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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% 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 specification 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, and the effects are relatively stronger for kids in rural areas. Falsification tests suggest that our results are not confounded by other factors. This evidence supports the argument that mother-tongue instruction improves educational outcomes in primary school.

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 retention, and school dropout. Many factors contribute to students' poor performance, among which, the effect of mother-tongue instruction 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 mother-tongue instruction 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

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Education and Training Policy in 1994 (Ministry of Education, 1994). The policy document has primarily given discretion to state governments in Ethiopia[1] 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[2] as the only medium of instruction in 1990 to about 25 languages in 2007 (Seidel & Moritz, 2007).

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 & 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 elsewhere, including, for instance, in Latvia (Ivlevs & King, 2013), Hong Kong (Yip, Tsang, & Cheung, 2003), Cameroon (Gfeller & Robinson, 1998), Ghana (Collison, 1974), and other countries (see, e.g., Bamgbose, 1991; Trudell, 2005 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 mother-tongue instruction (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 mother-tongue instruction 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% public-use microdata samples of the 1994 and 2007 Ethiopian population censuses. Specifically, the study uses a subsample of children who live in Amhara state[3] and its neighboring states of Afar 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,[4] 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,[5] 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% 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 mother-tongue instruction 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 specification 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 7.4 percentage points in within-state analysis and by 1.9 and 2.6 percentage points

[1] 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.

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

[3] 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% of the population in Ethiopia.

[4] Agew, Awngi, and Oromiffa are the native languages of the people from Agew, Agew-Awi, and Ormo ethnic groups, respectively. Data from the 2007 Ethiopian population census reveal that about 62% of the population in Wag Hemra zone are native speakers of Agew language. Similarly, the proportion of population who are native speakers of Awngi in Awi zone, Oromiffa in Oromiya zone, and Amharic in the rest of the zones in Amhara state are 65, 82, and 97%, respectively.

[5] Afarigna and Tigrigna are the native languages of the people from Afar anf Tigre ethnic groups, respectively. Data from the 2007 Ethiopian population census reveal that the proportion of population who are native speakers of Afarigna in Afar state and Tigrigna in Tigray state are 90 and 95%, respectively.

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 8.8 percentage points in within-state analysis and by 0.6 and 2.4 percentage points in between-states analysis. We also find that the 1994 education reform has stronger effect on educational outcomes of kids in rural areas relative to those in urban areas. Falsification tests, on the other hand, suggest that our results are not confounded by other factors. This evidence supports the argument that mother-tongue instruction 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), mother-tongue instruction reduces the pressure on students of 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, mother-tongue instruction may negatively affect proficiency in national and international languages (see, e.g., Angrist & 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 & 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 mother-tongue instruction 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 education. 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., Bleakley and Chin, 2004; Carliner, 1981; Chiswick, 1978; Chiswick and Repetto, 2000; Dustmann and Soest, 2001; and Dustmann & Fabbri, 2003).

[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.

The study by Ivlevs and King (2013) is the one that is 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, and grade retention 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% Latvian and 40% minority language, the 1994 education reform in Ethiopia has led to a 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 mother-tongue instruction 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 mother-tongue instruction 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 mother-tongue instruction 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 1994 education reform in Ethiopia

Cultural assimilation was considered as a primary means of bringing unity in Ethiopia during the era of Emperor Haile Selassie I (1930–1974). Thus, as part of the cultural assimilation process, Amharic was adopted as both the only official language of the country and medium of instruction in primary schools in Ethiopia.

In 1974, when a military group, called Derg, came to power, there were attempts to incorporate other languages into the education system. These attempts, however, were limited to the non-formal education programs such as adult literacy program. In the formal education system, on the contrary, Amharic has continued to be the only medium of instruction in primary schools for the entire period of Derg's administration (1974–1991).

When the current government came to power in 1991, discussion on ethnic suppression was at the center of the political dialogue. In an attempt to acknowledge enthnolinguistic diversity and equality, the current government has promoted, along with other measures, the use of local languages in the formal education system. This was formally initiated following the signing of the Education and Training Policy into law in 1994 (Ministry of Education, 1994).

Soon after the signing of the 1994 education reform, a massive, nation-wide effort was initiated to implement mother-tongue instruction in primary schools across districts in Ethiopia.[7] The implementation process mainly involved translation of educational materials (such as textbooks and teacher's guides) to multiple languages, training of existing teachers, and hiring of new teachers who can teach students in their mother tongue. The federal Ministry of Education was responsible for implementing mother-tongue instruction in primary schools across states in Ethiopia. Hence, the majority of implementation-related works, including translation of textbooks from Amharic to other local languages, were done centrally in Addis Ababa, the federal capital, even though states' education bureaus have participated in the process.

At the early stage of the implementation, shortage of qualified teachers and education materials were identified as major implementation challenges. Moving from the use of Amharic as the only language of instruction to the use of multiple languages in a short period of time was the main reason for these challenges. As time passes by, however, the federal Ministry of Education and the states' education bureaus have gained experience and, hence, improved their implementation procedures and practices. As a result, the implementation challenges have started to fade away with time.

Since states in Ethiopia vary widely in their ethnic composition and language diversity, the 1994 education reform has given states a discretion to adopt 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. By 2007, thus, about 25 languages were being used as media of instruction in primary schools in Ethiopia (Seidel & Moritz, 2007), a substantial increase considering Amharic was the only medium of instruction in 1990.

Contrary to schools in other states in Ethiopia, 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, Amharic is their mother tongue. In fact, Amharic is

[7] Since the adoption of mother-tongue instruction in Ethiopia has had strong political motive, all states in the country have formally initiated the implementation of mother-tongue instruction in primary school immediately after the signing of the 1994 education reform regardless of the states' economic levels.

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.

the official language of Amhara state – and the federal government of Ethiopia.

There are, of course, three main minority groups in Amhara state; namely, Agew, Agew-Awi, and Oromo ethnic groups that 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. On the contrary, 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 mother-tongue instruction on educational outcomes in primary school.

The 1994 education reform as 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% 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 mother-tongue instruction on educational outcomes in primary school using a difference-in-differences approach. In the difference-in-differences approach, we exploit both within- and between-states comparisons as an identification strategy.

In the within-Amhara-state analysis, 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. On the other hand, children in Amhara state (after dropping those in Awi, Oromiya, and Wag Hemra zones) and those in the neighboring states of Afar and Tigray are, respectively, assigned into *control* and *treatment* groups in the between-states analysis – see Table 1 and Fig. C.1 in Appendix C for how zones/states are assigned into treatment and control groups.

It is important to note that among schools in our sample only those in the treated group have changed their medium of instruction following the 1994 education reform. This implies that the possible (temporary) disruption in the teaching-learning process during the early period of implementation potentially affects educational outcomes of students in schools in the treated group, but not those in schools in the control group.

Since only schools in the treated group could potentially be negatively affected by the disruption associated with implementing mother-tongue instruction, the observed difference in educational outcomes between schools in treated and control groups could partly be driven by this disruption. Even if it is not clear how long the potential negative effect on educational outcomes of the disruption associated with implementation lasts, it is reasonable to assume that this is not a major concern in our paper. This is primarily because the post-reform data used in our paper were collected in 2007, which is 13 years after the signing of the 1994 education reform. If 13 years is not long enough for schools in the treated group to adjust and revert back any negative effect of the disruptive implementation on educational outcomes, then this will bias our coefficient estimates downward.

Finally, it is worth mentioning that the 1994 education reform has other aspects as well. However, the only aspect of the reform that has affected schools in treated and control groups differently is the adoption of mother-tongue instruction in primary school. Moreover, no other education policy change that affects schools in treated and control groups differently has been introduced during the period of analysis, i.e., between 1994 and 2007.

Even if other education policies that can affect schools in the three states in our sample differently have been introduced during the period of analysis, the results from within-Amhara-state difference-in-differences analysis will not be biased. This is because the smallest administrative unit in Ethiopia that can design and introduce education policies is a state government, implying schools

in all zones in Amhara state (including those in treated and control groups/zones in the within-Amhara-state difference-in-differences analysis) are exposed to the same set of education policies, with the only exception of the adoption of mother-tongue instruction in primary school.

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% public-use microdata samples of the 1994 and 2007 censuses are used.[8]

For population censuses, CSA administers two types of questionnaire: short-form and long-form. As the names imply, the main difference between the 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.[9]

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% 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% 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 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, that is, school-age (i.e., 7–14 year old) 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% 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% in the treated zones of Amhara, Afar, and Tigray states, respectively.

Table 2, on the other hand, shows that about 26.1% of students in the control zones of Amhara state were enrolled in the "right" grade for their age.[10] This figure is 24.3, 25.6, and 19.9% in the treated zones of Amhara, Afar, and Tigray states, respectively.[11] The table also shows that only about a fifth of the children in our sample live in urban area, and their average years of age is 10, which is not surprising considering only primary school-age (i.e., 7–14 year old) children are included in our sample.

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, the household assets and amenities variables in Table 2 show that families in general live in poor housing conditions. Finally, Table 2 shows that households in our sample live in communities where both unemployment rate and mean agricultural income are low. These households are, however, located within a reasonable distance from primary schools though more than a third of these schools are reported to be of "poor quality."

5. The impact of the education reform on selected educational outcomes

To identify the average treatment effect of the 1994 education reform (and, hence, 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 state. 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) if the reform has had a positive effect as schools in these zones have changed their media of instruction following 1994 education reform.

[8] The census data are also supplemented by three Ethiopian household survey data – namely, data from Welfare Monitoring Survey, Agricultural Sample Survey, and Producers Price Survey. See the Supplementary Data section in Appendix A for a discussion on the supplementary data used in this paper.

[9] Using public-use microdata samples of the censuses has an important advantage compared to using 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.

[10] In Ethiopia, the official school starting age is 7. Thus, student's-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.

[11] See Table B.1 in Appendix B for descriptive statistics of the educational outcome variables by year, state, and treatment status.

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 dummy	0.490	0.485	0.463	0.482
	(0.499)	(0.500)	(0.490)	(0.499)
Child age (in years)	10.233	10.208	10.338	10.333
	(2.251)	(2.240)	(2.203)	(2.281)
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)
Urban dummy	0.214	0.202	0.204	0.189
	(0.303)	(0.297)	(0.359)	(0.387)
Unemployment rate (in percent)	1.551	1.359	5.625	3.783
	(1.376)	(0.567)	(2.710)	(3.659)
Mean annual hh agricultural income (in 2011 ETB)‡	11,214	9,413	8,337	9,940
	(5,325)	(4,362)	(6,462)	(3,242)
Mean distance to primary school (in km)‡	1.705	5.115	6.070	2.675
	(1.342)	(2.906)	(3.205)	(1.001)
Proportion of "poor quality" primary schools‡	0.370	0.405	0.585	0.385
	(0.236)	(0.219)	(0.464)	(0.312)
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.
‡ denotes information on the variable is obtained from the supplementary data – see the Supplementary Data section in Appendix A for a discussion on the supplementary data used in this paper.
ETB denotes Ethiopian Birr. As of August 2016, the exchange rate between US dollar and ETB is about 1:22.

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) with those found in the neighboring states of Afar and Tigray. 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 as schools in comparison states have changed their media of instruction following 1994 education reform.

The comparison states were chosen based on their geographic proximity to Amhara state[12] 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

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

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 is not. As it is reasonable to assume that the 1994 education reform is exogenous to household behavior,[13] 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 have pre-existing differences that would have led to 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

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

where y_{izt} denotes educational outcomes[14] 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 Eq. (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. We assume that G is a standard normal cumulative distribution function and estimate a probit model, where the average marginal effect of the interaction term and its standard error are computed as suggested by Ai and Norton (2003).[15]

Table 3
Within-state analysis: difference-in-differences probit estimates of the effect of a change in medium of instruction on educational outcomes.

	(1)	(2)
Enrollment in primary school equation		
Treated	−0.045***	−0.053***
	(0.007)	(0.018)
After	0.462***	0.413***
	(0.001)	(0.010)
Treated*After	0.159***	0.074***
	(0.030)	(0.013)
Control variables	No	Yes
Observations	88023	88023
Student's age for grade equation		
Treated	−0.126***	−0.051***
	(0.016)	(0.013)
After	0.238***	0.134***
	(0.008)	(0.017)
Treated*After	0.136***	0.088***
	(0.017)	(0.026)
Control variables	No	Yes
Observations	43297	43297

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., child's age in years, binary indicator for gender of a child, 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), locality-level characteristics (i.e., proportion of households with piped water, pit latrine, and radio), proxies for macroeconomic trends (i.e., unemployment rate and agricultural income) and educational inputs (i.e., distance to primary school and self-reported quality of primary schools), and urban dummy.

Enrollment in primary school

The results from the difference-in-differences estimates, 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, are reported in the upper panel of Table 3.[16]

In column 1 of the upper panel of Table 3, we report the *unadjusted* effect of the 1994 education reform on the probability of enrollment in primary school. The coefficient estimate of the "*Treated*" dummy variable is negative and statistically significant at 1% level, suggesting pre-existing differences in the probability of enrollment in primary school between children in the treated and control zones

[13] If there is endogenous migration (say, for instance, parents who care more about their children's education migrate to zones/states that have changed the medium of instruction in primary school from Amharic to mother-tongue instruction), then this will bias our results. However, this is not a serious concern in our case since internal migration is not a common phenomenon in Ethiopia, particularly in states in our sample. Data from the 2007 Ethiopian population census reveal that only 6, 5, and 9% of the population in Afar, Amhara, and Tigray states have, respectively, migrated to these states in the pervious 10 years. Moreover, anecdotal evidence suggests that looking for better economic opportunities (rather than looking for better primary schools) is the primary reason for migration in Ethiopia.

[14] 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. Considering we do not observe test scores in our data and given the Ethiopian primary education system is characterized by, particularly during the period of analysis, delayed enrollment, low rate of primary school enrollment, and high rate of grade retention, these two dependent variables are good measures of educational performance.

[15] The average marginal effects of the explanatory variables on the likelihoods of educational outcomes are estimated by averaging the underlying partial effects over the distributions of the explanatory variables and the unobserved effects. The computation of the average marginal effects of the interaction term, in particular, follows the suggestion of Ai and Norton (2003) which is shown to be a correct way of estimating average marginal effects of interaction terms in a general class of nonlinear models such as the one used in this paper.

[16] The complete regression results from models presented in Tables 3–6 are reported in the supplementary appendix.

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 column 1 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 1994 education reform (and, hence, mother-tongue instruction) on the probability of enrollment in primary school, is the interaction term between the "*Treated*" and "*After*" variables, i.e., "*Treated* ∗ *After*." In column 1 of the upper panel of Table 3, the coefficient estimate of the interaction term is 0.159 and is statistically significant at 1% 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 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. 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 model presented here 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 results from the difference-in-differences model presented here cannot be interpreted as a "true" treatment effect.

Moreover, one of the identifying assumptions of the difference-in-differences approach is that there was no differential economic growth rates between 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. Here, we employ proxies for economic growth rates/macroeconomic trends to control for potential differences in overall macroeconomic trends between treated and control groups.[17]

Similarly, controlling for educational inputs is crucial if schools in the treated group have received extra funding to implement the 1994 education reform. Ideally, we would have liked to control for direct measures of educational inputs such as educational expenditure per student, teachers' work experience and academic qualification, and availability of books and other school inputs. Since we could not find data on these direct measures of educational inputs, particularly for the period before the 1994 education reform, we resort to an indirect measure of educational inputs, i.e., distance to primary school and the proportion of households that self-report primary schools in their neighborhood are of poor quality. Since it is plausible to assume that the proportion of households that self report schools in their neighborhood are of poor quality is correlated with the average quality of schools in their neighborhood, employing self-reported measure of school quality does not create a serious concern here.

In column 2, therefore, we present results from the difference-in-differences model that adjust for observable differences, including differences in educational inputs and macroeconomic trends,[18] between individuals in the treated and control zones in the regression framework.

As can be seen in column 2 of the upper panel of Table 3, controlling for observable characteristics, including educational inputs and macroeconomic trends, does not affect the sign and significance level of the coefficient estimate of the "*Treated* ∗ *After*" variable, but its magnitude has 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 mother-tongue instruction on the probability of enrollment in primary school.

Student's age for grade

Even if we do not observe a more precise measure of school performance such as test scores, we do observe 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.[19]

[17] Ideally, we would have liked to control for zone/state-level Gross Domestic Product (GDP). Since we do not observe zone/state-level GDP, we use unemployment rate and agricultural income as proxies for macroeconomic trends.

[18] Note that information on proxy variables for educational inputs (i.e., distance to primary school and self-reported quality of primary schools) and one of the proxy variables for macroeconomic trends (i.e., agricultural income) is obtained from the supplementary data – see the Supplementary Data section in Appendix A for a discussion on the supplementary data used in this paper.

[19] See footnote [10] in the Data section for how the binary indicator for whether a student attends the "right" grade for her/his age is defined.

Since delaying enrollment in primary school beyond the legal school starting age is common in Ethiopia,[20] 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, 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 aggregate effect of the 1994 education reform on both delayed enrollment and grade retention is crucial since both delayed enrollment and grade retention are still common problems in primary education in Ethiopia.

The lower panel of Table 3 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 both specifications presented in the lower panel of Table 3, 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 both specifications. In our preferred specification that adjusts for observable differences, including differences in educational inputs and macroeconomic trends, for instance, the coefficient estimate of the interaction term is 0.088, implying that the 1994 education reform has increased the probability that students attend the "right" grade for their age by about 8.8 percentage points. This can be interpreted as that the 1994 education reform (and, hence, 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 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:

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

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 Eq. (1).

Enrollment in primary school

Table 4 is the counterpart of Table 3 where the dependent variables and all control variables are exactly the same. However, in Table 4, 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 4, 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 the upper panel of Table 4, confirm what has been reported in the upper panel of 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% 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 specification suggest that mother-tongue instruction in primary school has increased the probability of enrollment in primary school by 1.9 percentage points in Afar state and 2.6 percentage points in Tigray state.

So far we have documented positive treatment effect of the 1994 education reform (and, hence, mother-tongue instruction) on the probability of enrollment in primary school in Ethiopia, both in within-Amhara-state and between-states difference-in-differences analysis. There could be a number of mechanisms through which the 1994 education reform could affect primary school enrollment. Unfortunately, however, the literature on primary school enrollment in developing countries focuses on the role access to school plays in enrollment, with very limited attention given to the role the quality of school, particularly mother-tongue instruction, plays in parents' decision whether to send their kids to school. This, coupled with the limited number of variables we observe in the census data used in this paper, makes it difficult to accurately explore the mechanisms through which the 1994 education reform affects primary school enrollment in Ethiopia.

[20] 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 & Lee, 2001).

Table 4
Between-states analysis: difference-in-differences probit estimates of the effect of a change in medium of instruction on educational outcomes.

	Treated state			
	Afar		Tigray	
	(1)	(2)	(3)	(4)
Enrollment in primary school equation				
Treated	−0.066***	−0.153***	−0.130***	−0.077***
	(0.019)	(0.020)	(0.003)	(0.016)
After	0.460***	0.377***	0.487***	0.426***
	(0.001)	(0.012)	(0.001)	(0.008)
Treated*After	0.054**	0.019***	0.093***	0.026***
	(0.021)	(0.003)	(0.005)	(0.006)
Control variables	No	Yes	No	Yes
Observations	99659	99659	122372	122372
Student's age for grade equation				
Treated	−0.019***	−0.012***	−0.086***	−0.083***
	(0.004)	(0.003)	(0.006)	(0.007)
After	0.123***	0.108***	0.121***	0.109***
	(0.003)	(0.010)	(0.003)	(0.012)
Treated*After	0.042***	0.006***	0.030***	0.024***
	(0.002)	(0.001)	(0.002)	(0.003)
Control variables	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., child's age in years, binary indicator for gender of a child, 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), locality-level characteristics (i.e., proportion of households with piped water, pit latrine, and radio), proxies for macroeconomic trends (i.e., unemployment rate and agricultural income) and educational inputs (i.e., distance to primary school and self-reported quality of primary schools), and urban dummy.

However, anecdotal evidence suggests that there are a number of potential mechanisms through which mother-tongue instruction may increase enrollment in primary school in developing countries. For instance, mother-tongue instruction typically reflects a familiar culture and set of values, which makes children and their families to be less apprehensive about attending school. Moreover, teachers in schools that use mother-tongue instruction are more likely to come from the same linguistic and cultural communities as their students. This, in turn, improves the communication between teachers, students, and parents, increasing parents' access to information about enrollment and schooling processes. Besides, these teachers can easily interact socially with students' families and are potentially more trustworthy and more subject to social control, reducing the risk that they will abuse their students sexually or otherwise (see, e.g., Benson, 2005). Finally, teachers from the same linguistic and cultural communities serve as role models for children. As a result of all these, many parents appear to be more willing to send their children to school when mother-tongue instruction is adopted (see, e.g., Klaus, 2003).

The positive effect of mother-tongue instruction on enrollment that we have documented here could be due to some or all of the above factors. Given the data we have, however, it is not plausible to test which of the above factors are at play here. This could be an interesting area for further research on the topic.

Student's age for grade

The results from the lower panel of Table 4, on the other hand, confirm what has been documented in the lower panel of Table 3. 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 the lower panel of Table 4. To be exact, our preferred specification suggests that the 1994 education reform has increased the probability that a child attends the "right" grade for her/his age by about 0.6 and 2.4 percentage points in Afar and Tigray states, respectively.

As mentioned earlier, the binary indicator for whether a student is in the "right" grade for her/his age, which is used as one of the dependent variables here, is affected by both the timing of enrollment in primary school and the rate at which the student progresses through grades. This means the positive treatment effect documented here implies that the 1994 education reform has increased the probability of grade progression or on-time enrollment or both. Given the data we have, however, disaggregating the effect of the 1994 education reform into its effect on grade progression and on-time enrollment is impossible. As discussed earlier, however, understanding the aggregate effect of the 1994 education reform on both delayed enrollment and grade retention is crucial since both delayed

enrollment and grade retention are still common problems in primary schools in Ethiopia.

The positive effect of the 1994 education reform on grade progression/on-time enrollment, which we have documented here both in within-Amhara-state and between-states difference-in-differences analysis, is not surprising, however, since prior researches have documented that parents are, as mentioned earlier, more likely to send their children to school when mother-tongue instruction is adopted (see, e.g., Klaus, 2003). Besides, anecdotal evidence suggests that students in schools that use second-language instruction make relatively little progress in learning actual contents as they spend a lot of time in learning the language of instruction (see, e.g., Trudell, 2005). On the other hand, students in schools that use mother-tongue instruction are able to express themselves well in the language of instruction, which, in turn, help them develop higher level of cognitive skills relatively quickly (see, e.g., Sonaiya, 2002). Moreover, the influence and support of parents on a child's learning is higher where students are taught in mother tongue as parents are not excluded from their children's educational experience as schools are now more relevant to the culture and set of values of children and their parents.

Again, all these factors could be at play here and explain why the 1994 education reform in Ethiopia has resulted in increase in the probabilities of grade progression or on-time enrollment or both. Given the data we have, however, it is difficult to quantitatively document which of these factors have actually led to improvement in school performance in Ethiopia.

Finally, it is worth mentioning that the magnitude of the effect of mother-tongue instruction on educational outcomes (i.e., both on the probabilities of enrollment and being in the "right" grade for the student's age) is consistently higher for the within-Amhara-state analysis, followed by between-states analysis that compares Amhara and Tigray states and then the one that compares Amhara and Afar states. The differences in the magnitude of the effect across these specifications seem to be explained (though not perfectly) by linguistic distance between Amharic and the other languages that have been adopted as media of instruction.

This can be demonstrated if we assume that a farther away a student's mother tongue is from Amharic, the higher the benefit of the shift from Amharic instruction to mother-tongue instruction is to the student and if we use the classification of Ethiopian languages by Greenberg (1966)[21] as a measure of linguistic distance between Amharic and the rest of the languages. Under these conditions, we expect the magnitude of the treatment effect to be higher for the within-Amhara-state analysis and the between-states analysis that compares Amhara and Afar states.

This is partly consistent with what we have found as the magnitude of the treatment effect in within-Amhara-state analysis is higher than that of between-states analysis that compares Amhara and Tigray states. Contrary to our expectation, however, the magnitude of the treatment effect in between-states analysis that compares Amhara and Tigray states is higher than the one that compares Amhara and Afar states, suggesting factors other than linguistic distance may also be at play here and partly explain differences in the magnitude of the treatment effect across these specifications.

[21] According to this classification, Amharic and Tigrigna fall into Sematic language group whereas Afarigna, Agew, Awngi, and Oromiffa fall into Cushitic language group.

Table 5
Within-state falsification test: difference-in-differences probit estimates of the effect of a change in medium of instruction on educational outcomes.

	(1)	(2)
Enrollment in primary school equation		
Treated, placebo	−0.022*	−0.025
	(0.012)	(0.027)
After	0.463***	0.031***
	(0.003)	(0.012)
Treated, placebo*After	0.004	0.018
	(0.005)	(0.013)
Control variables	No	Yes
Observations	83078	83078
Student's age for grade equation		
Treated, placebo	−0.024	−0.016
	(0.028)	(0.011)
After	0.129***	0.118***
	(0.005)	(0.031)
Treated, placebo*After	0.020	0.013
	(0.014)	(0.016)
Control variables	No	Yes
Observations	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., child's age in years, binary indicator for gender of a child, 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), locality-level characteristics (i.e., proportion of households with piped water, pit latrine, and radio), proxies for macroeconomic trends (i.e., unemployment rate and agricultural income) and educational inputs (i.e., distance to primary school and self-reported quality of primary schools), and urban dummy.

5.3. Falsification tests

Within-Amhara-state comparison

In the main analysis, which is presented in Sections 5.1 and 5.2, we have documented positive treatment effect. We have then argued that the treatment effect is driven by the 1994 education reform (and, hence, 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 5.

Table 6
Between-states falsification test: difference-in-differences probit estimates of the effect of a change in medium of instruction on educational outcomes.

	Placebo control state			
	Afar		Tigray	
	(1)	(2)	(3)	(4)
Enrollment in primary school equation				
Treated	−0.064	0.151	0.130	0.088
	(0.058)	(0.155)	(0.153)	(0.083)
After	0.458	0.384	0.486***	0.397***
	(0.396)	(0.362)	(0.001)	(0.021)
Treated*After	0.051	0.021	0.094	0.131
	(0.043)	(0.026)	(0.075)	(0.140)
Control variables	No	Yes	No	Yes
Observations	21526	21526	44239	44239
Student's age for grade equation				
Treated	−0.022	−0.017	0.075	0.071
	(0.042)	(0.020)	(0.080)	(0.090)
After	0.106***	0.099***	0.110***	0.101***
	(0.011)	(0.031)	(0.008)	(0.012)
Treated*After	0.041	0.020	0.090	0.066
	(0.042)	(0.028)	(0.086)	(0.071)
Control variables	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., child's age in years, binary indicator for gender of a child, 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), locality-level characteristics (i.e., proportion of households with piped water, pit latrine, and radio), proxies for macroeconomic trends (i.e., unemployment rate and agricultural income) and educational inputs (i.e., distance to primary school and self-reported quality of primary schools), and urban dummy.

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 treatment effect presented in Table 3 is driven by the 1994 education reform and not by other factors.

The results from this falsification tests are presented in Table 5, where the upper panel presents results where the dependent variable is a binary indicator for enrollment in primary school whereas the lower panel presents 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 both the upper and lower panels of Table 5, 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 and not by other factors.

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 6 presents results from this falsification test analysis where the upper panel presents results where the dependent variable is binary indicators for enrollment in

Table 7
Difference-in-differences probit estimates of the effect of a change in medium of instruction on educational outcomes by location of residence.

	Within-state comparison		Between-states comparison (Amhara Vs Afar)		Between-states comparison (Amhara Vs Tigray)	
	Rural	Urban	Rural	Urban	Rural	Urban
Enrollment in primary school equation						
Treated	−0.052***	−0.049*	−0.152***	−0.161***	−0.078***	−0.079***
	(0.013)	(0.026)	(0.007)	(0.017)	(0.016)	(0.011)
After	0.431***	0.409***	0.040***	0.039***	0.430***	0.429***
	(0.019)	(0.016)	(0.011)	(0.010)	(0.003)	(0.008)
Treated*After	0.083***	0.075***	0.022***	0.019***	0.026***	0.025***
	(0.022)	(0.029)	(0.006)	(0.007)	(0.010)	(0.003)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	69186	18837	80524	19135	99978	22394
Student's age for grade equation						
Treated	−0.050***	−0.039	−0.011***	−0.008***	−0.073***	−0.081***
	(0.015)	(0.026)	(0.002)	(0.003)	(0.013)	(0.017)
After	0.119***	0.111***	0.110***	0.112***	0.101***	0.116***
	(0.018)	(0.002)	(0.015)	(0.016)	(0.006)	(0.018)
Treated*After	0.087***	0.085***	0.009***	0.006**	0.024***	0.022***
	(0.013)	(0.020)	(0.001)	(0.001)	(0.006)	(0.006)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	34031	9266	37616	8939	48378	10836

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., child's age in years, binary indicator for gender of a child, 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), locality-level characteristics (i.e., proportion of households with piped water, pit latrine, and radio), and proxies for macroeconomic trends (i.e., unemployment rate and agricultural income) and educational inputs (i.e., distance to primary school and self-reported quality of primary schools).

primary school whereas the lower panel presents results where the dependent variable is whether the child attends the "right" grade for her/his age. Again, the coefficient estimate of the "*Treated* ∗ *After*" variable is uniformly insignificant in all specifications in Table 6, suggesting that the positive treatment effect presented in Table 4 is driven by the 1994 education reform and not by other factors.

5.4. Heterogeneity

So far we have documented positive and statistically significant treatment effect of the 1994 education reform. In this sub-section, we explore whether there is heterogeneity in treatment effect, i.e., if the effect of mother-tongue instruction on educational outcomes varies across different groups. Specifically, we explore whether the treatment effect varies by location of residence and gender – and present the results in Tables 7 and 8, respectively.

Table 7 presents the coefficient estimates of selected variables from the difference-in-differences probit regressions that are run separately for rural and urban subsamples. The upper and lower panels of Table 7 present results from enrollment and student's-age-for-grade equations, respectively. In all specifications, the treatment effect is positive and statistically significant as can be seen from the positive and statistically significant coefficient estimates of "*Treated* ∗ *After*" variable across all specifications for both rural and urban subsamples.

Interestingly, the magnitude of the coefficient estimates of the "*Treated* ∗ *After*" variable is higher for the rural subsample in all specifications. This is consistent with the argument that the 1994 education reform has had stronger effect for kids in rural areas relative to those in urban areas. This is not surprising, however, considering kids in urban areas are more likely to speak/understand other (dominant) languages, such as Amharic, in addition to their mother-tongue languages as communities in urban areas are more diverse, implying a move to mother-tongue instruction generally benefits rural kids more than it benefits urban kids.

We have also explored whether there is heterogeneous treatment effect by gender by running difference-in-differences regressions for boys and girls subsamples separately (see Table 8 for results). The coefficient estimates of the "*Treated* ∗ *After*" variable, reported in Table 8, are not systematically different for the boys and girls subsamples, implying that there is no heterogeneity in treatment effect by gender.

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 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 mother-tongue instruction on both enrollment in primary school and performance at school. This is in contrast to the continuous

Table 8
Difference-in-differences probit estimates of the effect of a change in medium of instruction on educational outcomes by gender

	Within-state comparison		Between-states comparison (Amhara Vs Afar)		Between-states comparison (Amhara Vs Tigray)	
	Boys	Girls	Boys	Girls	Boys	Girls
Enrollment in primary school equation						
Treated	−0.056***	−0.086**	−0.143***	−0.177***	−0.080***	−0.082***
	(0.017)	(0.036)	(0.027)	(0.022)	(0.019)	(0.022)
After	0.416***	0.447***	0.376***	0.391***	0.466***	0.431***
	(0.013)	(0.003)	(0.008)	(0.022)	(0.012)	(0.010)
Treated*After	0.070**	0.068***	0.019***	0.025*	0.025***	0.027***
	(0.029)	(0.020)	(0.003)	(0.014)	(0.007)	(0.006)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	42955	45068	46142	53517	63389	58983
Student's age for grade equation						
Treated	−0.048***	-0.049***	−0.009***	−0.012***	−0.082***	−0.085***
	(0.016)	(0.010)	(0.002)	(0.004)	(0.016)	(0.011)
After	0.120***	0.142***	0.102***	0.101***	0.111***	0.109***
	(0.015)	(0.017)	(0.010)	(0.012)	(0.010)	(0.012)
Treated*After	0.101***	0.082***	0.006***	0.004	0.022***	0.027***
	(0.026)	(0.019)	(0.002)	(0.003)	(0.006)	(0.002)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	22168	21129	25000	21555	30673	28541

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., child's age in years, 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), locality-level characteristics (i.e., proportion of households with piped water, pit latrine, and radio), proxies for macroeconomic trends (i.e., unemployment rate and agricultural income) and educational inputs (i.e., distance to primary school and self-reported quality of primary schools), and urban dummy.

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 & 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% public-use microdata samples of the 1994 and 2007 Ethiopian population censuses as pre- and post-reform data, respectively. The results from our preferred specification 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 7.4 percentage points in within-state analysis and by 1.9 and 2.6 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 8.8 percentage points in within-state analysis and by 0.6 and 2.4 percentage points in between-states analysis. We also find that the 1994 education reform has stronger effect on educational outcomes of kids in rural areas relative to those in urban areas. Falsification tests, on the other hand, suggest that our results are not confounded by other factors.

The findings in the present study support the argument that mother-tongue instruction improves educational outcomes in primary school. However, these findings should be treated carefully as gains in academic performance in primary school due to mother-tongue instruction do not necessarily translate to better labor market outcomes later in life. There is evidence that suggests that mother-tongue instruction 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 & Lavy, 1997).

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Appendix A. Supplementary data

As discussed earlier in the main text, the data used in this paper come from the 2% public-use microdata samples of the 1994 and 2007 Ethiopian population censuses. As it is common for population censuses, however, the number of variables collected in the Ethiopian population census is limited. Thus, we supplement the census data by household survey data from Ethiopia and control for educational inputs and macroeconomic trends, which are not observed in the census data.

More specifically, we supplement the census data by data from the 1996 and 2004 survey rounds of Welfare Monitoring Survey (WMS), the 1995 and 2007 survey rounds of Agricultural Sample Survey (AgSS), and the December 2011 Producers Price Survey (PPS), which are administered by the Central Statistics Agency of Ethiopia.

WMS is a cluster-based, nationally representative repeated cross section household survey which is designed to provide information on the "non-monetary" dimension of poverty in Ethiopian households. Thus, WMS contains a wide range of information on household demographics, access to selected facilities, self-reported assessments of the facilities households use, and other important economic variables.[22] We particularly employ data from WMS to control for proxies for educational inputs (i.e., distance to primary school and the proportion of households that self-report primary schools in their neighborhoods are of poor quality).[23]

It is important to note that the survey rounds of WMS data used in this paper are not perfectly aligned with the population census years. This is because the closest available survey rounds of WMS to the 1994 and 2007 census years are the 1996 and 2004 survey rounds of WMS, respectively. Considering the variables employed from the WMS data, i.e., distance to school and self-reported quality of schools, always capture their lagged values, using the 1996 WMS data is not a major concern here. If, however, there is no lag in these variables (say, for example, between the year in which expenditure on school inputs is increased and the year in which improvement in school quality is observed), then the coefficient estimates of the treatment effect could be biased.[24]

To control for macroeconomic trends in the regression framework, on the other hand, we employ data from AgSS and PPS and construct agricultural income variable which is used as an additional proxy for macroeconomic trends. AgSS is a repeated cross section, nationally representative annual household survey that collects a range of information on farm practices and activities, including crop production, land utilization, and farm management.[25] PPS, on the other hand, collects information on farm-gate prices across the country on a monthly basis.

Using information on *quantities* of agricultural items produced by households in the 1995 and 2007 AgSS and information on farm-gate *prices* from the December 2011 PPS, we have constructed zone/state-level real agricultural income variable for the years 1995[26] and 2007. While generating the agricultural income variable, we have adjusted not only for inflation but also for variations in prices of agricultural outputs across zones/states.

Appendix B. Additional table

[22] The 1996 and 2004 WMS covered a total of 11,569 and 36,302 households, respectively.

[23] Controlling for educational inputs is crucial if schools in the treated group have received extra funding to implement the 1994 education reform.

[24] Estimating our baseline difference-in-differences specifications without controlling for proxies for educational inputs and macroeconomic trends yields qualitatively similar coefficient estimates of the treatment effect.

[25] The 1995 and 2007 AgSS covered a total of 14,800 and 42,523 agricultural households, respectively.

[26] Though the pre-reform data used in this paper come from the 1994 census data, we employ data from the 1995 AgSS here because it is the closest available survey round of AgSS to the 1994 census year.

Table B.1
Summary statistics of the outcome variables used in the econometric analysis by year, state, and treatment status

	Amhara				Afar		Tigray	
	Control		Treated		Treated		Treated	
	1994	2007	1994	2007	1994	2007	1994	2007
Enrollment dummy	0.263	0.662	0.171	0.723	0.188	0.646	0.214	0.712
	(0.192)	(0.461)	(0.159)	(0.394)	(0.210)	(0.457)	(0.199)	(0.511)
Student's-age-for-grade dummy	0.186	0.349	0.098	0.386	0.161	0.363	0.105	0.297
	(0.122)	(0.256)	(0.100)	(0.289)	(0.119)	(0.267)	(0.103)	(0.230)
Observations	35723	47355	1978	2967	7462	9119	18468	20826

Notes: Standard deviations are reported in parentheses.

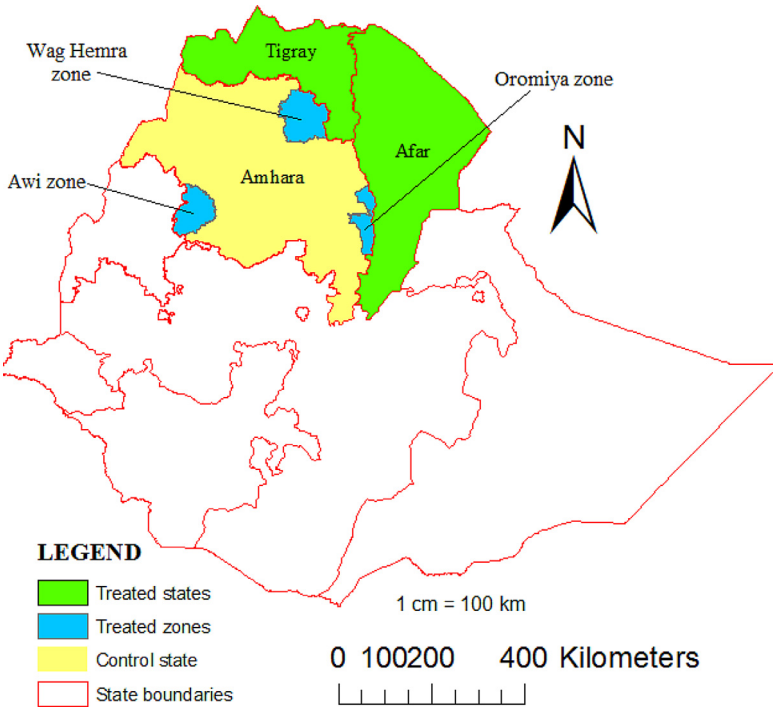

Fig. C.1. States and zones in Ethiopia by treatment status.

Appendix C. Graph

Supplementary material

Supplementary material associated with this article can be found, in the online version, at 10.1016/j.econedurev.2016.08.006.

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