during#1.ExposedR	0.163***	0.0841**	0.0747**	0.111***	0.0768**	0.0731**
	(0.0314)	(0.0378)	(0.0371)	(0.0285)	(0.0329)	(0.0326)
Observations	5,235	5,235	5,235	5,235	5,235	5,235
R-squared	0.050	0.111	0.144	0.053	0.108	0.119
Panel C: Female sample						
before = 1	0.0169	-0.171***	-0.186***	0.276***	0.128**	0.122**
	(0.0287)	(0.0507)	(0.0499)	(0.0344)	(0.0524)	(0.0517)
ExposedR = 1	-0.00614	0.511***	0.510***	-0.0135	0.0630	0.0630
	(0.0170)	(0.102)	(0.100)	(0.0133)	(0.127)	(0.122)
1.before#1.ExposedR	0.197***	-0.0316	-0.0600	0.0863	-0.106	-0.0942
	(0.0727)	(0.0821)	(0.0847)	(0.0764)	(0.0904)	(0.0898)
during = 1	0.0749***	-0.00995	-0.0167	0.114***	0.0610***	0.0594***
	(0.0138)	(0.0238)	(0.0233)	(0.0123)	(0.0209)	(0.0208)
1.during#1.ExposedR	0.198***	0.146***	0.131***	0.0767***	0.0150	0.0208
	(0.0331)	(0.0371)	(0.0365)	(0.0292)	(0.0329)	(0.0328)
Observations	5,381	5,381	5,381	5,381	5,381	5,381
R-squared	0.053	0.121	0.149	0.053	0.099	0.114
Region FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
Gender dummy	N	Y	Y	N	Y	Y
Age in yrs	N	Y	Y	N	Y	Y
Age#Region dummies	N	Y	Y	N	Y	Y
Urban dummy	N	Y	Y	N	Y	Y
School FE	N	Y	Y	N	Y	Y
Grade FE	N	Y	Y	N	Y	Y
Class FE	N	Y	Y	N	Y	Y
Teacher FE	N	Y	Y	N	Y	Y
Add.controls	N	N	Y	N	N	Y

Note: See notes to Table 2.4.

Table 2.C: The Effect of Conflict on Human Capital Accumulation

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Maths	Maths	Maths	Language	Language	Language
Panel A: All sample						
1.before#1.ExposedR	-7.102***	-2.815	-2.386	-10.74***	-4.321**	-3.780**
	(1.595)	(1.788)	(1.747)	(1.775)	(1.895)	(1.774)
1.during#1.ExposedR	-3.082***	-1.247	-0.920	-5.087***	-2.205**	-1.852**
	(0.794)	(0.849)	(0.828)	(0.852)	(0.870)	(0.851)
Observations	8,092	8,092	8,092	8,092	8,092	8,092
R-squared	0.038	0.237	0.289	0.048	0.251	0.304
Panel B: Male sample						
1.before#1.ExposedR	-4.037*	-0.324	-0.506	-6.931***	-1.892	-2.185
	(2.148)	(2.602)	(2.502)	(2.459)	(2.745)	(2.562)
1.during#1.ExposedR	-2.226**	0.0457	0.498	-4.774***	-1.889	-1.386
	(1.129)	(1.232)	(1.199)	(1.205)	(1.242)	(1.210)
Observations	4,024	4,024	4,024	4,024	4,024	4,024
R-squared	0.025	0.224	0.278	0.030	0.242	0.300
Panel C: Female sample						
1.before#1.ExposedR	-12.35***	-6.372***	-4.390*	-16.68***	-7.301***	-5.442**
	(2.117)	(2.299)	(2.366)	(2.297)	(2.531)	(2.436)
1.during#1.ExposedR	-4.078***	-3.065***	-2.760**	-5.394***	-2.805**	-2.575**
	(1.110)	(1.166)	(1.140)	(1.200)	(1.227)	(1.212)
Observations	4,068	4,068	4,068	4,068	4,068	4,068
R-squared	0.062	0.286	0.338	0.075	0.295	0.343
Region FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
Gender dummy	N	Y	Y	N	Y	Y
Age in yrs	N	Y	Y	N	Y	Y
Age#Region dummies	N	Y	Y	N	Y	Y
Urban dummy	N	Y	Y	N	Y	Y
School FE	N	Y	Y	N	Y	Y
Grade FE	N	Y	Y	N	Y	Y
Class FE	N	Y	Y	N	Y	Y
Teacher FE	N	Y	Y	N	Y	Y
Add.controls	N	N	Y	N	N	Y

Note: This table presents results similar to those of Table 2.2 but the sample does not include Addis Ababa and Oromia. See notes to Table 2.2.

Table 2.D: The Effect of Conflict on Human Capital Accumulation (2)

	(1) Repeat	(2) Repeat	(3) Repeat	(4) Dropout	(5) Dropout	(6) Dropout
Panel A: All sample						
1.before#1.ExposedR	0.158***	0.137	0.138	0.0622	0.0127	0.0193
	(0.0473)	(0.0943)	(0.0928)	(0.0482)	(0.0865)	(0.0865)
1.during#1.ExposedR	0.192***	0.209***	0.204***	0.116***	0.104***	0.102***
	(0.0242)	(0.0411)	(0.0405)	(0.0215)	(0.0358)	(0.0357)
Observations	7,598	7,598	7,598	7,598	7,598	7,598
R-squared	0.052	0.113	0.138	0.049	0.097	0.107
Panel B: Male sample						
1.before#1.ExposedR	0.133**	0.270**	0.251**	0.0300	-0.123	-0.137
	(0.0596)	(0.128)	(0.126)	(0.0592)	(0.117)	(0.117)
1.during#1.ExposedR	0.182***	0.253***	0.245***	0.123***	0.0544	0.0422
	(0.0334)	(0.0584)	(0.0578)	(0.0300)	(0.0507)	(0.0507)
Observations	3,851	3,851	3,851	3,851	3,851	3,851
R-squared	0.046	0.113	0.143	0.051	0.114	0.125
Panel C: Female sample						
1.before#1.ExposedR	0.191**	-0.0182	0.0212	0.121	0.186	0.197
	(0.0785)	(0.147)	(0.146)	(0.0828)	(0.134)	(0.134)
1.during#1.ExposedR	0.200***	0.163***	0.172***	0.104***	0.153***	0.160***
	(0.0352)	(0.0592)	(0.0581)	(0.0307)	(0.0504)	(0.0501)
Observations	3,747	3,747	3,747	3,747	3,747	3,747
R-squared	0.059	0.141	0.165	0.048	0.103	0.116
Region FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
Gender dummy	N	Y	Y	N	Y	Y
Age in yrs	N	Y	Y	N	Y	Y
Age#Region dummies	N	Y	Y	N	Y	Y
Urban dummy	N	Y	Y	N	Y	Y
School FE	N	Y	Y	N	Y	Y
Grade FE	N	Y	Y	N	Y	Y
Class FE	N	Y	Y	N	Y	Y
Teacher FE	N	Y	Y	N	Y	Y
Add.controls	N	N	Y	N	N	Y

Note: This table presents results similar to those of Table 2.3 but the sample does not include Addis Ababa and Oromia. See notes to Table 2.3.

Chapter 3

The Effect of Conflict on Childhood Health and Education: Panel Data Evidence

Abstract

It is well-documented that early-life outcomes can have lasting impacts during adulthood. This paper uses unique child level panel data from Ethiopia to investigate two potential channels – childhood health and schooling outcomes – through which the Ethiopia–Eritrea war may have long-term economic impacts. Identification is based on a difference-in-difference approach, using two points in time at which older and younger children have the same average age, and controlling for observable household and child-level time-variant characteristics. The paper contributes to an empirical literature that relies predominantly on cross-sectional comparisons of child cohorts born before and after the war in war-affected and unaffected regions. The results show that war-exposed children have a one-third of a standard deviation lower height-for-age z-score and a 12-percentage point higher incidence of childhood stunting. In addition, exposed children are less likely to be enrolled in school, complete fewer grades (given enrollment), and are more likely to exhibit reading problems (given enrollment). While analyzing the exact mechanisms is challenging, suggestive evidence indicates that child health reduces child education, in particular the probability of child enrollment at school. These are disconcerting findings, as early-life outcomes can have lasting impacts during adulthood. Future research that

focuses on mechanisms through which war affects children may improve the design of appropriate policies that aim to target and support children confronted with war.

3.1 Introduction

In addition to natural shocks, many countries – especially in Africa – are exposed to human induced shocks, such as armed conflict or war, which can have severe development implications (World Bank, 2011; Gates et al., 2012). War may severely affect the human capital of children, leading to poor health (including malnutrition) and lower education. The young are usually vulnerable to adverse shocks, such as those resulting from war, because attachment to their parents could be disrupted (Barbara, 2006). They are also at an early stage of growth and many human capital investments are age-specific (Justino et al., 2013). For instance, losing a parent can cause both emotional and physical harm, and increase vulnerability to future risk (Beegle et al., 2006).

War can have a lasting impact through its effect on childhood human capital. A substantial body of empirical work documents that poor health, nutritional, and educational deficiency in early life can have lasting consequences during adulthood health, education, and labor market outcomes (Alderman et al., 2006; Currie, 2008; Currie and Vogl, 2012; Duflo, 2001; Lucas, 1998; Lucas et al., 1999; Martorell, 1999; Silventoinen, 2003). For instance, Grantham-McGregor et al. (2007) argue that disadvantaged children in developing countries who do not reach their developmental potential are less likely to be productive adults.

This study looks at the effect of the 1998-2000 Ethiopia–Eritrea war on child health and schooling outcomes in Ethiopia. Unlike the existing empirical literature, it exploits child-level panel data to compare outcomes for children born before (older cohort) and after (younger cohort) the war, in war-affected and unaffected areas, using two points in time at which the two cohorts have the same average age and controlling for time-variant characteristics. The approach requires panel data because I have to be able to track the younger cohort up to the age of the older cohort.

Using a DID approach, I first replicate the results from the literature that does not restrict to cohorts of the same age and that uses the entire (pooled) sample and cross-sectional sub-samples from each survey round.[1] When using the entire (pooled) sample, estimates of the effect of the war ranges (depending on the specifications) from about -0.53 to -0.6 standard deviations in the height-for age z-score, which corresponds to a 17 to 19 percentage point higher likelihood of being stunted. Then, controlling for time-variant characteristics, I restrict the analysis to cohorts that have exactly the same average age (I refer to this sample as the restricted sample). The effect declines by nearly 40 percent, ranging from -0.30 to -0.38 standard deviations in the height-for age z-score, which corresponds to a 12 to 15 percentage point higher likelihood of being stunted.

In addition to studying child health, this paper is the first to identify the causal effect of the Ethiopia–Eritrea war on a range of other childhood human capital indicators, such as school enrollment, grade completion, and reading proficiency. The analysis is based on a unique data set, the Young Lives Survey, which includes a rich set of household and child-level covariates that can be used to provide robust evidence. Findings from the education analysis show that war-exposed children are less likely to be enrolled in school. They are also more likely to complete fewer grades and to exhibit reading problems, even conditional on being enrolled. An investigation of the underlying mechanisms suggests that child health partly operates as a channel through which child education is being reduced.

Despite the importance of the effects of war on economic welfare and human capital, there is only a small yet recently growing body of literature. Akresh et al.

[1] Results from cross-sectional OLS of each survey round are reported in Appendix–Table 3.A. Interestingly, the estimated effect declines as the age of the children increases (the estimates in Column 4 are -0.74 in round one , -0.59 in round two, and -0.47 in round three).

(2012a) study adults with and without exposure to the Nigerian civil war, which took place during a time when these adults were children and adolescents. They find that adults who were exposed to the war during childhood and adolescence (and who survived) exhibit reduced stature than unexposed adults 30 to 40 years later. Surprisingly, they find that exposure during adolescence has a larger effect than exposure during childhood.

Akresh et al. (2011) look at the effect of civil war and crop failure on child stunting using cross-sectional household data from Rwanda. They find that the height-for-age z-score of children (both boys and girls) who were exposed to conflict is about one standard deviation lower. Similarly, Bundervoet et al. (2009) find a 0.35 standard deviation (and a 0.047 standard deviation for each additional month of conflict exposure) lower height-for-age z-score for those exposed to civil war in rural Burundi. A study by Akresh et al. (2012b) that is closely related to this paper uses the 2002 cross-sectional Eritrean Development and Health Survey (DHS) to investigate the impact of the Ethiopian–Eritrean war on child height-for-age z-score, exploiting exogenous variation in the geographic extent and the timing of conflict. They find that exposure to war decreases the child height-for-age z-score by about 0.45 standard deviations.

Studies that focus on child schooling outcomes show negative impacts. These include studies conducted in countries such as Burundi for boys (Verwimp and Van Bavel, 2013), Peru for boys and girls (Leon, 2012), Guatemala for both disadvantaged rural Mayan boys and girls (Chamarbagwala and Morán, 2011), and Tajikistan for girls but not for boys (Shemyakina, 2011a).

However, evidence on the effect of conflict on educational attainment of children is mixed. A recent study by Valente (2013) based on data from Nepal finds that conflict intensity is associated with an increase in female and male schooling attainment (although the abductions by Maoists have a negative effect). A more recent study by Pivovarova and Swee (2015) concludes that there is no effect of war intensity on schooling attainment in Nepal. In addition, a study for Timor-Leste Justino et al. (2013) based on two waves of cross-sectional survey data finds mixed short-term and negative long-term effects of exposure to conflict.

In sum, empirical evidence is typically based on cross-sectional data and involves a comparison of cohorts that were born before and after war/conflict, using a DID approach and exploiting temporal and spatial variation. The key implicit assumption of these studies is that in the absence of war, changes in outcomes between those children born after (younger) and before (older) the war would have been the same for war-affected and unaffected areas. However, if poverty caused war or if war-affected areas tend to be economically poor or drought prone ex-ante, the key assumption may not hold because older cohorts may accumulate larger deficits than younger cohorts, and as a result the effect of the conflict may be overestimated (Duflo, 2003; Martorell and Habicht, 1986). The opposite holds if war-affected areas tend to be economically rich or less drought prone ex-ante. However, I argue that the former holds in the Ethiopian context and in most other circumstances because areas of civil war tend to be economically poor ex-ante, making it harder to identify causal effects (Blattman and Miguel, 2010).

The remainder of the chapter is organized as follows. Section 3.2 presents the data and methods including a detailed descriptions of the identification strategy and sources of potential bias. Section 3.3 presents the results and provides a discussion of potential mechanisms. Section 3.4 provides further robustness checks followed by a final section, which concludes by inferring the predicted impact of the war on potential adult earnings.

3.2 Data and Methods

3.2.1 The Data Set and Summary Statistics

The data set comes from the Young Lives panel data collected by the Young Lives team of Oxford University in collaboration with the Ethiopian Development Research Institute and researchers from Addis Ababa University and Save the Children UK in Ethiopia during three rounds in 2002, 2006 and 2009 (Alemu et al., 2003). Young Lives is a longitudinal childhood poverty study, which tracks a sample of poor children in four developing countries, including Ethiopia, Peru, Vietnam, and India (Outes-Leon and Sanchez, 2008). As of 2002, the study has followed two cohorts in each of these four countries. The younger cohort consists of 2,000 children per country with an average age of 1 year in 2002. The older cohort consists of 1,000 children per country aged between 7.5 and 8.5 years in 2002 (Alemu et al., 2003; Outes-Leon and Sanchez, 2008; Woldehanna et al., 2008, 2011).

Table 3.1: Birth Year and Survey Rounds

	Pre-war period				war period			post-war period			
Years born in:	1994	1995	1996	1997	1998	1999	2000	2001	2002	2006	2009
Younger cohort age								0	1	5	**8**
Older cohort age	0	1	2	3	4	5	6	7	**8**	12	15

Note: Age refers to the average age in years in a given year. There is small monthly variation around the average age. Officially, the war period is from May 1998 to June 2000. Source: Author's analysis based on Young Lives data.

The sample in Ethiopia was selected based on a multi-stage sampling design. The first stage included a selection of 5 out of 9 regions (Tigray, Amhara, SNNP, Addis Ababa, and Oromia). These five regions cover about 96 percent of the total population of the country. Of these regions, 20 sites (about 3-5 districts per region) were selected based on pro-poor bias, balancing Ethiopian regional and ethnic differences and also the cost of sampling. Areas of food deficiency were over-sampled while sites in remote areas were underrepresented due to cost and logistic issues. In addition, at least one Peasant Association (PA) (in rural areas) or kebele (in urban areas), the lowest level of administrative structure in the country in each district, was picked. Eventually, 100 children born between April 2001 and June 2002 (for the younger cohort) and 50 children born between April 1994 and June 1995 (for the older cohort) were selected in each sentinel site using simple random sampling (Alemu et al., 2003; Outes-Leon and Sanchez, 2008; Woldehanna et al., 2008, 2011).

The first outcome variable is the standardized height-for-age z-score, which is calculated as the actual height of each child minus the median height of a WHO reference population of healthy children of the same age and sex, divided by the standard deviation of this reference population (WHO, 2006). This implies that a height-for-age z-score of zero for a given child would mean that the child is as well-nourished as the WHO reference group children, on average. The reason for selecting height-for-age rather than other anthropometric health indicators such as weight-for-age and body mass index, is that it is an indicator of long-term nutrition and health status (Deaton, 2007; Hoddinott and Kinsey, 2001; McCarron et al., 2002; Silventoinen, 2003; Yamano et al., 2005). The other outcomes include whether a child is enrolled in school, the number of grades completed (given enrollment), and whether a child has reading problems (given enrollment).

Not all of these outcome variables are observed in all three survey rounds of both cohorts. However, data on all of the outcome variables are available for the first round of the older and the third round of the younger cohorts. These rounds reflect the age-overlapping cohorts of 8 year olds in 2002 and 2009 of the restricted sample that includes 1,870 younger children and 942 older children, totaling 2,812 children without missing values on any of the outcome variables used in the analysis (see Tables 3.1 and 3.2).

Panel A of Table 3.2 shows that the mean differences in child height, height-for-age z-score, and the proportion of stunted children between war-affected and unaffected regions are statistically significant for older children who were born before the war. These differences are either attributable to the war or to initial differences between regions resulting from other factors such as poverty. However, the differences are not statistically different from zero between war-affected and unaffected regions for younger children who were born after the war. Taken together, these numbers suggest that in the absence of war, children from both regions exhibit similar growth on average.

Table 3.2: Summary Statistics of Outcome Variables by Cohort and Region

	Older			Younger		
	Non-war	War		Non-war	War	
	Mean	Mean	diff.	Mean	Mean	Diff.
Panel A: Health						
Height (cm)	118.57	116.13	2.44***	120.70	120.54	0.16
Height-for-age z-score	-1.41	-1.73	0.32**	-1.20	-1.21	0.01
Stunted (%)	0.30	0.40	-0.11**	0.22	0.18	0.04
Panel B: Schooling						
Currently enrolled (%)	0.67	0.60	0.07	0.73	0.94	-0.21***
Number of grades completed	0.50	0.36	0.14**	0.54	1.08	-0.55***
Problems with reading (%)	0.75	0.95	-0.19***	0.73	0.74	-0.01
No. of children (N)	759	183		1494	376	

Note: Stunted (%) refers to a proportion of children with a height-for-age z-score of less than -2. This is the restricted sample consisting of 942 and 1870 children from older and younger cohorts respectively. $*p < 0.10, **p < 0.05, ***p < 0.01$ applies to all tables in this chapter unless otherwise mentioned. Source: Author's own calculations based on Young Lives data.

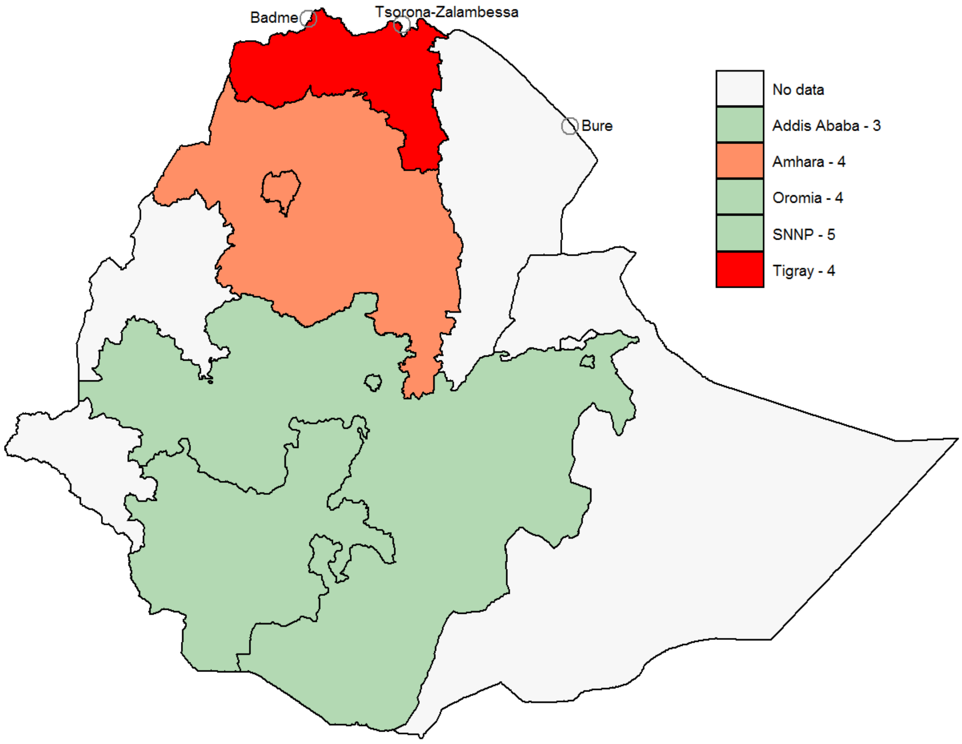
Panel B of Table 3.2 reveals that older children from war-affected region have significantly fewer grades completed and are significantly more likely to have reading problems than older children from the unaffected regions. Younger children from war-affected region are significantly more likely to be enrolled in school and complete significantly more grades. In contrast to the child health outcomes, the educational outcomes differ between younger children from war-affected region and younger children from unaffected regions even in the absence of war. These differences may be the result of regional differences in the provision of education.

3.2.2 Identification Strategy and Potential Bias

The identification strategy is based on variation in children's exposure to war across time and geographic location. The older children in the sample were born 3 to 4 years (from April 1994 to June 1995) before the war started and they experienced the full two-year war period (from May 1998 to June 2000), while the younger were born just after the war (from April 2001 to June 2002). Older children were not all equally affected by the war because it was concentrated in the Northern and Northeastern part of the country: in Badme, Tsorona-Zalambessa and Bure of the Tigray and Afar regions. Since the Young Lives data set does not include a sample from the Afar region, the area of war incidence in this study is the Tigray region, while the other regions in the sample are considered less war-intense, especially the SNNP, Addis Ababa, and Oromia regions. The Amhara region is the next closest region in proximity to the war.

In Figure 3.1, I show the number of the sentimental sites of the Young Lives, with red indicating Tigray and orange indicating Amhara and so on. At the border between Ethiopia and Eritrea, the approximate locations of the main battlefields are indicated by circular signs on the map. The combination of temporal and spatial dimensions of war exposure implies that a child is considered to be exposed to war if it was born before the war (i.e. it belongs to the older cohort) and is from the Tigray region. Thus, changes in outcomes between older and younger children for the unaffected regions effectively serve as a control group to changes in outcomes between older and younger cohorts in the war-affected region.

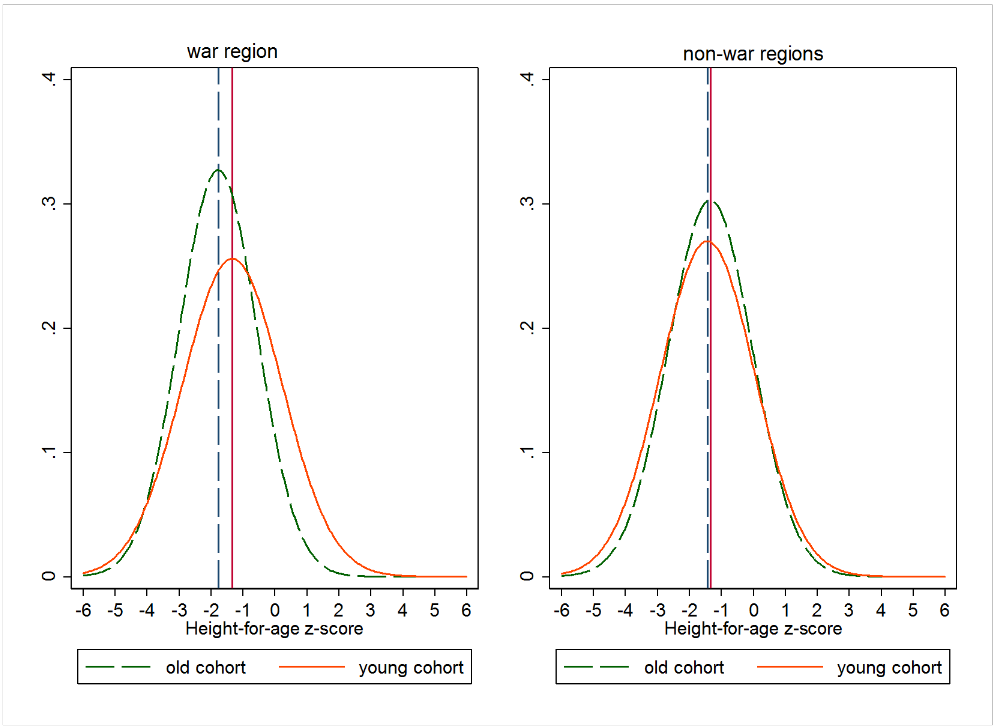
Figure 3.1: Map of Ethiopia Showing Young Lives Number of Sentinel Sites



Source: Author's analysis based on Young Lives panel data, Ethiopia.

Figure 3.2 shows the distribution of the height-for-age z-score by cohort and region of war exposure using the panel data pooled for all children. The figure reveals that all cohorts in both regions have a negatively skewed distribution, suggesting that the sampled children are, on average, of lower height relative to the WHO reference population of healthy children. However, there is a significant difference in the gap of the distribution in height-for-age z-score between older and younger cohorts for war-affected and unaffected regions. In the war-affected region, older children show a relatively higher incidence of stunting compared to younger children, while both age cohorts have nearly the same distribution in the unaffected regions.

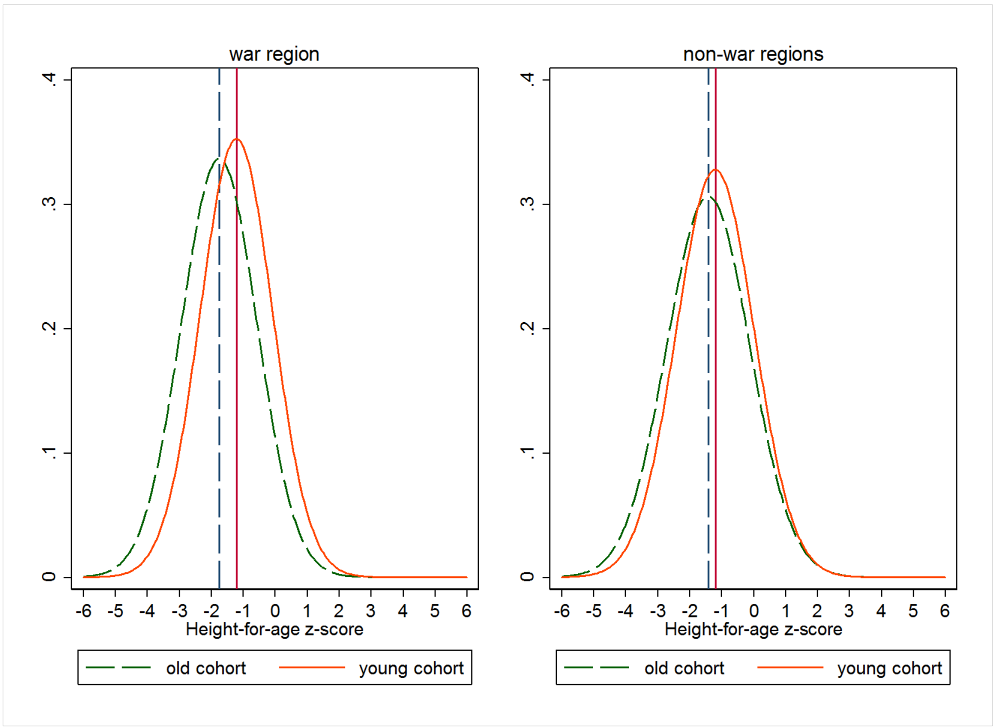
Figure 3.2: Height-for-age z-score Distribution by Cohort and Region (Unrestricted sample)



Source: Author's analysis based on Young Lives panel data, Ethiopia.

Even though the height-for-age z-score is calculated by adjusting for age and sex of the child, comparing children of the same age is more convincing due to non-linearity of child growth (Martorell and Habicht, 1986). Figure 3.3 shows the distribution of younger and older children in the restricted sample. Again, the gap of the distribution in height-for-age z-score between younger and older children is larger in the war-affected region as compared to the unaffected region. If changes between younger and older children are only due to the cohort effect and not due to the war, then I should have seen similar differences for both the war-affected and unaffected regions.

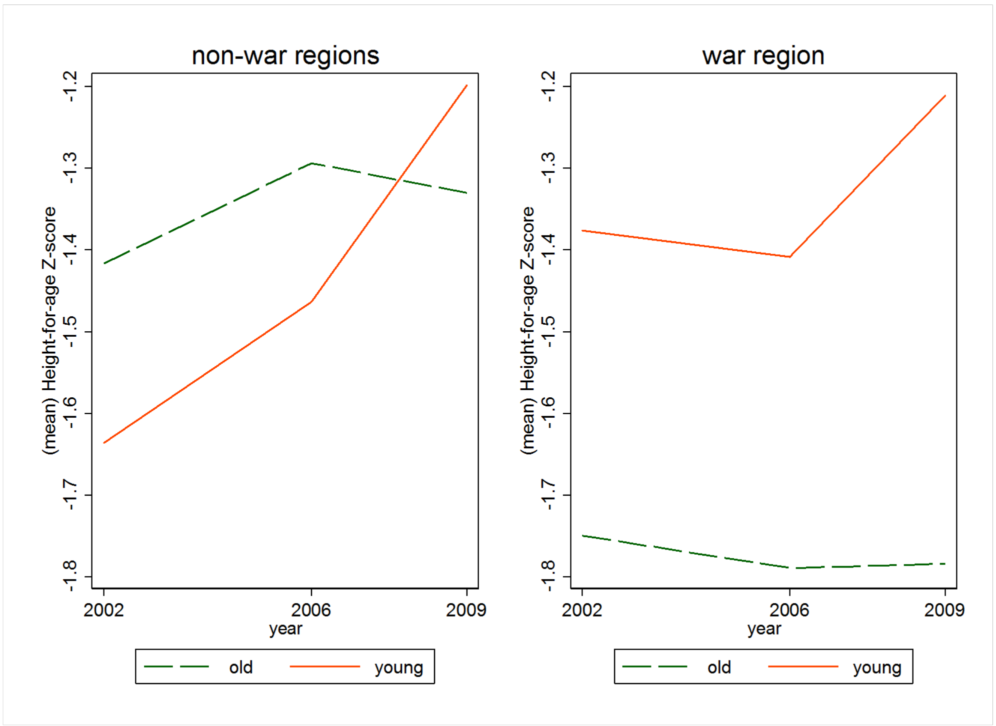
Figure 3.3: Height-for-age z-score Distribution by Cohort and Region (Restricted sample)



Source: Author's analysis based on Young Lives panel data, Ethiopia.

Furthermore, the data seem to suggest that differences in human capital accumulation among older children persist into young adulthood. Figure 3.4 shows how the mean height-for-age z-score of the younger children in war-affected and unaffected regions have converged by the age of eight despite initial differences. However, this difference in the height-for-age z-score is substantial and consistent for older children from the average age of eight to 15 (i.e., in all three survey rounds).

Figure 3.4: Trends in Mean Height-for-age z-core by Cohort and Region



Source: Author's analysis based on Young Lives panel data, Ethiopia.

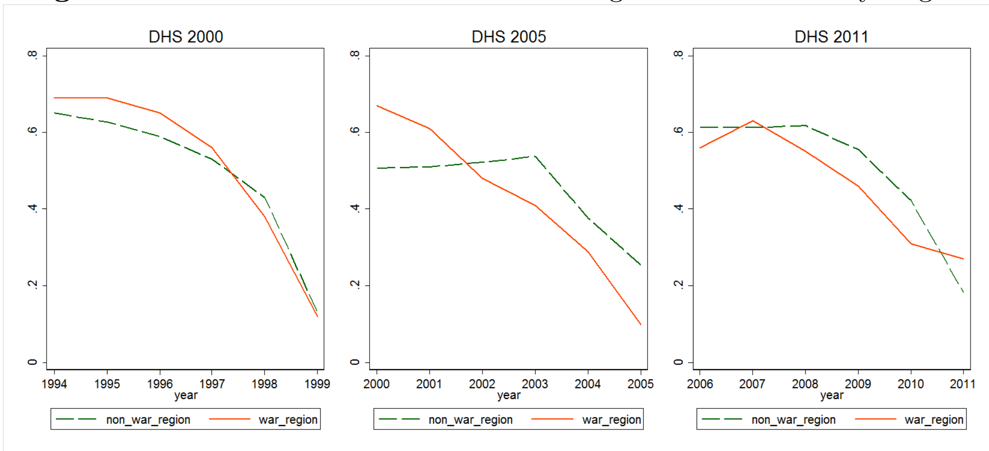
Using time and spatial variation, a given child will fall in either of four categories born before the war from war-affected regions ($\bar{y}_{w,o}$) and non-war-affected regions ($\bar{y}_{nw,o}$), or born after the war from war-affected regions ($\bar{y}_{w,y}$) and non-war-affected regions ($\bar{y}_{nw,y}$). Under the assumption that in the absence of war the regions would have followed similar trends in child health outcomes, the difference-in-difference estimate of the impact of war, $\hat{\theta}$, is unbiased and consistent.

$$\hat{\theta} = [\bar{y}_{w,o} - \bar{y}_{w,y}] - [\bar{y}_{nw,o} - \bar{y}_{nw,y}] \tag{3.1}$$

The use of this identification strategy requires a detailed discussion of several issues. First, the primary threat to this strategy, which assumes parallel trends, is the presence of time-variant unobserved heterogeneity across regions (Khandker et al., 2010; Bertrand et al., 2004). For instance, unobservable investments in health and education in these regions could vary systematically for reasons other than the war. As an attempt for such potential threats of unobservable confounding trends, I assess vaccination trends for cohorts of children born between 1994 and 2011 in war-affected and unaffected regions using three survey rounds of the Demographic and Health Survey (DHS)–2000, 2005 and 2011.

Child vaccination is an important determinant of childhood growth and health (Agadjanian and Prata, 2003; Amin, 1996) reflecting policy decisions. The results presented in Figure 3.5 show that there are no systematic differences in the percentage of children that received any vaccination across regions for cohorts born before and after the war period. The vaccination rates drop off sharply over time for all cohorts in all DHS survey rounds because the young are not yet vaccinated.

Figure 3.5: Trends in Vaccination Rate Using DHS 1994–2011 by Region



Source: Author's analysis based on DHS data 1994–2011, Ethiopia.

Second, using height as measure of health by itself is a threat to the identification strategy if war-affected regions are initially poorer because older children in relatively poor regions could be shorter than older children in non-poor regions as the former could accumulate large poverty-induced height deficits while younger cohorts may be more similar (Duflo, 2003; Martorell and Habicht, 1986). As discussed in the introduction, this phenomenon leads to an upward bias of the estimated effect, even if the common trend assumption is satisfied. To mitigate this problem, the main analysis will focus on the restricted sample (of children of the same average age). [2]

Third, another potential bias could come from either idiosyncratic or covariate shocks such as weather events that are not related to the war but may vary systematically between regions and cohorts. For this reason, the analysis controls for household reported shocks (at least during the post-war period) and village (community) level fixed effects. Usually, shocks such as drought and other natural disasters in Ethiopia are covariate shocks for households within a given village (or community) (Dercon et al., 2005).

Fourth, due to the war, it is also more likely that people were displaced from their initial settlement. However, this is not a threat to the identification strategy because most of such displacements took place within a region (Global IDP, 2004a,b), and because the Young Lives team were able to track most sampled children with a low attrition rate of 3 percent throughout the three waves, which indicates a stable migration pattern.

Fifth, mothers exposed to the war may be affected by post-war trauma and stress. Mulder et al. (2002) conclude that maternal psychological factors may significantly contribute to pregnancy complications and unfavorable development of the (unborn) child. Grimard and Laszlo (2014) also found that shocks in utero or at birth have a negative effect on womens height. However, the oldest child in the young cohort sample is born in April 2001, which implies that pregnancy to this child starts after the war (at July 2000) leaving the effect, if any, negligible. [3]

[2] Figure 3.A in the Appendix presents the regional poverty headcount overtime, showing that the war-exposed region (Tigray) is relatively poor during the pre-war period.

[3] If the younger cohort in the war region is affected by the war, this will potentially lead to an underestimate of the effect. For that reason, I conduct the same analysis after dropping children of the younger cohort who are relatively old (above 14 months at the time of the first survey in 2002). The results remain largely unchanged.

Sixth, an additional concern could be sample selection bias due to differences in mortality rates across regions over time because of the war. A child with better inherent health is more likely to survive than a child with similar characteristics and lower inherent health. This is because an inherently more healthy child needs a relatively smaller threshold stock of health to be able to survive a given shock or catastrophic event relative to an inherently less healthy child (Maccini and Yang, 2008). Such a selection bias will potentially lead to an underestimate of the true effect. For this reason, the estimated effect should be interpreted as an effect of the war on survivors.

Finally, measurement errors in child age, height, and education outcomes could be an issue. Parents may probably under report the age of a relatively short child (Akresh et al., 2011). However, the Young Lives data collection procedure asked for the exact date of birth, which minimizes this error. Measuring height of one year-old children may be difficult (Ulijaszek and Kerr, 1999) but the main analysis of this study considers outcomes of children with an average age of 8 years. Moreover, it appears unlikely that measurement errors differ systematically across regions and birth cohorts.

In general, while it is not possible to test the common trend assumption directly, the data set contains a rich set of information on child and household specific covariates, which help to control for several observables. These include child age and sex, parental literacy, age, sex, and education level of the household head, wealth index, share of food expenditure, ownership of land, milk and livestock animals, household composition and size, and whether a household faced a drought shock.

The DID model for estimating the impacts of the war for the restricted sample is specified as:

$$Y = \alpha + \beta_1 * OldCohort_c + \sum_i \beta_i * Region_i + \theta * OldCohort_c * Warregion_W + \gamma * X + \mu + \upsilon \tag{3.2}$$

where Y is the outcome variable of interest, c is a cohort fixed effect, i is the set of region dummy variables, $Warregion_W$ takes on a value 1 if a child is from a war-affected region and 0 otherwise, X is a vector of child, parental, and household covariates, μ is a village (community) level fixed effect, and υ is a random error term.[4]

3.3 Regression Results and Discussions

3.3.1 Child Health

I use the height-for-age z-score as a proxy for child health. However, a small reduction in the height-for-age z-score does not necessarily imply stunting unless that reduction is large enough so that a child's height-for-age z-score falls by more than 2 standard deviations. Therefore, I also use child stunting defined as a binary outcome variable that indicates whether a child's height-for-age z-score falls below a threshold of normally healthy children defined by a WHO reference group (WHO, 2006). The probability of stunting is estimated as a linear probability or probit model.

I begin by replicating the DID results from the literature using the whole sample. Panels A and B of Table 3.3 present results for the effect of war on child

[4]Moulton (1986) argues that in a regression model in which data are drawn from a population with grouped structure, the regression errors are often correlated within groups. These errors for children living in the same environment and that undergo similar events that potentially affect outcome variables, are more likely to be correlated. Consequently, assumptions of independent errors may not hold and unadjusted OLS standard errors may be biased downward. For that reason, heteroscedasticity-robust standard errors are clustered at the district level to allow for correlations across children within the district.

height-for-age z-score and child stunting. The first column (in all regressions) only controls for region and cohort fixed effects, providing the basic DID result.

Table 3.3: The Effect of War Exposure on Child Health: Pooled OLS Results

	1	2	3	4
Panel A				
Old*warR	-0.53***	-0.57***	-0.60***	-0.58***
	[0.14]	[0.14]	[0.14]	[0.14]
N	8,646	8,646	8,048	7,863
Panel B				
Old*warR	0.17***	0.18***	0.19***	0.18***
	[0.03]	[0.04]	[0.04]	[0.04]
N	8,646	8,589	7,996	7,814
Controls				
Region FE	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y
Woreda FE		Y	Y	Y
Community FE		Y	Y	Y
Survey round dummies		Y	Y	Y
Survey round dummies X region dummies		Y	Y	Y
Survey round dummies X cohort dummy		Y	Y	Y
Child age in months		Y	Y	Y
Child sex dummy		Y	Y	Y
Urban dummy		Y	Y	Y
Parent's age and literacy			Y	Y
Household Head age, sex, education			Y	Y
Additional controls				Y

Note: The dependent variable in Panel A is the height-for-age z-score. The dependent variable in Panel B takes on the value one if a child is stunted (less than -2 height-for-age z-score). Panel B reports results from a marginal effects of probit model. Additional controls include wealth index, household size, number of milk animals, and number of livestock in all survey rounds (2002, 2006, and 2009). The reported share of food expenditure and whether the household reported a drought shock in the 2006 and 2009 and land size (in hectare) in the 2006 survey rounds are included. The former two variables are not observed in 2002 while the latter variable is observed only in 2006. Robust standard errors clustered at a district level are reported in brackets. "Ol" refers to a child belonging to the older cohort i.e., born before the war and "warR" refers to the war region (Tigray). "Y" refers to yes. The unrestricted sample size (N) includes all of the three survey rounds for both older and younger cohorts. Source: author's analysis based on Young Lives data.

The estimate in Column 1, Panel A reveals that a child exposed to the war experienced an average reduction of about 0.53 of a standard deviation in the height-for-age z-score. This is equivalent to a 17 percentage point higher likelihood of being stunted. Column 2 extends this result by including district and Kebele fixed effects, survey round dummies, survey round dummies interacted with region dummies, survey round dummies interacted with cohort dummy, child age in months, child gender, and rural dummy. Columns 3 and 4 further control for additional covariates. The estimated parameters are robust across model specifications, both in magnitude and statistical significance, with effects of up to -0.6 standard deviations of the height-for-age z-score or a 19 percentage points higher likelihood of being stunted.

These results are similar to the literature in other African countries. For instance Akresh et al. (2011), using cross-sectional data from Rwanda, find a one standard deviation lower height-for-age z-score as a result of civil war and crop failure. Similarly, Bundervoet et al. (2009) calculates the effect of civil war in rural Burundi for the average exposure duration on the height-for-age z-score to be again one standard deviation lower. More recently, Akresh et al. (2012b) find an effect of about 0.45 standard deviations resulting from exposure to the Ethiopia–Eritrea war, using a sample of children from Eritrea.

Going beyond the previous literature, I present the results from a restricted sample that compares children of the same average age. Because both cohorts attain the same age at different times (the younger cohort being eight years old in 2009 and the older being eight years old in 2002), I control for a large set of time-variant characteristics. The effects of the war presented in Table 3.4 range from 0.30 to 0.38 standard deviations in the height-for-age z-score corresponding to a 12-15 percentage point higher likelihood of being stunted.

Looking at the interaction effect with the rural dummy, the results reveal that the effects are mainly driven by the rural sample. This could be due to displacement and disruption of agricultural production. Although it is difficult to obtain data for the war period to assess the extent of agricultural disruption, an FAO press release at the time of the conflict mentions that "negative impacts of the Eritrea-Ethiopia conflict have begun to show up in agricultural and trade activities." Controlling for reported drought occurrence, district and community fixed effects, and an additional rich set of covariates, it is unlikely that the results are driven by other unobservables. However, the results are insensitive to the gender of a child as can be seen in Column 6 of Table 3.4.

Table 3.4: The Effect of War Exposure on Child Health: OLS Restricted Sample

	1	2	3	4	5	6	7
Panel A							
Old*warR	-0.31*	-0.30*	-0.39**	-0.36**	-0.14	-0.34*	-0.38**
	[0.15]	[0.16]	[0.17]	[0.16]	[0.12]	[0.20]	[0.12]
Old*warR*rural					-0.27*		
					[0.15]		
Old*warR*male						-0.04	
						[0.27]	
N	2,812	2,812	2,170	2,141	2,141	2,141	2,141
Panel B							
Old*warR	0.13**	0.13**	0.12*	0.11*	0.09**	0.13*	0.11***
	[0.06]	[0.06]	[0.06]	[0.06]	[0.04]	[0.07]	[0.04]
Old*warR*rural					0.03		
					[0.06]		
Old*warR*male						-0.04	
						[0.07]	
N	2,812	2,764	2,132	2,104	2,104	2,104	2,104
Region FE	Y	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y	Y
Woreda FE		Y	Y	Y	Y	Y	Y
Community FE		Y	Y	Y	Y	Y	Y
Child age in months		Y	Y	Y	Y	Y	Y
Child sex dummy		Y	Y	Y	Y	Y	Y
Rural dummy		Y	Y	Y	Y	Y	Y
Parent's age and literacy			Y	Y	Y	Y	Y
Household Head age, sex, education			Y	Y	Y	Y	Y
Additional controls				Y	Y	Y	Y
Region dummies * age (months)							Y

Note: See notes to Table 3.3. Additional controls include a wealth index, the household size, the number of milk animals, and the number of livestock in all survey rounds (2002, 2006, and 2009). The reported share of food expenditure and whether the household reported a drought shock in 2006 and 2009 and land size (in hectare) in the 2006 survey round are included. The former two variables are not observed in 2002 while the latter variable is observed only in 2006. Robust standard errors clustered at a district level are reported in brackets. The restricted sample size (N) includes the first and third survey rounds for the older and younger cohorts respectively. Source: Author's analysis based on Young Lives data.

Overall, the sign of the estimated effects of war on child health is consistent with studies from other countries, suggesting that evidence on the detrimental impact of war on child health is externally valid. Indeed, the effect is large in magnitude and statistically significant. However, this study finds relatively smaller coefficients and has shown that the estimated coefficients could be larger in pooled or cross-sectional settings that do not involve a comparison of children of the same average age. Note that regression results from the pooled and cross-sectional analysis of each survey round show larger coefficients.

3.3.2 Child Education

The impact of war on child enrollment at school is presented in Table 3.5. The OLS estimates in Panel A indicate that, on average, a child exposed to the war is 28 to 38 percentage points less likely to be enrolled. This effect is consistently robust to including any of the covariates. The estimates reveal that the intensity of the war was serious enough to be able to interrupt child enrollment. Similar to the child health outcomes, the impact of the war is mainly evident for children from rural areas but is not related to the gender of a child (Columns 5 and 6).

In Panel B of Table 3.5, I further investigate if child health played a role as a mechanism of lower enrollment due to the war by including the height-for-age z-score as independent variable and by interacting it with the interaction of the older cohort and war region dummies. The results support this hypothesis. Controlling for all covariates, a decrease in the height-for-age z-score by one standard deviation reduces the enrollment probability of a child by 6 to 9 percentage points (Panel B second row). This same reduction corresponds to an additional decline in the likelihood of child enrollment by about 6 percentage points for war-exposed relative to non-exposed children (looking at the interaction of the older cohort with region of war and health).

Panel A of Table 3.6 shows that children exposed to war have nearly three-forth fewer grades completed than non-exposed children (statistically significant at the

Table 3.5: The Effect of War Exposure on Child Education (1): OLS Restricted Sample

	1	2	3	4	5	6	7
Panel A							
Old*warR	-0.28*	-0.31*	-0.35*	-0.36**	-0.05	-0.36**	-0.38***
	[0.16]	[0.17]	[0.17]	[0.17]	[0.09]	[0.13]	[0.06]
Old*warR*rural					-0.38**		
					[0.14]		
Old*warR*male						0.01	
						[0.11]	
N	2,812	2,812	2,170	2,141	2,141	2,141	2,141
Panel B							
Old*warR	-0.13	-0.16	-0.21	-0.21	0.05	-0.22*	-0.23**
	[0.12]	[0.13]	[0.14]	[0.14]	[0.10]	[0.12]	[0.07]
HAZ	0.09***	0.07***	0.07***	0.06***	0.06***	0.06***	0.06**
	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]
Old*warR*HAZ	0.07**	0.07**	0.07***	0.07***	0.06***	0.07***	0.07***
	[0.03]	[0.03]	[0.02]	[0.02]	[0.02]	[0.02]	[0.01]
Old*warR*rural					-0.34**		
					[0.13]		
Old*warR*male						0.01	
						[0.09]	
N	2,812	2,812	2,170	2,141	2,141	2,141	2,141
Region FE	Y	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y	Y
Woreda FE		Y	Y	Y	Y	Y	Y
Community FE		Y	Y	Y	Y	Y	Y
Child age in months		Y	Y	Y	Y	Y	Y
Child sex dummy		Y	Y	Y	Y	Y	Y
Rural dummy		Y	Y	Y	Y	Y	Y
Parent's age and literacy			Y	Y	Y	Y	Y
Household Head age, sex, education			Y	Y	Y	Y	Y
Additional controls				Y	Y	Y	Y
Region dummies * age (months)						Y	Y

Note: The dependent variable takes on the value 1 if a child is enrolled at school, and 0 otherwise. Panel B adds the height-for-age z-score (abbreviated as HAZ) as an explanatory variable by itself and its interaction with war region and older cohort dummies to examine the mechanism. Results are from a linear probability model but results of marginal effects from a probit model are almost the same. See note to Table 3.4. Source: Author's analysis based on Young Lives data.

one percent level). This effect is very large given the mean number of grades completed is 0.62. This could be the result of enrollment or attainment. To examine this issues further, Panel B shows the same regression results but conditional on being enrolled. The estimated coefficients are somewhat smaller, suggesting that enrollment matter for child grade completion, but there is still a very large attainment effect that may be attributed to the war. Conditional on being enrolled, war-exposed children attain about half a grade less compared to unexposed children.

Looking at Panel C of Table 3.6, it turns out that child health explains part of the variation in the number of grades a child completed conditional on being enrolled but there are no differential effects between war-exposed children and non-exposed children. This implies that child health has a substantial effect on grade completion of children but is only a channel through which the war affects school enrollment.

Another interesting finding from Table 3.6 is that the effect of the war on grade completion given enrollment is statistically different for boys and girls. Column 6 of Panel B shows that, conditional on being enrolled, exposed girls are likely to complete 0.68 fewer grades, while exposed boys are likely to complete 0.23 fewer grades, all else being equal.

Table 3.6: The Effect of War Exposure on Child Education (2): OLS Restricted Sample

	1	2	3	4	5	6	7
Panel A							
Old*warR	-0.69***	-0.68***	-0.70***	-0.69***	-0.47***	-0.80***	-0.70***
	[0.14]	[0.16]	[0.19]	[0.20]	[0.08]	[0.14]	[0.10]
Old*warR*rural					-0.26		
					[0.21]		
Old*warR*male						0.21	
						[0.14]	
N	2,812	2,812	2,170	2,141	2,141	2,141	2,141
Panel B							
Old*warR	-0.58***	-0.46***	-0.45**	-0.44**	-0.47***	-0.68***	-0.45**
	[0.09]	[0.11]	[0.16]	[0.17]	[0.10]	[0.14]	[0.14]
Old*warR*rural					0.05		
					[0.19]		
Old*warR*male						0.45***	
						[0.11]	
N	2,060	2,060	1,557	1,535	1,535	1,535	1,535
Panel C							
Old*warR	-0.60***	-0.50***	-0.39**	-0.37**	-0.41***	-0.62***	-0.39**
	[0.12]	[0.13]	[0.16]	[0.18]	[0.11]	[0.16]	[0.14]
HAZ	0.11***	0.12***	0.08**	0.08**	0.08**	0.08**	0.08*
	[0.02]	[0.03]	[0.03]	[0.03]	[0.03]	[0.03]	[0.03]
Old*warR*HAZ	-0.02	-0.04	0.03	0.04	0.04	0.02	0.04
	[0.06]	[0.07]	[0.04]	[0.04]	[0.04]	[0.03]	[0.03]
Old*warR*rural					0.05		
					[0.22]		
Old*warR*male						0.42***	
						[0.13]	
N	2,060	2,060	1,557	1,535	1,535	1,535	1,535
Region FE	Y	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y	Y
Woreda FE		Y	Y	Y	Y	Y	Y
Community FE		Y	Y	Y	Y	Y	Y
Child age in months		Y	Y	Y	Y	Y	Y
Child sex dummy		Y	Y	Y	Y	Y	Y
Rural dummy		Y	Y	Y	Y	Y	Y
Parent's age and literacy			Y	Y	Y	Y	Y
Household Head age, sex, education			Y	Y	Y	Y	Y
Additional controls				Y	Y	Y	Y
Region dummies * age (months)						Y	Y

Note: The dependent variable in Panel A is the number of grades a child has completed. In panel B, dependent variable is the number of grades a child completed conditional on being enrolled. In panel C, the dependent variable is the same as in panel B. It adds height-for-age z-score (abbreviated as HAZ) as an explanatory variable by itself and its interaction with war region and old cohort dummies to examine the mechanism. See note to table 3.4 regarding sample size. Source: author's analysis based on Young Lives data.

Looking at literacy (Table 3.7), exposure to war increases the probability of a child of having reading problems by about 20 percentage points (results from Panel A). This effect is statistically significant in all specifications. The effect on learning outcomes could be driven by the previous results of reductions in school enrollment and grade completion, but might also be due to deteriorating quality of schooling or related to psychological stress resulting from the war experience. It could also be due to the immediate closure of schools during times of intensive fighting.

Considering Panel B of Table 3.7 shows that the effect of the war does not change when I re-estimate on the probability of a child exhibiting reading problems conditional on being enrolled. This is an indication that the effect may operate through other mechanisms such as the deteriorating quality of the school or psychological stress instead of directly through a decline in child enrollment. Further investigation (Panel C) of the interaction of the effect with child health reveals a finding that is similar to the effect on grades completed (Table 6). That is, although the coefficient on the height-for-age z-score is statistically significant, its interaction with war exposure is not.

Table 3.7: The Effect of War Exposure on Child Education (3): OLS Restricted Sample

	1	2	3	4	5	6	7
Panel A							
Old*warR	0.18***	0.18**	0.20**	0.20**	0.31***	0.19**	0.21*
	[0.06]	[0.07]	[0.07]	[0.07]	[0.06]	[0.07]	[0.08]
Old*warR*rural					-0.14***		
					[0.04]		
Old*warR*male						0.02	
						[0.02]	
N	2,812	2,812	2,170	2,141	2,141	2,141	2,141
Panel B							
Old*warR	0.17**	0.19***	0.21***	0.21***	0.28***	0.18***	0.22**
	[0.06]	[0.05]	[0.05]	[0.05]	[0.04]	[0.06]	[0.07]
Old*warR*rural					-0.10***		
					[0.03]		
Old*warR*male						0.05	
						[0.04]	
N	2,060	2,060	1,557	1,535	1,535	1,535	1,535
Panel C							
Old*warR	0.19***	0.19***	0.20***	0.19***	0.25***	0.15**	0.20**
	[0.06]	[0.04]	[0.04]	[0.04]	[0.05]	[0.06]	[0.06]
HAZ	-0.04***	-0.03***	-0.02**	-0.02*	-0.02*	-0.02*	-0.02*
	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]
Old*warR*HAZ	0.02	-0.00	-0.00	-0.01	-0.01	-0.02	-0.02
	[0.02]	[0.02]	[0.02]	[0.02]	[0.02]	[0.02]	[0.01]
Old*warR*rural					-0.09**		
					[0.03]		
Old*warR*male						0.06	
						[0.05]	
N	2,060	2,060	1,557	1,535	1,535	1,535	1,535
Region FE	Y	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y	Y
Woreda FE		Y	Y	Y	Y	Y	Y
Community FE		Y	Y	Y	Y	Y	Y
Child age in months		Y	Y	Y	Y	Y	Y
Child sex dummy		Y	Y	Y	Y	Y	Y
Rural dummy		Y	Y	Y	Y	Y	Y
Parent's age and literacy			Y	Y	Y	Y	Y
Household Head age, sex, education			Y	Y	Y	Y	Y
Additional controls				Y	Y	Y	Y
Region dummies * age (months)						Y	Y

Note:The dependent variable in Panel A takes on the value 1 if a child has reading problems, and 0 otherwise. In Panels B and C, the dependent variable takes on the value 1 if a child has reading problems conditional on being enrolled, 0 otherwise. Panel C adds the height-for-age z-score (abbreviated as HAZ) as an explanatory variable by itself and its interaction with war region and old cohort dummies to examine the mechanism. See note to table 3.4 regarding sample size. Source: author's analysis based on Young Lives data.

To summarize, a few studies investigate the impact of war on educational outcomes of children, such as school enrollment, grade completion, and literacy, using (child) household data (Justino et al., 2013; Verwimp and Van Bavel, 2013). In addition to providing evidence on child health, this paper generates robust empirical evidence on the negative impact of war on educational outcomes of children. A result that is particularly surprising is that while the effect of the war on child health is not related to the gender of a child, it has a more detrimental effect on the education of girls. Conditional on being enrolled, girls exposed to the war are more likely to complete fewer grades (and exhibit reading problems to some extent) as compared to boys exposed to the war.

3.3.3 Discussion of Mechanisms

War can affect childhood human capital through a number of direct and indirect channels. While there is evidence that poor child health has played a role as a mechanism for reducing child education, in particular enrollment, proving the relevance of additional mechanisms remains challenging due to data limitations. However, the following paragraphs present a few speculations.

The stock of food available to children could have decreased as a result of the war-induced internal displacement of people and loss of life because these factors led to loss of crops, livestock, productive inputs and assets, especially for rural farmers (Akresh et al., 2012b). In addition to these indirect effects, the war has a direct impact on health and education infrastructure, thereby affecting relevant environmental inputs to child growth as well as health and education outcomes (Lai and Thyne, 2007). For instance, it is documented that on 5 June 1998, the Eritrean Air Force bombed Ayder School in Mekele (the capital city of the war-affected region) killing twelve children. Most, if not all, destructions of this kind took place in war-affected region, damaging physical infrastructure and significantly interrupting and decreasing the human capital accumulation process.

Due to the geographic proximity of war-affected areas, children may hear sounds from the battlefield, overhear parents' daily conversations about the war, or be confronted with the loss of life. Exposure to war may lead to Post-war

Trauma Stress Disorder (PTSD). Previous studies by Dyregrov et al. (2000); Papageorgiou et al. (2000); Thabet and Vostanis (1999) find a significant Post-Trauma Stress Disorder (PTSD) association with intrusion and avoidance for children exposed to the Rwandan genocide, and the Bosnian and Palestine wars. There is evidence that PTSD is a potential risk factor for mental health of children that may further affect their health and educational outcomes (Currie and Stabile, 2006).

Finally, war could affect child poverty and human capital through its general equilibrium effects. For instance, the rate of private investment (or demand for investment) could decrease in war-affected areas in response to the war. Moreover, return on investment is likely to decrease due to an immediate interruption of daily business, destruction of infrastructure, or instability caused by the war (Brck et al., 2013; Collier and Duponchel, 2013), while agricultural output may diminish due to a lack of supply or a disruption of the distribution of inputs.

3.4 Further Robustness Checks

The stability of the coefficient estimates when adding district and community fixed effects and a number of child and household covariates indicates that the results are robust to different specifications. Additionally, I provide two more robustness (placebo) checks in this section.

First, as there is small monthly variation in age of cohorts, I subdivide the sample of younger children into younger and older children and perform a DID analysis. The relevant coefficient is now associated with an interaction term between the older of the younger children and the war region (Tigray). I should not find any effects because the members of my sample of younger children were born after the war. The coefficients presented in Table 3.8 are largely insignificant, confirming the validity of the DID approach.

Table 3.8: Placebo Checks Using Younger Cohorts Only

	1	2	3
Panel A			
Old*warR	-0.18	-0.11	-0.11
	[0.13]	[0.13]	[0.08]
N	1,881	1,881	1,846
Panel B			
Old*warR	-0.06*	-0.05**	-0.04
	[0.03]	[0.02]	[0.02]
N	1,880	1,880	1,845
Panel C			
Old*warR	-0.01	-0.01	-0.00
	[0.08]	[0.10]	[0.06]
N	1,883	1,883	1,848
Panel D			
Old*warR	-0.04	-0.06	-0.06
	[0.08]	[0.11]	[0.06]
N	1,444	1,444	1,415
Panel E			
Old*warR	-0.04	-0.02	-0.03
	[0.04]	[0.04]	[0.03]
N	1,875	1,875	1,840
Panel F			
Old*warR	-0.05	-0.02	-0.01
	[0.04]	[0.05]	[0.04]
N	1,443	1,443	1,414

Note: In Panel A, the dependent variable is the height-for-age z-score. In Panel B, the dependent variable takes on the value 1 if a child is enrolled, and 0 otherwise. In Panel C, the dependent variable is the number of grades completed. In Panel D, the dependent variable is the number of grades completed conditional on being enrolled. In Panel E, the dependent variable takes on the value 1 if a child has reading problems, and 0 otherwise. In panel F, the dependent variable takes on the value 1 if a child has reading problems given enrollment, and 0 otherwise. Sample size (N) declines because the analysis is only based on younger cohort (maximum N=1881). In Panels D and F, N reduces more because the regression is estimated conditional on being enrolled. Column 1 controls for region and cohort fixed effects. Column 2 adds district and community fixed effects, age of child in months, gender of child dummy, and rural dummy. Column 3 adds a wealth index, the household size, the number of milk animals in 2002, 2006, 2006, and drought shocks in 2006 and 2009, and the interaction of regional dummies with age of child in months. Results reported in Panels B, E, and F are from a linear probability model but results of marginal effects from a probit model are almost the same. Robust standard errors clustered at district level are reported in brackets. "Old" refers to a child belonging to older cohort i.e., born before the war and "warR" refers to the war region (Tigray). Source: Author's analysis based on Young Lives data.

Second, I exclude the main war-affected region (Tigray) and conduct the analysis of the outcomes within the rest of four regions as follows. I choose each region at a time and assign it to the treatment group while the remaining regions are being assigned to the control group, which allows me to use four alternative treatment/control comparisons to perform the DID analysis. The resulting Placebo effects are expected to be insignificant because the main war-affected region is excluded from the analysis. A summary of the results obtained from the four cases is reported in Table 3.9.

The only significant coefficients are those of the Amhara region, which is closest to the war region. One possible explanation for the significant coefficients in Specifications 5 to 8 is that the war may have gone beyond the Tigray region and affected the some areas of Wollo and Gonder (in the Amhara region). This is a plausible explanation because these areas are closest to the war region. Note that the coefficients in Columns 5 to 8 (which capture effects on the number of grades completed and on reading problems) are only about half the size of the coefficients obtained from the original analysis, suggesting that the impact of the war diminishes gradually as one moves away from the highly war intense region (Tigray) to Amhara. None of the coefficients is found to be statistically significant (at 5 percent level) for the remaining regions, Addis Ababa, Oromia, and SNNP, confirming the robustness of the results presented in the paper.

Table 3.9: Placebo Checks Based on Control Regions

	1	2	3	4	5	6	7	8
Old*Amhara	-0.32	-0.36	-0.08	-0.12	-0.25**	-0.31***	0.13*	0.16**
	[0.19]	[0.21]	[0.12]	[0.13]	[0.10]	[0.09]	[0.06]	[0.07]
N	2,253	2,196	2,253	2,196	2,253	2,196	2,253	2,196
Old*Oromia	0.00	0.23	-0.10	0.01	0.22	0.35*	0.04	0.05
	[0.19]	[0.16]	[0.15]	[0.13]	[0.20]	[0.17]	[0.06]	[0.08]
N	2,253	2,196	2,253	2,196	2,253	2,196	2,253	2,196
Old*SNNP	0.27	0.19	0.15	0.14	0.05	-0.00	-0.18	-0.25*
	[0.22]	[0.25]	[0.20]	[0.23]	[0.13]	[0.15]	[0.11]	[0.12]
N	2,253	2,196	2,253	2,196	2,253	2,196	2,253	2,196
Old*Addis Ababa	-0.00	-0.09	0.01	-0.06	-0.03	-0.03	0.06	0.09
	[0.38]	[0.37]	[0.09]	[0.10]	[0.11]	[0.14]	[0.07]	[0.07]
N	2,253	2,196	2,253	2,196	2,253	2,196	2,253	2,196

Notes: In Columns 1 and 2, the dependent variable is the height-for-age z-score; in Columns 3 and 4, the dependent variable takes on the value 1 if a child is enrolled, and 0 otherwise; in Columns 5 and 6, the dependent variable is the number of grades completed; and in Columns 7 and 8, the dependent variable takes on the value 1 if a child has reading problems, and 0 otherwise. Odd columns control for region and cohort fixed effects only. Even columns add district and community fixed effects, age of child in months, gender of child dummy, rural dummy, and wealth index, household size, number of milk animals at 2002, 2006, 2006, and drought shocks in 2006 and 2009 survey rounds, and the interaction of regional dummies with age of child in months. Results reported in Columns 3, 4, 7, and 8 are from a linear probability model but results of marginal effects from a probit model are almost the same. Robust standard errors clustered at the district level are reported in brackets.“Ol” refers to a child belonging to the older cohort i.e., born before the war and Amhara takes on the value 1 if a child is from “Amhara” region, Oromia takes on the value 1 if a child is from “Oromia” region, and so on. Source: Author’s analysis based on Young Lives data.

3.5 Conclusion

This paper investigates two key potential channels child health and education through which the Ethiopia–Eritrea War may have affected long-term economic conditions. Using the Young Lives data set of children in Ethiopia and combining event data with location, I compare changes in outcomes between older (war-exposed) and younger (unexposed) children in war-affected and unaffected regions, using two points in time at which younger and older children have the same average age. A DID approach provides two primary benefits for identifying impact estimates of the war. First, this method avoids any bias due to time-constant unobserved heterogeneity across regions. Second, after controlling for possible observable trends, the analysis compares cohorts of the same average age, thereby minimizing the upward bias due to non-linearity in the growth process of children.

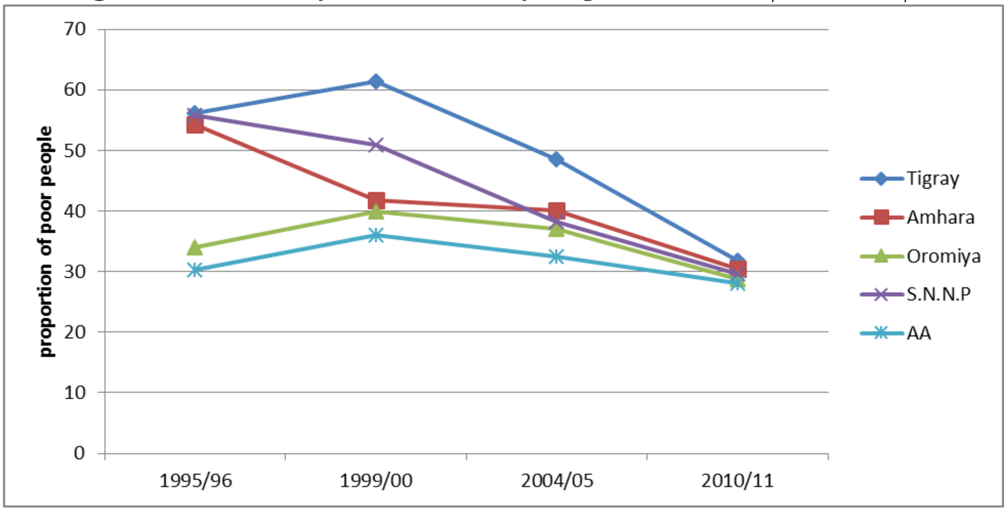
The results indicate that children who were exposed to the war are more likely to exhibit stunted growth than children who were not exposed to the war. In addition, war-exposed children are less likely to be enrolled in school, complete fewer grades (given enrollment), and are more likely to have reading problems (given enrollment). Poor child health turns out to be an important channel through which the war affects child education. The negative effects are particularly detrimental for children from poor backgrounds because poverty is likely to limit their potential of recovery.

Girma and Kedir (2005) estimate that the average returns to an additional year of schooling in Ethiopia range from 9.2 to 19.6 percent in terms of wages. Assuming that one year of schooling is equivalent to one grade completed, these estimates suggest that the wages of workers are about 10 percent lower if they were exposed to the Ethiopia–Eritrea war during childhood. Evidence of the detrimental impact of war on human capital of children emphasizes that interventions, which protect welfare of children growing up in war-affected regions need to be available and further encouraged. These interventions include immediate policy responses as well as post-war rehabilitation programs. First, governments and other responsible international organizations need to develop interventions or programs to ensure that children are physically protected or evacuated from conflict areas. Second, priority should be given to targeting children at risk of malnutrition because of family displacement or loss of life. Third, appropriate psychological interventions to treating post-war trauma should be considered. Fourth, the recovery process should be supported by post-war infrastructural developments, such as repatriation programs to help improve physical and psychological welfare of children, and (re-)constructing schools, health centers, and roads to improve health and education of children. The speed and coordination of organizations responding to crises is important in protecting children during and after war and armed conflict.

This paper has focused on quantifying the total effect of war on childhood human capital and attempted to understand the extent to which child health contributes to the effect of war on child education. However, it remains challenging to pin down the underlying additional mechanisms of the observed outcomes due to the complexity of war and limited data availability (especially pre-war). Future research that focuses on the mechanisms through which war affects children may improve the design of policies that aim to target and support children confronted with war.

3.6 Appendix

Figure 3.A: Poverty Head Count by Region from 1995/6 to 2010/11



Source: Ministry of Finance and Economic Development, FDRE, June 2013, Addis Ababa.

Table 3.A: The Effect of War Exposure on Child Health: OLS Cross-sectional

	(1)	(2)	(3)	(4)
Panel A: First round (2002)				
Old*warR	-0.59**	-0.73***	-0.76***	-0.74***
	[0.23]	[0.24]	[0.24]	[0.24]
N	2,910	2,910	2,645	2,622
Panel B: Second round (2006)				
Old*warR	-0.55***	-0.57***	-0.59***	-0.59***
	[0.14]	[0.15]	[0.16]	[0.16]
N	2,887	2,887	2,720	2,709
Panel C: Third round (2009)				
Old*warR	-0.43***	-0.44***	-0.48***	-0.47***
	[0.11]	[0.11]	[0.11]	[0.12]
N	2,849	2,849	2,683	2,678

Note: The dependent variable is the height-for-age z-score. Column 1 controls for region and cohort fixed effects. Column 2 adds district and community fixed effects, age of child in months, gender of child dummy, rural dummy. Column 3 adds parental and head sex, age, and education. Column 4 adds a wealth index, the household size, the number of milk animals in 2002 survey round; wealth index, land ownership, household size, the number of milk animals, the share of food expenditure, and drought shock in 2006; and wealth index, household size, the number of milk animals, share of food expenditure, and drought shock in 2009. Robust standard errors clustered at district level in brackets. "Old" refers to a child belonging to older cohort i.e., born before the war and "warR" refers to the war region (Tigray). Source: author's analysis based on Young Lives data.

Chapter 4

Grade Repetition, School Dropout, and Student Achievement in Ethiopia: A Waste of Resources?

Abstract

Schools or parents make students repeat a grade under the rationale that student retention will improve learning outcomes later. However, whether retention improves student achievement remains entirely an empirical question as theoretical predictions are inconclusive. This paper uses student level survey data from Ethiopia to examine the effect of grade repetition on achievement in maths and verbal test scores and the correlation of grade repetition with school dropout. The results indicate that grade retention does not improve student achievement in maths and evidence on the improvement in verbal test scores is relatively weak. In addition, grade repetition is highly correlated with school dropout. The findings have important policy implications because they suggest that students should not be retained in the absence of targeted policy interventions that may improve educational outcomes if students repeat a grade.

4.1 Introduction

Student retention[1] is the act of holding back students to remain in the same grade in a given academic year. This occurs by the retention rule of the school, primarily, if a student fails to achieve a minimum performance or attendance requirement to pass a grade (Koppensteiner, 2014). In addition, parents can ask for grade repetition if they believe that their children are immature relative to their peers.

According to the UNESCO Global Education Digest (2012), 32.2 million pupils worldwide repeated a grade in primary education in 2010. The report also states that "pupils who are over-age for their grade due to late entry and/or repetition are at greater risk of leaving school early and children with the least opportunities arising from poverty and compounding disadvantages are most likely to repeat grades and leave school early" (UNESCO, 2012). Furthermore, Manacorda (2012) argues that "grade repetition is common in less developed countries and often accompanied by *low* enrollment and *high* dropout rates, the combination of the two is often referred to as '*wastage*'."

The costs and benefits of student retention have been a matter of intense debate in the economics and psychology literature. Eide and Showalter (2001) and Alet et al. (2013) argue that economists have not contributed much to this lively and important debate although the heart of the problem is a resource allocation decision. The opponents of student retention argue that it harms students through stigma and lower self-esteem as a result of disturbing existing peer relationships and alienating students from school. This potentially leads to lower performance and school dropout (Bowman, 2005; Brophy, 2006; Jimerson and Ferguson, 2007). In addition, grade repetition delays labor market entry, imposing substantial monetary cost. Furthermore, every repeater affects school resources in the same way as a

[1] In this chapter, I use the terms student retention, grade retention, and grade repetition interchangeably.

new student, resulting in a trade-off of per pupil school inputs (Koppensteiner, 2014).

On the other hand, proponents argue that grade repetition raises the expectations of schools and so may provide an incentive to study hard (Gary-Bobo et al., 2016). It increases student achievement by allowing low achieving students to catch-up during an extra year of schooling, thereby mastering the relevant content before proceeding to the next grade. In addition, student retention makes classes more homogeneous (Koppensteiner, 2014).

Some schools or countries allow automatic grade promotion despite low performance of a student (e.g, most Scandinavian countries) while others such as France, Portugal, and Spain have strict retention rules (EACEA and Eurydice, 2011). In Ethiopia, on average, nearly 10 percent of primary school students repeated a grade every year over the past decade (UNESCO UIS, various years). This is above the average of developing countries, which is about 5 percent, and equivalent to the Sub-Saharan average. However, some schools allow automatic grade promotion despite low performance, yet, there is no empirical evidence to support such a policy. We do not know, at least in the context of Ethiopia, whether or not repeating a grade would help to improve subsequent student performance. Empirical evidence has important policy relevance because grade repetition may be viewed as waste of resources if it does not improve student performance.

This paper uses data from more than 10,000 children in grades 4 and 5 from 94 schools spread across 7 regions in Ethiopia to examine the effect of repeating a grade on subsequent students‘ achievement and the extent to which it is correlated with probability of school dropout. Grade repeaters and non-repeaters do not have the same observed and unobserved characteristics. If students repeat a grade because of poor performance, then repeaters are less able to perform than non-repeaters, causing a potential problem of classical omitted variable bias.

To minimize this problem, I, first, assess the extent of stability of the OLS estimate by adding a large set of covariates that determine the outcome variables. Second, I use an endogenous treatment–regression model and estimate a simultaneous recursive maximum likelihood such that a plausible assumption is made regarding the correlation structure between the unobservables that affect the treatment and the unobservables that affect the potential outcomes.

Third, following Altonji et al. (2005), I use a recursive bivariate probit model that examines the sensitivity of the estimates to the correlation between the unobserved factors that determine grade repetition and the outcome – school dropout. I show estimates of the grade repetition effect for different values of the correlation between the unobservable determinants of grade repetition and school dropout.

The results indicate that grade retention does not improve student achievement in maths and evidence on the improvement in verbal test scores is relatively weak. In addition, grade repetition is highly correlated with school dropout. However, the correlation between school dropout and grade repetition seems to be largely driven by unobserved heterogeneity.

This paper provides new empirical evidence that sheds some light on the policy implication of student retention in Ethiopia. Although the analysis is not causal and so results should be interpreted cautiously, the evidence is not in favor of the policy. Therefore, students should not be retained in the absence of targeted policy interventions that may improve educational outcomes if students repeat a grade.

The rest of the chapter is organized as follows. Section 4.2 discusses the empirical literature. Section 4.3 provides some background of the primary school system in Ethiopia and describes the dataset. Section 4.4 discusses the empirical strategy. Section 4.5 presents the OLS, bivariate probit, and endogenous treatment–regression model (maximum likelihood) results. The final section concludes.

4.2 Related Literature

The empirical evidence on the effect of grade repetition is rather mixed. On the negative side, student retention is found to be one of the most significant predictors of school dropout (Bowman, 2005; Jacob and Lefgren, 2004, 2009; Stearns et al., 2007). For instance, Jimerson et al. (2002) conduct a systematic review of 17 studies and conclude that grade retention is one of the most powerful predictors of dropout status. Using data from Junior high school in Uruguay, Manacorda (2012) finds that student retention causes substantial dropout and lower achievement even 4-5 years later. In addition, Bowman (2005) argues that the financial cost and the pronounced cost to children's self-esteem could outweigh the benefit.

Grade retention may also have negative effects on adolescence behavior (Jimerson and Ferguson, 2007; Pagani et al., 2001). Hong and Yu (2007) conclude that "there is no evidence that early grade retention brings benefits to the retainee‘s reading and maths learning toward the end of the elementary years." Similarly, McCoy and Reynolds (1999) find an association of grade retention with lower achievement and suggest that "intervention approaches other than grade retention are needed to better promote school achievement and adjustment." Silberglitt et al. (2006) also find that retained students did not experience a benefit in their growth rate, even compared to similar but promoted students, and perform less compared to a random sample of students.

In addition, Alet (2010) and Alet et al. (2013) use data from France and instrument initial student achievement by kindergarten schooling duration and type as well as grade repetition by quarter of birth. The main result indicates that grade repetition has only a transitory positive effect on achievement as these initial associated gains in achievement translate into losses a few years later. Similarly, Chen et al. (2010) use data from Shaanxi province in China and find a negative effect of grade retention on school performance of students.

Another line of research suggests that there is a negative external effect of student retention. A recent study by Hill (2014) uses course fixed effects from US high schools and shows that "5-10 percent of [the] share of repeaters in a given course results in a moderate to significant increase in the probability of course failure for first-time course-takers." He also provides evidence that distinguishes course repetition externalities from low ability peer effects (not related to grade repetition).

Do the results from the literature that largely document negative effects of grade repetition imply that automatic grade promotion helps to improve academic achievement? A recent study using a difference-in-difference approach by Koppensteiner (2014) finds that automatic grade promotion reduces maths test scores by 7 percent in Brazil. The author interprets this as a disincentive effect. Fertig (2004) finds a significantly positive effect of class repetition on educational outcomes in Germany. Using a nationally representative dataset in the US, Dong (2010) provides evidence that holding children back in kindergarten has positive but diminishing effects on the academic performance up to third grade. These findings suggest that automatic grade promotion may not improve student outcomes, and therefore, provide evidence in favor of retention policies.

In the African context, the empirical evidence is quite limited despite higher occurrence of grade repetition in the continent. Glick and Sahn (2010) use data from Senegal and exploit variation across schools in test score thresholds for promotion to compare retained and promoted students with the same level of performance. They conclude that repeating students are more likely to leave school before completing primary school than students with similar ability who are not held back, pointing to the need for alternative measures to improve the skills of lagging children. However, they cannot control for unobserved school fixed effects. In contrast, a more recent empirical study on Uganda that exploits variation across schools and time with regard to the adoption of automatic promotion policy (i.e., the new policy affected only the public schools) finds that this policy translates into an increase in learning outcomes in reading and mathematics at primary three and primary six levels (Okurut, 2015). However, this study assumes that public and private schools will have the same trend with regard to factors affecting learning outcomes.

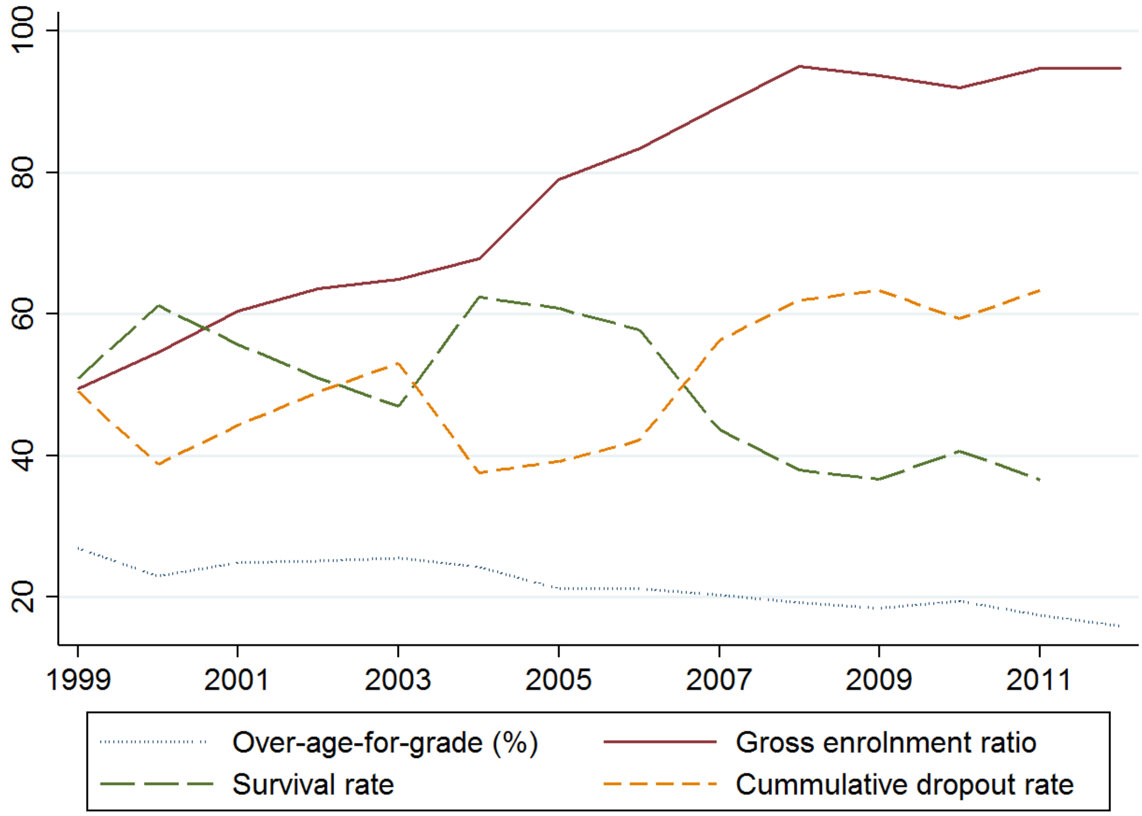
4.3 Background and Data Description

4.3.1 Background

In Ethiopia, children may go to pre-school around age 4-6, depending on the availability of schools and family income. Pre-school is not supported by government and is therefore associated with significant costs. In addition, most rural communities do not have access to pre-school irrespective of the parents ability to pay. Availability of pre-school is concentrated in major urban areas such as the capital city and regional cities. However, primary school is compulsory and completely free for those who attend public school.

Efforts on the expansion of primary school supply, enrollments, and closing the gender gap in terms of enrollment has dramatically increased over the past one or two decades. However, schooling quality, as reflected in measures such completion rate, extent of grade repetition, dropout etc still remains questionable. As can be seen in Figure 4.1, despite an increasing gross enrollment ratio, the cumulative dropout rate has increased from about 50 percent in 1999 to slightly above 60 percent in 2011. For instance, according to the most recent data from UNESCO in 2011, the survival rate to the last grade of primary education in Ethiopia was only 37 percent in contrast to the average of Sub-Saharan African countries and the average of the world which are 58 percent and 74 percent, respectively.

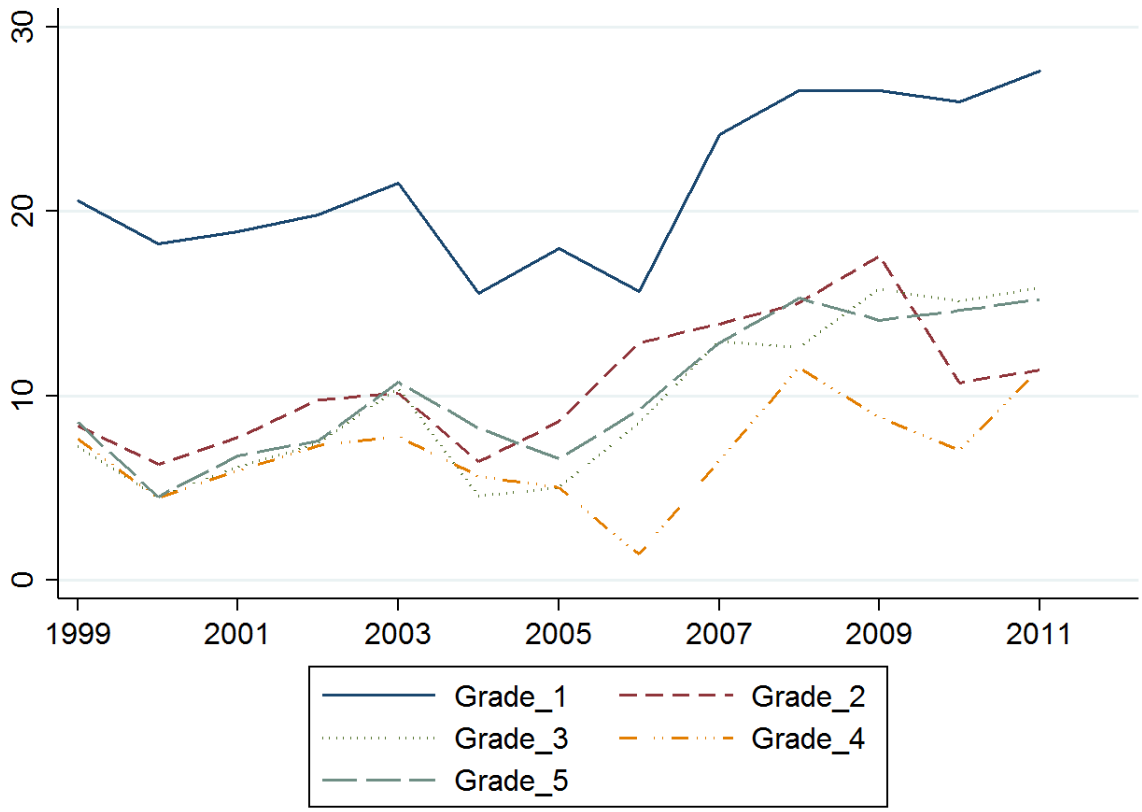
Figure 4.1: Enrollment, Survival, and Cumulative Dropout Rates in Primary School, Ethiopia



Source: Author's analysis from UNESCO database UIS.Stat.

Figure 4.2 shows dropout rates by grade from 1999-2011 in Ethiopia. Dropout rates have increased over time in each grade with the highest dropout rate occurring in grade 1 in 2011.

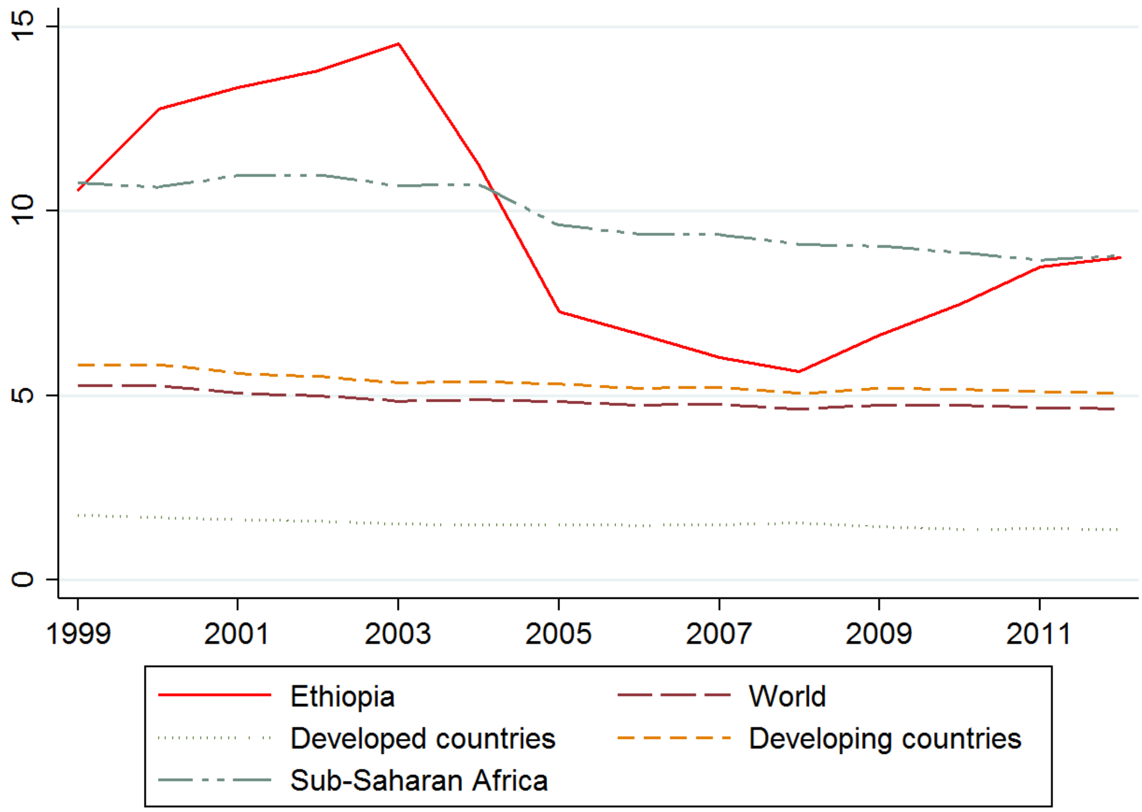
Figure 4.2: Dropout Rates by Grade in Primary School, Ethiopia



Source: Author's analysis based on UNESCO database UIS.Stat.

Figure 4.3 shows that the average grade repetition rate of primary education in Ethiopia is above the average of developing countries and has even been above sub-Saharan average in the late 1990s and early 2000s, with a sharp decline after 2003. Despite this trend, Abay (2013) argues that real school resources have been increasing over time while quality and efficiency of primary education have been deteriorating, which is reflected in grade repetition and school dropout rates. According to Abay (2013), the recently observed reduction in repetition rates is not genuine because it was mainly driven by the instruction of teachers to reduce repetition rates.

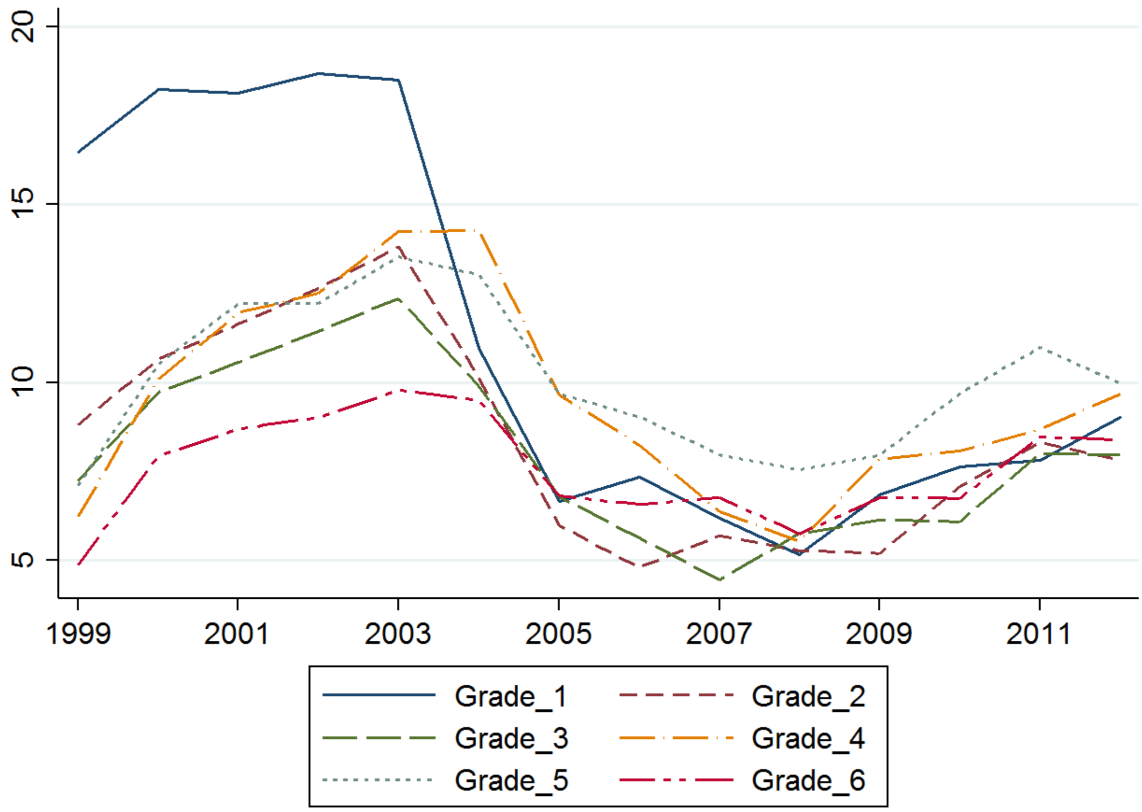
Figure 4.3: Percentage of Grade Repeaters in Primary School



Source: Author's analysis based on UNESCO database UIS.Stat.

Further disaggregation of the percentage of repeaters in Ethiopia, by grade, in Figure 4.4, shows higher rates and increasing trends during the pre-2004 and post-2008 periods. In the mid 2000's, repetition rates were lower and declining.

Figure 4.4: Percentage of Grade Repeaters by Grade in Primary School

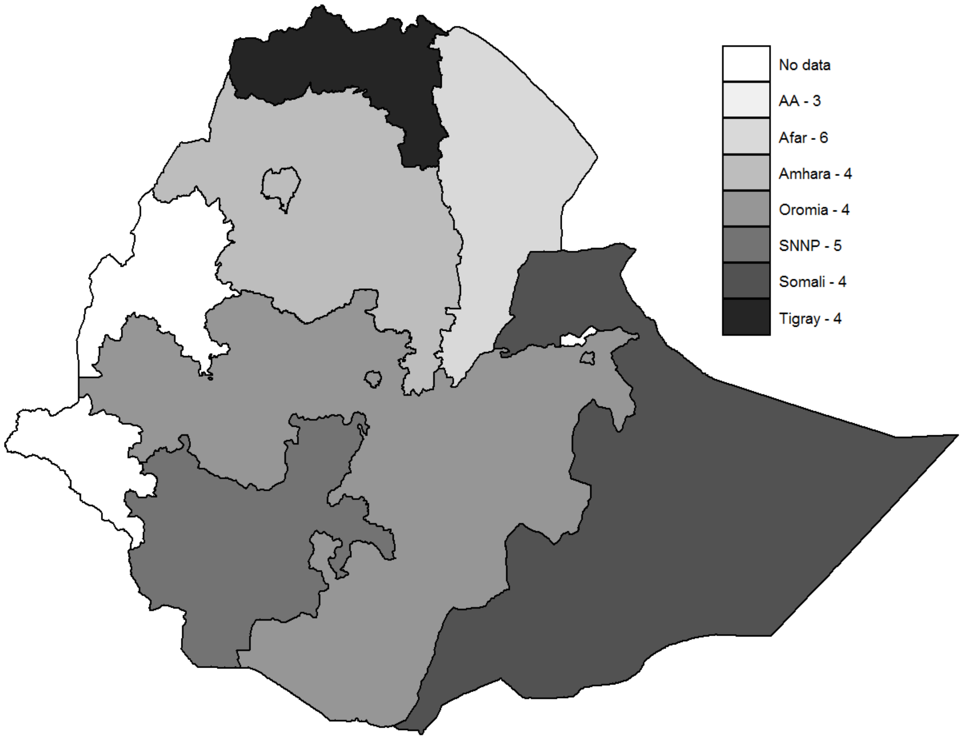


Source: Author's analysis based on UNESCO database UIS.Stat.

4.3.2 Data Description

I use cross-sectional data from the Young Lives Schools Survey in Ethiopia, which includes a large set of variables at the school, teacher, class, and pupil level. The survey includes about 10,000 pupils that are in grades 4 and 5 in the 2012/13 academic year in 94 schools in 7 regions of the country. These schools were selected from 30 sentinel sites effectively producing the census of pupils in grades 4 and 5 from these locations. Figure 4.5 below presents the number of sentinel sites from each region selected.

Figure 4.5: Number of Sentinel Sites by Region



Source: Author's analysis based on Young Lives data.

The summary statistics in Table 4.1 indicate that about 24 percent of the students in the sample have repeated a grade. This is a very large fraction given the national average of repetition rate did not exceed a maximum of 15 percent in the past two decades. This could be because the students in the sample come from relatively poor households as the Young Lives project focuses primarily on the investigation of childhood poverty. In addition, 18 percent of students have dropped out of school while 23 percent of students have a long-term health problem.

Surprisingly, the mean test score from the 25 verbal items shows a deterioration when comparing the score at the end of academic year (Verbal test score2) to that of the beginning of the academic year (Verbal test score). Although 6 of the items students responded to in the second round are new and relatively difficult questions from the first round, this raises questions about the school effectiveness. Even considering the same 19 questions, there is no improvement. There is only a slight improvement in maths achievement.

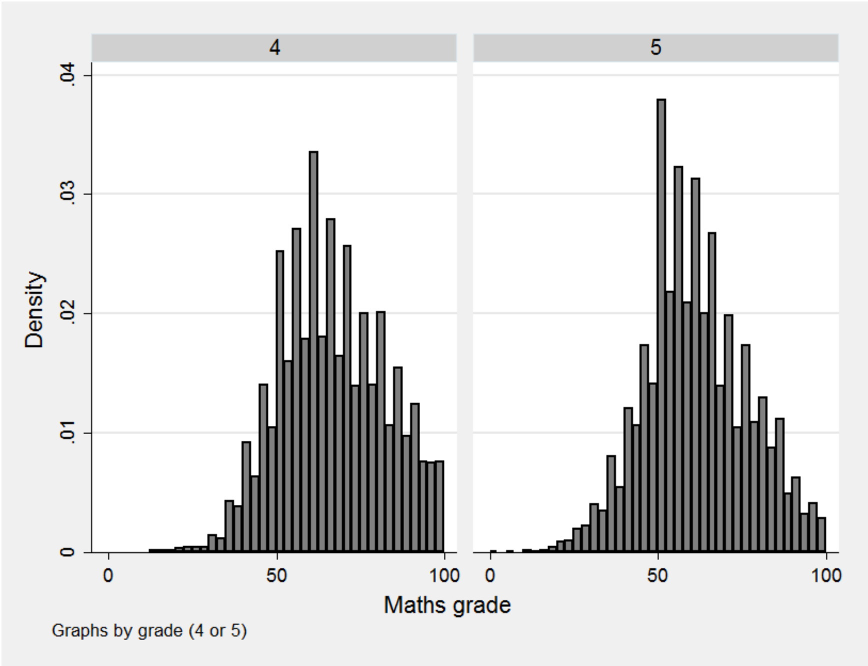
Moreover, there is a lot of variation in the age at which children start school and the age at which they achieve grade 4 and 5 as well as in student absenteeism. Only 50 percent of parents of the students in the sample can read and write. However, a large proportion of students reported that they receive some help from family members with their school work.

Table 4.1: Selected Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Urban=1	0.712	0.453	0	1	11641
Grade=4	0.512	0.5	0	1	11641
Maths test score	13.408	4.206	0	25	11641
Verbal test score	18.148	5.368	0	25	11641
Maths test score2	12.136	6.618	0	24	11619
Verbal test score2	13.842	7.327	0	25	11619
Change Maths test score	-1.272	6.137	-23	20	11619
Change Verbal test score	-4.306	6.756	-25	24	11619
Maths grade	63.919	15.725	7	100	10945
Language grade	68.219	16.071	8	100	10947
Dropped out of school	0.177	0.382	0	1	11549
Repeated grade	0.243	0.429	0	1	11610
Sex=Girl	0.503	0.5	0	1	11641
Age in yrs	11.584	1.831	8	34	11641
Age squared	137.549	48.922	64	1156	11641
Age starting school yrs	6.867	1.816	4	29	11641
Absence days since Oct 1	1.111	2.321	0	26	11641
Absence days since wave 1	5.887	7.717	0	72	10951
Attended preschool	0.516	0.5	0	1	11573
Longterm health problem	0.228	0.42	0	1	11641
No of meals/day=1	0.044	0.205	0	1	11615
No of meals/day=2	0.148	0.355	0	1	11615
No of meals/day>=3	0.808	0.394	0	1	11615
No of other people	6.963	3.572	1	30	11641
Mother read & write	0.479	0.5	0	1	11641
Father read & write	0.59	0.492	0	1	11641
No one read & write	0.071	0.257	0	1	11641
Help at home=never	0.182	0.386	0	1	11606
Help at home=always	0.459	0.498	0	1	11606
Help at home=sometimes	0.359	0.48	0	1	11606

Source: Author's own calculations based on Young Lives school survey data 2012/13, Ethiopia.

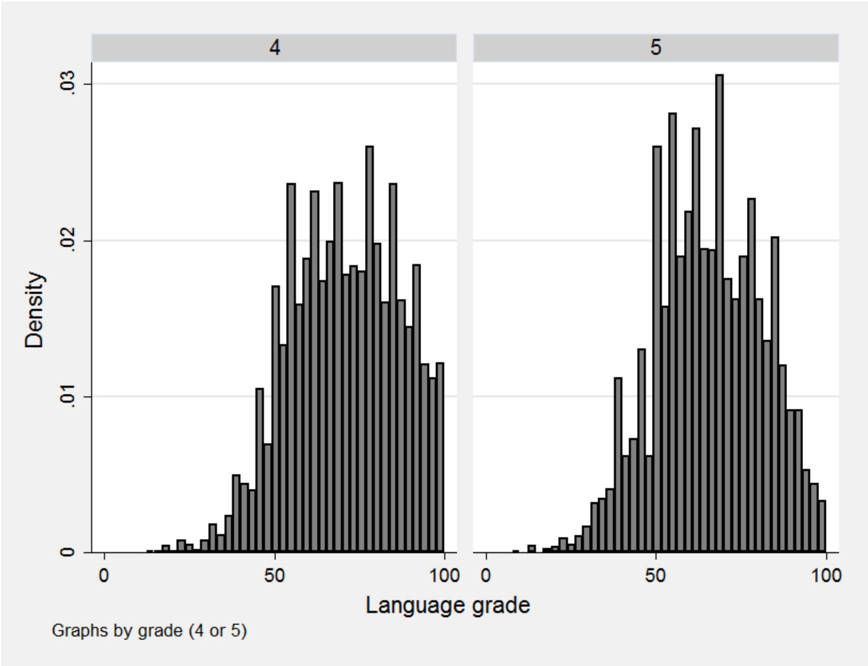
Figure 4.6: The Distribution of Maths Grade in Semester I



Source: Author's analysis based on Young Lives school survey data 2012/13, Ethiopia.

The distributions of the outcome variables of maths and language Semester I grades (out of 100), by grade, are shown in Figures 4.6 and 4.7, respectively. The distributions look similar for both grades and subjects and are skewed towards the right with means in the high 60's.

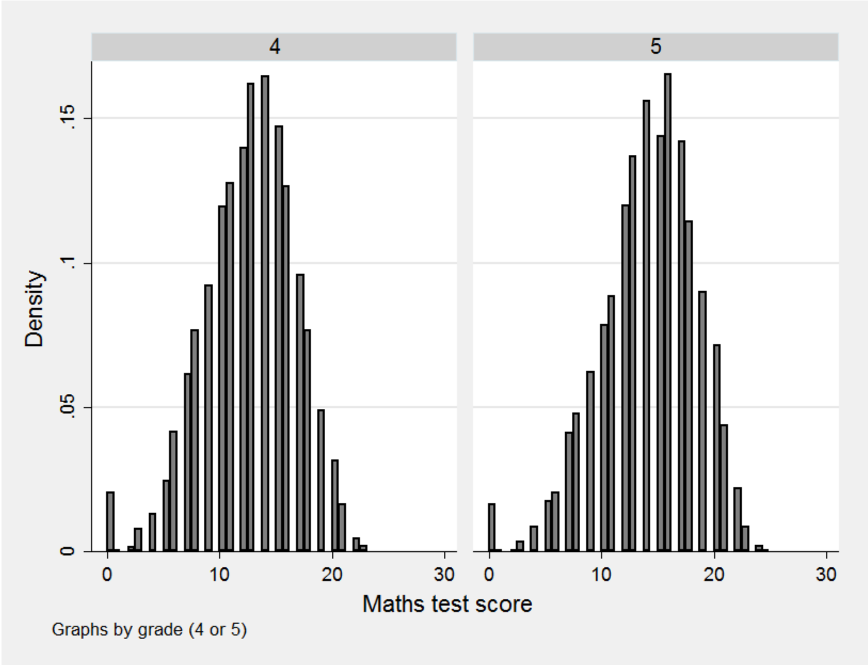
Figure 4.7: The Distribution of Language Grade in Semester I



Source: Author's analysis based on Young Lives school survey data 2012/13, Ethiopia.

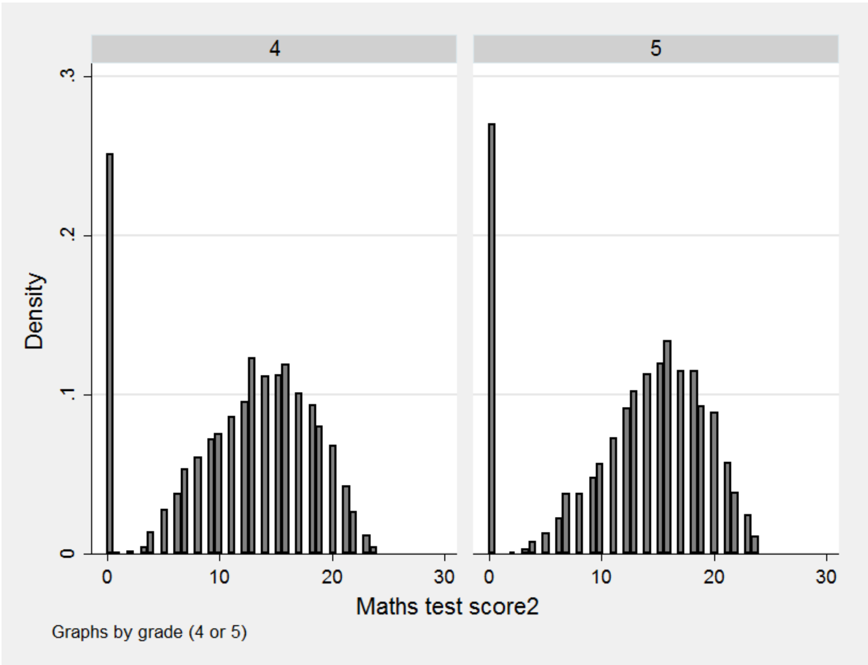
Turning to the distribution of the outcome variables of maths and verbal test scores at the beginning and the end of the academic year, there is a large number of 0s in Figures 4.9 and 4.11 as compared to Figures 4.8 and 4.10. This is not because students achieved 0 out of 25 items, but rather because of the absence of students for the test at the end of the academic year. However, some of the 0 values are a result of poor student performance (as is the case of Figures 4.8 and 4.10).

Figure 4.8: The Distribution of Maths Score at the Beginning of the Academic Year



Source: Author's analysis based on Young Lives school survey data 2012/13, Ethiopia.

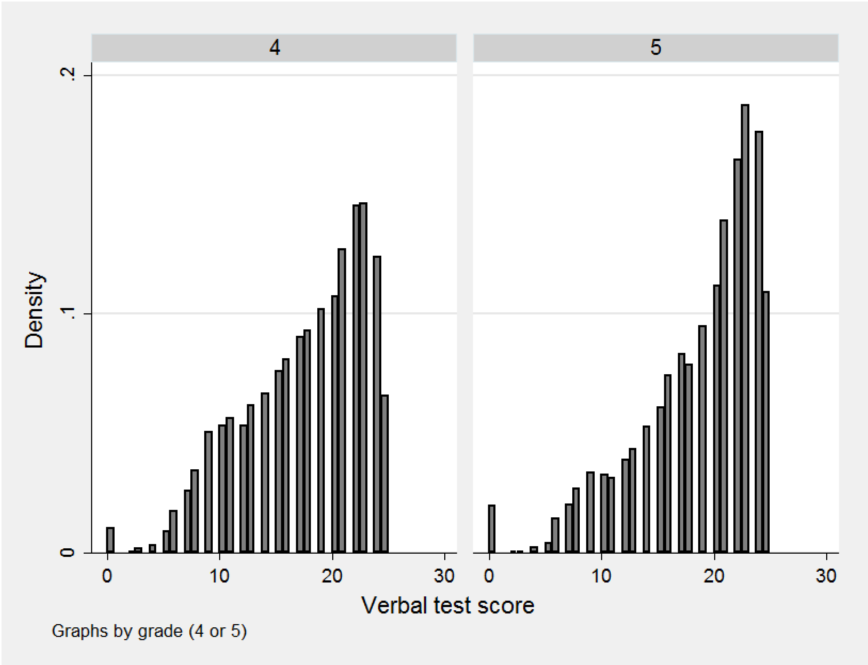
Figure 4.9: The Distribution of Maths Score at the End of the Academic Year



Source: Author's analysis based on Young Lives school survey data 2012/13, Ethiopia.

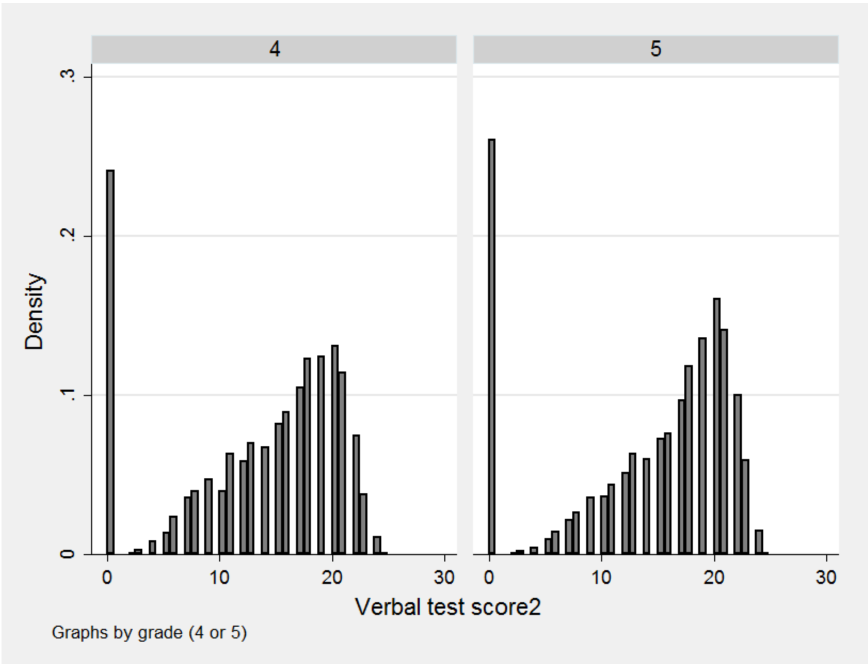
In contrast to the Semester I grade distributions above (Figures 4.6 and 4.7), the distributions of maths and verbal test scores differ for both the beginning and the end of the academic year (Figure 4.8 versus Figure 4.10 and Figure 4.9 versus Figure 4.11). There are more students scoring above 20 in the verbal tests compared to the maths tests.

Figure 4.10: The Distribution of Verbal Score at the Beginning of the Academic Year



Source: Author's analysis based on Young Lives school survey data 2012/13, Ethiopia.

Figure 4.11: The Distribution of Verbal Score at the End of the Academic Year



Source: Author's analysis based on Young Lives school survey data 2012/13, Ethiopia.

4.4 Empirical Strategy

From an empirical perspective, one would ideally assign students to repeating a grade or being promoted regardless of their performance in a given academic year. If such an assignment was completely random, then the mean difference in outcomes would be entirely attributable to repeating a grade. However, the students observed in this survey were not assigned randomly and the probability of repeating a grade depends on both observed and unobserved characteristics. In particular, students who perform below a certain standard will repeat a grade, resulting in a selection effect. Therefore, the OLS estimator (β) of the mean difference between achievement of those who have repeated a grade and those who have not may provide biased estimates. The OLS model can be written as follows:

$$y_j = \alpha + \beta R_j + X_j \gamma + \epsilon_j \tag{4.1}$$

where j indexes an individual. y_j is the outcome variable, R_j is an indicator for repeating a grade, X_j is a set of independent variables affecting achievement, and ϵ_j is the unobserved error term. α, β, and γ are vectors of model parameters. Thus, the true model of equation 4.1 will look like equation 4.2 after the relevant omitted variable, say ability, u, is included:

$$y_j = \alpha + \beta * R_j + \gamma * X_j + \theta * u_j + v_j \tag{4.2}$$

v is now well-behaved error term ($E(v_j) = 0$). The direction and magnitude of the bias of β will depend on θ – the effect of ability on achievement and the correlation of R with u. That is, calling $\hat{\beta}$ the estimate from equation 4.1 that omits u_j, $E(\hat{\beta})$=$\beta + \theta * \frac{cov(R,u)}{var(R)}$. In particular, $\hat{\beta}$ will underestimate the benefit of repeating a grade (providing negative bias) if high ability children are high achievers and less likely to repeat a grade (i.e., θ is positive but the correlation of R with u is negative). On the other hand, $\hat{\beta}$ could overestimate the benefit of repeating a grade (providing positive bias) if high ability parents force their (high ability) children to repeat a grade but higher ability increases achievement (i.e., θ is positive but the correlation of R with u is positive).

In the Ethiopian context, most students repeat a grade if they are unable to meet a minimum requirement (both in terms of performance and attendance measure) to be promoted. Thus, the former bias will be larger than the latter. In this case, it is likely that OLS will overestimate the negative effect of grade repetition.

To assess and minimize the bias, I use the following approaches. First, I assess the extent of movement of the β coefficient by adding student, family, and school level determinants of achievement and by studying how much additional variation comes from the inclusion of these covariates. This provides an informal assessment of how sensitive the OLS estimate is to adding a number of relevant covariates. This is because some of the observed characteristics such as child health, pre-school attendance, and parental education can pick up some of the variation that could be strongly correlated with child and parental ability and motivation.

Second, I use an endogenous treatment regression model that allows for a specific correlation structure between the unobservables that affect the treatment and the unobservables that affect the potential outcomes (Heckman, 1976, 1978; Maddala, 1986). That is, I simultaneously estimate the treatment and outcome equations recursively subject to certain constraints regarding the correlation of the unobservables with that of the independent and outcome variables using full information maximum likelihood. This allows us to see if the estimated parameters vary significantly when I make different assumptions about the correlation structure of the unobservables in both equations.

Third, following Altonji et al. (2005), I use recursive bivariate probit model that examines the sensitivity of the estimates to the correlation between the unobserved factors that determine grade repetition and the outcome – school dropout. I show estimates of the grade repetition effect for different values of the correlation between the unobservable determinants of grade repetition and school dropout.

4.4.1 The Endogenous Treatment–Regression Model

To formalize the specification, I begin with a general potential outcomes model with separate variance and correlation parameters for the treatment and control groups. Let y_{1j} and y_{0j} be the outcome variables of student j in the treatment and the control group respectively. t_j is a dummy variable that takes on the value 1 if a student has repeated a grade, and 0 otherwise. ϵ_{1j}, ϵ_{0j} are the respective error terms. The model can be written as:

$$\begin{aligned} y_{1j} &= X_j\beta_1 + \epsilon_{1j} \quad if \quad t_j = 1 \\ y_{0j} &= X_j\beta_0 + \epsilon_{0j} \quad if \quad t_j = 0, \\ where \quad t_j &= \begin{cases} 1, & if \quad W_j\gamma + u_j > 0 \\ 0, & Otherwise, \end{cases} \end{aligned} \tag{4.3}$$

and where W is the set of variables that determine the treatment. I cannot observe y_{1j} and y_{0j} for the same individual j at the same time. Instead, I observe $y_j = t_jy_{1j} + (1 - t_j)y_{0j}$. In this unconstrained model, the vector of the error terms $(\epsilon_{1j}, \epsilon_{0j}, u_j)'$ comes from a mean zero trivariate normal distribution with covariance matrix (StataCorp, 2015):

$$\begin{pmatrix} \sigma_0^2 & \sigma_{01} & \sigma_0\rho_0 \\ \sigma_{01} & \sigma_1^2 & \sigma_1\rho_1 \\ \sigma_0\rho_0 & \sigma_1\rho_1 & 1 \end{pmatrix} \tag{4.4}$$

where $corr(u_j, \epsilon_0) = \rho_0 = \frac{\sigma_{u0}^2}{\sigma_u \sigma_0}$ and $corr(u_j, \epsilon_1) = \rho_1 = \frac{\sigma_{u1}^2}{\sigma_u \sigma_1}$. σ_u^2 is assumed to be equal to 1.

Maddala (1986) derives the likelihood function for this model as follows:

$$lnf_j = \begin{cases} \ln \Phi\{\frac{W_j\gamma + (Y_j - X_j\beta_1)\rho_1/\sigma_1}{\sqrt{1-\rho_1^2}}\} - 1/2(\frac{Y_j - X_j\beta_1}{\sigma_1})^2 - ln\sqrt{2\pi\sigma_1}, & t_j = 1 \\ \ln \Phi\{\frac{-W_j\gamma - (Y_j - X_j\beta_0)\rho_0/\sigma_0}{\sqrt{1-\rho_0^2}}\} - 1/2(\frac{Y_j - X_j\beta_0}{\sigma_0})^2 - ln\sqrt{2\pi\sigma_0}, & t_j = 0, \end{cases} \tag{4.5}$$

$$lnL = \sum_{j=1}^{n} lnf_j,$$

where Φ is the cumulative distribution function of the standard normal distribution. The covariance between ϵ_{0j} and ϵ_{1j}, σ_{01}, cannot be estimated because the potential outcomes y_{0j} and y_{1j} are never observed simultaneously. Although additional exogenous variables will aid identification (Greene, 2000, 2013; Wooldridge, 2010), the set of variables indexed by W need not be different from the set of X variables for exclusion restriction to be satisfied (Lokshin and Sajaia, 2004; Miranda and Rabe-Hesketh, 2006). The key assumption is that joint normality of the error terms in the binary (treatment) and continuous (outcome) equations should hold. I assume as if σ_0 and σ_1 are unidentified by two parameters. In particular I consider as if ρ_0 and ρ_1 as unidentified.

To ensure σ_0 and σ_1 are greater than zero, note that σ_0 and σ_1 are not directly estimated in the maximum likelihood estimation; rather, lnσ_0 and lnσ_1 are estimated to ensure that the estimated parameters are positive. Similarly, the parameters ρ_0 and ρ_1 are also not directly estimated; rather, atanhρ_0 and atanhρ_0 are directly estimated using the following expression such that ρ_0 and ρ_1 are bounded between [-1, 1]:

$$atanh\rho = \frac{1}{2}\ln\{\frac{1+\rho}{1-\rho}\} \tag{4.6}$$

The new parameter vector is then,

$$V = (\beta_0', \beta_1', \gamma', atanh\rho_0, atanh\rho_1, \ln\sigma_0, \ln\sigma_1) \tag{4.7}$$

If ρ_1 is negative (positive), this indicates that unobservables that increase the likelihood of grade repetition tend to occur with unobservables that decrease (increase) test score outcomes. Similarly, if ρ_0 is positive (negative), this indicates that unobservables that increase the likelihood that a student has not repeated a grade tend to occur with unobservables that increase (decrease) test score outcomes. Furthermore, if the estimated ρ_0 is larger than the estimated ρ_1, this indicates a stronger relationship between the unobservables and treatment outcomes in the untreated group relative to the treated group.

The average treatment effect (ATE) is the average difference in outcomes between treatment and control group. That is,

$$E(y_{1j}-y_{0j}) = E\{E(y_{1j}-y_{0j}|X_j, \epsilon_{0j}, \epsilon_{1j})\} = E(X_j\beta_1+\epsilon_1-X_j\beta_0-\epsilon_0) = E(X_j(\beta_1-\beta_0)) \tag{4.8}$$

This expectation can be estimated as a predictive margin when $X_j(\beta_1-\beta_0)$ varies in X_j. Otherwise, the ATE is estimated as the coefficient of t_j in the model (StataCorp, 2015).

4.4.2 The Bivariate Probit Model

Suppose the bivariate probit model, following Altonji et al. (2005), is:

$$\begin{aligned} R^* &= X\gamma + u, \quad R = 1(R^* > 0) \\ S^* &= X\pi + \beta R + \epsilon, \quad S = 1(S^* > 0) \end{aligned} \tag{4.9}$$

$$\begin{pmatrix} u \\ \epsilon \end{pmatrix} \sim N\left(\begin{pmatrix}\begin{bmatrix} 0 \\ 0 \end{bmatrix}\end{pmatrix}, \begin{pmatrix}\begin{bmatrix} 1 & \rho \\ \rho & 1 \end{bmatrix}\end{pmatrix}\right)$$

where R and S stand for grade repetition and school dropout. X refers to a number of control variables. The observable variables R and S take the value 1 if the underlying latent variable is greater than 0, and 0 otherwise. The model is formally identified and can be estimated by a bivariate model using a full information maximum likelihood (FIML) (Hanushek, 2013; Greene, 2013; Wooldridge, 2010). In Table 4.8, I show estimates of grade repetition effects that correspond to a range of assumptions about ρ, the correlation between the error terms in the equations for R and S. Rosenbaum discusses such sensitivity tests (see Rosenbaum, 2002, chap. 4).

4.5 Results and Discussion

4.5.1 The OLS Results

Grade Repetition and Test Scores

Table 4.2 presents the OLS estimates of the effect of grade repetition on mathematics and language grades in the odd and even columns, respectively. The results indicate that the unconditional mean achievement in mathematics is nearly 8 percentage points lower for repeaters compared to promoted students (Column 1). This mean difference declines to 6 percentage points when regional and school level factors are controlled for (Column 3). These controls add about 20 percent more variation to the variation in achievement. When I further control for a large set of student and family level characteristics that explain additional 10 percent of the variation, grade repeaters are performing 5.6 percentage points less than non-repeaters, on average. A similar trend is observed in language performance. Although this relationship is not causal, these results imply that there is a strong association between repeating a grade and subsequently performing less in subject matters as this negative relationship does not decline substantially when the number of covariates increases.

Table 4.2: The Effect of Grade Repetition on Student Achievement

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Maths	Lang.	Maths	Lang.	Maths	Lang.
Repeated grade	-7.647***	-8.986***	-6.139***	-7.880***	-5.566***	-7.015***
	(0.330)	(0.343)	(0.307)	(0.318)	(0.313)	(0.324)
Observations	10,918	10,920	10,918	10,920	10,715	10,717
R-squared	0.043	0.057	0.246	0.260	0.299	0.304
Region FE	N	N	Y	Y	Y	Y
Grade FE	N	N	Y	Y	Y	Y
School FE	N	N	Y	Y	Y	Y
Class FE	N	N	Y	Y	Y	Y
Teacher FE	N	N	Y	Y	Y	Y
Add.controls	N	N	N	N	Y	Y

Note: The dependent variables are raw mathematics and language grades out of 100 in Semester I. Additional covariates include urban dummy, sex, age, age squared, age at starting school, number of days absent from school, number of other people, an indicators for attendance of pre-school, having long-term health problem, dropped out of school, mother and father alive, mother and father can read and write, no one can read and write in the family, the number of meals usually eaten per day, frequency of help with study. The same set of controls is included throughout all tables and so the same notes apply unless otherwise mentioned. Robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Source: Young Lives school survey data from Ethiopia.

Results for the test score outcomes out of 25 items of maths and verbal questions designed by the Young Lives team are presented in Table 4.3. The results show that grade repeaters have lower performance in mathematics and verbal test scores and this association does not decline when I control for school, student, and family level characteristics. In some cases, the coefficient estimates even rise when I control for additional covariates.

Table 4.3: The Effect of Grade Repetition on Student Achievement (2)

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Maths	Maths II	Maths	Maths II	Maths	Maths II
Repeated grade	-1.236***	-1.630***	-1.474***	-1.685***	-1.407***	-1.545***
	(0.0848)	(0.104)	(0.0762)	(0.0933)	(0.0803)	(0.0960)
Observations	11,478	9,785	11,478	9,785	10,624	9,588
R-squared	0.018	0.024	0.292	0.300	0.334	0.337
VARIABLES	Verbal	Verbal II	Verbal	Verbal II	Verbal	Verbal II
Repeated grade	-1.611***	-1.335***	-1.983***	-1.570***	-1.828***	-1.330***
	(0.111)	(0.112)	(0.0965)	(0.0951)	(0.101)	(0.0971)
Observations	11,503	9,785	11,503	9,785	10,627	9,588
R-squared	0.018	0.015	0.395	0.401	0.418	0.428
Region FE	N	N	Y	Y	Y	Y
Grade FE	N	N	Y	Y	Y	Y
School FE	N	N	Y	Y	Y	Y
Class FE	N	N	Y	Y	Y	Y
Teacher FE	N	N	Y	Y	Y	Y
Add.controls	N	N	N	N	Y	Y

Note: See notes to Table 4.2. The dependent variables are raw mathematics and verbal test scores out of 25 items at the beginning and the end of the academic year. Robust standard errors are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Source: Young Lives school survey data from Ethiopia.

These maths and verbal test scores are observed at the beginning and the end of the academic calendar. In the following table, I present results that show the association between grade repetition and changes in test score outcomes. I find that these results are different from previous results for maths and verbal scores. Specifically, there is a negative association between grade repetition and maths achievement, which declines when I control for additional covariates. However, there is some suggestive evidence that retained students tend to do better in verbal scores than non-repeaters, on average. This association gets stronger when I control for additional covariates.

Table 4.4: The Effect of Grade Repetition on Changes in Student Achievement

VARIABLES	(1) MathsC	(2) MathsC	(3) MathsC	(4) VerbalC	(5) VerbalC	(6) VerbalC
Repeated grade	-0.294***	-0.172**	-0.130	0.391***	0.482***	0.539***
	(0.0867)	(0.0859)	(0.0890)	(0.0900)	(0.0912)	(0.0937)
Observations	9,704	9,704	9,509	9,703	9,703	9,507
R-squared	0.001	0.079	0.085	0.002	0.052	0.056
Region FE	N	Y	Y	N	Y	Y
Grade FE	N	Y	Y	N	Y	Y
School FE	N	Y	Y	N	Y	Y
Class FE	N	Y	Y	N	Y	Y
Teacher FE	N	Y	Y	N	Y	Y
Add.controls	N	N	Y	N	N	Y

Note: See notes to Table 4.2. The dependent variables are changes (end of the academic year test–beginning of the academic year) in mathematics and verbal test scores out of 25 items. Robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Source: Young Lives school survey data from Ethiopia.

Grade Repetition and School Dropout

The findings from the association between grade repetition and school dropout largely confirm the evidence of the literature that grade repetition predicts school dropout. Conditional on regional, school, student, and family level characteristics, grade repeaters are about 10 percent more likely to drop out of school compared to promoted students (Table 4.5, Column 3).

However, this positive association between grade repetition and school dropout could be due to unobserved factors that affect both grade repetition and school dropout status. I further check the sensitivity of this result using a bivariate probit model that takes into account the correlation between the unobservables that affect grade repetition and unobservables that affect school dropout in Section 4.5.3.

Table 4.5: The Effect of Grade Repetition on School Dropout

VARIABLES	(1) Dropout	(2) Dropout	(3) Dropout
Repeated grade	0.147***	0.131***	0.0970***
	(0.00935)	(0.00950)	(0.00994)
Observations	11,524	11,524	10,646
R-squared	0.027	0.068	0.110
Region FE	N	Y	Y
Grade FE	N	Y	Y
School FE	N	Y	Y
Class FE	N	Y	Y
Teacher FE	N	Y	Y
Add.controls	N	N	Y

Note: See notes to Table 4.2. The dependent variable takes on a value 1 if a student has dropped out of school, and 0 otherwise. Marginal effects of the Probit model are similar. Robust standard errors are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Source: Young Lives school survey data from Ethiopia.

4.5.2 The Endogenous Treatment–Regression Model Results

Grade Repetition and Test Scores

This section studies the implications of using the alternative assumptions when estimating the effect of grade repetition on student achievement. A more flexible assumption is made that allows for a specific correlation structure between unobservables that affect the treatment and unobservables that affect the potential outcomes. Accordingly, there are two correlation coefficients for the error terms, ρ_0 and ρ_1. The former captures the correlation between the error terms that affect the treatment and the errors that affect the potential outcomes for the untreated group and is assumed to be positive because the key missing variable ability is likely to be associated positively with the probability of non-repeating a grade and student achievement. The latter captures the corresponding correlation for the treated group, which is assumed to take on negative values because ability will be negatively associated with the probability of repeating a grade but positively associated with student achievement.

I present results under the assumption that up to 0.5 (in absolute value) correlation among the error terms is allowed (for both ρ_0 and ρ_1) in Tables 4.6 and 4.7. The first column does not impose any constraints and therefore a Wald test of independence between the error terms for both treated and untreated groups can be reported (See notes to tables 4.6 and 4.7). The plausible values of ρ_0 and ρ_1 are not likely to be larger than 0.3 in absolute value. By increasing the correlation coefficient of the error terms up to 0.5, I can then see how big this correlation would have to be to switch the sign of the coefficient of the independent variable (grade repetition indicator).

Results from the full information maximum likelihood estimation (Table 4.6) show that ρ_0=0.14 and ρ_1=−0.12, consistent with the expected sign. The p value of the Wald test of independence is 0.0027, rejecting the null hypothesis that each correlation coefficient is 0, at a 1% level of significance. To switch the sign of the coefficient on the independent variable, the correlation between the errors that affect the treatment and the errors that affect the potential outcomes in the treated group would have to be at least −0.5 (in both Tables 4.6 and 4.7), which is highly unlikely.

Thus, the effect of grade repetition on student achievement in maths and language grades is likely to be negative. The null hypothesis that the effect of grade repetition on student achievement is positive can be rejected with high confidence.

Table 4.6: The Effect of Grade Repetition on Student Achievement: Maths

	(1)	(2)	(3)	(4)	(5)	(6)
	$\rho_1 = uncons.$	$\rho_1 = -0.1$	$\rho_1 = -0.2$	$\rho_1 = -0.3$	$\rho_1 = -0.4$	$\rho_1 = -0.5$
$\rho_0 = uncons.$	-4.686***					
	(1.791)					
$\rho_0 = 0.1$		-4.694***	-3.148***	-1.533***	0.196	2.102***
		(0.318)	(0.324)	(0.332)	(0.344)	(0.359)
$\rho_0 = 0.2$		-5.369***	-3.821***	-2.205***	-0.474	1.436***
		(0.318)	(0.325)	(0.333)	(0.345)	(0.361)
$\rho_0 = 0.3$		-6.059***	-4.510***	-2.892***	-1.157***	0.757**
		(0.319)	(0.326)	(0.335)	(0.347)	(0.363)
$\rho_0 = 0.4$		-6.776***	-5.225***	-3.603***	-1.865***	0.0557
		(0.321)	(0.328)	(0.337)	(0.349)	(0.365)
$\rho_0 = 0.5$		-7.534***	-5.979***	-4.354***	-2.610***	-0.682*
		(0.323)	(0.330)	(0.339)	(0.352)	(0.368)
N	10638	10638	10638	10638	10638	10638
ρ_0	0.141 (0.055)					
ρ_1	-0.118 (0.101)					
σ_0	13.43 (0.111)					
σ_1	12.51 (0.223)					
λ_0	1.888 (0.745)					
λ_1	-1.481 (1.276)					

Note: The dependent variable is the raw mathematics grade out of 100. Each cell is the average treatment effect of the endogenous regressor from a separate regression for each column and row value of ρ_1 and ρ_0 after controlling for all observables in Table 4.2. The first column and row does not constrain ρ_1 and ρ_0. Wald test of independence (ρ_1=ρ_0=0): chi(2)=11.82. Prob >chi2=0.0027. Robust standard errors are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Source: Young Lives school survey data from Ethiopia.

Table 4.7: The Effect of Grade Repetition on Student Achievement: Language

	(1)	(2)	(3)	(4)	(5)	(6)
	$\rho_1 = uncons.$	$\rho_1 = -0.1$	$\rho_1 = -0.2$	$\rho_1 = -0.3$	$\rho_1 = -0.4$	$\rho_1 = -0.5$
$\rho_0 = uncons.$	-4.975*					
	(2.653)					
$\rho_0 = 0.1$		-6.006***	-4.400***	-2.724***	-0.931***	1.042***
		(0.326)	(0.330)	(0.336)	(0.344)	(0.355)
$\rho_0 = 0.2$		-6.690***	-5.084***	-3.407***	-1.612***	0.366
		(0.327)	(0.330)	(0.336)	(0.345)	(0.357)
$\rho_0 = 0.3$		-7.391***	-5.783***	-4.104***	-2.306***	-0.324
		(0.327)	(0.331)	(0.338)	(0.346)	(0.359)
$\rho_0 = 0.4$		-8.118***	-6.508***	-4.826***	-3.024***	-1.036***
		(0.328)	(0.333)	(0.339)	(0.348)	(0.361)
$\rho_0 = 0.5$		-8.885***	-7.272***	-5.586***	-3.778***	-1.783***
		(0.330)	(0.335)	(0.341)	(0.351)	(0.364)
N	10640	10640	10640	10640	10640	10640
ρ_0	0.111 (0.111)					
ρ_1	-0.169 (0.127)					
σ_0	13.55 (0.123)					
σ_1	13.10 (0.271)					
λ_0	1.506 (1.508)					
λ_1	-2.218 (1.688)					

Note: The dependent variable is the raw language grade out of 100. Each cell is the average treatment effect of the endogenous regressor from a separate regression for each column and row value of ρ_1 and ρ_0 after controlling for all observables in Table 4.2. The first column and row does not constrain ρ_1 and ρ_0. Wald test of independence (ρ_1=ρ_0=0): chi(2)=7.99. Prob >chi2=0.0184. Robust standard errors are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Source: Young Lives school survey data from Ethiopia.

4.5.3 The Bivariate Probit Model Results: Grade Repetition and School Dropout

The results presented in Table 4.8 show that the correlation between grade repetition and school dropout is likely to be explained by the omitted variables (unobservables). Unlike the OLS estimate, once we control for the correlation between the error terms in the grade repetition and school dropout equations using a bivariate probit model, the effect of grade repetition is statistically insignificant even at 10 percent level (first column).

The correlation coefficient (ρ=0.6) suggests that there is a high degree of correlation between the unobservables that affect grade repetition and the unobservables that affect school dropout. Furthermore, when different correlation values are assumed for these error terms, the results vary. For instance, when I assume ρ=0.1 and ρ=0.3, the effect of grade repetition on student achievement switches from positive to negative (Columns 3 and 5). Therefore, the strong correlation between grade repetition and school dropout is likely due to grade repeaters and students who drop out of school being affected by the same unobservables.

Table 4.8: The Effect of Grade Repetition and School Dropout (2)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	$\rho = uncons.$	$\rho = 0$	$\rho = 0.1$	$\rho = 0.2$	$\rho = 0.3$	$\rho = 0.4$	$\rho = 0.5$
Repeated	-0.156	0.0867***	0.0492***	0.0114	-0.0269***	-0.0662***	-0.107***
	(0.135)	(0.00768)	(0.00771)	(0.00772)	(0.00772)	(0.00772)	(0.00771)
$\rho = uncons.$	0.615						
	(0.305)						
N	10646	10646	10646	10646	10646	10646	10646

Note: The dependent variable is takes on a value 1 if a student has dropped out of school, and 0 otherwise. Each cell is the average treatment effect of the endogenous regressor from a separate regression for each column value of ρ after controlling for all observables in Table 4.2. Wald test of ρ=0: chi2(1) = 2.13503. Prob > chi2 = 0.1440. Robust standard errors are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Source: Young Lives school survey data from Ethiopia.

4.6 Conclusions

Theoretically, student retention could improve or worsen student outcomes. The rate of grade repetition is quite high in developing countries, particularly in Sub-Saharan Africa, compared to developed countries. However, empirical research on whether grade repetition improves student outcomes in developing countries has been very limited. Furthermore, empirical evidence provides mixed results.

This paper examines the effect of grade repetition on student achievement and the likelihood of school dropout in Ethiopia. Because of omitted ability, which correlates with the likelihood of grade repetition and achievement in test scores, especially in a cross-sectional setting, OLS estimates of the relationship between student achievement and grade repetition overestimate the (absolute value of the) effect of grade repetition, leading to a negative bias. This study tackles this problem by, first, assessing the stability of the OLS estimate by adding a large set of covariates to the model.

Second, an endogenous treatment-regression model is used to simultaneously estimate the treatment and outcome equations recursively, using maximum likelihood, and plausible assumptions are being made regarding the correlation between the unobservables that affect the treatment and the unobservables that affect the potential outcomes. I obtain estimates of the grade repetition effect for different values of the correlation between the unobservable determinants of grade repetition and school dropout for both treated and untreated groups.

Third, I use a recursive bivariate probit model that examines the sensitivity of the estimates to the correlation between the unobserved factors that determine grade repetition and the outcome – school dropout. I obtain estimates of the grade repetition effect for different values of the correlation between the unobservable determinants of grade repetition and school dropout.

The results reveal that grade retention does not improve student performance, especially in maths, and evidence on the improvement in verbal test scores is relatively weak. On average, retained students have about 5 percentage points lower scores in mathematics and language grades compared to those who are being promoted. Moreover, retained students are at high risk of dropping out of school but it appears that this result is driven by the unobservables that affect both grade repetition and school dropout.

The results have strong policy implications especially for developing countries with binding resource constraints such as Ethiopia. There is little evidence in favor of student retention and retained students are likely to drop out of school. These imply that grade repetition is not a cost effective policy decision. The Department of Education of the Government of Ethiopia and other responsible stakeholders should examine this before holding back students. However, one also needs to be careful that automatic grade promotion does not bring unintended effects by creating a disincentive toward studying hard. More research is required to design appropriate policies.

Chapter 5

Conclusion

Substantial empirical evidence exists on the lasting economic impacts of early-life outcomes such as childhood education (cognitive and non-cognitive skills) and health (stunting and malnutrition) (Duflo, 2001; Gould et al., 2011; Kautz et al., 2014; McGovern et al., 2017). This thesis presents three self-contained empirical research papers on childhood human capital accumulation in Ethiopia. In particular, I explore the main potential channels – childhood health and schooling outcomes – through which long-term economic impacts will be realized. The first two studies examine the persistent effect of *early* life exposure to the 1998–2000 Ethiopia–Eritrean conflict on (a) childhood education and (b) childhood health and to what extent ill-health plays a mediating role in reducing education. The third study investigates the effect of student retention (as observed through grade repetition) on mathematics and language test scores and the correlation between grade repetition and school dropout.

The findings from the first two papers suggest that *early* life exposure to the 1998–2000 Ethiopia–Eritrean conflict has detrimental short-term and long-term consequences on human capital accumulation. In particular, the conflict reduced educational achievement of children by about 4 percentage points in mathematics and language test scores 12 years after the conflict. In addition, exposed children were more likely to drop out of school, repeat a grade, and complete fewer grades. Furthermore, exposed children were more likely to have stunted growth. The third paper demonstrates that student retention did not improve learning outcomes and is correlated with school dropout.

Given the prevalence of a large proportion of children under five who are (severely) stunted (CSA and ORF, 2001, 2006; CSA and ICF, 2012) and high rates of grade repetition and school dropout (UNESCO, 2016) in Ethiopia, these findings have important policy implications. First, the evidence from the first two studies imply that interventions that target pregnant mothers and young children at the time of conflict can reduce negative long-term welfare effects at least in terms of education and health outcomes. These may include, among others, physical protection or evacuation of mothers and young children from conflict areas and appropriate psychological interventions to treat post-war trauma.

Second, the evidence from the third study implies that student retention is not a cost effective policy because student retention does not increase test scores. However, one also needs to be careful that automatic grade promotion does not bring unintended effects by creating a disincentive toward studying hard.

There is still much to learn about how conflict affects human capital accumulation across and within countries. The theory is inconclusive and empirical evidence is limited but increasing. In addition, the empirical evidence on the effect of student retention is rather mixed and in particular, in the African context, the evidence is quite limited despite higher occurrence of grade repetition compared to other regions of the world. I hope the research presented in this thesis sheds some new light and provokes further research for scholars in these fields.

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