# Foreign language skills and labor market outcomes: The case of English in Mexico*

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**Abstract**

In this paper, I study the effect of English instruction on labor market outcomes 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 address concerns regarding endogeneity in the relationship between English skills and labor market outcomes, I exploit policy changes in various Mexican states that introduced English instruction (as a subject) in public elementary schools during the 1990s. Using a Two-Way Fixed Effects specification, I estimate the effect of these state policies introducing English instruction on labor market outcomes. I offer robust estimates in the presence of heterogeneous treatment effects due to variation in the treatment over time and across treated regions. My findings indicate that these policies offering English instruction at elementary school levels increased the likelihood of speaking English, school enrollment, and labor supply. However, the increase in labor supply was mainly driven by women and individuals with low educational attainment, which was perceived in the market as a small improvement in the marginal product of labor, leaving wages unaffected. Consistently, there is a generalized negative effect on women's subjective well-being.

**JEL Classification:** I21, I28, J24, J31.
**Keywords:** Primary School, Education Reform, Skills, Occupational Choice, Labor Income.

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# Introduction

Given the use of English as a *lingua franca* in the global economy, there could be an economic value to English skills in non-English speaking countries. 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 in the last three decades. This expansion has been commonly motivated by the assumption that English instruction will enhance English language skills and, ultimately, improve labor market outcomes. In this paper, I explore the case of English instruction in Mexico and its casual effects on wages and occupational decisions. The case of English in Mexico is particularly interesting for its proximity to the United States (US).

However, most existing research has focused on the context of immigrants in English-speaking countries. For example, previous studies highlight the positive association between English language skills and immigrants' earnings in the US (see Isphording (2014) and Chiswick and Miller (2015) for a review). 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 observed that the English premium among immigrants could vary depending on workers' age and education; younger and more educated immigrants tend to have greater returns to English at work (Lang and Siniver, 2009; Azam, Chin and Prakash, 2013).

Furthermore, evidence on the returns to English skills is scant in the context of non-English speaking countries and particularly rare in Latin American economies, where English is not the predominant language of communication. For example, there is evidence suggesting a positive correlation between English skills and earnings, in the context of former British colonies. Indeed, Azam, Chin and Prakash (2013) studied the case of India and Eriksson (2014) the case of South Africa. A recent wave of literature has offered descriptive and causal estimates on the returns to English language skills and labor force participation in the context of European and Asian countries. This growing literature has regularly found positive returns to English skills (Adamchik et al., 2019; Liwiński, 2019; Hahm and Gazzola, 2022; Zhang and Lien, 2020) and an increase in labor force participation among English speakers (Gazzola and Mazzacani, 2019). To my knowledge, aside from the context of Mexico, there are no studies in the context of Latin American countries.

In addition, much remains unknown about the effectiveness of exposure to English instruction on the acquisition of English abilities and the role of these abilities on earnings. Angrist, Chin and Godoy (2008) offered the first evidence that a change in the medium of instruction (from English to Spanish) may not affect the English proficiency of Puerto Ricans. In contrast, Eriksson (2014) found a positive effect on English proficiency when the medium of instruction shifted from Afrikaans to English. Notice, however, that the majority of non-English-speaking countries globally have introduced English as a subject rather than as the medium of instruction. For instance, the first study to exploit a policy change in exposure to English instruction (as a subject), finds a positive association between the probability of exposure to English instruction and wages (Chakraborty and Bakshi, 2016) but does not provide evidence on the acquisition of English skills. Likewise, although Gálvez-Soriano (2023)

has a more robust measure of exposure (in weekly hours of English instruction), he cannot provide causal evidence that exposure leads to the formation of English abilities due to a lack of necessary data. Instead, he offers evidence that exposure does not affect other cognitive skills, suggesting that his findings are consistent with the acquisition of English abilities.

To address these gaps in the literature, I explore the following research question: Can English programs improve labor market outcomes in the context of a non-English-speaking country? The context of my study is Mexico, an economy that primarily uses Spanish as its main language of communication. Furthermore, the significance of English in Mexico could be even more pronounced than in other developing countries due to its close relationship with the US in terms of trade and migration. Additionally, the external validity of my context is potentially broader than in the case of India or South Africa, which differ significantly from most developing countries as former British colonies where English abilities are often prevalent among the upper social class and government workers.

To answer my research question, I leverage individual-level data from the 2014 Subjective Well-being Survey, a dataset unique in including a measure of English proficiency, unlike other large nationally representative datasets. Specifically, respondents are queried about their English-speaking ability. Despite having a measure of English proficiency, estimating the causal effect of English skills remains challenging. The difference in labor market outcomes between individuals who speak English and those who do not cannot be interpreted as the causal effect of English skills due to potential selection biases in those who speak English. English-speaking ability is likely correlated with other variables influencing labor market outcomes. To estimate the causal effect, I exploit the implementation/expansion of six state English programs in Mexico. In my identification strategy, I employ locality-by-cohort variation in exposure to English instruction (driven by state policies since the early 1990s) within a Two-Way Fixed Effects (TWFE) framework. Subsequently, I explore occupational choices to better understand the mechanisms behind the effect on wages.

Hence, in this paper, I provide the first empirical evidence that exposure to English instruction may lead to the formation of English skills in Mexico. The TWFE estimate suggests that these English programs increase the likelihood of reporting speaking English by around 12.4 percentage points. The average size of the intervention I study is relatively small. It increased exposure to English instruction, on average, by almost 30 minutes per week. Scaling this up, it would suggest that one additional hour of English instruction per week would increase the likelihood of speaking English by 25 percentage points.

Furthermore, I present some of the first causal estimates on the impact of English programs on labor market outcomes in the context of a non-English-speaking country. The labor market outcomes I examine include the likelihood of working for pay, wages, and occupational choices. I find that the average intervention does not affect wages. This zero effect on wages is consistent with an increase in labor demand (associated with the improvement in the marginal product of labor) offset by an expansion in labor supply. Finally, I provide evidence that programs offering English instruction at school raised school enrollment. This finding suggests that the long-term effects of the English programs may positively influence wages through an improvement in schooling.

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 of 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) estimate the returns to English skills in Mexico using the same data set that I use in this paper. However, they do not address the concern about the endogeneity of English skills beyond controlling for some observable characteristics. Finally, in a companion paper, I provide the first empirical evidence that exposure to English instruction does not affect the wages of the mean worker in the Mexican formal sector. Nevertheless, I do find a positive and significant effect on wages only among high-achieving individuals (see Gálvez-Soriano (2023)).

With this paper, I contribute to the existing literature in three main dimensions. First, I provide a causal effect of English instruction on wages instead of solely looking at the returns to English skills, thus, offering an estimate of interest for policymakers. Second, I am the first one to study the effect of interventions offering English instruction at school on other labor market outcomes in the context of a developing, non-English-speaking country. Finally, I provide the first robust evidence on the acquisition of English skills as a consequence of exposure to English instruction in the context of a non-English speaking country.

The remaining of this paper proceeds as follows. In the first section, I offer a brief description of the English abilities in Mexico along with the background of the policy changes I exploit in this paper. In section 2, I describe the database I use. In section 3, I explain the empirical strategy. In section 4, 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 labor market outcomes. In this same section, I also provide robustness checks and analyze the potential mechanisms. Finally, section 5 summarizes with a discussion of my findings and a brief conclusion.

# 1 English language skills in Mexico

## 1.1 Prevalence of English skills

The primary language spoken in Mexico is Spanish, and all official documents are written in this language. As of 2014, approximately 7% of the Mexican adult population could speak English (see Table A.2). Similarly, data from the 2020 population census suggests that 6.1% of Mexicans declared being able to speak an indigenous language. In a preliminary descriptive analysis, I observed a positive correlation between English instruction and English abilities in Mexico. Through a regional comparison, I find that states implementing English programs in primary schools have a higher proportion of individuals with English skills than other states.[1] Thus, it could be inferred that exposure to English instruction may facilitate the acquisition of English skills. In the appendix, I detail the findings of my descriptive analysis, which includes four main results on the prevalence of English skills in Mexico. First, there

[1]States with English programs include Aguascalientes, Durango, Nuevo Leon, Sinaloa, Sonora, and Tamaulipas. While some other Mexican states have offered English instruction in public primary schools, it has been irregular, with only a few beneficiary schools and/or no expansion over time.

exists a gender-English gap, with more men declaring proficiency in English than women. Second, increased exposure to English instruction correlates with enhanced English skills. Third, English proficiency rises with educational attainment. Fourth, indigenous people lag in the process of learning English, and similarly, rural areas trail in this learning process.

In terms of geographical location, the population with English abilities is concentrated in six Mexican states: Aguascalientes, Baja California, Baja California Sur, Jalisco, Queretaro, and Quintana Roo. The less industrialized Mexican states (South and Southeast regions) have a small share of English speakers. English-speaking individuals are primarily found in urban areas. The states with more exposure to English instruction are not necessarily those with more English speakers. Finally, administrative records suggest that six out of 32 Mexican states consistently offered English instruction in public primary schools during the early 1990s. In the appendix, I also provide a detailed description of the geographical and occupational heterogeneity in the distribution of English speakers in Mexico.

## 1.2 Policy change: English programs in Mexican states

Since the early 1990s, several 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. In particular, English instruction in public primary schools is meant to improve the acquisition of English skills to facilitate labor mobility from Mexico to the US and Canada. Before launching these state English programs, only private schools offered English instruction, leaving more than 90% of the population unattended, which could potentially benefit from NAFTA. As noted in the previous subsection, using administrative data from the Mexican school census, I identified six states that have implemented/expanded English programs in public primary schools, consistently increasing the hours of English instruction offered in these schools. Hence, in this section, I will describe how the implementation occurred in most of these states. However, some of them do not have a publicly available registry of their English programs, implying 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, initially benefiting only high-achieving students of fourth, fifth, and sixth grades in one hundred randomly chosen elementary schools.[2] However, it was not 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 expansion is one of the focuses of exploration in this paper. Over time, the state government increased the English program's coverage among elementary schools, including preschools. For instance, in 2008, the program expanded coverage to students in fifth grade in schools already benefiting from the program in sixth grade. By 2008, the state English program covered approximately

[2]Elementary 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.

60% of all elementary schools in Nuevo Leon.[3]

The state of Sonora also launched its English program in the year 1993 as a trial stage, but it was not 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).

Then, the state of Tamaulipas launched an English program in the 2001-2002 school year, initially offering English instruction to the fourth grade of urban primary schools in the state, benefiting 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 preschools in the state incorporated into the English program.[4] The Tamaulipas policy change I exploit in this paper is the first implementation of 2001.

As for the remaining states—Aguascalientes, Durango, and Sinaloa—no official sources indicate the process of the 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.3). 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 20% coverage of the students enrolled in Durango primary schools.

# 2 Data

My primary 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 levels. In 2014, this survey was conducted as part of the Mexican Household Income and Expenditure Survey (ENIGH) and served as an annex to the Socioeconomic Conditions Module. This arrangement allows for variables characterizing the socioeconomic conditions of individuals and households to coexist in the same database, currently employed to measure poverty in Mexico.

The 2014 BIARE round is notable as it included a one-time inquiry into individuals' English-speaking abilities. Respondents of this survey are adults aged 18 years and older, with the survey focusing on one adult per household. Consequently, my sample size is smaller than that in the ENIGH 2014 for the same age range. BIARE survey interviews are all conducted face-to-face, requiring information to be provided exclusively by the respondent, not through a third party. Respondents are asked a set of questions concerning their demographic and economic characteristics, perceived well-being, and their ability to speak English.

[3] All this information is publicly available on the Nuevo Leon English program's website.

[4] All this information is publicly available on the Tamaulipas English program's website.

The second source of information I utilize is the Mexican school census, also known as Statistics 911. This census enables the identification of public schools that have offered English instruction in Mexico. This information is crucial for constructing a variable representing exposure to English instruction, allowing me to gauge the extent of state English programs implemented in Mexico since the early 1990s. To construct this exposure variable, I exclusively consider public elementary schools in the morning shift, as afternoon shifts are unstable, with the same school potentially not providing both shifts every academic year.

I measure 'exposure' using the average hours of English instruction per week, by cohort and by locality.[5] The hours of English instruction are measured as the ratio of total weekly hours of English instruction to the total number of classes per school. For each school-cohort, I calculate the average hours over the six years comprising primary school in Mexico. Finally, by cohort, I determine the average hours of English instruction 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 primary schools in Mexico do not offer English instruction.

I link my exposure variable to the BIARE database by cohort and locality. For example, in the BIARE database, the 1996 birth cohort attended sixth grade (the final grade of primary school in Mexico) in 2007. Hence, the data I constructed for 2007 using the Mexican school census includes the average exposure from 2002 to 2007 for the cohort 1996. Since I impute the average exposure at the locality level, my estimate of the policy change on exposure could be understated because this exposure variable includes schools with zero hours of English instruction. Nevertheless, this exposure measure remains informative. Furthermore, the final database I built in this paper is the first (in Mexico) to include both variables: exposure to English instruction and English skills.

I also explore the effect of English programs on occupational choices as a potential mechanism that mediates the impact on wages. In this part of the analysis, I use the O*NET classification of occupations as it provides detailed information on the tasks required by each job. I started using the 2011 Mexican System of Classification of Occupations (SINCO, for its acronym in Spanish), as it is the official classification of occupations in Mexico. Then, I crosswalk the 2011 SINCO with the 2010 Standard Occupational Classification (SOC) that is officially used in the US. The latest O*NET classification provides the crosswalk with the 2018 SOC, so I also worked on the crosswalk of the 2010 SOC and 2018 SOC. Finally, I used the O*NET classification by work activities. In particular, to define physically demanding jobs, I averaged the following two classifications: (i) Handling and Moving Objects, and (ii) Performing General Physical Activities. The other classification I use in this paper is Communicating and Interacting, which I consider as a proxy for jobs requiring communication skills.

As an additional mechanism, I leverage the richness of the BIARE database to examine the impact of English programs on subjective well-being measures. Specifically, I focus on three dimensions: i) satisfaction with economic activity (occupation), ii) satisfaction with the

[5] A 'locality' is the smallest geographically delimited area in Mexico, ranging from a single household in a rural context (*rancheria*) to over 100,000 inhabitants in larger Mexican cities. The consecutive geographical regions by size are counties (municipalities), composed of several localities, and states, which are subdivided by counties.

standard of living, and iii) satisfaction with personal achievements. Respondents were asked to rate their satisfaction level regarding these dimensions on a scale from zero to 10, with 10 indicating maximum satisfaction and zero indicating no satisfaction. The distribution of responses skews towards higher values, so I created dummy variables to represent satisfaction based on a threshold of 9 points. This categorization considers individuals with scores of 9 or 10 as satisfied and those below 9 as unsatisfied.

The final database enables the identification of individuals with English abilities and the hours of English instruction to which they were exposed in primary school. I only consider respondents born between 1984 and 1994 who work for pay. These birth cohorts correspond to the youngest individuals observed in the BIARE survey, including those who may have had exposure to English instruction (younger cohorts) and those who did not have exposure (older cohorts).

A preliminary analysis between Mexican English speakers and non-English speakers (see Table 1) 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 older, and less likely to be indigenous, or married.

Finally, the occupation variables are dummies indicating a value of one if the "importance" of the required skill falls within the top quartile of the occupation distribution. For instance, according to O*NET, economists are not required to engage in physically demanding work or possess communication skills, while lawyers are required to have communication skills. The preliminary descriptive analysis suggests that English speakers are more inclined to work in occupations necessitating communication skills and less inclined to work in physically demanding roles compared to non-English speakers. Furthermore, English speakers are more satisfied with their jobs, their standard of living, and with their personal achievements.

# 3 Empirical strategy

We can model the relationship between English skills, $Eng_{ijc}$, and earnings, $\omega_{ijc}$, using the following equation:

$$\omega_{ijc} = \alpha + \beta\, Eng_{ijc} + \delta_j + \kappa_c + \boldsymbol{X_{ijc}}\boldsymbol{\Pi} + \epsilon_{ijc}, \tag{1}$$

where $\omega_{ijc}$ is the log of wages, the English skills variable, $Eng_{ijc}$, is binary; it takes the value of one if the individual $i$ speaks English and zero otherwise. Each individual $i$ belongs to a cohort $c$ and lives in locality $j$. I fully control for locality fixed effects, $\delta_j$, common cohort effects, $\kappa_c$, and a vector of controls, $\boldsymbol{X_{ijc}}$, with socio-demographic characteristics, such as education and gender.

However, the English skills variable could be endogenous in this wage equation. Two potential sources of endogeneity are: omitted variables and measurement error. First, the omitted variables issue arises from not controlling for unobservable individual characteristics such as abilities, which could be correlated with both English skills and wages. Second, it

is likely that my English skills variable has measurement error as it captures self-reported ability. In this context, an OLS estimation would produce a biased estimate of $\beta$. Hence, instead of exploring the returns to English skills, I study the effect of English policies on labor market outcomes.

A staggered DiD specification, on the other hand, will allow me to offer an estimate of the causal effect of the English policies on labor market outcomes, $y_{ijc}$. Let us define $HadPolicy_{jc}$ as the main effect variable, a dummy that takes the value of one if the individual $i$ lives in a treatment locality, $j$, and belongs to one of the affected cohorts, $c$, while it takes the value of zero otherwise. The reduced form equation is as follows:

$$y_{ijc} = \theta + \psi\, HadPolicy_{jc} + \delta_j + \kappa_c + \mu_{sc} + \boldsymbol{X_{ijc}}\boldsymbol{\Psi} + \varepsilon_{ijc} \tag{2}$$

where $\psi$ measures the effect of English programs on the acquisition of English abilities. As in Equation 1, I fully control for locality fixed effects, $\delta_j$, common cohort effects, $\kappa_c$, and a vector of controls, $\boldsymbol{X_{isc}}$, with demographic characteristics. Hence, this specification could also be categorized as a Two-Way Fixed Effects (TWFE) model.

Notice that in all specifications of this paper, I cluster the standard errors at the locality level, following the recommendation by Abadie et al. (2023). This choice is due to the sampling method used in the BIARE survey, where respondents were sampled from the Mexican population through a clustered sampling process at the locality level. BIARE employs a multi-stage cluster sampling, where a set of primary sampling units (PSUs) are initially randomly selected, followed by the random selection of several households from each PSU. According to Mexico's National Institute of Statistics and Geography (INEGI), the PSUs correspond best to localities.

The causal interpretation of my results will depend on the validity of 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 provide evidence that supports the validity of this assumption by analyzing the following event study-type equation:

$$y_{ijc} = \theta + \sum_k \psi_{c-c_j^*}\, I_{(k=c-c_s^*)} + \delta_j + \kappa_c + \mu_{sc} + \boldsymbol{X_{ijc}}\boldsymbol{\Psi} + \varepsilon_{ijc},$$

where $c_j^*$ denotes the first cohort affected by the intervention in locality $j$. The difference $c - c_j^*$ is the time relative to $c_j^*$, with negative values reflecting older cohorts not exposed to the policy. $I_{(k=c-c_j^*)}$ is an indicator function that produces dummy variables for $k = c - c_j^*$, so $\psi_{c-c_j^*}$ gives the effect of leads and lags of policy adoption. The omitted category is -1. Negative categories with zero effect validate the PTA, as shown in Figure A.4.

# 4 Results

## 4.1 Descriptive analysis on the returns to English skills in Mexico

A descriptive analysis using a simple ordinary least squares (OLS) estimation suggests that the returns to English skills in Mexico are zero, which contradicts the findings in the existing literature. In this part of the analysis, I use Equation 1, which controls for observable characteristics, cohort, and locality FE. In particular, I offer five estimates that progressively address the omitted variables problem by including more controls: first, a naive estimate (without controls); second, including cohort FE, gender, and ethnicity; third, including education FE; fourth, incorporating rural and marital status dummy variables; and finally, including locality FE. I also present estimates using 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 complementarity between English and education. Two main findings emerge from this initial approach: first, the omitted variables problem is primarily due to education, and second, English speakers do not earn more than non-English speakers.

Education accounts for approximately 80% of the selection bias arising from the observable omitted variables problem. A naive estimate would suggest that English speakers earn 110% more than non-English speakers in Mexico (0.744 natural log points). However, this estimate contains confounding factors such as gender and age. Once I control for these variables, the estimated effect reduces considerably to 93%. Furthermore, the most significant factor contributing to the omitted variables problem is education, reducing the estimate to 11%, though it is not statistically significant. Other crucial controls include socio-demographic characteristics. For example, individuals living in urban areas, and non-indigenous are more likely to speak English. Thus, I further mitigate the omitted variables problem by controlling for these characteristics, as shown in column (4) of Table 2. With this specification, the bias is further reduced, resulting in a negative point estimate that remains statistically insignificant. The remaining estimate (from column 5) mitigates the downward bias by accounting for non-time-varying unobservable characteristics at the locality level. These unobservables may capture characteristics of the poor localities that, after the English programs, make them more likely to offer English instruction, but that ultimately explain wage disparities.

Once I control for observable and unobservable characteristics of the individuals and their socioeconomic context, I find that English speakers do not earn more than non-English speakers. This result is different from previous findings of Delgado Helleseter (2020) and Charles-Leija and Torres (2022), who suggest that the wage premium of Mexican English speakers is 28% and 19.4%, respectively. This preliminary result derives from my proposed model, which implies two main assumptions. First, the set of controls I include in the model accounts 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 non-English speakers (see column 5 of Table 2). Finally, I do not find significant differences in the returns to English skills by educational attainment. However, I do find that English-speaking women with low

educational attainment earn higher wages than men.

## 4.2 English instruction and wages

The previous estimates, however, do not establish a causal effect of English abilities on wages. To provide a more reliable estimate, I propose a Difference-in-Differences (DiD) strategy, leveraging locality-by-cohort variation in exposure to English instruction in primary school. With this approach, I estimate the intention-to-treat (ITT) effect of offering English instruction in primary school. In this section, I primarily investigate the effect of exposure to English instruction on wages, as motivated by the existing literature. However, later, I demonstrate how the English programs significantly impacted occupational choices, altering the composition of the labor market. Indeed, column (4) of Panel A in Table 3 suggests that the English programs reduced the likelihood of working for pay, which raises a sample selection issue affecting the estimates for wages. In Panel B, I excluded from the sample the two youngest cohorts, which successfully mitigated the sample selection issue (see column (4) of Panel B). Then, I restricted the sample to individuals who work for pay to obtain the estimates for the other variables of interest. Unfortunately, the treatment and comparison groups showed different pre-trends for wages. Hence, in Panel C, I include controls to improve the comparability of both groups.

Local interventions offering English instruction in public primary schools facilitated the acquisition of English skills but had zero long-term effects on wages. Table 3 presents these results, obtained from a traditional staggered DiD specification (Equation 1) estimated with OLS. These interventions did not affect the likelihood of working for pay (column (4)), ruling out issues of sample selection as the analysis focuses solely on individuals employed for pay. Two main results are noteworthy. First, the state English programs increased the hours of English instruction by more than 30 minutes per week. It's important to note that this variable is measured as average hours at the locality level, with some schools offering zero hours of English instruction. Hence, the effect of the program's magnitude is expected to be greater when considering school-level data. Second, individuals residing in localities with, on average, positive hours of English instruction are more likely to self-report proficiency in English. This effect suggests the acquisition of English skills. Furthermore, considering the hours of English instruction as an exogenous variable (due to the implementation/expansion of the English programs), we may argue that English programs in Mexico may have increased the likelihood of speaking English by 21 percentage points (0.107/0.511). On the other hand, I find that these state English programs affecting the exposure to English instruction at early stages of life and the subsequent acquisition of English abilities, did not have long-term effects on wages.

Finally, I find heterogeneous effects by gender and educational attainment (see Table A.3). I did find a significant difference in the acquisition of English skills, with more men reporting being able to speak in English than women. Notice that this effect could be associated with an under-reporting of the English skills abilities from women and not necessarily due to a differential effect of the policy. On the other hand, the policy systematically selected high-educational attainment individuals for paid jobs, but this selection is not statistically different from high-educational attainment individuals. In addition, the policy significantly

reduced the labor supply of low-educational attainment workers, conditional on working for pay. Finally, women who had exposure to English programs are less likely to find a formal job.

## 4.3 Robustness checks

***Robust estimates in the presence of heterogeneous treatment effects***

A recent wave of the DiD literature has noted potential concerns in the estimation of the parameters of interest when there are multiple periods, variations in the treatment timing, and non-parallel trends. In my context, this literature suggests that the estimates from a TWFE specification could be biased due to the presence of heterogeneous treatment effects. For example, the TWFE coefficients could even have the opposite sign of all individual-level treatment effects due to "negative weighting" problems (see de Chaisemartin and D'Haultfoeuille (2022) and Roth et al. (2023) for a review).

Indeed, the timing of the state English programs studied in this paper varied among the treatment states. Additionally, schools that adopted these programs offered different hours of English instruction per week. Thus, we may expect to find negative weights in the estimation of the average treatment effect on the treated (ATT), as explained by Goodman-Bacon (2021). Hence, I first evaluate the presence of negative weights in the estimation of the ATT offered in Panel A of Table 3 using the Goodman-Bacon decomposition. I find that, for all my specifications (except for the one in column (4)), I only have one negative weight out of 63. In the case of column (4), there are zero negative weights out of 115. These negative weights may not imply a severe bias in the ATT. However, I specifically demonstrate the robustness of my results in this subsection.

In Table 4, I provide robust estimates in the presence of heterogeneous treatment effects. To this purpose, I use the methods proposed by Callaway and Sant'Anna (2021), Sun and Abraham (2021), de Chaisemartin and D'Haultfoeuille (2020), and Borusyak, Jaravel and Spiess (2024). In general, all my original estimates are robust in terms of sign and significance and very close in magnitude. However, some subtleties are worth mentioning. First, the evidence suggests that the state English programs increased the acquisition of English skills (see column (2) of Table 4). Notice that despite the potential violation of the parallel trends assumption for this particular outcome, the robust estimates that derive from the appropriate comparison of treatment and control groups point to the same significant result in terms of the acquisition of English skills (see Panels B-E). Furthermore, the Callaway and Sant'Anna (2021) pre-trends test supports the validity of the PTA. Second, the effect on wages is still statistically equal to zero, and even the sign of the effect flips in Panels C and E, which we may interpret as more evidence of the average zero effect on wages. Third, even though the point estimate in column (6) is positive for most models, the estimate shown in Panel E is negative yet statistically equal to zero.

Finally, in Figure 1, I provide suggestive evidence of the validity of the PTA using all five methods summarized in Table 4. Notice that, among all five estimation methods, de Chaisemartin and D'Haultfoeuille (2020) provides the most reliable estimates in the context of my paper. Hence, for the remainder of the analysis of this paper, I will be referring to the results

obtained with this method. Furthermore, notice that there is no evidence of anticipatory effects of the policy for any of the outcomes of interest. Hence, both pieces of evidence along with the assumption of independent sampling (which we can take as given thanks to INEGI's sampling method), allow me to conclude that the de Chaisemartin and D'Haultfoeuille (2020) estimates are consistent and with asymptotically valid confidence intervals. The main exceptions are labor supply and formal job participation (Panels (e) and (f) of Figure 1), which leads me to explore other labor market outcomes of interest.

***Old and young cohorts***

There is a potential concern that the cohort span studied in this paper is "too wide", challenging the comparison between affected and unaffected cohorts. This concern arises because older cohorts may experience different job opportunities than younger ones, which could question the validity of the parallel trends assumption. For this purpose, I performed two sensitivity analyses. First, I analyzed the sensitivity of my results by varying the number of lags and leads, which I summarized in Panels B-D of Table 5. Second, I narrowed the number of cohorts before and after the policy and obtained the average effect of the policy for each of these different samples, which is summarized in Panels E-G of Table 5.

Due to the staggered implementation of the state English programs, the lags and leads do not correspond exactly to the cohorts before and after the policy implementation. So, in a first attempt to evaluate the robustness of my results I varied the number of periods, but not the number of cohorts. I start by excluding one lead and lag and I finish excluding three of each. Second, I actually narrowed the cohort span by excluding the first and last and, finalizing excluding the first and last three cohorts. These exclusions implied less number of periods.

The main conclusion from both analyses is that the positive effect of the English programs on English language abilities is potentially not robust to changes in the number of periods/cohorts. However, this result may be just the consequence of fewer observations causing the estimates to be noisy and, hence, not significant. This is particularly an issue for the English-speaking variable as the number of English speakers is small in the whole sample. A supporting evidence on this is that, for almost all specifications shown in Table 5, the effect of the interventions on the likelihood of speaking English has about the same magnitude but the standard error changes. Other than this main exception, the estimates for all the remaining outcomes of interest are robust to different periods and cohort spans.

Finally, in terms of the identifying assumption, I can still provide suggestive evidence on the validity of the PTA for all outcomes of interest and all the specifications with narrower periods/cohorts. Nevertheless, remember that for two outcomes (labor supply and the likelihood of working in formal jobs) I arguably offered evidence on the PTA with the original specification. Notice, however, that with narrower cohorts it is possible to offer more reliable evidence on the validity of the PTA for labor supply and the likelihood of working in formal jobs (see Figure A.7 and Figure A.8). Furthermore, with narrower periods and cohorts the point estimates do not change much in magnitude and the direction of the effect is still the same.

### *Sensitivity analysis in treatment and control states*

As we naturally expect that the point estimates change with different samples, in this subsection I evaluate if the significance or the direction of the estimates also change. I work with three different exercises. First, I include the previously excluded states, Coahuila and Morelos, as part of the comparison group. Second, I change the comparison group by excluding all southern states that are traditionally different from the northern states. Finally, I evaluate if my results are driven by a particular state English program by excluding the treated states, in different regression models.

First, the original reason for excluding the states of Coahuila and Morelos is that these two states declared implementing English programs but, in practice, they faced challenges offering English instruction because of the lack of English teachers and/or the lack of resources. Nevertheless, I show that the inclusion of these two states as part of the comparison group does not significantly affect my main results, except for two outcomes. The point estimate for the likelihood of speaking English has not changed, but it has become noisier. In addition, the point estimate for wages changed in sign, but it is still statistically zero (see Panel B of Table 6). Notice that although the effect on the likelihood of having a formal job is significant, I cannot provide evidence on the validity of the PTA (see Figure A.9).

Second, excluding southern states from the control group produces similar results to the main specification but makes the estimates for the likelihood of speaking English less precise. Indeed, keep in mind that the proportion of Mexicans who speak English is small, so it is natural to get less precise estimates with fewer observations in the data (see Panel C of Table 6). However, the fact that the point estimate remains positive and around the same magnitude supports the evidence that the individuals acquired English abilities after exposure to English instruction at school. One additional piece of evidence supports this argument. If we take the example of wages, the estimates are not only less precise with the exclusion of observations but also the point estimates suggest changes in the direction of the effect, which points to the conclusion of a zero impact on wages. Finally, I found similar results when excluding states from the treatment group. It is worth mentioning, however, that the results suggest that most of the effect on the acquisition of English skills could be attributed to Nuevo Leon's English program (see Panels C to E of Table 6). The event-study graphs for all these specifications are shown in Figure A.10.

### *State trends*

One additional concern is that the zero effects that we have observed so far in the labor market are due to regional differences over time. For example, it is known that the northern states grow faster than the other regions of Mexico and that they are closely related to the US through international trade and also in terms of the labor market. Hence, if the state English programs coincided with external factors that affected their labor markets, we may expect significantly different results.

This concern is, in general, not an issue. In Panel B of Table 7, I show the results of estimating Equation 2 with state by cohort FE, which captures potentially different state trends over cohorts. Notice that the effect on wages is still statistically zero and more

imprecise. Similarly, the effect on the likelihood of speaking English is still positive but with a larger standard error. In addition, it is important to mention that, even though the effect on the likelihood of having a formal job is significant, I cannot provide evidence of the validity of the PTA for this particular variable (see Figure A.11).

In my context, there is no clear evidence that there were external factors affecting differently the hours of English instruction or the acquisition of English skills. Furthermore, in terms of the model specification, it is better not to include state by cohort FE due to the substantial increase in the magnitude of the standard errors. However, these external factors seem to be relevant in determining the effect of the state English programs on labor market outcomes. Hence, in the next section, I include state trends in all my specifications as it helps to validate the PTA when purely looking at the labor market.

## 4.4 Mechanisms

In general, the state interventions that introduced English instruction in public elementary schools could have affected individuals' wages through three main channels: effects on individuals' abilities (including English skills), occupational choices, and educational attainment or school enrollment. In this section I explore each potential channel separately. In summary, I provide suggestive evidence that the zero effect on wages is not a result of trade-offs between English and other cognitive abilities but a result of occupational choices. Furthermore, it might be possible that once the individuals in the youngest cohorts are incorporated into the labor market, the effect on wages could become statistically positive.

### *Cognitive skills*

The results of this paper indicate an improvement in human capital through enhanced language skills, specifically the acquisition of English skills, with no additional effects on other types of abilities. The positive effect on English skills is supported by the results discussed in the previous sections and new evidence presented in this section, which shows an increased likelihood of working in occupations requiring English skills. The zero effect on other cognitive abilities has been previously documented in the literature.

First, the results from Table 3 suggest that the state English programs increased the likelihood of speaking English by at least 7.4 percentage points (although the effect could be as large as 14.3 percentage points). Notice that this effect is not robust in terms of significance as the estimate becomes noisier with changes in the sample size (see Table 5, and Table 6). Nevertheless, the magnitude of the point estimate remains robust. Furthermore, the increase in the size of the standard errors can be attributed mainly to the reduction in the number of observations and the low proportion of Mexicans who speak English and not due to an imprecise estimate.

Since the results about the acquisition of English skills are not completely robust, I offer additional supporting evidence on the acquisition of English skills by analyzing the likelihood of working in occupations that require English skills. The intuition is that if an individual acquires English abilities, he/she may be more inclined to work in occupations that require these types of abilities. To test this hypothesis, I classified the occupations by English

requirements according to the proportion of English speakers. I defined an occupation with English skills requirements if it is located in the top ten percentile of the distribution of jobs by the proportion of English speakers. This cutoff considers jobs with more than 5.4% of English speakers. Notice that, although this proportion does not measure an actual requirement of English skills, they should be highly correlated. Figure 2 shows supporting evidence of my hypothesis. The state English programs increased the likelihood of working in occupations with English skills requirements but only among women and individuals with high educational attainment. The effect on women is particularly interesting because it opposes the effect observed in Table A.3, but it is aligned with my story in terms of a potential sub-reporting of English skills among women. Furthermore, the potentially positive effect among individuals with high educational attainment is consistent with the results shown in Table A.3 and with the idea that there are complementarities between education and English skills.

Second, it is natural to think that wages will vary along with changes in the marginal product of labor. For example, if the inclusion of English as a subject affected the teaching time of other subjects, there may be a trade-off between foreign language and other cognitive skills. This potential trade-off would produce an ambiguous effect on the marginal product of labor and, ultimately, on wages. However, the existing literature has shown that exposure to English instruction does not affect cognitive skills in Mexico (see Gálvez-Soriano (2023)). Furthermore, as I explained before, results from Table A.3 provide strong evidence that the English programs increased the likelihood that individuals speak English. In light of these results, the increase in the marginal product of labor did not result in a significant increase in wages. This outcome is consistent with a general equilibrium effect, where the increase in the demand for labor was accompanied by an increase in labor supply.

***Occupational decisions and general equilibrium effects in the labor market***

Another factor likely explaining the zero effect on wages is an increase in labor supply. State English programs may have influenced individuals' occupational decisions. Specifically, we are interested in analyzing whether the state English programs affected working conditions and/or labor supply. In Figure 3, I provide evidence of an increase in labor supply. In particular, the state English programs increased the likelihood that women and individuals with low educational attainment have jobs. This result is interesting because the English programs are helping close the gap in labor force participation between men and women. Furthermore, it is relevant that the quality of workers who now participate in the labor market is low. This latter finding can be interpreted in two ways. First, the English programs may have the potential to reduce the number of inactive members of society (those who are not working and have decided not to study) by encouraging them to enter the labor market. Second, the increase in low-quality English speakers in the market may negatively affect the perceived marginal product of labor.

Consistent with the previous results I find that, conditional on working for pay, the state English programs increased the probability of working in physically demanding jobs, which are associated with low-skilled workers. Noticed that high-educational attainment individuals are also more likely to work in occupations that require communication skills (see Panel (a) and (c) of Figure 4). This result suggests that the increase in labor supply was not only

due to low-quality workers but also high-quality ones. Furthermore, the effect on physically demanding jobs is mainly driven by men. On the other hand, women are more likely to work in jobs requiring communication skills (see Panel (b) and (d) of Figure 4). For all this analysis, I utilized the O*Net classification of occupations, which assigns a score to each occupation based on the intensity of specific tasks required for that job. I focused on jobs that require physical work or those that demand communication skills, as these are likely to be the job characteristics individuals with English skills consider changing when making their occupational choices.

Hence, from the perspective of a partial equilibrium effect, we could have expected that the improvement in language skills increased the marginal product of labor, shifting the demand for labor to the right. More labor and higher wages are expected (see Panel (a) of Figure 5). However, in practice, we only observed the increase in labor but no changes in wages. The natural explanation for these results is an increase in labor supply. I showed that the state English programs increased the likelihood that women and low-skilled workers find jobs, shifting the labor supply to the right, offsetting the increase in wages and substantially increasing the amount of labor in the economy (see Panel (b) of Figure 5). Nevertheless, the low-quality workers are sending a negative signal to the market, resulting in only a small shift to the right of the demand curve or even no shift at all. In this context, the shift to the right of the labor supply curve could either offset the increase in wages or even push them to a lower level. My results suggest that the labor supply compensates for the small increase in labor demand (see Panel (c) of Figure 5).

But, how are these changes being perceived by workers themselves? I document that high-educational attainment workers reveal being satisfied with their jobs, with no significant differences by sex (see Panel (a) and (b) of Figure 6). However, these same types of workers are unhappy with their standard of living, which is consistent with the fact that they are not finding better-paying jobs. Similarly, women are also unsatisfied with their standard of living (see Panel (c) and (d) of Figure 6). Finally, I do not observe significant differences in terms of satisfaction with personal achievements by educational attainment. However, women are not satisfied with their achievements. This could also be associated with the lack of improvement in wages.

Finally, notice that the average effect of English programs on wages may exclude potentially high earners. These are the individuals who, instead of participating in the labor market, are currently enrolled in school. In fact, the youngest cohorts, who are more likely to be enrolled in school, have greater exposure to English programs. However, even when I compare individuals within the same cohort, those who were affected by the English program are more likely to be enrolled in school (see Panel (a) of Figure 7). These are good news for policymakers as we may expect a future improvement in human capital accumulation due to exposure to English programs, which has not yet been totally reflected on wages. On the other hand, the results of this analysis are consistent with my previous findings in terms of the increase in labor supply. Indeed, the state English programs increase labor supply, and part of this effect is due to fewer individuals being enrolled in school. However, this is not necessarily a negative effect as this result is driven by high educational attainment individuals (see Panel (b) and (c) of Figure 7).

# 5 Conclusions

For years, developing non-English-speaking countries have offered English instruction in schools under the assumption that their populations will benefit from acquiring English skills by finding better-paid jobs and better labor opportunities. In this paper, I test this hypothesis in the context of Mexico, which is arguably the most representative economy that could benefit from English instruction due to its close relationship with the US. Contrary to the traditional view and existing evidence in the literature, I find that English programs offering English instruction in schools may not affect wages, likely due to general equilibrium effects caused by changes in labor supply.

To provide an estimate with a causal interpretation, I exploit the implementation of several state English programs that offered English instruction in public primary schools in Mexico, using a TWFE specification. First, I provide strong evidence that English programs increase the acquisition of English abilities. In fact, if we consider the number of hours of English instruction as an exogenous variable (due to the implementation/expansion of the state English programs), we may argue that English instruction in Mexico has increased the acquisition of English skills by 21 percentage points (0.107/0.511). Second, despite the improvement in the marginal product of labor, I found that the state English programs did not affect wages. This outcome is consistent with a general equilibrium effect, where the increase in the demand for labor was accompanied by an increase in labor supply.

Indeed, from the perspective of a partial equilibrium effect, we might have expected that the improvement in language skills would increase the marginal product of labor, shifting the demand for labor to the right, resulting in more labor and higher wages. However, we observed only an increase in labor, with no changes in wages. The natural explanation for these results is an increase in labor supply. I showed that the state English programs increased the likelihood that women and low-skilled workers found jobs, shifting the labor supply to the right, offsetting the increase in wages, and substantially increasing the amount of labor in the economy. Nevertheless, the low-quality workers sent a negative signal to the market, resulting in only a small shift to the right of the demand curve or even no shift at all. In this context, the rightward shift of the labor supply curve could either offset the increase in wages or even push them lower. My results suggest that the labor supply compensated for the small increase in labor demand. Additionally, the increase in labor supply was characterized by more women in physically demanding jobs, who were less satisfied with their achievements and standard of living.

Finally, notice that the average effect of English programs on wages may exclude potentially high earners. These are the individuals who, instead of participating in the labor market, are currently enrolled in school. In fact, the state English programs increase the probability that the individuals recently exposed to English instruction continue enrolled in school, but as time goes by, they are more inclined to join the labor market. In summary, these results suggest that the state English programs did not improve earnings or labor opportunities. This is because, instead, these English programs increased the labor force participation of women and low-educational attainment individuals, who were perceived as low-quality workers. This latter effect shrank labor demand and expanded labor supply, which left wages unaffected.

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# Figures and tables

**Table 1:** Descriptive statistics

| Variable | Full Sample | Speak English (a) | Don't spk English (b) | Diff. (a-b) |
|---|---|---|---|---|
| *Dependent variable* | | | | |
| Wage (monthly pesos) | 5,164.59 | 9,575.90 | 4,767.58 | 4,808.31*** |
| English (speaking ability) | 0.08 | 1.00 | 0.00 | - |
| Hrs English | 0.11 | 0.14 | 0.11 | 0.03 |
| Labor supply (hours) | 46.04 | 44.74 | 46.15 | -1.41 |
| Formal job | 0.47 | 0.64 | 0.45 | 0.19*** |
| Physically demanding job | 0.26 | 0.10 | 0.27 | -0.17*** |
| Job with comm. skills | 0.27 | 0.58 | 0.24 | 0.34*** |
| Satisfied with job | 0.52 | 0.59 | 0.52 | 0.07* |
| Satisfied with SOL | 0.38 | 0.49 | 0.38 | 0.11*** |
| Satisfied with achievements | 0.43 | 0.56 | 0.41 | 0.15*** |
| *Independent variables* | | | | |
| Age (years) | 25.72 | 26.61 | 25.64 | 0.97*** |
| Education (years) | 10.62 | 14.16 | 10.30 | 3.87*** |
| Female (%) | 0.40 | 0.39 | 0.40 | -0.01 |
| Indigenous (%) | 0.06 | 0.03 | 0.06 | -0.03** |
| Married (%) | 0.54 | 0.39 | 0.55 | -0.16*** |
| Rural (%) | 0.20 | 0.08 | 0.21 | -0.13*** |
| Observations | 4,548 | 383 | 4,165 | 4,548 |

*Note:* The sample consists of Mexicans who were born between 1984 and 1994 who work for pay. Statistics shown in this table are obtained considering the survey weights.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

**Table 2:** 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) |
| *Panel A: Men and Women* | | | | | | | | | |
| Speak Eng | 0.744*** | 0.656*** | 0.105 | -0.077 | -0.047 | 0.239 | -0.115 | 0.441*** | -0.206 |
| | (0.156) | (0.152) | (0.151) | (0.147) | (0.171) | (0.409) | (0.314) | (0.161) | (0.183) |
| Observations | 4,548 | 4,548 | 4,548 | 4,548 | 4,548 | 2,580 | 2,580 | 1,968 | 1,968 |
| Adjusted $R^2$ | 0.009 | 0.077 | 0.133 | 0.179 | 0.417 | 0.000 | 0.498 | 0.008 | 0.371 |
| *Panel B: Men ($\beta^M$)* | | | | | | | | | |
| Speak Eng | 0.592*** | 0.503*** | 0.136 | 0.032 | 0.039 | -0.457 | 0.022 | 0.611*** | -0.033 |
| | (0.179) | (0.173) | (0.174) | (0.160) | (0.188) | (0.505) | (0.260) | (0.168) | (0.309) |
| Observations | 2,643 | 2,643 | 2,643 | 2,643 | 2,643 | 1,640 | 1,640 | 1,003 | 1,003 |
| Adjusted $R^2$ | 0.010 | 0.044 | 0.088 | 0.145 | 0.575 | 0.002 | 0.638 | 0.021 | 0.660 |
| *Panel C: Women ($\beta^W$)* | | | | | | | | | |
| Speak Eng | 0.959*** | 0.898*** | 0.092 | -0.152 | -0.264 | 1.802*** | 0.548 | 0.166 | -0.567 |
| | (0.246) | (0.246) | (0.245) | (0.256) | (0.373) | (0.299) | (0.796) | (0.264) | (0.422) |
| Observations | 1,905 | 1,905 | 1,905 | 1,905 | 1,905 | 940 | 940 | 965 | 965 |
| Adjusted $R^2$ | 0.010 | 0.057 | 0.144 | 0.225 | 0.631 | 0.006 | 0.749 | 0.001 | 0.578 |
| $\beta^M = \beta^W$ [p-value] | [0.196] | [0.124] | [0.184] | [0.102] | [0.150] | [0.000] | [0.007] | [0.126] | [0.252] |
| Basic controls | NO | YES | YES | YES | YES | NO | YES | NO | 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 who were born between 1984 and 1994, and who work for pay. Basic controls include: cohort fixed effects, gender and indigenous dummy. Other controls include: geographical context (rural/urban), marital status and locality fixed effects. Standard errors clustered at locality level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

**Table 3:** English programs, labor market outcomes, and selection

| | (1) Hrs Eng | (2) Speak Eng | (3) ln(wage) | (4) Paid work | (5) Labor supply | (6) Formal work |
|---|---|---|---|---|---|---|
| *Panel A: Full sample* | | | | | | |
| Had Policy | 0.540*** | 0.099*** | -0.334 | -0.070** | | |
| | (0.066) | (0.034) | (0.316) | (0.033) | | |
| | [0.000] | [0.009] | [0.263] | [0.044] | | |
| Observations | 9,554 | 9,554 | 9,554 | 9,554 | | |
| Adjusted $R^2$ | 0.784 | 0.037 | 0.103 | 0.110 | | |
| *Panel B: Sub-sample (older individuals who work for pay) without controls* | | | | | | |
| Had Policy | 0.495*** | 0.106** | -0.501** | 0.002 | -0.138 | 0.023 |
| | (0.075) | (0.052) | (0.205) | (0.047) | (0.100) | (0.071) |
| | [0.000] | [0.035] | [0.010] | [0.962] | [0.199] | [0.753] |
| Observations | 4,548 | 4,548 | 4,548 | 8,098 | 4,278 | 4,548 |
| Adjusted $R^2$ | 0.729 | 0.058 | 0.226 | 0.073 | 0.102 | 0.223 |
| *Panel C: Sub-sample (older individuals who work for pay) with controls* | | | | | | |
| Had Policy | 0.493*** | 0.124** | -0.282 | 0.051 | -0.094 | 0.054 |
| | (0.075) | (0.054) | (0.230) | (0.045) | (0.103) | (0.072) |
| | [0.000] | [0.012] | [0.172] | [0.237] | [0.348] | [0.435] |
| Observations | 4,548 | 4,548 | 4,548 | 8,098 | 4,278 | 4,548 |
| Adjusted $R^2$ | 0.729 | 0.122 | 0.291 | 0.188 | 0.145 | 0.275 |
| Mean Dep. Var. | 0.103 | 0.083 | 7.710 | 0.541 | 3.720 | 0.471 |

*Note:* This table shows the effect of state English programs on labor market outcomes. The sub-sample consists of Mexicans who were born between 1984 and 1994 and who work for pay. All the specifications include cohort FE, locality FE and state-by-cohort FE. Notice that p-values in brackets are obtained with a wild bootstrap-t procedure with 999 replications and Rademacher weights. Standard errors clustered at locality level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

**Table 4:** English programs and robustness in the presence of heterogeneous treatment effects

| | (1) Hrs Eng | (2) Speak Eng | (3) ln(wage) | (4) Paid work | (5) Labor supply | (6) Formal work |
|---|---|---|---|---|---|---|
| *Panel A: Canonical TWFE with OLS* | | | | | | |
| Had Policy | 0.493*** | 0.124** | -0.282 | 0.051 | -0.094 | 0.054 |
| | (0.075) | (0.054) | (0.230) | (0.045) | (0.103) | (0.072) |
| | [0.000] | [0.012] | [0.172] | [0.237] | [0.348] | [0.435] |
| Observations | 4,548 | 4,548 | 4,548 | 8,098 | 4,278 | 4,548 |
| Adjusted $R^2$ | 0.729 | 0.122 | 0.291 | 0.188 | 0.145 | 0.275 |
| *Panel B: Sun and Abraham (2021) interaction weighted estimator* | | | | | | |
| Had Policy | 0.508*** | 0.143*** | -0.244 | 0.061 | -0.086 | 0.039 |
| | (0.067) | (0.040) | (0.215) | (0.040) | (0.092) | (0.068) |
| Observations | 4,182 | 4,182 | 4,182 | 7,844 | 3,900 | 4,182 |
| Adjusted $R^2$ | 0.719 | 0.135 | 0.281 | 0.183 | 0.131 | 0.270 |
| *Panel C: Callaway and Sant'Anna (2021)* | | | | | | |
| Had Policy | 0.331*** | 0.135*** | 0.347 | -0.024 | -0.209** | 0.284 |
| | (0.048) | (0.043) | (0.454) | (0.257) | (0.096) | (0.217) |
| Observations | 4,487 | 4,487 | 4,487 | 7,974 | 4,224 | 4,487 |
| Pre-trend test [p-value] | [0.201] | [0.154] | [0.725] | [0.063] | [0.000] | [0.370] |
| *Panel D: Borusyak, Jaravel, and Spiess (2024)* | | | | | | |
| Had Policy | 0.504*** | 0.127*** | -0.191 | 0.060* | -0.060 | 0.061 |
| | (0.043) | (0.038) | (0.173) | (0.036) | (0.057) | (0.053) |
| Observations | 4,545 | 4,545 | 4,545 | 8,096 | 4,275 | 4,545 |
| *Panel E: de Chaisemartin and D'Haultfoeuille (2020)* | | | | | | |
| Had Policy | 0.493*** | 0.074*** | 0.006 | 0.069 | -0.129 | -0.103 |
| | (0.071) | (0.024) | (0.071) | (0.069) | (0.101) | (0.071) |
| Observations | 4,182 | 4,182 | 4,182 | 7,844 | 3,900 | 4,182 |
| Mean Dep. Var. | 0.103 | 0.083 | 7.710 | 0.541 | 3.720 | 0.471 |

*Note:* This table shows the effect of state English programs on labor market outcomes. The sample consists of Mexicans who were born between 1984 and 1994. Controls include gender, indigenous people dummy, marital status, education fixed effects, cohort fixed effects, and locality fixed effects. Notice that, in Panel A, p-values in brackets are obtained with a wild bootstrap-t procedure with 999 replications and Rademacher weights. On the other hand, in Panel C, p-values in brackets correspond to the "pre-trend test", in which the null hypothesis states that all estimates in pre-treatment periods are equal to zero. Standard errors clustered at locality level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

**Table 5:** Sensitivity analysis changing the number of leads and lags

| | (1) Hrs Eng | (2) Speak Eng | (3) ln(wage) | (4) Paid work | (5) Labor supply | (6) Formal work |
|---|---|---|---|---|---|---|
| *Panel A: Full sample* | | | | | | |
| Had Policy | 0.493*** | 0.074*** | 0.006 | 0.069 | -0.129 | -0.103 |
| | (0.071) | (0.024) | (0.071) | (0.069) | (0.101) | (0.071) |
| *Panel B: Excluding one lag and one lead* | | | | | | |
| Had Policy | 0.484*** | 0.069 | 0.010 | 0.058 | -0.103 | -0.090 |
| | (0.080) | (0.071) | (0.288) | (0.073) | (0.091) | (0.069) |
| *Panel C: Excluding two lags and two leads* | | | | | | |
| Had Policy | 0.459*** | 0.066 | 0.027 | 0.032 | -0.112 | -0.109 |
| | (0.077) | (0.080) | (0.288) | (0.083) | (0.119) | (0.074) |
| *Panel D: Excluding three lags and three leads* | | | | | | |
| Had Policy | 0.439*** | 0.072 | 0.031 | 0.035 | -0.109 | -0.087 |
| | (0.069) | (0.084) | (0.313) | (0.085) | (0.105) | (0.071) |
| *Panel E: Excluding one old and young cohorts* | | | | | | |
| Had Policy | 0.482*** | 0.066 | 0.003 | 0.092 | -0.105 | -0.083 |
| | (0.068) | (0.083) | (0.270) | (0.065) | (0.092) | (0.073) |
| *Panel F: Excluding two old and young cohorts* | | | | | | |
| Had Policy | 0.454*** | 0.065 | 0.034 | 0.089 | -0.105 | -0.091 |
| | (0.068) | (0.078) | (0.282) | (0.062) | (0.110) | (0.068) |
| *Panel G: Excluding three old and young cohorts* | | | | | | |
| Had Policy | 0.434*** | -0.035 | 0.000 | 0.018 | -0.060 | -0.139 |
| | (0.058) | (0.026) | (0.480) | (0.085) | (0.129) | (0.084) |

*Note:* This table shows the effect of state English programs on labor market outcomes. The sample consists of Mexicans who were born between 1984 and 1994. Controls include gender, indigenous people dummy, marital status, education fixed effects, cohort fixed effects, and locality fixed effects. The estimates are obtained using the method proposed by de Chaisemartin and D'Haultfoeuille (2020). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

**Table 6:** Sensitivity analysis changing the composition of the treatment and comparison groups

| | (1) Hrs Eng | (2) Speak Eng | (3) ln(wage) | (4) Paid work | (5) Labor supply | (6) Formal work |
|---|---|---|---|---|---|---|
| *Panel A: Full sample* | | | | | | |
| Had Policy | 0.493*** | 0.074*** | 0.006 | 0.069 | -0.129 | -0.103 |
| | (0.071) | (0.024) | (0.071) | (0.069) | (0.101) | (0.071) |
| *Panel B: Including Coahuila and Morelos* | | | | | | |
| Had Policy | 0.469*** | 0.074 | -0.241 | 0.016 | -0.135 | -0.156** |
| | (0.064) | (0.086) | (0.181) | (0.068) | (0.090) | (0.075) |
| *Panel C: Excluding southern states* | | | | | | |
| Had Policy | 0.471*** | 0.069 | -0.004 | 0.064 | -0.129 | -0.103 |
| | (0.073) | (0.069) | (0.320) | (0.076) | (0.081) | (0.061) |
| *Panel D: Excluding Aguascalientes* | | | | | | |
| Had Policy | 0.333*** | 0.146* | 0.241 | 0.000 | -0.134 | 0.007 |
| | (0.057) | (0.081) | (0.386) | (0.073) | (0.098) | (0.110) |
| *Panel E: Excluding Durango* | | | | | | |
| Had Policy | 0.509*** | 0.068 | 0.011 | 0.085 | -0.108 | -0.079 |
| | (0.046) | (0.058) | (0.281) | (0.072) | (0.097) | (0.074) |
| *Panel F: Excluding Nuevo Leon* | | | | | | |
| Had Policy | 0.481*** | 0.034 | -0.072 | 0.012 | -0.168 | -0.140** |
| | (0.076) | (0.060) | (0.296) | (0.082) | (0.108) | (0.063) |
| *Panel G: Excluding Sinaloa* | | | | | | |
| Had Policy | 0.501*** | 0.077 | -0.027 | 0.072 | -0.152 | -0.126 |
| | (0.073) | (0.070) | (0.270) | (0.068) | (0.096) | (0.077) |
| *Panel G: Excluding Sonora* | | | | | | |
| Had Policy | 0.549*** | 0.058 | 0.053 | 0.108 | -0.078 | -0.132 |
| | (0.078) | (0.088) | (0.301) | (0.073) | (0.083) | (0.099) |
| *Panel G: Excluding Tamaulipas* | | | | | | |
| Had Policy | 0.539*** | 0.092 | -0.116 | 0.084 | -0.143 | -0.113 |
| | (0.076) | (0.096) | (0.201) | (0.092) | (0.106) | (0.095) |

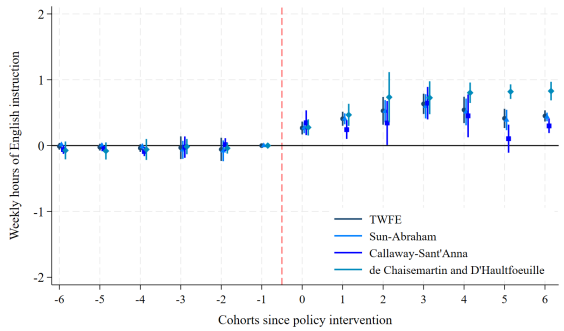
*Note:* This table shows the effect of state English programs on labor market outcomes. The sample consists of Mexicans who were born between 1984 and 1994. Controls include gender, indigenous people dummy, marital status, education fixed effects, cohort fixed effects, and locality fixed effects. The estimates are obtained using the method proposed by de Chaisemartin and D'Haultfoeuille (2020). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

**Table 7:** Robustness check with state trends

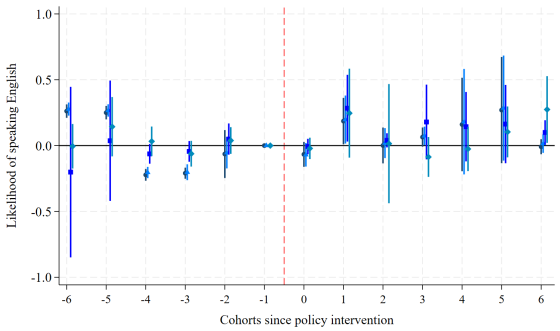
| | (1) Hrs Eng | (2) Speak Eng | (3) ln(wage) | (4) Paid work | (5) Labor supply | (6) Formal work |
|---|---|---|---|---|---|---|
| *Panel A: Full sample* | | | | | | |
| Had Policy | 0.493*** | 0.074*** | 0.006 | 0.069 | -0.129 | -0.103 |
| | (0.071) | (0.024) | (0.071) | (0.069) | (0.101) | (0.071) |
| *Panel B: Including state by cohort FE* | | | | | | |
| Had Policy | 0.497*** | 0.059 | 2.231 | 0.090 | 1.164 | -0.180** |
| | (0.081) | (0.138) | (1.180) | (0.201) | (0.762) | (0.210) |

*Note:* This table shows the effect of state English programs on labor market outcomes. The sample consists of Mexicans who were born between 1984 and 1994. Controls include gender, indigenous people dummy, marital status, education fixed effects, cohort fixed effects, and locality fixed effects. The estimates are obtained using the method proposed by de Chaisemartin and D'Haultfoeuille (2020). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

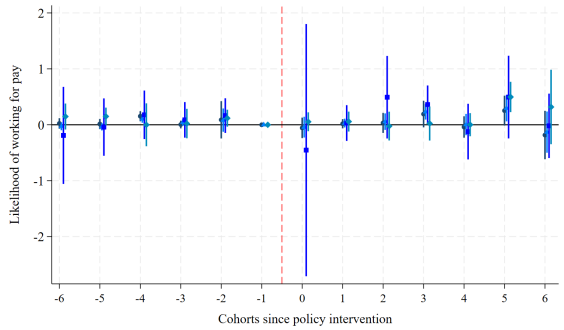
**Figure 1:** Event-study graphs with robust estimates in the presence of heterogeneous treatment effects



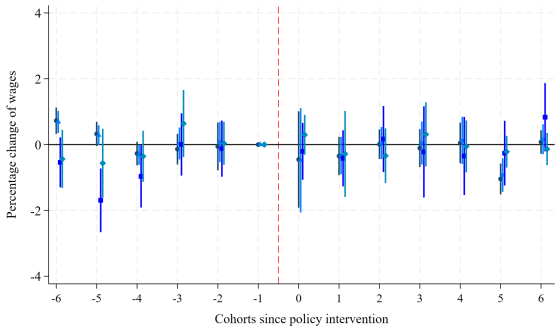

**(a)** Hours of English



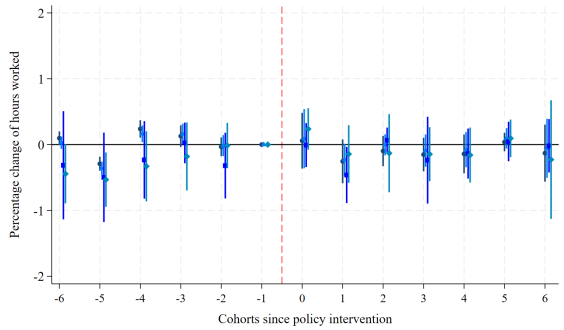

**(b)** Speak English



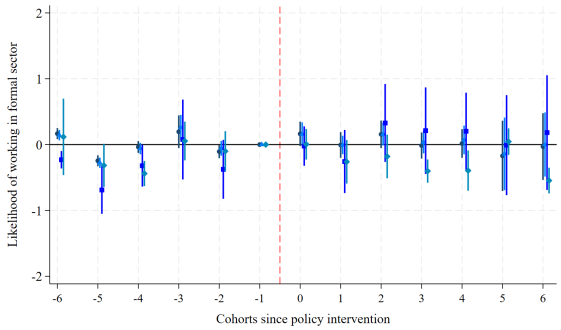

**(c)** Paid work




**(d)** Ln(wage)



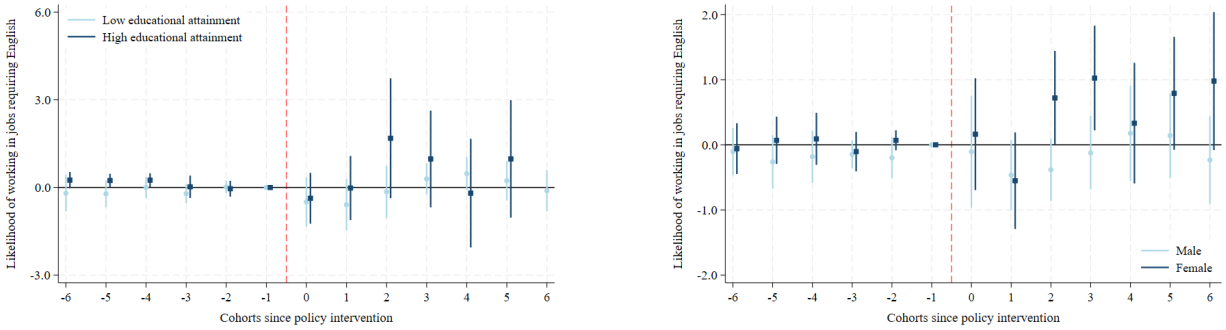

**(e)** Labor supply




**(f)** Formal job

*Note:* This figure plots robust estimates in the presence of heterogeneous treatment effects, obtained from the canonical TWFE, Callaway and Sant'Anna (2021), Sun and Abraham (2021), and de Chaisemartin and D'Haultfoeuille (2020). Each estimate comes from the interaction between the treatment variable and an indicator function of each cohort since the policy intervention, in an event study type regression. The omitted period is -1. 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. Confidence intervals at 95% level.

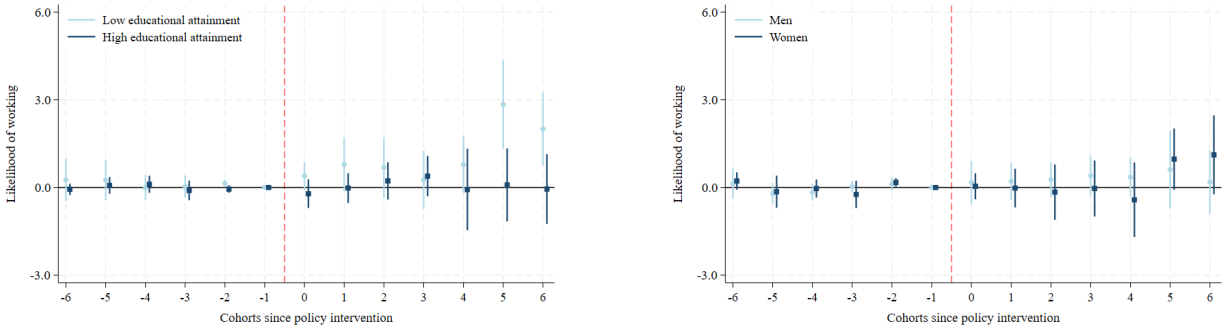
**Figure 2:** Likelihood of working in jobs requiring English skills




**(a)** By educational attainment **(b)** By sex

*Note:* This figure plots robust estimates (as proposed by de Chaisemartin and D'Haultfoeuille (2020)) from 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 period is -1. 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. Confidence intervals at 95% level.

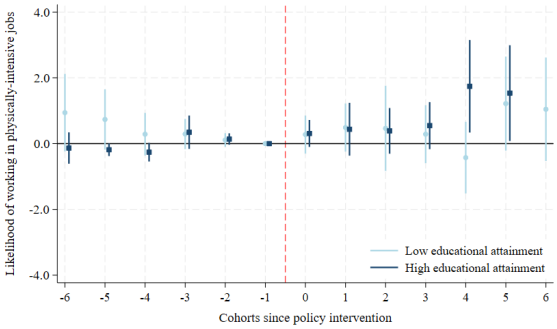
**Figure 3:** Likelihood of working



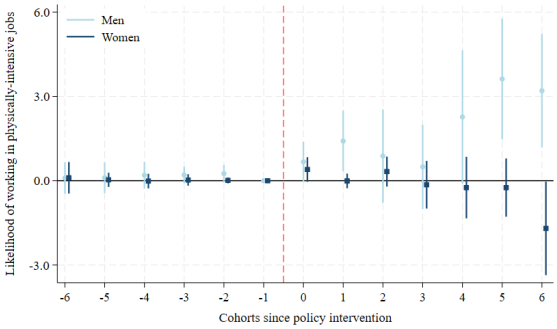

**(a)** By educational attainment **(b)** By sex

*Note:* This figure plots robust estimates (as proposed by de Chaisemartin and D'Haultfoeuille (2020)) from 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 period is -1. 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. Confidence intervals at 95% level.

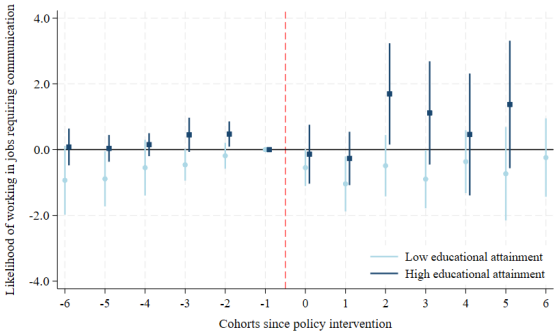
**Figure 4:** Occupational decisions after exposure to English instruction



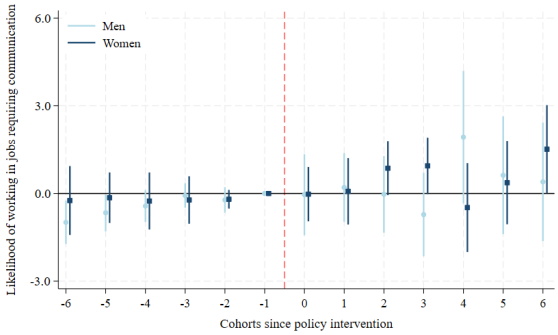

**(a)** Physically demanding jobs by education




**(b)** Physically demanding jobs by sex



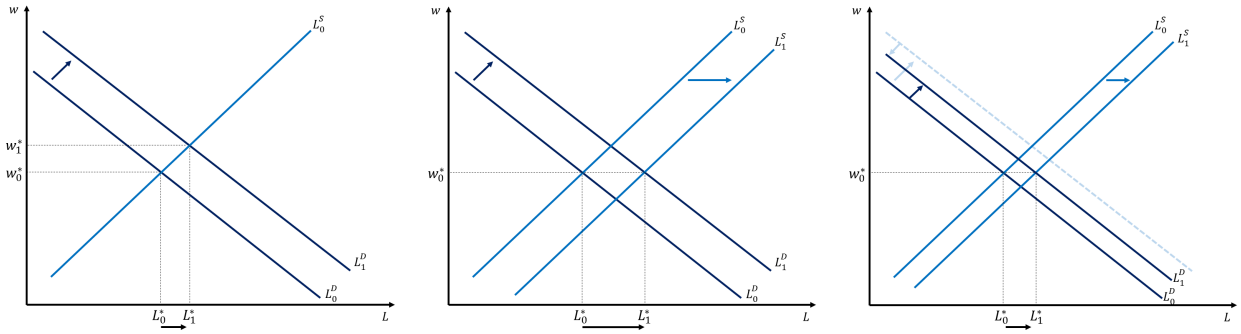

**(c)** Jobs requiring communication by education




**(d)** Jobs requiring communication by sex

*Note:* This figure plots robust estimates (as proposed by de Chaisemartin and D'Haultfoeuille (2020)) from 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 period is -1. 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. Confidence intervals at 95% level.

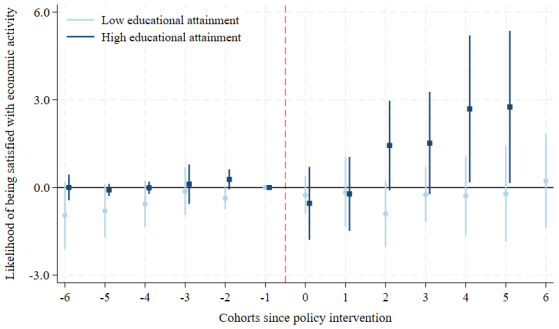
**Figure 5:** Mexican labor market after the state English programs



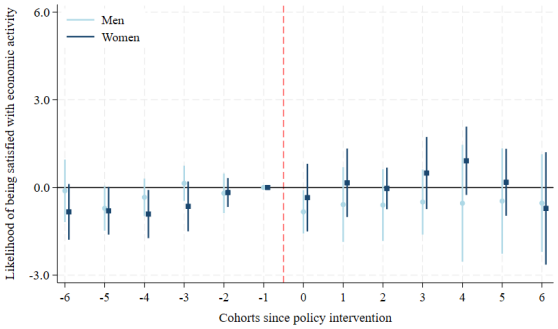

**(a)** Sole demand effect **(b)** Shifts in demand and supply **(c)** Response of demand

*Note:* Panel (a) shows the general equilibrium effects of an increase in labor demand as a response to an improvement in the marginal product of labor. Higher wages and more labor are expected. However, if now more workers are willing to participate in the labor market as a consequence of the policy, the boost in supply could offset the rise of wages and substantially increases the number of workers in the economy, as shown in Panel (b). Furthermore, if the new workers have a low quality, firms will perceive a shorter improvement in the marginal product of labor. The increase in labor supply still offsets the rise of wages, but now the increment in labor is smaller, as shown in Panel (c).

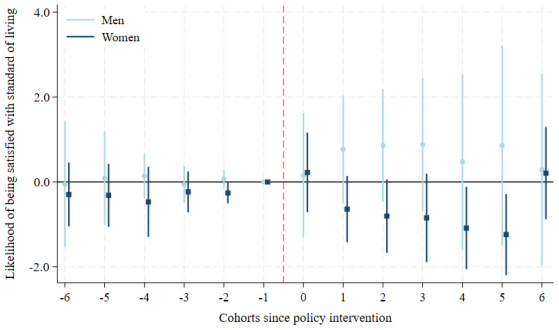
**Figure 6:** Subjective well-being after exposure to English instruction



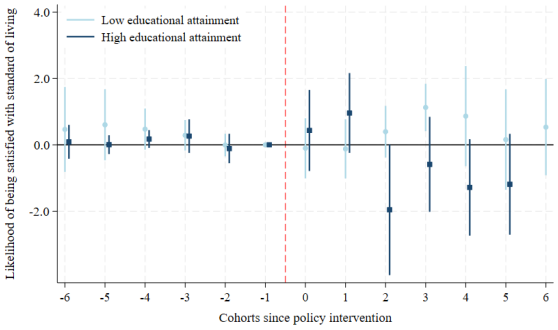

**(a)** Economic activity by education



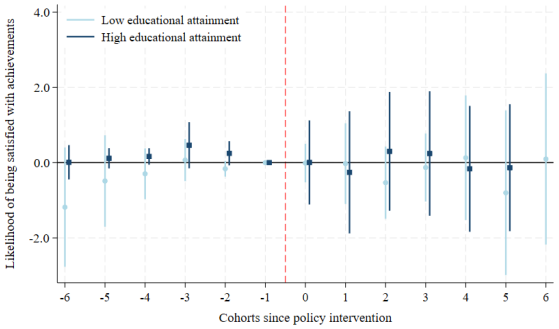

**(b)** Economic activity by sex



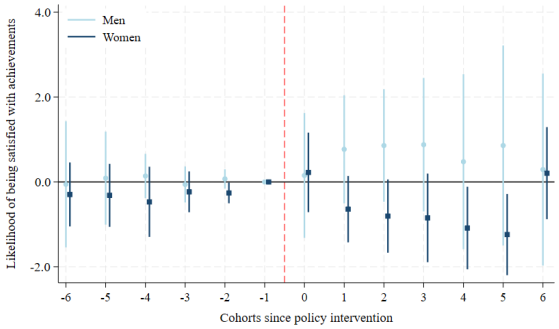

**(c)** Standard of living by education




**(d)** Standard of living by sex



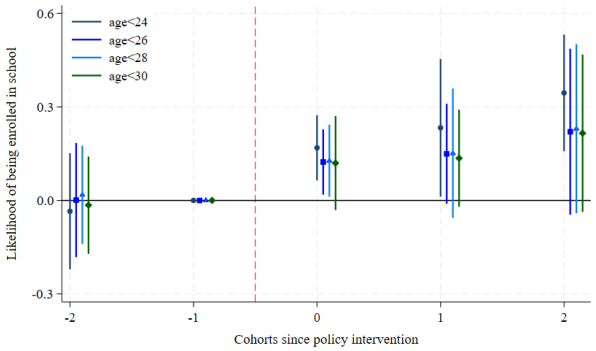

**(e)** Achievements by education



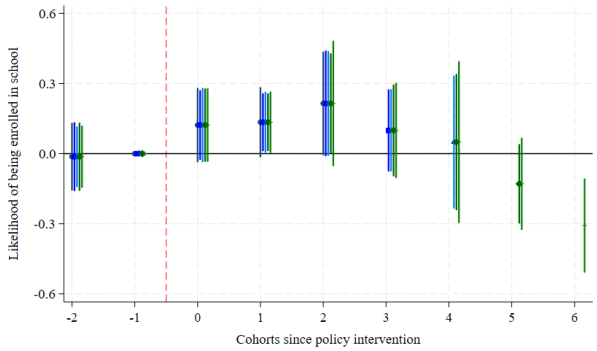

**(f)** Achievements by sex

*Note:* This figure plots robust estimates (as proposed by de Chaisemartin and D'Haultfoeuille (2020)) from 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 period is -1. 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. Confidence intervals at 95% level.

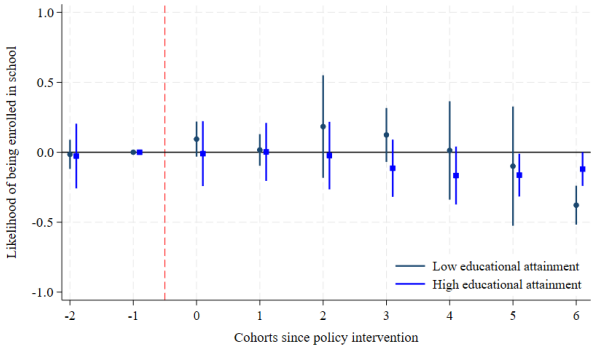
**Figure 7:** Educational decisions after exposure to English instruction




**(a)** Young vs old cohorts




**(b)** Cohorts with more exposure




**(c)** Effect by sex

*Note*: This figure plots robust estimates (as proposed by de Chaisemartin and D'Haultfoeuille (2020)) from 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 period is -1. 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. Confidence intervals at 95% level. Panel (a) shows point estimates with different samples varying its composition by age. Panel (b) considers the full sample but varies the sample by including more leads in the analysis. Panel (c) shows the estimates with full sample and all leads, by sex.

# Appendix

## A.1 Prevalence of English skills among the Mexican population

The descriptive analysis in this section relies on the English proficiency measure available in the 2014 Mexican Subjective Well-being Survey (BIARE, acronym in Spanish), detailed further in the data section. This measure is derived from survey respondents' answers to the question of whether they speak English. While not a detailed measure of English language skills, it remains a valid gauge of English proficiency, and this makes it feasible to describe the prevalence of these types of skills in Mexico and analyze their relationship with labor market outcomes. Using this measure of English proficiency, I document four empirical observations. First, there exists a gender-English gap, with more men declaring proficiency in English than women. Second, increased exposure to English instruction correlates with enhanced English skills. Third, English proficiency rises with educational attainment. Fourth, indigenous people lag in the process of learning English, and similarly, rural areas trail in this learning process.

BIARE surveys only adults aged 18 and older, but I focus on the range 18-65 to primarily capture individuals participating in the labor market (excluding retired workers). Additionally, I provide a comparison between Mexican states that implemented English programs in public primary schools and states that did not. Hence, this preliminary overview highlights two significant dimensions of heterogeneity in English abilities: among types of individuals and among geographical regions.

I document the existence of a gender-English gap in Mexico, with more men declaring to speak English than women. About 9.5% of the male population in Mexico speaks English, while 5.1% 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 with more exposure to English instruction report higher English skills than older individuals. I derived this observation from two facts. First, younger individuals (18-35 years old) are more likely to speak English than elderly individuals (51-65 years old). This higher likelihood could be attributed to the English programs implemented in the early 1990s and early 2000s in Mexico, which predominantly affected younger birth cohorts (as explained in subsection 1.2). Secondly, particularly among the younger birth cohorts, significant differences in English abilities emerge when comparing individuals in states with English programs to those in states without such programs. Once again, this finding supports the hypothesis that English programs enhance the acquisition of English abilities.

The ability to speak English increases with educational attainment. The proportion of college graduates (and higher attainment individuals) with English-speaking skills in Mexico is more than three times the corresponding proportion for all Mexican adults. 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 incomplete primary school 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.

I also document the existence of an ethnicity-English gap in Mexico, with nearly 7.3% of non-indigenous people with English skills and almost 2.5% 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 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 a substantial difference in English ability between urban and rural areas, with the former having 8.3% of English speakers, while the latter have only 2.3%. 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 issue has characterized the context of rural schools in Mexico.

## A.2 Geographical heterogeneity of English skills

In this subsection, I present five observations concerning the geographical heterogeneity of English in Mexico. First, the population with English abilities is mainly located in six Mexican states: Aguascalientes, Baja California, Baja California Sur, Jalisco, Queretaro, and Quintana Roo. Second, the less industrialized Mexican states (South and Southeast regions) have a small share of English speakers. Third, English-speaking individuals are primarily found in urban areas. Fourth, the states with more exposure to English instruction are not necessarily those with more English speakers. Finally, administrative records suggest that six out of 32 Mexican states consistently offered English instruction in public primary schools during the early 1990s.

More than half of the Mexican states have shares of English-speaking individuals higher than the figure for the national level (6.99%), from which four states more than double this proportion (Aguascalientes, Baja California, Baja California Sur, and Quintana Roo). The Mexican states of Quintana Roo and Baja California Sur likely have more English speakers than most of the Mexican states 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 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 state of Quintana Roo. 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 A.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 exhibit the lowest shares of English speakers. However, there are seven with relatively higher shares: Aguascalientes, Baja California, Baja California Sur, Durango, Hidalgo, Nayarit, and Zacatecas (refer to panel (d) of Figure A.1). The first three states have a relatively higher share of English speakers due to the reasons mentioned in observation number one. Durango and Zacatecas serve as international migrant sender states, particularly to the US, with a relatively greater proportion of migrants in rural areas compared to urban ones. Additionally, the state of Hidalgo hosts international companies in automotive, auto parts, technology, telecommunications, and transportation such as GEMI International, Motorola Solutions, Grupo Marpa, and Transtell, situated in rural regions of the state. Meanwhile, Nayarit is renowned for exporting agricultural products (figs, pineapples, avocados, guava, mangoes, cantaloupes, watermelons, and papayas) to the US, which may explain the non-negligible share 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 A.1, reflecting a high share of English speakers, are touristic, migrant senders, and/or hosts of international companies, as explained before. For states that coincide in terms of exposure and the proportion of English speakers (Aguascalientes, Sonora, Morelos, Nuevo Leon, and Queretaro), it is difficult to disentangle the reasons for this high proportion, but I will explore some of them later in this paper. As 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 six out of 32 Mexican states offered English instruction in public primary schools during the early 1990s. Indeed, using historical administrative data from the Mexican school census, I find that the states of Aguascalientes, Durango, Nuevo Leon, Sinaloa, Sonora, and Tamaulipas have implemented state English

programs, which increased the weekly hours of English instruction offered in Mexican public schools (see Figure A.3 from the Appendix).

## A.3 Occupations and English skills in Mexico

In this subsection, I describe the economic occupations in which Mexican English speakers concentrate. I document three main observations. 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.

The occupations that do not require English abilities in Mexico are farming, elementary, and crafts occupations (see the first categories in panel (a) of Figure A.2).[6] This finding is not surprising because these types of occupations do not require a high level of communication skills and, instead, these occupations are more manual-intensive. Thus, on average, in these occupations, only two out of a hundred workers speak English. The English speakers in these occupations likely acquired their English abilities either in school or through a migration network. On the other hand, these occupations are among the worst paid. This finding suggests a positive correlation between English skills and earnings. Notice, however, that individuals working as machine operators also have a low proportion of English speakers and still earn as much as clerical workers (who have a high proportion of English speakers). This exception will be particularly relevant in terms of occupational decisions for individuals exposed to English instruction, as I explain in subsection 4.4.

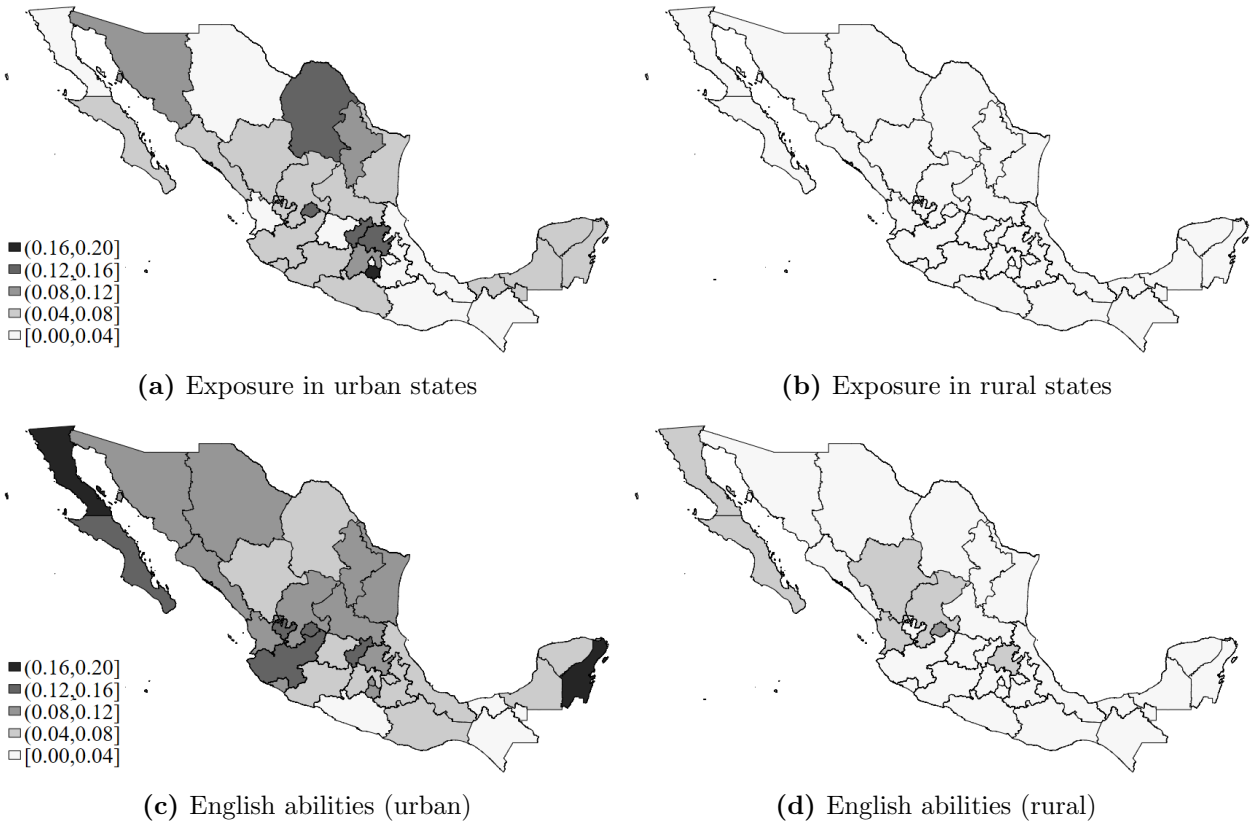
On the other hand, clerical support, professionals, and managerial occupations concentrate most of the English speakers in Mexico (see the last four categories in panel (a) of Figure A.2). In the case of clerical support, it is natural to have more English speakers since these types of occupations are 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 tourism 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 latter finding is consistent with tasks requiring communication skills and public relations, especially in medium-sized and large companies. Finally, 40% of the individuals working abroad have English skills, consistent with Mexicans who cross the US border every day to work in American companies. The remaining occupations (customer service and sales occupations) are below the average English skills in Mexico.

Among the occupations with more English speakers, workers are better paid, more educated, and there is a more even proportion of females. The average monthly wage in Mexico

[6]Notice, 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.

is 6,262 pesos (for adults 18-65 years old in 2014 who work for pay), while monthly wages in clerical support occupations are about 1.1 times higher; professionals and technicians earn 1.7 times the mean wage in Mexico, while the mean wages in managerial occupations is 2.4 times higher. This result suggests a positive correlation between English abilities and earnings. Likewise, as previously noted in section A.1, I find a positive correlation between English abilities and education. Finally, customer service, sales, and clerical support occupations have the most even proportion of female workers.

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




**(a)** Exposure in urban states

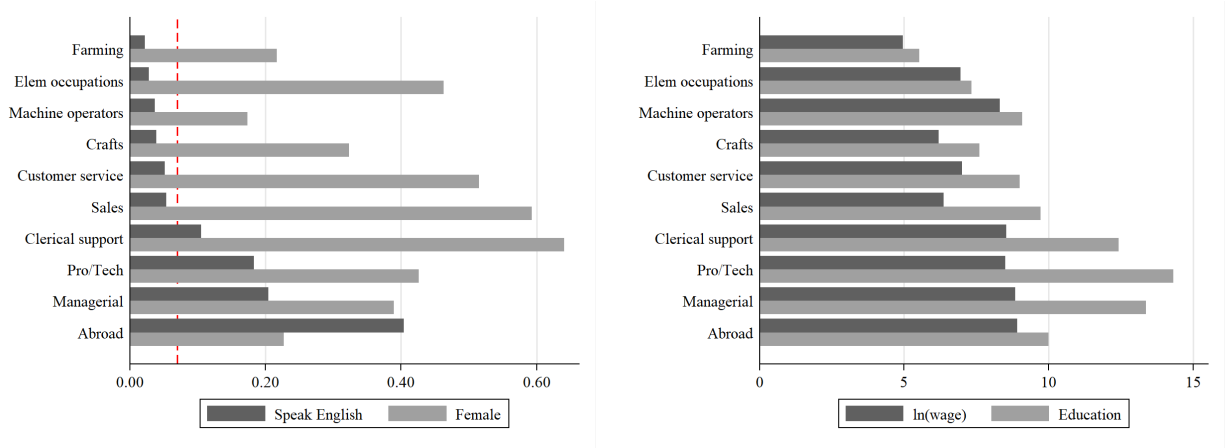
**(b)** Exposure in rural states

**(c)** English abilities (urban)

**(d)** English abilities (rural)

*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).

**Figure A.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 2011 Mexican System of Classification of Occupations (SINCO, for its acronym in Spanish) 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 consists of Mexicans ages 18–65 who self-reported their ability to speak in English. The vertical dotted line represents the mean of English speakers in this sample 0.0699 (6.99%).

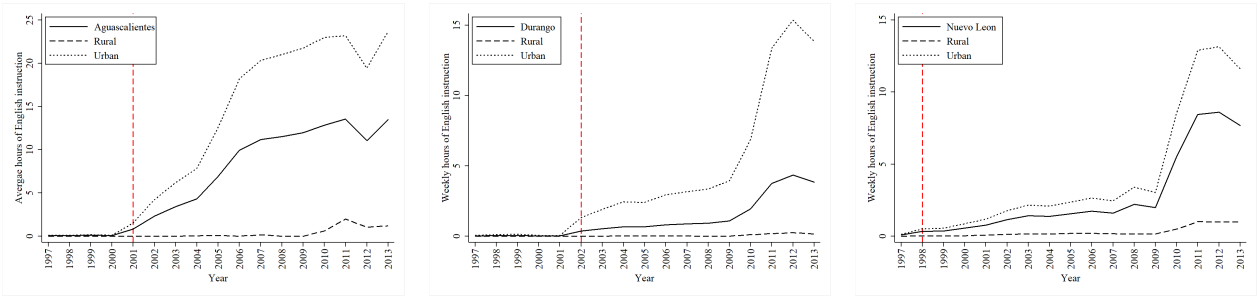
**Table A.1:** Policy changes in Mexican states

| State | Year of impl. | Policy change | Cohorts affected | Hrs of English | | Policy details |
|---|---|---|---|---|---|---|
| | | | | Before policy | After policy | |
| Nuevo Leon | 1993 | 1998 | 1987-1996 | 0.97 | 2.75 | Only sixth grades |
| Sonora | 1993 | 2004 | 1993-1996 | 1.64 | 5.52 | Only 1st and 2nd grades |
| Tamaulipas | 2001 | 2001 | 1990-1996 | 1.21 | 2.89 | Only fourth grades |
| Aguascalientes | 2001 | 2001 | 1990-1996 | 2.36 | 8.13 | No info. available |
| Durango | 2002 | 2002 | 1991-1996 | 0.33 | 1.00 | Started w/trial stage |
| Sinaloa | 2004 | 2004 | 1993-1996 | 0.70 | 1.86 | No info. available |

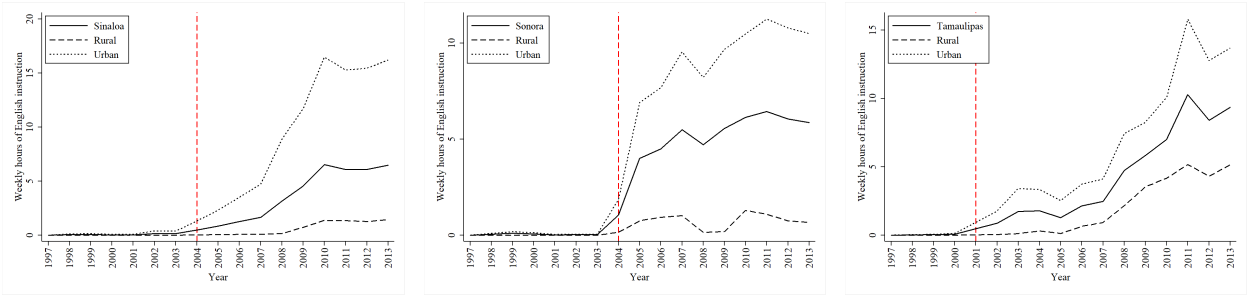
*Note:* The reported hours of English instruction refer to the average hours per class reported by the school in the years before and after the policy change.

*Source:* I computed the hours of English instruction using the Mexican school census (Statistics 911). Policy details from Nuevo Leon, Sonora, and Tamaulipas were obtained from their respective websites (see subsection 1.2 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:** Mexican states with English programs



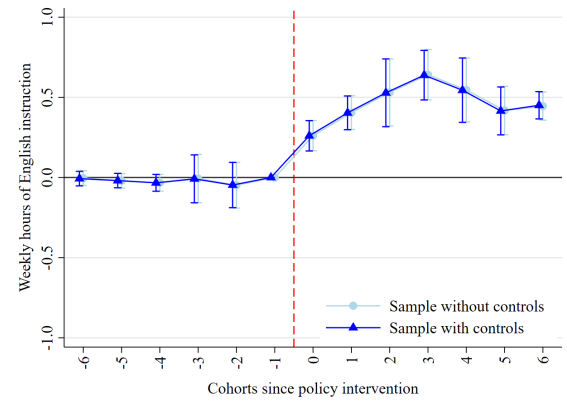

**(a)** Aguascalientes **(b)** Durango **(c)** Nuevo Leon



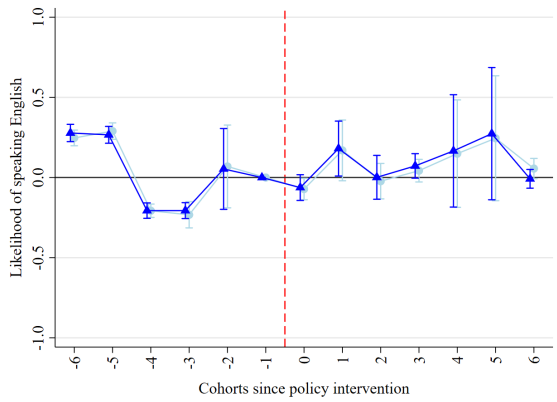

**(d)** Sinaloa **(e)** Sonora **(f)** Tamaulipas

*Note:* Average weekly hours of English instruction are plotted. The data used comes from the administrative records in the Mexican school census. The vertical dotted lines represent the year of the policy change that introduced/expanded English instruction in public primary schools.

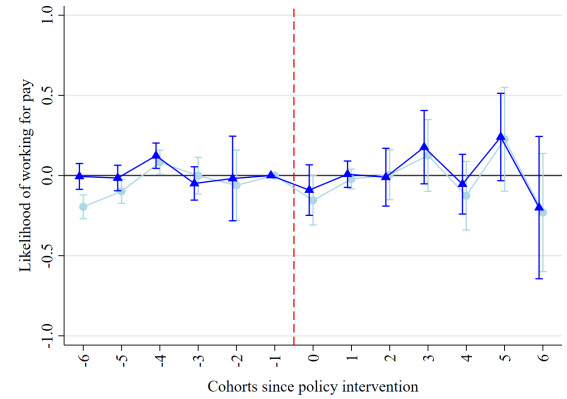
**Figure A.4:** Event-study graphs from TWFE specification



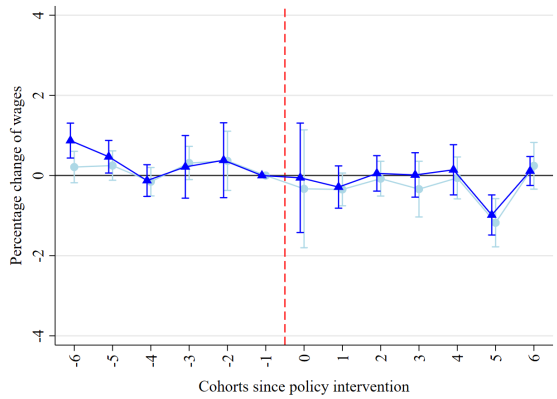

**(a)** Hours of English



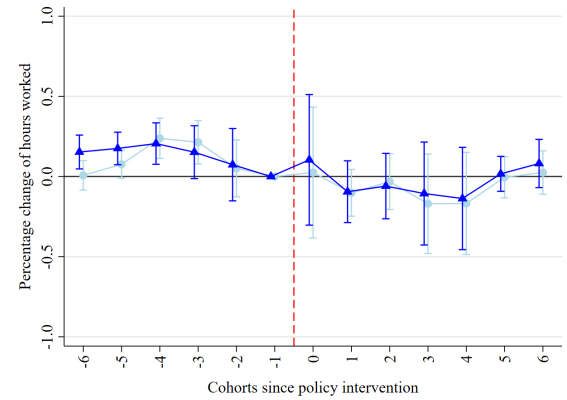

**(b)** Speak English



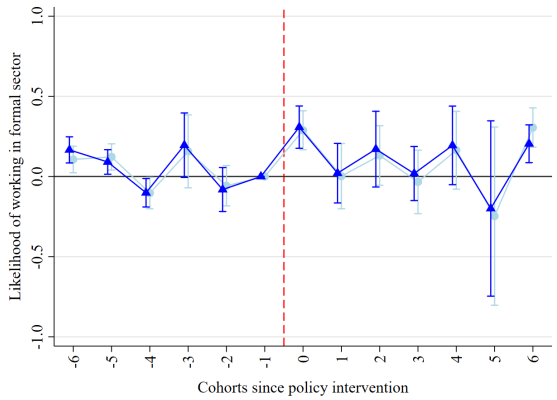

**(c)** Paid work




**(d)** Ln(wage)



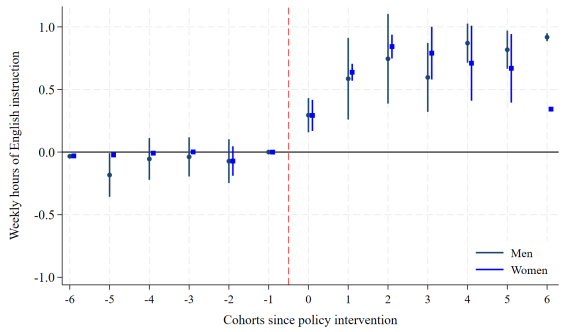

**(e)** Labor supply



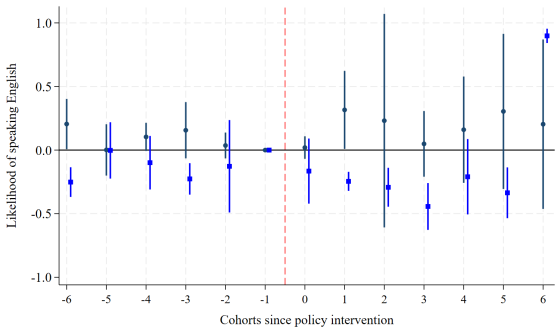

**(f)** Formal job

*Note:* This figure plots the TWFE estimates for the interaction terms between the treatment variable and an indicator function of each cohort since the policy intervention, in an event study type regression. The omitted period is -1. 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. Confidence intervals at 95% level.

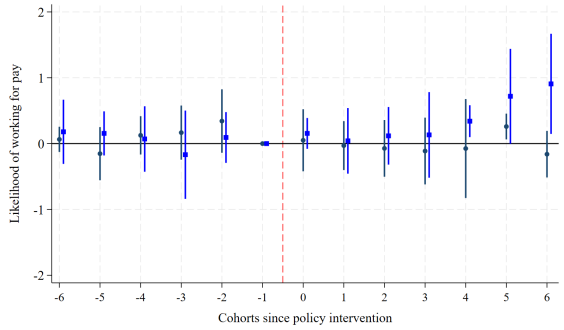
**Figure A.5:** Event-study graphs with robust estimates in the presence of heterogeneous treatment effects (by sex)



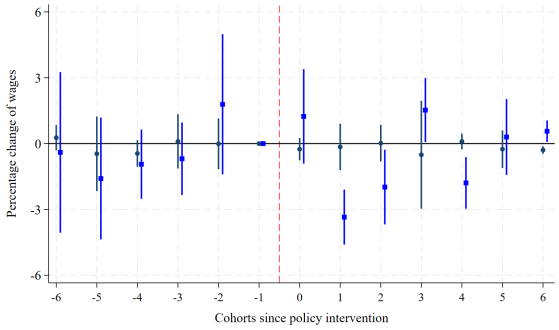

**(a)** Hours of English



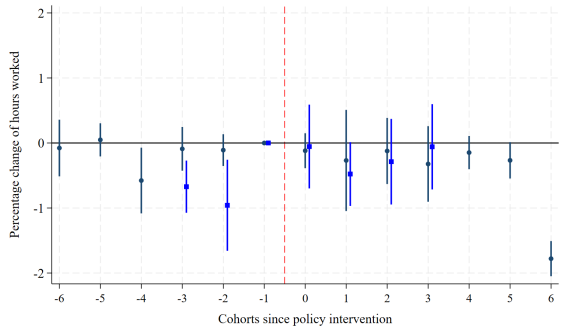

**(b)** Speak English



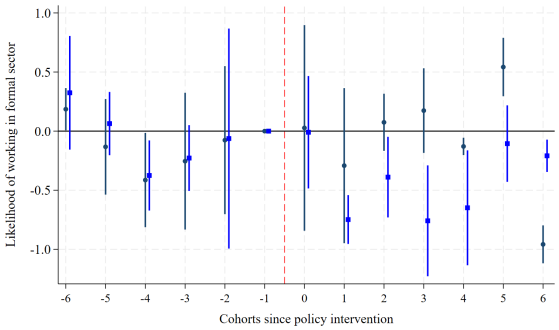

**(c)** Paid work




**(d)** Ln(wage)



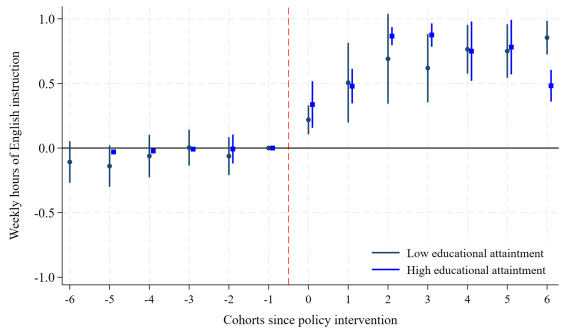

**(e)** Labor supply



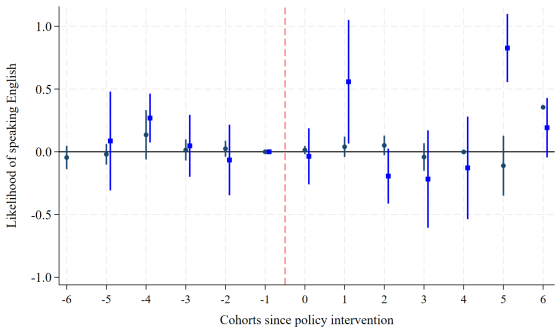

**(f)** Formal job

*Note:* This figure plots robust estimates in the presence of heterogeneous treatment effects, obtained using the method proposed by de Chaisemartin and D'Haultfoeuille (2020). Each estimate comes from the interaction between the treatment variable and an indicator function of each cohort since the policy intervention, in an event study type regression. The omitted period is -1. 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. Confidence intervals at 95% level.

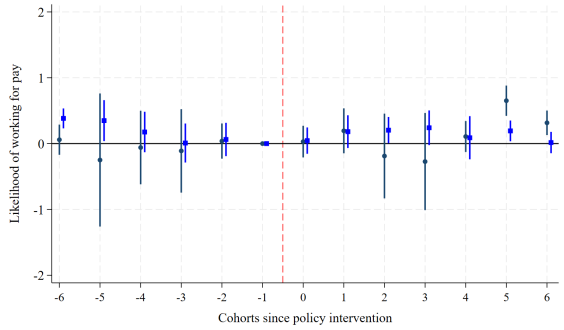
**Figure A.6:** Event-study graphs with robust estimates in the presence of heterogeneous treatment effects (by educational attainment)



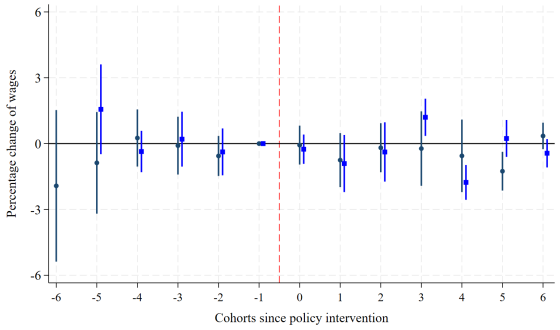

**(a)** Hours of English



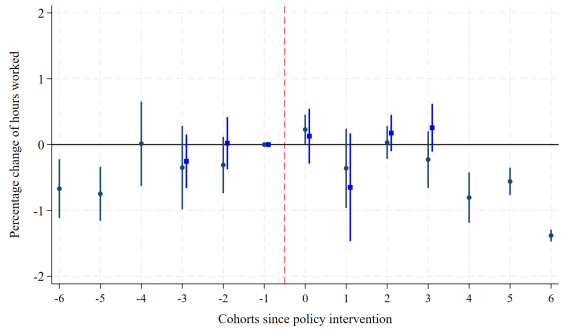

**(b)** Speak English



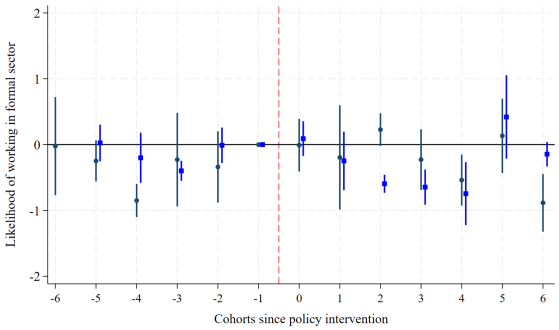

**(c)** Paid work




**(d)** Ln(wage)



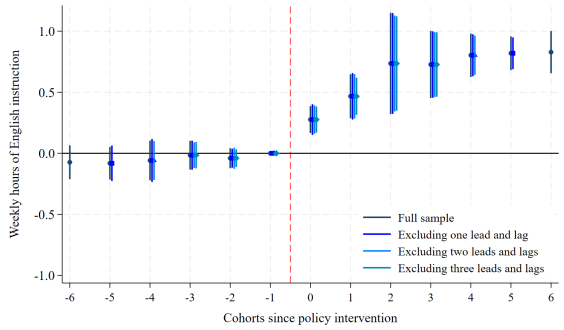

**(e)** Labor supply



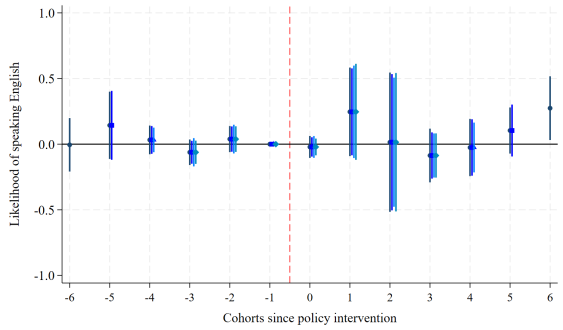

**(f)** Formal job

*Note:* This figure plots robust estimates in the presence of heterogeneous treatment effects, obtained using the method proposed by de Chaisemartin and D'Haultfoeuille (2020). Each estimate comes from the interaction between the treatment variable and an indicator function of each cohort since the policy intervention, in an event study type regression. The omitted period is -1. 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. Confidence intervals at 95% level.

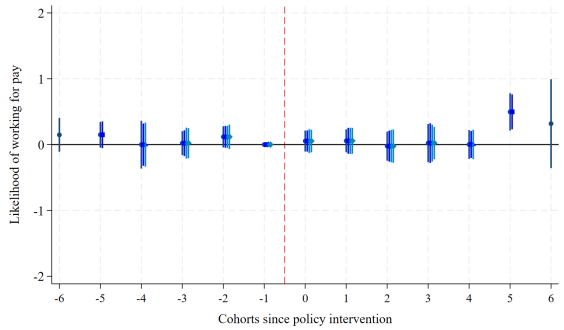
**Figure A.7:** Event-study graphs from sensitivity analysis by leads and lags



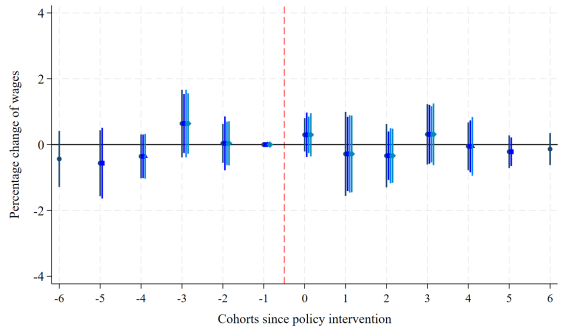

**(a)** Hours of English



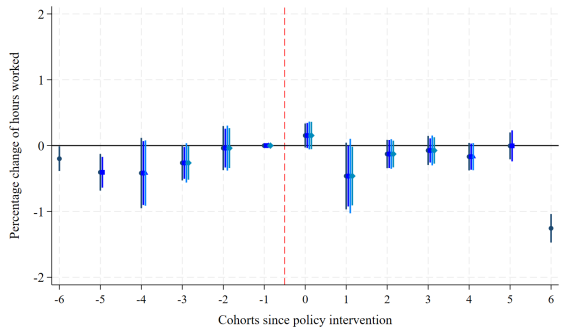

**(b)** Speak English



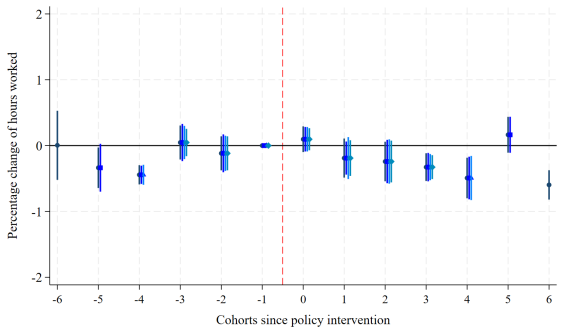

**(c)** Paid work




**(d)** Ln(wage)



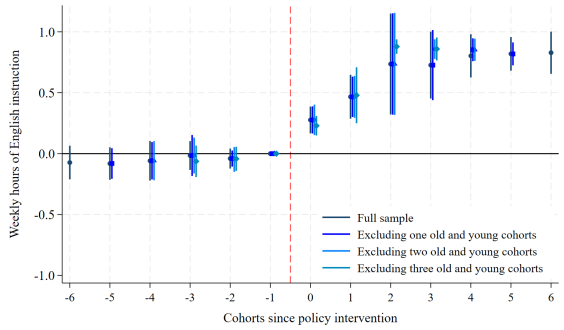

**(e)** Labor supply



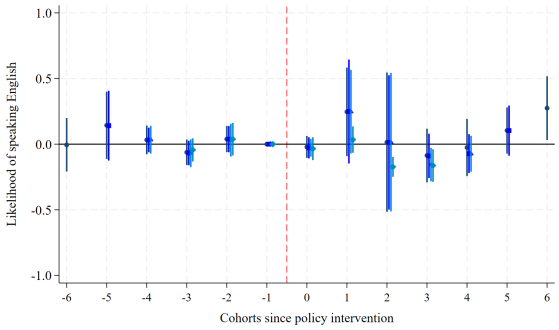

**(f)** Formal job

*Note:* This figure plots robust estimates in the presence of heterogeneous treatment effects, obtained using the method proposed by de Chaisemartin and D'Haultfoeuille (2020). Each estimate comes from the interaction between the treatment variable and an indicator function of each cohort since the policy intervention, in an event study type regression. The omitted period is -1. 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. Confidence intervals at 95% level.

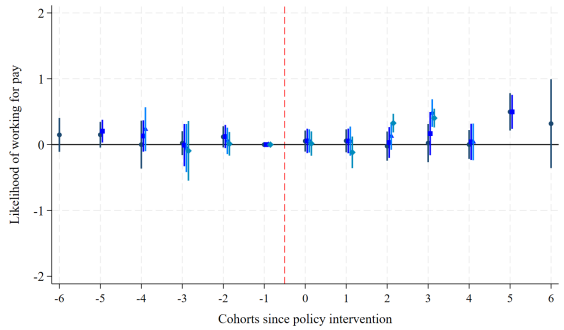
**Figure A.8:** Event-study graphs from sensitivity analysis by cohorts



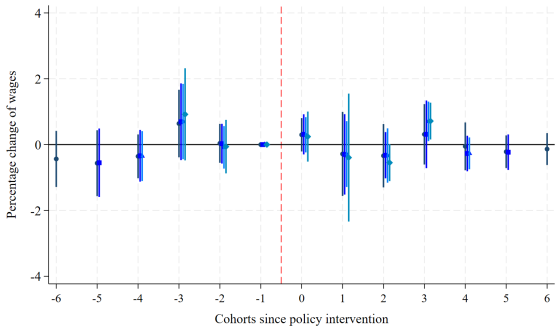

**(a)** Hours of English



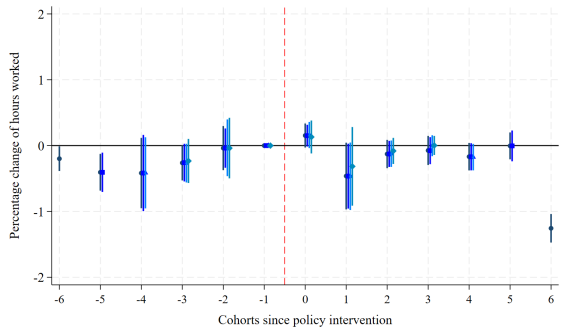

**(b)** Speak English



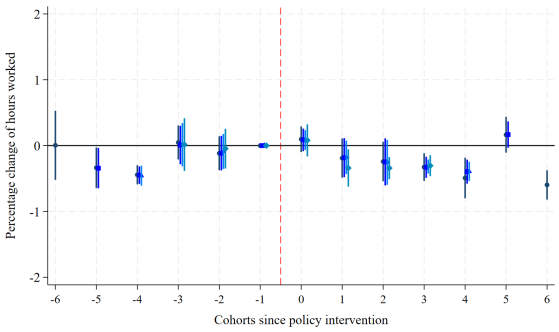

**(c)** Paid work




**(d)** Ln(wage)



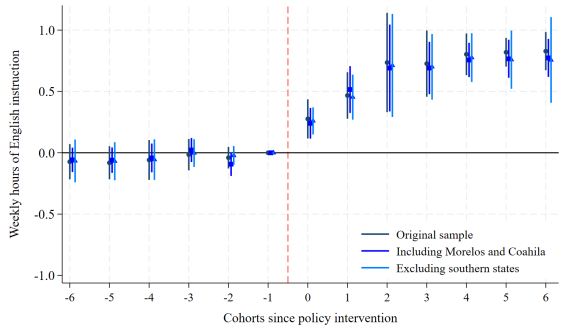

**(e)** Labor supply



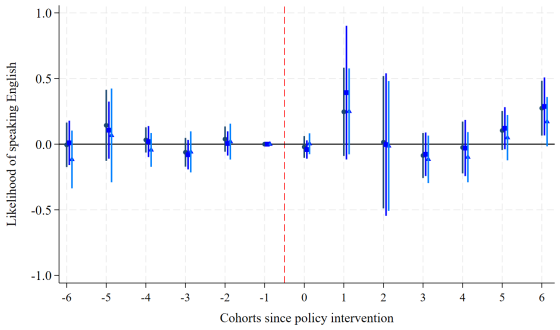

**(f)** Formal job

*Note:* This figure plots robust estimates in the presence of heterogeneous treatment effects, obtained using the method proposed by de Chaisemartin and D'Haultfoeuille (2020). Each estimate comes from the interaction between the treatment variable and an indicator function of each cohort since the policy intervention, in an event study type regression. The omitted period is -1. 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. Confidence intervals at 95% level.

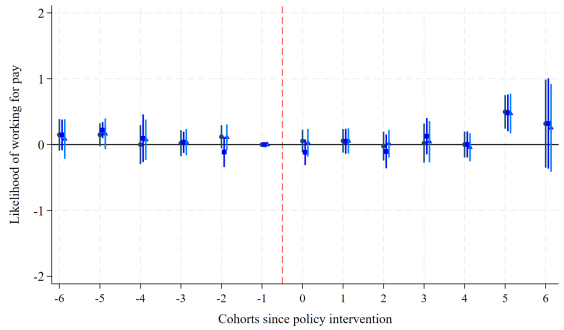
**Figure A.9:** Event-study graphs from sensitivity analysis by comparison states



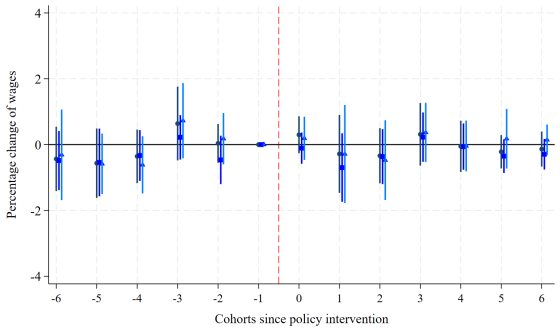

**(a)** Hours of English



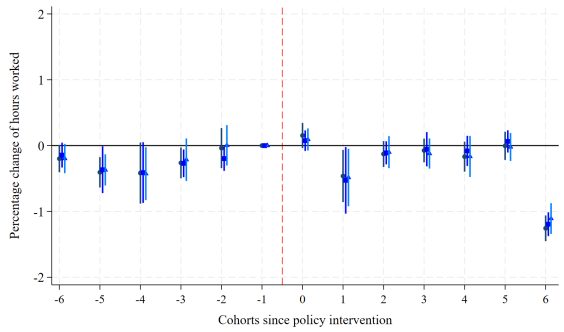

**(b)** Speak English



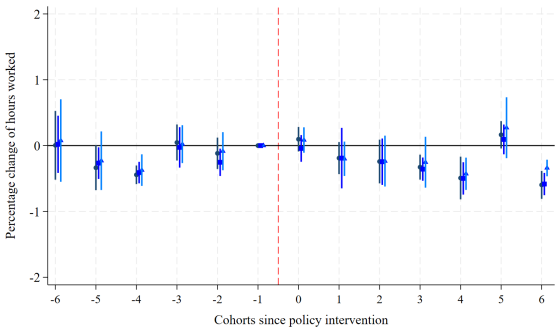

**(c)** Paid work




**(d)** Ln(wage)



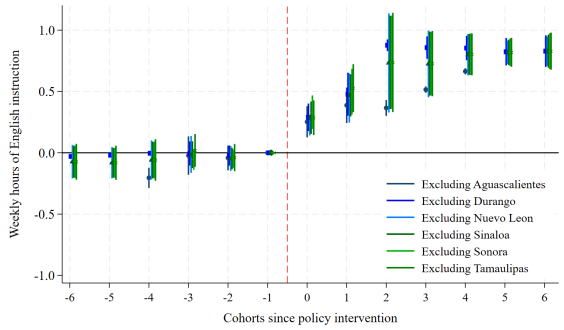

**(e)** Labor supply



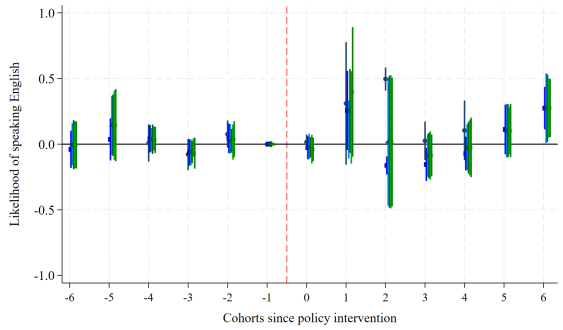

**(f)** Formal job

*Note:* This figure plots robust estimates in the presence of heterogeneous treatment effects, obtained using the method proposed by de Chaisemartin and D'Haultfoeuille (2020). Each estimate comes from the interaction between the treatment variable and an indicator function of each cohort since the policy intervention, in an event study type regression. The omitted period is -1. 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. Confidence intervals at 95% level.

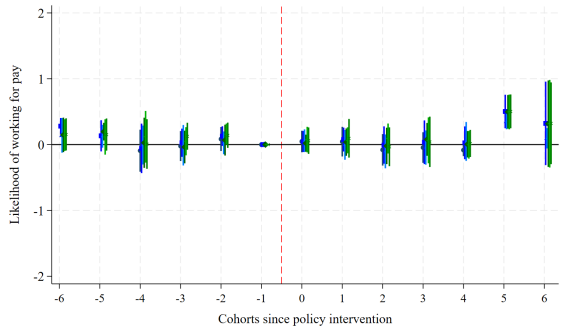
**Figure A.10:** Event-study graphs from sensitivity analysis by treated states



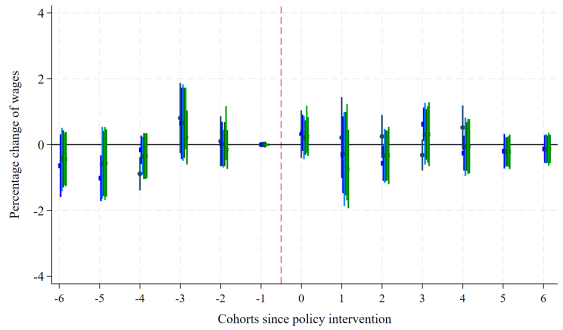

**(a)** Hours of English



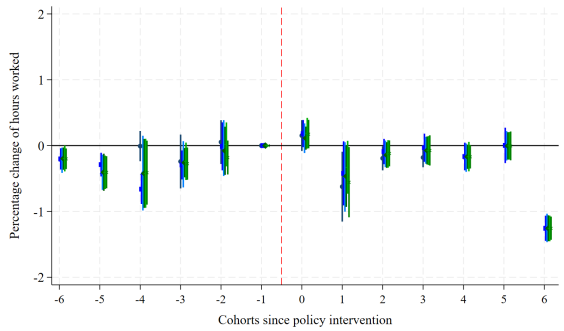

**(b)** Speak English



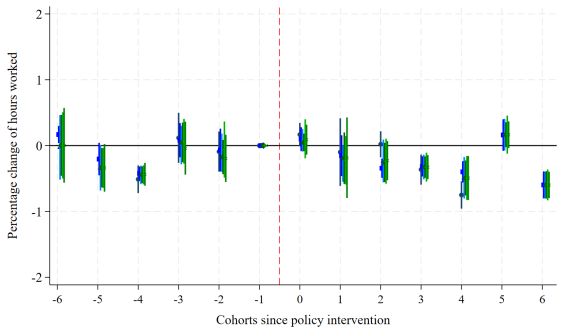

**(c)** Paid work




**(d)** Ln(wage)



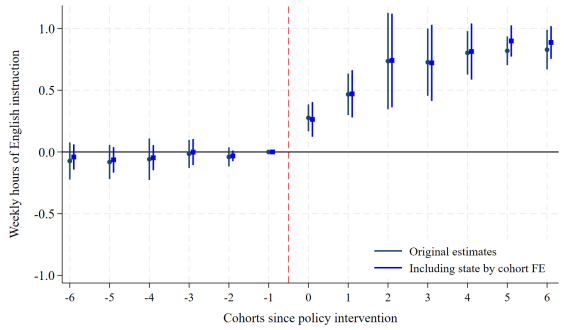

**(e)** Labor supply



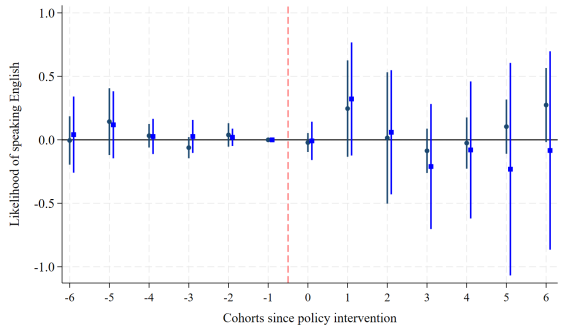

**(f)** Formal job

*Note:* This figure plots robust estimates in the presence of heterogeneous treatment effects, obtained using the method proposed by de Chaisemartin and D'Haultfoeuille (2020). Each estimate comes from the interaction between the treatment variable and an indicator function of each cohort since the policy intervention, in an event study type regression. The omitted period is -1. 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. Confidence intervals at 95% level.

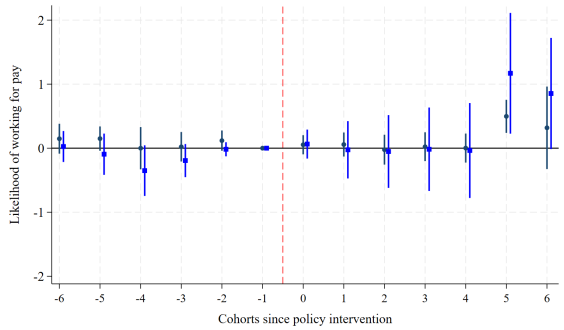
**Figure A.11:** Event-study graphs with state trends



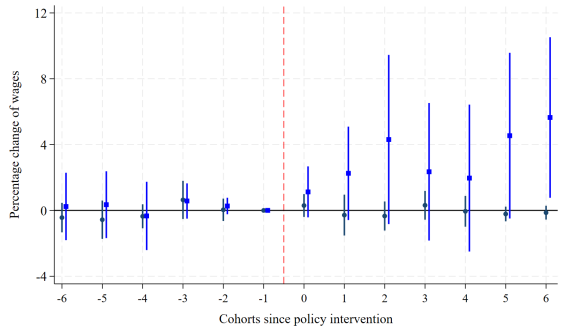

**(a)** Hours of English



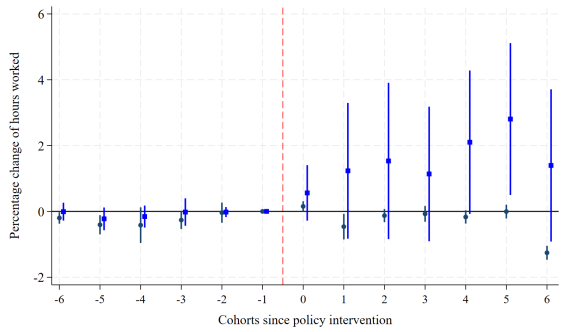

**(b)** Speak English



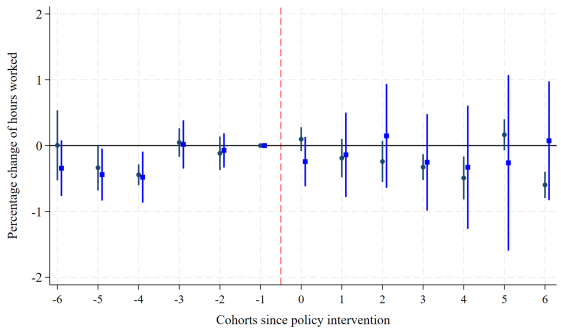

**(c)** Paid work




**(d)** Ln(wage)




**(e)** Labor supply




**(f)** Formal job

*Note:* This figure plots robust estimates in the presence of heterogeneous treatment effects, obtained using the method proposed by de Chaisemartin and D'Haultfoeuille (2020). Each estimate comes from the interaction between the treatment variable and an indicator function of each cohort since the policy intervention, in an event study type regression. The omitted period is -1. 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. Confidence intervals at 95% level.

**Table A.2:** 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 | 6.99 | 8.65 | 6.71 | 1.95*** |
| *By gender* | | | | |
| Male | 9.46 | 11.24 | 9.16 | 2.08*** |
| Female | 5.06 | 6.58 | 4.81 | 1.78*** |
| *By age* | | | | |
| 18-35 | 8.27 | 11.25 | 7.80 | 3.45*** |
| 36-50 | 7.01 | 8.24 | 6.79 | 1.44* |
| 51-65 | 4.46 | 4.75 | 4.41 | 0.34 |
| *By educational attainment* | | | | |
| Incomplete primary (0-5 years) | 0.82 | 0.49 | 0.85 | -0.36 |
| Primary school (6 years) | 1.48 | 1.85 | 1.43 | 0.42 |
| Lower secondary (7-9 years) | 2.42 | 2.92 | 2.32 | 0.60 |
| Upper secondary (10-12 years) | 7.45 | 7.10 | 7.51 | -0.41 |
| College or higher (13-24 years) | 21.91 | 24.94 | 21.28 | 3.65** |
| *By ethnicity* | | | | |
| Indigenous | 2.46 | 5.35 | 2.36 | 2.98 |
| Non-indigenous | 7.28 | 8.70 | 7.03 | 1.67*** |
| *By geography* | | | | |
| Urban | 8.26 | 9.55 | 8.02 | 1.54*** |
| Rural | 2.30 | 3.28 | 2.19 | 1.09** |

*Note:* The sample consists of Mexicans ages 18–65 who self-reported their ability to speak in English. Data from the 2014 Mexican Subjective Well-being Survey (BIARE). The full sample is composed by 33,512 observations. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

**Table A.3:** Effect of English programs and heterogeneity by sex and education

| | (1) Hrs Eng | (2) Speak Eng | (3) ln(wage) | (4) Paid work | (5) Labor supply | (6) Formal work |
|---|---|---|---|---|---|---|
| *Panel A: Men* | | | | | | |
| Had Policy | 0.647*** | 0.186 | -0.213 | -0.027 | -0.261 | 0.026 |
| | (0.092) | (0.121) | (0.319) | (0.153) | (0.215) | (0.146) |
| Observations | 2,643 | 2,643 | 2,643 | 3,533 | 2,534 | 2,643 |
| *Panel B: Women* | | | | | | |
| Had Policy | 0.504*** | -0.207** | -0.388 | 0.200 | -0.163 | -0.307*** |
| | (0.002) | (0.080) | (0.240) | (0.165) | (0.199) | (0.139) |
| Observations | 1,905 | 1,905 | 1,905 | 4,565 | 1,744 | 1,905 |
| *Panel C: Low educational attainment* | | | | | | |
| Had Policy | 0.507*** | 0.015 | -0.388 | 0.023 | -0.266** | -0.131 |
| | (0.086) | (0.024) | (0.260) | (0.151) | (0.120) | (0.158) |
| Observations | 2,580 | 2,580 | 2,580 | 4,417 | 2,405 | 2,580 |
| *Panel D: High educational attainment* | | | | | | |
| Had Policy | 0.482*** | 0.192 | -0.440 | 0.149** | -0.089 | -0.146 |
| | (0.063) | (0.121) | (0.330) | (0.072) | (0.196) | (0.095) |
| Observations | 1,968 | 1,968 | 1,968 | 3,681 | 1,873 | 1,968 |

*Note:* This table shows the effect of state English programs on labor market outcomes. The sample consists of Mexicans who were born between 1984 and 1994. Controls include gender, indigenous people dummy, marital status, education fixed effects, cohort fixed effects, and locality fixed effects. The estimates are obtained using the method proposed by de Chaisemartin and D'Haultfoeuille (2020). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$