Does Learning in Mother Tongue Matter? Evidence from a Natural Experiment in Ethiopia

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Abstract

This paper offers empirical evidence on whether learning in mother tongue improves educational outcomes in primary school. We exploit the variation in changes in medium of instruction across schools located in different districts in Ethiopia following the 1994 education reform. This reform has provided opportunity for states in Ethiopia to choose the medium of instruction in primary schools located within their jurisdictions. Since the reform has affected only schools in some districts, but not in others, we assign children into *treatment* and *control* groups depending on whether the medium of instruction in the districts in which children live has changed following the reform. Using data from the 2-percent public-use microdata samples of the 1994 and 2007 Ethiopian population censuses as pre- and post-reform data, respectively, we estimate difference-in-differences models. The results from our preferred model suggest that the 1994 education reform has increased the probabilities of both enrollment in primary school and whether a child attends the "right" grade for her/his age. Falsification tests suggest that our results are not confounded by other factors. This evidence supports the argument that learning in mother tongue improves educational outcomes in primary school.

Keywords: medium of instruction, primary school education, difference-in-differences model, Ethiopia

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1. Introduction

After decades-long effort, developing countries have substantially increased their primary school enrollment rates. However, primary education in many developing countries continues to suffer from students' poor performance, grade repetition, and school dropout. Many factors contribute to students' poor performance, among which, the effect of learning in their mother tongue has received limited attention. It is estimated that about 1.38 billion people in the world speak local languages that are not used for formal education, and an estimated 221 million school-age children in developing countries are speakers of these lesser known or unwritten languages (Dutcher, 2004).

Given that such a large number of school-age children speak languages that are not used as the medium of instruction in schools (and, hence, potentially attend primary schools where the medium of instruction is different from their mother tongue), it is surprising that the effect of learning in mother tongue on performance at school has not yet been thoroughly investigated.

In multilingual countries where settlement is mixed and many language-minority individuals live within the boundaries of states where the official language is different from their mother tongue, many students are forced to learn in their second language in primary school. A large number of countries have taken steps to accommodate language-minority students by adopting mother tongue instruction in primary school.

For instance, as a multilingual country, Ethiopia, which is also the focus of the present study, signed into law the Education and Training Policy in 1994 (Ministry of Education, 1994). The policy document has primarily given discretion to state governments in Ethiopia[2] to choose the language of instruction in primary schools located in their jurisdictions. Following this discretion, many states in Ethiopia have adopted their respective official languages (and, at times, additional minority languages) as the medium of instruction in primary schools. As a result, the medium of instruction in primary schools in the country has increased from using Amharic[3] as the only medium of instruction in 1990 to about 25 languages in 2007 (Seidel and Moritz, 2007).

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

[3]Amharic has been the only official language of the federal government of Ethiopia since the Ethiopian history has been recorded.

The literature on the role of the medium of instruction in educational outcomes in primary school is limited, and the majority of studies on the topic come from bilingual education literature. Moreover, findings from the limited studies on the topic are not conclusive. In Canada, for instance, it is found out that English-speaking students who were taught in French were not at a disadvantage compared to either their French-speaking peers or English-speaking students who were taught in English (Swain and Lapkin, 1982). In the US, on the other hand, it is documented that immigrant students who were taught in their native language performed significantly better than their English-taught peers (Willig, 1985). Similar findings where students who were taught in their second language perform poorly relative to those who were taught in their mother tongue are also documented in the developing world, including, for instance, in Latvia (Ivlevs and King, 2013), Hong Kong (Yip et al., 2003), Cameroon (Gfeller and Robinson, 1998), Ghana (Collison, 1974), and other countries (see, e.g., Trudell (2005); Bamgbose (1991) and Mehrotra (1998)).

These findings, however, should be treated carefully since most of them were conducted on a small number of classes, and hence findings could vary with class-specific factors. Besides, these studies, particularly those from bilingual education literature, did not convincingly disentangle the effect of learning in mother tongue (on educational outcomes) from that of unobservable characteristics. In order to obtain results that document causal relationship and that can be generalized to wider contexts, it is important to mitigate biases from potential endogeneity. Natural experiments, which can be considered as exogenous source of variation, provide an opportunity to employ strong identification strategy and document causal relationships. In this paper, therefore, we employ the 1994 education reform in Ethiopia as a natural experiment in the identification strategy and explore the causal effect of learning in mother tongue on the probabilities of both enrollment in primary school and whether a child attends the "right" grade for her/his age, conditional on enrollment in primary school.

The data used in this paper come from a 2-percent public-use microdata samples of the 1994 and 2007 Ethiopian population censuses. Specifically, the study uses a sub-sample of children who live in Amhara state[4] and its neighboring states of Afar

[4]There are nine states and two chartered cities in Ethiopia. The 2007 Ethiopian population census shows that Amhara state is the second largest state in Ethiopia in terms of population size, with a total population of about 17 million and accounts for about 23 percent of the population in Ethiopia.

and Tigray. This is mainly because Amhara state is the only state in Ethiopia where the 1994 education reform has resulted in differential impact on the change in the medium of instruction across primary schools located within the state's boundary. More specifically, the 1994 education reform has resulted in a change in the medium of instruction in primary schools located in Awi, Oromiya, and Wag Hemra zones of Amhara state from Amharic to Awngi, Oromiffa, and Agew,[5] respectively. Primary schools in other zones of Amhara state, on the other hand, have continued to use Amharic as their medium of instruction after the 1994 education reform.

The differential impact of the 1994 education reform on the change in the medium of instruction across primary schools located in different zones in Amhara state enables us to use a difference-in-differences approach as an identification strategy where we assign children in Awi, Oromiya, and Wag Hemra zones of Amhara state into *treatment* group and children in other zones of Amhara state into *control* group.

Similarly, the reform offers an opportunity to conduct between-states analysis since it has led to a change in the medium of instruction in primary schools in the neighboring states of Afar and Tigray from Amharic to Afarigna and Tigrigna, respectively. Thus, in the part of the analysis that exploits between-states variations, we assign children in the neighboring states of Afar and Tigray into *treatment* group and children in Amhara state (after dropping those in Awi, Oromiya, and Wag Hemra zones of Amhara state) into *control* group.

Using data from the 2-percent public-use microdata samples of the 1994 and 2007 Ethiopian population censuses as pre- and post-reform data, respectively, we estimate difference-in-differences models and examine the causal effect of learning in mother tongue on the probabilities of both enrollment in primary school and whether a child attends the "right" grade for her/his age, conditional on enrollment. The results from our preferred (difference-in-differences) model suggest that the 1994 education reform in Ethiopia, which has led to a change in the medium of instruction in primary school to mother tongue instruction, has increased the probability of enrollment in primary school by 8.4 percentage points in within-state analysis and by 2.2 and 3.6 percentage points in between-states analysis. Similarly, our results suggest that the education reform has increased the probability of a child attending the "right" grade for her/his age by 9.8 percentage points in within-state analysis and by 0.5 and

[5] Agew, Awngi, and Oromiffa are the native languages of the people from Agew, Agew-Awi, and Ormo ethnic groups, respectively.

2.6 percentage points in between-states analysis. Falsification tests suggest that our results are not confounded by other factors. This evidence supports the argument that learning in mother tongue improves educational outcomes in primary school.

The remainder of the paper is organized as follows. The following section briefly reviews prior research on the role of the medium of instruction in educational outcomes in primary school, and Section 3 discusses the education reform in Ethiopia. Section 4 describes the data, while Section 5 discusses the identification strategy, outlines the empirical approach, and presents the econometric results. The final section concludes the study.

2. Medium of Instruction and Performance in Primary School

Most of the previous works on the role of medium of instruction in educational outcomes in primary school come from bilingual education literature. The majority of these studies were conducted in African and Asian countries, perhaps because post-colonial-era policymakers in these countries have shown interest in changing the medium of instruction, at least in primary schools, from former colonial languages to indigenous languages.

In addition to helping to maintain cultural and linguistic diversity and motivating children from marginalized groups to attend school (see, e.g., Cummins, 1999), learning in mother tongue reduces the pressure on students by relieving them from learning both second language and subject contents simultaneously. Evidence suggests that children who are taught to read in their mother tongue first learn to read in their second language faster than those who are first taught to read in their second language (see, e.g., Cummins (2000) and Brock-Utne (2007)).

Mother tongue instruction in primary school, moreover, has a psychological advantage since it facilitates adjustment between home and school and does not alienate children from their identity (Trudell, 2005). The psychological effect on students' performance plays a more important role in primary school where the need for adjustment to the school environment is of paramount importance to learn subject contents effectively.

However, adopting mother tongue instruction is complicated and costly. For instance, using a dominant second language of wider communication (such as English in Kenya) as the language of instruction in primary school has a comparative cost advantage over mother tongue instruction (see, e.g., Clayton (1998) and Harlech-

Jones (1998)). Perhaps this explains why, immediately after most formerly colonized countries attained their independence, cost consideration was at the center of the debate as to whether to continue to use the former colonial language or adopt the indigenous language as the medium of instruction.[6]

Furthermore, learning in mother tongue may negatively affect proficiency in national and international languages (see, e.g., Angrist and Lavy, 1997), which are typically the languages of instruction in colleges. This could, in turn, negatively affect their labor market outcomes later in life (see, e.g., Angrist and Lavy, 1997), and, hence, hamper successful career development and upward social mobility.

When we look closely at empirical evidence from bilingual education literature, it suggests that teaching students in a language that neither students nor teachers understand and use well enough is one of the major reasons for students' poor performance in primary school in developing countries. It has been documented, for instance, that learning in a second language decreases students' participation in the classroom (Trudell, 2005) and their overall performance in primary school (Bamgbose, 1991), and increases the probability of them dropping out of school (Mehrotra, 1998).

Prior case studies from classroom experiments from bilingual education literature have documented a strong positive correlation between learning in mother tongue and mastering subject contents in primary school (see, e.g., Brock-Utne (2007) and Bunyi (1999)). Though these case studies provide helpful insights, it is not plausible to argue that their findings suggest a causal relationship. This is mainly because of their methodological limitations. First, they are conducted on an extremely small number of classrooms, and, hence, the findings could vary with classroom-specific factors.

Second, and perhaps the most serious limitation, is that the research projects themselves have facilitated (and funded) the translation of textbooks to additional languages (see, e.g., Brock-Utne, 2007) so that researchers would be able to assign classrooms into treatment and control groups. However, this type of textbook translation could bias the results since research projects are not well suited to conducting this specific task relative to, say, relevant government offices such as ministries of ed-

[6]See, e.g., Thorburn (1971) and Jernudd (1971) for the cost-benefit analysis of using the (colonial) language of wider communication versus the indigenous language as the medium of instruction in developing countries.

ucation. A related problem is the short time span between the time of translation of textbooks and the time at which students' performance is measured, which, in most cases, is only a few weeks apart from each other. This leaves no time for students (and teachers) to adjust to the change in the medium of instruction and can only capture, at best, an extremely short term effect of the change in the medium of instruction on students' performance.

On the other hand, prior research from the economics literature has focused on exploring the effect of immigrants' proficiency in the languages of destination countries on immigrants' labor market outcomes (see, e.g., Chiswick (1978); Dustmann and Van Soest (2001); Carliner (1981); Chiswick and Repetto (2000); Bleakley and Chin (2004); and Dustmann and Fabbri (2003)).

The study by Ivlevs and King (2013) is the most closely related to our study. They explore the effect of a change in medium of instruction in Latvian schools from minority language (i.e., predominantly Russian) to majority language (i.e., Latvian). Exploiting the education reform in their identification strategy, they found that students' performance levels on secondary-school exams have decreased significantly after the education reform for students who attended minority schools relative to the performance of those in majority schools.

As interesting as it is, the study by Ivlevs and King (2013) differs from the present study in a number of ways. To mention some: first, our study is conducted in a typical low income country in Africa where primary school enrollment, school dropout rate, and grade repetition are still major challenges. Moreover, the present study focuses on educational outcomes in primary school.

Second, unlike the reform in Latvia, which has legislated for minority schools to change the medium of instruction from a minority language (i.e., predominantly Russian) to a composite of 60 percent Latvian and 40 percent minority language, the 1994 education reform in Ethiopia has led to the complete immersion of language-minority students into instruction in the minority language. This, in turn, has resulted in learning in mother tongue for a great majority of language-minority students. The unique nature of the 1994 education reform in Ethiopia, along with Ethiopia's distinct economic, social, and institutional settings, provide an opportunity to better identify the causal effect of learning in mother tongue on educational outcomes in primary school.

We are not aware of similar studies conducted in Ethiopia and much of Sub-

Saharan Africa that convincingly document the causal effect of learning in mother tongue on educational outcomes in primary school. This paper, therefore, builds on the medium-of-instruction literature that has been extensively discussed in bilingual education literature. The primary contribution of this paper, however, is that its stronger identification strategy helps us shed some light on the *causal* effect of learning in mother tongue on educational outcomes in primary school in a typical developing country. Documenting causality is particularly possible in this paper because we exploit the variation in changes in the medium of instruction across schools located in different districts in Ethiopia following the 1994 education reform.

3. The Education Reform in Ethiopia

The Ethiopian education sector has gained the due attention of the government since the change in government in May 1991. Consequently, the sector has undergone many policy changes and received a large and increasing budget share of the government. Among the many changes the sector has experienced in the 1990s, the change in the medium of instruction in public primary schools is the major one.

The Ethiopian government signed into law its Education and Training Policy in 1994. Among other things, the policy document has provided opportunity for states in Ethiopia to choose the medium of instruction in primary schools located in their jurisdictions (Ministry of Education, 1994). Prior to this policy change, all students in primary schools in Ethiopia were taught in Amharic, the only official language of the country. After the policy change, states have had the option to either use the country's official language or their own respective official state languages (or as many languages as states choose to use) as the medium of instruction in primary schools. As a result, the medium of instruction in primary schools in the country has increased from using Amharic as the only medium of instruction in 1990 to approximately 25 languages in 2007 (Seidel and Moritz, 2007).

The Impact of the Education Reform on Medium of Instruction in Schools in Ethiopia

As mentioned earlier, the education reform has resulted in a change in the medium of instruction in primary schools in *all* states in Ethiopia from Amharic to (at least) the states' respective official languages. Even if Ethiopia is a multiethnic/multilingual country with more than 90 languages being spoken in the country (Bamgbose, 1991), the diversity within states varies tremendously. For instance, some

states (e.g., Afar, Amhara, and Tigray) are homogenous where up to 90 percent of the population are from one ethnic group (see the 2007 Ethiopian population census report for details) and speak a common language whereas other states are extremely diverse in terms of their ethnic/language compositions. For instance, a large number of ethnic groups live within the boundary of the Southern Nations, Nationalities, and People's (SNNP) state. People from Sidama, the dominant ethnic group in SNNP state, account for only about 19 percent of the population therein (see the 2007 Ethiopian population census report for details), and it has been documented that more than 56 languages are spoken in SNNP state (Heugh et al., 2007).

For the reason that states in Ethiopia vary widely in their ethnic composition and language diversity, the 1994 education reform has given states the discretion to choose not only the language of instruction in primary schools but also to choose as many languages as they want as media of instruction as they see fit for the states' specific circumstances. As a result, all states have adopted their respective official languages (and, at times, additional minority languages) as media of instruction in primary schools. Note that Amharic was the only medium of instruction in all primary schools in the country before the 1994 education reform. Thus, schools in all states except most schools in Amhara state have changed their medium of instruction in primary schools from Amharic to other languages.

The Impact of the Education Reform on Medium of Instruction in Schools in Amhara State

Most schools in Amhara state have continued to use Amharic as the medium of instruction after the 1994 education reform. This is mainly because the majority of residents in Amhara state are from the Amhara ethnic group and, hence, their mother tongue is Amharic. In fact, Amharic is the official language of Amhara state – and the federal government of Ethiopia. Of course, there are also minority groups in Amhara state. The three main minority groups are Agew, Agew-Awi, and Oromo ethnic groups who predominantly reside in Wag Hemra, Awi, and Oromiya zones of Amhara state, respectively. As a result, primary schools in Wag Hemra, Awi, and Oromiya zones have changed their media of instruction from Amharic to Agew, Awngi, and Oromiffa, respectively.

The fact that there are three main minority groups in Amhara state has resulted in a differential impact of the 1994 education reform on the change in the medium of instruction across schools in Amhara state. More specifically, the 1994

education reform has resulted in a change in the medium of instruction in primary schools in Awi, Oromiya, and Wag Hemra zones of Amhara state. In contrast, primary schools in other zones of Amhara state have continued to use Amharic as their medium of instruction even after the 1994 education reform. This provides a natural experiment to explore the causal effect of learning in mother tongue on educational outcomes in primary school.

The Education Reform as an Exogenous Source of Variation

As discussed earlier, the 1994 education reform has had a differential impact on the change in the medium of instruction across districts in Ethiopia. This, coupled with data from the 2-percent public-use microdata samples of the 1994 and 2007 Ethiopian population censuses (see Section 4 for information on data used in this paper), enables us to identify the causal effect of learning in mother tongue on educational outcomes in primary school using the difference-in-differences approach. In the difference-in-differences approach, we exploit both within- and between-states comparisons as an identification strategy.

We assign children in Awi, Oromiya, and Wag Hemra zones of Amhara state into *treatment* group and children in other zones of Amhara state into *control* group to exploit within-Amhara-state variation in the difference-in-differences model. Similarly, we assign children in Amhara state (after dropping those in Awi, Oromiya, and Wag Hemra zones) into *control* group and those in the neighboring states of Afar and Tigray into *treatment* group in the part of the analysis where we exploit between-states variation as an alternative source of variation. Then, using data from the 2-percent public-use microdata samples of the 1994 and 2007 Ethiopian population censuses as pre- and post-reform data, respectively, we estimate difference-in-differences models to identify the causal effect of learning in mother tongue on educational outcomes in primary school.

Table 1 summarizes the differential impact of the 1994 education reform on the change in the medium of instruction across zones in Amhara state and the neighboring states of Afar and Tigray. Moreover, the upper panel of Table 1 depicts how zones in Amhara state are assigned into treatment and control groups in the (within-Amhara-state) difference-in-differences model. The lower panel of Table 1, on the other hand, depicts how Amhara state and its neighboring states of Afar and Tigray are assigned into control and treatment groups when we exploit between-states variation in the

Table 1: Medium of Instruction in Amhara and its Neighboring States of Afar and Tigray by Year and Treatment Status

	Medium of Instruction		Treated Group?
Within-Amhara-state Comparison	*1994*	*2007*	
Bahir Dar Special Zone	Amharic	Amharic	No
North Gonder Zone	Amharic	Amharic	No
South Gonder Zone	Amharic	Amharic	No
North Wollo Zone	Amharic	Amharic	No
South Wollo Zone	Amharic	Amharic	No
North Shewa Zone	Amharic	Amharic	No
East Gojam Zone	Amharic	Amharic	No
West Gojam Zone	Amharic	Amharic	No
Wag Hemra Zone	Amharic	Agew	Yes
Awi Zone	Amharic	Awngi	Yes
Oromiya Zone	Amharic	Oromiffa	Yes
Between-states Comparison	*1994*	*2007*	
Amhara State (excluding Wag Hemra, Awi, and Oromiya zones)	Amharic	Amharic	No
Afar State	Amharic	Afarigna	Yes
Tigray State	Amharic	Tigrigna	Yes

Notes: Ethiopia is a federal country with three levels of government: federal, state (or regional), and local. Zones are local governments that are equivalent to US counties.

difference-in-differences model.[7]

4. Data

The data used in this paper come from the two most recent Ethiopian population censuses that were administered by the Central Statistical Agency (CSA) of Ethiopia. Specifically, data from the 2-percent public-use microdata samples of the 1994 and 2007 censuses are used.

For population censuses, CSA administers two types of questionnaire: short-form and long-form. As the names imply, the main difference between short-form and long-form questionnaires is the number of questions included. The questions included in the short-form questionnaire are designed to capture information on basic household demographic characteristics such as gender, age, and mother tongue.[8]

The long-form questionnaire, on the other hand, contains all the questions included in the short-form questionnaire and some additional questions. These additional questions help gather information on disability, education, economic activity, migration, fertility, mortality, and housing conditions.

The long-form questionnaires are administered to one-in-five random samples of Ethiopian households and are used as sources of the Ethiopian census microfiles. CSA randomly selects 10 percent of the long-form-questionnaire respondents and makes their information available to the public. Thus, the total number of observations in the public-use microdata sample files constitute 2% (i.e., 20%*10%) of Ethiopian households. This gives us about 1 million and 1.3 million observations in the 2-percent public-use microdata samples of the 1994 and 2007 censuses, respectively.

In this paper, however, we have restricted the sample of analysis to households found in Amhara state and its neighboring states of Afar and Tigray. This is primarily because it is only in Amhara state that the 1994 education reform has had a differential impact on the change in medium of instruction, where some schools have continued to use Amharic as the medium of instruction (and, hence, are assigned

[7] See Section 5 for further details on the identification strategy used in this paper.

[8] Using public-use microdata samples of the censuses has an important advantage compared to using other household survey data from Ethiopia, particularly because information on mother tongue language is collected in the censuses. This enables us to use information on mother tongue language to develop an identification strategy that exploits the differential impacts of the 1994 education reform across language groups.

into *control* group) whereas other schools have changed the medium of instruction from Amharic to Agew, Awngi, and Oromiffa (and, hence, are assigned into *treatment* group). In a complementary analysis where we exploit between-states variation as an alternative source of variation, we compare educational outcomes between children in Amhara state and the neighboring states of Afar and Tigray – see Section 5 for details on the identification strategy used in this paper.

Table 2 presents descriptive statistics for a sample of children used in the econometric analysis, i.e., school-age children who lived in Amhara state and its neighboring states of Afar and Tigray in 1994 and 2007. The table shows that, on average, about 46 percent of school-age children in the control zones of Amhara state were enrolled in primary school. This enrollment rate is comparable to what has been observed in the treated group where it was averaged at 44.5, 42.3, and 45.8 percent in the treated zones of Amhara, Afar, and Tigray states, respectively.

Table 2, on the other hand, shows that about 26.1 percent of students in the control zones of Amhara state were enrolled in the "right" grade for their age.[9] This figure is 24.3, 25.6, and 19.9 percent in the treated zones of Amhara, Afar, and Tigray states, respectively.

Generally speaking, parents in our sample are less educated, with fathers and mothers having completed approximately 2 and 1.5 years of schooling, respectively. When we look at the housing conditions of families in Amhara state and its neighboring states of Afar and Tigray, Table 2 shows, in the household assets and amenities variable, that families in general live in poor housing conditions. Finally, Table 2 shows that the unemployment rate in Amhara state is relatively lower than that in Afar and Tigray states.

5. The Impact of the Education Reform on Selected Educational Outcomes

To identify the average treatment effect of the 1994 education reform (and, hence, the change in the medium of instruction to mother tongue instruction) on educational outcomes in primary school, we exploit two different sources of variation. First, we exploit the variation in educational outcomes across zones in Amhara

[9]In Ethiopia, the official school starting age is 7. Thus, the students' age-for-grade (i.e., whether a student is attending the "right" grade for her/his age) dummy takes a value of 1 if, for example, a 7-year old student is currently enrolled in grade 1, and 0 otherwise. Similarly, it takes a value of 1 if an 8-year old student is currently enrolled in grade 2, and 0 otherwise, and so on. This indicator variable is used as one of the educational outcome variables in the econometric analysis.

Table 2: Summary Statistics of Variables used in the Econometric Analysis by State and Treatment Status

	Amhara		Afar	Tigray
	Control	Treated	Treated	Treated
Enrollment dummy	0.460	0.445	0.423	0.458
	(0.485)	(0.475)	(0.494)	(0.500)
Student's age-for-grade dummy	0.261	0.243	0.256	0.199
	(0.198)	(0.196)	(0.203)	(0.189)
Female headed hh dummy	0.052	0.051	0.036	0.075
	(0.358)	(0.352)	(0.389)	(0.415)
Father's years of schooling	2.149	2.179	1.744	2.372
	(2.762)	(2.781)	(2.087)	(2.004)
Mother's years of schooling	1.359	1.546	0.996	1.651
	(1.718)	(1.793)	(1.778)	(0.091)
Household size	5.623	6.396	5.703	5.583
	(1.672)	(2.009)	(1.346)	(2.982)
Dummy for hh's ownership of pit latrine	0.145	0.197	0.077	0.157
	(0.462)	(0.470)	(0.434)	(0.478)
Dummy for hh's ownership of piped water	0.153	0.165	0.117	0.288
	(0.459)	(0.451)	(0.499)	(0.497)
Dummy for hh's ownership of radio	0.193	0.178	0.279	0.244
	(0.477)	(0.468)	(0.467)	(0.494)
Proportion of hhs with pit latrine	0.147	0.198	0.069	0.158
	(0.159)	(0.271)	(0.387)	(0.279)
Proportion of hhs with piped water	0.155	0.167	0.146	0.289
	(0.129)	(0.286)	(0.346)	(0.100)
Proportion of hhs with radio	0.194	0.179	0.284	0.248
	(0.199)	(0.329)	(0.228)	(0.176)
Unemployment rate	1.551	1.359	5.625	3.783
	(1.376)	(0.567)	(2.710)	(3.659)
Observations	83078	4945	16581	39294

Notes: Standard deviations are reported in parentheses.
Proportion of households is defined over the locality of the child's residence, which is roughly equivalent to a village or urban neighborhood.

state. Among zones in Amhara state, only schools in Awi, Oromiya, and Wag Hemra zones have changed the medium of instruction in primary school following the 1994 education reform. Hence, if the reform has had a positive effect, we would expect improvements in educational outcomes in schools in Awi, Oromiya, and Wag Hemra zones (relative to those found in other zones in Amhara state).

Second, we exploit alternative sources of variation by comparing educational outcomes between schools in Amhara state (after dropping those in Awi, Oromiya, and Wag Hemra zones)[10] with those found in the neighboring states of Afar and Tigray. Primary schools in these comparison states have changed their medium of instruction following the 1994 education reform, but schools in Amhara state (except those in Awi, Oromiya, and Wag Hemra zones) have continued to use Amharic as the medium of instruction. We would expect improvements in educational outcomes in schools in the comparison states (relative to those found in Amhara state) if the reform has had a positive effect.

The comparison states were chosen based on their geographic proximity to Amhara state[11] and because the population in each comparison state is homogenous as the population in Amhara state is. In this specification, the estimates are robust to shocks specific to Amhara state.

5.1. Within-Amhara-state Analysis

As mentioned earlier, the 1994 education reform has resulted in a change in the medium of instruction in primary schools in Awi, Oromiya, and Wag Hemra zones of Amhara state from Amharic to Awngi, Oromiffa, and Agew, respectively. For schools found in other zones in Amhara state, however, the medium of instruction in primary school has continued to be Amharic after the 1994 education reform.

If we are able to observe the same set of children when they are exposed to the education reform and when they are not, the average treatment effect of the reform would be the differences in expected values under the two scenarios. Since it is virtually impossible to observe the same set of children under both scenarios, the average treatment effect can only be obtained if we have data on two groups of randomly assigned children where one group is exposed to the reform while the other

[10] In this part of the analysis, we drop Awi, Oromiya, and Wag Hemra zones of Amhara state from the analysis because, unlike primary schools in other zones in Amhara state, primary schools in these zones have changed the medium of instruction following the 1994 education reform.

[11] Both Afar and Tigray states share a border with Amhara state.

is not. As it is reasonable to assume that the 1994 education reform is exogenous to household behavior, we assign children in Awi, Oromiya, and Wag Hemra zones of Amhara state into *treatment* group and children in other zones in Amhara state into *control* group.

Even under the assumption that children's assignment into treatment and control groups is random, there is a need to control for pre-existing differences between the two groups since children in the two groups may differ initially and have different educational outcomes even in the absence of the reform. Since we observe children both before and after the reform, we control for pre-existing differences in the difference-in-differences regression framework, which takes the form

$$Pr(y_{izt} = 1) = G(\alpha_0 + \eta_0 Treated_z + \tau_0 After_{it} + \gamma_0 (Treated_z * After_{it}) + \boldsymbol{\beta}_1 \mathbf{X}_{it}), \tag{1}$$

where y_{izt} denotes educational outcomes[12] of child i who lives in zone z in year t; $Treated_z$ is a binary indicator for zones in Amhara state that have changed the medium of instruction in primary schools after the 1994 education reform (i.e., Awi, Oromiya, and Wag Hemra zones); $After_{it}$ is a dummy variable taking 1 if the child is being observed after the reform (i.e., in the 2007 data), and 0 otherwise (i.e., in the 1994 data); $\mathbf{X}_{it}$ is a vector of individual, household, and community-level characteristics.

The primary (explanatory) variable of interest is the interaction term, "$Treated_z * After_{it}$," and γ_0 captures the treatment effect, i.e., the effect on the probabilities of educational outcomes of a child due to the child living in the treated zones (relative to those that live in the control zones) after the 1994 education reform.

While estimating Equation (1), the standard errors are clustered by enumeration area, a census tract, to account for correlation in the error terms within the enumeration area over time. For the most part, we assume that G is the standard normal cumulative distribution function and estimate a probit model, in which case the average marginal effect of the interaction term and its standard error are computed as suggested by Ai and Norton (2003).

[12] Two educational outcomes are considered in this paper: probabilities of both enrollment in primary school and whether a student attends the "right" grade for her/his age.

Enrollment in Primary School

The results from the difference-in-differences estimates are reported in Table 3, where the dependent variable is a binary indicator for whether a primary school-age (i.e., 7-14 year old) child is enrolled in primary school.[13] The first three columns report results from the Linear Probability Model (LPM) whereas the last three columns are from the probit model. In columns 1 and 4 of Table 3, we report the *unadjusted* effect of the 1994 education reform on the probability of enrollment in primary school.

The coefficient estimates of the "$Treated$" dummy variable in columns 1 and 4 of Table 3 are negative and statistically significant at 1 percent level in both the LPM and probit model, suggesting pre-existing differences in the probability of enrollment in primary school between children in the treated and control zones before the 1994 education reform. That is, children who lived in Awi, Oromiya, and Wag Hemra zones of Amhara state before the 1994 education reform were less likely to enroll in primary school relative to those who lived in other zones in Amhara state.

The results reported in columns 1 and 4 of Table 3 also show that there is a general upward trend in enrollment in primary school over time among children both in the treated and control zones as revealed by the positive and statistically significant coefficient estimates of the "$After$" variable. This is not surprising considering the government has built a large number of primary schools throughout the country in the last two decades with the objective of achieving universal primary school enrollment by 2015.

The primary (explanatory) variable of interest, which captures the effect of the education reform (and, hence, the effect of learning in mother tongue) on the probability of enrollment in primary school, is the interaction term between the "$Treated$" and "$After$" variables, i.e., "$Treated * After$." In columns 1 and 4 of Table 3, the coefficient estimate of the interaction term is 0.129 in the LPM and 0.159 in the probit model, and both are statistically significant at 1 percent level. This suggests that enrollment in primary school among children who live in Awi, Oromiya, and Wag Hemra zones of Amhara state has increased, on average, by between 12.9 and 15.9 percentage points relative to children who live in other zones in Amhara state.

Note that we have controlled for pre-existing differences in the probability of enrollment in primary school between children in the treated and control zones.

[13]The complete regression results from models presented in Tables 3, 4, 5, 6, 7, 8, and 9 are reported in the supplementary appendix.

Table 3: **Within-state Analysis:** Difference-in-Differences Estimates of the Effect of a Change in Medium of Instruction on Enrollment in Primary School
Dependent Variable: Binary Indicator for Enrollment in Primary School

	LPM			Probit Model		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.033***	-0.061***	-0.074***	-0.045***	-0.071***	-0.078***
	(0.006)	(0.013)	(0.013)	(0.007)	(0.016)	(0.016)
After	0.721***	0.689***	0.690***	0.462***	0.434***	0.435***
	(0.002)	(0.014)	(0.010)	(0.001)	(0.010)	(0.008)
Treated*After	0.129***	0.070***	0.082***	0.159***	0.079***	0.084***
	(0.030)	(0.017)	(0.017)	(0.030)	(0.019)	(0.019)
Unemployment rate			0.031***			0.022***
			(0.004)			(0.002)
Constant	0.101***	0.185***	0.174***			
	(0.002)	(0.018)	(0.017)			
Individual characteristics	No	Yes	Yes	No	Yes	Yes
Household characteristics	No	Yes	Yes	No	Yes	Yes
Locality characteristics	No	Yes	Yes	No	Yes	Yes
Observations	88023	88023	88023	88023	88023	88023
R^2	0.530	0.532	0.537			

Notes: *p < 0.10, ** p < 0.05, *** p < 0.01.
Reported coefficients are average marginal effects.
Robust standard errors are clustered by enumeration area, a census tract, and are reported in parentheses.
The regression controls are individual-level characteristics (i.e., mother's and father's years of schooling and a binary indicator for gender of household head), household-level characteristics (i.e., household size, binary indicators for whether a household has piped water, pit latrine, and radio), and locality-level characteristics (i.e., proportion of households with piped water, pit latrine, and radio).

Furthermore, we have controlled for the trend in the probability of enrollment in primary school over time. Hence, the increase in the probability of enrollment in primary school among children who live in the treated zones (i.e., Awi, Oromiya, and Wag Hemra zones) relative to those who live in the control zones (i.e., other zones in Amhara state) is attributed to the fact that children in the treated zones have started learning in their mother tongue after the 1994 education reform.

The results from the difference-in-differences models presented in columns 1 and 4 of Table 3 can be interpreted as the causal effect of the 1994 education reform under the assumption that in the absence of the education reform the change in the probability of enrollment in primary school would not have been systematically different in treated and control zones. If this assumption is not satisfied, the difference in differences presented here cannot be interpreted as the "true" treatment effect. In columns 2 and 5, therefore, we present results from the difference-in-differences models that adjust for observable differences between individuals in the treated and control zones in the regression framework.

As shown in columns 2 and 5 of Table 3, controlling for observable characteristics does not affect the sign and significance level of the coefficient estimates of the "$Treated * After$" variable, but their magnitudes have decreased when we control for observable characteristics. This again implies that the 1994 education reform has increased the probability of enrollment in primary school, an evidence of positive effect of learning in mother tongue on the probability of enrollment in primary school.

One of the identifying assumptions of the difference-in-differences approach is that there was no differential economic growth rate between the treated and control zones during the period of analysis. If this assumption is violated, the difference-in-differences estimates confound the effect of the 1994 education reform with the effect of differential economic growth rates on the probability of enrollment in primary school, which would have been observed even in the absence of the 1994 education reform. Therefore, it is crucial to account for potential differences in economic growth rates between treated and control zones in the regression framework.

In columns 3 and 6 of Table 3, therefore, we present results from the difference-in-differences estimates that also control for differences in economic growth rates; here, we employ the unemployment rate to proxy the economic growth rate. Again, the coefficient estimates of the interaction term, "$Treated * After$," have the same sign and level of significance as before, but their magnitudes are slightly different.

This implies that the reported positive effect of the 1994 education reform on the probability of enrollment in primary school is not driven by differences in economic growth rates between the treated and control zones during the period of analysis. Rather, it is because children in the treated zone have learned in their mother tongue after the 1994 education reform.

When we look at the coefficient estimate of the "*unemployment rate*" variable in columns 3 and 6 of Table 3, it is positive and statistically significant. This suggests that the probability of enrollment in primary school increases with unemployment rate. This is not consistent with our expectation since unemployment rate is used as a proxy variable for economic growth rate. It is not clear why the sign of the coefficient estimate of "*unemployment rate*" is not what we would normally expect. However, the positive effect of unemployment on the probability of enrollment in primary school could be due to the fact that higher levels of unemployment among adults may free up children's child-labor obligations – which, at times, substitute for adult labor – and this, in turn, increases the probability of enrollment in primary school among school-age children.[14]

Student's Age for Grade

Even if we do not observe school performance measures such as test scores, we do observe both a student's age and the grade the student is currently attending. This allows us to use the student's age for grade to measure performance at school (see, for example, Horowitz and Souza, 2011, for a study that used a similar variable to measure performance at school). In the current study, the student's age for grade is a binary indicator taking a value of 1 if the student attends the "right" grade for her/his age, and 0 otherwise.[15]

Since delaying enrollment in primary school beyond the legal school starting age is common in Ethiopia,[16] the value taken by the binary indicator for the student's age for grade is affected by both the timing of enrollment in primary school and the rate at which the student progresses through grades in primary school. As a result,

[14]It has been documented that children with lower child-labor obligation spent more hours on school (see, e.g., Seid and Gurmu, 2015).

[15]See footnote 9 in the Data section for how the binary indicator for whether a student attends the "right" grade for her/his age is defined.

[16]Evidence suggests that children in developing countries, including those in Ethiopia, delay primary school enrollment by a few years beyond the legal school starting age (e.g., see, Barro and Lee, 2001).

the analysis here documents the effect of the 1994 education reform on both the timing of enrollment in primary school and the rate of grade progression.

Given the data we have, identifying the separate effects of the 1994 education reform on the timing of enrollment in primary school and the rate of grade progression is impossible. However, understanding the effect of the 1994 education reform on both delayed enrollment and the rate of grade progression is crucial since both delayed enrollment and grade repetition are common problems in primary education in Ethiopia.

Table 4 presents the results from the difference-in-differences models where the dependent variable is a binary indicator for whether a child attends the "right" grade for her/his age, conditional on enrollment in primary school. In Table 4, the first three columns present results from LPM whereas the last three columns present results from the probit models.

In each specification presented in Table 4, the coefficient estimate of the "$Treated$" variable is negative and significant, suggesting that students in the treated zones were less likely to attend the "right" grade for their age (relative to those who lived in control zones) prior to the 1994 education reform.

On the other hand, the coefficient estimate of the interaction term, i.e., "$Treated*After$," is uniformly positive and significant in all specifications. In our preferred specification, for instance, the coefficient estimate of the interaction term is 0.098, implying that the 1994 education reform has increased the probability that students attend the "right" grade for their age by about 9.8 percentage points. This can be interpreted as that the education reform (and, hence, the change in the medium of instruction in primary school to mother tongue instruction) has increased the probability of on-time enrollment in primary school or the rate at which students progress through grades in primary school or both.

5.2. Robustness to Alternative Sources of Variation

In this subsection, we extend the previous analysis by comparing educational outcomes in Amhara state (after dropping Awi, Oromiya, and Wag Hemra zones) with that of Afar and Tigray states. As mentioned earlier, the comparison states are primarily chosen based on their geographic proximity to Amhara state, and because the population in each comparison state is homogenous as the population in Amhara state is. Exploiting variations in educational outcomes between Amhara state and

Table 4: **Within-state Analysis:** Difference-in-Differences Estimates of the Effect of a Change in Medium of Instruction on Student's Age for Grade
Dependent Variable: Binary Indicator for whether a Student Attends the "Right" Grade for Her/His Age

	LPM			Probit Model		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.231***	-0.028***	-0.034***	-0.126***	-0.057***	-0.045***
	(0.021)	(0.004)	(0.003)	(0.016)	(0.020)	(0.009)
After	0.332***	0.176***	0.171***	0.238***	0.126***	0.121***
	(0.010)	(0.021)	(0.020)	(0.008)	(0.012)	(0.018)
Treated*After	0.121***	0.073***	0.064***	0.136***	0.111***	0.098***
	(0.022)	(0.017)	(0.019)	(0.017)	(0.020)	(0.022)
Unemployment rate			0.009***			0.006***
			(0.001)			(0.001)
Constant	0.244***	0.310***	0.304***			
	(0.004)	(0.023)	(0.023)			
Individual characteristics	No	Yes	Yes	No	Yes	Yes
Household characteristics	No	Yes	Yes	No	Yes	Yes
Locality characteristics	No	Yes	Yes	No	Yes	Yes
Observations	43297	43297	43297	43297	43297	43297
R^2	0.316	0.324	0.330			

Notes: *p < 0.10, ** p < 0.05, *** p < 0.01.
Reported coefficients are average marginal effects.
Robust standard errors are clustered by enumeration area, a census tract, and are reported in parentheses.
The regression controls are individual-level characteristics (i.e., mother's and father's years of schooling and a binary indicator for gender of household head), household-level characteristics (i.e., household size, binary indicators for whether a household has piped water, pit latrine, and radio), and locality-level characteristics (i.e., proportion of households with piped water, pit latrine, and radio).

the neighboring comparison states would allow us to control for shocks specific to Amhara state.

Here, we assign children in Amhara state into *control* group and children in Afar and Tigray states into *treatment* group. We model the educational outcomes of child i who lives in state s in year t, y_{ist}, as:

$$Pr(y_{ist} = 1) = G(\alpha_0 + \eta_0 Treated_s + \tau_0 After_{it} + \gamma_0 (Treated_s * After_{it}) + \boldsymbol{\beta}_1 \mathbf{X}_{it}), \tag{2}$$

where $Treated_s$ is a binary indicator taking 1 for the treated states (i.e., Afar and Tigray states), and 0 otherwise (i.e., Amhara state); and all the other variables and notations are as defined above in Equation (1).

Enrollment in Primary School

Table 5 is the counterpart of Table 3 where the dependent variable and all control variables are exactly the same. However, in Table 5, the comparison is between Amhara state and the neighboring states of Afar and Tigray whereas in Table 3 the comparison is between zones in Amhara state.

The between-states comparisons, which are presented in Table 5, are reported separately for Afar and Tigray states where the results from the comparison between Amhara and Afar states are reported in columns 1 and 2 whereas the results from the comparison between Amhara and Tigray states are reported in columns 3 and 4.

Again, the results from the difference-in-differences model, which are presented in Table 5, confirm what has been reported in Table 3. Specifically, children in the treated states of Afar and Tigray were less likely to enroll in primary school relative to those in the control zones of Amhara state prior to the 1994 education reform as revealed by the negative and statistically significant coefficient estimate of the "$Treated$" variable.

The coefficient estimates of the variable that captures the effect of the 1994 education reform on the probability of enrollment in primary school, "$Treated * After$," is positive and statistically significant at 1 percent level, suggesting that the between-states comparison also confirms the positive effect of the 1994 education reform on enrollment in primary school. Specifically, the results from our preferred model suggest that learning in mother tongue in primary school has increased the probability of enrollment in primary school by 2.2 percentage points in Afar state

Table 5: **Between-states Analysis:** Difference-in-Differences Probit Estimates of the Effect of a Change in Medium of Instruction on Enrollment in Primary School
Dependent Variable: Binary Indicator for Enrollment in Primary School

	Treated State			
	Afar		Tigray	
	(1)	(2)	(3)	(4)
Treated	-0.066***	-0.171***	-0.130***	-0.098***
	(0.019)	(0.021)	(0.003)	(0.012)
After	0.460***	0.421***	0.487***	0.449***
	(0.001)	(0.007)	(0.001)	(0.007)
Treated*After	0.054**	0.022***	0.093***	0.036***
	(0.021)	(0.002)	(0.005)	(0.004)
Unemployment rate		0.014***		0.025***
		(0.003)		(0.002)
Individual characteristics	No	Yes	No	Yes
Household characteristics	No	Yes	No	Yes
Locality characteristics	No	Yes	No	Yes
Observations	99659	99659	122372	122372

Notes: *p < 0.10, ** p < 0.05, *** p < 0.01.
Reported coefficients are average marginal effects.
Robust standard errors are clustered by enumeration area, a census tract, and are reported in parentheses. The regression controls are individual-level characteristics (i.e., mother's and father's years of schooling and a binary indicator for gender of household head), household-level characteristics (i.e., household size, binary indicators for whether a household has piped water, pit latrine, and radio), and locality-level characteristics (i.e., proportion of households with piped water, pit latrine, and radio).

and 3.6 percentage points in Tigray state.

Student's Age for Grade

Similarly, Table 6 is the counterpart of Table 4 where the former presents the between-states comparison whereas the latter presents the within-state comparison. The results from Table 6 also confirm what has been documented in Table 4. That is, conditional on enrollment in primary school, the 1994 education reform has increased the probability that a child attends the "right" grade for her/his age. This is revealed by the positive and statistically significant coefficient estimate of the "$Treated * After$" variable in Table 6. To be exact, our preferred model suggests that the 1994 education reform has increased the probability that a child attends the "right" grade for her/his age by about 0.5 and 2.6 percentage points in Afar and Tigray states, respectively.

5.3. Falsification Tests

Within-Amhara-state Comparison

In the main analysis, which is presented in Subsections 5.1 and 5.2, we have documented the positive treatment effect. We have then argued that the positive treatment effect is driven by the 1994 education reform (and, hence, the change in the medium of instruction to mother tongue instruction). This implicitly assumes that the difference-in-differences estimates pick up the treatment effect of the 1994 education reform and not the effect of other potential factors that may affect educational outcomes in primary school, even in the absence of the 1994 education reform. To assess the validity of this claim, we conduct (within-state) falsification tests and present the results in Table 7.

In the falsification tests, we drop children who live in Awi, Oromiya, and Wag Hemra zones, which are the "true" treated zones. This would leave us with eight zones in Amhara state. Typically, zones in Ethiopia, including those in Amhara state, are assigned with administrative numbers (such as 01, 02, 03, etc.) for administrative convenience. We exploit the administrative zone numbers to create *placebo* treatment and control groups from the remaining eight zones in Amhara state. Specifically, we assign *odd-numbered* zones into treatment group and *even-numbered* zones into control group. Since such assignment of zones into treatment and control groups is random, we would expect a nil treatment effect in this falsification test analysis if the positive

Table 6: **Between-states Analysis:** Difference-in-Differences Probit Estimates of the Effect of a Change in Medium of Instruction on Student's Age for Grade
Dependent Variable: Binary Indicator for whether a Student Attends the "Right" Grade for Her/His Age

	Treated State			
	Afar		Tigray	
	(1)	(2)	(3)	(4)
Treated	-0.019***	-0.013***	-0.086***	-0.090***
	(0.004)	(0.001)	(0.006)	(0.009)
After	0.123***	0.117***	0.121***	0.117***
	(0.003)	(0.009)	(0.003)	(0.009)
Treated*After	0.042***	0.005***	0.030***	0.026***
	(0.002)	(0.001)	(0.002)	(0.003)
Unemployment rate		0.013		0.004***
		(0.010)		(0.001)
Individual characteristics	No	Yes	No	Yes
Household characteristics	No	Yes	No	Yes
Locality characteristics	No	Yes	No	Yes
Observations	46555	46555	59214	59214

Notes: *p < 0.10, ** p < 0.05, *** p < 0.01.
Reported coefficients are average marginal effects.
Robust standard errors are clustered by enumeration area, a census tract, and are reported in parentheses. The regression controls are individual-level characteristics (i.e., mother's and father's years of schooling and a binary indicator for gender of household head), household-level characteristics (i.e., household size, binary indicators for whether a household has piped water, pit latrine, and radio), and locality-level characteristics (i.e., proportion of households with piped water, pit latrine, and radio).

Table 7: **Within-state Falsification Test:** Difference-in-Differences Probit Estimates of the Effect of a Change in Medium of Instruction on Educational Outcomes

	Dependent Variable			
	Enrollment		Student's Age for Grade	
	(1)	(2)	(3)	(4)
Treated, placebo	-0.022*	-0.031	-0.024	-0.016
	(0.012)	(0.028)	(0.028)	(0.011)
After	0.463***	0.440***	0.129***	0.134***
	(0.003)	(0.010)	(0.005)	(0.028)
Treated, placebo*After	0.004	0.011	0.020	0.017
	(0.005)	(0.012)	(0.014)	(0.014)
Unemployment rate		0.022***		0.009***
		(0.002)		(0.002)
Individual characteristics	No	Yes	No	Yes
Household characteristics	No	Yes	No	Yes
Locality characteristics	No	Yes	No	Yes
Observations	83078	83078	41382	41382

Notes: *p < 0.10, ** p < 0.05, *** p < 0.01.
Reported coefficients are average marginal effects.
Robust standard errors are clustered by enumeration area, a census tract, and are reported in parentheses. The regression controls are individual-level characteristics (i.e., mother's and father's years of schooling and a binary indicator for gender of household head), household-level characteristics (i.e., household size, binary indicators for whether a household has piped water, pit latrine, and radio), and locality-level characteristics (i.e., proportion of households with piped water, pit latrine, and radio).

treatment effect presented in Tables 3 and 4 in the main analysis that compares zones in Amhara state is driven by the 1994 education reform and not by other factors.

The results from the falsification tests are presented in Table 7, where the first two columns present results where the dependent variable is a binary indicator for enrollment in primary school whereas the last two columns present results where the dependent variable is a binary indicator for whether the child attends the "right" grade for her/his age, conditional on enrollment in primary school. As can be seen from Table 7, the coefficient estimates of the interaction term, "$Treated, placebo * After$," are insignificant in all specifications. This confirms that the positive treatment effect presented in the main analysis that compares zones in Amhara state is driven by the 1994 education reform.

Between-States Comparison

In the (within-state) falsification tests presented above, we assigned children in "true" *control* zones in Amhara state into *placebo* treatment and control groups. The flip side of this assignment is assigning children in "true" *treatment* groups into *placebo* control and treatment groups. This is exactly what we do in the complementary falsification tests presented below.

As discussed earlier, Awi, Oromiya, and Wag Hemra zones are the only zones in Amhara state that have changed the medium of instruction in primary schools following the 1994 education reform. Thus, for this part of the analysis, we drop all zones in Amhara state except Awi, Oromiya, and Wag Hemra zones, which are assigned into treatment group. Afar and Tigray states, on the other hand, are assigned into control group even if primary schools in these states have actually changed their medium of instruction following the 1994 education reform. In this complementary falsification analysis, the estimates are robust to shocks specific to Amhara state.

Tables 8 and 9 present results from this falsification test analysis where the dependent variables are binary indicators for enrollment in primary school in Table 8 and whether the child attends the "right" grade for her/his age in Table 9. Again, the coefficient estimate of the "$Treated * After$" variable is uniformly insignificant in both tables, suggesting that the positive treatment effect presented in Tables 5 and 6 in the main analysis that compares Amhara state with its neighboring states of Afar and Tigray is driven by the 1994 education reform.

Table 8: **Between-states Falsification Test:** Difference-in-Differences Probit Estimates of the Effect of a Change in Medium of Instruction on Enrollment in Primary School
Dependent Variable: Binary Indicator for Enrollment in Primary School

	Placebo Control State			
	Afar		Tigray	
	(1)	(2)	(3)	(4)
Treated	-0.064	0.168	0.130	0.100
	(0.058)	(0.167)	(0.153)	(0.092)
After	0.458	0.419	0.486***	0.447***
	(0.396)	(0.377)	(0.001)	(0.007)
Treated*After	0.051	0.021	0.094	0.146
	(0.043)	(0.025)	(0.075)	(0.139)
Unemployment rate		0.023		0.024***
		(0.016)		(0.002)
Individual characteristics	No	Yes	No	Yes
Household characteristics	No	Yes	No	Yes
Locality characteristics	No	Yes	No	Yes
Observations	21526	21526	44239	44239

Notes: *p < 0.10, ** p < 0.05, *** p < 0.01.
Reported coefficients are average marginal effects.
Robust standard errors are clustered by enumeration area, a census tract, and are reported in parentheses. The regression controls are individual-level characteristics (i.e., mother's and father's years of schooling and a binary indicator for gender of household head), household-level characteristics (i.e., household size, binary indicators for whether a household has piped water, pit latrine, and radio), and locality-level characteristics (i.e., proportion of households with piped water, pit latrine, and radio).

Table 9: **Between-states Falsification Test:** Difference-in-Differences Probit Estimates of the Effect of a Change in Medium of Instruction on Student's Age for Grade
Dependent Variable: Binary Indicator for whether a Student Attends the "Right" Grade for Her/His Age

	Placebo Control State			
	Afar		Tigray	
	(1)	(2)	(3)	(4)
Treated	-0.022	-0.016	0.075	0.079
	(0.042)	(0.013)	(0.080)	(0.091)
After	0.106***	0.119***	0.110***	0.128***
	(0.011)	(0.031)	(0.008)	(0.012)
Treated*After	0.041	0.026	0.090	0.077
	(0.042)	(0.025)	(0.086)	(0.074)
Unemployment rate		0.005***		0.008***
		(0.001)		(0.003)
Individual characteristics	No	Yes	No	Yes
Household characteristics	No	Yes	No	Yes
Locality characteristics	No	Yes	No	Yes
Observations	7088	7088	19747	19747

Notes: *p < 0.10, ** p < 0.05, *** p < 0.01.
Reported coefficients are average marginal effects.
Robust standard errors are clustered by enumeration area, a census tract, and are reported in parentheses. The regression controls are individual-level characteristics (i.e., mother's and father's years of schooling and a binary indicator for gender of household head), household-level characteristics (i.e., household size, binary indicators for whether a household has piped water, pit latrine, and radio), and locality-level characteristics (i.e., proportion of households with piped water, pit latrine, and radio).

6. Conclusion

Many developing countries have made primary schools more and more accessible in the last two decades and, hence, have been able to substantially increase the primary school enrollment rate. However, access to primary schools and enrollment is still well below universal. Furthermore, poor performance at school and school dropout have remained serious challenges.

Researchers have explored various barriers to enrollment in primary school and performance at school. However, we know little about the effect of learning in mother tongue on both enrollment in primary school and performance at school. This is in contrast to the continuous interest among policymakers, particularly those from multilingual developing countries, in adopting mother tongue instruction in primary school.

Consider Ethiopia, which is also the focus of the present study, as an example. Ethiopia is a typical developing country where more than 90 languages are currently spoken within its geographic boundary (Bamgbose, 1991). Considering Ethiopia is a multilingual country (along with some political considerations), the government of Ethiopia signed into law the Education and Training Policy in 1994 (Ministry of Education, 1994). The policy document has primarily given discretion to state governments in Ethiopia to choose the language of instruction in primary schools located in their jurisdictions. As a result, the medium of instruction in primary schools in the country has increased from using Amharic as the only medium of instruction in 1990 to about 25 languages in 2007 (Seidel and Moritz, 2007).

In this paper, we exploit the variation in the change in medium of instruction across schools located in different districts in Ethiopia following the 1994 education reform. In Amhara state, the reform has affected only schools in some districts, but not in others. Thus, in the part of the analysis that exploits within-Amhara-state variation, we assigned zones into *treatment* and *control* groups depending on whether zones in Amhara state have changed the medium of instruction in primary school following the reform. Similarly, we exploited between-states variation by assigning children in the neighboring states of Afar and Tigray into *treatment* group and children in Amhara state (after dropping those in Awi, Oromiya, and Wag Hemra zones of Amhara state) into *control* group.

This allows us to estimate difference-in-differences models using data from the 2-percent public-use microdata samples of the 1994 and 2007 Ethiopian population

censuses as pre- and post-reform data, respectively. The results from our preferred (difference-in-differences) model suggest that the 1994 education reform in Ethiopia, which has led to a change in the medium of instruction in primary school to mother tongue instruction, has increased the probability of enrollment in primary school by 8.4 percentage points in within-state analysis and by 2.2 and 3.6 percentage points in between-states analysis. Similarly, our results suggest that the education reform has increased the probability of a child attending the "right" grade for her/his age by 9.8 percentage points in within-state analysis and by 0.5 and 2.6 percentage points in between-states analysis. Falsification tests suggest that our results are not confounded by other factors.

The findings in the present study support the argument that learning in mother tongue improves educational outcomes in primary school. However, these findings should be treated carefully as gains in academic performance in primary school due to learning in mother tongue do not necessarily translate to better labor market outcomes later in life. There is evidence that suggests that learning in mother tongue may negatively affect proficiency in national and international languages, which, in turn, negatively affects students' labor market outcomes later in life (see, e.g., Angrist and Lavy, 1997).

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