

English skills and labor market outcomes in Mexico*

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February 2023 (Latest version [here](#))

Abstract

In this paper, I study the prevalence of English skills and the labor market returns to English skills in Mexico. I use individual-level data from the 2014 Subjective Well-being Survey, which unlike other large nationally representative data sets includes a measure of English proficiency, to describe English-speaking ability in Mexico and to estimate the effect of English skills on labor market earnings. To address the concern that English skills may be endogenous in this relationship, I take advantage of policy changes in several Mexican states that introduced English instruction (as a subject) in public elementary schools in the 1990s. Using a difference-in-differences (DD) strategy, I estimate the effect of these state policies introducing English instruction; this study is the first to evaluate these policies. I then use the variation in English skills arising from these policy changes to estimate the causal effect of English skills on labor market outcomes. First, I find that English-speaking ability is not common among Mexicans in Mexico, with only 3% of adults aged 18–65 reporting they speak English. Second, the state policies introducing English instruction in elementary schools increased the likelihood of speaking English, did not affect wages, and shifted workers out of physically-demanding occupations. I do not find evidence of differential trends between the policy-adopting states and never-adopting comparison states, which provides support for the parallel trend assumption needed to interpret these DD estimates as the causal effects of the state policies. Instrumental variables estimates of the effect of speaking English on earnings using the state policies introducing English instruction as instruments for the English skills variable suggest positive effects of speaking English on earnings; however, these estimates are imprecise and not significant at conventional levels of significance.

JEL Classification: I21, I28, J24, J31.

Keywords: Primary School, Education Reform, Occupational Choice, Labor Income.

*I am deeply thankful to Aimee Chin for her guidance and comments along this research. I am also grateful for the valuable comments of Chinhui Juhn, Jorge Pérez Pérez, Jessica Serrano, and Julieta Osornio. This work was supported by Banco de México, the Mexican National Council for Science and Technology (CONACYT), and the University of Houston. I benefited from presenting at Banco de México research seminar and the Southern Economic Association annual conference. This research also benefited from my Summer Fellowship at Banco de México in 2022.

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Introduction

Given the use of English as a lingua franca in the global economy and Mexico's proximity to the United States (US), there could be an economic value to English skills in Mexico. In this context, there has been an unprecedented expansion of English programs to offer English instruction in the public education system of non-English speaking countries. This expansion has been usually motivated by the assumption that English instruction will increase English-language skills and, ultimately, improve labor market outcomes. Surprisingly, there is very little research on the prevalence of English skills and the labor market returns to English skills in Mexico. Indeed, The existing literature has traditionally studied the effect of English abilities on earnings in English-speaking countries, while there is very little research in contexts where English is not the main language of communication. The first evidence was provided by [Azam, Chin and Prakash \(2013\)](#) in the context of India and [Eriksson \(2014\)](#) in South Africa.

Nevertheless, we do not know much about the effectiveness of exposure to English instruction on the acquisition of English abilities. [Angrist, Chin and Godoy \(2008\)](#) offered the first evidence that changing the medium of instruction (from English to Spanish) would not affect the English proficiency of Puerto Ricans. On the other hand, [Eriksson \(2014\)](#) finds a positive effect on English proficiency when the medium of instruction changed from Afrikaans to English. However, most of the non-English speaking countries in the world have introduced the English language as a subject and not as the medium of instruction. For example, in the first research that exploits a policy change in the exposure to English instruction (as a subject), [Chakraborty and Bakshi \(2016\)](#) work with the probability of exposure to evaluate its effect on wages. However, they do not actually measure if this exposure leads to the acquisition of English skills. Likewise, although [Gálvez-Soriano, Salcedo and Cabrera-Hernández \(2023\)](#) has a better measure of exposure (as weekly hours of English instruction), he cannot provide causal evidence that exposure leads to the formation of English abilities because of the lack of data needed for this purpose. Instead, he offers evidence that exposure does not affect other cognitive skills, suggesting that his findings are consistent with the acquisition of English abilities.

On the other hand, the evidence on the returns to English skills is still scarce in the context of non-English speaking countries and very rare in Latin American economies. In particular, most of the existing research points out that English language skills are positively associated with immigrants' earnings in English-speaking countries (see [Ispphording \(2014\)](#) and [Chiswick and Miller \(2015\)](#) for a review). This literature provides evidence that immigrants with English skills in the US have higher wages and better educational attainment. Similar results on wages have been found in the context of immigrants to Australia and European countries ([Dustmann, 1994](#); [Chiswick and Miller, 1995](#); [Dustmann and Soest, 2001](#); [Hayfron, 2001](#); [Shields and Price, 2002](#); [Williams, 2011](#)). Likewise, it has been found that the English premium among immigrants could vary depending on the workers' age and education, i.e. younger and more educated immigrants have greater returns to English at work ([Lang and Siniver, 2009](#); [Azam, Chin and Prakash, 2013](#)). Nevertheless, our understanding of the effects of English instruction on the acquisition of English abilities and the returns of these skills in non-English speaking countries is still limited.

To fill these gaps in the literature, I propose to answer the following research questions: Does exposure to English instruction increase the acquisition of English abilities in a non-English speaking country? What are the returns to English skills in a non-English speaking country? The context I study is Mexico, an economy whose main language of communication is Spanish, but also one for which the value of English could be even more pronounced than in other developing countries because of its close relationship to the US in terms of trade and migration. Besides, the external validity of my context is potentially wider than in the case of India or South Africa, which are particularly different from most of the developing countries because of their status as former British colonies, where the upper-social class and government workers usually have English abilities.

To answer my research questions, I exploit the implementation/expansion of seven state English programs in Mexico. In my identification strategy, I use state-by-cohort variation in exposure to English instruction (which was driven by the implementation of the different state policies since the early 90s) in a Difference in Differences (DD) strategy. Then, I work with a more representative sample to perform an analysis of heterogeneous effects and to study occupational choices. To accomplish this latter, I offer estimates from a Staggered Difference in Differences (SDD) model, which pools the aforementioned Mexican states. Finally, I offer a causal estimate of the returns to English skills using an instrumental variables approach using the exogenous variation caused by the interventions as an instrument for the English skills endogenous variable.

Hence, in this paper, I provide the first empirical evidence that exposure to English instruction leads to the formation of English skills in Mexico. In fact, I exploit the implementation of seven state English programs as an exogenous variation to the exposure to English instruction, which may affect the acquisition of English skills. The SDD estimate from the first stage equation suggests that this type of intervention increases the acquisition of English abilities by almost 2 percent. The average size of the intervention I study is relatively small. It increased exposure to English instruction, on average, by 18 minutes per week. Hence, my results suggest that exposure to English instruction does increase the number of English speakers in Mexico by 6 percent. I also find that after the intervention, women acquired more English skills than men. This same finding is also true for non-indigenous people with respect to the indigenous ones, and for individuals living in urban areas with respect to those living in a rural context.

Furthermore, I offer some of the first causal estimates on the returns to English skills and the effect of English programs on labor market outcomes in the context of a non-English-speaking country. The set of labor market outcomes I examine includes the likelihood of working for pay, wages and occupational choices. I find that the average intervention does not affect neither the likelihood of working for pay nor wages, although the point estimate for this latter is positive. Interestingly, the potential positive effect on wages is driven by women.

There are three related papers in the context of Mexico. [Delgado Helleseter \(2020\)](#) offered the first estimate on the returns to English abilities in the market of jobs posted online. He finds that the wages of Mexican English speakers are approximately 28 percent higher than those of non-English speakers. However, his sample is composed only by online advertisements from a single online job board, which implies concerns about the sample selection and

the external validity of his results. Second, [Charles-Leija and Torres \(2022\)](#) claimed to offer an estimate of the returns to English skills in Mexico. However, their model has serious problems of identification because they do not solve the selection problem associated with differences between individuals who report being English speakers and those who do not. Instead, they propose to solve the selection into the sample due to more men participating in the labor market than women, which produced a remaining bias in their estimates. Finally, [Gálvez-Soriano, Salcedo and Cabrera-Hernández \(2023\)](#) provide the first empirical evidence that exposure to English instruction does not affect the wages of the mean worker in the Mexican formal sector. However, they do find a positive and significant effect on wages, only among high-achieving individuals.

Although the existing literature in Mexico has offered a reliable estimate of the exposure to English instruction on labor market outcomes ([Gálvez-Soriano, Salcedo and Cabrera-Hernández, 2023](#)), no one has yet provided causal evidence on the returns to English skills ([Delgado Helleseter, 2020; Charles-Leija and Torres, 2022](#)). Furthermore, this paper offers the first empirical evidence that exposure to English instruction improves the acquisition of English abilities, in the context of a Latin American economy. Similarly, I am the first to estimate the causal effect of English skills in a Latin American economy and among the first in the context of a non-English-speaking country.

The remaining of this paper proceeds as follows. In the first section, I offer a comprehensive description of the English abilities in Mexico. Then, in [section 2](#) I explain the background of the policy changes I exploit in this paper. In [section 3](#), I describe the database I use. In [section 4](#), I explain the empirical strategy I propose. In [section 5](#), I first show the results of a naive estimate on the returns to English skills, then I offer an estimate of the effect of English programs on the acquisition of English abilities and causal evidence on the returns to these abilities. [Section 6](#) provides robustness checks of my DD models. Finally, [section 7](#) summarizes with a discussion of my findings and a brief conclusion.

1 English language skills in Mexico

1.1 Five facts about the English skills of the Mexican population

In this subsection, I provide an overview of the Mexican population in relation to their ability to speak English. I document five important facts. First, there is a gender-English gap in Mexico, with more men declaring to speak English. Second, more exposure to English instruction is associated with more English skills. Third, the ability to speak English increases with educational attainment. Fourth, indigenous people are way behind in the process of learning English. Similarly, rural areas are falling behind in this learning process.

The data I use in this descriptive analysis come from the 2014 Mexican Subjective Well-being Survey (BIARE, for its acronym in Spanish). BIARE surveys only adults ages 18 and older, but I concentrate on the range 18-65 to capture mostly individuals that participate in the labor market (excluding retired workers). Additionally, I offer a comparison between Mexican states that implemented English programs in public primary schools and states that did not. Hence, this overview shows two important dimensions of heterogeneity in English

abilities: among types of individuals and among regions.

The most spoken language in Mexico is Spanish and all official documents are written in this language. By the year 2014, only 2.8% of the Mexican adult population knew how to speak English (see [Table 1](#)), while about 7% declared knowing how to speak an indigenous language. On the other hand, in this first descriptive analysis, I document a positive correlation between English instruction and English abilities in Mexico. Indeed, using a comparison of regions I find that states which have implemented English programs (to offer English instruction in primary schools) have more proportion of individuals with English skills than the other states.¹ Hence, it could be inferred that exposure to English instruction may improve the acquisition of English skills.²

I document the existence of a gender-English gap in Mexico, with more men declaring to speak English than women. About 3.4% of the male population in Mexico speaks English, while 2.1% of the female population does. Notice, however, that since the English skills variable is self-reported, it could potentially overstate the gender-English gap, with fewer women reporting having English abilities. On the other hand, as in the former finding (for the overall Mexican population), there are more men (and women) with English abilities in states that have offered English instruction in primary schools than in those that did not. This latter result motivates the research question of whether exposure to English instruction improves the acquisition of English abilities in the context of a non-English speaking country.

Young adults who have had more exposure to English instruction report more English skills than older ones. This fact is supported in two dimensions. First, young individuals (18-35 years old) are more likely to speak English than the elderly (51-65 years old). This could be a result of the recent English programs implemented in the early 1990s and in the early 2000s in Mexico that affected only the young birth cohorts (as I explain in [section 2](#)). Second, particularly among the young birth cohorts, there are significant differences in English abilities when we compare individuals living in states with English programs and individuals living in other states (without English programs). Once again, this finding supports the idea of English programs increasing the acquisition of English abilities.

The ability to speak English increases with educational attainment. The proportion of college graduates (and higher) with English-speaking skills in Mexico is more than three times the corresponding proportion for all Mexican adults. While this proportion is about the same between individuals with upper secondary and the national figure. On the other hand, less than one percent of the individuals with lower secondary or less speak English. These results suggest that either most of the acquisition of English skills is held during higher education or that most individuals who can afford higher education are likely to learn English. Furthermore, this also points out the importance to include education in my empirical analysis as a non-linear variable. Indeed, in all models shown in [section 4](#), I control for education fixed effects in order to capture the non-linear relationship between education and English skills.

¹States with English programs are: Aguascalientes, Coahuila, Durango, Nuevo Leon, Sinaloa, Sonora and Tamaulipas. There are other Mexican states that have offered English instruction in public primary schools, but on an irregular basis, with a few beneficiary schools and/or with no expansion over time.

²Later in this paper, I formally prove this claim.

Table 1: Adult English speaking ability in Mexico

Variable	Full Sample	States w/ English (a)	States wo/ English (b)	Diff. (a-b)
All individuals ages 18-65	2.75 (16.36)	3.38 (18.08)	2.63 (15.99)	0.75*** (0.17)
<i>By gender</i>				
Male	3.44 (18.22)	4.03 (19.66)	3.32 (17.91)	0.71** (0.26)
Female	2.13 (14.45)	2.77 (16.42)	2.01 (14.03)	0.76*** (0.21)
<i>By age</i>				
18-35	3.07 (17.25)	4.09 (19.81)	2.87 (16.71)	1.22*** (0.27)
36-50	2.94 (16.88)	3.51 (18.39)	2.82 (16.55)	0.69** (0.30)
51-65	1.80 (13.31)	1.79 (13.26)	1.81 (13.32)	-0.02 (0.27)
<i>By educational attainment</i>				
Incomplete primary (0-5 years)	0.33 (5.73)	0.20 (4.46)	0.35 (5.87)	-0.15 (0.10)
Primary school (6 years)	0.59 (7.68)	0.72 (8.44)	0.57 (7.55)	0.14 (0.18)
Lower secondary (7-9 years)	0.96 (9.74)	1.10 (10.43)	0.92 (9.57)	0.18 (0.17)
Upper secondary (10-12 years)	2.79 (16.46)	2.65 (16.05)	2.82 (16.54)	-0.17 (0.31)
College or higher (13-24 years)	8.67 (28.14)	10.10 (30.13)	8.33 (27.63)	1.77*** (0.64)
<i>By ethnicity</i>				
Indigenous	0.99 (9.88)	2.23 (14.76)	0.94 (9.67)	1.29 (0.91)
Non-indigenous	2.87 (16.69)	3.40 (18.11)	2.75 (16.37)	0.64*** (0.18)
<i>By geography</i>				
Urban	3.24 (17.71)	3.74 (18.96)	3.13 (17.42)	0.60*** (0.20)
Rural	0.92 (9.53)	1.20 (10.87)	0.88 (9.35)	0.31 (0.19)

Note: The sample consist of Mexicans ages 18–65 who self-reported their ability to speak English, using the 2014 Mexican Subjective Well-being Survey (BIARE). The full sample is composed by 83,630 observations. Standard deviations are shown in parentheses below the mean percent of a given subpopulation with English ability, while standard errors are shown in parentheses below the difference in means. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

I also document the existence of an ethnicity-English gap in Mexico, with nearly 2.9% percent of non-indigenous people with English skills and almost 1% of indigenous people who speak English. I claim that this is a result of the lack of English education in marginalized areas where indigenous people have settled. In fact, most of the efforts to teach indigenous people a second language are for them to learn Spanish than to learn a foreign language. Another piece of evidence is the fact that I do not find differences in English abilities between indigenous people living in states that have implemented English programs and indigenous people living in states without these programs, which may suggest that the state English programs have not reached the marginalized villages where indigenous people live.

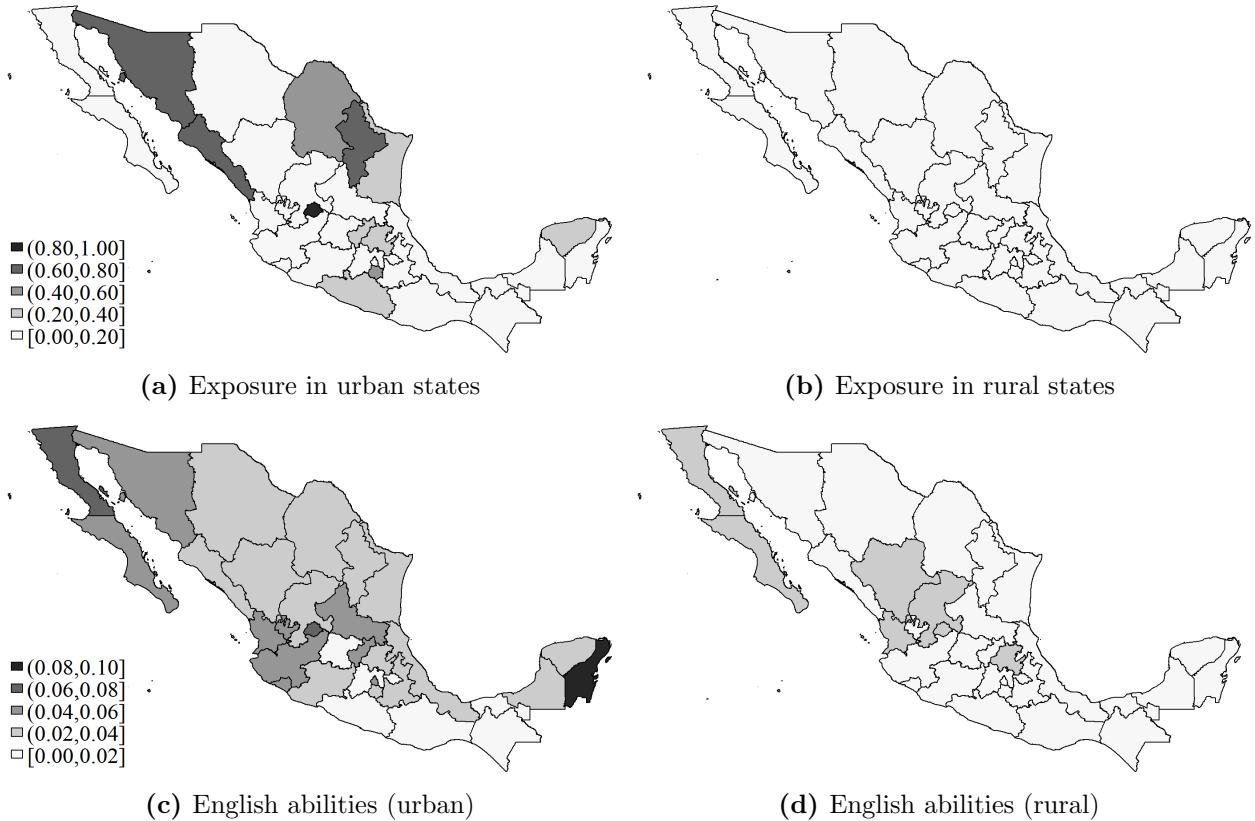
Similarly, there is a considerable geographical variation in the prevalence of English skills in Mexico. In particular, I find an important difference in English ability between urban and rural areas, with the former showing a 3.2% of English speakers, while the latter has only 0.9%. Previous estimates by [CIDAC \(2008\)](#) suggested that this number was 6% for the urban adult population in Mexico. The story behind this difference between the urban and rural contexts is similar to the indigenous people case. Indeed, the state governments that have implemented English programs relegated rural and marginalized areas preventing their schools from implementing English programs. The reason is that most of the first English programs consisted of virtual courses because of the lack of English teachers, which means that schools without the equipment needed (computers, access to the Internet, projector, etc.) could not benefit from these programs. This has been the case for rural schools in Mexico.

1.2 Geographical heterogeneity of English skills

This subsection contributes by offering five facts regarding the English geographical heterogeneity in Mexico. First, the population with English abilities is concentrated in four Mexican states: Quintana Roo, Baja California, Aguascalientes, and Baja California Sur. Second, the less industrialized Mexican states (South and Southeast region) have a low proportion of English speakers. Third, English-speaking individuals are mainly present in urban areas with a few exceptions in rural contexts. Four, the states with more exposure to English instruction are not necessarily those with more English speakers. Finally, administrative records suggest that seven out of 32 Mexican states have consistently offered English instruction in public primary schools during the early 1990s.

Half of the Mexican states have proportions of English-speaking individuals higher than the figure for the national level (2.75%), from which four states more than double this proportion (Quintana Roo, Baja California, Aguascalientes, and Baja California Sur). It is likely that Quintana Roo and Baja California Sur have more English speakers than the mean Mexican state because they are located in tourist regions with the two most famous destinations: Cancún and Los Cabos, respectively. On the other hand, Aguascalientes hosts international companies from the automotive assembly, auto parts manufacturing, industrial robotics, and electronics manufacturing, which are suppliers of Nissan, Mazda, Mercedes-Benz, GM, Honda, Infiniti, and Volkswagen, among others. Likewise, Baja California manufactures for international companies in the industries of aerospace, automotive, electronics, and medical device manufacturing. But also, because of its closeness with the US, some Mexican workers

Figure 1: Exposure to English instruction and English abilities in Mexican states



Note: Maps in upper panels (a) and (b) represent the proportion of Mexican public elementary schools that offered English instruction in a given state, in 2008. Maps in lower panels (c) and (d) represent the proportion of individuals aged 16–65 who self-reported their ability to speak English, using data from the 2014 Mexican Subjective Well-being Survey (BIARE).

living in Tijuana commute every day to work in American companies located in Chula Vista and San Diego.

The South and Southeast region of Mexico has a low proportion of English speakers except for the states of Quintana Roo and Yucatan. This region is well known for its high proportion of people in poverty condition. In fact, the seven states colored in light gray from panel (c) of Figure 1 are classified among the poorest Mexican states according to the biannual report of CONEVAL (2020), except for the State of Mexico and Guanajuato, which are around the middle of the distribution (ranked in the place 12 and 17, respectively). Two potential reasons could explain the lack of English speakers in this region. First, the poor quality of education, with these states having the lowest average grades in the ENLACE test. And, second, the lack of incentives due to the poor economic activity, with this region contributing the least to the national economic activity (except for Tabasco, which contributes substantially to the sector with oil extraction).

Mexican rural areas have the lowest proportions of English speakers. There are seven exceptions: Aguascalientes, Baja California, Baja California Sur, Durango, Hidalgo, Nayarit

and Zacatecas (see panel (d) of [Figure 1](#)). The first three states have a relatively higher proportion of English speakers for the aforementioned reasons (fact number one). Durango and Zacatecas are international migrant sender states (especially to the US) and the proportion of migrants is relatively greater in rural areas compared to the urban ones. Finally, the state of Hidalgo is a host of international companies in automotive, auto parts, technology, telecommunications, and transportation such as: GEMI International, Motorola Solutions, Grupo Marpa, Transtell, among others, which are located in rural regions of the state; while Nayarit is known for exporting agricultural products (figs, pineapples, avocados, guava, mangoes, cantaloupes, watermelons and papayas) to the US, which may explain why there is a relatively high proportion of English speakers in the rural context.

The Mexican states with more exposure to English instruction are not necessarily those with more English speakers. Indeed, most of the states colored in darker gray in panel (c) of [Figure 1](#), which have a high proportion of English speakers, are touristic, migrant senders, and/or hosts of international companies, as explained before. However, other states with lower proportion, such as: Coahuila, Nuevo Leon, Sinaloa and Tamaulipas will probably catch up because of their efforts to offer English instruction at school. For the states that coincide in terms of exposure and proportion of English speakers (Aguascalientes, Sonora, Queretaro, Mexico City and Morelos) it is difficult to disentangle the reason for this high proportion, but I will explore some of them later in this paper. For the remaining states (Quintana Roo, Baja California, Baja California Sur, Nayarit, Chihuahua and San Luis Potosi) we could infer that the formation of English abilities is not directly explained by the exposure to English instruction.

Finally, administrative records suggest that seven out of 32 Mexican states have consistently offered English instruction in public primary schools during the early 90s. Indeed, using historical administrative data from the Mexican school census, I find that the states of Aguascalientes, Coahuila, Durango, Nuevo Leon, Sinaloa, Sonora, and Tamaulipas have implemented a state English program, which increased substantially and/or consistently the weekly hours of English instruction in Mexican public schools (see ??). However, later in this paper, I document that only Aguascalientes, Coahuila, Nuevo Leon, and Sinaloa had a significant increase in terms of hours of English instruction as a result of their respective English program implementation.

1.3 Occupations and English skills in Mexico

In this subsection, I provide a description of the economic occupations in which Mexican English speakers concentrate. I document four main facts. First, elementary and manual unskilled occupations do not “require” English abilities in Mexico. Second, clerical support, professionals, and managerial occupations concentrate most English speakers in Mexico. Third, occupations with more English speakers pay more to their workers, have a more even proportion of females, and these workers are more educated. And, fourth, the rank of occupations by English abilities does not change significantly when considering only low-educated workers.

The occupations that do not require English abilities in Mexico are: farming, elementary occupations, machine operators and crafts (see the first four categories in panel (a) of [Fig-](#)

ure 2).³ This finding is not surprising because these types of occupations do not require a high level of communication skills and, instead, they are more manual-intensive. Thus, on average, in these occupations, only one out of a hundred workers speaks English. It is likely that the English speakers in these occupations acquired their English abilities either in school or through a migration network. On the other hand, these occupations are the worst paid and with the lowest proportion of women. This may suggest a negative correlation between English skills and earnings.

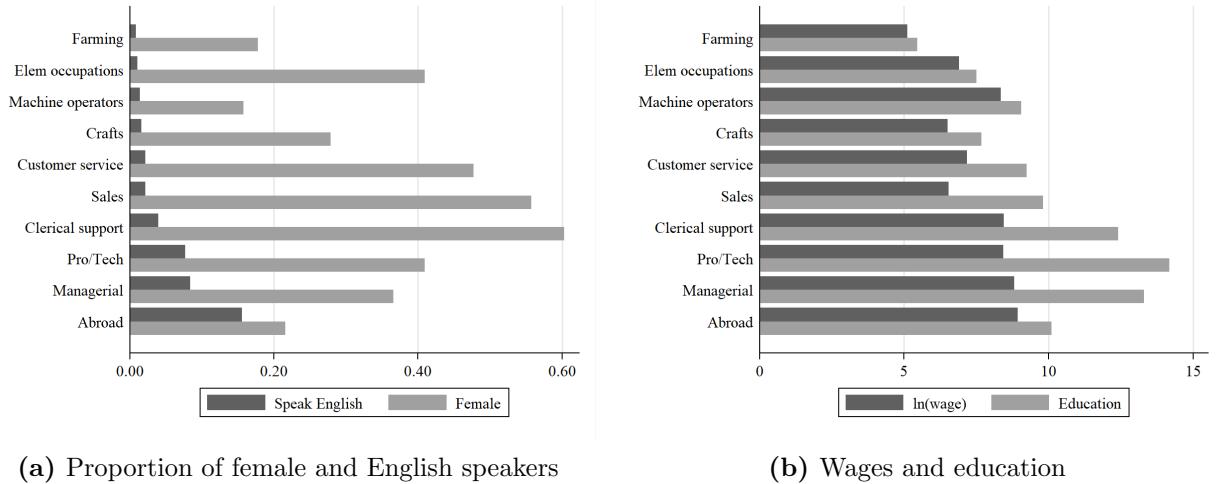
On the other hand, clerical support, professionals and managerial occupations concentrate most of the English speakers in Mexico (see the last four categories in panel (a) of [Figure 2](#)). In the case of clerical support, it is natural to have more English speakers since these types of occupations are intensive in communication skills. However, it is also true that most of these occupations, for example, assistants and secretariats are not required to have English abilities, except for executive positions or in tourist and international companies. On the other hand, professionals and technicians are more likely to have English abilities because, in upper secondary and professional education, the English language subject is compulsory and required to graduate with some degrees. Managerial occupations are the most likely to require English skills, with three times the national average proportion of English speakers. This is consistent with tasks requiring communication skills and public relations, especially in medium and large companies. Finally, 16% of the individuals working abroad have English skills, being consistent with Mexicans who cross the US border every day to work in American companies. The remaining occupations (customer service and sales occupations) are close to the average English skills in Mexico.

Among the occupations with more English speakers, workers are better paid, they are more educated, and there is a more even proportion of females. Indeed, the average monthly wage in Mexico is 4,211 pesos (for adults 18–65 years old), while the monthly wage in clerical support occupations is about 1.5 times higher, professionals and technicians earn more than double the mean wage in Mexico, while the wage of managerial occupations is 3.3 times higher. This result suggests a positive correlation between English abilities and wages. Likewise, as previously noted in [subsection 1.1](#), I find a positive correlation between English abilities and education. Finally, Customer service, sales, and clerical support occupations have the evenest proportion of female workers.

Using a sub-sample of low-educated individuals I find that there are no significant changes in the rank of occupations according to their proportion of English speakers (see [Figure A.1](#)). However, occupations in sales (which previously were close to the national mean proportion of English speakers) are now below the average. I defined the sub-sample of low-educated individuals considering nine or fewer years of education. This sub-sample intends to capture Mexicans who, if exposed to English instruction in primary school will see an effect, which could potentially be directly attributable to exposure and not to spillover effects of the program. Hence, it is likely that the rank shown in [Figure A.1](#) was directly affected by exposure to English instruction in primary school. In that sense, we could say that occupations requiring English skills among low-educated individuals are: customer service, clerical

³Notice, however, that it is strong to claim anything about English requirements because the proportion of English speakers is not the same as the demand for workers with English abilities, but there should be a high correspondence.

Figure 2: English abilities, wages and education by occupations



(a) Proportion of female and English speakers

(b) Wages and education

Note: This figure shows the percentage of individuals who have English speaking abilities, mean wages, percentage of women and their mean schooling, by aggregates of occupations in Mexico. The occupations were determined using the International Standard Classification of Occupations (ISCO-08) from the International Labor Organization (ILO) at one digit code level. The managerial category includes supervisors from other occupations. The abroad category contains individuals who reported working abroad, but it is a mix of all other categories. The sample contains Mexicans ages 18–65 who self-reported their ability to speak in English. Mean English ability for this sample is 0.028 (2.8%).

support, professionals and managerial. Finally, for this sub-sample of low-educated individuals, schooling is more homogeneous among occupations, which suggests that differences in wages are more likely to be attributed to English abilities.

1.4 Economic industries and English skills in Mexico

This subsection offers a characterization of the economic industries and their composition of English speakers. There are three aspects to highlight in this part of the analysis (which derived from findings of Figure 3). First, the economic industry with fewer English speakers is agriculture, with the worst-paid jobs and less educated workers. Second, the economic industries with more English speakers are professionals and telecommunications, with better-paid jobs, more educated workers, and a more even proportion of women. Third, if we only consider low-educated workers, the rank of economic industries by the proportion of English speakers changes substantially, with transportation, hospitality, and entertainment having more English speakers.

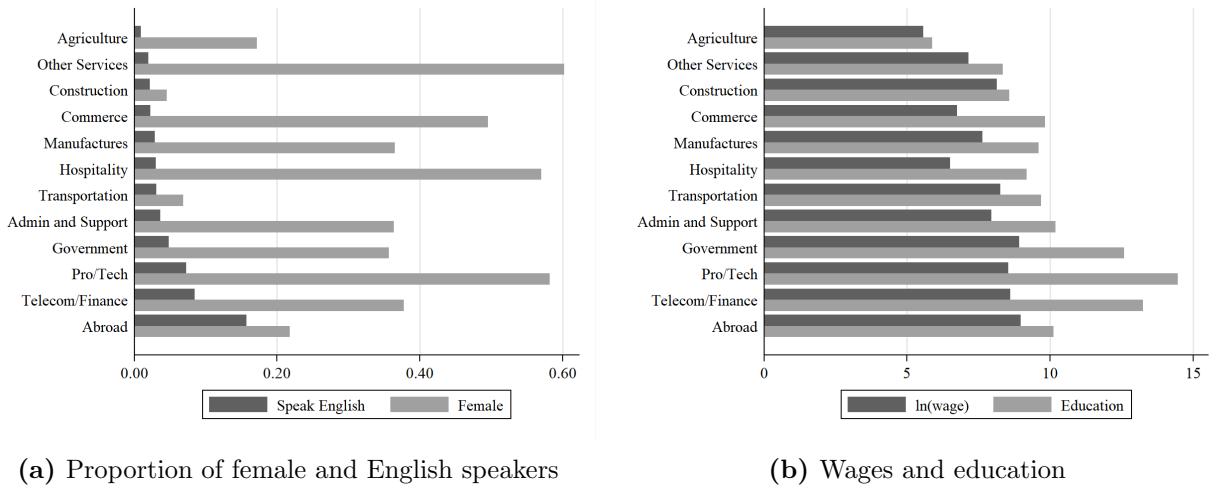
The economic industry with fewer English speakers is agriculture, with the worst-paid jobs and less educated workers. Then, with more than double the proportion of English speakers than in agriculture, the following industries are below the national average proportion: other services, construction, and commerce. This may be explained by the low proportion of international companies operating in those industries in Mexico. All these first four economic industries contain about half of the Mexican workers, with about one-half earning less than

the national average and the other half earning more. Again, low earners are also those with less English skills (consistent with the aforementioned positive correlation).

On the other hand, the economic industries with more English speakers are: professionals, technical, management, telecommunications, finance and real estate. These industries are among the better paid and with workers having the highest educational levels (showing again the positive correlation among English, wages and education). The remaining industries (manufacturing, hospitality, entertainment, transportation, administrative and government) have also more English speakers than the national average, but significantly fewer than the aforementioned industries.

Using a sub-sample of low-educated individuals I find the rank of economic industries (by the proportion of English speakers) changes substantially, with transportation, hospitality, and entertainment having now more English speakers. Hence, as explained in subsection 1.3, this may suggest that low-educated individuals are changing their decisions on the economic industry they will work in, after the exposure to English instruction in primary school. Nevertheless, this is something I will formally show in section 5. Even in this sub-sample, individuals working in economic industries with more English speakers are better paid and more educated than those working in low-English-intensive industries.

Figure 3: English abilities, wages and education by industries



(a) Proportion of female and English speakers

(b) Wages and education

Note: This figure shows the percentage of individuals who have English speaking abilities, mean wages, percentage of women and their mean schooling, by aggregates of economic industries in Mexico. The economic industries were determined using the North American Industry Classification System (NAICS) at two digit code level. The 'Construction' category includes mining, utilities and construction industries. The 'Telecom/Finance' category includes telecommunications, finance and real state industries. Finally, the 'Professional/Technical' category includes professional, technical and management industries. The abroad category contains individuals who reported working abroad, but it is a mix of all economic industries. The sample contains Mexicans ages 18–65 who self-reported their ability to speak in English. Mean English ability for this sample is 0.028 (2.8%).

2 Policy background in Mexican states

Starting in the early 1990s, a few Mexican states implemented English programs to offer English instruction in public primary schools. An important motivation for these states was the recently signed North American Free Trade Agreement (NAFTA) in December 1992, which came into force on January 1, 1994. English instruction in public primary schools is meant to improve the acquisition of English skills to facilitate labor mobility from Mexico to the United States and Canada. Indeed, before launching these state English programs, only private schools offered English instruction, but not the public ones, which used to leave unattended an important fraction of the population that potentially may benefit from NAFTA. As previously noted in subsection 1.3, using administrative data from the Mexican school census, I identified seven states that have implemented/expanded English programs in public primary schools, which have consistently increased the hours of English instruction offered in these schools. Hence, in this section, I will describe how was the implementation in most of these states. However, some of these do not have a publicly available registry of their English programs, which implies that my identification strategy would be mainly data-driven for those particular cases.

The first two Mexican states that offered English instruction in primary schools were Nuevo Leon and Sonora. The English program in the Mexican state of Nuevo Leon was launched in 1993 with one hundred randomly chosen elementary schools benefiting only high-achievement students of fourth, fifth and sixth grades.⁴ However, it was until the year 1998 that the state English program implemented English instruction as part of the regular curricula in the participant schools and only in sixth grade. This is the expansion that I exploit in this paper. Progressively, the state government increased the English program's coverage among elementary schools including some preschools. For example, in 2008 the program expanded the coverage to students in fifth grade in those schools that were already beneficiaries of the program in sixth grade. So that by 2008 the state English program covered around 60% of all elementary schools of Nuevo Leon.⁵

The state of Sonora also launched its English program in the year 1993 as a trial stage, but it was until the year 2004 that the English language was incorporated as a subject in the regular curricula of public primary schools. In the expansion of 2004, the program aimed to offer English instruction to only the first and second grades of 10 out of 72 counties. After that, the program gradually expanded the coverage to all grades that comprise primary school in Mexico (from first to sixth) and to more beneficiary counties, reaching a state coverage of almost 50% by the year 2010 (Reyes Cruz, Murrieta Loyo and Hernández Méndez, 2011).

The Mexican state of Coahuila followed the former two states with the implementation of an English program in 1995, benefiting 100 primary schools located in 11 (out of 38) counties. This program started offering English instruction only to the first grade of beneficiary schools. In the year 1998, this English program started a trial stage to offer English instruction in preschool and in 1999 the state government increased the coverage among preschools and

⁴Elementary schools in Mexico comprise from first to sixth grade, middle schools comprise from seventh to ninth grade, while high school comprises from tenth to twelfth grade. All three educational levels are part of the basic compulsory education system.

⁵All this information is publicly available in the Nuevo Leon English program's [website](#).

primary schools. Since then, the program has progressively expanded among schools and grades. And, nowadays, the program benefits the three grades that comprise preschool in Mexico, the six grades that comprise primary school (from first to sixth), and the three grades that comprise middle school (from seventh to ninth). The current coverage includes 26 (out of 38) counties from the state.⁶ The expansion that I exploit in this paper is the one of 1999.

Then, the state of Tamaulipas launched an English program in the 2001-2002 school year, offering English instruction to the fourth grade of urban primary schools in the state. This initial program benefited 44,777 students. For the 2003-2004 school year, the state English program expanded its coverage from fourth to sixth grade. Subsequently, in 2005, the program expanded to cover all six grades that comprise primary school in Mexico. In the 2011-2012 school year, all the preschools in the state were incorporated into the English program.⁷ The Tamaulipas policy change that I exploit in this paper is the first time implementation of 2001.

For the remaining states; Aguascalientes, Durango and Sinaloa, there are no official sources indicating the process of implementation of their English programs. However, administrative data from the Mexican school census suggests that these states launched their English programs in 2001, 2002 and 2004, respectively (see [Figure A.4](#) and [Figure A.5](#)). Unofficial [sources](#) point out that the implementation of the English program in Durango was in 2002 as a pilot program, which operated for six years in a few public primary schools. In 2008, English instruction was incorporated into the regular curricula of the beneficiary primary schools. In 2009 the program reached a 20% coverage of the kids enrolled in Durango primary schools.

3 Data

My main source of information is the 2014 Mexican Subjective Well-being Survey (BIARE, for its acronym in Spanish). BIARE is a representative survey of the Mexican population at the national and state level. In 2014, this survey was conducted as part of the Mexican Household Income and Expenditure Survey (ENIGH), and as an annex of the Socioeconomic Conditions Module. This latter implies that it is possible to have variables that characterize the socioeconomic condition of individuals and households in the same database, which is currently used to measure poverty in Mexico.

The 2014 BIARE round is special because it asked for a unique occasion about individuals' English speaking abilities. Respondents of this survey are adults 18 years of age and older. And the survey concentrates only on one adult per household. Interviews in this survey are all face-to-face and it requires that the information be provided by the respondent only; not through a third party. Respondents are asked a set of questions regarding their demographic and economic characteristics, their perceived well-being, as well as their ability to speak English.

The second source of information I use is the Mexican school census (also known as

⁶All this information is publicly available in the Coahuila English program's [website](#).

⁷All this information is publicly available in the Tamaulipas English program's [website](#).

Statistics 911). The school census allows for identifying the public schools that have offered English instruction in Mexico. This source of information is also key to construct a variable of exposure to English instruction, which allows me to identify the magnitude of the state English programs implemented in Mexico since the early 90s. To construct this exposure variable I only consider public elementary schools in the morning shift, as the afternoon shifts are unstable because the same school may not provide both shifts every year.

I measure exposure, by cohort and by locality, as weekly hours of English instruction using the ratio of total weekly hours of English instruction in each school and the total number of classes.⁸ Then, for each school-cohort, I take the average hours over the six years that comprise primary school in Mexico. Finally, I take the average, by cohort, of all schools in a given locality weighting by the number of students per school. Hence, the locality average is always smaller than the figure per school because most of the primary schools in Mexico do not offer English instruction.

Table 2: Descriptive statistics

Variable	Full Sample	Speak English (a)	Don't spk English (b)	Diff. (a-b)
<i>Dependent variable</i>				
Wages (monthly pesos)	6,222.40	15,032.45	5,939.54	9,092.91***
<i>Independent variables</i>				
English (speaking ability)	.03	1.00	0.00	-
Hrs English	0.08	0.12	0.07	0.05***
Age (years)	39.05	38.31	39.08	-0.76**
Education (years)	9.67	14.02	9.53	4.49***
Female (%)	0.38	0.33	0.38	-0.05***
Indigenous (%)	0.06	0.02	0.06	-0.04***
Married (%)	0.66	0.58	0.66	-0.08***
Rural (%)	0.20	0.08	0.20	-0.12***
Observations	49,884	1664	48220	49,884

Note: These summary statistics consist of Mexicans ages 18–65 who work for pay and self-reported their ability to speak in English. Statistics shown in this table are obtained considering the survey weights. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

I link my exposure variable to the BIARE database by cohort and by locality. In the BIARE database, cohort 1996 attended sixth grade (the last grade of primary school in Mexico) in 2007, while the data I constructed for 2007 with the Mexican school census includes the average exposure from 2002 to 2007, for cohort 1996. Since I impute the average exposure at the locality level, my estimate of the policy change on the exposure could be understated

⁸A ‘locality’ is the smallest geographical delimited area in Mexico. It could include as few as one single household in a rural context (*rancheria*) or more than 100,000 inhabitants in big Mexican cities. The next geographical size is the county, composed by several localities. Finally, states are subdivided by these counties.

because this exposure variable includes schools with zero hours of English instruction. Nevertheless, this exposure measure is still informative. Furthermore, the final database I built is the first (in Mexico) to include both variables, exposure to English instruction and English skills.

The final database allows for identifying what individuals have English abilities and the hours of English instruction they had exposure to in primary school. I only consider respondents between 18 and 65 years of age, which helps to rule out kids who are more likely to be enrolled in school and retired individuals. A preliminary analysis between Mexican English speakers and non-English speakers (shown in [Table 2](#)) suggests that the former are more educated probably because they are acquiring English abilities in school; they earn more, which is consistent with the generalized idea of the positive returns to English skills in non-English speaking countries; and most of the English speakers are located in urban areas. On the other hand, Mexican English speakers are younger, potentially because they were more likely to be exposed to the recently implemented English programs in Mexico. Finally, I also show that English speakers are less likely to be women or married.

My descriptive analysis suggests that individuals who had more exposure to English instruction are more likely to have English abilities (see the third row of [Table 2](#)). This is consistent with my previous analysis in [section 1](#), where I showed that younger individuals (who are more likely to have exposure to English instruction) have more English skills than older ones, and I also showed that there are significant differences in English abilities when we compare individuals living in states with English programs and individuals living in the other states (without English programs).

4 Empirical strategy

To answer my research question, I start offering a first estimate of the returns to English skills using a model in which I control for observable characteristics that substantially mitigate the selection bias. Then, I provide a more causal estimate by exploiting different policy changes that introduced English instruction in public elementary schools in seven Mexican states: Aguascalientes, Coahuila, Durango, Nuevo Leon, Sonora, Sinaloa and Tamaulipas. Each of those states launched its English programs at different moments of time and with different intensities, in both terms of coverage and hours of English instruction. Since treated individuals are those children enrolled in school at the moment of the intervention (because they were exposed to English instruction), only certain birth-cohorts could have been potentially affected. In general, younger cohorts had some exposure to English instruction, while older cohorts had minimum or no exposure at all.

In this paper, I exploit two sources of variation: state and cohort. Both are driven by policy interventions. Then, I propose to use these two sources of variation in a Staggered Difference in Differences (SDD) model to estimate the first stage and the reduced form equations. Then, I explore the second stage to provide an instrumental variables (IV) estimate using the exogeneity caused by the intervention as an instrument of the endogenous regressor (English skills). This latter offers the local average treatment effect for those individuals who actually acquired English abilities.

4.1 Returns to English skills (main model)

In Mexico, like in many other non-English speaking countries, children could have access to English instruction almost exclusively either through the private education system or through private courses and private tutors. However, English instruction cannot be accessed by most of the population because of the high monetary cost that it involves. Furthermore, even for whom the monetary cost is not a determinant restriction, learning a foreign language would imply non-monetary costs (time and effort) that could prevent individuals from learning it.

The general view among non-English speaking countries is that English abilities will provide better labor market opportunities. However, this claim has not been formally proved by the existing literature. A naive approach would compare wages, ω_i , from English speakers, $Eng_i = 1$, and non-English speakers, $Eng_i = 0$. However, a comparison between these two groups would produce a biased estimate because of the omitted variables problem. If we do not address this issue, we would overstate our estimate because education and experience have been proven to positively affect wages. Thus omitting these two important explanatory variables from our model would violate the spherical errors assumption and exaggerate our returns to English skills estimate.

I first estimate the returns to English skills controlling for education, experience and its square, as in [Mincer \(1974\)](#). Notice, however, that I fully control for education fixed effects (FE) in order to take into account the non-linear relationship between education and English abilities (as explained in [subsection 1.1](#)). Second, although the previous estimate would take into account a lot of the differences between English speakers (ES) and non-English speakers (NES) related to education and experience, there is still a concern about selection bias. In fact, an important factor of the omitted variables problem is attributed to differences in the family background. For example, ES could have parents who care more about them and their human capital accumulation because these parents are younger (and understand better the value of English), wealthier, or because they have more education. Although I do not observe these family background characteristics, I try to mitigate the omitted variables problem by controlling for gender, marital status, ethnicity, and student status of each observed individual, i , as well as a geographical context variable (rural/urban) and the aforementioned Mincer controls, in a vector \mathbf{X}_i , as follows:

$$\omega_i = \alpha + \beta \cdot Eng_i + \mathbf{X}_i \boldsymbol{\Pi} + \epsilon_i. \quad (1)$$

The estimate, β , would be still biased if we do not take into account geographical differences. For example, it is more likely that individuals living in northern states speak English either because some of them work in the US or simply because they have a closer relationship with the US in terms of culture and education. On the other hand, southern states are farther and less related to the American culture, so it is natural to expect fewer ES (as explained in [subsection 1.2](#)). Hence, I propose two modifications to [Equation 1](#). The first one includes state FE, while the second one controls for locality FE. This latter specification intends to capture differences in smaller geographical areas such as touristic places or migrant-sending regions.

4.2 Alternative identification strategy: state English programs

4.2.1 Difference in Differences model

Even after controlling for observables, \mathbf{X}_i , we may be worried about a remaining omitted variables problem due to unobserved abilities and other family background variables. However, an exogenous variation that affects English abilities may be used as an instrument in an IV approach. I exploit policy changes in several Mexican states, which offered English instruction in Mexican public primary schools to provide a causal estimate of the effect of English skills on labor market outcomes.

In this subsection, I show the proposed first stage and reduced form equations using individual DD models for each Mexican state that introduced English instruction in public primary schools. As comparison groups, I propose neighboring states that had not offered English instruction in public primary schools. However, to rule out concerns about ad-hoc comparison states, I also offer estimates using different control groups. [Table A.1](#) summarizes the moment of the intervention, the policy change I exploit, the cohorts affected by this policy, and the comparison states I propose.

Let us consider y_{isc} as any of the main outcomes I study in this paper: exposure to English instruction, English abilities, labor force participation and wages. I estimate the intention to treat effect of offering English instruction in primary schools on these outcomes, using the following specification:

$$y_{isc} = \phi + \gamma \cdot (treatment_s \times after_c) + \delta \cdot treatment_s + \kappa_c + \mathbf{X}_{isc}\Gamma + \varepsilon_{isc},$$

where $treatment_s$ is a dummy variable that takes the value of one if individual i lives in a state that introduced an English program (treated state) and zero otherwise. Likewise, $after_c$ is a dummy variable that takes the value of one if the individual i belongs to one of the cohorts that potentially had exposure to English instruction (either in the treated state or hypothetically in the comparison one), and zero for cohorts that had no exposure. Notice, that this specification is similar to [Equation 1](#) without the ‘English skills’ variable but with controls for treatment, common cohort effects and the interaction term ($treatment_s \times after_c$).

In the first stage equation the endogenous regressor, Eng_{isc} , is the dependent variable, so that γ gives the effect of the policy change, on the acquisition of English skills. In the reduced form equation, wage (ω_{isc}) is the dependent variable. Hence, γ would give the intention to treat effect of offering English instruction in primary schools on wages. In both equations, my identifying assumption is the parallel trend assumption (PTA), which suggests that the change in the outcomes of interest between pre-treatment and post-treatment cohorts would have been the same in the treatment and the comparison states had the English program not introduced/expanded in the states with English programs. Hence, interpreting γ as the effect of the policy requires that the PTA holds.

I offer suggestive evidence on the validity of my identifying assumption using the following event study type specification:

$$y_{isc} = \phi + \sum_c \gamma_c \cdot I_{(treatment_{sc}=c)} + \delta \cdot treatment_s + \kappa_c + \mathbf{X}_{isc}\Gamma + \varepsilon_{isc},$$

where $I_{(treatment_{sc}=c)}$ is an indicator function, which identifies if individual i potentially had exposure, depending on the cohort and state he/she was born. In all models, the cohort before the intervention is the reference one (the cohort that just missed the policy). In order to support the validity of the parallel trend assumption, we should expect that the English program had no significant effect on pre-treatment cohorts. I show the results of estimating this equation in [section A.2](#).

4.2.2 Staggered Difference in Differences

In this subsection, I pool the states that introduced/expanded English programs in public primary schools to offer an average effect of this type of policies in Mexico. Although it is already of interest to know the individual effect per state, we would like to say something about the potential effect of an English program at a regional or national level. This latter is of particular interest given the lack of data needed to evaluate the National English Program in Basic Education (NEPBE) launched in 2009, nowadays known as the National English Program (or PRONI for its acronym in Spanish).

Such a model of pooled states with interventions of different intensities and implemented in different moments of time is known in the literature as Staggered Difference in Differences (SDD). To estimate this model I define $HadPolicy_{sc}$ as a dummy variable that takes the value of one if the individual i lives in a treatment state and he/she belongs to one of the affected cohorts, while it takes the value of zero otherwise. The proposed first stage and reduced form equations are the following:

$$Eng_{isc} = \theta^{fs} + \psi^{fs} \cdot HadPolicy_{sc} + \delta_s^{fs} + \kappa_c^{fs} + \mathbf{X}_{isc} \boldsymbol{\Psi}^{fs} + \varepsilon_{isc}^{fs} \quad (2)$$

$$\omega_{isc} = \theta^{rf} + \psi^{rf} \cdot HadPolicy_{sc} + \delta_s^{rf} + \kappa_c^{rf} + \mathbf{X}_{isc} \boldsymbol{\Psi}^{rf} + \varepsilon_{isc}^{rf}, \quad (3)$$

where ψ^{fs} measures the effect of an English program in Mexico on exposure to English instruction, the acquisition of English abilities, and labor market outcomes. In these specifications, I fully control for state fixed effects, δ_s , common cohort effects, κ_c , and a vector of controls, \mathbf{X}_{isc} , with demographic and household characteristics as previously defined. Hence, this model could also be categorized as a Two Way Fixed Effects (TWFE) model.

Notice that the validity of my results will depend on the parallel trend assumption, which suggests that the change in the outcomes of interest between pre-treatment and post-treatment cohorts would have been the same in the treatment and the comparison states had the English program not been introduced/expanded in the former. I will provide evidence that supports the validity of this assumption by analyzing the following event study-type equation:

$$y_{isc} = \theta + \sum_c \psi_c \cdot I_{(treatment_{sc}=c-c_s^*)} + \delta_s + \kappa_c + \mathbf{X}_{isc} \boldsymbol{\Psi} + \varepsilon_{isc},$$

where c_s^* denotes the first cohort affected by the intervention in state s . The difference $c - c_s^*$ is the time relative to c_s^* , with negative values reflecting older cohorts not exposed

to the policy. The omitted category is -1. Negative categories with zero effect validate the identifying assumption (PTA). In [Figure A.13](#), I provide suggestive evidence on the validity of this identifying assumption.

4.2.3 Instrumental Variables

We could be interested in measuring the effect of having English abilities, Eng_{isc} , on wages, ω_{isc} , instead of the effect of offering English instruction, as described by [Equation 3](#). However, our estimate β could be biased because of a remaining omitted variables problem. Indeed, we should not compare individuals with English abilities and individuals without them because it is likely that the former come from a wealthier family, had more education, and/or live in better neighborhoods (as shown in [Table 2](#)). To overcome this problem, I propose to use an instrumental variables (IV) approach, where I would take advantage of the exogenous variation caused by the state English programs to instrument the endogenous regressor, Eng_{isc} .

The first stage and reduced form equations of my IV strategy are [Equation 2](#) and [Equation 3](#), respectively. On the other hand, the second stage is the following IV specification:

$$\omega_{isc} = \phi_0 + \phi_1 \cdot \widehat{Eng}_{isc} + \delta_s + \kappa_c + \mathbf{X}_{isc}\Phi + v_{isc}. \quad (4)$$

The estimate ϕ_1 will provide the Local Average Treatment Effect (LATE) of having English abilities on wages. Notice that, my results rely on two conditions: the relevance condition and the exclusion restriction. I provide evidence on the former by looking at the statistical significance of ψ^{fs} in the first stage equation (see [Equation 2](#)). On the other hand, I am aware that there is no formal test for the exclusion restriction. However, I claim that the exogenous change caused by the intervention affects wages only through the acquisition of English abilities. The supporting idea behind this claim is that English programs in Mexico do not affect other cognitive abilities (as recently shown by [Gálvez-Soriano, Salcedo and Cabrera-Hernández \(2023\)](#)). Hence, we could think that any effect of English programs on labor market outcomes would be only through the acquisition of English abilities.

5 Results

We are interested in estimating the returns to English skills in Mexico. However, there are two main challenges that I address in this paper. First, there is no database that measures English abilities in Mexico so, I get around this issue by using the BIARE survey, which for a unique occasion asked about English speaking abilities in 2014. Second, there is an evident omitted variables problem. We could not compare individuals who decided to learn English with those that did not. Indeed, it is likely that the former come from wealthier families, live in better neighborhoods, and/or have more educated parents who care about the human capital accumulation of their kids. We could also think of a situation where geographical location plays an important role. This is the case in Mexico, where individuals living in northern states could be more interested in the acquisition of English skills to find jobs in the US or to work in firms requiring English skills in Mexico.

To mitigate the omitted variables bias, I control for observable characteristics that affect wages and for locality FE to compare individuals within the same geographical context. First, I rely on a locality fixed effects approach, which controls for unobservable characteristics that affect wages and may influence the decision of learning English, and that are fixed over time. Additionally, I control for observable characteristics, which potentially explain wages and may also explain why some individuals acquired English abilities and some did not.

Notice that even after controlling for localities FE and observable characteristics, there could be a residual selection problem either because within localities there are unobservable differences among individuals or because there are unobservable differences at the locality level that are not fixed over time. To solve this problem I propose to use a DD strategy where I exploit the implementation of state English programs, which exogenously affected the exposure to English instruction among individuals within the same state. In other words, I exploit a state-by-cohort variation in my second identification strategy.

5.1 Returns to English skills in Mexico

In this section, I provide a first estimate of the returns to English skills relying on locality FE and controlling for observable characteristics. I offer five estimates that progressively show how I mitigate the omitted variables problem: a naive estimate, using basic demographic controls, including education, controlling for other demographic characteristics, and controlling for locality FE. I also offer estimates using a low-education and high-education samples. The former isolates the effect of English abilities on wages from spillover effects of education, experience, and unobserved abilities. The latter includes the complementary between English and education. There are four main findings from this initial approach. First, the omitted variables problem is mostly due to education. Second, English speakers do not earn more than non-English speakers. Third, point estimates suggest that men English speakers earn more than women English speakers. And, fourth, there are no significant returns to English skills among low-educated individuals, but only among high educational attainment men.

Education accounts for almost 90% of the selection bias due to the observable omitted variables problem. A naive estimate would suggest that English speakers earn 167% more than non-English speakers in Mexico (0.985 natural log points). However, this estimate contains confounding factors such as gender and age. Indeed, once I control for these variables the estimated effect reduces considerably to 125%. Furthermore, the most important factor that causes the omitted variables problem is education, reducing the estimate to 9.5% and not statistically significant. Other important controls include sociodemographic characteristics. For example, for kids living in urban areas, male and non-indigenous people are more likely to know English. Hence, I further mitigate the omitted variables problem by controlling for these characteristics as shown in column (4) of [Table 3](#). With this specification the bias got reduced even more, resulting in a negative estimate, which is still not statistically significant. The remaining estimate (from column 5) reduce the bias by controlling for geographical differences at a locality level.

Table 3: Returns to English abilities in Mexico

	Full sample				Low-education				High-education			
	(1) ln(wage)	(2) ln(wage)	(3) ln(wage)	(4) ln(wage)	(5) ln(wage)	(6) ln(wage)	(7) ln(wage)	(8) ln(wage)	(9) ln(wage)			
<i>Panel A: Men and Women</i>												
Speak Eng	0.985*** (0.094)	0.811*** (0.091)	0.091 (0.079)	-0.028 (0.080)	-0.048 (0.077)	0.004 (0.282)	-0.364 (0.290)	0.586*** (0.079)	0.079 (0.065)			
Observations	49,884	49,884	49,884	49,884	49,884	29,043	29,043	20,841	20,841			
Adjusted R^2	0.005	0.080	0.153	0.182	0.248	0.000	0.253	0.005	0.194			
<i>Panel B: Men (β^M)</i>												
Speak Eng	0.771*** (0.113)	0.650*** (0.109)	0.098 (0.098)	0.054 (0.098)	0.027 (0.100)	-0.266 (0.306)	-0.272 (0.307)	0.558*** (0.089)	0.156* (0.084)			
Observations	30,765	30,765	30,765	30,765	30,765	18,725	18,725	12,040	12,040			
Adjusted R^2	0.005	0.055	0.124	0.161	0.261	0.000	0.265	0.007	0.247			
<i>Panel C: Women (β^W)</i>												
Speak Eng	1.289*** (0.172)	1.125*** (0.171)	0.142 (0.155)	-0.084 (0.151)	-0.090 (0.150)	-0.031 (0.819)	-0.146 (0.634)	0.565*** (0.156)	-0.057 (0.148)			
Observations	19,119	19,119	19,119	19,119	19,119	10,318	10,318	8,801	8,801			
Adjusted R^2	0.005	0.055	0.144	0.188	0.306	0.000	0.324	0.003	0.255			
$\beta^M = \beta^W$ [p-value]												
Basic controls	[0.013] NO	[0.012] YES	[0.028] NO	[0.036] YES	[0.037] YES	[0.791] NO	[0.757] YES	[0.971] NO	[0.613] YES			
Education	NO	NO	YES	YES	YES	NO	YES	NO	YES			
Other controls	NO	NO	NO	YES	YES	NO	YES	NO	YES			
Locality FE	NO	NO	NO	NO	YES	NO	YES	NO	YES			

Note: This table shows the effect of having English abilities on wages in Mexico. The sample consists of Mexicans ages 18–65 who work for pay. Basic controls include: cohort fixed effects, gender and indigenous dummy. Other controls include: geographical context (rural/urban), marital status and state fixed effects. Standard errors clustered at locality level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In Mexico, English speakers do not earn more than non-English speakers. This conclusion derives from my proposed model, which implies two main assumptions. First, the set of controls I include in the model account for most of the omitted variables. Second, the remaining unobservable factors that affect the acquisition of English abilities are constant over time and aggregated at the locality level. If we are willing to believe that these assumptions are valid, we may conclude that English speakers earn approximately the same as the non-English speakers (see column 5 of [Table 3](#)). This result is different than previous findings of [Delgado Helleseter \(2020\)](#), which suggest that the wage premium of Mexican English speakers is 28%.

Men English speakers earn more than women English speakers. Indeed, I find that the returns to English skills estimate is driven by men, who earn 2.7% more than non-English speakers (although this effect is not statistically significant). Women, on the other hand, do not earn more whether they have English skills or not, although the point estimate is positive. Furthermore, there is a statistically significant gender wage gap. Indeed, despite having the same abilities, women do not receive the same English wage premium that men do and, in fact, point estimate suggests that the effect could be negative. It is likely that this gender wage gap can be explained by occupational decisions that may differ between men and women. I will explore more on this mechanism in the following sections. Previous results found by [Azam, Chin and Prakash \(2013\)](#) are in line with my own findings as they found that women with English skills earn less than men English speakers (22.3% and 34.5%, respectively).

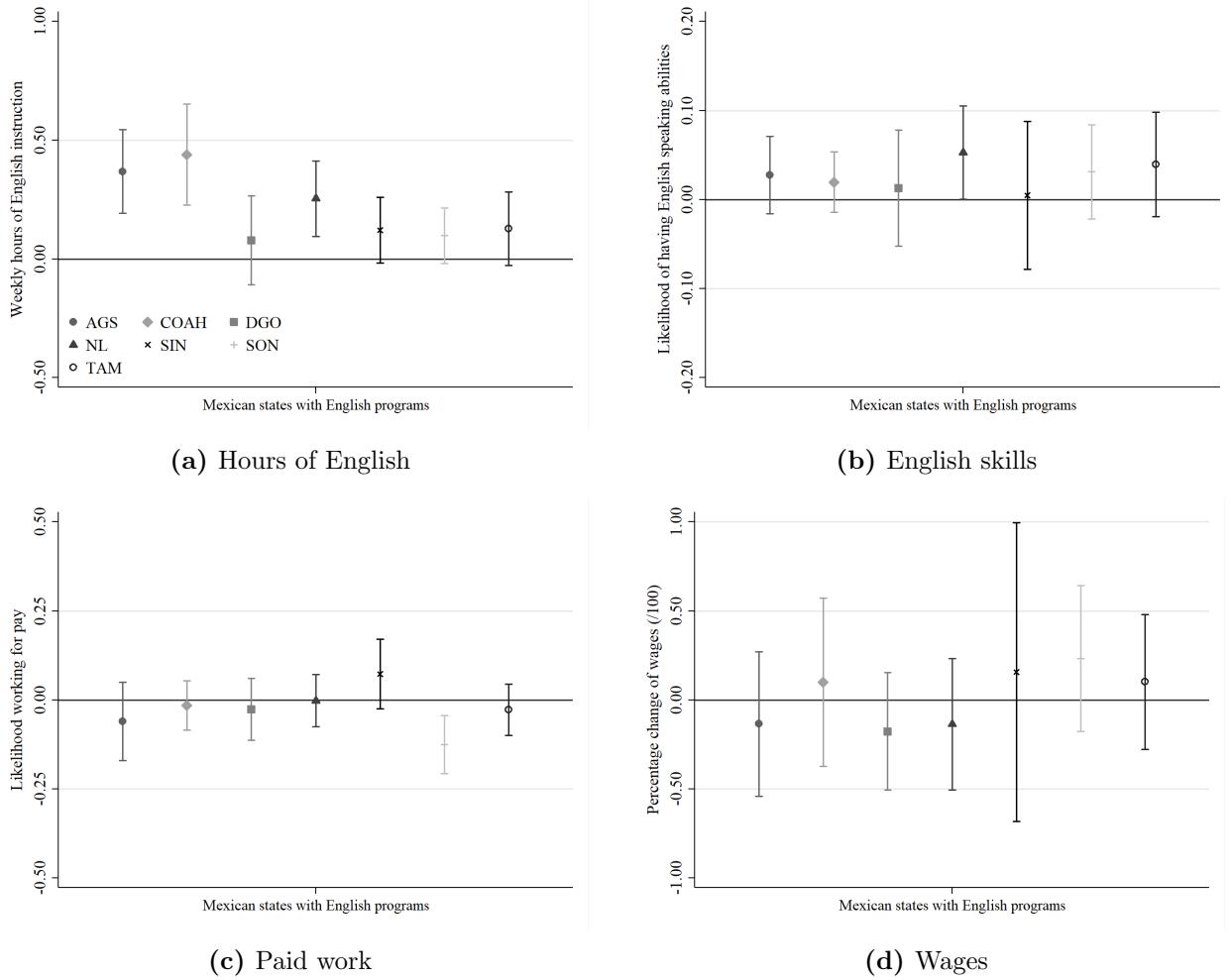
There are no significant returns to English skills among low-educated individuals, but positive returns for men. Previously, I showed that the ability to speak English increases with educational attainment. However, the exogenous change in exposure to English instruction that I will explore in the next section takes place in elementary and middle school. Hence, as an additional exercise, I propose to look at the returns to English skills among low-educated individuals (nine years of education or less). This exercise will isolate the effect of English skills (obtained during the basic education system) from the positive externalities that exposure at early stages of life has on English proficiency and labor market outcomes. I do not find any significant effect of English skills on wages. Nevertheless, this exercise removes the potentially positive mechanism of finding a better job, as an English speaker. Consistently, I do find positive effects only among high educational attainment men.

5.2 Exposure, English abilities and labor market outcomes

The model from the previous section control for a lot of the omitted variables bias using observed characteristics of the individuals as well as unobserved locality characteristics that control for a better socioeconomic context in which English speakers live. However, this model may not yet provide a causal effect of English skills on wages. First, there are unobserved characteristics that I am not including in the model such as cognitive abilities and family background characteristics that I do not observe in the data, which may affect both wages and the decision of learning English. Second, it could be the case that there are economic factors that change over time and that affect the decision of learning English, which are not captured by the locality FE.

In this section, I offer a causal effect of English skills on wages. First, I provide an estimate of the Intention to Treat (ITT) effect of offering English instruction at school. I obtain this ITT estimate for each state that implemented/expanded its English program since the early 90s or 2000s. Then I pool these states in a staggered DD model. Second, with this latter model, I study heterogeneous effects in four dimensions: gender, indigenous condition, geographical context (rural/urban), and education. Finally, I propose an IV estimate, where I use the exogenous variation generated by the state English programs as an instrument. This latter provides the Local Average Treatment Effect (LATE), which can be interpreted as the returns to English abilities among those individuals who reported having English skills after being offered English instruction at school.

Figure 4: ITT of offering English instruction (DD estimates by state)



Note: This figure shows the effect of several state English programs on exposure to English instruction, the acquisition of English skills, paid work, and wages. Each plotted estimate corresponds to a different equation per state. The sample consists of Mexicans ages 18–39, which corresponds to the cohorts affected by the policy change and those that just missed the intervention.

5.2.1 Intention to Treat Effect

In this subsection, I provide an alternative estimate of the returns to English skills in Mexico, which rules out concerns of remaining omitted variables bias. To this purpose, I propose a DD strategy where I use state-by-cohort variation in exposure to English instruction in primary school. With this strategy, I estimate the ITT effect of offering English instruction in primary school. I study four main outcomes: exposure to English instruction, English abilities, paid work and wages. Notice that the effect of the first stage is captured by the ‘English ability’ outcome. In other words, a statistically significant first stage would provide evidence that exposure to English instruction leads to the acquisition of English skills in Mexico. The remaining regression outcomes provide results of the reduced form model.

By analyzing each state separately, I obtain four main findings on the effect of English programs on labor market outcomes. First, three out of seven states significantly increased the hours of English instruction they offered in primary schools as a result of the intervention. Second, the point estimates suggest an increase in the acquisition of English abilities, but this effect was only significant for Tamaulipas. Third, the acquisition of English abilities improves with the years of schooling. And, fourth, the state English programs had no effects on either paid work or wages (except for Sonora, see [Figure 4](#)).

Not all states offered enough hours of English instruction to represent a statistically significant increase in exposure from the perspective of the individuals intended to be treated. In particular, Aguascalientes, Coahuila and Tamaulipas had a significant increase in hours of English instruction with the implementation of their respective state English programs. Point estimates suggest that the remaining states (Durango, Nuevo Leon, Sinaloa and Sonora) also increase the exposure to English instruction at school, but this increase was not statistically significant for individuals in my sample.

A significant increase in hours of English instruction translates into the acquisition of English skills. Indeed, only for states with a significant increase in exposure, there is a significant increase in the acquisition of English skills (at the 90% confidence). This result suggests that a larger implementation/expansion of an English program, such as the one offered by the Mexican government in 2009 with the National English Program in Basic Education (NEPBE), would have increased significantly English abilities among the population who had exposure to the program.

The acquisition of English abilities improves with the years of schooling. This claim comes from the the estimates shown in [Figure A.3](#).⁹ Notice, however, that this is only true for the states with a significant acquisition of English skills. Two potential mechanisms explain this result: familiarity with the English language at early stages of life and high-ability individuals learning better. First, early exposure may lead kids to pick up the English language better than kids without previous exposure. This would be consistent with the existing literature in language development ([Lenneberg, 1967](#); [Newport, 2002](#)) and second-language acquisition ([Bleakley and Chin, 2010](#)). Second, it is likely that high-ability individuals are also the ones who learn English better and the ones who show higher educational attainment.

⁹The low education sample is composed of individuals who reported having nine years of education or less. High education is the complement.

Table 4: Intention to Treat effect of offering English instruction at school
(SDD estimate)

	(1) Hrs Eng	(2) Speak Eng	(3) ln(wage)	(4) Paid work	(5) Student
<i>Panel A: Full sample</i>					
Had Policy	0.308*** (0.046)	0.015** (0.008)	0.019 (0.080)	-0.020 (0.013)	0.038*** (0.014)
Observations	13,131	13,131	13,131	22,517	22,517
Adjusted R^2	0.596	0.075	0.153	0.219	0.370
<i>Panel B: Men (β^M)</i>					
Had Policy	0.295*** (0.046)	0.016 (0.011)	-0.044 (0.069)	-0.023 (0.018)	0.044*** (0.017)
Observations	8,008	8,008	8,008	11,021	11,021
Adjusted R^2	0.581	0.069	0.186	0.313	0.408
<i>Panel C: Women (β^W)</i>					
Had Policy	0.331*** (0.057)	0.014 (0.014)	0.152 (0.152)	-0.003 (0.023)	0.031* (0.017)
Observations	5,123	5,123	5,123	11,496	11,496
Adjusted R^2	0.604	0.056	0.150	0.158	0.326
$\beta^M = \beta^W$ [p-value]	[0.269]	[0.940]	[0.172]	[0.276]	[0.396]
<i>Panel D: Low education sample (β^L)</i>					
Had Policy	0.305*** (0.062)	0.010 (0.007)	-0.169 (0.123)	0.016 (0.021)	0.005 (0.014)
Observations	6,624	6,624	6,624	10,898	10,898
Adjusted R^2	0.522	0.016	0.162	0.225	0.043
<i>Panel E: High education sample (β^H)</i>					
Had Policy	0.304*** (0.040)	0.019 (0.014)	0.227** (0.099)	-0.025 (0.018)	0.031* (0.017)
Observations	6,507	6,507	6,507	11,619	11,619
Adjusted R^2	0.659	0.070	0.146	0.263	0.432
$\beta^L = \beta^H$ [p-value]	[0.954]	[0.522]	[0.004]	[0.058]	[0.204]

Note: This table shows the effect of several state English programs on exposure to weekly hours of English instruction (column 1), English abilities (column 2), labor force participation and wages (columns 3 and 4, respectively). The sample consists of Mexicans ages 18–39. Controls include: gender, indigenous people dummy, education, cohort fixed effects and locality fixed effects. Low education sample includes individuals with nine or less years of education. High education sample is composed by individuals with more than nine years of education. Standard errors clustered at locality level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The state English programs had no effects on either labor force participation or wages. One exception was Tamaulipas, which is also the state with a greater effect on English skills acquisition. For the remaining states, there is not any effect on labor force participation and no effect on wages. Although the point estimate suggests that the effect on wages could be positive. This is also true using the low-education sample among the states with a significant increase in exposure and English skills. However, results using the low-education sample may suggest a negative effect on wages among the states with a non-significant effect on exposure (except for Sinaloa where the point estimate is positive).

On the other hand, consistent with the previous results, my staggered DD specification suggests that the implementation of English programs does not affect wages. However, the point estimate indicates that these policies may increase wages by 1.9%. Furthermore, I find an unintended positive effect of these policies increasing the enrollment rates by 3.8%. This result suggests that the potential effect of exposure to English instruction on wages may be positive and significant in future research as the existing analysis systematically excludes potentially high earners who are still enrolled in school.

Finally, I find that English programs may lead to the acquisition of English abilities. Indeed, these types of English programs increase English skills by 1.5% (see column (2) of [Table 4](#)). Furthermore, if we consider hours of English instruction as an exogenous variable (due to the implementation/expansion of the English programs), we may argue that an English program in Mexico may have increased the acquisition of English skills by 4.9% ($0.015/0.308$).

5.2.2 Heterogeneous effects

In this subsection, I explore four sources of heterogeneous effects: by gender, education, ethnicity and geographical context. First, I find that the potentially positive effect on wages is driven by women, while the effect on school enrollment is driven by men. Second, state programs offering English instruction in elementary school increase wages only among individuals with high educational attainment. Third, English programs increased English skills only among non-indigenous people, with potentially positive effects on wages. Similarly, English programs increased English abilities only among individuals living in urban areas but not in rural ones.

The main average effects shown in [subsubsection 5.2.1](#) are not a result of gender differences. However, I find that the potentially positive effect on wages is driven by women, while the effect on school enrollment is driven by men. Additionally, the exposure to English instruction is the same for men and women, which suggests that other spillover effects explain the stronger contribution of women to the wage result, and men contributing more on the school enrollment. As there is not a similar empirical approach used in the existing literature, it is not possible to compare these results with other papers. However, in [subsubsection 5.2.4](#), I will explore occupational decisions as a potential mechanism that explains wages differential effects. On the other hand, the result on school enrollment is likely to be explained by cultural aspects that prevent women from pursuing higher degrees as usually they dropout to take care of children and/or elderly in the household.

State programs offering English instruction in elementary school increase wages only

among individuals with high educational attainment. This potential increase in wages could be due to the acquisition of English skills, but may also involve other mechanisms ranging from a signaling effect to improvements in other cognitive skills. However, this latter mechanism is less plausible in light of recent evidence provided by [Gálvez-Soriano, Salcedo and Cabrera-Hernández \(2023\)](#), where he shows that exposure to English instruction has no effect on cognitive skills (language and mathematical abilities).

These English programs increased English skills only among non-indigenous people. It is likely that this result is a consequence of a non-significant effect of the English programs on exposure to English instruction. In other words, indigenous people did not have enough exposure as to improve their English skills. Consequently, I do not find any effect on indigenous people's wages. On the other hand, English programs significantly increased English skills and, potentially, also increased wages of non-indigenous people (see panel A of [Table A.2](#)).

Similarly, English programs increased English abilities only among individuals living in urban areas but not in rural ones. In particular, I find that English programs increase English abilities by almost 2%. The results on English skills may be due to differences in the implementation of the programs themselves. This means that the first English programs were implemented only in urban areas due to the requirements of access to computers and to the Internet, which is almost exclusively available in the urban context. Consequently, rural areas did not increase exposure and, hence, there is no effect on English skills and wages (see panel B of [Table A.2](#)).

Table 5: Returns to English abilities
(IV estimate)

	(1) Structural-OLS	(2) First Stage	(3) Reduced Form	(4) Structural-IV
Speak Eng	0.142 (0.102)			1.252 (5.084)
Had Policy		0.015** (0.008)	0.019 (0.080)	
Observations	13,131	13,131	13,131	13,131
Adjusted R^2	0.153	0.075	0.153	

Note: This table shows the structural equation (column 1), first stage (column 2), reduced form and IV estimates (columns 3 and 4, respectively). The sample consists of Mexicans ages 18–39. Controls include: gender, indigenous people dummy, years of schooling of household head, education fixed effects, cohort fixed effects and locality fixed effects. Standard errors clustered at locality level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5.2.3 IV estimate of the effect of English on earnings

Estimates shown in [subsubsection 5.2.3](#) suggest that English programs may increase wages (according to the reduced form equation). However, this effect may be explained by mechanisms other than the acquisition of English abilities. To isolate the effect of English programs on wages, I propose to instrument the endogenous regressor, Eng_{isc} , with the policy change

variable, $HadPolicy_{sc}$. This would give me the local average treatment effect of English programs (among individuals who actually acquired English skills) on wages.

My results suggest that the increase in wages is not due to the acquisition of English abilities. Indeed, I do not find a significant effect of English skills on wages, although the point estimate suggests a positive effect (see [Table 5](#)). Hence, it could be possible that the potentially positive effects on wages shown in the reduced form equation is due to positive externalities of English instruction, which could operate through other cognitive skills, more education, and occupational choices, among others. In the next section, I will explore occupational choices as a potential mechanism.

5.2.4 Mechanisms

Among the potential mechanisms that explain the non-significant effect of English skills on wages, occupational choices seem to provide a plausible explanation as there may be movements among occupations leaving earnings unaffected. Similarly, although other cognitive skills may also explain the effect on wages, the existing literature has shown that exposure to English instruction has no effect on cognitive skills in Mexico (see [subsubsection 5.2.2](#)).

To study the effect of English programs on occupations, I use the International Standard Classification of Occupations (ISCO-08) from the International Labor Organization (ILO) at one digit code level. The farming category includes agricultural, forestry, and fishery laborers. The category ‘elementary’ refers to cleaners, helpers, building construction laborers, transport and storage laborers, food preparation assistants, street sales workers, and garbage collectors. Machine operators comprise plant and machine operators, and assemblers in several economic industries (this category also includes locomotive, car, van, motorcycle, taxi, heavy truck, bus, mobile plant and ships operators). Most of the workers in the construction industry are classified in Crafts along with blacksmiths, toolmakers, machinery mechanics and repairers, and handicraft and printing workers. ‘Customer service’ comprises travel attendants, conductors, guides, cooks, waiters, bartenders, hairdressers, beauticians, personal care workers, and protective services workers. The ‘sales’ category includes salespersons, cashiers, and ticket clerks. ‘Professionals’ contains occupations of science, engineering, health, teaching, business and administration, information and communications technology, and legal, social, and cultural workers.

Using this classification, I obtain three main findings. First, there is a substitution of occupations with low requirements of English skills for occupations requiring more English skills. Second, men are not moving to professional and technical occupations. Instead, men are substituting crafts and elementary occupations for clerical support ones. And, third, women make a strong substitution between elementary occupations and sales for professionals and technical occupations. Overall, these results suggest that although English abilities may not affect wages, individuals are better off because they are moving from occupations that require more physical effort to occupations more intensive in cognitive skills.

There is a substitution of occupations with low requirements of English skills for occupations requiring more English skills. Occupations from column (1) to column (6) in [Table 6](#) are the six occupations with less requirement of English abilities (according to my previous findings of [Figure 2](#)). Indeed, the proportion of English speakers in sales occupations (2.2%)

is below the national mean (2.75%), while clerical support occupations are almost double this proportion (with 4% of English speakers). Furthermore, in general, I find a substitution of the six first occupations (with fewer proportions of English speakers) for the three occupations with more proportion of English speakers (plus the category of individuals who reported working abroad). Two exceptions are farming and machine operators. The case of farming occupations could be explained by individuals who decided to migrate after acquiring English skills and returned to their home villages to continue working in agricultural-related activities. On the other hand, a potential story for the increase in the likelihood of working as a machine operator could be explained as a result of more individuals moving to occupations requiring understanding a minimum of English to operate machines with controller descriptions in English.

Men are not moving to professional and technical occupations, but they substitute crafts and elementary occupations for clerical support ones. Indeed, as in the case of the national results (including men and women), men who had exposure to the policy substituted occupations with low requirements of English skills for occupations with more requirements. However, one interesting exception is professional and technical occupations. In other words, it is unlikely that men who used to work as cleaners, helpers, and craftsmen are now professional workers. Instead, workers in elementary occupations are now working as clerks and assistants, which means that they are moving from physically demanding jobs to occupations more intensive in cognitive skills.

Women make a strong substitution of elementary and sales occupations for professional and technical occupations. In fact, as men do, women also substitute jobs with low English skills requirements for jobs more demanding on English skills. And, although women make a strong substitution of elementary occupations (like men), they additionally substitute sales occupations (unlike men). This is an interesting finding in light of proportionally more women than men working in sales. Furthermore, these women who are moving away from elementary and sales are going to professional and technical occupations. This is also a striking finding because it could be inferred that women who had exposure to the intervention are more likely to graduate from college or with a technical degree.

Table 6: ITT effect of offering English instruction at school on occupational decisions (SDD estimate)

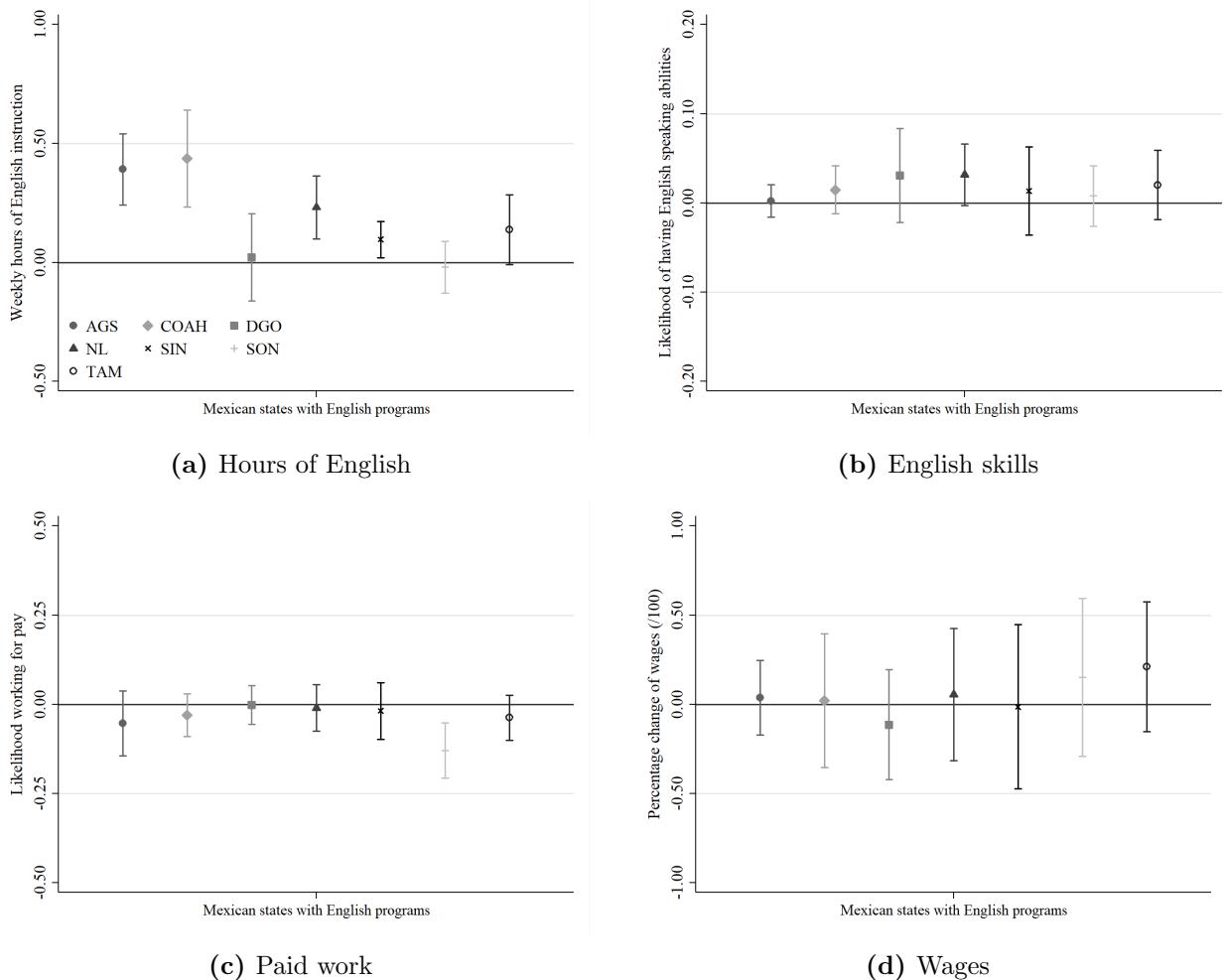
	(1) Farming	(2) Elem operator	(3) Machine operator	(4) Crafts service	(5) Customer service	(6) Sales	(7) Clerical support	(8) Prof/ Tech	(9) Mgmt	(10) Abroad
<i>Panel A: Full sample</i>										
Had Policy	0.006 (0.007)	-0.031 (0.020)	0.015 (0.016)	-0.020 (0.013)	-0.004 (0.013)	-0.017 (0.014)	0.015 (0.013)	0.025 (0.017)	0.013 (0.020)	-0.002 (0.004)
Observations	13,131	13,131	13,131	13,131	13,131	13,131	13,131	13,131	13,131	13,131
Adjusted R^2	0.259	0.174	0.094	0.062	0.011	0.045	0.047	0.250	0.050	0.025
<i>Panel B: Heterogeneous effects by gender</i>										
Men (β^M)										
Had Policy	0.001 (0.011)	-0.028 (0.026)	0.019 (0.024)	-0.022 (0.019)	-0.007 (0.015)	-0.017 (0.013)	0.026* (0.013)	0.012 (0.024)	0.021 (0.026)	-0.004 (0.006)
Observations	8,008	8,008	8,008	8,008	8,008	8,008	8,008	8,008	8,008	8,008
Adjusted R^2	0.309	0.192	0.082	0.064	0.008	0.019	0.008	0.180	0.068	0.034
Women (β^W)										
Had Policy	0.006 (0.007)	-0.047* (0.027)	0.010 (0.021)	-0.014 (0.017)	0.010 (0.023)	-0.010 (0.028)	0.001 (0.031)	0.050** (0.025)	-0.008 (0.028)	0.003 (0.004)
Observations	5,123	5,123	5,123	5,123	5,123	5,123	5,123	5,123	5,123	5,123
Adjusted R^2	0.370	0.234	0.119	0.059	0.013	0.071	0.048	0.343	0.026	0.050
$\beta^M = \beta^W$ [p-value]	[0.427]	[0.580]	[0.978]	[0.837]	[0.370]	[0.904]	[0.435]	[0.255]	[0.343]	[0.283]

Note: This table shows heterogeneous effects of offering English instruction in several Mexican states on occupational decisions. The occupations were determined using the International Standard Classification of Occupations (ISCO-08) from the International Labor Organization (ILO) at one digit code level. The managerial category includes supervisors from other occupations. The abroad category contains individuals who reported working abroad, but it is a mix of all other categories. The sample contains Mexicans ages 18–33 who self-reported their ability to speak in English. Standard errors clustered at locality level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

6 Robustness Checks

In this section, I offer three robustness checks that provide more reliability to my results. First, I provide evidence that my results from [Figure 4](#) are robust to changes in the comparison states. Then, I propose a narrower comparison window in my staggered DD specification to show that my results are robust to narrower group-cohorts. Third, I propose an alternative estimate in response to the recent critique on DD models producing biased estimates in the presence of heterogeneous treatment effects (see [De Chaisemartin and D'Haultfoeuille \(2022\)](#) for a review). In particular, I explore the heterogeneity-robust DD estimators proposed by [Callaway and Sant'Anna \(2021\)](#) and [Sun and Abraham \(2021\)](#).

Figure 5: ITT of offering English instruction (DD estimates with multiple comparison groups)



Note: This figure shows the effect of several state English programs on exposure to English instruction, the acquisition of English skills, paid work, and wages. Each plotted estimate corresponds to a different equation per state. The sample consists of Mexicans ages 18–39, which corresponds to the cohorts affected by the policy change and those that just missed the intervention.

[Figure 5](#) shows the results of the robustness check where I change the comparison group.

Originally, I proposed one single comparison state per single policy, while now I am proposing to compare each treatment state with all neighboring states (always including the originally proposed comparison one). The idea of this exercise is to show that my estimates do not significantly change to a different comparison group.

I do find that my original estimates are robust to changes in the comparison group. Indeed, most of my estimates from [Figure 4](#) keep the same sign and significance even after changing the comparison states. There are two exceptions. First, the effect of Durango's English program is not statistically significant on English skills. This result is not surprising since the original estimate was significant only at the 90% confidence level. Second, the increase in exposure to English instruction in the state of Sinaloa becomes statistically significant, which positively impacts wages. However, the parallel trend assumption does seem to hold for the exposure variable (see [Figure A.18](#)). For the remaining states, I provide suggestive evidence on the validity of the parallel trend assumption (see Appendix, ??).

Table 7: Returns to English abilities
(IV estimate with narrower comparison group)

	(1) Structural-OLS	(2) First Stage	(3) Reduced Form	(4) Structural-IV
Speak Eng	0.086 (0.156)			-4.081 (6.348)
Had Policy		0.018* (0.010)	-0.073 (0.111)	
Observations	5,926	5,926	5,926	5,926
Adjusted R^2	0.159	0.050	0.159	

Note: This table shows the structural equation (column 1), first stage (column 2), reduced form and IV estimates (columns 3 and 4, respectively). The sample consists of Mexicans ages 18–33. Controls include: gender, indigenous people dummy, years of schooling of household head, education fixed effects, cohort fixed effects and locality fixed effects. Standard errors clustered at locality level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In the second robustness check, I address the concern that I may be comparing young adults affected by the intervention with older individuals whose exposure to English and technology could vary significantly from those younger cohorts. Part of this concern is already addressed by the cohorts FE. However, if exposure to English and technology varies over time, my results may be affected by these older cohorts. Hence, I propose an exercise in which I compare the treated cohorts to a narrower window of untreated cohorts.

My results suggest that the estimates from my staggered DD specification and my IV strategy are robust to changes in the comparison group. First, I provide suggestive evidence on the validity of the parallel trend assumption in [Figure A.13](#). Then, I show the IV estimates from this exercise in [Table 7](#). I find that the estimates with a different comparison group are the same in sign and significance as the original results. However, the instrument estimate in the reduced form equation becomes non-significant. Nevertheless, the conclusion derived from the IV estimate turns out the same. Indeed, I still find that English skills have no effect on wages.

Table 8: Intention to Treat effect of offering English instruction at school
(Robust SDD estimate)

	(1) Hrs Eng	(2) Speak Eng	(3) ln(wage)	(4) Paid work	(5) Student
<i>Panel A: Full sample</i>					
Had Policy	0.323*** (0.045)	0.015** (0.007)	0.021 (0.081)	-0.015 (0.012)	0.035*** (0.013)
Observations	13,063	13,063	13,063	22,493	22,493
Adjusted R^2	0.616	0.073	0.148	0.219	0.371

Note: This table shows the effect of several state English programs on exposure to weekly hours of English instruction (column 1), English abilities (column 2), labor force participation and wages (columns 3 and 4, respectively). The sample consists of Mexicans ages 18–39. Controls include: gender, indigenous people dummy, education, cohort fixed effects and locality fixed effects. Low education sample includes individuals with nine or less years of education. High education sample is composed by individuals with more than nine years of education. Standard errors clustered at locality level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Finally, I explore the heterogeneity-robust DD estimator proposed by [Callaway and Sant'Anna \(2021\)](#) to determine if my original staggered DD estimates are robust to heterogeneous treatment effects. The results shown in [Table 8](#) suggest that my original estimates are robust in terms of sign, significance and very close in magnitude to changes in the specification to take into account a potential bias due to heterogeneity in treatment effects.

7 Conclusions

In this paper, I offer a comprehensive description of the English abilities in Mexico, which was never done before by the existing literature due to the lack of data on English skills. Furthermore, I provide the first causal estimate on the effect of exposure to English instruction on the acquisition of English abilities as well as the returns to English skills in Mexico. This latter finding contributes to the existing literature, which has mostly offered evidence on the returns to English skills in the context of immigrants in English-speaking countries or in the context of former British colonies (India and South Africa). My results provide some of the first estimates in the context of a non-English-speaking country and the first among Latin American economies.

In my descriptive analysis, I document five important facts. First, there is a gender-English gap in Mexico, with more men declaring to speak English. Second, more exposure to English instruction is associated with more English skills. Third, the ability to speak English increases with educational attainment. Fourth, indigenous people are way behind in the process of learning English. Similarly, rural areas are falling behind in this learning process. Additionally, I find that elementary and manual unskilled occupations do not require English abilities in Mexico. On the other hand, clerical support, professional, and managerial

occupations concentrate most English speakers in Mexico. After the descriptive analysis, I explore the relationship between English skills and wages.

A first estimate of the returns to English skills in Mexico would suggest that English speakers do not earn more than non-English speakers. Additionally, I find that the omitted variables problem, which produces a biased estimate, is mostly due to education. Furthermore, male English speakers earn more than women English speakers. And, there are no significant returns to English skills among low-educated individuals. All these estimates rely on a model in which I control for observable characteristics. However, we may still be concerned about an endogeneity problem due to omitted variables such as abilities and family background variables. Furthermore, it may also be the case that my estimates are biased due to a measurement error in the English skills variable.

To provide a causal estimate, I exploit the implementation of several state English programs that offered English instruction in public primary schools. First, I provide an estimate of the Intention to Treat (ITT) effect of offering English instruction at school. I obtain this ITT estimate for each state that implemented/expanded an English program since the early 90s or 2000s. Then I pool these states in a staggered DD model. Second, with this latter model, I study heterogeneous effects in three dimensions: gender, indigenous condition, and geographical context (rural/urban). Finally, I propose an IV estimate, where I use the exogenous variation generated by the state English programs as an instrument. This latter provides the Local Average Treatment Effect (LATE), which can be interpreted as the returns to English abilities among those individuals who reported having English skills after being offered English instruction at school.

My results suggest that the implementation of English programs does not have any effect on wages. However, the point estimate suggests a potentially positive effect, especially among high-attainment individuals. This potential increase in wages could be due to the acquisition of English skills, but may also involve other mechanisms ranging from a signaling effect to improvements in other cognitive skills. However, this latter mechanism is less plausible in light of recent evidence provided by [Gálvez-Soriano, Salcedo and Cabrera-Hernández \(2023\)](#), where he shows that exposure to English instruction has no effect on cognitive skills (language and mathematical abilities).

I also find that state policies offering English instruction in elementary schools may lead to the acquisition of English abilities. Indeed, these types of interventions increase English abilities by 1.5%. Furthermore, if we consider hours of English instruction as an exogenous variable (due to the implementation/expansion of the state English programs), we may argue that an English instruction in Mexico may increase the acquisition of English skills by 4.9% ($0.015/0.308$).

Looking at the heterogeneous effects I find that, first, the potentially positive effect on wages is driven by women, while the effect on school enrollment is driven by men. Second, state programs offering English instruction in elementary school increase wages only among individuals with high educational attainment. Third, English programs increased English skills only among non-indigenous people, with potentially positive effects on wages. Similarly, English programs increased English abilities only among individuals living in urban areas but not in rural ones.

Furthermore, my IV estimate provides evidence that English skills do not conduce to higher wages in Mexico. Likewise, my results suggest that the effect of English programs increasing wages is not due to the acquisition of English abilities. Indeed, I do not find a significant effect of English skills on wages, although the point estimate suggests a positive effect.

Regarding the mechanisms, I do find that there is a substitution of occupations with low-English skills requirements for occupations requiring more English skills. Second, men substitute crafts and elementary occupations for clerical support ones. And, third, women make a strong substitution of elementary occupations and sales for professionals and technical occupations. Overall, these results suggest that although English abilities may not affect wages, individuals are better off because they are moving from occupations that require more physical effort to occupations more intensive in cognitive skills.

Finally, I am aware, however, that my results rely on the self-reported English ability variable, which could still result in a biased estimate due to a measurement error problem. In particular, it is likely that English skills are sub-reported in the BIARE survey, which leads to an overstated effect in the second stage. And, although the final effect is not statistically significant, the point estimate seems implausible. Nevertheless, this first approach to estimating the returns to English skills in Mexico results in an improvement of our understanding of the value of English among non-English-speaking countries. Furthermore, this document could be a referent for future research as it provides the first causal estimate of its kind among the existing literature.

References

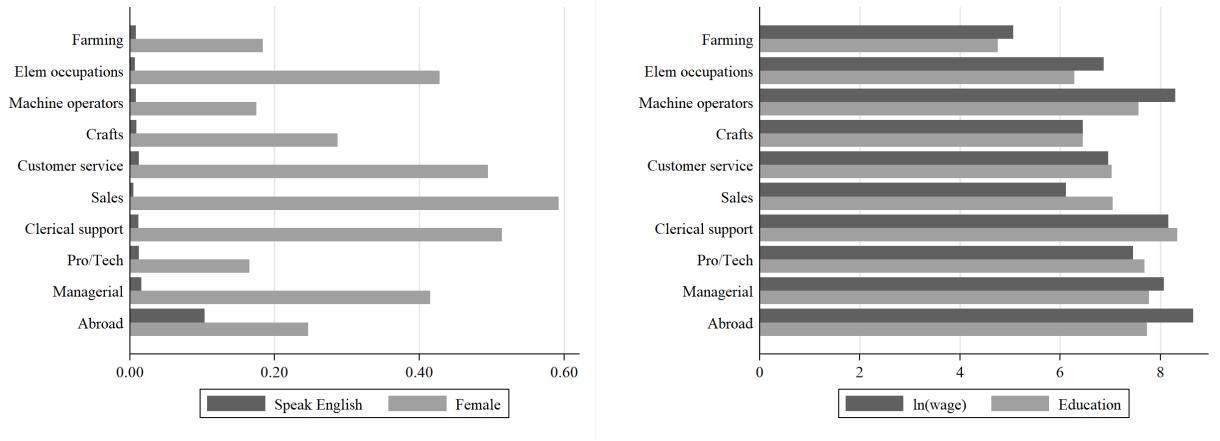
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Appendix

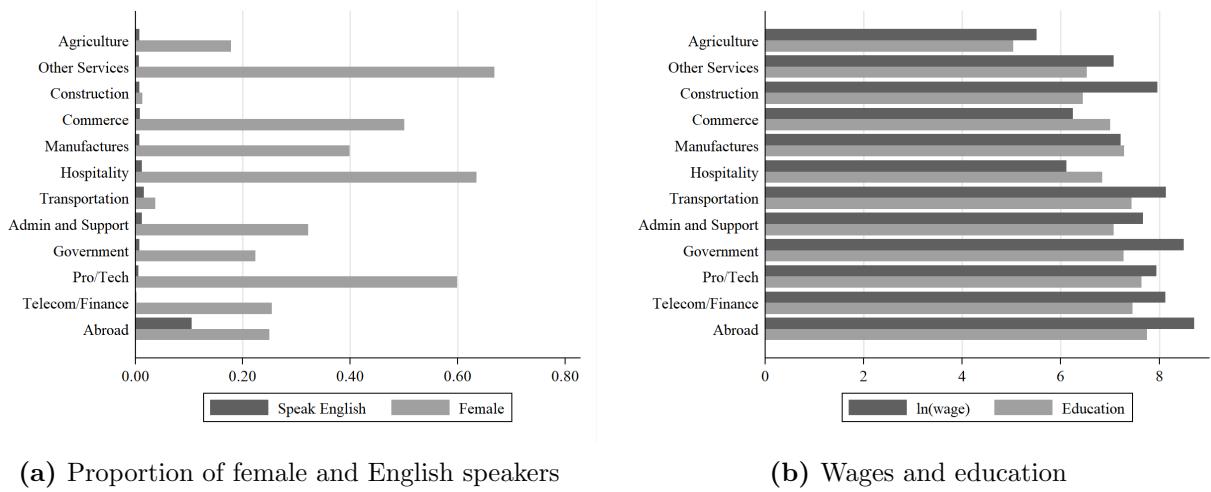
A.1 English abilities, occupations and economic industries

Figure A.1: English abilities, wages and education by occupations (low education)



Note: This figure shows the percentage of individuals who have English speaking abilities, mean wages, percentage of women and their mean schooling, by aggregates of occupations in Mexico. The occupations were determined using the International Standard Classification of Occupations (ISCO-08) from the International Labor Organization (ILO) at one digit code level. The managerial category includes supervisors from other occupations. The abroad category contains individuals who reported working abroad, but it is a mix of all other categories. The sample contains Mexicans ages 18–65 who self-reported their ability to speak in English and who have nine years of education or less. Mean English ability for this sample is 0.007 (0.7%).

Figure A.2: English abilities, wages and education by industries (low education)



(a) Proportion of female and English speakers

(b) Wages and education

Note: This figure shows the percentage of individuals who have English speaking abilities, mean wages, percentage of women and their mean schooling, by aggregates of economic industries in Mexico. The economic industries were determined using the North American Industry Classification System (NAICS) at two digit code level. The 'Construction' category includes mining, utilities and construction industries. The 'Telecom/Finance' category includes telecommunications, finance and real state industries. Finally, the 'Professional/Technical' category includes professional, technical and management industries. The abroad category contains individuals who reported working abroad, but it is a mix of all economic industries. The abroad category contains individuals who reported working abroad, but it is a mix of all economic industries. The sample contains Mexicans ages 18–65 who self-reported their ability to speak in English and who have nine or less years of education. Mean English ability for this sample is 0.007 (0.7%).

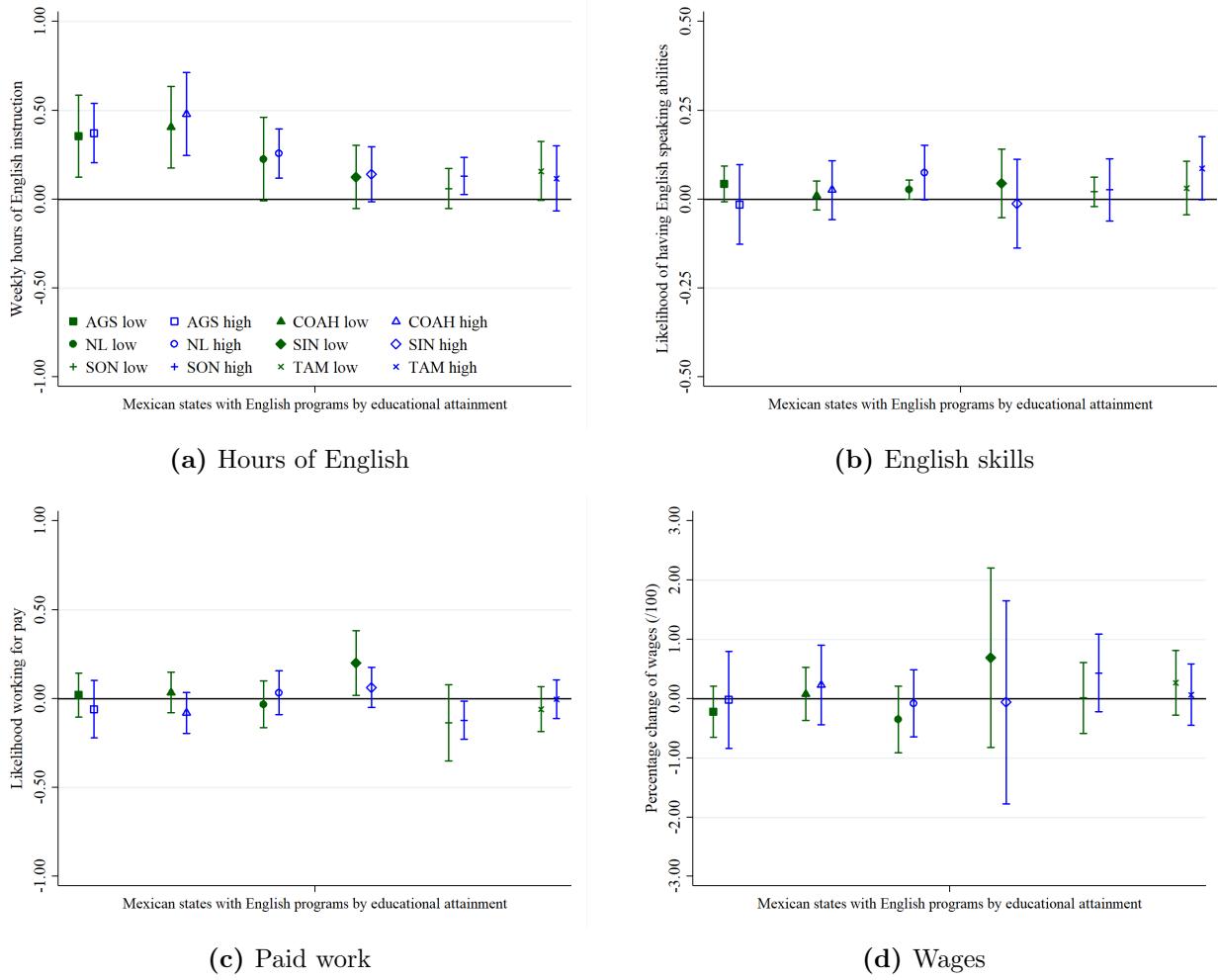
Table A.1: Policy changes in Mexican states

State	Year of impl.	Policy change	Cohorts affected	Hrs of English		Policy details	Comparison state
				Before policy	After policy		
Nuevo Leon	1993	1998	1981-1996	0.97	2.75	Only sixth grades	SLP
Sonora	1993	2004	1989-1996	1.64	5.52	Only 1st and 2nd grades	BC
Coahuila	1995	1999	1979-1996	2.73	9.09	Started w/trial stage	Chihuahua
Tamaulipas	2001	2001	1983-1996	1.21	2.89	Only fourth grades	BC
Aguascalientes	2001	2001	1986-1995	2.36	8.13	No info. available	Zacatecas
Durango	2002	2002	1985-1996	0.33	1.00	Started w/trial stage	SLP
Sinaloa	2004	2004	1989-1996	0.70	1.86	No info. available	Nayarit

Note: These summary statistics consist of Mexicans ages 18–65 who self-reported their ability to speak English.

Source: I computed the hours of English instruction using the Mexican school census (Statistics 911). Policy details from Nuevo Leon, Sonora, Coahuila, and Tamaulipas were obtained from their respective websites (see [section 2](#) for the original sources). Details from Durango were obtained from an unofficial [source](#). There are not information available for the state English programs of Aguascalientes and Sinaloa. However, for all states, the information provided from the data in the school census coincides with official and unofficial sources in terms of the release year of each state English program.

Figure A.3: ITT of offering English instruction (DD estimates by state)



Note: This figure shows the effect of several state English programs on exposure to English instruction, the acquisition of English skills, paid work, and wages. Each plotted estimate corresponds to a different equation per state. The sample consists of Mexicans ages 18–39, which corresponds to the cohorts affected by the policy change and those that just missed the intervention. Low education is considered as nine years of education or less, while high education is considered as more than nine years of schooling.

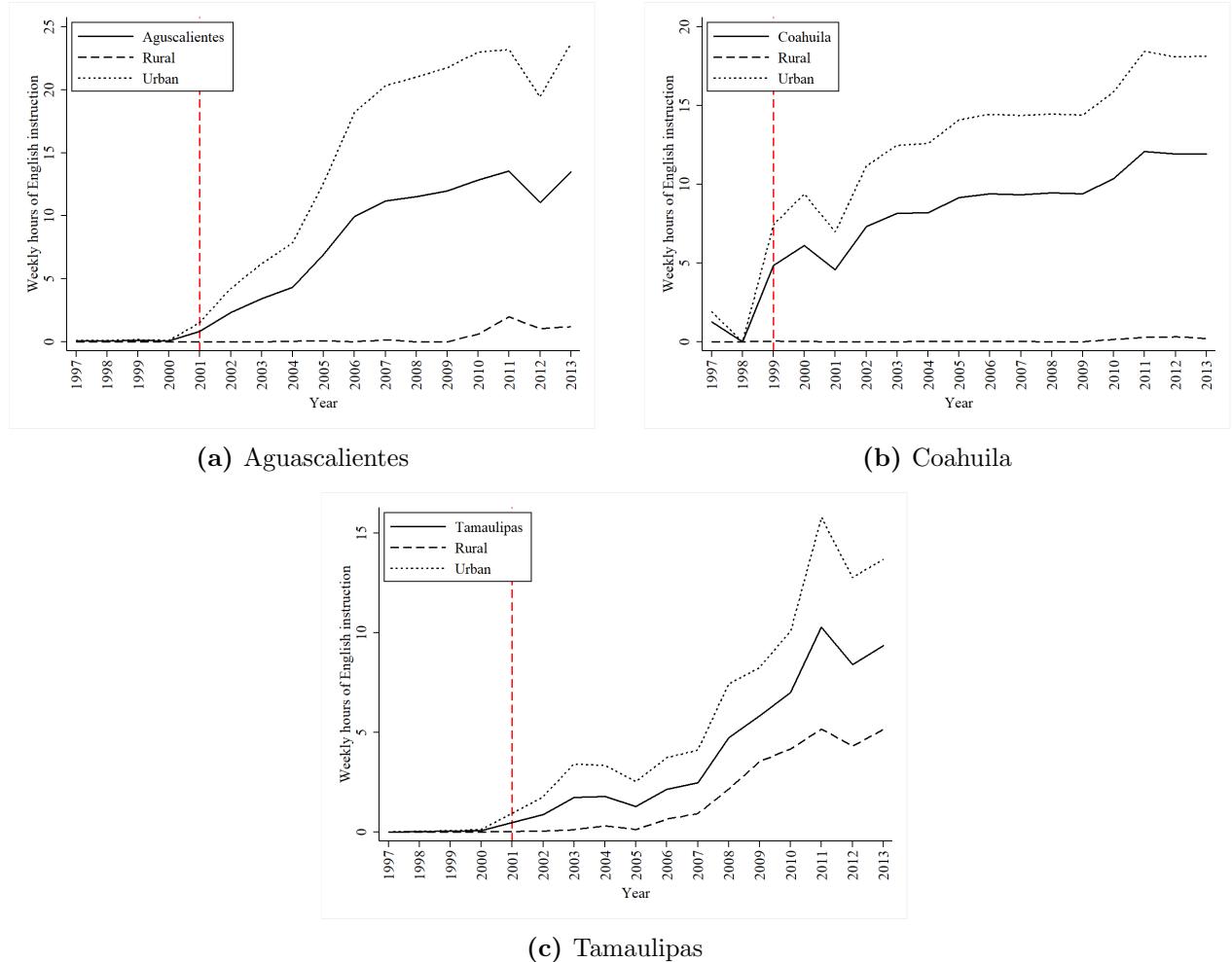
A.2 Parallel Trend Assumption

Table A.2: Heterogeneous effects of offering English instruction at school (SDD estimate)

	(1) Hrs Eng	(2) Speak Eng	(3) ln(wage)	(4) Paid work	(5) Student
<i>Panel A: By ethnicity</i>					
Indigenous (β^I)					
Had Policy	0.110 (0.160)	-0.034 (0.091)	0.309 (1.202)	0.076 (0.187)	0.286** (0.126)
Observations	188	188	188	340	340
Adjusted R^2	0.929	0.410	0.193	0.380	0.329
Non-Indigenous (β^N)					
Had Policy	0.306*** (0.046)	0.015* (0.008)	0.030 (0.081)	-0.020 (0.013)	0.036*** (0.014)
Observations	12,943	12,943	12,943	22,177	22,177
Adjusted R^2	0.593	0.076	0.148	0.219	0.371
$\beta^I = \beta^N$ [p-value]	[0.348]	[0.208]	[0.212]	[0.819]	[0.011]
<i>Panel B: By geographical context</i>					
Rural (β^R)					
Had Policy	-0.025 (0.024)	-0.004 (0.013)	-0.394 (0.240)	0.008 (0.035)	0.034 (0.026)
Observations	2,171	2,171	2,171	4,208	4,208
Adjusted R^2	0.003	0.062	0.227	0.262	0.310
Urban (β^U)					
Had Policy	0.338*** (0.050)	0.018** (0.009)	0.078 (0.084)	-0.016 (0.014)	0.030** (0.014)
Observations	10,960	10,960	10,960	18,309	18,309
Adjusted R^2	0.623	0.088	0.131	0.219	0.385
$\beta^R = \beta^U$ [p-value]	[0.000]	[0.235]	[0.072]	[0.514]	[0.779]

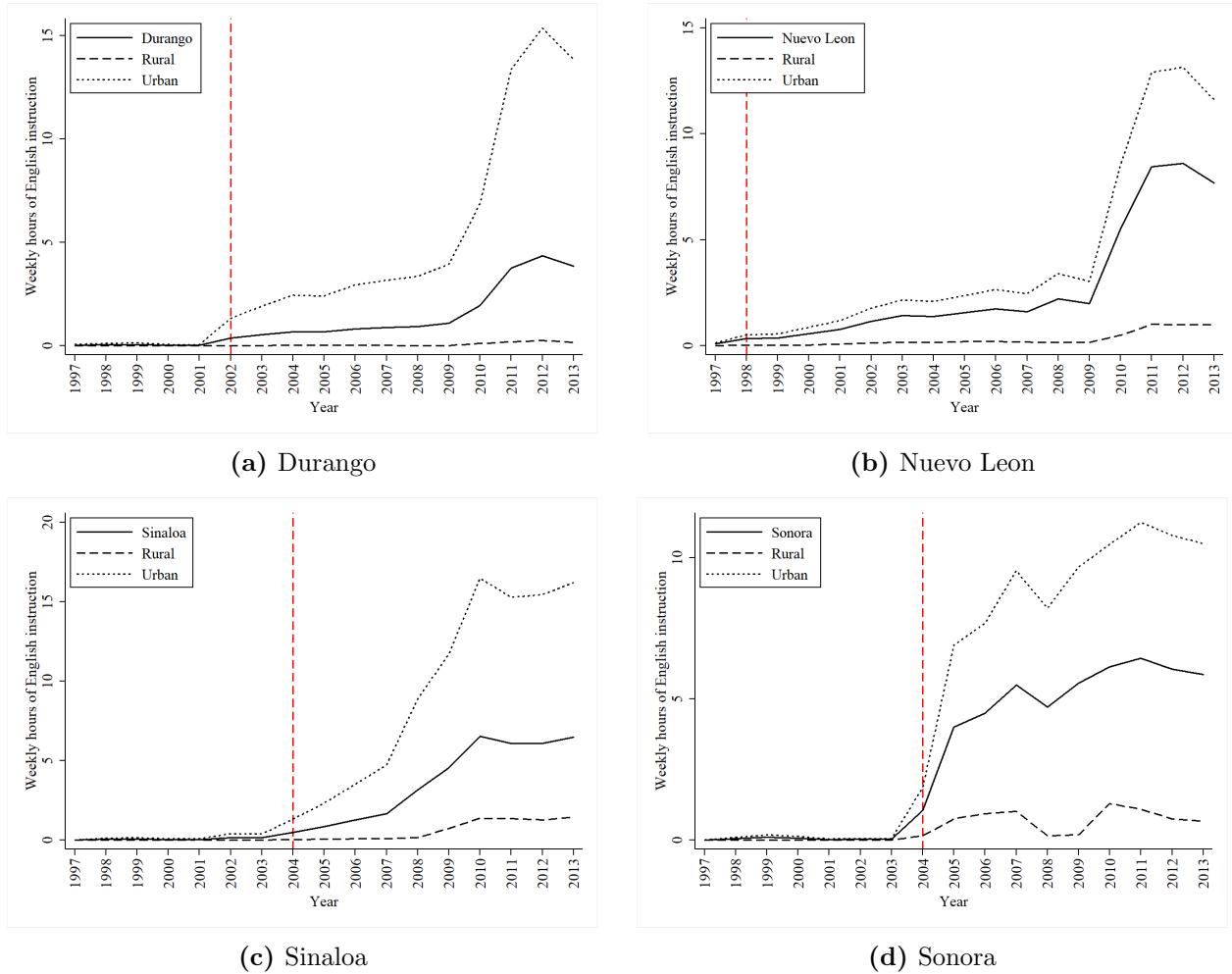
Note: This table shows heterogeneous effects of offering English instruction on exposure to hours of English instruction, English abilities, wages, paid work and school enrollment. Standard errors clustered at locality level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure A.4: Mexican states with a significant increase in hours of English instruction in public primary schools



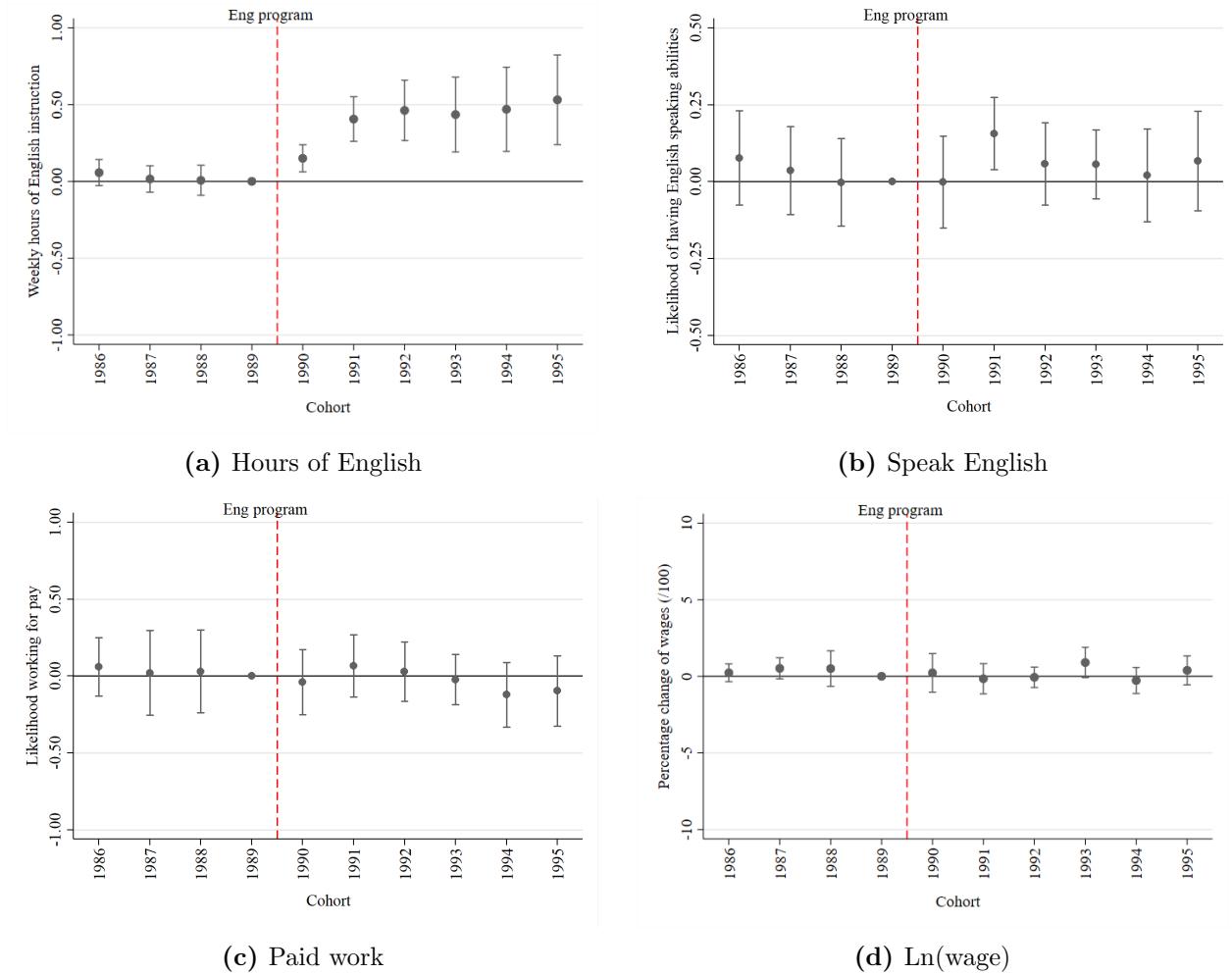
Note: Average weekly hours of English instruction are plotted. The data used comes from the administrative records in the Mexican school census (also known as Statistics 911). The vertical dotted lines represent the first year of implementation/expansion of the state English program. The four states shown in this figure represent only the Mexican states that had a significant increase in hours of English instruction during the implementation/expansion of their respective English programs.

Figure A.5: Mexican states with some English instruction in public primary schools



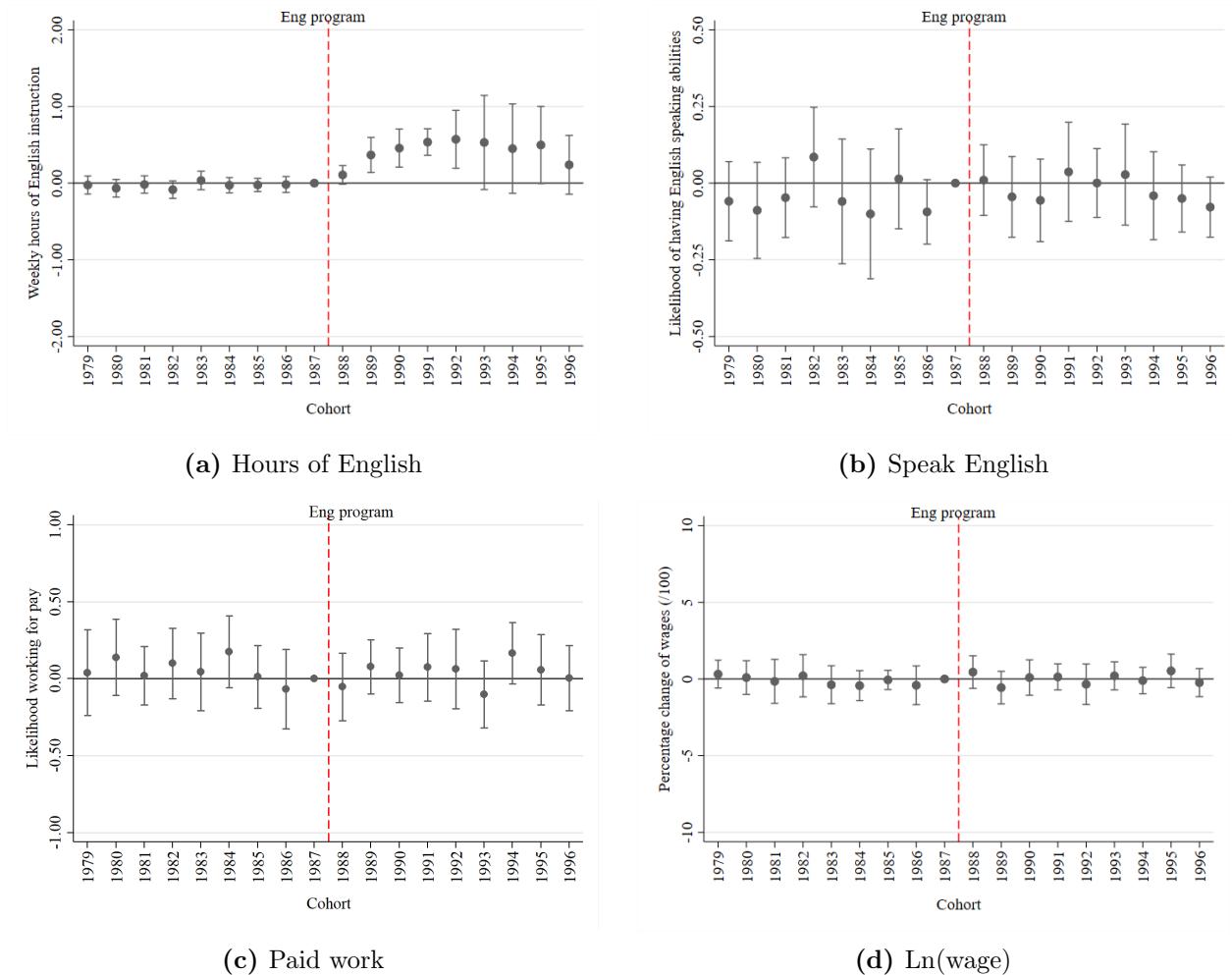
Note: Average weekly hours of English instruction are plotted. The data used comes from the administrative records in the Mexican school census (also known as Statistics 911). The vertical dotted lines represent the first year of implementation/expansion of the state English program. The four states shown in this figure represent only the Mexican states that implemented some English program, but offered it to a small number of schools and/or to some selected grades, resulting in an insignificant increase in hours of English instruction.

Figure A.6: Pre-trends test for Aguascalientes



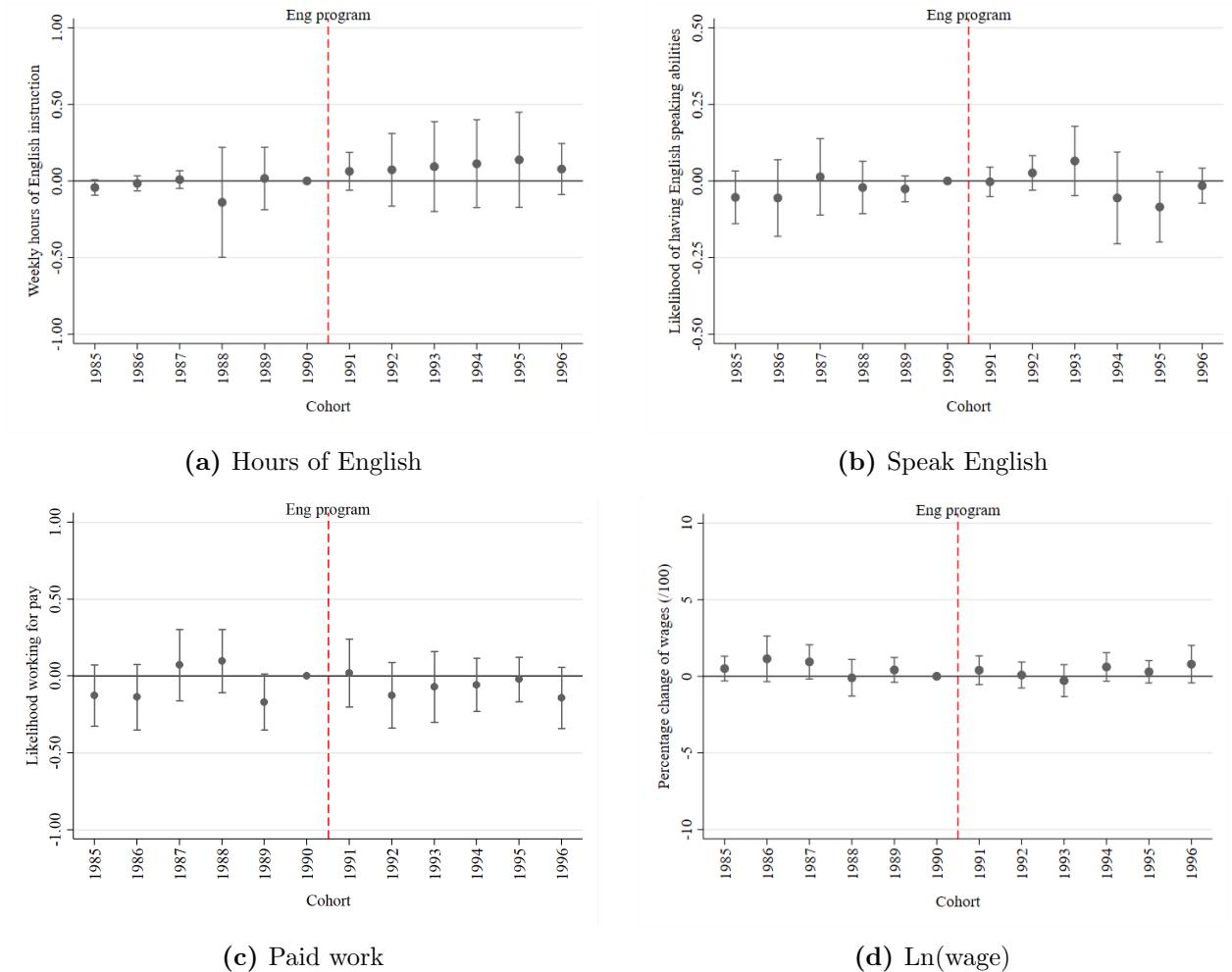
Note: Plotted estimates represent the interaction terms between the treatment variable, and an indicator function for each cohort (1986-1995) in an event study type regression. The omitted cohort is 1989. The vertical dotted line indicates the introduction of the state English program in Aguascalientes. The no statistically significant estimates at the left of the vertical dotted line suggest parallel trends before the policy implementation.

Figure A.7: Pre-trends test for Coahuila



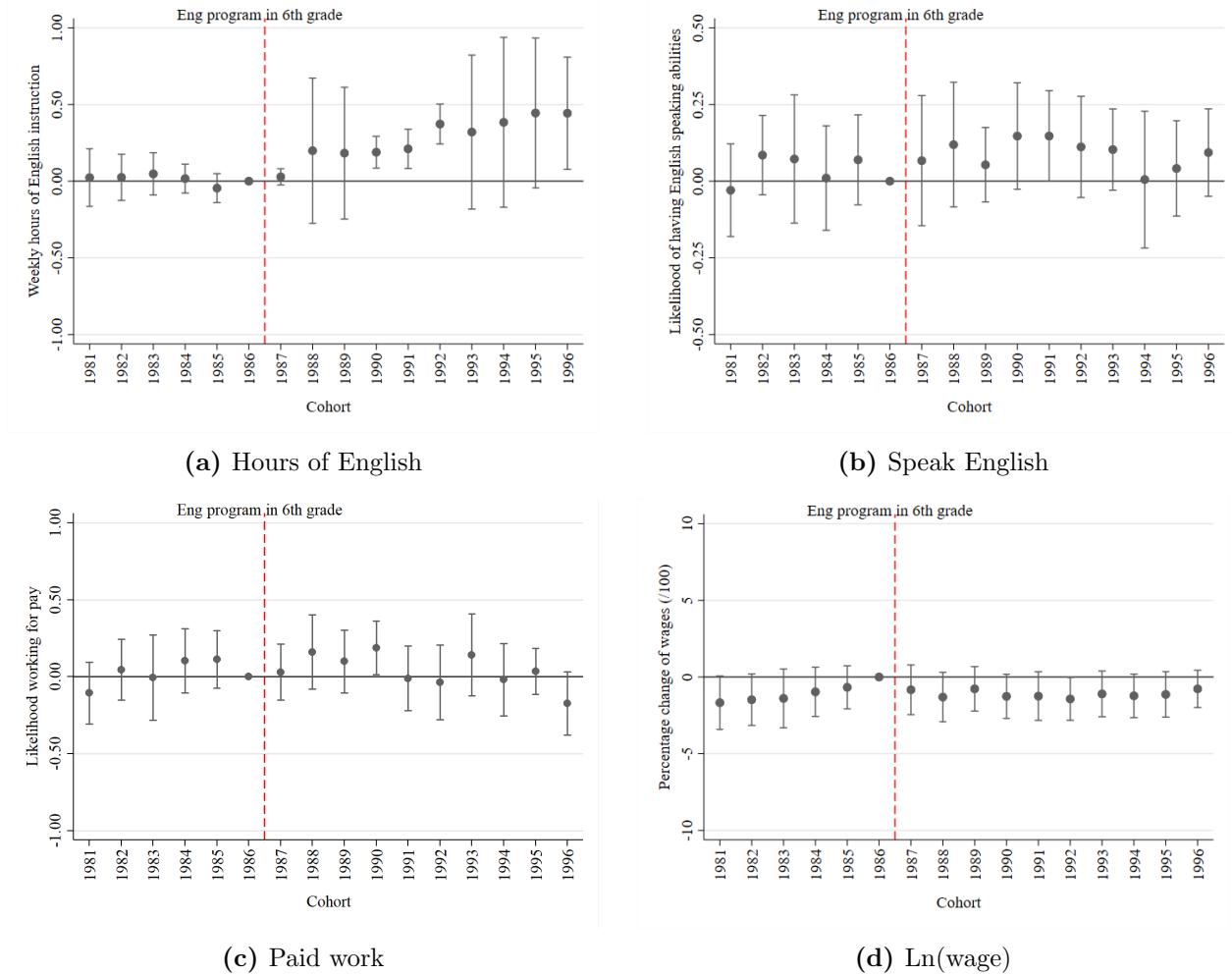
Note: Plotted estimates represent the interaction terms between the treatment variable and an indicator function for each cohort (1986-1995) in an event study type regression. The omitted cohort is 1989. The vertical dotted line indicates the introduction of the state English program in Coahuila. The no statistically significant estimates at the left of the vertical dotted line suggest parallel trends before the policy implementation.

Figure A.8: Pre-trends test for Durango



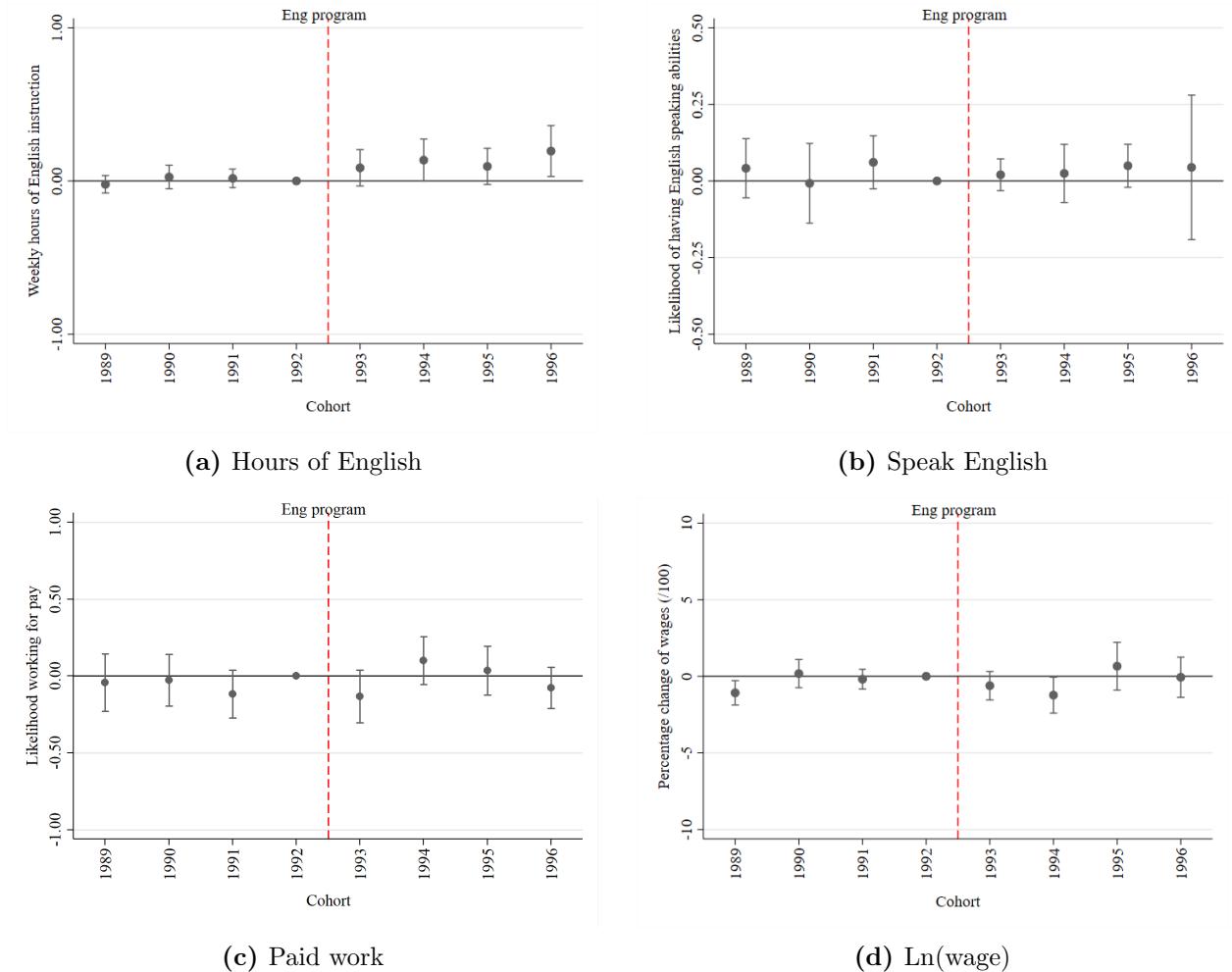
Note: Plotted estimates represent the interaction terms between the treatment variable and an indicator function for each cohort (1985-1996) in an event study type regression. The omitted cohort is 1990. The vertical dotted line indicates the introduction of the state English program in Durango. The no statistically significant estimates at the left of the vertical dotted line suggest parallel trends before the policy implementation.

Figure A.9: Pre-trends test for Nuevo Leon



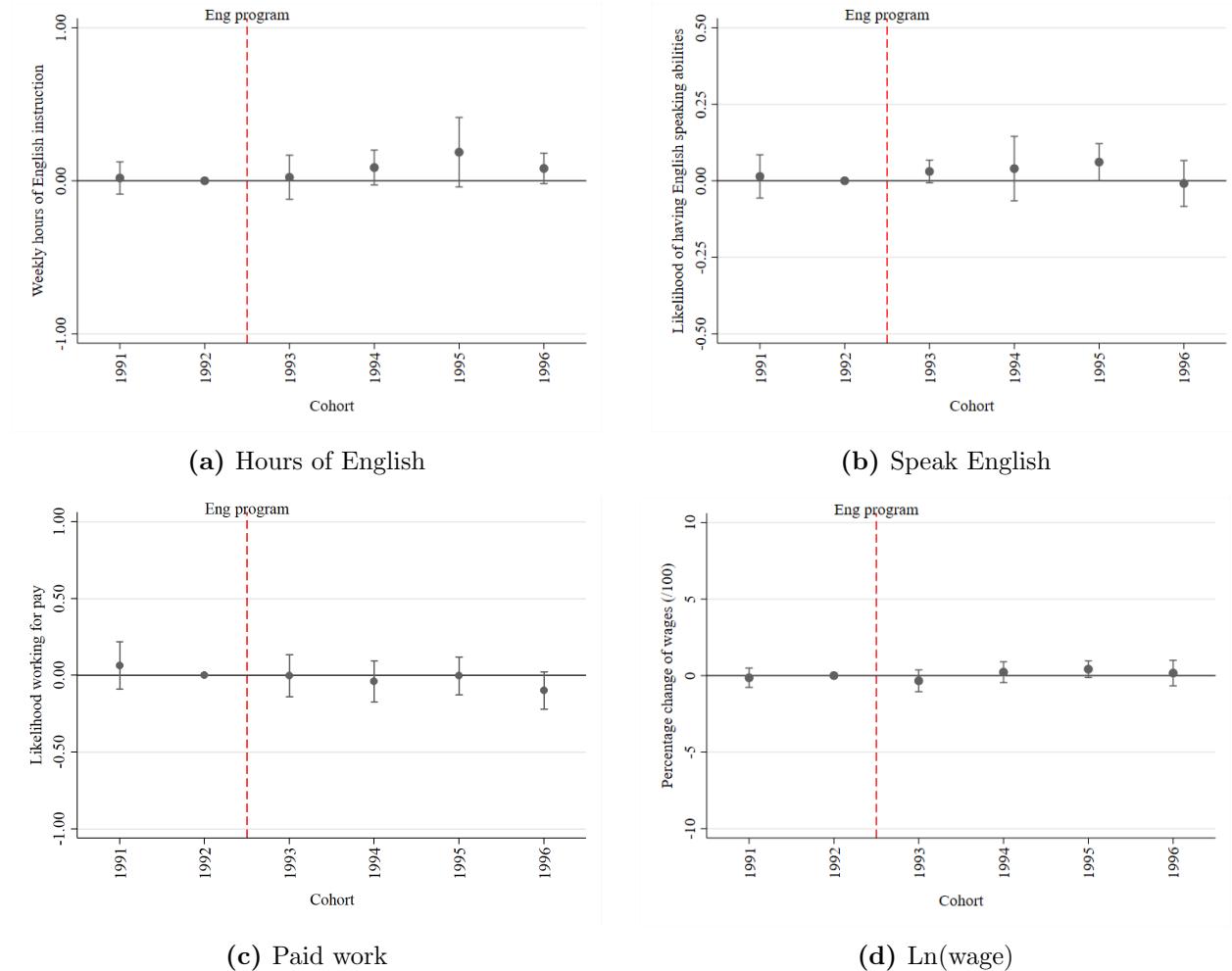
Note: Plotted estimates represent the interaction terms between the treatment variable and an indicator function for each cohort (1981-1996) in an event study type regression. The omitted cohort is 1986. The vertical dotted line indicates the introduction of the state English program in Nuevo Leon. The no statistically significant estimates at the left of the vertical dotted line suggest parallel trends before the policy implementation.

Figure A.10: Pre-trends test for Sinaloa



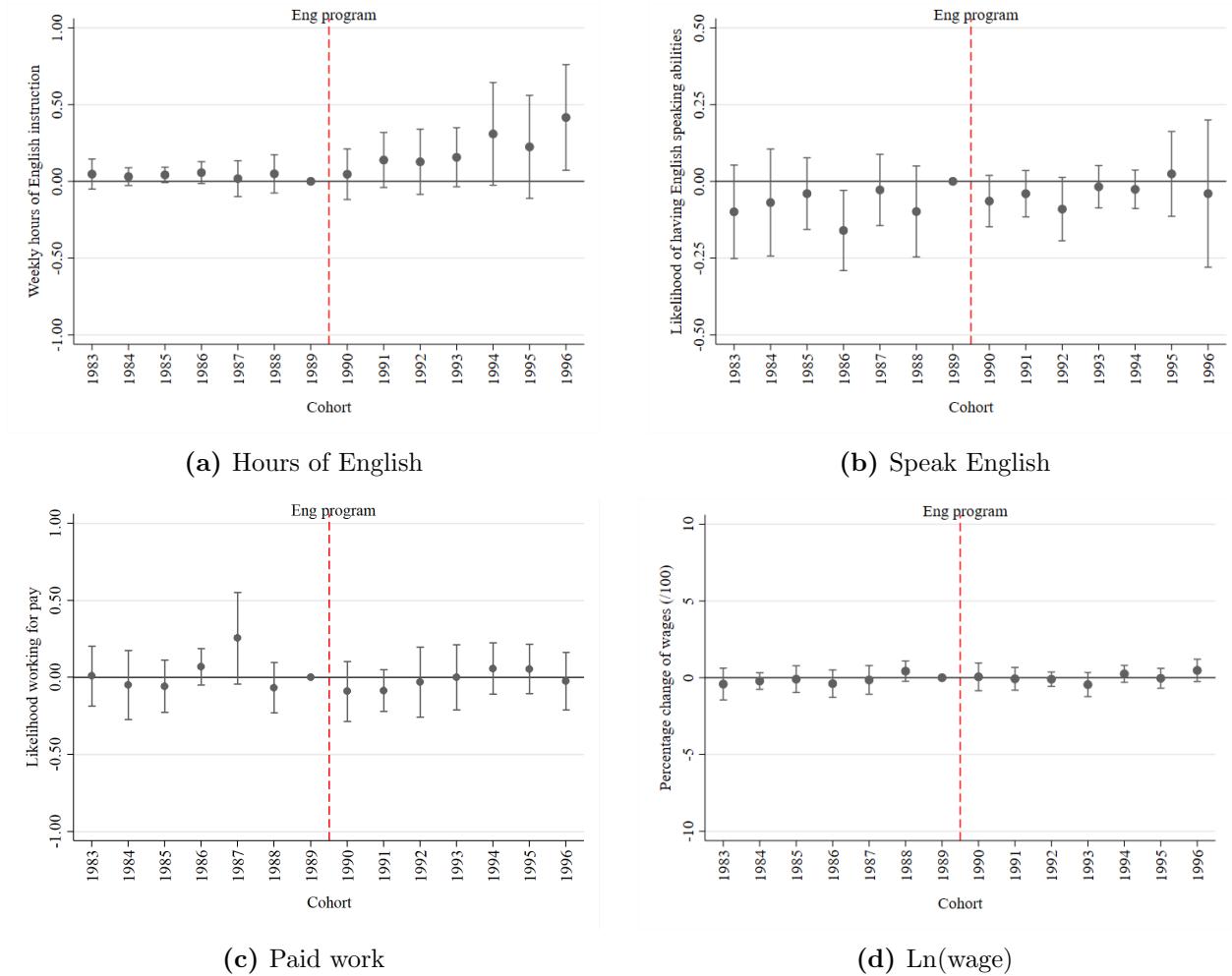
Note: Plotted estimates represent the interaction terms between the treatment variable and an indicator function for each cohort (1989-1996) in an event study type regression. The omitted cohort is 1991. The vertical dotted line indicates the introduction of the state English program in Sinaloa. The no statistically significant estimates at the left of the vertical dotted line suggest parallel trends before the policy implementation.

Figure A.11: Pre-trends test for Sonora



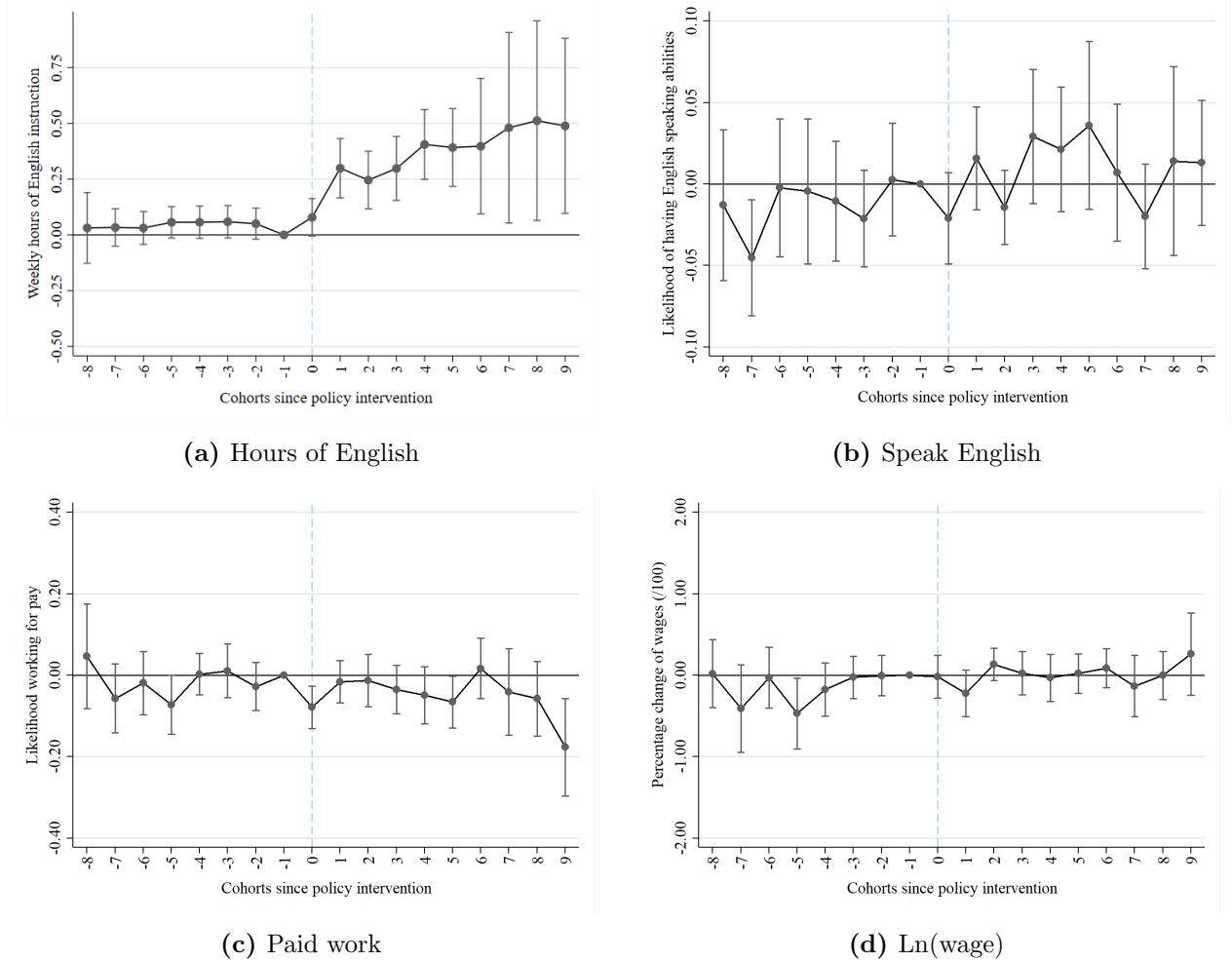
Note: Plotted estimates represent the interaction terms between the treatment variable and an indicator function for each cohort (1989-1996) in an event study type regression. The omitted cohort is 1991. The vertical dotted line indicates the introduction of the state English program in Sonora. The no statistically significant estimates at the left of the vertical dotted line suggest parallel trends before the policy implementation.

Figure A.12: Pre-trends test for Tamaulipas



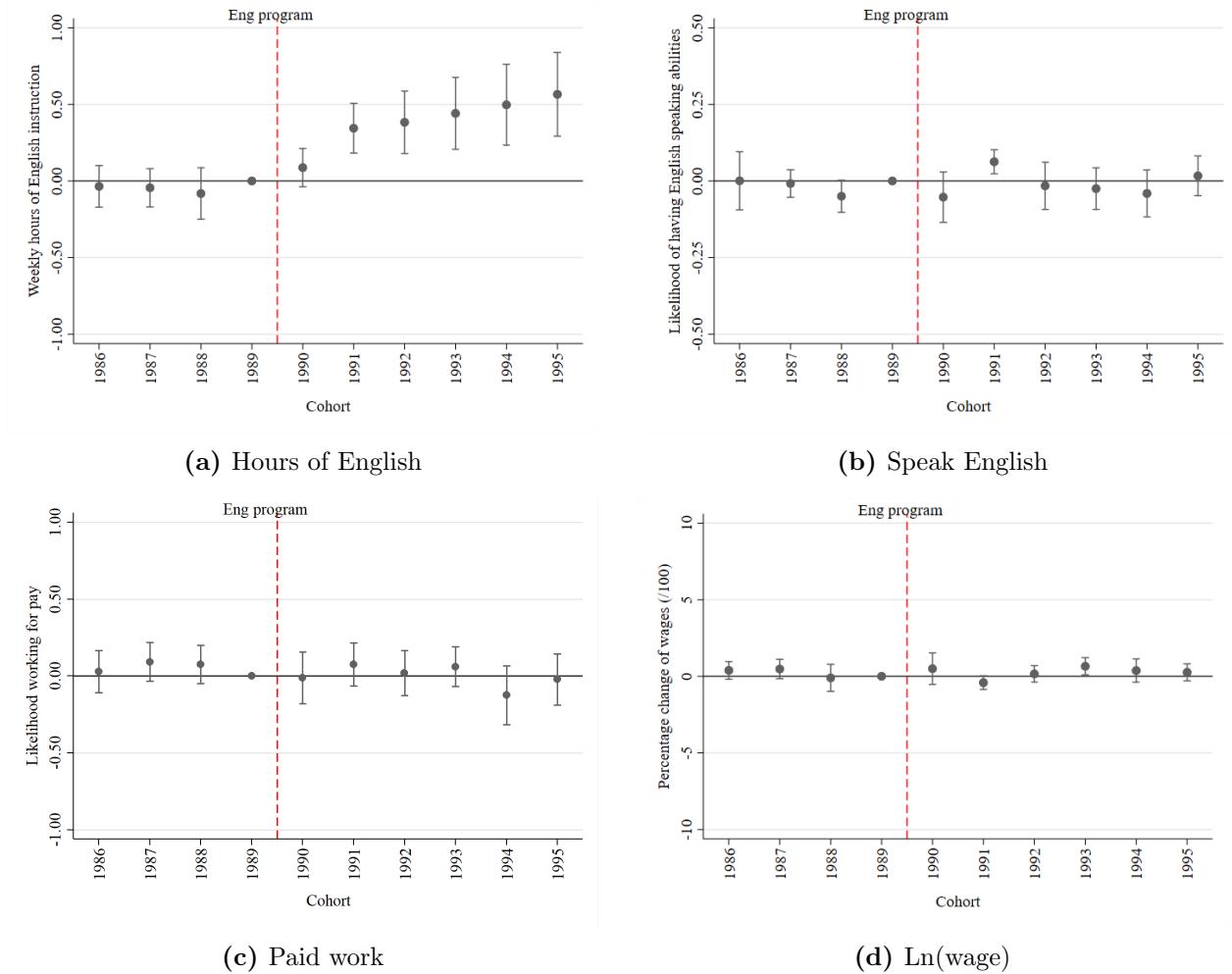
Note: Plotted estimates represent the interaction terms between the treatment variable and an indicator function for each cohort (1983-1996) in an event study type regression. The omitted cohort is 1991. The vertical dotted line indicates the introduction of the state English program in Tamaulipas. The no statistically significant estimates at the left of the vertical dotted line suggest parallel trends before the policy implementation.

Figure A.13: Pre-trends test pooling all states (SDD estimate)



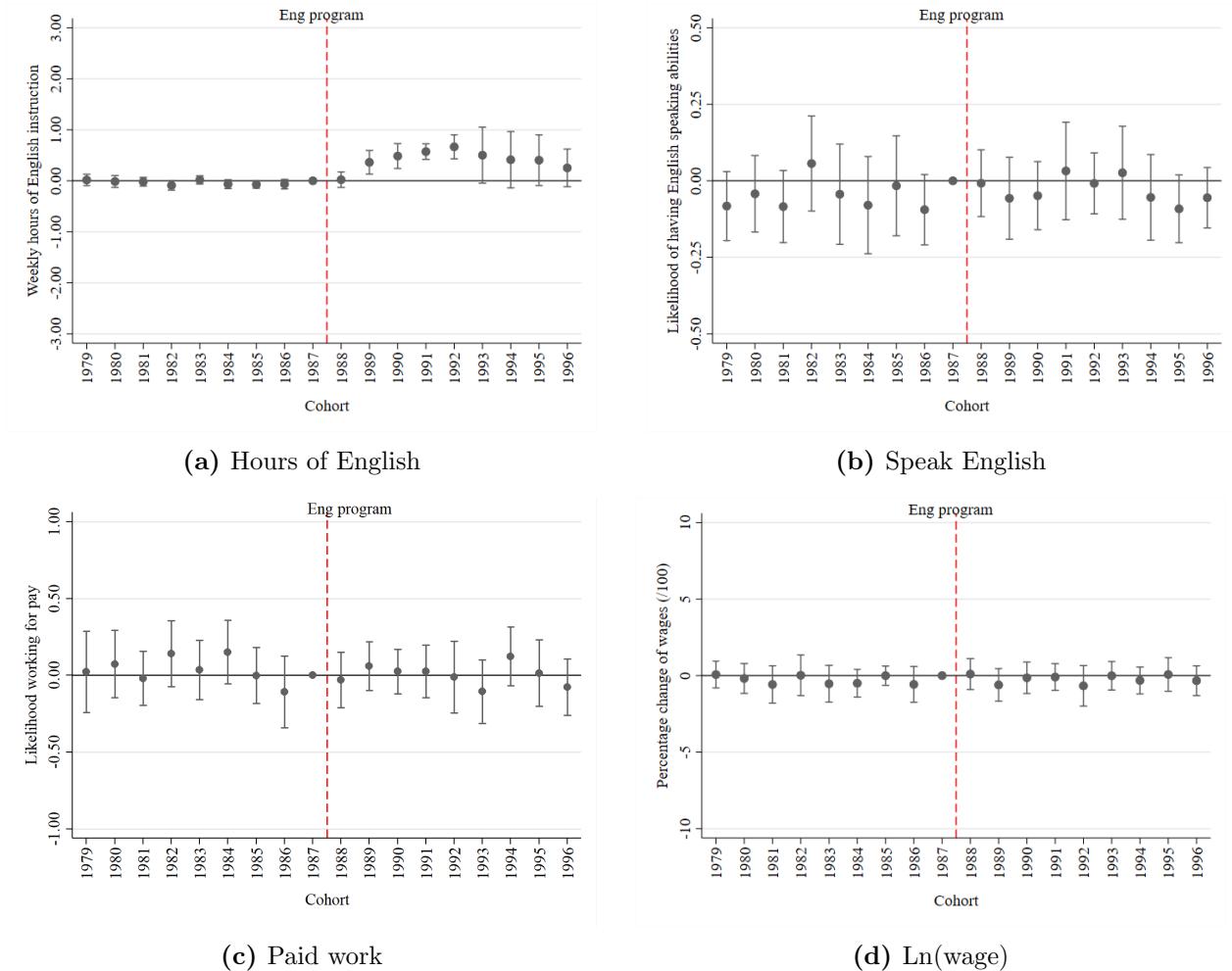
Note: Plotted estimates represent the interaction terms between the treatment variable and an indicator function for each cohort since the policy intervention, in an event study type regression. The omitted cohort is one year before the state English programs. The vertical dotted lines indicate the moment of the intervention. The no statistically significant estimates at the left of the vertical dotted line suggest parallel trends before the policy.

Figure A.14: Pre-trends test for Aguascalientes (multiple comparison groups)



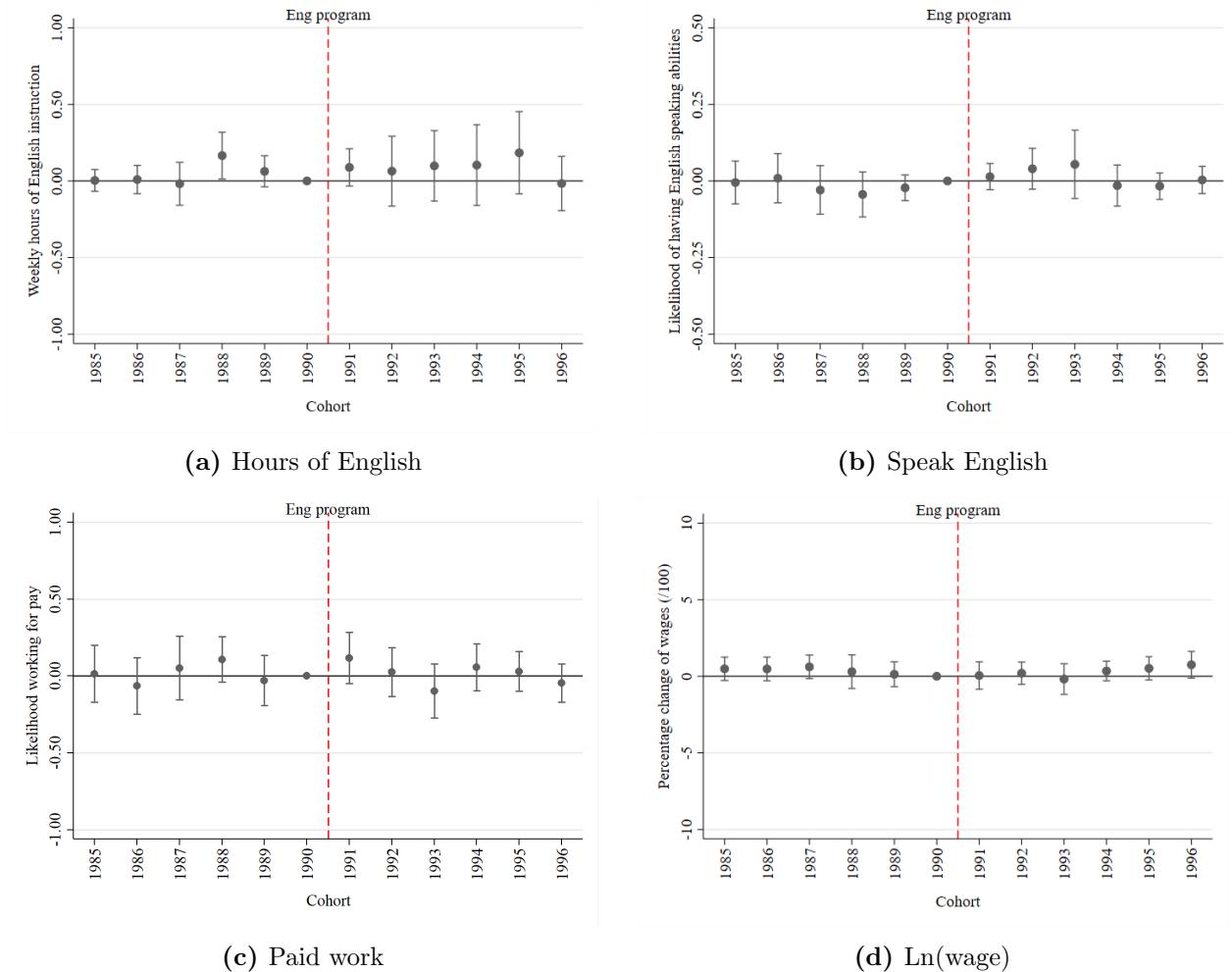
Note: Plotted estimates represent the interaction terms between the treatment variable and an indicator function for each cohort (1986-1995) in an event study type regression. The omitted cohort is 1989. The vertical dotted line indicates the introduction of the state English program in Aguascalientes. The no statistically significant estimates at the left of the vertical dotted line suggest parallel trends before the policy implementation.

Figure A.15: Pre-trends test for Coahuila (multiple comparison groups)



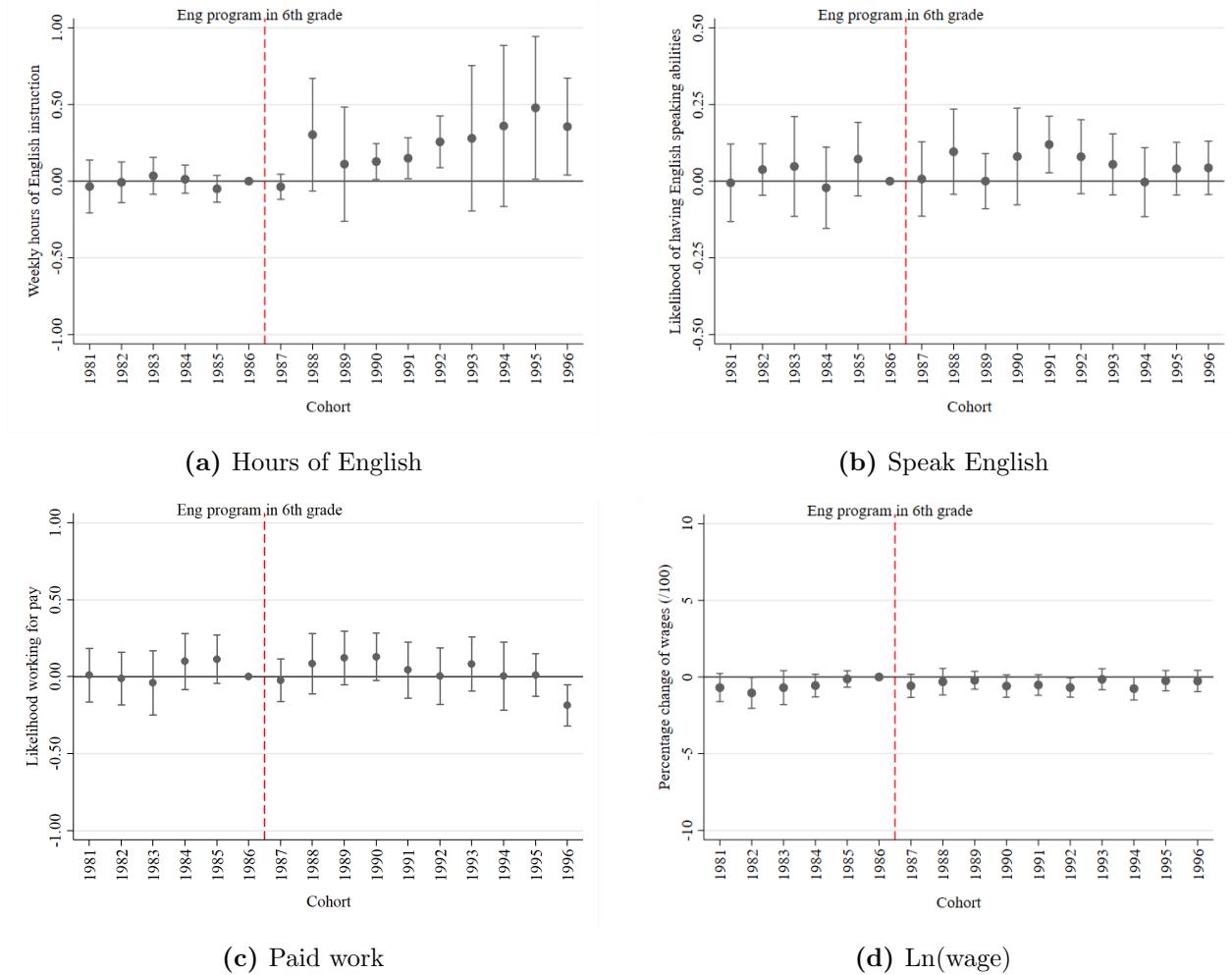
Note: Plotted estimates represent the interaction terms between the treatment variable and an indicator function for each cohort (1986-1995) in an event study type regression. The omitted cohort is 1989. The vertical dotted line indicates the introduction of the state English program in Coahuila. The no statistically significant estimates at the left of the vertical dotted line suggest parallel trends before the policy implementation.

Figure A.16: Pre-trends test for Durango (multiple comparison groups)



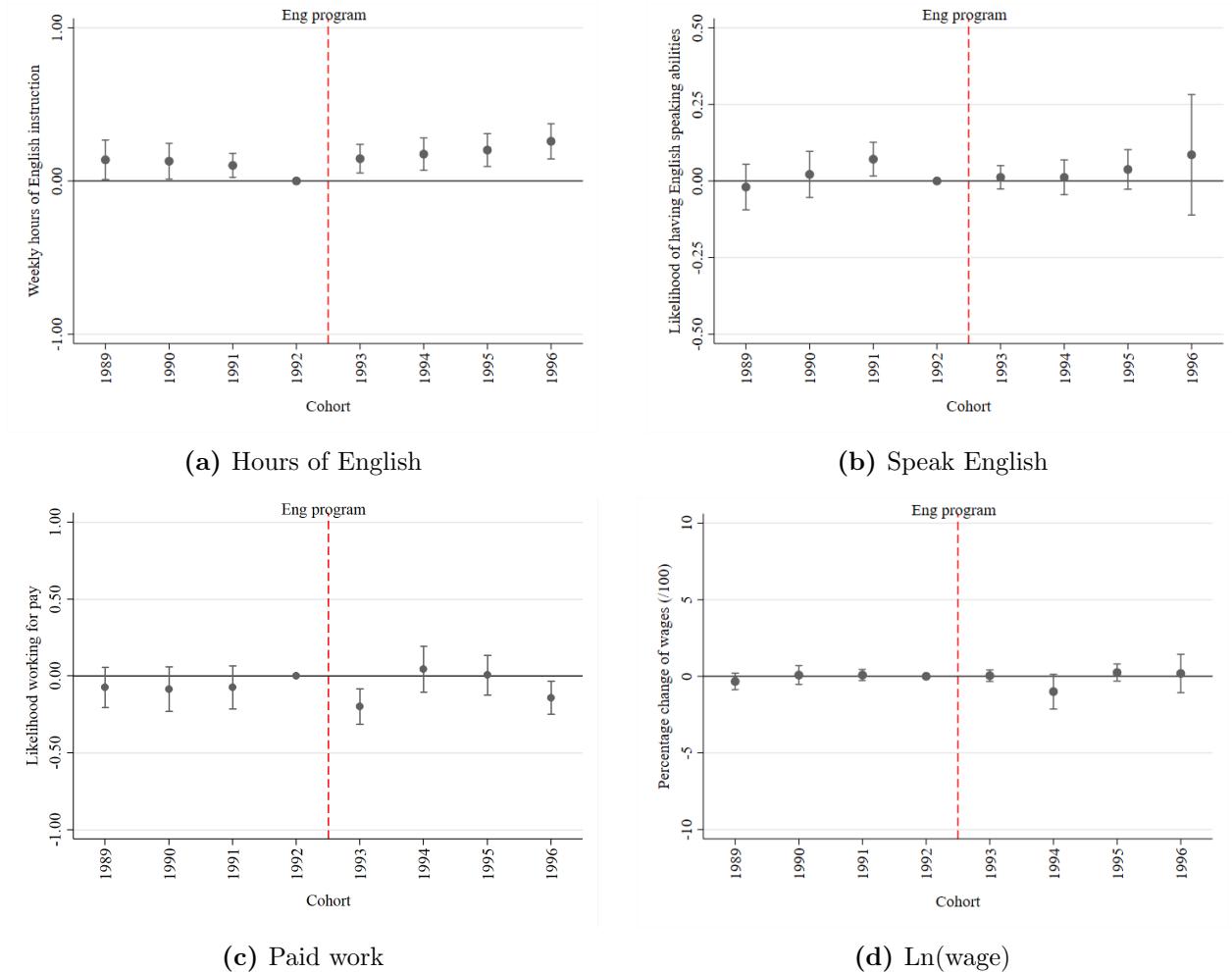
Note: Plotted estimates represent the interaction terms between the treatment variable and an indicator function for each cohort (1985-1996) in an event study type regression. The omitted cohort is 1990. The vertical dotted line indicates the introduction of the state English program in Durango. The no statistically significant estimates at the left of the vertical dotted line suggest parallel trends before the policy implementation.

Figure A.17: Pre-trends test for Nuevo Leon (multiple comparison groups)



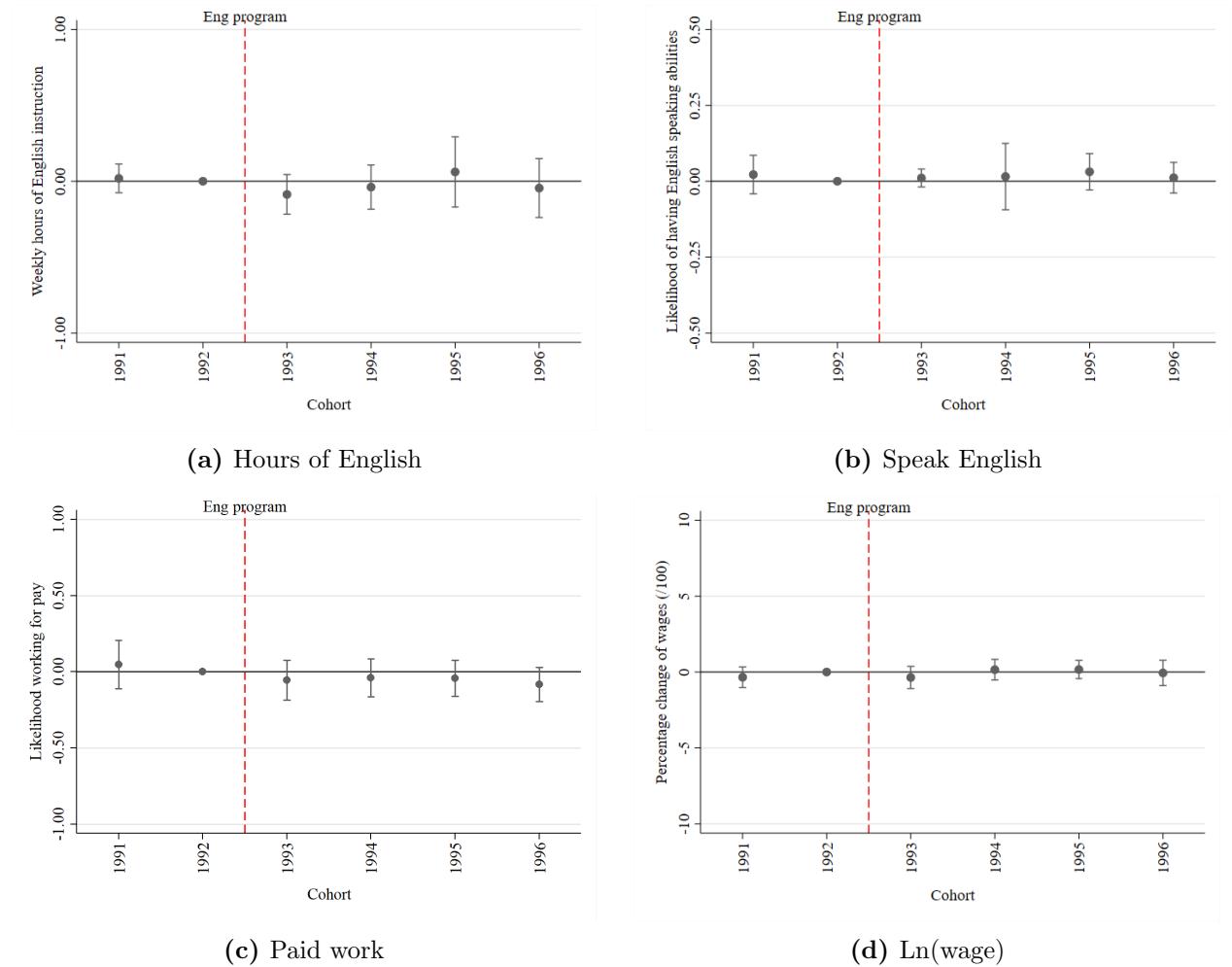
Note: Plotted estimates represent the interaction terms between the treatment variable and an indicator function for each cohort (1981-1996) in an event study type regression. The omitted cohort is 1986. The vertical dotted line indicates the introduction of the state English program in Nuevo Leon. The no statistically significant estimates at the left of the vertical dotted line suggest parallel trends before the policy implementation.

Figure A.18: Pre-trends test for Sinaloa (multiple comparison groups)



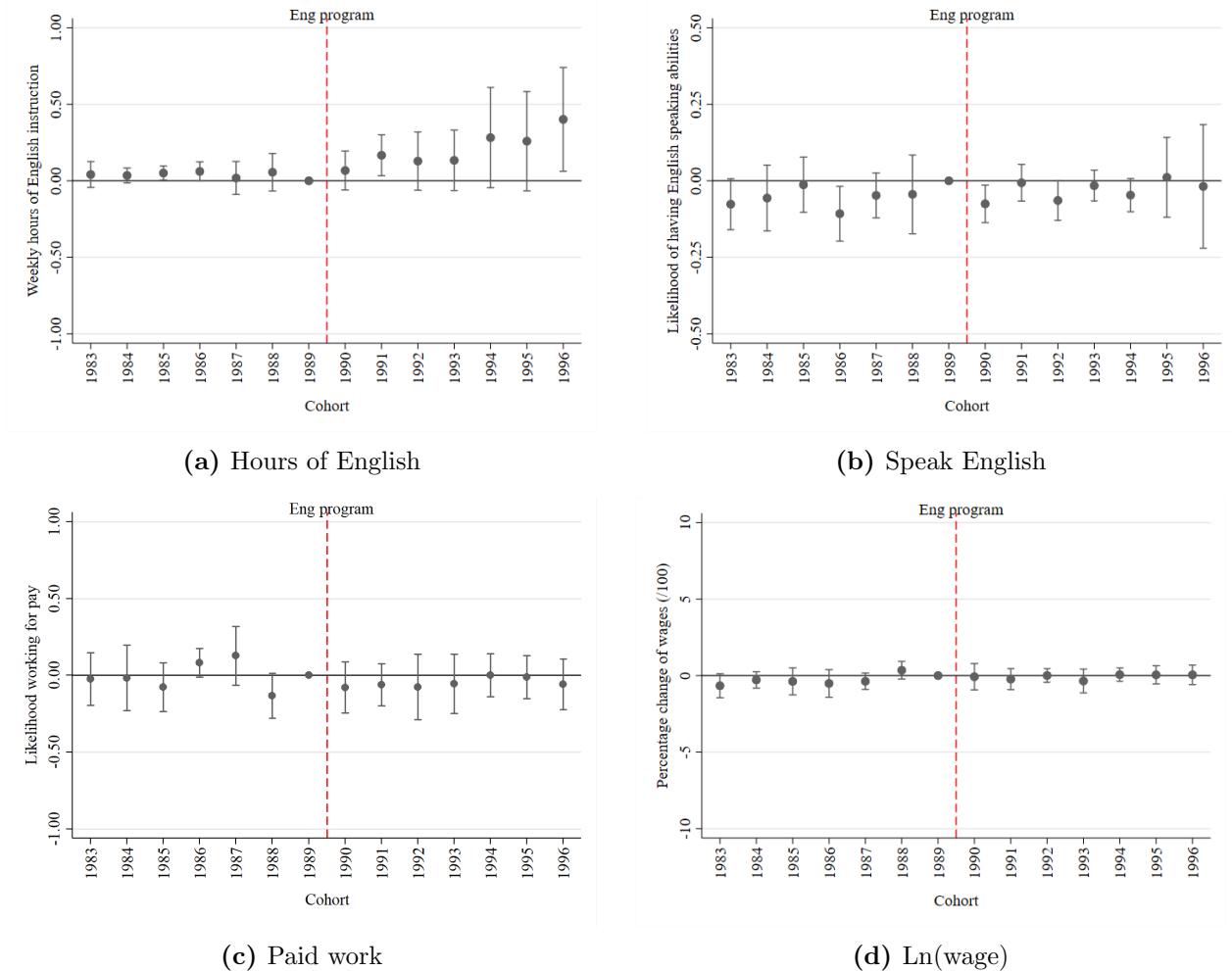
Note: Plotted estimates represent the interaction terms between the treatment variable and an indicator function for each cohort (1989-1996) in an event study type regression. The omitted cohort is 1991. The vertical dotted line indicates the introduction of the state English program in Sinaloa. The no statistically significant estimates at the left of the vertical dotted line suggest parallel trends before the policy implementation.

Figure A.19: Pre-trends test for Sonora (multiple comparison groups)



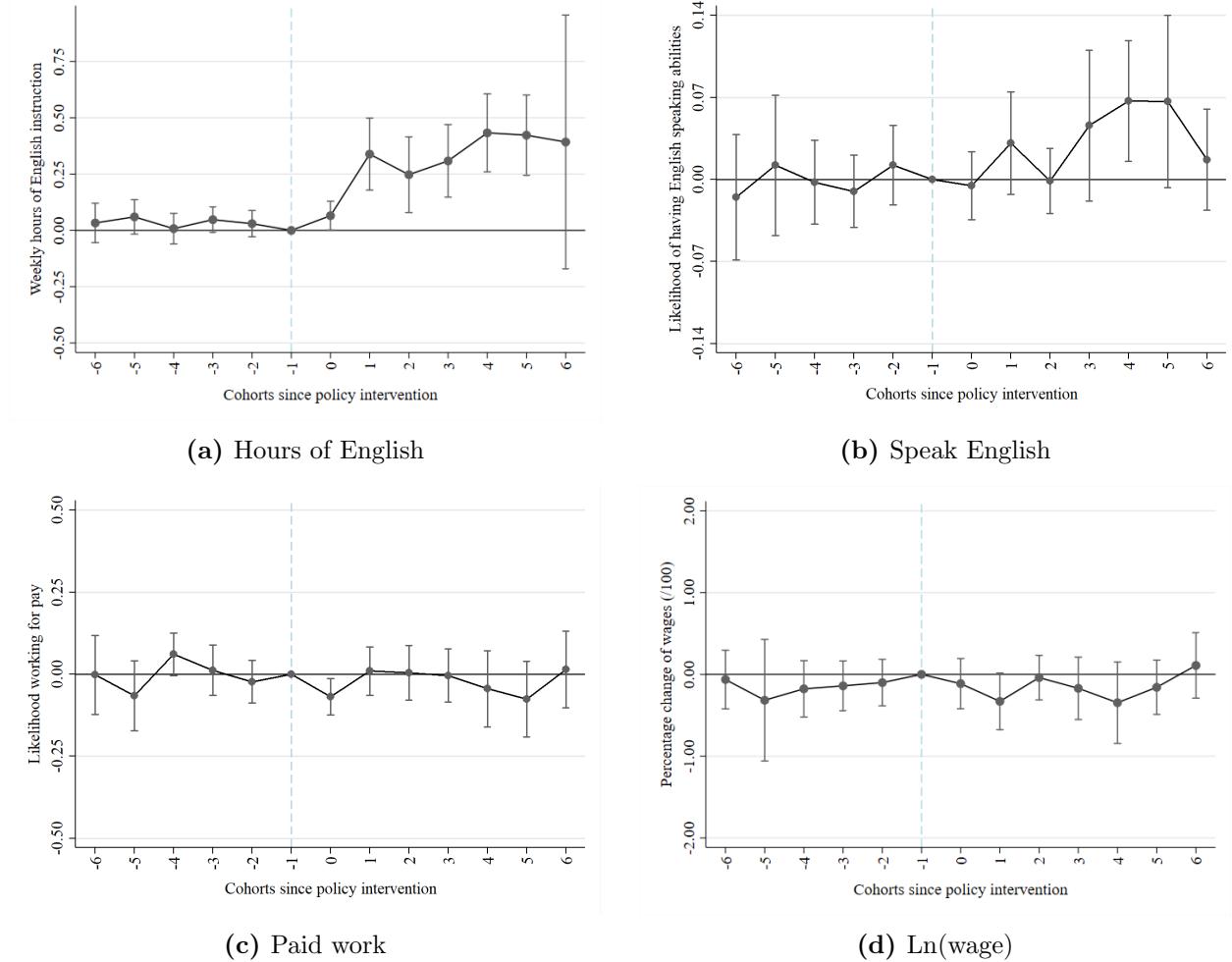
Note: Plotted estimates represent the interaction terms between the treatment variable and an indicator function for each cohort (1989-1996) in an event study type regression. The omitted cohort is 1991. The vertical dotted line indicates the introduction of the state English program in Sonora. The no statistically significant estimates at the left of the vertical dotted line suggest parallel trends before the policy implementation.

Figure A.20: Pre-trends test for Tamaulipas (multiple comparison groups)



Note: Plotted estimates represent the interaction terms between the treatment variable and an indicator function for each cohort (1983-1996) in an event study type regression. The omitted cohort is 1991. The vertical dotted line indicates the introduction of the state English program in Tamaulipas. The no statistically significant estimates at the left of the vertical dotted line suggest parallel trends before the policy implementation.

Figure A.21: Pre-trends test pooling all states (SDD estimate with a narrower comparison group)



Note: Plotted estimates represent the interaction terms between the treatment variable and an indicator function for each cohort since the policy intervention, in an event study type regression. The omitted cohort is one year before the state English programs. The vertical dotted line indicate the moment of the intervention. The no statistically significant estimates at the left of the vertical dotted line suggest parallel trends before the policy.