Impact of English instruction on labor market outcomes: The case of Mexico*

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Abstract

In this paper, we measure the effect of exposure to a foreign language in school on student achievement and labor market outcomes. We exploit a policy change in Mexico that introduced English instruction in elementary schools through the National English Program in Basic Education (NEPBE) in 2009. We construct a novel database, which contains nationwide information on elementary school students linked to school panel data on characteristics like hours of English instruction as well as their labor market records in adulthood. Using a Two-Way Fixed Effects (TWFE) model, we find that exposure to English instruction reduces the likelihood that an individual participates in formal sector employment. It is likely that this result is due to exposure affecting enrollment in high school and college, as my analysis focuses on young adults aged 16-24. Focusing on a sub-sample that is unlikely to be enrolled by age 16, we find that exposure to English instruction has no effect on wages. However, we do find a positive effect among high-achieving individuals. On the other hand, exposure reduced men's mobility but increased women's. This could be explained by women substituting jobs in agriculture for manufacturing industries. Furthermore, within manufacturing, we find a strong substitution of low-English intensive jobs for high-English intensive ones. We also evaluate the effect of exposure to English instruction on students' achievement to determine if part of the effect on wages is due to a reallocation of resources towards English instruction in primary schools, which can potentially affect the formation of human capital. We find no effects on reading and mathematics test scores, which suggests that the estimated effect of exposure to English language on wages is not reflecting changes to general cognitive skills.

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Introduction

Is exposure to a foreign language conducive to better labor market outcomes? Intuitively, the answer to this question depends on whether exposure leads to the acquisition of foreign language skills, affects other cognitive abilities, or individuals' occupational decisions. However, this is an open question from which the existing literature has provided limited casual evidence. One of the most interesting case study is the English language. In the context of a globalizing world in which English is the lingua franca, there has been a growing interest from most developing countries in offering English instruction at school under the assumption that this exposure will increase English-language skills and ultimately improve labor market outcomes. While there is a large literature on the effect of English skills on earnings in English-speaking countries (see Bleakley and Chin (2004); and Chiswick and Miller (2015) for a review), there is little research on the returns to English skills in developing economies. Much of the existing evidence in developing countries has been studied in the context of former British colonies such as India and South Africa (Azam, Chin and Prakash, 2013; Eriksson, 2014), which are different from the rest of the non-English-speaking countries.

In this paper, we offer some of the first empirical evidence on the effect of exposure to English instruction in elementary school on labor market outcomes in the context of a non-English-speaking country; Mexico. For Mexicans, the value of the English language is even more pronounced than in other countries due to its close relationship with the United States (US) in terms of trade and migration. In particular, we exploit a policy change in Mexico introducing English instruction in elementary schools through the National English Program in Basic Education (NEPBE). This program provides a unique setting where children had exposure to the English language as a subject and not through a change in the medium of instruction as studied by the existing literature (Angrist and Lavy, 1997; Angrist, Chin and Godoy, 2008). We address threats to identification on two dimensions: school's self-selection and sample selection. The former is due to the omitted variables problem. The latter, on the other hand, is a result of observing only young formal workers, whose peers are more likely to be enrolled in school if they had exposure.

To conduct my research, we construct a novel database, which contains all public-school students in Mexico linked to information about the elementary school they attended as well as their labor market records in adulthood. The data on the universe of Mexican public elementary schools is rich with unique measures of school inputs specific to English instruction including the number of hours of weekly English instruction and the number of English teachers at the school. This enables direct measurement of exposure to English instruction in a way that distinct from previous studies limited by data constraints.

Using a Two-Way Fixed Effects (TWFE) model, which incorporates school fixed effects (FE) and cohort FE, we find that exposure to English instruction reduces the likelihood that an individual participates in formal sector employment. It is likely that this result is due to exposure affecting enrollment in high school and college, as my analysis focuses on young adults aged 16-24 (the recency of the NEPBE means the affected cohorts are still young). Note that the school FE address the first selection problem (due to omitted variables) by controlling for time-invariant characteristics of schools and local neighborhoods that may be correlated with the exposure to English instruction for individuals and their labor market opportunities. Furthermore, we also mitigate the omitted variables problem by controlling for student ability, measured as Math test scores in sixth grade.

Focusing on a sub-sample that is unlikely to be enrolled in school by age 16, we find that exposure to English instruction does not affect wages of the average Mexican worker. On the other hand, exposure reduces men's geographical mobility and increases women's mobility. This gender heterogeneity in mobility is explained by reallocation of workers in certain economic industries. For example, men substitute jobs in construction for manufacturing, while women substitute jobs in agriculture for manufacturing industries. In other words, women are moving from rural to more urban areas. Likewise, we show evidence that individuals who had exposure to English instruction moved from low-English to high-English intensive jobs in both, manufacturing and services industries.

Furthermore, we find heterogeneous wage effects by abilities. In particular, we show that exposure to English instruction increases wages of only high-achieving kids. Sample selection bias may not be able to be fully accounted for among the sample of students in the highest ability quartile. This remaining sample selection suggests that high-achieving individuals, living in low-enrollment counties, seek to pursue a college degree even despite the lack of educational opportunities. There is no evidence of heterogeneous effects on geographical mobility by cognitive abilities.

Among the mechanisms explored, we evaluate the effect of exposure to English instruction on students' achievement to determine if part of the effect on wages is due to a reallocation of resources towards English instruction in primary schools, which can potentially affect the formation of human capital. There was no evidence of an effect on Language and Math test scores, suggesting that the estimated effect of exposure to the English language on wages is not reflecting changes to general cognitive skills. These findings are consistent with exposure to English instruction increasing the acquisition of English language skills, as we have recently shown in a companion work (Gálvez-Soriano, 2023).

The results are robust to changes in the model specification as a response to the recent critique in the literature of a potential bias in the TWFE estimator due to heterogeneous treatment effects (see De Chaisemartin and D'Haultfoeuille (2022), for a review). Additionally, we offer evidence that my results are robust to changes in the measure of exposure, to different sub-samples, and to a different specification that accounts for concerns about potential non-fixed labor market conditions among cohorts.

In summary, we show strong evidence that, in the context of a non-English-speaking country, exposure to English language instruction in elementary school may lead to find better paid jobs only among high-achieving kids. However, low-achieving individuals who had exposure are also better off as they substitute jobs in industries that require more physical work for jobs in industries more intensive in cognitive skills. These improvements may be explained by the acquisition of English skills as exposure did not affect other cognitive abilities. Note, however, that these results no longer correspond to the average worker, but to workers with a low educational attainment. This suggests that future research is likely to find positive effects on wages for the average worker, including those with higher educational attainment (Lang and Siniver, 2009; Azam, Chin and Prakash, 2013). The explanation is that, later in time, we will be able to observe in the market individuals currently enrolled in school and, hence, will have higher educational attainment and wages. Furthermore, finding positive effects on wages would be more consistent with previous research in Mexico and India (Delgado Helleseter (2020) and Chakraborty and Bakshi (2016), respectively).

The remaining sections of this paper proceed as follows. In the first section, we explain the background of the policy. In section 2, we explain the empirical strategy we propose. In section 3, we describe the novel database we construct. In section 4, we show the results of the effect of the English program on labor market outcomes and student achievement. Section 5 addresses some of the main potential concerns that may arise after looking at my results. Finally, section 6 summarizes with a discussion of my findings and a brief conclusion.

1 Background

1.1 The Mexican education system and labor force participation

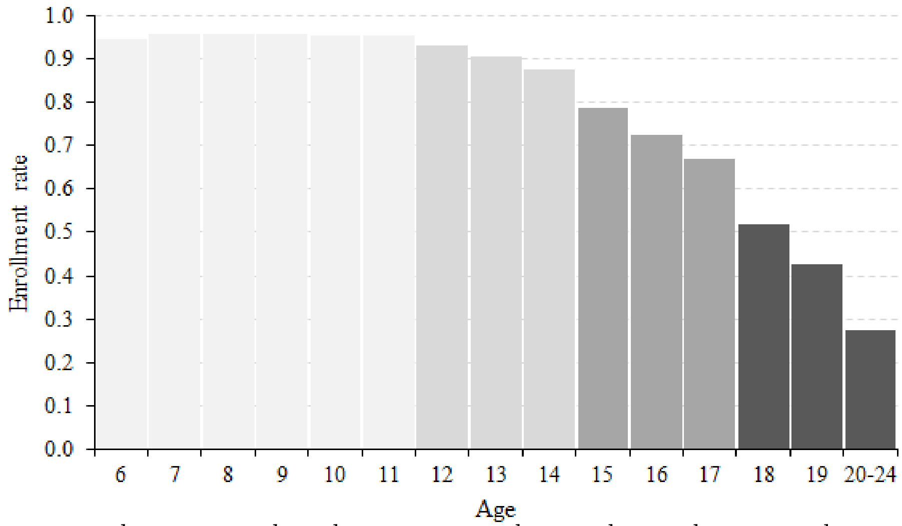
In Mexico, K-12 schooling is divided into three levels: elementary school (or primary; grades 1-6), middle school (or lower secondary; grades 7-9), and high school (or upper secondary, grades 10-12). Basic compulsory schooling consists of elementary and middle school. In 2013, Mexico expanded compulsory schooling to grade 12, but in practice, many individuals do not attend high school. The lack of schools in some communities (which implies high commuting costs) and the higher fees relative to K-9 grades, prevent individuals to continue their higher education.

Regularly, students enrolled in elementary school range in age from six to eleven years old (at the beginning of the second term of each academic year). Students enrolled in middle school are between 12 and 14 years of age, while students enrolled in high school are between 15 and 17 years old (at the beginning of the second term of each academic year as well). College students are usually between 18 and 21 years of age. All these ages may vary resulting in older students if they entered late the education system and/or if they failed one or more grades.

Enrollment rates in primary school are close to full enrollment but, in college, this rate falls dramatically. In Figure 1 we plot enrollment rates by age using data from the 2020 Mexican population census. To interpret these data as enrollment rates per grade we assume that students who reported attending school are enrolled in the grade that corresponds to their age. Thus, the different bars' colors represent different levels of education. Several factors could explain the decreasing enrollment rates in higher academic levels, such as the lack of schools or the opportunity cost of higher education. However, it is surprising that high school enrollment is quite low even despite its character of being compulsory (since 2013). This may be explained because the 2013 compulsory policy does not enforce parents to send their kids to school, but the schools to offer free education. In other words, the decision to attend high school is still self-determined. Furthermore, the enrollment rates in the last two years of college education and the first grades of graduate school are just 27 percent. These numbers are consistent with higher education costs and the lack of schools, where the existing ones are predominately in urban and suburban areas, which implies higher commuting costs for individuals living in a rural context.

There is a substantial labor force participation rate among individuals finishing upper

Figure 1: Enrollment rates by age (based on 2020 Mexican census data).



Note: Enrollment rates by age are plotted. we assume that students who reported attending school are enrolled in the grade that corresponds to their age. Hence, the first six light gray bars represent primary school enrollment, the next three darker bars represent middle school and high school enrollment, and the dark gray bars represent the college and graduate degrees enrollment.

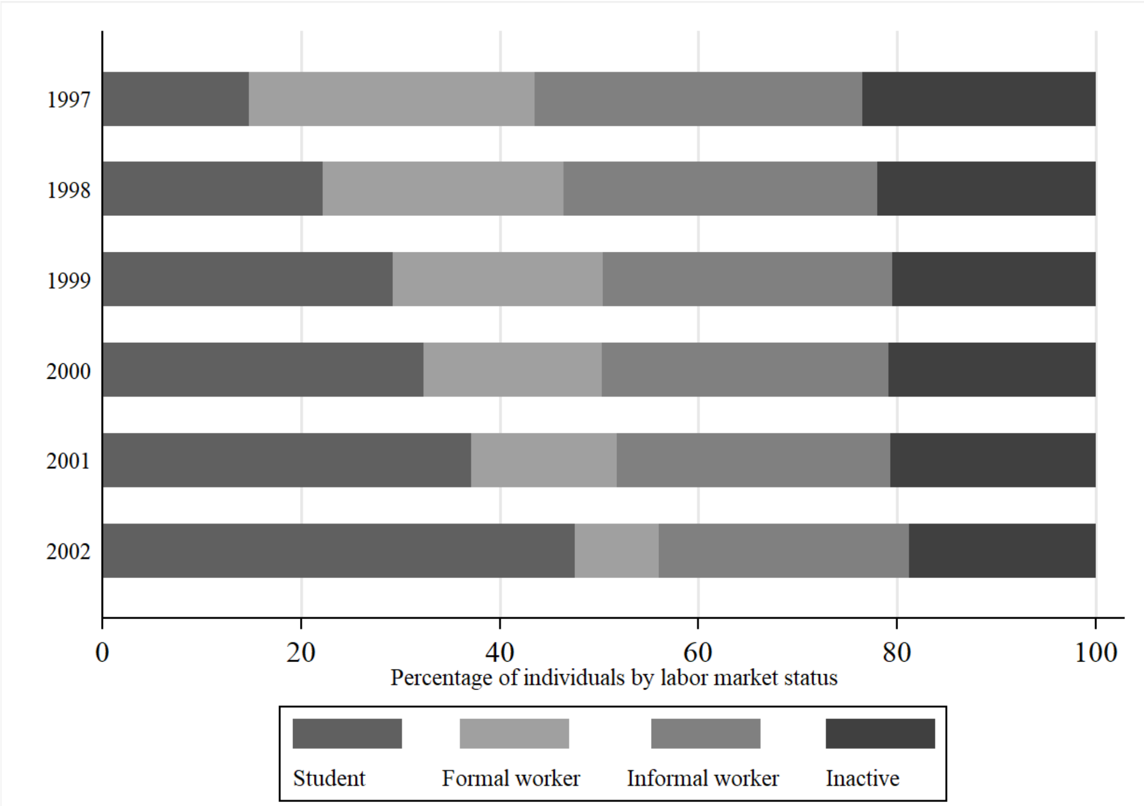
secondary school. In Figure 2 we show the labor participation rate in Mexico for the six cohorts that we study in this paper, using the 2020 Mexican population census. Individuals who were born between 1997 and 1999 (21-23 years old in 2020) had no exposure to English instruction in primary school, while individuals who were born between 2000 and 2002 (18-20 years old in 2020) had some exposure. The decreasing participation rate in younger cohorts is consistent with the fact that younger kids have higher enrollment rates. Furthermore, from the 48 percent of 18-year-old individuals not enrolled in school, 73 percent are employed. These data tell us that there is an important labor participation rate even for the youngest cohorts we will analyze in this paper, which suggests we will see them in the formal labor data even after considering that some will continue studying.

1.2 The National English Program in Mexico

The main language of communication in Mexico is Spanish and, although there is not an official estimation of the proportion of Mexicans who can speak and/or understand English, the reality is that just a few people have some kind of English knowledge in Mexico. For example, according to the English Proficiency Index (IPE) generated by the company Education First, Mexico is classified as a country with low proficiency in English. Indeed, in 2020 this index ranked Mexico in place 82 out of 100 non-English speaking countries all over the world, and in place 18 out of 19 Latin American countries.[1] Furthermore, according to the survey of human capital in Mexico held by CIDAC (2008), six percent of the urban population declared to be able to speak English.

[1]All these results are available in the 2020 edition of the EF English Proficiency Index report, published by Education First (2020).

Figure 2: Composition of the Mexican labor force by cohort (based on 2020 Mexican census data).

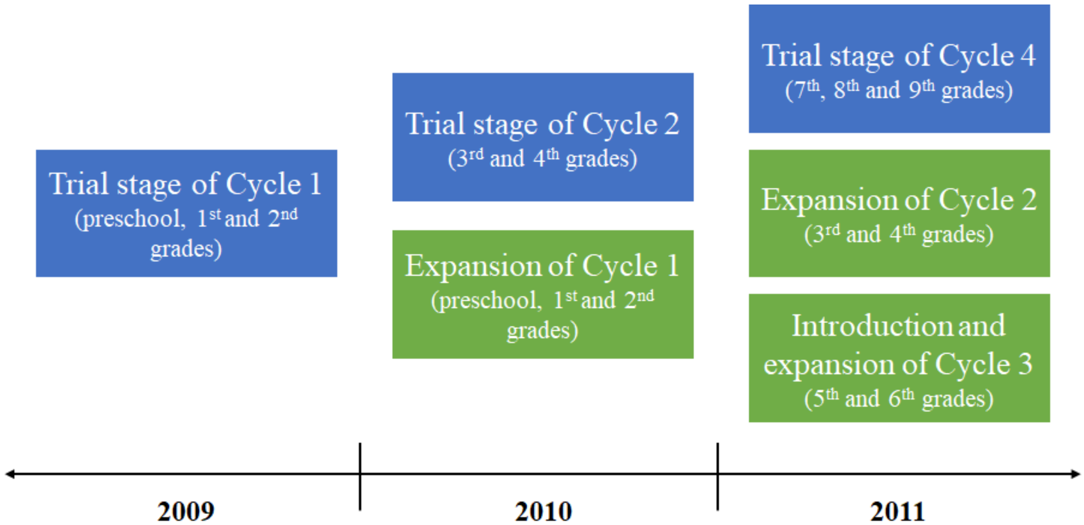


Note: Percentage of Mexicans in certain economic statuses (by cohort) are plotted. In the vertical axis, we show age cohorts. For example, individuals who belong to the 2002 age cohort are 18 years old in 2020 and they have a labor participation rate of 33 percent, from which one-seventh work in formal jobs. Almost 40 percent of the individuals in this cohort are still studying. The other extreme case is the 1997 age cohort where individuals are 23 years of age in 2020 and their labor participation rate is 54 percent, from which one-third are working in the formal sector. Only 10 percent of individuals in this oldest cohort are studying.

Knowing about the weakness among Mexicans to communicate in English, in 2009 the Mexican Ministry of Education (SEP, by its acronym in Spanish) launched a program called National English Program in Basic Education (NEPBE), which intended to introduce English instruction in all Mexican public elementary schools. Before this program, English instruction was somewhat generalized among middle schools (because English as a subject was included in the regular curricula of middle schools since 1993), but with the NEPBE English language education became compulsory since elementary school and it articulated the primary and secondary programs to give continuation of the English instruction between both educational levels.

Before the NEPBE, there were efforts to implement some kind of English instruction among elementary schools in 21 out of 32 Mexican states. In fact, in some of those states (Aguascalientes, Coahuila, Durango, Morelos, Nuevo Leon, Sinaloa, Sonora and Tamaulipas) the already initiated English programs had strong fundamentals and had exceptional coverage compared to the other Mexican states. However, the results were heterogeneous all around the country due to the differences in coverage, achievement levels, contents, English teachers

Figure 3: The NEPBE implementation: trial and expansion stages



Note: The NEPBE was launched in 2009 as a trial stage with the so-called Cycle 1. In 2010 the program continued the trial stage with Cycle 2 and expanded Cycle 1. Finally, in 2011 the program introduced and expanded Cycle 3, benefiting fifth and sixth graders.

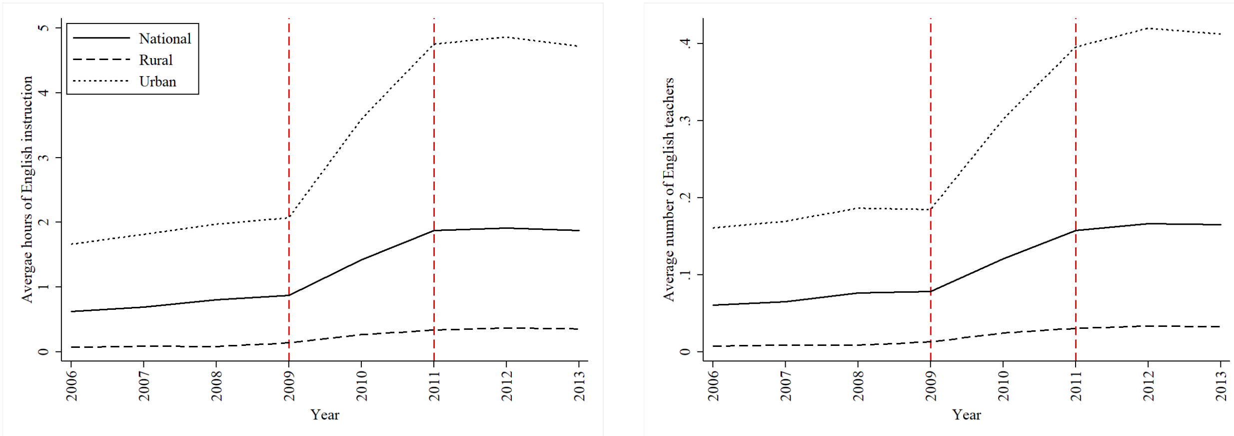
supply and teaching hours. Hence, the NEPBE aimed to generalize the English instruction with the same content and with the same allocation of time. Nonetheless, the possibility of putting this program into practice has still been limited by the shortage of teachers trained for this purpose (Sayer, 2015*a*,*b*; Ramírez-Romero and Sayer, 2016).

The limitations faced by previous state English programs were dealt with by the central Mexican government through the implementation of the national English program by cycles and not by school grades, which improved continuity and articulation among the different grades and levels of the Mexican basic education system. The so-called Cycle 1 comprises the third grade of preschool, as well as the first and second grades of elementary school; Cycle 2 includes the third and fourth grades of elementary school; Cycle 3, comprises the fifth and sixth grades, while Cycle 4 includes all grades of middle school (SEP, 2011).

The NEPBE was launched in 2009 (as a pilot stage), however, the program expanded in 2011 because before then the Mexican government suggested implementing it only among the first four grades of primary education (from first to fourth grades) and in a few randomly assigned schools. Hence, in 2011, fifth and sixth graders had exposure for the first time to the English program, but also more schools became beneficiaries (see Figure 3). Likewise, due to changes in the central government political party, in 2014 the NEPBE was reallocated to be part of a wider program called Program to Strength the Quality of Basic Education, playing a secondary role in the national agenda. Finally, in 2017 the program changed again to national relevance and since then it is known as the National English Program (or PRONI, for its acronym in Spanish).

Most of the beneficiary schools saw a real change in hours of English instruction and also in the number of English teachers until the year 2011. Furthermore, there was no significant change in rural areas. This latter was mainly due to the operation rules of the program,

Figure 4: English instruction and English teachers over time (rural vs urban)



(a) Hours of English instruction

(b) Number of English teachers

Source: Own elaboration with data from the Mexican school census, Ministry of Public Education (SEP). *Note:* Hours of English instruction are measured as the weekly hours average over the universe of Mexican elementary schools. Similarly, the number of English teachers refers to the average number of English teachers across elementary schools. The vertical dotted line in 2009 highlights the implementation year of the NEPBE's trial stage, while the dotted line in 2011 highlights the expansion of the program.

which prevented poor and marginalized schools to implement the English program since they do not have the equipment needed and/or well-established commuting roads (as we explain below). On the other hand, it is more evident that the English program increased the hours of English instruction and the number of English teachers in 2011 (see Figure 4).

Indeed, we document that elementary schools in rural areas were less affected by the English program. Among the Mexican states, only Aguascalientes, Colima, Morelos, Sinaloa and Tamaulipas increased English instruction (between 2008 and 2011) in terms of the proportion of rural schools with some kind of English instruction (see panels (a) and (b) of Figure 5). On the other hand, with the NEPBE the proportion of urban schools with English instruction increased (in the same period) in several northern (Baja California Sur, Coahuila, Durango, Nayarit, San Luis Potosi, Sinaloa, Sonora and Tamaulipas) and some southern (Campeche, Colima, Guerrero, Morelos and Yucatan) Mexican states (see panels (c) and (d) of Figure 5).

2 Empirical strategy

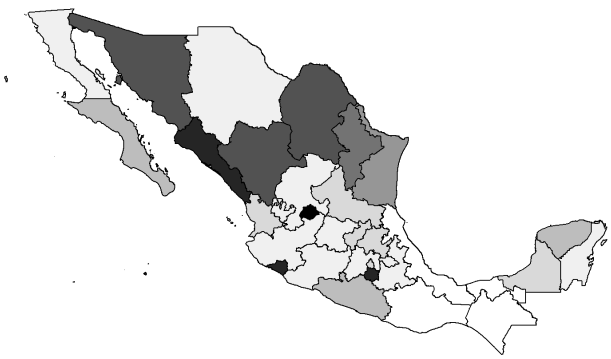
The main variables of interest in this paper are the labor market outcomes of individuals who had exposure to English instruction at early stages of life. In particular; the probability to work in the formal sector, wages, geographical mobility, and the likelihood to work in certain economic industries. In order to better understand the reduced form effect of English exposure on the labor market it is important to understand first how English exposure affects student achievement. This is particularly useful in our context where we do not have measures of English language abilities. Indeed, understanding the relationship between

Figure 5: Proportion of Mexican primary schools with English instruction (urban vs rural)

(a) Rural schools in 2008

(b) Rural schools in 2011

(c) Urban schools in 2008



(d) Urban schools in 2011

Note: Before the NEPBE, English instruction was rare among rural schools. With the NEPBE, the proportion of rural schools with English instruction increased (between 2008 and 2011) in a few Mexican states: Colima, Morelos, Sinaloa and Tamaulipas. On the other hand, with the NEPBE, the proportion of urban schools with English instruction increased (between 2008 and 2011) in several northern (Baja California Sur, Coahuila, Durango, Nayarit, San Luis Potosi, Sinaloa, Sonora and Tamaulipas), and some southern (Campeche, Colima, Guerrero, Morelos and Yucatan) Mexican states.

English exposure and student achievement helps determine if English instruction is a substitute or complement of other abilities that contribute to the formation of human capital and, ultimately, to labor market outcomes.

In my analysis, we use school-by-cohort variation in exposure to English instruction in Mexican elementary schools to uncover the causal relationship between English instruction and labor market outcomes. Much of this variation is driven by the policy change induced by the implementation of the NEPBE (see subsection 1.2 for evidence on this claim).

There are two main challenges that we address in this paper: the omitted variables bias and the sample selection. First, one source of omitted variables bias is the set of unobservable characteristics that made some schools participate in the program while others did not. Thus, there could be differences among students who attended different schools, which are not necessarily related to differences in English language instruction. To deal with this issue, we exploit the richness of my data set using a school fixed effects approach (which deals with omitted school, neighborhood, and general socioeconomic status type variables).

The other source of omitted variables bias is the unobserved ability. In fact, we may obtain biased estimates because high-ability students will be more likely to have better labor market outcomes, but also they are more likely to learn English. we mitigate this latter issue by explicitly controlling for students' abilities (measured by their Language and Math test scores in sixth grade).

Second, the sample selection problem arises because exposure to English instruction reduces the probability that an individual participates in the formal labor market. Furthermore, the labor market outcomes we observe come from the social security administrative records (which only include the formal sector). It is likely that the sample selection is a consequence of exposure to English instruction increasing the likelihood that individuals keep enrolled in school. We propose to solve the sample selection by studying a sub-sample of individuals who are less likely to be enrolled. My main results derive from this low-enrollment sample.

2.1 Exposure to English instruction and labor market outcomes

Let us consider y_{isc} as the dependent variable, which can be any of the labor market outcomes of interest that characterize each individual i, who studied in school s, and belongs to cohort $c = \{1997, 1998, 1999, 2000, 2001, 2002\}$. We observe the labor market outcomes of each individual in time $t = \{2018, 2019, 2020, 2021\}$ through the Mexican social security data. By the year 2021, all individuals in my sample had completed high school. However, we are excluding those students who decided to pursue a college or a graduate degree (as we explain below) in order to have individuals with similar educational levels.[2] This is not a concern because the college and graduate enrollment rates are low in Mexico (about 27 percent in 2020, see Figure 1).

There are kids who had more exposure to English instruction in elementary school (younger cohorts), while other kids had less or no exposure at all (older cohorts). Most of this variation is due to the NEPBE, but instead of using variation due to the intervention, we are able to use actual exposure to English instruction. As an example of the variation that might have been induced by the NEPBE, consider students enrolled in fourth grade in 2011 (2002 cohort), these kids would have had potentially five years of exposure in primary school. Students enrolled in fifth grade in 2011 (2001 cohort) would have had three years of exposure. Likewise, students enrolled in sixth grade in 2011 (2000 cohort) would have had exposure to only one year of English instruction in primary school. On the other hand, the three older age cohorts (1997-1999) had fewer or no exposure to English instruction in primary school (see Figure 6).

In order to exploit the variation in exposure to English language instruction, we construct an exposure variable, $ExpEng_{sc}$. This variable takes into account differences in exposure by cohort and in adoption among primary schools, by averaging the number of hours of English instruction from first to sixth grade. Considering these two sources of variation; by school and by cohort, we propose the following Two-Way Fixed Effects (TWFE) equation:

$$y_{isc} = \alpha + \beta \cdot ExpEng_{sc} + \boldsymbol{X_{isc}\gamma} + \zeta_c + \nu_s + \tau_t + \varepsilon_{isc}, \tag{1}$$

[2] In the social security data we do not observe the education of each worker.

Figure 6: Exposure to English instruction by cohort

Cohort	Primary school					
	1st	2nd	3rd	4th	5th	6th
1997	2003	2004	2005	2006	2007	2008
1998	2004	2005	2006	2007	2008	2009
1999	2005	2006	2007	2008	2009	2010
2000	2006	2007	2008	2009	2010	**2011**
2001	2007	2008	2009	2010	**2011**	2012
2002	2008	2009	2010	**2011**	2012	2013

Note: The rows in the figure represent the cohorts and columns represent the school progression from primary to college (by grades). The cells marked in dark gray suggest that those age cohorts had no exposure to English instruction in the indicated grades. Cells highlighted in light gray show the grades for which the cohorts would have had exposure to English instruction, according to the NEPBE (in bold the final year of expansion, 2011).

where we control for common cohort trends, ζ_c, and unobserved attributes like the quality of the school, neighborhood effects, and growing up environment, ν_s.

In all my specifications we include time fixed effects (FE), τ_t, in order to control for aggregate economic conditions at the time of the labor market observation. We also include a vector of control variables, $\boldsymbol{X_{isc}}$, which includes: gender, number of classmates in sixth grade, age (as a proxy of experience), and its square as in the Mincer (1974) equation, a dummy for permanent jobs, number of jobs in a year, number of permanent jobs in a year, number of teachers with college degree and number of teachers with a masters degree, both in elementary in school.

By including school FE we can deal with a lot of omitted variables problems, like those that are correlated to the neighborhood and school characteristics, which are common characteristics of students within the same school. Hence, if we believe that the unobserved characteristics are constant over time, my empirical strategy will solve the omitted variables bias problem and the estimator β will provide a causal interpretation of the effect of exposure to English instruction at early stages of life on labor market outcomes.

Additionally, there could be a concern that despite controlling for time-invariant characteristics of the school, there might still be omitted variables correlated with the exposure to English instruction measure and labor market outcomes: cognitive abilities. Due to the richness of my data, we can control for a measure of individual ability. In particular, we control for student achievement measured as Language and Math test scores in sixth grade. This enables me to control for the quality of the students.

2.2 Mechanisms: English abilities, cognitive skills and economic industries

There are (many, but) three main mechanisms through which exposure to English instruction affects labor market outcomes: the acquisition of English abilities, the effect on other cognitive abilities, and the effects on the likelihood to work on jobs requiring English skills. The first mechanism suggests that exposure to English instruction actually had a positive effect on the acquisition of English language skills. Hence, this new skill could have an effect on the labor market outcomes by expanding the possibility of finding well-paid jobs. Since there are no measures of English abilities in Mexico, it is difficult to test this mechanism. However, we rely on Gálvez-Soriano (2023) findings, which suggest that exposure to English instruction could lead to the acquisition of English skills in Mexico. Second, English instruction could affect other subjects (and, hence, other cognitive abilities), which could have implications on labor market outcomes. Indeed, adding time to English instruction could decrease the time allocated to Language and/or Math subjects. Nonetheless, it could also be the case that English instruction strengthens language skills through a complementary between English and the native language, which is consistent with the existing research suggesting that taking foreign language classes increases language abilities. Finally, exposure to English instruction could affect an individual's decision on the economic industry he/she will work in, depending on the requirements of English abilities in those industries. In this subsection, we explore these three mechanisms.

2.2.1 English abilities

In the literature, it has been understudied if exposure to English instruction increases the acquisition of English abilities (in the context of a non-English speaking country). Hence, this is still an interesting question to debate. In the particular case of Mexico, there is not an official measure of English skills, which makes it more difficult to evaluate the effect of exposure on English abilities. Nevertheless, from descriptive studies we know that most of the Mexicans who report some kind of English skill declared they learned it in school (CIDAC, 2008). This finding suggests a positive correlation between exposure and English abilities.

The existing evidence supports the idea that exposure to English language instruction in school could lead to the acquisition of English abilities (Eriksson, 2014; Gálvez-Soriano, 2023). In the particular case of Mexico, Székely, O'Donoghue and Pérez (2015) conducted a descriptive study that may mislead the evidence, suggesting that exposure does not increase the acquisition of English skills. However, there are several reasons to believe that this study seriously understates the effects of exposure to English instruction. First, the timing; the authors conducted a survey in 2014 of middle school graduates, which means that these students had little or no exposure to English instruction in primary school. Second, the sample; the authors surveyed students without distinguishing those who studied in schools with English instruction and those who did not. This means that their sample contains both types of students, understating the proportion of individuals with English abilities. Finally, the geographical location; most students in their sample live in states with a low proportion of primary schools with English instruction (Baja California, Jalisco, Guanajuato, Mexico, Puebla, Chiapas and Mexico City, with less than 11 percent of schools with English instruction) and only two states of the sample with a relatively high proportion (Nuevo Leon and Sinaloa, see Figure 5).

Indeed, the only formal evidence regarding the effect of exposure to English instruction on the acquisition of English skills in Mexico was recently provided by Gálvez-Soriano (2023). This author uses state-by-cohort variation exploiting the implementation/expansion of English programs in elementary schools of eight out of 32 Mexican states with a Staggered Difference in Differences strategy. His results offer evidence that exposure to English instruction in primary school may lead to the acquisition of English skills, especially when the increase in exposure is statistically significant (at least ten more minutes of English instruction per week). Furthermore, the increase in exposure due to the NEPBE was greater than previous increments due to the state English programs, which supports the idea that kids actually acquired English skills after the intervention we study in this paper.

2.2.2 Cognitive skills: student achievement

Although we do not observe English abilities, we can argue that if English instruction does not have an effect on student achievement, the effect of English instruction on labor market outcomes is mainly through an improvement in English language skills. Indeed, as we discussed above, the introduction of English instruction in primary schools could affect the teaching time of other subjects. If this is the case, labor market outcomes would be affected by the NEPBE because of either the rivalry in time among subjects or by the complementary language abilities. Thus, we would find it difficult to separate the effect of English abilities and other cognitive abilities on earnings if the NEPBE had significant effects on Language and Math subjects because these are the basic skills needed and used in the labor market.

To evaluate the effect of English instruction on students' achievement we propose to use a similar specification as in Equation 1. The difference is that now the dependent variable is a measure of student achievement for different cohorts of sixth graders (as shown in Figure 6). The main independent variable of interest is the measure of exposure to English language classes, $ExpEng_{sc}$, which we constructed exploiting the panel structure of my database as the average of the hours of English instruction over the six years that comprise primary school in Mexico (just as we explained above). The rest of the variables were previously explained in subsection 2.1.

Nevertheless, notice that using a regression of student achievement in English instruction would produce a biased estimator because of the omitted variables problem. we solve this issue by proposing a TWFE regression, where we fully control for school FE, ν_s and common cohort trends, ζ_c. The selection bias is caused because beneficiary schools were not randomly assigned (after the trial stage). In this sense, we are concerned that better schools chose to adopt the NEPBE, i.e, schools with more information, better teachers or more resources. Similarly, it could also be the case that these beneficiary schools are located in villages with wealthier neighborhoods and/or better access to services. This particular issue is solved when we control for school FE, which captures school and regional differences if we assume that those differences are constant over time. Finally, we also control for cognitive abilities, as well as students' and schools' characteristics, $\boldsymbol{X_{isc}}$.

Under this model, the effect of exposure to English instruction on student achievement is captured by ϕ in the following equation:

$$test\_score_{isc} = \theta + \phi \cdot ExpEng_{sc} + \boldsymbol{X_{isc}\gamma} + \zeta_c + \nu_s + \varepsilon_{isc}, \tag{2}$$

where $test\_score_{isc}$ is the sixth grade standardized test score of student i, who goes to school s and belongs to cohort c.

2.2.3 Economic industries requiring English skills

The effect of exposure to English instruction on wages can be better understood, as we mentioned before, by studying its first effect on students' achievement. This analysis would increase our understanding of how taking foreign language classes affects the formation of human capital. However, a third mechanism operates directly in the labor market. Indeed, if we are willing to believe that individuals actually acquired English abilities after exposure to English instruction, there could be effects on the likelihood to choose jobs in economic industries that require English skills.

We explore this third mechanism by looking at the movements within economic industries: manufacturing and services. In particular, we study these two industries by classifying them into English-intensive and non-English-intensive industries. We construct the English intensity classification using the criteria recently proposed by Gálvez-Soriano (2023). In this classification, an industry is considered as English intensive if it is part of the highest two quartiles of the distribution of industries ordered according to the proportion of employees with English skills (see section A.3 for a detailed description of the high and low English intensive industries). The model we propose in this subsection is similar to Equation 1. The difference is that now the dependent variable, $EngInd_{isc}$, is a dummy that identifies industries with high-English and low-English intensive jobs, for both manufacturing and services (four dependent variables in total), as follows:

$$EngInd_{isc} = \alpha + \beta \cdot ExpEng_{sc} + \boldsymbol{X_{isc}\gamma} + \zeta_c + \nu_s + \tau_t + \varepsilon_{isc}, \tag{3}$$

where β can be interpreted as the likelihood of working in a specific type of economic industry (depending on its English requirements). The regressions derived from this analysis are unconditional on being in manufacturing (or services) industries because the omitted category in the dummy $EngInd_{isc}$ refers to all the other industries.

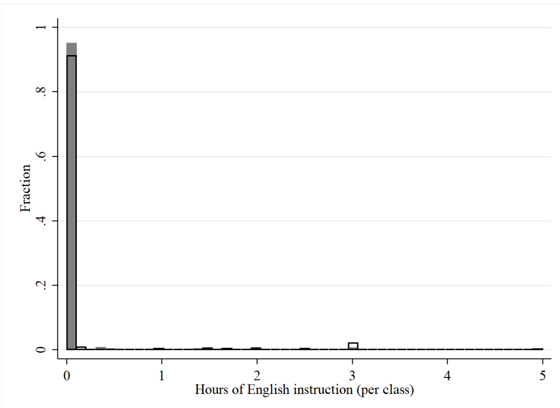
3 Data

We use three sources of data: school census, standardized test scores (in elementary schools), and formal labor market data.[3] We construct a novel database using these three sources of information because we are able to link each student to their primary schools and to their

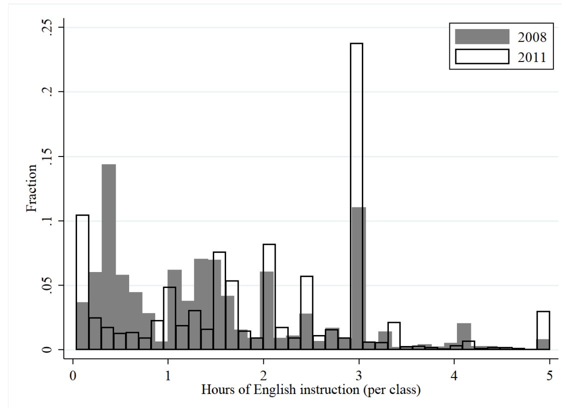
[3]As we explain below, we have access to administrative records of more than 90 percent of all Mexican workers in the formal sector, but we do not observe those individuals who are inactive, students or those that work in the informal sector.

jobs when they go to the labor market. Indeed, we can observe each student in the time since they are in third grade and then ten years later if they find a job in the formal sector. In this section, we will explain how we use each data set.

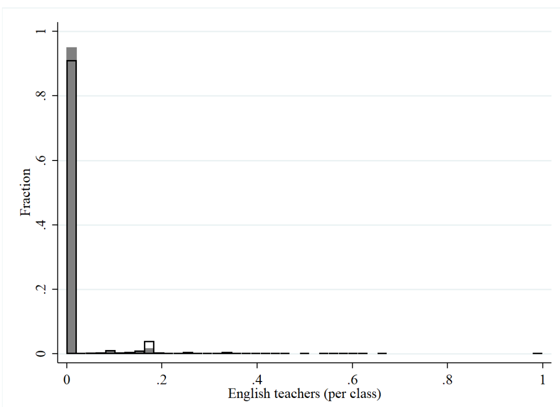
Figure 7: Hours of English instruction and English teachers distributions (2008 vs. 2011)



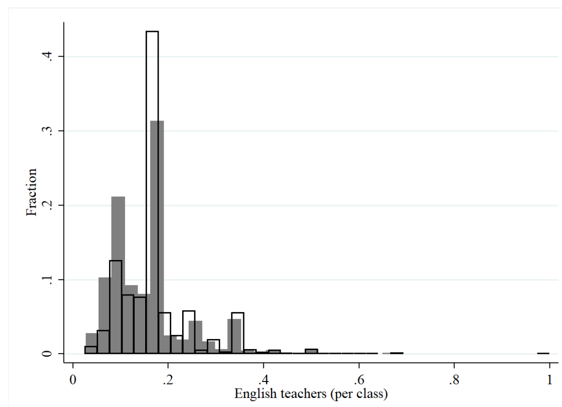
(a) Hours of English instruction



(b) Hours of English instruction (w/o zeros)



(c) Number of English teachers



(d) Number of English teachers (w/o zeros)

Note: Frequency of the indicated variables are plotted. Histograms at the right do not show zeros, which capture most of the distribution. Hours of English instruction are calculated by dividing the average weekly hours in a school by the total number of classes. Similarly, the number of English teachers is calculated by dividing the total number of English teachers by the total number of classes in a school.

3.1 Mexican school census

The first source of information we use in this research is the Mexican school census (also known as Statistics 911). The school census allows for identifying the schools that have offered English instruction before and after the implementation of the NEPBE. This is necessary because there are no official statistics about the list of beneficiary schools of this program, at least not before 2017. It is worth mentioning that we exclude from the analysis those schools that are beneficiaries of the full-time school (FTS) program for two reasons.[4] First, students' test scores could be positively affected by the FTS program. Second, the schools that participated in the FTS program were more likely to implement English instruction in all grades (from first to sixth) and with more weekly hours. Furthermore, we only consider public elementary schools in the morning shift. Hence, removing all these aforementioned schools from my database let me obtain a cleaner effect of English instruction on labor market outcomes (and on school achievement).

Between the years 2008 and 2011, the distribution of weekly hours of English instruction and the number of English teachers shifted to the right, making evident the introduction of the NEPBE in Mexico. Indeed, as we explained in section 1, the trial stage of the NEPBE started in 2009 and progressively expanded from first to sixth grade. Thus, the English program reached out the planned expansion to all grades of primary school in 2011. In Figure 7, we compare the distribution of weekly hours and the number of English teachers between 2008 and 2011 (i.e., one year before the implementation of the program and at the year of the final expansion).

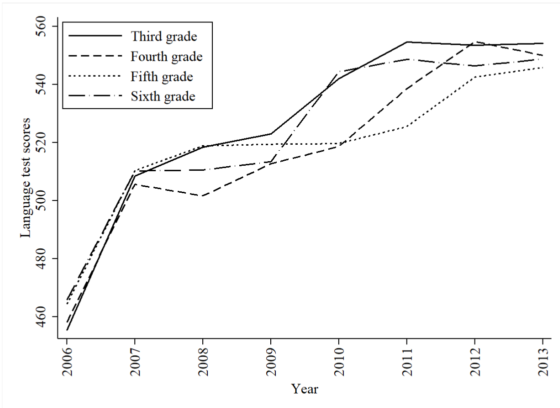
We measure weekly hours of English instruction as the ratio of total weekly hours of English instruction in each school and the total number of classes. For example, in a school where the reported weekly hours are 30, but the total number of classes is six (one section per grade), there are actually five weekly hours of English instruction per class (30/6 hrs.). In a similar example, another school also reports 30 hours of English instruction but has two sections per grade. This latter school offers actually 2.5 weekly hours of English instruction per class (30/12 hrs.).

Most of the distribution of hours of English instruction concentrates at zero, suggesting that most Mexican schools do not offer English instruction. With the implementation of the program, the density of zeros decreased (see panel (a) of Figure 7). In panel (b) of Figure 7 we show the distribution of weekly hours of English instruction before and after the implementation of the NEPBE but without the zero values from the distribution. This figure suggests that some of the schools that had zero hours of English instruction in 2008 had a positive amount in 2011. Furthermore, many schools offered an amount of English instruction around at three hours (close to the suggested 2.5 hours by the Mexican government).

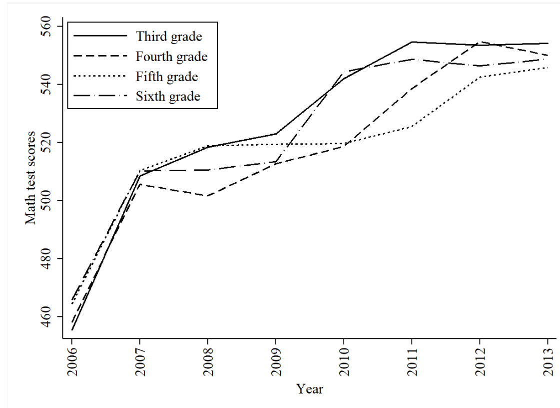
English teachers are a scarce resource in Mexico. Hence, in many Mexican schools, there is only one English teacher. We measure the number of English teachers as the ratio of teachers and classes. Following the examples of hours of English instruction, in a school with one teacher and six classes, the number of English teachers will be 0.16 (1/6), or 0.08 if the school has 12 classes (two sections per grade). In panel (d) of Figure 7, we show the distribution of English teachers between 2008 and 2011 without the zero values from the distribution. The distribution of English teachers moved to the right after the implementation of the NEPBE. Indeed, in 2008 the distribution concentrated between zero and 0.2 English teachers per class, while with the English program the distribution concentrated roughly between 0.08 and 0.38 English teachers per class.

[4] The full-time school program was launched in 2007 with the objective of increasing the number of hours students spend at school. The trial phase of the program was implemented in 500 elementary and middle schools, located in 15 out of 32 Mexican states (Cabrera-Hernández, 2020).

Figure 8: Language and Math test scores over time and by grade



(a) Language test scores



(b) Math test scores

Note: Language and Math test scores are plotted over time and by grade. Two things can be noted from the graphs. First, test scores are increasing over time. Second, test scores of third graders are, on average, higher than fourth, fifth or sixth graders' test scores, every year.

3.2 ENLACE test scores

The second source of information is a standardized test known as ENLACE (National Evaluation of School Achievement in Educational Centers). ENLACE is a nationwide standardized test that used to be applied to all students enrolled in public and private elementary and middle Mexican schools. This test was designed to examine students' Mathematics and Language (Spanish) achievement. This test was first applied in 2006 and discontinued in 2014, but replaced by the National Plan for the Learning Evaluation (PLANEA, for its acronym in Spanish).

We standardize test scores, ts_{isct}, of each student i in school s at time t using the following formula:

$$test\_score_{ist} = \frac{ts_{isct} - \mu_t}{\sigma_t},$$

where $test\_score_{ist}$ is the standardized test score, while μ_t and σ_t are the mean and standard deviation of test scores, respectively, pooling all Mexican students by grade and by each observed year. This standardization takes into account that the test difficulty is different among grades and that it could change over time (as shown in Figure 8, test scores increase over time and differ by grade).

3.3 Social Security data

The third source of information corresponds to the labor data obtained from the Mexican Institute of Social Security (IMSS).[5] IMSS provides medical services and a contributory

[5]The data was accessed through the Econlab at Banco de México. The EconLab collected and processed the data as part of its effort to promote evidence-based research and foster ties between Banco de México's

pension scheme to more than 90 percent of the formal workforce in Mexico (and nearly 40 percent of all Mexican workers).[6] This means that my estimates consider only individuals who work in the formal sector, which rises concerns about a second selection problem (selection into the sample). In section 4 We provide evidence of this problem, as well as a solution to obtain unbiased estimates. It is also worth mentioning that we are using the universe of Mexican students and almost the universe of formal workers (except for civil servants and the military), which provides more reliability of my proposed solution to the selection into the sample.

We use IMSS data from the years 2018-2021, so there is enough time for individuals in my data set to enter the labor market if they had exposure to the policy during basic education. Indeed, we have already discussed that the NEPBE was first implemented in 2009 (as a pilot program) and expanded progressively with an important number of beneficiary schools in 2011. By the year 2011, three cohorts had exposure to English language instruction: 2000, 2001 and 2002. In particular, students who were born in 2002 had exposure to a minimum of three years of English instruction in primary school, and potentially five years (see Figure 6). The next three older cohorts (1997-1999) had no exposure to English language instruction in primary school. Furthermore, none of these cohorts could have finished college if we observe them in the labor market data and if we restrict age to individuals who are 22 or younger (and allowed older individuals if they entered the labor market before age 22).

IMSS database is rich and complex. The data frequency is monthly, and each month could have more than one observation for the same worker because some workers have more than one job (in different or the same economic industry). To deal with this heterogeneity, we make some transformations to the data:

1. First, we take the average of the wages reported over one year, by worker, by economic sector and by employer.
2. Second, when a worker has multiple jobs, we drop the jobs with the lowest wages if those are non-permanent jobs.
3. Then, if there are individuals with permanent and non-permanent jobs, we use the information only of the permanent job.
4. Finally, for individuals who have more than one job with the same wage we choose the job in which they have worked most part of the year.

Wages reported in the social security data (IMSS database) are daily and before taxes. Furthermore, there is no information on the number of hours or days worked, but we assume that an employee works 30 days, on average. Hence, earnings reported in this paper correspond to a monthly wage (before taxes).

On the other hand, social security data contains detailed information regarding the economic industry each individual work in when they go to the labor market. To give a clean

research staff and the academic community. Inquiries regarding the terms under which the data can be accessed should be directed to: econlab@banxico.org.mx.

[6]We estimate these percentages based on total IMSS affiliated workers (reported by IMSS itself) divided by the total formal employed workers (measured with the Mexican Labor Survey [ENOE]).

interpretation of the workers' mobility among industries, we define four main economic industries to study in this paper: Agriculture, Construction, Manufacturing and Services. We use the North American Industry Classification System (NAICS) to classify jobs in their corresponding industries. For example, Agriculture contains: agriculture, forestry, fishing and hunting. The economic industry we call Construction contains: mining, utilities and construction (codes 21, 22 and 23, respectively). The manufacturing economic industry only contains manufacturing (codes 31-33). Finally, the economic sector we call Services contains all the services, from retail sales to public administration (codes 42-92).

Table 1: Descriptive statistics

Variable	Mean	SD	Min	Max
Individual characteristics				
Female	0.39	0.49	0	1
Age	20.88	1.51	16	24
Language test score	-0.06	0.97	-2.84	3.53
Math test score	-0.04	0.97	-2.69	3.40
School characteristics				
Hours of English instruction	0.23	0.60	0	9.41
English teachers	0.02	0.05	0	1
Number of students (6th grade)	28.87	9.49	1	119
Number of teachers with college	0.87	0.20	0	2.15
Number of teachers with masters	0.05	0.07	0	0.91
Rural (%)	0.27	0.44	0	1
Labor market characteristics				
Wage (monthly pesos)	6,586	3,383	2,510	67,215
Permanent job	0.81	0.39	0	1
Number of jobs (in a year)	1.48	0.83	1	17
Number of permanent jobs	1.20	0.83	0	14
Company size (workers)	1,922	5,456	1	92,972
Distance home-work (km)	107	265	0	2,029
Observations	4,055,434			

Note: The sample consists of primary school students who were matched to their labor market outcomes (about 10 years after they completed primary school). These statistics represent averages over all individuals in the sample, including all six cohorts I study in this paper (1997-2002) and also over the four observed years of labor data (2018-2021).

3.4 Descriptive statistics

The final data set, which contains the match between elementary school students and their labor market outcomes when employed in the formal sector, contains more than four million observations. This represents about one-third of the total number of Mexican students in the six cohorts we study in this paper (1997-2002). The matched database includes individuals' characteristics, schools' characteristics, and labor market variables such as wages, number of jobs in a year, number of permanent jobs, distance and company size (see Table 1).

In the matched database, almost four of each 10 individuals are women and they are, on average, 20.9 years old. There is a lot of variation in terms of cognitive abilities (language and math), but the average individual is slightly to the left of the distribution of language and math test scores. The average school offers to each class about 14 minutes of English instruction per week (0.23*60=14). This last measure considers the zero hours of instruction offered by most of the Mexican primary schools, which explains the "short" average English instruction time. There are almost 29 students per class and more than 2/3 of the schools are located in urban localities.

Regarding labor market characteristics, the average worker earns a wage of 6,586 pesos per month, which is almost three times the poverty line in Mexico. Most of the workers in the formal sector (eight out of 10) have permanent jobs and the average worker has 1.5 jobs per year. This latter result could be associated with the fact that we observe young workers (between 16 and 24 years of age) with a lot of mobility in their first years participating in the labor market, but also because some workers in my sample have multiple jobs. Finally, the average company size is 1,922 workers, but there is a lot of variation, going from one single worker to about 93 thousand employees.

4 Results

In this section, we offer evidence that exposure to English instruction at early stages of life does not have a significant effect on wages for the average worker, but it does have a positive effect among high-ability individuals. we also show that exposure leads workers to substitute jobs in agriculture and construction for jobs in manufacturing industries. Furthermore, within manufacturing and services, individuals who had exposure are more likely to work in high-English intensive jobs. These results are based on a TWFE approach (relying on school FE and cohort FE), where we are able to match students who had exposure to English language instruction to their labor market outcomes around 10 years after finishing elementary school.

We also provide evidence that exposure had no effect on other cognitive skills. This suggests that the effects we found on the labor market outcomes could be explained due to the acquisition of English abilities. These latter results are based on a TWFE model, in which we no longer need to link individuals to their labor market outcomes because in this analysis we are interested in studying their test scores at school. Using the school FE enables me to control for differences among schools that could generate a selection bias problem when estimating the effect of exposure to English instruction on student achievement.

In all my results, the usage of a school FE strategy allows me to solve the positive selection problem associated with the better characteristics of beneficiary schools over those that did not adopt the English program. This can be confirmed by comparing columns 1-3 with column 4 of Table A.1 (from the appendix), where the estimate is smaller once we mitigate the selection problem. Furthermore, in both specifications we control for abilities, which enables me to control for the quality of the students if this is changing over time.

4.1 English instruction and labor market outcomes

The exposure to English instruction at early stages of life could have changed the young students' human capital accumulation in several ways (which we discuss in the next subsection) and, hence, their labor market outcomes. If we compare students from the same schools, same ages and same backgrounds, but different exposure to English instruction, we could say something about the effect of this latter on labor market outcomes. The strategy we use to compare students with similar characteristics is to rely on a school FE approach and to control for individuals' abilities (which usually is an unobservable variable). We have access to both, the school FE approach and the ability variable as a control, because of the richness of my database.

The identifying assumption in my proposed model suggests that the omitted variables problem is only based on school characteristics that usually do not change in time. Indeed, there are unobservable school characteristics that contribute to determining what kind of schools are beneficiaries of the program and what kind of schools are not. These unobservables are fixed over time. We claim that this assumption is not very strong because there are school attributes (such as better teachers, wealthier neighborhoods, schools with more resources and more information) that do not actually change over a short period of time. This is true because of stable annual budget negotiations, teachers' unions and neighborhoods' stable conditions in the short run. Furthermore, by focusing on such few cohorts as we do, it is possible that exposure variation is as good as random after controlling for school FE. Likewise, over this tight period, kids had very similar conditions except for the policy intervention.

There is one additional challenge that cannot be addressed with my identification strategy: the sample selection. Although we are able to match primary school students to their labor market outcomes, we can only observe students who decided to work in the formal sector when they participate in the labor market. This caveat causes an additional selection problem (selection into the formal sector sample). We show evidence of this selection problem in panel A, column (1) of Table 2. My results suggest that one additional hour of English instruction reduces the probability of working in the formal sector by (more than) one percentage point. It is likely that this negative selection is explained by the educational decisions of young individuals, who are still enrolled in school.

Due to this latter selection problem, we are worried that the estimates are downward biased, so we cannot interpret the exposure to English instruction on labor market outcomes as the real effect (see columns 2-4 of panel A). The intuition is that the potential high earners are being excluded from the formal labor sample. Thus, we only observe low-skilled and low-earners who would not have studied anyway, even after the exposure to English instruction.

To solve the sample selection, we propose to use a sub-sample of individuals living in counties with low enrollment rates. From the previous analysis derived in Figure 2 of subsection 1.1, we know that if an individual is not working in the formal sector, he/she could be involved in one of three potential statuses: inactive, working in the informal sector or studying. Furthermore, we also noted that the proportion of people who are inactive and those working in the informal sector is pretty homogeneous among the cohorts we study in this paper. However, the variation among different cohorts is potentially generated because the younger individuals are still studying. Hence, since the selection into the sample is potentially caused by children who decide to pursue a high school or college degree, we could mitigate this selection problem by considering a sub-sample of individuals working in counties with low college enrollment rates.

Table 2: Exposure to English instruction and labor market outcomes (Social Security data)

	(1) Formal sector	(2) ln(wage)	(3) ln(distance)	(4) Move state
Panel A: Full sample				
Hrs English	-0.013***	-0.015***	-0.035***	-0.004***
	(0.001)	(0.002)	(0.008)	(0.001)
Observations	16,938,183	4,055,434	4,055,434	4,055,434
Adjusted R^2	0.105	0.270	0.477	0.555
Panel B: Low enrollment sample				
Hrs English	-0.012	-0.005	-0.058	0.015**
	(0.008)	(0.011)	(0.044)	(0.007)
Observations	1,554,827	259,666	259,666	259,666
Adjusted R^2	0.123	0.312	0.677	0.727
Panel C: Low enrollment sample (Men)				
Hrs English (β^M)	-0.016	-0.002	-0.130**	0.004
	(0.011)	(0.016)	(0.057)	(0.012)
Observations	750,812	166,165	166,165	166,165
Adjusted R^2	0.149	0.315	0.680	0.729
Panel D: Low enrollment sample (Women)				
Hrs English (β^W)	-0.010	-0.022	0.063*	0.033**
	(0.010)	(0.015)	(0.034)	(0.012)
Observations	804,015	93,501	93,501	93,501
Adjusted R^2	0.107	0.363	0.700	0.756
$\beta^M = \beta^W$ [p-value]	[0.012]	[0.448]	[0.190]	[0.090]
State of work FE	NO	YES	YES	YES
Mean of dep. var.	0.17	8.68	3.69	0.45

Note: This table shows the effect of exposure to English instruction on labor market outcomes. The sample contains all Mexican workers who belong to the cohorts 1997-2002, who are less than 25 and who are employed in the formal sector. All regressions include controls. Standard errors clustered at school level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

We construct this sub-sample using the 2020 Mexican population census. In particular, we define a variable that identifies counties[7] with low-enrollment rates in the year 2020 as follows:

[7]We use counties instead of localities (the smallest geographical area in Mexico) in order to not violate the confidentiality of individuals living in localities with a few households, which are easily identifiable.

1. We concentrate only on the youngest cohort (2002), which corresponds to the first year of college when observed in 2020.

2. We identify the employment status of each individual: inactive, student or worker.

3. We create a variable for those individuals who are students, but not workers.

4. By county, we take the ratio of students to population (in the cohort 2002).

5. We create a variable that identifies counties with 38 or less percentage of individuals enrolled in college (freshmen).[8]

Considering these criteria, we end up with a sub-sample of individuals with similar proportions of employment statuses among cohorts (see Figure A.2), which represents 6.4 percent of the original data. This new "low-enrollment sample" solves the sample selection problem. We provide evidence that this issue is solved in panel B, column (1) of Table 2, where we do not find any effect of English instruction on the probability of working in the formal sector.

Using this low-enrollment sample we find that exposure to one additional hour of English instruction per week does not have any significant effect on wages. However, the point estimate suggests that the effect could be negative and close to two percent for women, while zero for men. Nevertheless, these estimated coefficients are not statistically different (see the t-test at the end of Table 2). On the other hand, although this sub-sample helps to overcome the selection into the sample, we find that in these low-enrollment counties men are less likely to work in the formal sector than women. This latter result may suggest that there are more men enrolled in school than women in these types of counties.

We also document that the exposure to English instruction reduces the geographical mobility of male Mexican workers, as it increases women's [9]. Indeed, point estimates suggest that exposure reduces men's mobility by 13 percent and increases women's by 6 percent, although both estimates are not statistically different. Furthermore, we find that exposure to English instruction increases significantly women's likelihood of working in a state different from their home state, which suggests that women workers who had exposure to English instruction in primary school find more labor opportunities outside their home states. Men, on the other hand, are employed in places that are closer to their home counties within the same home state (see columns (3) and (4) of panels C and D in Table 2).

On the other hand, the lack of effect of exposure on wages conceals a subtle ability heterogeneity. In fact, we show that high-ability students who had exposure to English instruction saw an increase in their wages when they enter the labor market, compared to those that had no exposure. To measure ability we use the Math test score in sixth grade. We classify individuals according to their abilities by quartiles. The quartile of reference includes low-ability students at the bottom of the distribution (first quartile). Then we use the same specification as in Equation 1, but with interactions of each indicator variable per quartile and the exposure variable to capture the effect of exposure by ability.

[8]We choose this percentage of enrollment based on a sensibility analysis that we summarize in Figure A.1.

[9]We measure geographical mobility as the distance in miles from the individual's home county to his/her working county.

Table 3: Exposure to English instruction and labor market outcomes by abilities (Social Security data)

	(1) Formal sector	(2) ln(wage)	(3) ln(distance)	(4) Move state
Panel A: Low enrollment sample				
Hrs English	-0.007	-0.013	-0.079	0.021**
	(0.009)	(0.012)	(0.049)	(0.010)
Eng×Q2	-0.003	-0.003	-0.018	-0.011
	(0.006)	(0.009)	(0.047)	(0.008)
Eng×Q3	-0.005	0.031***	0.012	-0.017
	(0.006)	(0.009)	(0.036)	(0.011)
Eng×Q4	-0.013**	0.012	0.106***	0.001
	(0.006)	(0.012)	(0.040)	(0.012)
Observations	1,554,827	259,666	259,666	259,666
Adjusted R^2	0.123	0.312	0.677	0.727
Panel B: Low enrollment sample (Men)				
Hrs English	-0.014	-0.010	-0.145**	0.008
	(0.012)	(0.018)	(0.064)	(0.014)
Eng×Q2	0.007	-0.001	-0.023	-0.005
	(0.009)	(0.011)	(0.060)	(0.010)
Eng×Q3	-0.006	0.040***	0.008	-0.014
	(0.011)	(0.014)	(0.049)	(0.012)
Eng×Q4	-0.013	0.010	0.104*	-0.001
	(0.011)	(0.017)	(0.058)	(0.014)
Observations	750,812	166,165	166,165	166,165
Adjusted R^2	0.149	0.315	0.680	0.729
Panel C: Low enrollment sample (Women)				
Hrs English	-0.007	-0.030*	0.029	0.042**
	(0.010)	(0.016)	(0.084)	(0.017)
Eng×Q2	-0.006	-0.007	-0.002	-0.024**
	(0.007)	(0.012)	(0.065)	(0.012)
Eng×Q3	-0.000	0.017*	0.017	-0.020
	(0.006)	(0.010)	(0.087)	(0.017)
Eng×Q4	-0.008	0.017	0.109	0.004
	(0.007)	(0.017)	(0.080)	(0.019)
Observations	804,015	93,501	93,501	93,501
Adjusted R^2	0.107	0.363	0.701	0.756
State of work FE	NO	YES	YES	YES

Note: This table shows the effect of exposure to English instruction on labor market outcomes by quartiles of abilities. The omitted category contains individuals with the lowest abilities. All regressions include controls. Standard errors clustered at school level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

My results suggest that exposure to English instruction has positive effects on the wages of high-ability students. In particular, one additional hour of exposure increases the wages of individuals in the third quartile by 3.1 percent (with respect to those with low abilities) and has no significant effect on geographical mobility. However, we find that the sample of individuals in the fourth quartile (top-ability students) still has a selection problem, which understates the wages estimate (even though, the point estimate is positive). This persistent selection problem may suggest that individuals with the highest abilities who had exposure are more likely to keep enrolled in school despite the lack of opportunities in their home counties. No further interpretation should be done for the geographical mobility of individuals in the top quartile due to the remaining selection problem. On the other hand, individuals in the second quartile are not affected by exposure to English instruction (neither on wages nor on geographical mobility). This may be due to the fact that low-ability students do not take advantage of their English courses or that they are not acquiring English skills.

Most of the positive effect on wages in the third quartile is driven by men, who see a four percent increase in wages if they had exposure to English instruction in primary school (with respect to low-achieving kids in the first quartile). The effect on women is about two percent but not statistically significant. As in the joint sample (men and women together), there are no differences in the effect on geographical mobility by the men quartiles, although women in the second quartile are less likely to move from their home state.

These results are consistent with individuals moving among economic industries. Indeed, we show that kids who had exposure to English instruction substitute jobs in agriculture and construction for jobs in manufacturing industries when they grow up. In particular, we find that men substitute jobs in construction for jobs in manufacturing, which may explain why their mobility got reduced. This is because the construction type jobs (mining, utilities and construction itself) are usually located in the suburbs or in less populated areas outside the central business districts (CBD), while manufacturing-type jobs are located in more populated areas or within the CBD (see columns (2) and (3) of panel C in Table 4). Similarly, women substitute jobs in agriculture for manufacturing jobs, which geographically means a movement from rural areas to a more urban context. This explains why women who had exposure become more mobile and are more likely to move from their home states (see columns (1) and (3) of panel D in Table 4).

The substitution of agriculture and construction for manufacturing industries is heterogeneous in abilities. First, we find that the substitution of agriculture for manufacturing industries is driven by individuals in the middle of the abilities distribution (second and third quartiles). This is maybe due to the fact that low-ability individuals are not able to learn English, resulting unaffected by the exposure. On the other hand, it is likely that individuals at the top of the abilities distribution show no effect because of the selection problem; in other words, they are still enrolled in school. Second, the substitution of construction for manufacturing industries is driven by low-ability individuals (first and second quartiles), while the overall effect for high-ability individuals is not statistically different from zero, although point estimate is still negative (see panel A of Table 5).

In general, all women are better off moving away from agriculture, but only a few high-ability men decide to move from construction to manufacturing when the conditions are

Table 4: Exposure to English instruction and economic industries (Social Security data)

	(1) Agri- culture	(2) Con- struction	(3) Manu- facturing	(4) Serv- ices
Panel A: Full sample				
Hrs English	-0.005***	-0.005***	0.000	0.010***
	(0.001)	(0.001)	(0.002)	(0.002)
Observations	4,055,434	4,055,434	4,055,434	4,055,434
Adjusted R^2	0.316	0.175	0.231	0.261
Panel B: Low enrollment sample				
Hrs English	-0.012**	-0.025**	0.040**	-0.003
	(0.006)	(0.010)	(0.017)	(0.016)
Observations	259,666	259,666	259,666	259,666
Adjusted R^2	0.402	0.388	0.342	0.292
Panel C: Low enrollment sample (Men)				
Hrs English (β^M)	-0.005	-0.026*	0.040**	-0.010
	(0.008)	(0.014)	(0.020)	(0.020)
Observations	166,165	166,165	166,165	166,165
Adjusted R^2	0.424	0.424	0.352	0.273
Panel D: Low enrollment sample (Women)				
Hrs English (β^W)	-0.024***	-0.006	0.043**	-0.012
	(0.008)	(0.006)	(0.021)	(0.024)
Observations	93,501	93,501	93,501	93,501
Adjusted R^2	0.446	0.139	0.383	0.383
$\beta^M = \beta^W$ [p-value]	[0.055]	[0.000]	[0.003]	[0.974]
Mean of dep. var.	0.11	0.16	0.39	0.34

Note: This table shows the effect of exposure to English instruction on economic industries. The sample contains all Mexican workers who belong to the cohorts 1997-2002, who are less than 25 and who are employed in some economic industry. All regressions include controls. Standard errors clustered at school level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

favorable (such as better wages, better work conditions or a closer workplace). Indeed, women move away from agriculture for all ability types, with a stronger effect in the second quartile, but men have a significant substitution only for high-ability individuals (see panels B and C of Table 5). Likewise, high-ability men (in the third quartile) behave differently from individuals in the rest of the abilities distribution when moving away from construction (with a smaller effect). These two latter results may explain why we found a significant increase in wages only for these types of individuals. In other words, high-ability individuals move away from agriculture but, although only men move away from construction, the effect on high-ability men is less strong in the substitution of construction for manufacturing industries. This heterogeneity driven by high-ability individuals conciliates with their increase in wages.

4.2 Mechanisms: Economic industries and cognitive abilities

In this subsection, we present results from two out of the three main potential mechanisms: the likelihood of working in industries with jobs requiring English skills and exposure affecting other cognitive abilities (measured with student achievement). First, we show that within manufacturing and services industries, we find movements from low-English intensive to high-English intensive jobs. Second, we provide evidence that exposure does not affect other cognitive skills, suggesting that the main results from the previous section could be explained by the acquisition of English skills.

4.2.1 English instruction and economic industries

One of the mechanisms that explain the results shown previously is the possibility that, after exposure, individuals are more likely to find jobs that require English skills when they enter the labor market. In this subsection, we describe the effect of exposure to English instruction on the likelihood to work in different types of economic industries (to their requirements of English abilities). Since the industries classification sums to one, opposite signs can be interpreted as substitutions.

This particular mechanism could help to explain what is making more attractive the manufacturing industry and why there is no apparent change in services. The potential explanation may suggest that exposure to English instruction increased the probability of working in jobs requiring English abilities, such as automotive and telecommunications. we provide evidence that this story is actually plausible (see Panel B of Table 6).

To explore the possibility that individuals who had exposure are more likely to work in jobs requiring English abilities, we use the industries classification proposed by Gálvez-Soriano (2023). This classification shows what are the industries with more proportion of workers with English skills. Although the original sorting is expressed at a two-digit NAICS code, it is possible to have a finer classification using a four-digit NAICS code. On the other hand, we are aware that this proportion of English speakers does not necessarily mean that those industries actually require English abilities but there should be some correspondence.

Using this classification we define a manufacturing industry as high-English intensive if the proportion of workers with English skills is greater than 0.8 percent (which corresponds

Table 5: Exposure to English instruction and economic industries by abilities (Social Security data)

	(1) Agri-culture	(2) Con-struction	(3) Manu-facturing	(4) Serv-ices
Panel A: Low enrollment sample				
Hrs English	-0.005	-0.035***	0.049***	-0.008
	(0.007)	(0.010)	(0.018)	(0.018)
Eng×Q2	-0.014***	0.006	-0.010	0.017
	(0.004)	(0.005)	(0.011)	(0.011)
Eng×Q3	-0.011*	0.020***	-0.008	-0.001
	(0.006)	(0.006)	(0.012)	(0.012)
Eng×Q4	-0.005	0.022***	-0.022*	0.004
	(0.006)	(0.007)	(0.013)	(0.010)
Observations	259,666	259,666	259,666	259,666
Adjusted R^2	0.402	0.388	0.342	0.292
Panel B: Low enrollment sample (Men)				
Hrs English	0.002	-0.036***	0.041*	-0.006
	(0.010)	(0.014)	(0.022)	(0.020)
Eng×Q2	-0.011	0.004	0.007	0.001
	(0.007)	(0.007)	(0.012)	(0.013)
Eng×Q3	-0.016**	0.023***	-0.009	0.002
	(0.007)	(0.008)	(0.015)	(0.015)
Eng×Q4	-0.004	0.029***	-0.004	-0.021
	(0.009)	(0.011)	(0.018)	(0.014)
Observations	166,165	166,165	166,165	166,165
Adjusted R^2	0.424	0.424	0.352	0.273
Panel C: Low enrollment sample (Women)				
Hrs English	-0.018**	-0.008	0.062**	-0.036
	(0.009)	(0.006)	(0.028)	(0.028)
Eng×Q2	-0.018*	0.003	-0.029	0.044**
	(0.010)	(0.007)	(0.024)	(0.018)
Eng×Q3	-0.005	0.000	0.000	0.004
	(0.011)	(0.004)	(0.022)	(0.021)
Eng×Q4	-0.006	0.003	-0.052**	0.054***
	(0.009)	(0.005)	(0.022)	(0.020)
Observations	93,501	93,501	93,501	93,501
Adjusted R^2	0.446	0.139	0.384	0.383
Shares	0.04	0.08	0.35	0.53

Note: This table shows the effect of exposure to English instruction on economic industries, by abilities. The sample contains all Mexican workers who belong to the cohorts 1997-2002, who are less than 25 and who are employed in some economic industry. All regressions include controls. Standard errors clustered at school level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Exposure to English instruction and economic industries (Social Security data)

	(1)	(2)	(3)	(4)
	Manufacturing		Services	
	High English	Low English	High English	Low English
Panel A: Full sample				
Hrs English	0.006***	-0.008***	0.016***	-0.004**
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	4,055,434	4,055,434	4,055,434	4,055,434
Adjusted R^2	0.098	0.128	0.092	0.099
Panel B: Low enrollment sample				
Hrs English	0.060***	-0.026**	0.046***	-0.039***
	(0.013)	(0.012)	(0.014)	(0.011)
Observations	259,666	259,666	259,666	259,666
Adjusted R^2	0.175	0.189	0.145	0.116
Panel C: Low enrollment sample (Men)				
Hrs English (β^M)	0.075***	-0.035**	0.033**	-0.035**
	(0.016)	(0.016)	(0.015)	(0.014)
Observations	166,165	166,165	166,165	166,165
Adjusted R^2	0.175	0.202	0.163	0.111
Panel D: Low enrollment sample (Women)				
Hrs English (β^W)	0.038*	-0.011	0.047*	-0.039*
	(0.020)	(0.018)	(0.027)	(0.023)
Observations	93,501	93,501	93,501	93,501
Adjusted R^2	0.226	0.229	0.191	0.173
$\beta^M = \beta^W$ [p-value]	[0.058]	[0.070]	[0.454]	[0.594]
Shares	0.17	0.17	0.29	0.24

Note: This table shows the effect of exposure to English instruction on economic industries according to their requirements to English abilities. The sample contains all Mexican workers who belong to the cohorts 1997-2002, who are less than 25 and who are employed in the formal sector. All regressions include controls. Shares are obtained from the low-enrollment sample and are expressed with respect to the universe of economic industries. Standard errors clustered at school level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

to the highest quartile of the distribution of industries by the proportion of this type of workers). Industries in this classification include: Animal food manufacturing, Beverage and tobacco industries, Apparel manufacturing, Leather and hide tanning, Wood industry, Paper industry, Chemical industry, Nonmetallic mineral products, Metal products, Manufacturing of computer and communications equipment, Electric appliances, and Transportation equipment. The complement of this group constitutes the low-English-intensive manufacturing industries.

Similarly, services with a high proportion of English speakers include: Wholesale trade of groceries, Wholesale trade of industrial machinery, Retail trade in self-service shops, Retail trade of health care items, Retail trade of perfumery and jewelry products, Retail trade of household furniture, Retail trade of automotive parts and accessories, Freight truck transportation, Warehousing services, Telecommunications, Commercial and industrial machinery and equipment rental, Administrative and support services, Artistic, cultural and sporting services, Traveler accommodation, Special food services, Drinking places, Personal and household goods repair and maintenance, Religious organizations, Personal and household goods repair and maintenance, and Justice, public order, and safety activities. We classify these industries if they have more than 0.8 percent of workers with English skills. The complement of these services industries constitutes the low-English intensive jobs in services.[10]

My results suggest that workers who had exposure to English instruction in primary school substitute jobs in low-English intensive industries for high-English intensive ones. Indeed, individuals who had exposure to English instruction are more likely to find jobs in high-English manufacturing industries than in low-English ones. In particular, we show that one additional hour of English instruction increases the probability that they end up in this type of industry by six percent. On the other hand, one additional hour of English instruction in primary school makes individuals 4.6 percent more likely to find a high-English intensive service-industry job, and less likely to find a low-English intensive one.

Substitutions within manufacturing industries are driven by men, while within services there are no significant differences between men and women. This may suggest that despite men's geographical mobility got reduced (as shown in Table 2), they found potentially better opportunities within the same industry, while women are facing more obstacles to do so (even though they increased their geographical mobility). Indeed, although point estimates suggest that women move from high to low English-intensive jobs, these estimates are barely significant. Furthermore, this substitution is double the size for men. On the other hand, men have a very significant substitution between high and low English-intensive services industries (in favor of the former) and, although point estimates suggest a larger effect for women, these estimates are barely statistically different from zero and there is no statistical difference between men and women estimates.

On the other hand, we found that high-ability men (who are positively affected by English instruction in terms of wages) have a weaker substitution for low-English-intensive manufacturing jobs than low-ability men. This may suggest that some of the occupations for English speakers in manufacturing industries are less intensive in communication skills. Furthermore, we found that English abilities pay off more in the services industry. And, high-ability women

[10] In the appendix A.3, Table A.3 and Table A.4, we provide a detailed description of the economic industries classified as high-English intensive using a four-digit NAICS code.

have a stronger substitution effect within manufacturing industries, in favor of high English-intensive jobs. On the other hand, these high-ability women have a weaker substitution in the services industries (relative to men), resulting in a worse allocation of their potentially acquired English abilities (see Table A.2 from appendix A.3).

4.2.2 English instruction and student achievement

Results from estimating Equation 2 provide evidence that the English program had no effect on test scores. Indeed, we find that the NEPBE had no effect on neither Language nor Math test scores. This result suggests that the effects on the labor market outcomes (explained previously) are consistent with the acquisition of English abilities. In other words, although it is not possible to test directly the first stage effect of exposure to English instruction (due to the NEPBE) on English skills, we provide suggestive evidence that the English program did not affect other cognitive skills. As we explained in subsection 2.2, school FE in this model account for most of the selection bias problem, which provides more reliability of my findings.

We show the results of estimating my TWFE model exploiting the panel structure of my database in Table 7. To obtain the results of this table, we are using the same cohorts we examined with the labor market outcomes model, so we can think about this as analyzing the Spanish and Math skills at the end of primary school due to the English exposure for those same individuals we examine their labor market outcomes later in life.

There are three interesting things to notice from Table 7. First, there is a selection of which schools offer English instruction in Mexico. Second, we find no effect of English instruction on test scores (neither language nor mathematics). And, third, the nonexistent effect on student achievement rules out the potential second mechanism, which suggested a possible effect on other cognitive skills.

First, as noted previously, there is a selection of which schools offer English instruction. Indeed, when we control for school FE, taking care of many time-invariant characteristics of schools and neighborhoods, the estimated coefficient associated with the exposure to English instruction variable goes down. To see this implication compare columns (1) and (2) of panel A in Table 7, for language abilities, and columns (3) and (4) for mathematics abilities. We obtain similar results in Table A.1, for the main labor market outcomes.

Second, we find no effect of English instruction on test scores. This means that English instruction did not reduce neither language (Spanish) nor mathematics skills, as feared if more time is devoted to English at the expense of other subjects. On the other hand, it did not increase language skills either, suggesting no complementarities between English and Spanish in the context of Mexico.

Third, this nonexistent effect on math and language abilities suggests is consistent with exposure increasing the acquisition of English skills. In other words, it is likely that the previously discussed estimates, regarding the effect of English instruction on labor market outcomes, are not driven by learning in other subjects, so it could be primarily interpreted as the direct effect of the acquisition of English language skills.

Table 7: Exposure to English instruction and student achievement

	(1) Language 6th	(2) Language 6th	(3) Math 6th	(4) Math 6th
Panel A: Full sample in ENLACE database				
Hrs English	0.0335***	0.0099*	0.0155***	-0.0081
	(0.0033)	(0.0054)	(0.0036)	(0.0062)
Observations	16,938,183	16,938,183	16,938,183	16,938,183
Adjusted R^2	0.426	0.472	0.429	0.482
Panel B: Full sample in Social Security data				
Hrs English	0.0284***	-0.0015	0.0105***	-0.0225***
	(0.0033)	(0.0075)	(0.0037)	(0.0086)
Observations	4,055,434	4,055,434	4,055,434	4,055,434
Adjusted R^2	0.404	0.453	0.413	0.470
Panel C: Low enrollment sample				
Hrs English	0.0476	0.0548	0.0094	0.0128
	(0.0470)	(0.0867)	(0.0344)	(0.0652)
Observations	259,666	259,666	259,666	259,666
Adjusted R^2	0.351	0.444	0.381	0.478
Panel D: Low enrollment sample (Men)				
Hrs English (β^M)	0.0637	0.0824	0.0147	0.0104
	(0.0531)	(0.0928)	(0.0386)	(0.0794)
Observations	166,165	166,165	166,165	166,165
Adjusted R^2	0.310	0.426	0.369	0.481
Panel E: Low enrollment sample (Women)				
Hrs English (β^W)	0.0162	-0.0085	-0.0003	0.0080
	(0.0385)	(0.0968)	(0.0325)	(0.0673)
Observations	93,501	93,501	93,501	93,501
Adjusted R^2	0.371	0.487	0.398	0.521
$\beta^M = \beta^W$ [p-value]	[0.3015]	[0.4168]	[0.9693]	[0.9358]
State FE	YES	NO	YES	NO
School FE	NO	YES	NO	YES

Note: This table shows the effect of exposure to English instruction on test scores. All regressions include controls. The sample contains students who later in life will work in the formal sector and who studied primary school in counties where the upper-secondary and college enrollment rates are low. Standard errors clustered at school level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5 Robustness checks and other potential mechanisms

5.1 Biased estimates under heterogeneous treatment effects?

Recent literature has shown that the estimates from TWFE and Difference in Differences (DD) settings may be biased if the treatment effects are heterogeneous among groups or over time (see De Chaisemartin and D'Haultfoeuille (2022), for a review). In the TWFE strategy, which we use in this paper, the estimator of interest can be expressed as a weighted average of all possible two-group/two-period DD estimators in the data (Goodman-Bacon, 2021). One of the potential causes of the biased estimates is that some of these weights may be negative. Hence, the TWFE estimator may not satisfy the "no-sign reversal property".

In this paper, we propose a setting in which the treatment variable is continuous (as it is measured in hours) and the treatment is heterogeneous among schools and cohorts. Hence, it is likely that my estimates are biased. To deal with the recent critics of the TWFE and DD strategies we respond with three initial steps. First, we compute the weights of my TWFE estimator to determine if we are obtaining a biased estimate due to negative weights. Second, we propose an alternative estimate using a binary treatment. Finally, we drop the always-treated groups in my database. Alternative estimates will be proposed in future versions of this paper.

First, we compute the weights of my TWFE estimator in the wage equation, using the procedure proposed by De Chaisemartin, D'Haultfoeuille and Deeb (2020). We find that almost half of my weights are negative, which could produce a biased estimate. In fact, it is likely that my estimate represents a lower band as it is compatible with a Data Generating Process where the average of the treatment effects is equal to zero, while previous estimates in the literature suggest a positive effect (see for example Delgado Helleseter (2020) and Gálvez-Soriano (2023)).

Second, De Chaisemartin and D'Haultfoeuille (2022) show that with a non-binary treatment (as in our case), it becomes more likely that some of the weights are negative. Hence, we propose an alternative estimate using a binary treatment variable. We construct this variable as a dummy, which takes the value of one if individual i attended a primary school that offered English instruction, and zero otherwise. My results suggest that with this specification now only (more than) one-fourth of the weights are negative. Furthermore, my estimates support the idea that the original TWFE estimation may be downward biased in the wage equation as the now the point estimate is slightly positive but not statistically significant, which also supports the robustness of my results (see Panel A of Table 8).

Finally, an alternative solution to mitigate the negative weights is to drop the always-treated groups, as recently proposed by Jakiela (2021). In our case, we eliminate the schools that already had English instruction before the implementation of the NEPBE. In fact, just by dropping the always-treated groups, we reduce the negative weights to represent more than one-third of the total number of weights. Hence, we combine both solutions to obtain an additional estimate formed with one-fourth of negative weights. My results suggest that my original estimates are robust to improvements on the estimator's weights. Furthermore,

Table 8: Solutions for TWFE with heterogeneous treatment effects (Social Security data)

	(1) Formal sector	(2) ln(wage)	(3) ln(distance)	(4) Move state
Panel A: Binary treatment				
Eng	-0.009	0.000	-0.020	0.014*
	(0.006)	(0.011)	(0.042)	(0.008)
Observations	1,554,827	259,666	259,666	259,666
Adjusted R^2	0.125	0.292	0.675	0.726
Panel B: Binary treatment w/o always treated				
Eng	-0.011*	0.002	-0.016	0.016*
	(0.006)	(0.011)	(0.043)	(0.009)
Observations	1,531,834	254,287	254,287	254,287
Adjusted R^2	0.125	0.292	0.675	0.726

Note: This table shows the effect of exposure to English instruction on labor market outcomes. The sample in Panel A contains all Mexican workers who belong to the cohorts 1997-2002, who are less than 25 and who are employed in the formal sector. Sample in Panel B contains individuals in Panel A who attended primary schools, which did not offer English instruction before the implementation of the NEPBE. All regressions include controls. Standard errors clustered at school level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

we offer suggestive evidence that my original estimates from the wage equation may be downward biased (see Panel A of Table 8).

5.2 Different measure of exposure: English teachers

The results we offer in this paper rely on the exposure variable we propose. We constructed this exposure variable using weekly hours of English instruction (by class). The Mexican school census contains information reported by the school Principal. Among this information, the Principal reports the total weekly hours of English instruction in the school. That is why we express this variable in a more comprehensive unit divided by the number of classes in school. Then, by school-cohort we average the hours of exposure along the six years that comprise elementary school in Mexico (from first to sixth grade).

A potential critique of this exposure variable suggests that it may be noisy to measure exposure in hours. The reason is that since the school's Principal is the one who reports the total hours of English instruction, summing over all existing classes in the school may involve a measurement error. Hence, using an alternative measure with minimum or no-measurement error would provide more reliable results. Furthermore, showing that the estimates with this alternative exposure variable and the original one are similar in direction and statistical significance, would provide robustness to my original findings.

An alternative measure of exposure could be the number of English teachers in school. The potential measurement error would be minimal with this alternative exposure variable

Table 9: English instruction and labor market outcomes (Alternative exposure variable)

	(1) Formal sector	(2) ln(wage)	(3) ln(distance)	(4) Move state
Panel A: Full sample				
Eng Teachers	-0.182***	-0.167***	-0.419***	-0.048**
	(0.020)	(0.029)	(0.114)	(0.019)
Observations	16,938,183	4,055,434	4,055,434	4,055,434
Adjusted R^2	0.105	0.270	0.477	0.555
Panel B: Low enrollment sample				
Eng Teachers	-0.202*	-0.127	-0.772	0.072*
	(0.120)	(0.196)	(0.751)	(0.040)
Observations	1,554,827	259,666	259,666	259,666
Adjusted R^2	0.123	0.312	0.677	0.727
Panel C: Low enrollment sample (Men)				
Eng Teachers (β^M)	-0.140	-0.290	-1.644*	-0.086
	(0.173)	(0.294)	(0.983)	(0.226)
Observations	750,812	166,165	166,165	166,165
Adjusted R^2	0.149	0.315	0.680	0.729
Panel D: Low enrollment sample (Women)				
Eng Teachers (β^W)	-0.273*	0.078	0.866	0.295*
	(0.149)	(0.306)	(1.106)	(0.169)
Observations	804,015	93,501	93,501	93,501
Adjusted R^2	0.107	0.363	0.700	0.756
$\beta^M = \beta^W$ [p-value]	[0.023]	[0.757]	[0.083]	[0.084]
State of work FE	NO	YES	YES	YES

Note: This table shows the effect of exposure to English instruction on labor market outcomes. The sample contains all Mexican workers who belong to the cohorts 1997-2002, who are less than 25 and who are employed in the formal sector (using the Social Security data). This table is different from Table 2 because here I use an alternative exposure variable; number of English teachers instead of hours of English instruction. All regressions include controls. Standard errors clustered at school level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

because it is easier to sum teachers than hours. We construct this alternative variable using an analogous methodology as we did for hours of English instruction. First, we expressed the total number of English teachers in relative terms to the number of classes in the school. Second, for each school-cohort, we average the number of teachers over the six years that comprise elementary schools in Mexico.

Results shown in Table 9 suggest that the main results provided in this paper are robust to changes in the measure of exposure. First, Panel A supports the sample selection problem we identified previously because individuals who had exposure are less likely to work in the formal sector. Using the previously proposed low-enrollment sample, we mitigate the sample selection. Furthermore, we do not find any significant effect of exposure on wages, which is consistent with my previous findings. we also find the same direction of the effect of exposure on geographical mobility. However, this effect is less statistically significant for both, men and women. Part of the less significant effect on geographical mobility could be due to the remaining sample selection that is driven by women who are still less likely to work in the formal sector. This latter finding may suggest that women who had exposure to English instruction are more likely to either be enrolled in school or to stay at home doing housework.

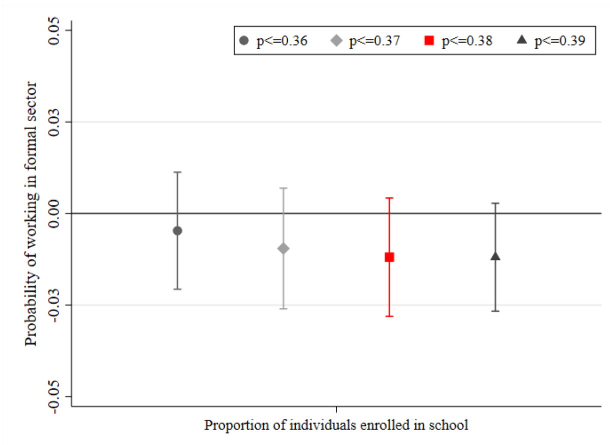
5.3 Different low-enrollment rates

We proposed to solve the sample selection problem by considering only a low-enrollment sample, which reduces the likelihood that an individual who had exposure continues enrolled in school. Indeed, in Table 2 we offered evidence that kids who had exposure to English instruction are less likely to work in the formal sector. We claimed that this finding is a result of exposure affecting school enrollment (as suggested by Figure 2). Furthermore, individuals observed in my sample are still young enough to have more likelihood to be enrolled in school. Hence, a sub-sample of counties with low enrollment rates would be less likely to have a selection problem.

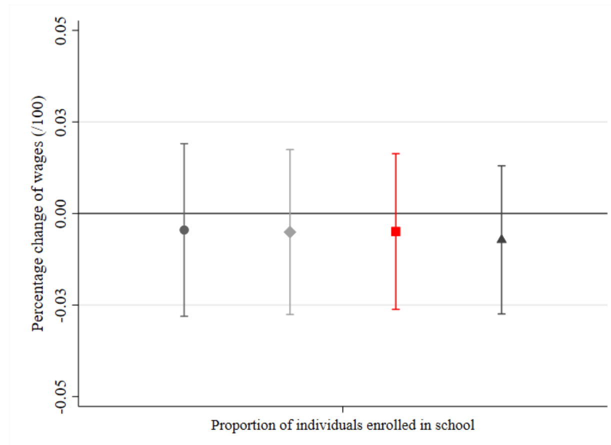
However, a valid critique of this proposed solution is the way we chose the low-enrollment sample. In particular, after looking at Figure A.1 one could think that it is arbitrary to choose a cutoff of 38 percent enrollment rate, especially because we could have used enrollment rates between 36 and 39 percent. In this subsection, we use those alternative cutoffs to show that my main results are robust to changes in the low-enrollment rate cutoff.

In Figure 9 we show that choosing counties with higher or lower enrollment rates does not significantly change the main results. In fact, point estimates are about at the same level, in the same direction and the estimates are statistically the same. Maybe the only outcome that seems to be more sensitive to the threshold we choose is the distance from home to the working county. Indeed, with a sub-sample of counties with an enrollment rate less or equal to 39 percent, the estimate for distance is close to being significant. However, this effect is only driven by men (as we explained in subsection 4.1), which does not affect the story of more mobility among women and less mobility among men, as a consequence of exposure to English instruction.

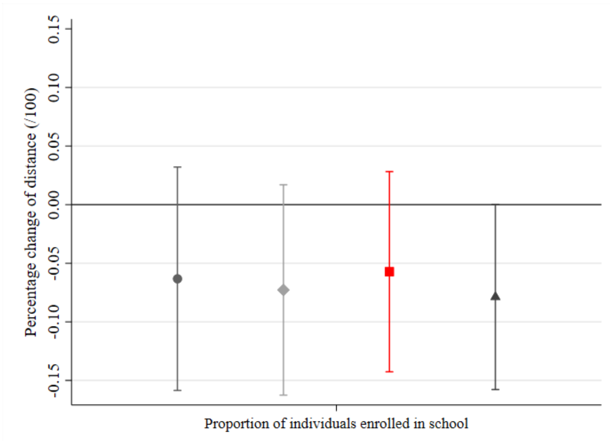
Figure 9: Effect on labor market outcomes with different enrollment rate samples



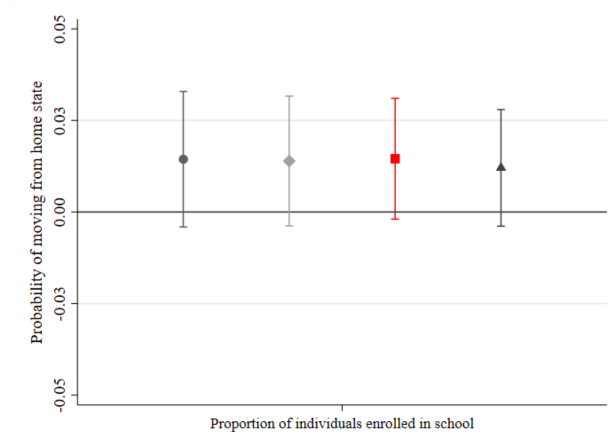
(a) Formal sector



(b) Ln(monthly wage)



(c) Distance home-job county



(d) Moves from home state

Note: Estimates from different regressions are plotted, where the difference comes from the low enrollment samples used.

5.4 Should we worry about non-fixed labor market conditions among cohorts?

The identifying assumption in my main specification (shown in Equation 1) suggests that the omitted variables that explain the differences between schools that adopted the NEPBE and the schools that did not are fixed among cohorts. Due to the narrow span of cohorts, we study in this paper, we could be confident that this assumption is not too strong. Indeed, we could assume that the six cohorts we study in this paper had access to the same conditions within the same school (except for the exposure to English instruction) and the same market opportunities within the same local market. In other words, the only difference among individuals within the same school, but from different cohorts, is the exposure to English instruction.

Table 10: Exposure to English instruction and labor market outcomes (with state-by-cohort FE, Social Security data)

	(1) Formal sector	(2) ln(wage)	(3) ln(distance)	(4) Move state
Panel B: Low enrollment sample				
Hrs English	-0.007	0.008	-0.045	0.012
	(0.010)	(0.013)	(0.051)	(0.010)
Observations	1,554,827	259,666	259,666	259,666
Adjusted R^2	0.124	0.313	0.677	0.728
Panel C: Low enrollment sample (Men)				
Hrs English (β^M)	-0.013	0.014	-0.113*	0.005
	(0.014)	(0.019)	(0.065)	(0.013)
Observations	750,812	166,165	166,165	166,165
Adjusted R^2	0.150	0.317	0.680	0.730
Panel D: Low enrollment sample (Women)				
Hrs English (β^W)	-0.003	-0.003	0.021	0.023*
	(0.011)	(0.017)	(0.090)	(0.013)
Observations	804,015	93,501	93,501	93,501
Adjusted R^2	0.108	0.365	0.701	0.757
$\beta^M = \beta^W$ [p-value]	[0.013]	[0.452]	[0.196]	[0.096]
State of work FE	NO	YES	YES	YES

Note: This table shows the effect of exposure to English instruction on labor market outcomes. The sample contains all Mexican workers who belong to the cohorts 1997-2002, who are less than 25 and who are employed in the formal sector. All regressions include controls. Standard errors clustered at school level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

However, we may be still worried about individual attributes that can be correlated to our outcome variables. For example, some schools were selected in locations where it could be profitable to expand English instruction as demanded by some economic industries in the region. A practical example is the Mexican northern states, which may be more interested in offering English instruction at school due to their close relation to the US. Other example includes the traditional tourist states, such as Baja California Sur (Los Cabos) and Quintana Roo (Cancun), where English is demanded for tourism-related jobs.

To test this potential concern, we propose an optional specification where we include state by cohort FE. In this specification, each state r has its own cohort FE, which would be consistent with the story of different cohort trends by region. This analysis will allow me to avoid comparing a rapid growth expectation state to a low growth one. In particular, we propose a similar specification as in Equation 1, but including state by cohort FE (ϕ_{rc}) as follows,

$$y_{isc} = \alpha + \beta \cdot ExpEng_{sc} + \boldsymbol{X_{isc}\gamma} + \zeta_c + \nu_s + \tau_t + \phi_{rc} + \varepsilon_{isc}. \quad (4)$$

My results suggest that different cohort trends by region are not an issue in this paper. Indeed, even after including state by cohort FE, we do not find significant differences with my main specification. The main potential difference is a loss of significance in the effect of exposure on mobility (in terms of distance) among women, but the point estimate still suggests a positive effect (see Table 10).

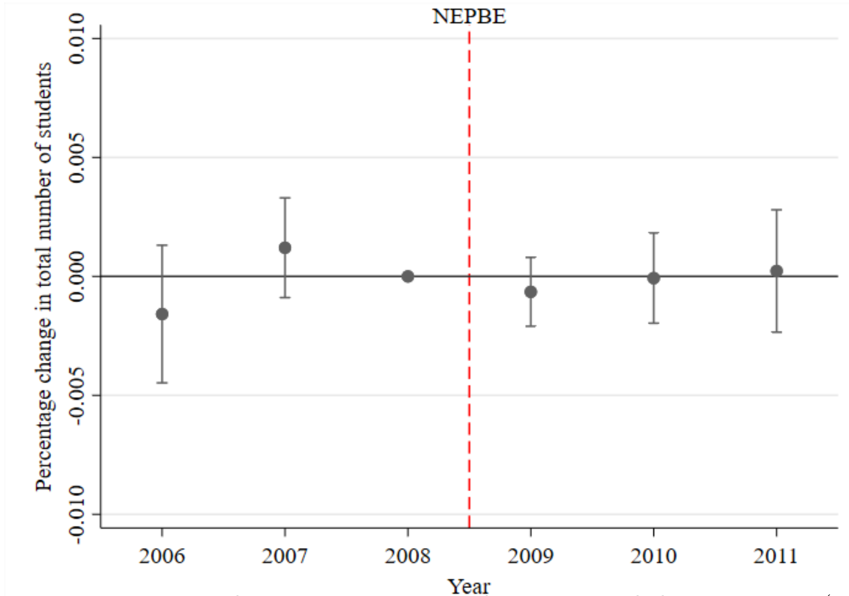
5.5 Did the NEPBE affect enrollment in private schools?

One potential concern is that the introduction of the English program in elementary schools had affected the socio-demographic composition of children enrolled in school and, hence, their potential labor market outcomes. In Mexico, the English language is a subject usually taught in private elementary schools, which makes them more attractive to parents because public schools do not offer this subject. With the incorporation of the English language in the regular curricula of public elementary schools (due to the NEPBE), it is possible that parents could change their decision to send their kids to private schools. This decision is particularly relevant to parents who have just enough budget to send their kids to private schools, but not enough to make this an unconstrained decision.

In this subsection, we offer evidence that the introduction of the NEPBE did not affect enrollment in private schools. we use data from the Mexican school census for six years around the intervention (2006-2011), which also coincides with the cohorts we study in this paper. We only work with data from private elementary schools.

To accomplish this task, we propose a TWFE model that exploits two sources of variation: school (some schools offered English instruction and some did not) and year (before and after the introduction of the NEPBE). In particular, we explore the effect of English instruction, $HrsEng_{st}$, measured in weekly hours of instruction per class, on the percentage change of kids enrolled in private schools, $Enrollment_{st}$. The results, we show in this subsection come from an event study-type equation, as follows:

Figure 10: English instruction and enrollment in private elementary schools.



Note: Plotted estimates represent the interaction terms between English instruction (expressed in weekly hours per class) and an indicator function for each year (2006-2011) in an event study type regression. The omitted year is 2008. Data contains only private elementary schools. The vertical dotted line indicates the introduction of the English program in public primary schools. The no statistically significant estimates at the left of the vertical dotted line suggest parallel trends before the policy implementation.

$$Enrollment_{st} = \theta + \sum_t \lambda_t \cdot HrsEng_{st} \times I_{(HrsEng_{st}=t)} + \boldsymbol{X_{st}\Psi} + \nu_s + \tau_t + \varepsilon_{st}, \tag{5}$$

where $I_{(HrsEng_{st}=t)}$ is an indicator function that takes the value of one if school s offered English instruction in time t. Notice that we control for school fixed effects, ν_s, time fixed effects, τ_t, and a vector of controls, $\boldsymbol{X_{st}}$, containing information of parents' expenditure on school supplies, uniforms and tuition, which may also affect enrollment.

Estimates of λ_t from Equation 5 are plotted in Figure 10. We do not find evidence that the NEPBE had affected enrollment in private elementary schools. Furthermore, we do not find pre-existing trends, which support the validity of my estimates.

5.6 Did the NEPBE affect other school inputs?

A previously unexplored mechanism may suggest that the introduction of English instruction had affected school inputs other than the effective teaching time, such as the number of teachers. In subsection 4.2 we showed that exposure to English instruction did not affect other cognitive skills, ruling out this potential mechanism. In this subsection, we study the most important school inputs: teachers.

The idea is the following: the NEPBE required hiring English teachers, however, since resources are scarce, it could be the case that schools had substituted the regular elementary school teachers for English teachers. This would represent an important mechanism that could affect unobserved cognitive (other than language and mathematics) and non-cognitive abilities. And, although this additional mechanism does not affect my results and main conclusions, it could shed more light on why we did not find an average significant effect of exposure on wages.

we use the Mexican school census to analyze this additional mechanism. As in the previous exercise (of subsection 5.5), we concentrate on six years around the intervention (2006-2011), which also coincides with the cohorts we study in this paper. The difference is that now we only work with data from public elementary schools. We also restrict my sample to include only the morning shift and to exclude schools that participated in the FTS program, as explained in subsection 3.1.

We propose a TWFE model that exploits school-by-year variation. We explore the effect of English instruction, $HrsEng_{st}$, measured in weekly hours of instruction per class, on the percentage change of teachers in school s at time t, $Teachers_{st}$. We are able to observe the number of teachers, depending on their schooling level: elementary school, middle school, high school, and college graduates. The results, we show in this subsection come from an event study-type equation, as follows:

$$Teachers_{st} = \delta + \sum_{t} \rho_t \cdot HrsEng_{st} \times I_{(HrsEng_{st}=t)} + \boldsymbol{X_{st}}\boldsymbol{\Pi} + \nu_s + \tau_t + \varepsilon_{st}, \tag{6}$$

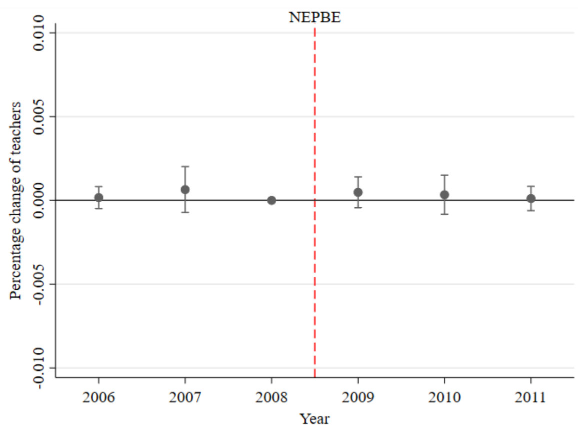
where $I_{(HrsEng_{st}=t)}$ is an indicator function that takes the value of one if school s offered English instruction at time t. Notice that we control for school fixed effects, ν_s, time fixed effects, τ_t, and a vector of controls, $\boldsymbol{X_{st}}$, containing information of geographical context (urban/rural) and the total number of students.

My results suggest that English teachers hired after the NEPBE have a low schooling level (middle school and high school) and that there may be a substitution of teachers with a college degree for low-schooling English teachers. Indeed, since the school census does not allow me to observe English teachers by their educational levels, results from Figure 11 have a potential explanation, as follows. First, from previous results in Figure 4 we know that the NEPBE induced schools to hire English teachers. Hence, any increase in the number of teachers after the intervention can be attributed to the English program. In our particular case, the policy increased the number of teachers with elementary school and middle school education. Second, we do not find strong evidence that schools substituted regular teachers for English teachers, but points estimates support this idea. In particular, it seems that schools substituted teachers with a college degree for low-educated English teachers.

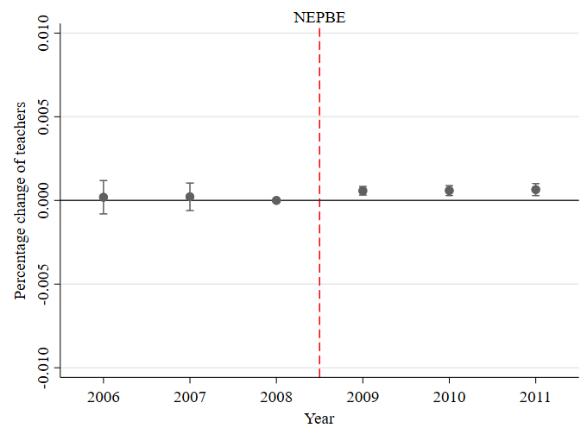
6 Conclusions and discussion

In this paper, we evaluate the effect of exposure to English instruction at early stages of life on labor market outcomes, in the context of a non-English speaking country. For this purpose, we construct a novel database in which we observe primary school students of all Mexican schools, their student achievement, and their labor market outcomes around 10 years after graduation from primary school. We exploit school by cohort variation of exposure to

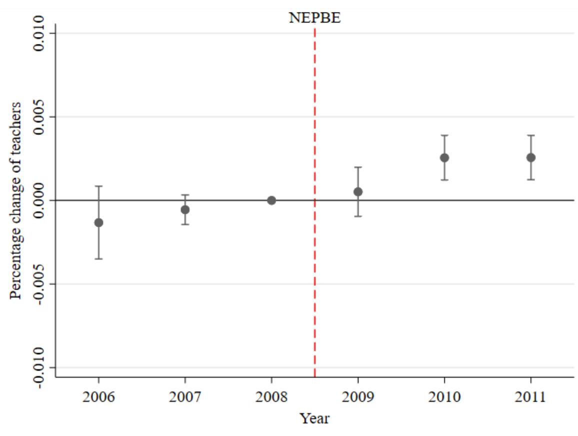
Figure 11: English instruction and teachers (by schooling)



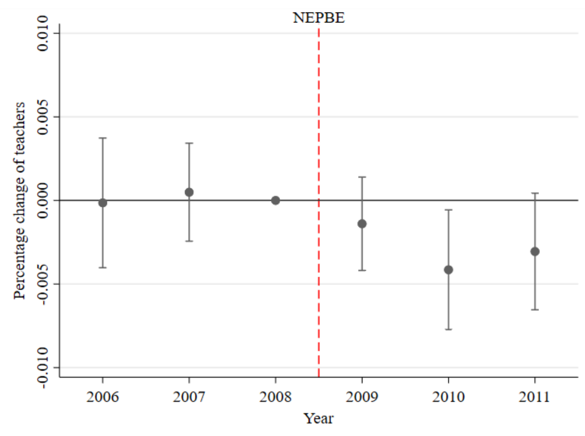
(a) With elementary school



(b) With middle school



(c) With high school



(d) With college degree

Note: Plotted estimates represent the interaction terms between English instruction (expressed in weekly hours per class) and an indicator function for each year (2006-2011) in an event study type regression. The omitted year is 2008. Data contains only public elementary schools in the morning shift and excludes schools that participated in the FTS program. The vertical dotted line indicates the introduction of the English program in public primary schools. The no statistically significant estimates at the left of the vertical dotted line suggest parallel trends before the policy implementation.

English language instruction in a TWFE model. My proposed specification relies on a school FE approach and explicit control for individuals' abilities, both of which intend to mitigate the selection problem.

We face a selection bias problem caused by schools that self-selected to participate in the NEPBE and because of a potential bias for omitted unobservable variables (cognitive abilities). The self-selection would result in an unfair comparison between schools with more information, more resources, better teachers or located in better neighborhoods with schools that did not participate in the English program because of the lack of information and resources. We solved this selection problem by controlling for school FE, which allowed me to compare students within the same school, but with different exposure to English instruction (different cohorts). Additionally, we explicitly control for individual's abilities (using test scores in sixth grade), which is usually an unobservable variable in the literature.

We deal with a second selection problem: sample selection. Indeed, we find that exposure to English instruction reduces the likelihood that an individual participates in formal sector employment. It is likely that this result is due to exposure to English instruction affecting enrolling in high school and college, as my analysis focuses on young adults aged 16-24 (the recency of the NEPBE means the affected cohorts are still young). We propose to analyze a low-enrollment sample to solve this second selection problem. Indeed, since the selection into the sample is potentially caused by children who decide to pursue a high school or college degree, we could mitigate this selection problem by considering a sub-sample of individuals living in counties with low college enrollment rates.

Focusing on a sub-sample that is unlikely to be enrolled by age 16, we find that exposure to English instruction did not affect wages, but individuals who had exposure are better off because they are moving to jobs that require less physical work. Indeed, we find that exposure to English instruction has no average effect on wages, but individuals who had exposure substitute jobs in agriculture and construction for jobs in manufacturing industries. Furthermore, we offer evidence that men who had exposure only substituted construction for manufacturing industries, which are closer to their home counties. On the other hand, women substitute agriculture for manufacturing industries, moving from rural to urban areas (they work farther from their home counties and they are more likely to move from their home states).

Although we do not find wage improvements after exposure to English instruction, for the average worker, we do find a positive effect on the wages of high-ability individuals. This finding is the result of an analysis of heterogeneity by abilities, where we document that individuals in the third quartile of the abilities distribution have higher wages if they had exposure in primary school. This effect is driven by men, although women have also a positive effect after exposure. Furthermore, there is a persistent selection problem among individuals in the top quartile, suggesting that even despite the lack of schools in their home counties, these high-ability individuals keep enrolled in school.

Gender heterogeneity and the improvement of wages among high-ability individuals could be due to the gender wage gap affecting in two main ways: unequal substitution among economic industries and unequal substitution within the same industries. First, it is likely that the gender wage gap creates incentives for women to substitute agriculture for manufacturing industries, no matter their ability level. Namely, only high-ability women are looking for fairer labor conditions. On the other hand, due to the lack of incentives generated by the gender wage gap among men, only high-ability men substitute in the same direction as women, while their substitution of construction for manufacturing industries is weaker than for low-ability men. This latter result suggests that a few high-ability men decide to move from construction to manufacturing industries only when the labor conditions are more favorable in this latter (such as better wages, better work conditions or a closer workplace). Second, high-ability men have a weaker substitution of low-English intensive manufacturing jobs than low-ability men. This may suggest that some of the occupations for English speakers within manufacturing industries are less intensive in communication skills. Furthermore, we found that English abilities pay off more in the services industry. And, high-ability women have a stronger substitution effect within manufacturing industries, in favor of high English-intensive jobs. On the other hand, these high-ability women have a weaker substitution in the services industries (relative to men), resulting in a worse allocation of their potentially acquired English abilities

It is likely that these effects on labor market outcomes are consistent with exposure increasing the acquisition of English abilities. It is not possible, however, to test this implication directly due to the lack of measures of English abilities in Mexico. Instead, we evaluate whether exposure affected other cognitive abilities. Thus, we identify two potential effects: 1) a positive effect because of a potential complementarity between English and Spanish, and 2) a negative effect as a consequence of rivalry among subjects due to a change in teaching time allocation. My results suggest that there is a selection into which schools offer English instruction, confirming the selection problem we were concerned about. Furthermore, we find no effects on Language and Math test scores, which suggests that the estimated effect of exposure to the English language on wages is not reflecting changes to general cognitive skills. These findings are consistent with exposure to English instruction affecting labor market outcomes.

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Appendix

A.1 Solving two selection problems

In this research, we face two selection problems. The first selection problem is caused by schools that self-selected to participate in the NEPBE. This self-selection resulted in an unfair comparison between schools with more information, more resources, better teachers or located in better neighborhoods with schools that did not participate in the program because of the lack of information and resources. As explained in subsection 2.1, we solved this selection problem using a school FE approach (see Table A.1), which allowed me to compare students within the same school, but with different exposure to English instruction (different cohorts).

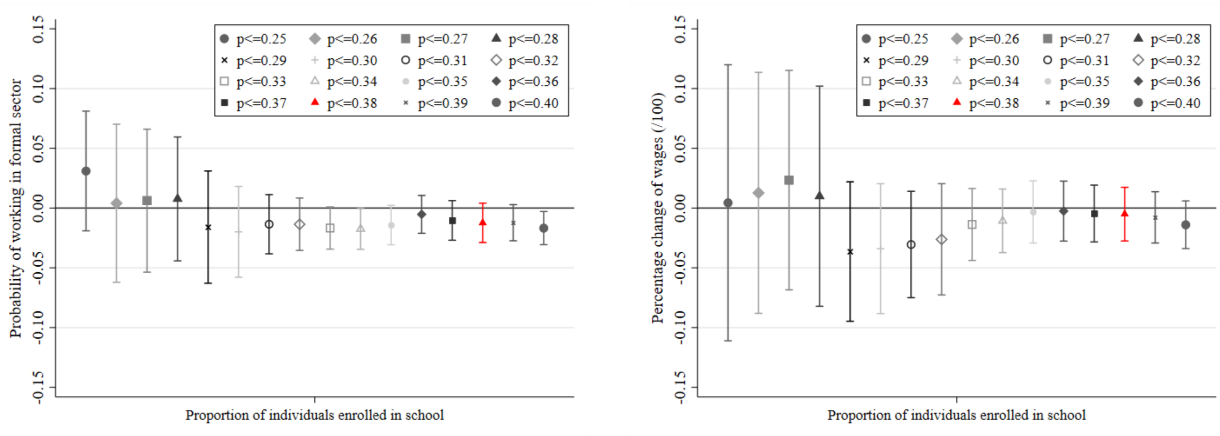
Table A.1: English instruction and the selection problem (Social Security data)

	(1) ln(wage)	(2) ln(wage)	(3) ln(wage)	(4) ln(wage)
Panel A: Hours of English instruction				
Hrs English	-0.005**	-0.009***	-0.008***	-0.015***
	(0.002)	(0.001)	(0.001)	(0.002)
Observations	4,055,434	4,055,434	4,055,434	4,055,434
Adjusted R^2	0.227	0.252	0.265	0.270
Panel B: Hours of English instruction (low enrollment)				
Hrs English	0.001	-0.006	-0.002	-0.005
	(0.007)	(0.007)	(0.008)	(0.011)
Observations	259,666	259,666	259,666	259,666
Adjusted R^2	0.234	0.268	0.308	0.312
State FE	YES	NO	NO	NO
County FE	NO	YES	NO	NO
Locality FE	NO	NO	YES	NO
School FE	NO	NO	NO	YES

Note: This table shows the effect of exposure to English instruction on wages under different fixed effects levels to see how the selection problem is solved using school FE. The sample contains all Mexican workers who belong to the cohorts 1997-2002, who are less than 25 and who are employed in the formal sector. All regressions include controls. Standard errors clustered at school level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The second selection problem is a result of individuals in my data sample self-selecting to participate in the formal sector: sample selection. Indeed, since we can only match kids to their labor registers if they work in the formal sector, we might consistently lose certain types of individuals who decide not to participate in the formal labor market. It is likely that the individuals we do not observe in the social security data are high-ability individuals or potentially high earners who decided to continue being enrolled in school. This latter conclusion derives from two facts. First, individuals in my sample are still young (16-24 years of age), which makes them more likely to be enrolled in school. Second, among the cohorts we study in this paper, most of the variation in economic statuses is due to education and formal work (see Figure 2).

Figure A.1: Effect of exposure to English instruction on wages (by the proportion of enrollment)



(a) Formal sector **(b)** Wages

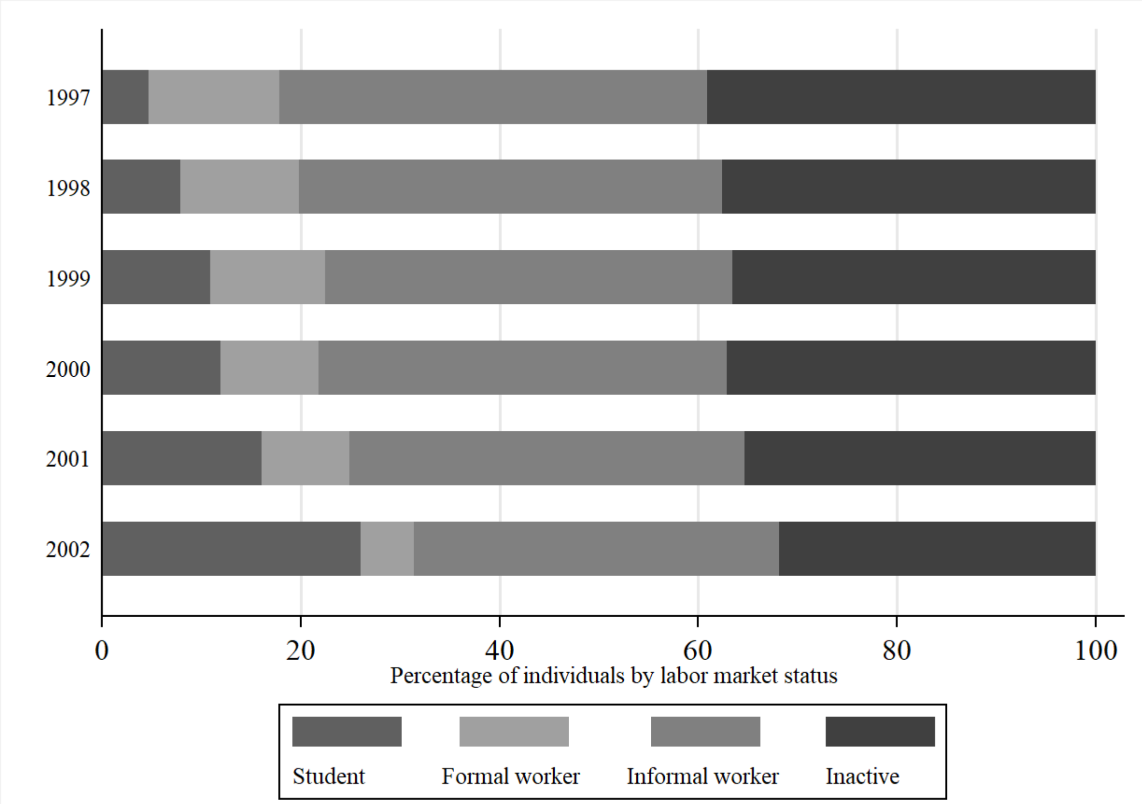
Source: Own elaboration with data from the Mexican Social Security data (IMSS, by its acronym in Spanish). *Note:* Estimates from different regressions are plotted, where the difference comes from the low enrollment samples used. Most of the estimates where the proportion of individuals enrolled in school is less than 0.39 are unbiased.

To solve the sample selection problem we propose to use a sub-sample of individuals living in counties with low-enrollment rates. The idea behind this potential solution is that if individuals have no chance to continue studying due to the lack of opportunities in their home counties, it will be more likely that exposure to English instruction does not affect their decision to participate in the labor market.

To determine the counties with low-enrollment rates we use a sensibility analysis. Indeed, for different enrollment rates, we estimate the effect of exposure on formal labor force participation. These different enrollment rates are inclusive. In other words, a 0.25 rate includes counties with a 0.25 enrollment rate or less, and so on. Figure A.1 shows a graphical representation of this exercise of sensibility. Each point estimate plotted represents a single regression with a particular enrollment rate.

Results from this exercise suggest using enrollment rates around 0.38 where the effect of exposure on labor force participation is not statistically different from zero (see panel (a) of Figure A.1). Panel (b) complements the analysis by showing the same sensibility analysis, but for wages as the dependent variable. Notice how the estimate is biased for samples in which the selection problem is a determinant. Using this low-enrollment sample, economic statuses look more homogeneous among the cohorts we study in this paper (see Figure A.2).

Figure A.2: Composition of the Mexican labor force by cohort (low-enrollment sample).



Note: Percentage of Mexicans in certain labor market statuses (by cohort) are plotted. Notice that, in this sub-sample, the proportion of individuals performing certain occupations is relatively homogeneous across cohorts, but there is still some variation in school enrollment.

A.2 Manufacturing and services industries with low and high English-intensive requirements (by abilities)

In subsection 4.1 we showed that exposure to English instruction increased the wages of high-ability individuals. This positive effect on wages can be explained because both, men and women in the high part of the abilities distribution, substitute jobs in agriculture for manufacturing industries. On the other hand, only men substituted construction for manufacturing industries, although this substitution is less strong for high-ability men. Likewise, we found that men who had exposure reduced their geographical mobility (the opposite is true for women). However, men found potentially better opportunities within the same industry, while women are facing more obstacles to do so (even though they increased their geographical mobility).

Furthermore, we documented that within manufacturing and services industries, there is a strong substitution between low and high-English-intensive jobs in favor of the latter. This

Table A.2: Exposure to English instruction and economic industries by abilities (Social Security data)

	(1)	(2)	(3)	(4)
	Manufacturing		Services	
	High English	Low English	High English	Low English
Panel A: Low enrollment sample				
Hrs English	0.065***	-0.020	0.040***	-0.037***
	(0.014)	(0.015)	(0.015)	(0.012)
Eng×Q2	0.001	-0.012	0.021*	-0.005
	(0.009)	(0.010)	(0.011)	(0.008)
Eng×Q3	-0.007	0.000	0.001	-0.003
	(0.011)	(0.011)	(0.010)	(0.007)
Eng×Q4	-0.012	-0.014	0.004	0.000
	(0.011)	(0.014)	(0.009)	(0.008)
Observations	259,666	259,666	259,666	259,666
Adjusted R^2	0.175	0.189	0.145	0.116
Panel B: Low enrollment sample (Men)				
Hrs English	0.083***	-0.041**	0.031**	-0.031**
	(0.018)	(0.018)	(0.015)	(0.013)
Eng×Q2	-0.002	0.005	0.015	-0.006
	(0.011)	(0.012)	(0.012)	(0.007)
Eng×Q3	-0.024**	0.019	0.009	-0.008
	(0.010)	(0.013)	(0.012)	(0.007)
Eng×Q4	-0.014	0.006	-0.011	-0.005
	(0.014)	(0.016)	(0.013)	(0.011)
Observations	166,165	166,165	166,165	166,165
Adjusted R^2	0.175	0.202	0.163	0.111
Panel C: Low enrollment sample (Women)				
Hrs English	0.025	0.022	0.036	-0.046*
	(0.024)	(0.026)	(0.029)	(0.026)
Eng×Q2	0.017	-0.042*	0.024	-0.001
	(0.019)	(0.022)	(0.020)	(0.015)
Eng×Q3	0.034	-0.032	-0.010	0.006
	(0.022)	(0.021)	(0.020)	(0.016)
Eng×Q4	-0.001	-0.055**	0.031	0.019
	(0.018)	(0.027)	(0.022)	(0.021)
Observations	93,501	93,501	93,501	93,501
Adjusted R^2	0.226	0.229	0.192	0.174
Shares	0.17	0.17	0.29	0.24

Note: This table shows the effect of exposure to English instruction on economic industries, by abilities. The sample contains all Mexican workers who belong to the cohorts 1997-2002, who are less than 25 and who are employed in some economic industry. All regressions include controls. Standard errors clustered at school level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

effect is stronger for men than for women. In this section, we provide evidence that high-ability individuals benefited more from exposure to English instruction than other individuals in the distribution for a different substitution than the aforementioned one.

High-ability men (in the third quartile of the abilities distribution) have a weaker substitution of low-English intensive manufacturing jobs than low-ability men. This may suggest that some of the occupations for English speakers in manufacturing industries are less intensive in communication skills. Thus it is likely that these occupations are more manual intensive such as machine operators. This story makes sense due to the substitution between construction and manufacturing industries that men do, since similar manual skills are required.

English abilities pay off more in the services industry. Indeed, men around the middle of the abilities distribution (including the third quartile) have a stronger substitution of low-English intensive services jobs for high-English intensive ones. The story behind this result is that English abilities pay off more in the services industry because occupations in this industry are more likely to require communication skills.

The weaker effect of exposure on women's wages could be explained by an opposite substitution behavior. In other words, high-ability women have a stronger substitution effect within manufacturing industries, in favor of high English-intensive jobs. On the other hand, these high-ability women have a weaker substitution in the services industries (relative to men), resulting in a worse allocation of their potentially acquired English abilities.

A.3 High-English intensive industries

In this section, we show a detailed description of the industries we classified as high-English intensive using the classification recently proposed by Gálvez-Soriano (2023). The data source to construct this classification is the Mexican subjective well-being survey (BIARE, for its acronym in Spanish). BIARE is a representative survey at the national and state level, it asks adults (18 and older) about their English skills. In particular, the survey asks: Do you speak English? BIARE database contains a comprehensive description of the economic industries for all Mexicans who reported actively participating in the labor force.

Economic industries are classified using the NIACS classification system at four-digit detail. On the other hand, social security data (from IMSS) reports its own economic industry classification. We paired the IMSS classification with the NAICS system using the match proposed by Banco de México (2021). However, this latter has a detail of a two-digit NAICS code. Hence, we expanded this original match using pairing the descriptions reported in both, IMSS and NAICS systems.

Table A.3: Economic Manufacturing Industries

4-digit code	Industry name	5-digit code	Industry name
3110	Animal food manufacturing	31131	Sugar and confectionery product manufacturing
		31141	Fruit and vegetable preserving manufacturing
		31151	Dairy product manufacturing
		31161	Animal slaughtering and processing
3120	Beverage and tobacco industries	31211	Beverage manufacturing
3150	Apparel manufacturing	31511	Apparel knitting mills
		31521	Cut and sew apparel manufacturing
3160	Leather and hide tanning and finishing	31611	Leather and hide tanning and finishing
		31621	Footwear manufacturing
3220	Paper industry	32211	Pulp, paper, and paperboard mills
3250	Chemical industry	32511	Basic chemical manufacturing
		32521	Resin, synthetic rubber, and artificial and synthetic fibers
		32541	Pharmaceutical and medicine manufacturing
		32551	Paint, coating, and adhesive manufacturing
		32591	Other chemical product and preparation manufacturing
3270	Nonmetallic mineral products	32711	Clay product and refractory manufacturing
		32731	Cement and concrete product manufacturing
3320	Metal products manufacturing	33241	Boiler, tank, and shipping container manufacturing
		33251	Hardware manufacturing
		33281	Coating, engraving, heat treating, and allied activities
3340	Manufacturing of computer	33461	Manufacturing and reproducing magnetic and optical media
3350	Electric appliances and electric power generation	33511	Electric lighting equipment manufacturing
		33521	Household appliance manufacturing
		33531	Electrical equipment manufacturing
3360	Transportation equipment	33611	Motor vehicle manufacturing
		33641	Aerospace product and parts manufacturing
		33651	Railroad rolling stock manufacturing
		33661	Ship and boat building
3370	Household furniture	33710	Nonupholstered wood household furniture manufacturing

Note: Manufacturing industries with high shares of workers with English abilities. This classification was obtained from Gálvez-Soriano (2023), using 2014 BIARE survey.

Table A.4: Economic Services Industries

4-digit code	Industry name	5-digit code	Industry name
4310	Wholesale trade of groceries, food, beverages and tobacco	43111	Grocery merchant wholesalers
		43112	Tobacco and alcoholic beverage merchant wholesalers
4350	Wholesale trade of industrial machinery and equipment	43522	Wholesale trade of manufacturing machinery and equipment
		43541	Computer and software merchant wholesalers
4620	Retail trade in self-service shops and department stores	46211	Retail trade in self-service shops
		46221	Retail trade in department stores
4641	Retail trade of health care items	46412	Optical goods and other health care stores
4651	Retail trade of perfumery and jewelry	46511	Cosmetics, beauty supplies, and perfume stores
4661	Retail trade of household furniture	46611	Furniture stores
4682	Automotive parts and accessories	46821	Automotive parts, accessories, and tire stores
4841	Freight truck transportation	48410	General freight trucking
4931	Warehousing services	49310	Warehousing and storage
5170	Telecommunications	51731	Wired and wireless telecommunications carriers
5324	Commercial and industrial machinery	53242	Office machinery and equipment rental and leasing
5610	Administrative and support services	56160	Investigation and security services
		56170	Services to buildings and dwellings
7100	Artistic, cultural and sporting services	71121	Spectator sports
		71311	Amusement parks and arcades
7211	Traveler accommodation	72111	Hotels and motels
7223	Special food services	72231	Food and beverage preparation services
7224	Drinking places (alcoholic beverages)	72241	Nightclubs, bars and similar drinking places
8114	Personal and household goods repair	81140	Personal and household goods repair and maintenance
8131	Religious organizations	81311	Religious organizations
9314	Justice, public order, and safety	93141	Justice, public order, and safety activities

Note: Services industries with high shares of workers with English abilities. This classification was obtained from Gálvez-Soriano (2023), using 2014 BIARE survey.

After matching both data sets, we classified an industry as high-English intensive if the proportion of workers in this industry is greater than 0.8 percent. Notice that most of the English speakers' distribution concentrates at zero. Table A.3 and Table A.4 report the resulting classification using industries at four-digit NAICS codes for manufacturing and services, respectively.