Mother-tongue Instruction and Later Labor Market Outcomes: Evidence from a Natural Experiment in Ethiopia

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Abstract

This paper offers empirical evidence on the effect of mother-tongue instruction in primary school on students' later labor market outcomes. Moreover, it explores whether the effect varies by the duration of exposure to mother-tongue instruction. Since Ethiopia has adopted mother-tongue instruction after the 1994 Ethiopian education reform, many students who have attended primary school after 1994 are exposed to mother tongue instruction, resulting in a variation in exposure to mother-tongue instruction by birth cohort. In Amhara state, Amharic is adopted as medium of instruction both before and after the education reform. This is in contrast to other states in Ethiopia that have changed the medium of instruction in primary school following the education reform. Among students who have been exposed to mother-tongue instruction following the education reform, however, the duration of their exposure to mother-tongue instruction varies depending on the state in which they have attended primary school. This is because states in Ethiopia mandate students to transition from mother-tongue to English instruction either in grade 5, 7, or 9. Exploiting these two plausibly exogenous sources of variations (across states and birth cohorts) and using data from the 2013 Ethiopian Labor Force Survey, we estimate difference-in-differences model. Estimates from our preferred specifications suggest that mother-tongue instruction in primary school improves later labor market outcomes, but the size of its effect decreases with the number of years an individual was exposed to mother-tongue instruction in primary school.

Keywords: Medium of instruction, Labor market, Difference-in-differences, Ethiopia

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JEL: I25, J24, O10, O12

1. Introduction

In recent years, political considerations, along with the objective of achieving universal primary education through improving enrollment among language-minority kids, have led some countries in the developing world to adopt mother-tongue instruction in primary school. This move is observed in many countries, including, for instance, in Ethiopia (Seid, 2016), South Africa (Eriksson, 2014; Taylor and von Fintel, 2016), Peru (Hynsjö and Damon, 2016), Latvia (Ivlevs and King, 2013), and other countries.

A growing evidence shows that mother-tongue instruction in primary school improves school performance. On the contrary, we have a limited understanding on whether mother-tongue instruction has long-lasting effect on students' later labor market outcomes. Some studies, however, highlighted that mother-tongue instruction may negatively affect proficiency in national and international languages (e.g., Angrist and Lavy, 1997). This could potentially hamper performance in the labor market since it is documented that there is a premium for language proficiency in the labor markets of many developed countries. For instance, it has been documented that immigrants who are proficient in the dominant language of the host country perform better in the labor market (see, e.g., Bleakley and Chin (2004); Carliner (1981); Chiswick (1978); Chiswick and Repetto (2000); Dustmann and Fabbri (2003); and Dustmann and Van Soest (2001)).

The findings that there is a premium for proficiency in a dominant language in the labor market, coupled with the possibility that mother-tongue instruction may decrease language proficiency in national and international languages, imply that mother-tongue instruction may negatively affect later labor market outcomes. However, it is unclear whether the lessens learned in studies from developed countries can readily be extrapolated to the labor markets of developing countries like Ethiopia, which is the focus of the present study.

On the other hand, mother-tongue instruction may positively affect later labor market outcomes through its positive effect on the accumulation of human capital as it improves students' performance at school. There is evidence that suggests mother-tongue instruction in primary school improves educational outcomes (e.g., Hynsjö and Damon, 2016; Piper et al., 2016; Seid, 2016). Interestingly, the gains in school performance due to mother-tongue instruction continues even after students transition to English instruction classrooms in later years in primary school (e.g., Seid, 2017). It is, however, not clear for how long the gains in school performance due to mother-tongue instruction lasts.

The literature on the topic, hence, seems to suggest that mother-tongue instruction in primary school has two opposing effects on later labor market outcomes: through its positive

effect on students' school performance and through its negative effect on proficiency in both national and international languages. Thus, exploring the effect of mother-tongue instruction on later labor market outcomes is an empirical question, which this paper attempts to answer. Specifically, this paper attempts to empirically document the effect of mother-tongue instruction in primary school on students' later labor market outcomes in a typical developing country.[2] Moreover, we document whether the duration of exposure to mother-tongue instruction in primary school matters in terms of its effect on later labor market outcomes.

Exploring the effect of the duration of exposure to mother-tongue instruction on later labor market outcomes is important, partly because the time spent on mother-tongue instruction comes directly from the time that could have been spent on English instruction. That is, students who are exposed to mother-tongue instruction for too long may have fewer years left to be instructed in English. Since it may take $5-7$ years to become fluent in English (e.g., Hakuta, 2000), this might have negative implication on later labor market outcomes of students who are exposed to mother-tongue instruction for too long. The estimate that it may take $5-7$ years to become fluent in English is for students who live in a country where English is a dominant language. It might take more years for students to become fluent in English if they live in a country where English is not widely spoken outside classrooms.

Studies that are closely related to ours are Angrist and Lavy (1997) and Eriksson (2014). Angrist and Lavy (1997) exploit a switch in language of instruction from French to Arabic in Moroccan schools and find negative effect of "Arabization" on returns to schooling. Eriksson (2014), on the other hand, exploits an increase in the duration of mother-tongue instruction from 4 and 6 years to 8 years in South African schools and finds positive effect of mother-tongue instruction on later labor market outcomes. The contrasting evidence in these two studies could be explained by differences in the scoio-economic conditions in Morocco and South Africa, but it is worth mentioning that Arabic is not the mother tongue of the Berbers, who account for the majority of Moroccan population. Thus, the switch to Arabic instruction can be considered as a move to mother-tongue instruction for a small fraction of Moroccan population. This is in contrast to the language policy change in South Africa that has increased the duration of exposure to mother-tongue instruction for black students.

Both of these studies are interesting and provide great insights, but they differ from the present study in a number of ways. To mention some: first, our study is conducted in a

[2]Given the data we have, however, we are not able to disentangle how much of its effect is through its positive effect on students' school performance and through its negative effect on language proficiency.

relatively poorer country. Besides, we exploit the change in medium of instruction in primary school from a dominant local language (i.e., Amharic[3]) to a number of other local languages, which in turn resulted in mother-tongue instruction in primary school for the majority of Ethiopian students. In Angrist and Lavy (1997), the change in medium of instruction is from French to Arabic, which does not seem to result in mother-tongue instruction for the majority of Moroccan students. Eriksson (2014), on the other hand, does not exploit the introduction of mother-tongue instruction per se in her identification strategy. She rather exploits the increase in the duration of exposure to mother-tongue instruction from 4 and 6 years to 8 years. Angrist and Lavy (1997), on the other hand, exploits the change in language of instruction in middle and secondary schools, not in primary school.

Second, unlike Ethiopia, both Morocco and South Africa were colonized by France and Britain, respectively. As a result, it seems that the language premium for a foreign language (i.e., French in Morocco and English in South Africa) in their respective labor markets could be higher. Since Ethiopia has never been colonized, the language premium for a foreign language (i.e., English) in the Ethiopian labor market could be relatively smaller, which has its own implication on the effect on later labor market outcomes of mother-tongue instruction in primary school, which could potentially hinder proficiency in English.

Third, unlike the studies by Angrist and Lavy (1997) and Eriksson (2014), the present study explores the (later labor market) impact of both mother-tongue instruction and the duration of exposure to mother-tongue instruction in primary school.

We are not aware of similar studies conducted in Ethiopia and much of Sub-Saharan Africa that convincingly document the causal effects of mother-tongue instruction (and the duration of exposure to mother-tongue instruction) in primary school on later labor market outcomes, particularly when the change in medium of instruction is from a dominant local language to other local languages. Documenting causality is particularly possible in this paper because we exploit the variation in exposure to mother-tongue instruction across birth cohorts and states[4] in Ethiopia which was created by the 1994 education reform.

Ethiopia has introduced mother-tongue instruction in primary school following the signing of the Education and Training Policy into law in 1994 (Ministry of Education, 1994). Thus, students who have attended primary school after 1994 were exposed to mother-tongue

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

[4]Ethiopia is a federal country with three levels of government: federal, state (or regional), and local. The country has nine states and two chartered cities. Zones are lower-level/local governments that can be considered as equivalent to US counties.

instruction in primary school whereas the majority of their peers who attended primary school prior to 1994 were not exposed to mother-tongue instruction at all. This has resulted in a variation in exposure to mother-tongue instruction in primary school by birth cohort.

On the other hand, the 1994 education reform has differential impact on mother-tongue instruction in primary school across states in Ethiopia for two main reasons. First, in Amhara state, primary schools have continued to use Amharic as their medium of instruction after the 1994 education reform. This is because the majority of residents in the state come from the Amhara ethnic group and, hence, their mother tongue is Amharic. In fact, Amharic is the official language of Amhara state – and that of the federal government of Ethiopia. This makes Amhara state unique in the sense that primary schools in the state use Amharic as a medium of instruction both before and after the 1994 education reform. The only exception to this are schools found in 3 different zones in Amhara state (i.e., Awi, Oromiya, and Wag Hemra zones) where they have changed their media of instruction from Amharic to other local languages in order to accommodate language-minority kids that predominantly reside in their respective zones.

Second, among other states in Ethiopia that have introduced mother-tongue instruction in primary school following the 1994 education reform, the duration of exposure to mother-tongue instruction varies depending on the state in which a student attends primary school. This is because states in Ethiopia mandate students to transition from mother-tongue to English instruction either in grade 5, 7, or 9.

In the econometrics analysis, thus, we exploit the variations in exposure to mother-tongue instruction in primary school across birth cohorts and states (as well as the variation in the *duration* of exposure to mother-tongue instruction among states that have introduced mother-tongue instruction) which were created as a result of the 1994 education reform and estimate difference-in-differences models using data from the 2013 Ethiopian Labor Force Survey.

Estimates from our preferred specifications suggest that mother-tongue instruction in primary school improves later labor market outcomes, particularly among private-sector employees. Specifically, individuals who were exposed to mother-tongue instruction in primary school are more likely to be employed (either in the private or public sector). And among individuals who are employed in the private sector, those who were exposed to mother-tongue instruction in primary school are more likely to be employed for permanent positions, earn more, and also more likely to report that they are satisfied with their current job (relative to those who were not exposed to mother-tongue instruction in primary school). We also

find out that the magnitude of the treatment effect depends on the duration of exposure to mother-tongue instruction in primary school, where the largest and smallest effects are observed for individuals who were exposed to mother-tongue instruction for 4 and 8 years, respectively. On the other hand, we find out that these effects are heterogenous by sector, where the treatment effect is the largest in the tertiary sector. Falsification tests suggest that our results are not confounded by other factors. Moreover, our results are robust to alternative data source.

Taken together, our findings are consistent with the argument that the positive effect of mother-tongue instruction in primary school on later labor market outcomes seems to come through its positive effect on academic achievement (e.g., Seid, 2016). But this positive effect could be outweighed by the negative effect of mother-tongue instruction on proficiency in national and international languages (e.g., Angrist and Lavy, 1997) if students are exposed to mother-tongue instruction for too long, resulting in the decrease in the gains in later labor market outcomes with increase in the duration of exposure to mother-tongue instruction.

The remainder of the paper is organized as follows. The following section provides a brief background on schooling and language in Ethiopia. Section 3 describes the data, while Section 4 discusses the estimation strategy and presents the econometric results. The final section concludes the study.

2. Schooling and Language in Ethiopia

The Ethiopian government signed the Education and Training Policy into law in 1994. Among other things, the 1994 education reform has given discretion to states in Ethiopia to adopt mother-tongue instruction in primary school.[5] Following this discretion, states in Ethiopia have adopted mother-tongue instruction in primary school, resulting in an increase in the languages of instruction in primary school from using Amharic as the only language of instruction in 1990 to about 25 languages in 2007 (Seidel and Moritz, 2007).[6] The only state that has continued to use Amharic as medium of instruction in primary school after the 1994 education reform is Amhara state (excluding Awi, Oromiya, and Wag Hemra zones) since Amharic is the official language of the state.

[5]Here, 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 states differently is the introduction of mother-tongue instruction in primary school. Moreover, no other education policy change that affects schools in treated and control states differently has been introduced during the period of analysis.

[6]See Seid (2016) for further discussion on the 1994 Ethiopian education reform and its effect on educational outcomes in primary school in Ethiopia.

The 1994 education reform, on the other hand, mandates students to transition from mother-tongue to English instruction after completing few years of primary schooling. However, the decision on the timing of transition to English instruction is left for state governments, resulting in a variation in the timing of transition to English instruction across students from different states in Ethiopia.[7]

Generally speaking, there are four broad groups of states in terms of the way they transition students in their state to English instruction. Students in the first group (i.e., Gambella and SNNP states) transition to English instruction in grade 5 after four years of mother-tongue instruction. Students in the second group (i.e., Addis Ababa, Afar, Benishangul-Gumuz, and Dire Dawa cities/states) transition to English instruction in grade 7. Students in the third group (i.e., Amhara, Harari, and Somali states) *partially* transition to English instruction in grade 7. That is, grade 7 students in these states start to learn science and mathematics in English while mother tongue continues to be the medium of instruction for the other subjects. Students in these states make the complete transition to English instruction in grade 9. Students in the fourth group (i.e., Oromiya and Tigray states) transition to English instruction in grade 9 after eight years of mother-tongue instruction.

The 1994 Education Reform as Exogenous Source of Variations

The 1994 Ethiopian education reform is a plausible exogenous source of variations in exposure to mother-tongue instruction in primary school across students from different birth cohorts and states. Since all states in Ethiopia have adopted mother-tongue instruction following the signing of the 1994 education reform, students in Ethiopia who have attended primary school after 1994 are more likely to be taught in their mother tongue. This is in stark contrast to the fact that the majority of their peers who attended primary school prior to 1994 were not taught in their mother tongue. This leads to a variation in exposure to mother-tongue instruction by birth cohort, where individuals are either fully exposed, partially exposed, or not exposed at all to mother-tongue instruction depending on the age of an individual in 1994, the year when the education reform was introduced.

Note that the official school starting age in Ethiopia is 7. Thus, individuals younger than 7 years in 1994 were fully exposed to mother-tongue instruction. Individuals aged $7-14$ in 1994, on the other hand, were partially exposed to mother-tongue instruction since they were already in primary school when the 1994 education reform was introduced. Finally,

[7]See Seid (2017) for a discussion on the impact of mother-tongue instruction in early grades on the performance of students later in primary school after they transition to English-instruction classrooms.

individuals older than 14 years in 1994 were not exposed to mother-tongue instruction at all.[8] For individuals surveyed in the 2013 Ethiopian Labor Force Survey, which is the primary source of data in this paper, this implies that individuals younger than 26 were fully exposed to mother-tongue instruction in primary school, those between 26 and 33 years old were partially exposed to mother-tongue instruction, and those older than 33 were not exposed to mother-tongue instruction at all.

In order to capture this variation in exposure to mother-tongue instruction by birth cohort in the econometrics analysis, we assign students who were fully exposed to mother-tongue instruction (and were at least 18 years in 2013) into *treated birth cohort* whereas those who were not exposed to mother-tongue instruction at all (and were younger than 42 years in 2013) are assigned into *control bith cohort*.[9] In our econometric analysis, we drop individuals who were partially exposed to mother-tongue instruction (i.e., those aged $26-33$ in 2013) from our sample of analysis to avoid potential contamination of our results due to individuals' partial exposure to mother-tongue instruction in primary school.

It is important to note that among individuals who were younger than 7 years in 1994 – and, hence, were fully exposed to mother-tongue instruction in primary school – the duration of their exposure to mother-tongue instruction varies by the state in which they have attended primary school. This is true for students who have attended primary school in all states in Ethiopia, except those in Amhara state (excluding Awi, Oromiya, and Wag Hemra zones) who have always been exposed to mother-tongue instruction in primary school irrespective of their birth cohorts. Thus, we assign Amhara state (excluding Awi, Oromiya, and Wag Hemra zones) into *control state*.

As mentioned earlier, students in other states in Ethiopia are, on the contrary, exposed to mother-tongue instruction in primary school depending on their birth cohort. However, the duration of their exposure to mother-tongue instruction in primary school varies by the

[8] Since some children delay primary school enrollment and repeat grade, some individuals who were older than 14 years in 1994 might be partially exposed to mother-tongue instruction in primary school. We checked the sensitivity of our results to potential bias due to delayed enrollment and grade retention by using an additional specification which uses a more disaggregated age group. The results from this specification (not reported here) are qualitatively similar to those reported in our main analysis in Section 4, suggesting that bias due to delayed enrollment and grade retention is not a serious concern in our study.

[9] More specifically, individuals aged $18-25$ and $34-41$ in 2013 are assigned into treated and control birth cohorts, respectively. Individuals younger than 18 years in 2013 are excluded since they might still be in school when the 2013 Labor Force Survey was conducted. Besides, this ensures that our analysis exclusively focuses on the labor market outcomes of adults by adopting a stricter cutoff age for child labor. On the other hand, we exclude individuals older than 41 years in 2013 so that the age range for individuals included in both treated and control birth cohorts is equal.

state in which they have attended primary school. This is because the duration of exposure to mother-tongue instruction in primary school is either 4, 6, or 8 years depending on the state in which students have attended primary school.

In order to capture the variation in the duration of exposure to mother-tongue instruction in primary school across states in the econometric analysis, we assign Gambella state into the *first group of treated state* since students in the state are exposed to mother-tongue instruction for the first 4 years of primary schooling. On the other hand, we assign Somali state into the *second group of treated state* since students in the state are exposed to mother-tongue instruction for the first 6 years of primary schooling. Students in this group partially transition to English instruction in grade 7 where they are taught science and mathematics in English in grades $7-8$ before they make the complete transition to English instruction in grade 9. Finally, we assign Oromiya and Tigray states into the *third group of treated states* since students in these states are exposed to mother-tongue instruction for the first 8 years of schooling before they transition to English instruction in grade 9.[10]

3. Data

The data used in this paper come from the 2013 Ethiopian Labor Force Survey (LFS) which was administered by Ethiopia's Central Statistical Agency (CSA).[11] The Ethiopian LFS is a nationally representative, repeated cross-section household survey which is designed to provide information on the Ethiopian labor force. Thus, LFS contains a wide range of information on household demographics, labor force participation, employment status, wages and benefits, and other important economic variables. So far, CSA has administered three rounds of LFS in 1999, 2005, and 2013. In the present study, however, we employ data that come from the 2013 LFS only. This is because students who have attended primary school after the 1994 education reform were too young to explore their labor market outcomes using data that come from the 1999 and 2005 survey rounds of the Ethiopian LFS.[12]

[10] Of the total 9 states and 2 federal cities in Ethiopia, our final sample includes observations from 5 states. See Section 3 for a discussion on our sample restrictions and the reasoning behind the sample restrictions.

[11] The 2013 LFS data are supplemented by data from the 2-percent, public-use microdata samples of the 2007 Ethiopian population census when we check the robustness of our results. See Appendix B for a brief description of the 2007 Ethiopian population census.

[12] As mentioned earlier, the official school starting age in Ethiopia is 7, implying that students who were too young to attend primary school when the 1994 education reform was signed into law were younger than 12 and 18 years in 1999 and 2005, respectively. This suggests that data from the 1999 and 2005 LFS are not best suited to explore the labor market outcomes of individuals who have attended primary school after the

A total of 240,660 individuals across all states in Ethiopia were surveyed in the 2013 LFS. However, we restrict our sample to individuals aged $18-25$ and $34-41$ in 2013 since, in this paper, only individuals in these age brackets are assigned into treated and control birth cohorts, respectively.[13]

It is worth mentioning that we observe individuals' current state of residence in the 2013 LFS data, but not the state in which individuals have actually attended primary school. This poses a potential problem since some people may work and live in a state different from the state in which they have attended primary school, making it difficult to observe whether an individual is exposed to mother-tongue instruction in primary school. This is a common problem in researches that attempt to link early childhood conditions to later labor market outcomes using cross-sectional data. In the present study, we address the problem by restricting our sample to individuals who have never migrated out of their home state. This restriction ensures that individuals in our sample have actually attended primary school in their current state of residence which in turn enables us to observe whether an individual surveyed in 2013 LFS was exposed (and if so, for how long) to mother-tongue instruction while she/he was in primary school.

Data from the 2013 LFS show that about 73 percent of individuals in the survey have never migrated out of their home state. Generally speaking, individuals who have chosen not to migrate may be inherently different from those who have chosen to migrate. For instance, individuals who have chosen not to migrate may be more risk averse, less able, and/or less competitive. In the present study, however, it is fair to say that restricting our sample to individuals who have never migrated should not cause a serious concern in the internal validity of our results since our data do not show any systematic differences in the patterns of migration by birth cohort and states' treatment status.[14] Disaggregating the internal migration by selected observable characteristics (i.e., by education level and gender) also shows that (results not reported here) there is no systematic differences in migration rates by these selected observable characteristics across birth cohort and states' treatment status.[15]

We further restrict the sample of analysis to individuals who lived in five selected

1994 education reform.

[13] See Section 2 for further discussion on how individuals are assigned into treated and control birth cohorts.

[14] See Table A.1 in Appendix A for the patterns of internal migration by birth cohort and states' treatment status

[15] Since individuals in our sample are a "selected" group who have chosen not to migrate, however, our findings may not be generalized to individuals who have chosen to migrate out of their home state at some point in their life.

Ethiopian states – i.e., Amhara (excluding Awi, Oromiya, and Wag Hemra zones), Gambella, Oromiya, Somali, and Tigray – in 2013. We drop observations from Afar and Benishangul-Gumuz states since these states, unlike other states in Ethiopia, have changed their education language policies more than once since the 1994 education reform was introduced, making it difficult to accurately observe the duration of individuals' exposure to mother-tongue instruction in primary school by their birth cohort.

Note that we do not observe the mother tongue of individuals surveyed in the 2013 LFS. Thus, we use the dominant language in the state in which an individual has attended primary school as a proxy for the individual's mother tongue. Admittedly, this is not a perfect proxy since it assumes that an individual's mother tongue is the same as the dominant language in the sate in which the individual attended primary school. Thus, we attempt to improve the predictive power of our proxy variable by dropping observations from states/cities that each has heterogeneous population in terms of the population's ethno-linguistic background (i.e., we drop observations from Harari and SNNP states and from the two federal cities, Addis Ababa and Dire Dawa[16]).

Given that we have restricted our sample to states in Ethiopia that each has a population that are relatively more homogenous in their ethno-linguistic background[17] and to individuals who have lived in their home state since birth, we believe that the dominant language in the state can be considered as a reasonably good proxy for individuals' mother tongue. In the part of the paper where we discuss robustness checks, we explore whether our results are sensitive to the choice of our proxy variable by employing a different data that collect information on individuals' mother tongue, i.e., data from the 2-percent, public-use microdata samples of the 2007 Ethiopian population census.

The other sample restriction we impose is that we drop individuals who have not completed primary education (i.e., 8 years of schooling). This is because states that are assigned into the third treatment group in our econometrics analysis are those states that have adopted mother-tongue instruction for the first 8 years of primary schooling. Since

[16]The ethno-linguistic diversity of Addis Ababa and Dire dawa partly explains why primary schools in these cities have continued to use Amharic as medium of instruction even after the 1994 education reform.

[17]The population in the states that are included in our final sample are relatively ethno-linguistically homogenous. Data from the 2007 Ethiopian population census reveal that the faction of population who are native speakers of Amharic in Amhara state (excluding Awi, Oromiya, and Wag Hemra zones) is 97 percent. The proportion of native speakers of Oromiffa in Oromiya state, Somali in Somali state, and Tigrigna in Tigray state are 89, 97, and 95 percent, respectively. Similarly, the same census data show that about 76 percent of the population in Gambella state are native speakers of the three dominant languages in the state (i.e., Agnuak, Nuer, and Mezhenger) which are also adopted as the only media of instruction in primary school in the state.

a student in these states can be exposed to 8 years of mother-tongue instruction only if the student has completed at least 8 years of schooling, we drop individuals who have not completed primary education from our sample of analysis.

These sample restrictions leave us with a final sample size of 11,306 individuals aged $18-25$ and 18,409 individuals aged $34-41$ across five states in Ethiopia.

We consider four labor market outcomes as our dependent variables. That is, probability of employment, and among those who are employed, we consider probabilities of permanent employment and whether an employee is satisfied with her/his current job and hourly earning as our dependent variables. In what follows, employment is measured by current activity status (i.e., activity status of an individual in the last 7 days). We have checked whether our results from the econometric analysis (that are presented in Section 4) differ if we measure employment by usual activity status (activity status of an individual for the most part of the last 12 months), and we found that our results are qualitatively similar whether employment is measured by current or usual activity status.

Table 1 presents descriptive statistics for a sample of individuals included in the econometric analysis by states' treatment status.[18] The labor market outcomes are presented separately for the private and public sectors. As the table shows, it seems that the labor market outcomes are generally better for private-sector employees since they earn more and are more likely to report that they are satisfied with their current job (relative to their peers in the public sector). Public-sector employees, on the other hand, are more likely to hold permanent positions than private-sector employees. This is common across states regardless of the states' treatment status.

On the other hand, Table 1 shows that individuals in our sample have no striking differences along observable characteristics across states. However, individuals in our sample have completed more years of schooling relative to the national average. This is not surprising given that we have restricted our sample to individuals who have completed at least 8 years of schooling.

4. The Impact of Mother-tongue Instruction on Later Labor Market Outcomes

In this paper, we are primarily interested in exploring the causal effect of mother-tongue instruction in primary school on students' later labor market outcomes. Documenting causality is complicated by the potential endogeneity of mother-tongue instruction since

[18] See Table A.2 in Appendix A for descriptive statistics of the labor market outcomes by birth cohort and the states' treatment status.

Table 1: Summary Statistics of Variables used in the Econometric Analysis by the States' Treatment Status

	Control State	Treated States		
		Treatment 1	Treatment 2	Treatment 3
Employment‡	0.872	0.866	0.852	0.839
	(0.324)	(0.305)	(0.309)	(0.316)
Permanent employment‡ - private sector	0.356	0.337	0.320	0.330
	(0.206)	(0.283)	(0.283)	(0.325)
Permanent employment‡ - public sector	0.558	0.533	0.538	0.547
	(0.194)	(0.112)	(0.409)	(0.226)
Hourly earning - private sector	48.616	45.784	40.648	42.903
	(18.074)	(14.747)	(15.633)	(15.226)
Hourly earning - public sector	28.571	30.349	33.824	30.204
	(12.328)	(9.653)	(7.200)	(8.101)
Job satisfaction‡ - private sector	0.566	0.493	0.567	0.549
	(0.411)	(0.524)	(0.333)	(0.297)
Job satisfaction‡ - public sector	0.453	0.457	0.424	0.435
	(0.298)	(0.283)	(0.251)	(0.344)
Female‡	0.502	0.504	0.485	0.493
	(0.497)	(0.500)	(0.499)	(0.501)
Married‡	0.566	0.575	0.583	0.553
	(0.496)	(0.494)	(0.493)	(0.497)
Years of schooling	10.267	9.806	10.162	9.993
	(1.901)	(1.757)	(2.103)	(1.893)
Work experience (in years)	10.325	10.474	10.103	10.349
	(6.595)	(5.757)	(5.836)	(6.283)
No. of under-five-years-old kids	2.966	2.919	3.117	2.943
	(0.935)	(0.926)	(0.752)	(0.967)
Primary sector‡	0.536	0.537	0.536	0.542
	(0.256)	(0.276)	(0.241)	(0.248)
Secondary sector‡	0.099	0.095	0.110	0.127
	(0.135)	(0.106)	(0.100)	(0.113)
Tertiary sector‡	0.375	0.370	0.354	0.330
	(0.233)	(0.249)	(0.210)	(0.208)
State-level unemployment rate (in percent)	4.826	5.409	6.311	4.782
	(0.000)	(0.000)	(0.000)	(0.916)
Urban‡	0.144	0.211	0.155	0.151
	(0.225)	(0.236)	(0.212)	(0.209)
Observations	9326	2282	3171	14936

Notes: Standard deviations are reported in parentheses.
‡ denotes binary indicator variable. Hourly earning is reported in Ethiopian Birr.
The control state is Amhara (excluding Awi, Oromiya, and Wag Hemra zones) whereas the treated states are Gambella (treatment 1), Somali (treatment 2), and Oromiya and Tigray (treatment 3).
The primary sector includes agriculture and mining; the secondary sector includes manufacturing, utilities, and construction; and the tertiary sector includes trade, transport, financial services, and community services.

parents who care more about their children's education may ensure that their children are instructed in their mother tongue at school. Parents can do so, for instance, by moving to districts where the language of instruction at school is the same as their children's mother tongue. Parents can also successfully lobby school administrators and local politicians to change the language of instruction in primary schools located in their districts.

This implies that estimates from naive OLS regression of labor market outcomes on whether an individual was instructed in her/his mother tongue in primary school are biased. In situations where educational language policies are exogenously introduced, they can be used as natural experiments to mitigate potential biases due to endogeneity of language of instruction. In the present study, thus, we exploit the different dimensions of variations that were created by a nation-wide introduction of an education reform in Ethiopia which was signed into law in 1994. In the subsections below, we discuss the empirical strategy, present the results from the econometric analysis, run falsification tests, explore whether there is heterogeneity in treatment effect, and, finally, check the robustness of our results to alternative data source.

4.1. Empirical Strategy

As an identification strategy, we exploit two plausibly exogenous sources of variations that were created by the 1994 education reform. First, we exploit the variation in exposure to mother-tongue instruction by birth cohort. Since only students who have attended primary school after 1994 are exposed to mother-tongue instruction in primary school, we assign individuals who were younger than 7 years (which is the official school starting age in Ethiopia) in 1994 into *treated birth cohort*. On the other hand, we assign students who were above primary-school age (i.e., above 14 years) in 1994 into *control birth cohort* since this group of students were not exposed to mother-tongue instruction at all.

The second source of variation is differences in exposure to mother-tongue instruction across states. Since Amhara state (excluding Awi, Oromiya, and Wag Hemra zones) is the only state in Ethiopia that has continued to use Amharic as the medium of instruction in primary school after the 1994 education reform, we assign individuals in Amhara state (excluding Awi, Oromiya, and Wag Hemra zones) into *control state*.

While constructing the treatment group, on the other hand, we categorize states into three different groups depending on the variation in the duration of exposure (of primary-school students) to mother-tongue instruction across states. Specifically, we assign Gambella state into the *first group of treated state* since students in the state are exposed to mother-tongue instruction for 4 years. Similarly, we assign Somali state into the *second group of*

treated state and Oromiya and Tigray states into the *third group of treated states* since students in these groups of states are exposed to mother-tongue instruction for 6 and 8 years, respectively. We model the labor market outcomes as:

$$Pr(y_{is} = 1) = G(\alpha_0 + \eta_0 TC_{is} + \tau_0 TS_s + \gamma_0 (TC_{is} * TS_s) + \boldsymbol{\beta}_1 \mathbf{X}_{is} + \psi), \tag{1}$$

where y_{is} denotes the labor market outcomes[19] of individual i in state s; TC_{is} is a dummy variable taking 1 for the treated birth cohort (i.e., for individuals aged $18-25$ in 2013), and 0 otherwise (i.e., for individuals aged $34-41$ in 2013); TS_s is a binary indicator taking 1 for treated states – i.e., Gambella state (treatment 1) or Somali state (treatment 2) or Oromiya and Tigray states (treatment 3) – $\mathbf{X}_{is}$ is a vector of control variables; and ψ denotes cohort-of-birth fixed effects.[20] As discussed earlier, we employ three groups of treated states in this paper and, hence, Equation (1) is estimated separately for each group of treated states.

The primary (explanatory) variable of interest is the interaction term, "$TC_{is} * TS_s$," and γ_0 captures the treatment effect, i.e., the effect on the labor market outcomes of an individual due to the individual was exposed to mother-tongue instruction in primary school (relative to those who were not exposed to mother-tongue instruction in primary school).

While estimating Equation (1),[21] the standard errors are clustered by enumeration area, a census tract, to account for correlation in the error terms within the enumeration area. 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).[22]

[19] As mentioned earlier, we employ four labor market outcomes as our dependent variables. That is, probability of employment, and among those who are employed, we consider probabilities of permanent employment and whether an employee is satisfied with her/his current job and hourly earning as our dependent variables.

[20] These fixed effects capture any secular changes occurring across Ethiopia in any given year.

[21] Note that Equation (1) is estimated for three of our labor market outcomes that are defined as binary indicators (i.e., probabilities of employment, permanent employment, and whether an employee is satisfied with her/his job). For the other outcome variable, i.e., hourly earning, we estimate the following equation:

$$log(y_{is}) = \alpha_0 + \eta_0 TC_{is} + \tau_0 TS_s + \gamma_0 (TC_{is} * TS_s) + \boldsymbol{\beta}_1 \mathbf{X}_{is} + \psi + \epsilon_{is}, \tag{2}$$

where ϵ_{is} is the idiosyncratic error term and all the other notations are as defined in Equation (1)

[22] The average marginal effects of the explanatory variables on the likelihoods of labor market 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.

4.2. Results

The results from the difference-in-differences regressions are presented in Tables 2 and 3. The last three columns of Table 2 present results from private-sector employees subsample whereas the first column presents results from all individuals in our sample. Table 3, on the other hand, presents results from public-sector employees subsample.[23] Since anecdotal evidence suggests that the languages that are more valued differ in the private and public sectors of the Ethiopian labor market,[24] it seems reasonable to run the difference-in-differences regressions separately for private- and public-sector employees subsamples.

In Tables 2 and 3, the control state is Amhara state (excluding Awi, Oromiya, and Wag Hemra zones) in all panels (i.e., the upper, middle, and lower panels). However, the treated group of states varies by panel, where in the upper panel the teated state is Gambella (where students are exposed to mother-tongue instruction for 4 years), in the middle panel the treated state is Somali (where students are exposed to mother-tongue instruction for 6 years), and in the lower panel the treated states are Oromiya and Tigray (where students are exposed to mother-tongue instruction for 8 years).

Let us first discuss the results from the difference-in-differences regressions which are presented in Table 2. As can be seen from the upper panel of the table, the coefficient estimates of the primary (explanatory) variable of interest, $Treated\ cohort * Treated\ state$, is positive and statistically significant in column 1, suggesting exposure to mother-tongue instruction in primary school for 4 years increases the probability of employment (either in the private or public sector). Similarly, the last 3 columns of Table 2 show that the treatment effect is uniformly positive and statistically significant at 1 percent level in the upper panel of the table. This suggests that individuals who were exposed to mother-tongue instruction in primary school for the first 4 years perform better in the labor market later in life (relative to those who were not exposed to mother-tongue instruction in primary school). Specifically, among individuals who are employed in the private sector, those who were exposed to mother-tongue instruction in primary school for 4 years are more likely to be employed for permanent positions, earn more, and also more likely to report that they are satisfied with their current job.

[23]For the purpose of this study, an employee is considered as a private-sector employee if she/he works in a private firm/organization or non-governmental organization (including international organization). On the other hand, an employee is considered as a public-sector employee if she/he works in the government or government parastatal.

[24]In the public sector, it seems both Amharic, the official language of the federal government, and other local languages, mostly the official language of the state, are more valued. In the private sector, on the other hand, English (and to some extent Amharic and the official language of the state) seem to be more valued.

Table 2: Difference-in-Differences Estimates of the Effect of Mother-tongue Instruction on Later Labor Market Outcome

	Full sample	Private-sector employees subsample		
	Emp't	Perm. emp't	Log(earning)	Job satis.
Treatment 1				
Treated cohort	0.019***	0.019***	0.028***	0.109***
	(0.005)	(0.003)	(0.006)	(0.016)
Treated state	0.008**	0.003***	0.021*	0.124
	(0.004)	(0.001)	(0.011)	(0.171)
Treated cohort*Treated state	0.043***	0.018***	0.072***	0.187***
	(0.012)	(0.004)	(0.009)	(0.021)
Control variables	Yes	Yes	Yes	Yes
Observations	11608	6196	6196	6196
Treatment 2				
Treated cohort	0.023***	0.011***	0.031***	0.098***
	(0.008)	(0.002)	(0.006)	(0.013)
Treated state	0.012***	0.009***	0.040	0.091**
	(0.003)	(0.003)	(0.049)	(0.043)
Treated cohort*Treated state	0.031***	0.007***	0.049***	0.132**
	(0.007)	(0.002)	(0.014)	(0.054)
Control variables	Yes	Yes	Yes	Yes
Observations	12497	7406	7406	7406
Treatment 3				
Treated cohort	0.018***	0.006***	0.022***	0.116***
	(0.006)	(0.002)	(0.007)	(0.018)
Treated state	0.007***	0.003***	0.014***	0.083***
	(0.002)	(0.001)	(0.003)	(0.014)
Treated cohort*Treated state	0.016**	0.006	0.032	0.111
	(0.008)	(0.004)	(0.023)	(0.070)
Control variables	Yes	Yes	Yes	Yes
Observations	24262	13979	13979	13979

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 binary indicators for gender and whether an individual is married, years of schooling, work experience, work experience squared, number of under-five-years-old kids in the household, binary indicator for urban, state-level unemployment rate, and cohort-of-birth fixed effects. For regressions that are run for employees subsample, we include binary indicators for whether an individual works in tertiary or secondary sector (primary sector is the omitted group) as additional controls.

Estimates reported in the middle panel of Table 2 depict a similar picture since the coefficient estimates of the interaction term are positive and statistically significant. This, again, implies that exposure to mother-tongue instruction in primary school for the first 6 years (and then being partially transitioned to English instruction in grades $7-8$) help improve the labor market outcomes of individuals later in life compared to those who were not exposed to mother-tongue instruction in primary school. The lower panel of Table 2, on the contrary, tells us a mixed story. Here, the interaction term is positive and statistically significant at 5 percent level only in the first column where the dependent variable is the probability of employment (either in the private or public sector). In the last three columns of Table 2, however, its coefficient estimates are not significant. We can interpret this as the positive effect of mother-tongue instruction in primary school (on later labor market outcomes) decreases with years of exposure to mother-tongue instruction. This argument is further reinforced by the fact that the magnitude of the coefficient estimates of the interaction term are higher in the upper panel relative to that of the middle panel across all columns.

Taken together, Table 2 suggests that mother-tongue instruction in primary school improves the labor market outcomes of students later in life. However, the magnitude of the treatment effect depends on the duration of exposure to mother-tongue instruction in primary school. Even if our data do not allow us to calculate the optimal years of exposure to mother-tongue instruction that maximizes later labor market outcomes, the results reported in Table 2 suggest that exposure to mother-tongue instruction for 4 years yields the maximum returns in the Ethiopian labor market, followed by 6 and 8 years of exposure to mother-tongue instruction, respectively. In fact, Table 2 suggests that the evidence is not convincingly conclusive whether exposure to mother-tongue instruction in primary school for 8 years is better than not being exposed to mother-tongue instruction at all in terms of improving later labor market outcomes.

The findings presented in Table 2 are consistent with the argument that the gains in academic achievement in primary school due to mother-tongue instruction (e.g., Seid, 2016) could be outweighed by the decrease in proficiency in national and international languages (e.g., Angrist and Lavy, 1997) if students are exposed to mother-tongue instruction for too long, resulting in the decrease in the gains in later labor market outcomes with increase in the duration of exposure to mother-tongue instruction.

When we look at the results for the public-sector employees, which are presented in Table 3, they tell us a different story. Unlike the case for the private-sector employees, mother-tongue instruction in primary school does not seem to affect later labor market

Table 3: Difference-in-Differences Estimates of the Effect of Mother-tongue Instruction on Later Labor Market Outcome

	Public-sector employees subsample		
	Perm. emp't	Log(earning)	Job satis.
Treatment 1			
Treated cohort	0.013***	0.033***	0.099***
	(0.004)	(0.009)	(0.022)
Treated state	0.081*	0.025*	0.101*
	(0.047)	(0.014)	(0.061)
Treated cohort*Treated state	0.020**	0.055	0.092
	(0.009)	(0.038)	(0.108)
Control variables	Yes	Yes	Yes
Observations	3798	3798	3798
Treatment 2			
Treated cohort	0.015**	0.044***	0.062***
	(0.006)	(0.015)	(0.024)
Treated state	0.013***	0.038***	0.007***
	(0.003)	(0.014)	(0.002)
Treated cohort*Treated state	0.009**	0.058	0.163
	(0.004)	(0.061)	(0.137)
Control variables	Yes	Yes	Yes
Observations	3328	3328	3328
Treatment 3			
Treated cohort	0.013***	0.016***	0.109
	(0.005)	(0.005)	(0.136)
Treated state	0.005**	0.013***	0.112
	(0.002)	(0.004)	(0.099)
Treated cohort*Treated state	0.013	0.041	0.197
	(0.029)	(0.031)	(0.182)
Control variables	Yes	Yes	Yes
Observations	6886	6886	6886

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 binary indicators for gender and whether an individual is married, years of schooling, work experience, work experience squared, number of under-five-years-old kids in the household, binary indicator for urban, state-level unemployment rate, cohort-of-birth fixed effects, and binary indicators for whether an individual works in tertiary or secondary sector (primary sector is the omitted group).

outcomes for public-sector employees. In Table 3, the coefficient estimate of the interaction term is statistically significant (at 5 percent level) in the upper and middle panels of the table only when the dependent variable is the probability of permanent employment.

Our finding that the labor market return to mother-tongue instruction in primary school is higher in the private sector than in the public sector is consistent with the argument that English (and to some extent Amharic) are valued more in the private sector than in the public sector in the Ethiopian labor market. This is not surprising since state governments in Ethiopia have adopted the dominant local languages in their respective states as the working languages of their governments. Besides, anecdotal evidence from Ethiopia seems to suggest that the premium for proficiency in English is larger in the private sector than in the public sector.

4.3. Falsification Tests

In the main analysis, which is presented here above, we have documented positive treatment effect. We have then argued that the treatment effect is driven by the differential effect of the 1994 education reform across birth cohorts and states. This implicitly assumes that the difference-in-differences estimates pick up the treatment effect of the 1994 education reform (i.e., exposure to mother-tongue instruction in primary school) and not the effect of other potential factors that may affect later labor market outcomes even in the absence of the 1994 education reform and exposure to mother-tongue instruction in primary school.[25] To assess the validity of this claim, we conduct falsification tests and present the results in Tables 4 and 5.

In the falsification tests, we keep the control birth cohort group (i.e., individuals aged $34-41$ in 2013) as it is, but we construct a *placebo* treated birth cohort group and estimate the difference-in-differences model using the placebo treated birth cohort group (instead of using the "true" treated birth cohort group which includes individuals aged $18-25$ in 2013). For the treated birth cohort group to be considered as a placebo, individuals in this group should not be exposed to mother-tongue instruction in primary school. Thus, we have constructed a placebo treated birth cohort group which consists of individuals aged $42-49$ in 2013. Clearly, individuals in the placebo treated birth cohort group are older than those in the control birth cohort group and, hence, they are not exposed to mother-tongue

[25]Note that we also control for state-level unemployment rate and cohort-of-birth fixed effects in the regression framework, which presumably control for differences in macroeconomic trends between treated and control states. Thus, our results are less likely to be confounded by differential macroeconomic trends between treated and control states during the period of analysis.

instruction in primary school.[26]

Since we are comparing two groups of birth cohorts where all individuals in both groups are not exposed to mother-tongue instruction in primary school, we expect to find no treatment effect in the falsification test analysis if the positive treatment effect documented in Tables 2 and 3 is driven by the fact that individuals are exposed to mother-tongue instruction while they were in primary school, and not by other confounding factors.

The results from the falsification tests are presented in Tables 4 and 5. As can be seen from the tables, the coefficient estimates of the interaction term (*Treated cohort*, *placebo* $*$ *Treated state* variable) are uniformly statistically insignificant in all columns and panels of Tables 4 and 5. This confirms that the positive treatment effect presented in the main analysis is driven by the fact that individuals are exposed to mother-tongue instruction while they were in primary school, and not by other confounding factors.

4.4. Heterogeneity

So far we have documented positive effect of mother-tongue instruction in primary school on later labor market outcomes. Moreover, we have shown that the treatment effect varies by the duration of exposure to mother-tongue instruction, where the largest and smallest effects are observed for individuals who were exposed to mother-tongue instruction for 4 and 8 years in primary school, respectively. Prior studies suggest that mother-tongue instruction improves educational outcomes in primary school (e.g., Seid, 2016), but it has a potential to limit students' labor market opportunities later in life as it makes students less proficient in both national and international languages (e.g., Angrist and Lavy, 1997). Our findings seem to suggest that both of these forces are at play here even if our research is unable to identify the exact mechanisms of the treatment effect.

In addition to exploring the effect of mother-tongue instruction in primary school on later labor market outcomes, it is interesting to further explore whether the results documented in the present study are heterogeneous across different groups. One way to do this is to look at the differences in the results by economic sector, education level, gender, and location of residence. Since we have documented in the present study that the treatment effect is positive and statistically significant only for private-sector employees, our heterogeneity analysis primarily focuses on this group of employees.

[26]For uniformity, we exclude individuals older than 49 years from the placebo treated birth cohort group so that the age range in both the control birth cohort group and the placebo treated birth cohort group is equal.

Table 4: **Falsification Tests:** Difference-in-Differences Estimates of the Effect of Mother-tongue Instruction on Later Labor Market Outcome

	Full sample	Private-sector employees subsample		
	Emp't	Perm. emp't	Log(earning)	Job satis.
Treatment 1				
Treated cohort, placebo	0.011*	0.008**	0.012*	0.041***
	(0.006)	(0.004)	(0.007)	(0.012)
Treated state	0.015**	0.009*	0.017	0.088
	(0.006)	(0.005)	(0.013)	(0.057)
Treated cohort, placebo*Treated state	0.128	0.121	0.181	0.139
	(0.102)	(0.085)	(0.167)	(0.103)
Control variables	Yes	Yes	Yes	Yes
Observations	15911	8423	8423	8423
Treatment 2				
Treated cohort, placebo	0.016**	0.015	0.057*	0.081
	(0.007)	(0.011)	(0.030)	(0.051)
Treated state	0.010**	0.011	0.031**	0.012***
	(0.005)	(0.007)	(0.014)	(0.003)
Treated cohort, placebo*Treated state	0.083	0.062	0.096	0.100
	(0.081)	(0.062)	(0.069)	(0.085)
Control variables	Yes	Yes	Yes	Yes
Observations	16468	10072	10072	10072
Treatment 3				
Treated cohort, placebo	0.008***	0.012***	0.013	0.067
	(0.002)	(0.003)	(0.019)	(0.061)
Treated state	0.017***	0.006***	0.018**	0.103**
	(0.006)	(0.001)	(0.008)	(0.047)
Treated cohort, placebo*Treated state	0.077	0.039	0.166	0.174
	(0.062)	(0.043)	(0.126)	(0.127)
Control variables	Yes	Yes	Yes	Yes
Observations	28720	16002	16002	16002

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 binary indicators for gender and whether an individual is married, years of schooling, work experience, work experience squared, number of under-five-years-old kids in the household, binary indicator for urban, state-level unemployment rate, and cohort-of-birth fixed effects. For regressions that are run for employees subsample, we include binary indicators for whether an individual works in tertiary or secondary sector (primary sector is the omitted group) as additional controls.

Table 5: **Falsification Tests:** Difference-in-Differences Estimates of the Effect of Mother-tongue Instruction on Later Labor Market Outcome

	Public-sector employees subsample		
	Perm. emp't	Log(earning)	Job satis.
Treatment 1			
Treated cohort, placebo	0.024***	0.056***	0.156
	(0.006)	(0.012)	(0.126)
Treated state	0.039**	0.023*	0.027
	(0.004)	(0.012)	(0.018)
Treated cohort, placebo*Treated state	0.094	0.145	0.186
	(0.073)	(0.119)	(0.136)
Control variables	Yes	Yes	Yes
Observations	5061	5061	5061
Treatment 2			
Treated cohort, placebo	0.023	0.030*	0.101
	(0.017)	(0.016)	(0.099)
Treated state	0.027	0.036***	0.011
	(0.028)	(0.008)	(0.010)
Treated cohort, placebo*Treated state	0.028	0.097	0.146
	(0.020)	(0.085)	(0.148)
Control variables	Yes	Yes	Yes
Observations	4221	4221	4221
Treatment 3			
Treated cohort, placebo	0.028	0.017***	0.165
	(0.020)	(0.003)	(0.103)
Treated state	0.017**	0.008**	0.118
	(0.007)	(0.004)	(0.129)
Treated cohort, placebo*Treated state	0.042	0.136	0.129
	(0.029)	(0.134)	(0.142)
Control variables	Yes	Yes	Yes
Observations	8138	8138	8138

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 binary indicators for gender and whether an individual is married, years of schooling, work experience, work experience squared, number of under-five-years-old kids in the household, binary indicator for urban, state-level unemployment rate, cohort-of-birth fixed effects, and binary indicators for whether an individual works in tertiary or secondary sector (primary sector is the omitted group).

In the heterogeneity analysis, we first run difference-in-differences regressions separately for different subsamples by economic sector (primary versus secondary versus tertiary sectors[27]) and education level (those with high-school education versus those with above high-school education) to check whether there is heterogeneity of the treatment effect. The results from this analysis[28] suggest that there is no heterogeneity in treatment effect by education level. However, we find out that there is heterogeneity in treatment effect by sector, where the treatment effect is the largest for employees in the tertiary sector whereas its effect is the smallest for employees in the primary sector. This finding is consistent with the argument that the premium for (English and possibly Amharic) languages is larger in the tertiary sector, which is not surprising considering that prior anecdotal evidence from Ethiopia and other countries highlighted that language premium in the labor market is generally higher in the service sector.

We have also conducted a similar experiment to investigate whether the results are heterogeneous by gender and location of residence (urban versus rural) by running the difference-in-differences regressions separately for relevant, comparable subsamples. The coefficient estimates of the interaction term (which are not reported here in the interest of space) are not systematically different for relevant subsamples, implying that there is no heterogeneity in treatment effect by gender and location of residence.

4.5. Robustness of Our Results to Alternative Data Source

As discussed earlier, the analysis so far uses data from the 2013 LFS. Data from the Ethiopian LFS seem to best suit to answer question related to labor market performance since it collects a wide range of information on labor market outcomes and demographic characteristics. The 2013 LFS, however, do not collect information on individuals' mother tongue. So far, thus, we use the dominant language in the state of residence of an individual as a proxy for the individual's mother-tongue. Since we have restricted our sample to states that are relatively more (ethno-linguistically) homogeneous and to individuals who have lived in their home state since birth, we believe the dominant language in the state can be considered as a reasonably good proxy for individuals' mother tongue – see Section 3 for further discussion on this.

[27] As mentioned earlier, for the purpose of this study, primary sector includes agriculture and mining; secondary sector includes manufacturing, utilities, and construction; and tertiary sector includes trade, transport, financial services, and community services.

[28] In the interest of space, we do not present results from the heterogeneity analysis here, but they could be found on the supplementary online appendix.

To check whether our results are sensitive to the fact that we do not actually observe individuals' mother tongue, we repeat the difference-in-differences analysis using an alternative data source: data from the 2-percent, public-use microdata samples of the 2007 Ethiopian population census. The primary advantage of using the census data is information on individuals' mother tongue is collected.[29] Thus, we can use information on individuals' mother tongue and the language that is adopted as medium of instruction in primary school in a given state to clearly observe whether an individual is actually being exposed (and if so, for how long) to mother-tongue instruction while the individual was in primary school.

Using the 2007 census data, however, has two shortcomings compared to using the 2013 LFS. First, as it is common in most population censuses, the range of information gathered in the census is limited. Thus, we employ the probability of employment as the only labor market outcome. Second, the most recent census in Ethiopia was conducted in 2007, a year which is (relatively) not that far apart from 1994, the year mother-tongue instruction was introduced. This implies that individuals who were too young to attend primary school in 1994 were younger than 20 years in 2007. Thus, individuals that can be included in the treated birth cohort are those aged $18-19$ in 2007 census[30] (the age range when we use data from the 2013 LFS is $18-25$). To ensure that the age range for individuals in the treated and control birth cohorts are equal, we have assigned individuals aged $28-29$ in 2007 (i.e., those who were too old to be affected by the 1994 education reform) into control birth cohort.

Except the differences mentioned in the above two paragraphs, all the other aspects of the difference-in-differences analysis presented here in this subsection are the same as those presented in the main analysis. The results from the robustness analysis that use data from the 2-percent, public-use microdata samples of the 2007 Ethiopian population census are presented in Table 6.

Table 6 shows that the coefficient estimates of the interaction term, $Treated\ cohort * Treated\ state$, are positive and statistically significant at 1 percent level in all panels. This implies that the results presented in Table 6 confirm what we have found and discussed in the main analysis. That is, mother-tongue instruction in primary school improves the labor market outcomes of students later in life. Moreover, the size of the effect depends on the duration of exposure to mother-tongue instruction in primary school where the largest and smallest effects are observed for individuals who were exposed to mother-tongue instruction

[29] See Appendix B for a brief description of the 2007 Ethiopian population census.

[30] It must be noted that, however, small sample size is not an issue here since we use census data.

Table 6: Difference-in-Differences Estimates of the Effect of Mother-tongue Instruction on Employment (Using Census Data)

	Emp't
Treatment 1	
Treated cohort	0.007**
	(0.003)
Treated state	0.005**
	(0.002)
Treated cohort*Treated state	0.021***
	(0.006)
Control variables	Yes
Observations	46529
Treatment 2	
Treated cohort	0.010
	(0.007)
Treated state	0.009**
	(0.004)
Treated cohort*Treated state	0.013***
	(0.002)
Control variables	Yes
Observations	58306
Treatment 3	
Treated cohort	0.005**
	(0.002)
Treated state	0.003***
	(0.001)
Treated cohort*Treated state	0.006***
	(0.001)
Control variables	Yes
Observations	112448

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 binary indicators for gender and whether an individual is married, years of schooling, potential work experience, potential work experience squared, number of under-five-years-old kids in the household, binary indicator for urban, state-level unemployment rate, and cohort-of-birth fixed effects.

for 4 and 8 years in primary school, respectively.

5. Conclusions

A number of developing countries have adopted mother-tongue instruction in primary school in recent years. This is partly due to political considerations and partly it is to accommodate language-minority kids. In line with this, there is a growing evidence on the effect of mother-tongue instruction on school performance. However, our understanding on whether mother-tongue instruction has long-lasting effect on students' later labor market outcomes is limited. In this paper, thus, we attempt to fill this gap in the literature by exploring the effect of mother-tongue instruction (and the duration of exposure to mother-tongue instruction) in primary school on students' later labor market outcomes using the 2013 LFS data from Ethiopia.

Ethiopia provides an ideal setting to explore this question since it has adopted mother-tongue instruction in primary school following the signing of the 1994 education reform. Partly due to the ethno-linguistic diversity of people in Ethiopia, the 1994 education reform has resulted in a variation in exposure to mother-tongue instruction across states and birth cohorts. We exploit these plausibly exogenous variations and estimate difference-in-differences models.

Estimates from our preferred specifications suggest that mother-tongue instruction in primary school improves later labor market outcomes (particularly among private-sector employees), but the size of its effect decreases with the number of years an individual was exposed to mother-tongue instruction in primary school. The findings in this paper are consistent with the argument that the positive effect of mother-tongue instruction in primary school on later labor market outcomes seems to primarily come through its positive effect on academic achievement. But, as a student's exposure to mother-tongue instruction increases, the positive effect of mother-tongue instruction through gain in academic achievement could be outweighed by the negative effect of mother-tongue instruction on proficiency in national and international languages, resulting in the decrease in the gains in later labor market outcomes with increase in the duration of exposure to mother-tongue instruction.

The findings from the present study have important policy implications since they highlight that the choice of medium of instruction in primary school can set students from different language groups to different trajectories in life through its effect on students' later labor market outcomes. However, the suggested mechanisms of treatment effect should be treated carefully since our data do not allow us to thoroughly explore potential mechanisms

further.

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Appendices

Appendix A. Additional Tables

Table A.1: Out-of-state Migration Rates by Birth Cohort and Treatment Status of States in Ethiopia

	Control State	Treated States		
		Treatment 1	Treatment 2	Treatment 3
Treated Birth Cohort	0.267	0.255	0.273	0.262
	(0.387)	(0.346)	(0.283)	(0.344)
Control Birth Cohort	0.259	0.245	0.261	0.258
	(0.332)	(0.321)	(0.271)	(0.299)
Observations	9326	2282	3171	14936

Notes: Standard deviations are reported in parentheses.
Treated and control birth cohorts are individuals aged $18-25$ and $34-41$ in 2013, respectively. On the other hand, the control state is Amhara (excluding Awi, Oromiya, and Wag Hemra zones) whereas the treated states are Gambella (treatment 1), Somali (treatment 2), and Oromiya and Tigray (treatment 3).

Appendix B. Supplementary Data

As discussed earlier in the main text, the data used in this paper come from the 2013 LFS. As it is common for all household surveys administered by the Ethiopian CSA, however, information on individuals' mother tongue is not collected in the 2013 LFS. Thus, we supplement the 2013 LFS data by data from the 2-percent, public-use microdata samples of the 2007 Ethiopian population census to check the robustness of our results to alternative data source.[31]

The 2007 census is the most recent population census in Ethiopia and was administered by the Ethiopian CSA. For population censuses, CSA administers two types of questionnaires: short-form and long-form, where the difference between these two forms of questionnaires is the number of questions included. The short-form questionnaire is designed to capture information on basic household demographic characteristics such as gender, age, and mother tongue. The long-form questionnaire, on the other hand, contains all the questions included in the short-form questionnaire and some additional questions that help gather information on disability, education, economic activity, migration, fertility, mortality, and housing conditions.

The long-form questionnaire is administered to one-in-five random samples of Ethiopian households and is used as a source 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 percent (i.e., 20 percent*10 percent) of Ethiopian households. This

[31] The discussion in the following paragraphs draws on Seid (2016).

Table A.2: Summary Statistics of Dependent Variables used in the Econometric Analysis by Birth Cohort and the States' Treatment Status

	Control State		Treated States					
	Control: aged		Treatment 1: aged		Treatment 2: aged		Treatment 3: aged	
	$18-25$	$34-41$	$18-25$	$34-41$	$18-25$	$34-41$	$18-25$	$34-41$
Employment‡	0.893	0.859	0.931	0.823	0.904	0.822	0.875	0.817
	(0.286)	(0.265)	(0.328)	(0.299)	(0.308)	(0.394)	(0.326)	(0.389)
Permanent employment‡ - private sector	0.359	0.354	0.363	0.320	0.335	0.311	0.341	0.324
	(0.211)	(0.267)	(0.326)	(0.389)	(0.306)	(0.387)	(0.301)	(0.335)
Permanent employment‡ - public sector	0.563	0.555	0.544	0.526	0.533	0.541	0.558	0.541
	(0.162)	(0.183)	(0.116)	(0.105)	(0.456)	(0.358)	(0.236)	(0.292)
Hourly earning - private sector	49.073	47.152	49.323	43.400	43.123	39.224	44.562	41.870
	(20.623)	(18.320)	(15.571)	(14.428)	(16.482)	(17.807)	(15.867)	(14.301)
Hourly earning - public sector	27.699	29.101	30.826	30.020	34.280	33.566	31.466	29.422
	(8.618)	(10.341)	(10.258)	(11.396)	(6.397)	(7.877)	(7.553)	(7.224)
Job satisfaction‡ - private sector	0.583	0.556	0.623	0.406	0.614	0.540	0.571	0.536
	(0.426)	(0.468)	(0.333)	(0.359)	(0.298)	(0.326)	(0.153)	(0.258)
Job satisfaction‡ - public sector	0.449	0.455	0.452	0.461	0.443	0.413	0.449	0.427
	(0.301)	(0.286)	(0.288)	(0.271)	(0.231)	(0.248)	(0.326)	(0.307)
Observations	3544	5782	913	1369	1173	1998	5676	9260

Notes: Standard deviations are reported in parentheses. ‡ denotes binary indicator variable. Hourly earning is reported in Ethiopian Birr. Treated and control birth cohorts are individuals aged $18-25$ and $34-41$ in 2013, respectively. On the other hand, the control state is Amhara (excluding Awi, Oromiya, and Wag Hemra zones) whereas the treated states are Gambella (treatment 1), Somali (treatment 2), and Oromiya and Tigray (treatment 3).

gives us about 1.3 million observations in the 2-percent, public-use microdata samples of the 2007 census. However, we apply all the sample restrictions discussed in the Data section (and the additional sample restrictions discussed in the subsection that discusses robustness checks) to arrive at the final sample that is used in the econometrics analysis presented in the subsection that presents results from robustness checks.