Immigrant Integration in the United States: The Role of Adult English Language Training

Blake Heller and Kirsten Slungaard Mumma*

Harvard University

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Abstract

While current debates center on whether and how to admit immigrants to the United States, little attention has been paid to interventions designed to help them integrate after they arrive. Public adult education programs are the primary policy lever for building the language skills of the over 23 million adults with limited English proficiency in the United States. We leverage the enrollment lottery of a publicly funded adult English for Speakers of Other Languages (ESOL) program in Massachusetts to estimate the effects of English language training on voting behavior and employer-reported earnings. Attending ESOL classes more than doubles rates of voter registration and increases annual earnings by $2,400 (56%). We estimate that increased tax revenue from earnings gains fully pay for program costs over time, generating a 6% annual return for taxpayers. Our results demonstrate the social value of post-migration investments in the human capital of adult immigrants.

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1 Introduction

Current debates on immigration policy in the United States center on how many immigrants should be allowed to enter the country and how those immigrants are selected. Advocates of so-called merit-based immigration policies favor granting visas to adult immigrants with high levels of pre-migration human capital, including educational attainment, technical expertise, and language skills (Alvarez, 2017; Hatch, 2018; Ingber & Martin, 2019). However, debates about "low" and "high" skilled immigration largely ignore the possibility of improving adult immigrants' skills after they arrive. In this paper, we assess the returns to post-migration investments in a particular type of human capital: English language skills.

In the United States and around the world, the ability to speak and understand a host country's primary language is strongly associated with measures of immigrant integration. Language skills are complementary to other forms of human capital, enhancing an immigrant's ability to transfer pre-migration knowledge, skills, and experience across national borders (Khan, 1997; Berman, Lang, & Siniver, 2003; Chiswick & Miller, 2007). Examining differences in earnings across seven developed countries, Chiswick and Miller (2015) find that host country language fluency is associated with a 5% to 30% wage premium, conditional on other observable characteristics. Language skills are also related to measures of social and civic incorporation, including having relationships outside one's ethnic group, becoming a naturalized citizen, and being politically engaged (Cho, 1999; Bleakley & Chin, 2010; Slungaard Mumma, 2020).

Despite these economic and social benefits, more than 23 million adults in the United States lack proficiency in the English language (U.S. Census Bureau, 2018a). Since 1990, the limited English proficient (LEP) population in the country has grown by over 80%, representing about 9% of the adult population today (Zong & Batalova, 2015). Both the incoming level of English proficiency and the rate at which new immigrants acquire English skills have declined since the mid-twentieth century (Carliner, 2000; Borjas, 2015).

Public adult education programs are the primary source of governmental investment in the skills of adult immigrants in the United States, providing low-cost or no-cost English language instruction to adult learners outside the traditional K-12 and higher education systems. Every year, these programs serve about 700,000 students in English for Speakers of Other Languages (ESOL[1]) classes, a fraction of the population that could benefit from English language training (U.S. Department of Education, 2017). Demand for ESOL services exceeds supply at programs across the country. In 2017, roughly 11,000 English learners enrolled in ESOL programs in Massachusetts while 17,000 more remained on program waitlists. Wait times at popular programs can exceed two years. Despite sustained demand for ESOL services and rapid growth of the target population, adult education has

[1]ESOL (English for Speakers of Other Languages) and ESL (English as a Second Language) are used interchangeably in adult education. In this paper, we use the term "ESOL", which is preferred by the Massachusetts Department of Elementary and Secondary Education.

been largely ignored by policymakers as a tool for immigrant integration, remaining "a neglected backwater of our education system" (Chisman, Wrigley, & Ewen, 1993). Since 1990, public funding for adult education has declined by about 30% in real dollars despite the near doubling of the LEP population (U.S. Department of Education, 2018).[2] Over that same period, real public spending on elementary and secondary education grew by over 60% (Ibid).

In this paper, we show that post-migration investments in the human capital of adult immigrants can generate substantial public returns. Specifically, we reconstruct eight years of twice-annual randomized enrollment lotteries for one of the largest adult ESOL programs in Massachusetts (Framingham Adult ESL Plus) to identify the impact of ESOL services on voter registration, voter participation, and employer-reported earnings. Our sample includes over 4,700 individuals who applied to this program for the first time between fall 2008 and spring 2016, and we observe applicants for up to ten years after their first lottery attempt.

We find positive effects of attending adult ESOL classes on measures of civic engagement and employer-reported earnings. Attending adult ESOL classes increases voter registration by 9 percentage points, more than doubling participants' probability of being a registered voter or casting a vote. The effects on voting are large, on par with the effects of social pressure mailing campaigns and in-person canvassing interventions (Gerber & Green, 2000; Gerber, Green, & Latimer, 2008). We find particularly strong effects on voting in 2016, when restrictive immigration policies were a cornerstone of then-candidate Donald Trump's campaign. While we are unable to observe citizenship status in our data, our effect on new voter registration may partially reflect the program's impact on the probability a participant becomes a naturalized citizen.

Attending ESOL classes also has large, positive, persistent effects on employer-reported earnings. Beginning two years after their first lottery application, individuals who are induced to enroll report $2,400 more in annual earnings, about 56% more than the control group. Participants are three times as likely to report middle-class earnings of $60,000-$70,000 in any year. The effects on annual earnings are roughly equivalent to the average increase in employer-reported earnings we observe over two additional years in the United States for the control group.[3] The effects on reported earnings are strongest for individuals with a record of pre-lottery reported earnings and for those with higher levels of baseline English ability, suggesting the returns to language learning are highest for those with higher levels of pre-existing human capital.

Our results are robust to a variety of alternative specifications designed to address concerns about missing data, endogenous mobility, and other threats to internal validity. We show our

[2]In 2015-16, total public spending on adult education in the United States was just under $2 billion (U.S. Department of Education, 2018).

[3]From year 0 to 5, where year 0 is the year an individual first applied to a lottery, the cross-sectional increase in average employer-reported earnings for individuals in the control group with any reported income was $6,005, implying a $1,248 increase in earnings for every additional year in the U.S.. This is equivalent to about the half the size of our effect on average annual earnings. Estimates are similar if we consider earnings growth over other ranges (e.g., years 0-6, 0-7, etc.).

results pass a number of placebo tests for effects on pre-lottery outcomes. In addition, we conduct a series of checks to address concerns about out-of-state mobility. First, we match our applicants to out-of-state voting records from four other states, finding limited evidence of out-of-state mobility and no evidence that this varies by lottery status. Second, we show that our results are not driven by differential "stopping out" behavior that would be consistent with substantial out-of-state mobility for individuals with stable earnings histories.

Finally, we conduct a cost-benefit analysis to calculate the public returns to investments in adult English instruction based on increased tax revenue. Our estimates imply a six percent positive net return to taxpayers from public investments in adult ESOL programs, implying an infinite marginal value of public funds (MVPF) at or below a six percent discount rate (Hendren & Sprung-Keyser, 2020). We note that this rate of return, which likely underestimates the full social benefits of adult ESOL by ignoring differences in outcomes other than tax payments, is similar to the historical returns to equity and just below the estimated returns to investments in early childhood education (Heckman et al., 2010). We conclude that public investments in the human capital of adult immigrants after arrival can have positive, meaningful private and social benefits.

Our research contributes to several literatures. Most broadly, we contribute to the literature on immigrant integration in the United States (e.g., Bloemraad, 2006; Borjas 2008, 2015). More specifically, we contribute to the literature on immigrant language skills (e.g., Dustmann & Fabbri, 2003; Chiswick & Miller, 2007; Yao & van Ours, 2015). Since language skills are endogenous, there is limited causal or quasi-experimental research on the returns to language ability. Bleakley and Chin (2004) identify the effect of English language skills on the adult outcomes of childhood immigrants by instrumenting for language skills using an interaction between age at arrival and coming from a non-English speaking country, exploiting variation in language skills for immigrants who arrive before and after the "critical period" of language acquisition. The authors find that increasing English proficiency by one level[4] raises earnings by over 30% and increases educational attainment by four years (2004). Bleakley and Chin also find that English ability increases measures of social assimilation (2010) and has positive spillover effects on the English skills and preschool attendance of children in immigrant families (2008). While a large body of research has considered the effects of language policies for children in American public schools (e.g., Angrist, Chin, & Godoy, 2008; Chin, Daysal, & Imberman, 2013; Kuziemko, 2014; Lleras-Muney & Shertzer, 2015), few well-identified studies have considered the effects of adult language instruction, and all are based on programs outside the United States (e.g., Sarvimäki & Hämäläinen, 2016; Lochman, Rapoport, & Speciale, 2019; Arendt et al., 2020). The magnitude of the earnings effects we observe are consistent with global evidence on the effects of host country language training for immigrants.

Finally, our study contributes to the broader literature on adult training and education in

[4]Bleakley and Chin use an ordinal measure of English ability based on the U.S. Census language question: 0="Speaks English not at all"; 1="Speaks English not well", 2="Speaks English well", 3="Speaks English very well" or speaks English at home.

United States (e.g., Bloom et al., 1997; Heckman, 2000; Hamilton et al., 2001; Heinrich et al., 2013). There is little quantitative research on adult ESOL programs in the U.S., and the research that exists is more than two decades old. Two studies of programs in the 1980s and 1990s that used random assignment to assign individuals to job training paired with adult education classes (including adult ESOL for some participants) found positive effects on earnings and employment, though results were not broken down separately for ESOL students (Zambrowski & Gordon, 1993; Hamilton et al., 2001; Wrigley et al., 2003). Ours is the first study we are aware of that uses random assignment to study the impact of ESOL services delivered in a contemporary, business-as-usual setting—that is, operating under typical conditions with a general population of students and no additional interventions.

The remainder of this paper is organized as follows: in Section 2, we provide background on the ESOL program we study. In Section 3, we describe our data sources and present key descriptive statistics for our sample. In Section 4, we present our empirical strategy and econometric models. We present our main results and robustness checks in Section 5. In Section 6, we present results from a cost-benefit analysis. We conclude in Section 7.

2 The Framingham Adult ESL Plus Program

In 2017, there were 103 public adult education programs serving over 18,000 students in Massachusetts, 58% of whom were enrolled in ESOL classes (MCAE, 2020). Framingham Adult ESL Plus (FAESL+) is one of the largest adult education programs in the state, enrolling over 750 students each year in Framingham, Massachusetts, a mid-size city with a large Brazilian community. In addition to ESOL classes, the program also offers high school equivalency exam preparation[5] and citizenship classes. The program serves immigrants from over 30 countries with a mix of educational backgrounds, from those who did not complete secondary school to those who hold doctoral degrees.[6] Classes are offered in morning and evening sessions and are held at a local middle school or at the Brazilian-American Center, a local non-profit organization.

The focus of the FAESL+ curriculum is on increasing communication and literacy skills of its students through relevant, real-world applications. A typical FAESL+ student attends classes for six hours per week over a 15-week fall or spring semester. Students are placed in classes based on their English proficiency level, with a mix of primary languages represented in each classroom. Most first-time students are classified as beginners. Classroom activities could include learning how to share an email address, talking about the weather and days of the week, or practicing making

[5]While we do observe a handful of ESOL students enrolling in high school equivalency preparation classes at FAESL+, lottery winners are no more likely to earn a credential than non-winners, so we do not think differential access to high school equivalency preparation courses could explain the observed impacts on reported earnings or civic engagement.

[6]See `http://www.faesl.org/about.html` for more program details.

a phone call in English. While adult education instructors are not required to hold a specific credential, many hold degrees in education and have experience teaching in K-12 classrooms.

ESOL courses offered by FAESL+ are consistently oversubscribed. Between fall 2008 and spring 2016, FAESL+ received at least four new applications for every open seat. While continuing students are guaranteed a spot the following semester, admission for all other students is determined by a random lottery conducted in January and August every year. Prospective students submit an application in-person, applying to the morning or evening time slot. Evening classes, which fall outside normal working hours, host four times as many students as morning classes and receive over 80% of applications. After applications are submitted, FAESL+ staff members publicly draw lottery numbers and invite selected applicants to take a formal placement exam. Seats are allocated to students based on their level and time preference in the order in which their lottery number was drawn. If there are no more seats available, students whose lottery numbers were drawn are offered a seat in a weekly volunteer-led prep class and may join a teacher-led course if a seat becomes available in the first three weeks of class.[7] Students who do not win a spot in the FAESL+ program are encouraged to re-apply and are given information about other adult ESOL programs and volunteer-led classes in the area.[8] About a quarter of applicants in our sample who do not win a spot in the program on their first lottery attempt ultimately enroll in the FAESL+ program in the future, 2.5 semesters later, on average.

The demographics of applicants to FAESL+ reflect the characteristics of the LEP population of Framingham and nearby communities. Table 1 presents summary statistics for all students enrolled in public adult ESOL programs in Massachusetts from fall 2008 to spring 2016 (Column 1), students enrolled in the FAESL+ program over that time (Column 2), and our sample of FAESL+ program applicants who applied for the first time during that period (Column 3). Compared to the statewide student population, students in the FAESL+ program are more likely to have an identifiably white or Brazilian surname and less likely to be identified as Asian, Black, or Hispanic. FAESL+ students are also less likely to match to statewide voting files or employer-reported earnings data, as we discuss in the following section.

[7] We identify students who are offered a seat in the volunteer-led prep class as "non-winners."

[8] We observe less than 1% (42/4,761) of individuals in our lottery sample ever participating in another publicly funded ESOL program in the state. We are unable to observe participation in private, volunteer-led, or non-profit English learning programs that are not funded and overseen by the Massachusetts Department of Elementary and Secondary Education. The program does not prioritize previous lottery applicants or prep class attendants, with the exception that through spring 2016 the program had a policy that any individual who participated in five consecutive lotteries in a row without winning was guaranteed a spot in class.

3 Data and Descriptive Statistics

3.1 FAESL+ Lottery and Enrollment Records

We reconstruct lottery outcomes for individuals who applied to the FAESL+ program using three data sources: (1) statewide enrollment data for all students in public adult education programs in the state from the Massachusetts Department of Elementary and Secondary Education (MA DESE), (2) statewide waitlist records for students who applied but were not immediately offered a chance to enroll (also from MA DESE), and (3) administrative records and course lottery notes from the FAESL+ program. An individual's probability of being offered a seat in the FAESL+ program (i.e., winning the lottery in a given semester) is a function of (1) the semester they apply, (2) their incoming English proficiency level, and (3) their preference for attending a morning or evening class. By triangulating between these three administrative datasets and manually reviewing program notes, we were able to reconstruct FAESL+ ESOL lotteries for first-time applicants from fall 2008 to spring 2016, including availability (a.m./p.m.), and initial English level (beginning, intermediate, or advanced). We categorize applicants as beginning, intermediate, or advanced based on the level reported in the waitlist, initial placement test results, or initial class level assignment.[9] Table 2 presents the distribution of first-time applicants in our sample by their first application year. Our analytic sample includes 4,761 individuals (1,248 winners and 3,513 non-winners) who applied to this program between fall 2008 and spring 2016 and have non-missing date-of-birth and initial level information (see section 5.4.2 for a discussion of missing data).

Since race and ethnicity are coded inconsistently across data sources, we create a standardized indicator of (likely) race, ethnicity, or Brazilian nationality based on an individual's surname. We merge surnames in our sample to (1) a dataset produced by the U.S. Census Bureau that reports the breakdown of race and ethnicity for surnames occurring more than 100 times in the 2010 Census, and (2) a list of the most common surnames in Brazil compiled by Forebears, a genealogical website (U.S. Census Bureau, 2016; Forebears, 2019). We created indicators for having an identifiably American Indian/Native American, Asian/Pacific Islander, Black, Hispanic, or white (non-Hispanic) surname if 80% of respondents to the U.S. Census with that name belong to that racial or ethnic group.[10] We create an indicator for having a Brazilian surname if an individual has one of the 100 most common surnames in Brazil. Results were qualitatively similar under alternative specifications, such as using a 75% or 90% threshold for defining race or ethnicity, or identifying Brazilian surnames using the top 200 surnames in Brazil or the five most common surnames in Brazil, which cover 45% of all

[9] The FAESL+ program used three different placement assessments over the period of our study. Scores were equated to EFL levels based on National Reporting System for Adult Education guidelines (see `https://www.nrsweb.org/`) and mapped to levels based on Massachusetts Adult and Community Learning Services standards (see `http://www.doe.mass.edu/acls/assessment/EFL-FAQ.html`).

[10] Only 0.17% of applicants in our final analytic sample (8/4,761) have a surname that is identifiably Black (non-Hispanic) and no applicants possess a surname that is identifiably American Indian/Native American.

registrations in the Relacao Annual de Informacoes Socials (Monasterio, 2017).

Gender is coded consistently across data sources but is missing for 11.4% of individuals. To increase coverage, we impute gender for those with missing information using (1) a dataset of ∼74,000 Brazilian first names and their distribution by gender, and (2) a dataset produced by the U.S. Census Bureau that contains a sample of first names covering 90% of male and female respondents to the 1990 U.S. Census (U.S. Census Bureau, 2014; Sonnet, 2015).[11]

3.2 Outcome Data and Match Rates

Massachusetts voter registration and participation data were purchased from NationBuilder. The voting file contains name, date of birth (DOB), year and month of registration, and election participation from 2000 to 2017 for all currently registered voters in the state of Massachusetts as of December 2018. We find that 10% of individuals in our sample registered to vote, matching by name and DOB. The match rate for our sample—and enrolled FAESL+ students overall—is about half the match rate of all ESOL students in the state (22%), perhaps because the FAESL+ program serves a larger share of undocumented immigrants, immigrants whose visa category makes them ineligible for naturalization, or more recent immigrants than other programs in the state.

Employer-reported earnings data in Massachusetts were provided by the Massachusetts Department of Unemployment Assistance (MA DUA). These data include quarterly earnings (by employer), employer zip codes, and industry codes covering the period from January 2010 to September 2019. We merge lottery applicants and statewide ESOL program participants to MA DUA data using name and date of birth through a process facilitated by MA DESE. Individuals in our lottery sample report earnings from employers with 177 unique four-digit NAICS industry codes. Restaurants, services to buildings and dwellings, grocery stores, department stores, skilled nursing facilities, and individual and family services account for 49% of quarterly earnings observations. The mean annual reported earnings for individuals with non-zero reported earnings in our sample is $27,140. Overall, we match 24% of individuals in our sample to employer-reported earnings for at least one quarter. The match rate for our sample and FAESL+ students overall is below the statewide ESOL student match rate of 45%, similar to the proportional difference in match rates for voting records.

Earnings data from MA DUA represent a fraction of all income earned by individuals in our sample. While 72% of enrolled students who responded to an entry questionnaire reported being employed at baseline, we matched only 29% of enrolled students to MA DUA records. DUA-reported earnings do not cover all types of income, including income earned from self-employment, contract

[11] We assign individuals with missing gender data to male (female) status if 90% of individuals with that first name report that gender in the Brazilian dataset. Of the remainder, we assign individuals to male (female) if their name appears on the gendered lists of Census first names, using the higher-ranked gender in the rare case of names that appear on both lists.

labor, small farms, the federal government, or working for one's spouse or child. In addition, since earnings are matched based on social security numbers extracted from Massachusetts Registry of Motor Vehicles (MA RMV) records, only individuals who have ever had a Massachusetts driver's license or state identification card can match to reported earnings records.[12] Finally, MA DUA earnings records do not include wages paid "under the table" (i.e., without being reported for tax purposes). This includes most wages paid to undocumented immigrants as well as wages paid but not reported for informal or off-the-books jobs where immigrant labor is overrepresented (Losby et al., 2002). For these reasons, we are careful to interpret effects on earnings as effects on employer-reported earnings and not total income.

3.3 Balance Tests and First-Stage Estimates

To assess whether we successfully reconstructed FAESL+ enrollment lotteries, we test whether lottery outcomes predict the observable characteristics of applicants. Table 3 reports results from a balance test for baseline covariates by lottery outcome. Column (1) presents the mean of each covariate for applicants who did not win their first lottery attempt. Column (2) presents the estimate of the coefficient on "won lottery" from separate regressions where the characteristic listed on the left is regressed on an indicator for an individual having won their first lottery attempt and lottery group fixed effects (first semester applied interacted with level and availability). There are no significant differences in characteristics between the treatment and control groups. At the bottom of Panel A, we present the p-value from an F-test of the joint significance of all of the coefficients in Panel A, conditional on lottery fixed-effects. The results of the joint F-test suggest our pooled lottery sample is balanced along observable dimensions. In appendix Table A1, we present F-tests conducted separately for each of the 16 lotteries we reconstruct. Of these lotteries, 14 pass the F-test at the 5% level.

Next, we assess whether lottery outcomes predict program participation and enrollment intensity. Panel B of Table 3 shows the first-stage effects of winning one's first lottery attempt on FAESL+ enrollment, the number of terms enrolled, and number of hours attended. ESOL program applicants who win their first lottery attempt are about 50 percentage points more likely to ever participate in the FAESL+ program, enroll for 1.6 additional terms, and attend an additional 125 hours of ESOL classes. The first-stage effects reflect the fact that some applicants re-apply if they do not win their first lottery attempt and others win access to a spot but do not enroll. In our sample, 24.4% of the control group eventually enrolled in the FAESL+ program, and 19.6% of first-time lottery winners never appeared in enrollment records.[13]

[12]We submitted a list of all combinations of names and dates of birth we observed for an individual to MA DUA via MA DESE. MA DUA linked names and dates of birth to social security numbers by matching to records in the MA RMV, then used social security numbers pulled from MA RMV data to merge in earnings data. Undocumented immigrants in Massachusetts were unable to get a driver's license as of 2019.

[13]Individuals who attend the FAESL+ program for <12 hours of instruction are not reported as enrolled students

While we cannot observe program impacts on language skills directly, we expect these differences in program participation to meaningfully improve adult students' English language skills. Treatment-on-the-treated (TOT) estimates imply that individuals who are induced to enroll by winning their first lottery enroll for just over three semesters on average.[14] Assuming a student attends all classes, this represents an incremental 216 hours of instruction, just under the time it takes an average adult student to advance two proficiency levels under the National Reporting System (McHugh, Gelatt, & Fix, 2007).[15] For a student beginning at the lowest level of English proficiency, with no ability to read, write, or speak in English, advancing to level 3 on the state standards for English proficiency corresponds to being able to read and complete basic forms, understand a basic news report, and leave a coherent phone message for a child at school (MA DESE, 2019).

4 Empirical Strategy

We want to measure the effects of FAESL+ attendance on voter registration, voter participation, and employer-reported earnings, which we express as follows:

$$Y_i = \beta_0 + \beta_1 Attend_i + \beta_2 X_i + \theta_{clt} + \epsilon_{iclt}, \tag{1}$$

where Y_i is the outcome of individual i; $Attend_i$ is an indicator that is equal to one if individual i ever attended the FAESL+ program; X_i is a vector of individual-level covariates (i.e., age at lottery, imputed race or ethnicity, imputed Brazilian nationality, and gender); and θ_{clt} is a vector of lottery fixed-effects interacting first semester applied c with the student's initial ESOL level l (beginning, intermediate, or advanced), and the individual's time availability t (i.e., AM or PM).[16] OLS estimates of β_1 will be biased if program attendance or enrollment is associated with unobserved factors such as individual motivation, ability, or persistence. To obtain unbiased estimates of β_1, we instrument for $Attend$ using a binary indicator that is equal to one if an individual won his or her first lottery attempt (Won). The first stage equation is:

$$Attend_i = \delta_0 + \delta_1 Won_i + \delta_2 X_i + \nu_{clt} + \upsilon_{iclt}. \tag{2}$$

in the state adult education reporting system; these students would be classified as "no shows" in our results.

[14] We estimate this parameter directly, but it can be inferred from the ratio of the first two first-stage estimates in panel B of Table 3 (i.e., 1.62/0.503)

[15] The authors estimate that the average adult takes 110 hours of instruction to advance on English proficiency level as defined by the National Reporting System, the basis for the MA DESE standards.

[16] While courses are offered at more granular sub-levels of English ability (e.g., "low beginner," "high beginner"), the three broad categories of English ability were the primary determinants of an applicant's probability of receiving an offer to enroll and were used by administrators to manage waitlist admissions. In some cases, classes for advanced courses are not oversubscribed and all interested students are admitted. These students do not contribute identifying variation to our estimates of program effects.

Given random assignment of lottery outcomes, we obtain unbiased treatment-on-the-treated (TOT) estimates of β_1 for individuals who are induced to enroll or not enroll at FAESL+ as a result of their lottery outcome (i.e., compliers) from the second-stage equation:

$$Y_i = \beta_0 + \beta_1 \widehat{Attend}_i + \beta_2 X_i + \theta_{clt} + \epsilon_{iclt}, \tag{3}$$

where $\widehat{Attend}_i$ is the predicted value of $Attend_i$, estimated from equation (2).[17]

To estimate the average effect of attending the FAESL+ program on average annual employer-reported earnings, we adapt equation (1) as follows:

$$Y_{ip} = \lambda_0 + \lambda_1 Attend_i + \lambda_2 X_i + \xi_{clt} + \psi_p + e_{icltp}, \tag{4}$$

where Y_{ip} is a measure of earnings for a period p (e.g., year relative to first lottery), and ψ_p is a vector of period fixed effects. Equation (4) is estimated using a longitudinal dataset of individual-by-year observations. To obtain unbiased estimates of λ_1, we again estimate the first-stage relationship between winning one's first lottery and attending the FAESL+ program as in equation (2).

We instrument for FAESL+ attendance and obtain unbiased TOT estimates of λ_1 for compliers from the second-stage equation:

$$Y_{ip} = \lambda_0 + \lambda_1 \widehat{Attend}_i + \lambda_2 X_i + \xi_{clt} + \psi_p + e_{icltp}, \tag{5}$$

where $\widehat{Attend}_i$ is the predicted value of $Attend_i$, estimated from equation (2), and λ_1 can be interpreted as the average causal impact of attending FAESL+ on annual earnings for individuals who were induced to enroll at FAESL+ as a result of their lottery outcome. In models that pool individual data over multiple years, standard errors are clustered at the individual level.

5 Results

5.1 Voter Registration and Participation

Attending adult ESOL classes significantly increases measures of participants' civic engagement. Panel A of Table 4 reports program impacts on voting behavior. In our control group, 6.6% of individuals were registered to vote in the state of Massachusetts, as shown in column (2). Our IV estimates in column (4) indicate that enrolling in the FAESL+ program increases the probability of being a registered voter in the post-lottery period by 8.9 (s.e. 2.2)[18] percentage points, more than

[17] We also present results using alternative specification of equations (2)–(4) that uses "terms completed" ($Terms$) at FAESL+ as a measure of enrollment intensity; this has the effect of rescaling our second stage estimates by the first-stage effect of Won on the number of terms completed (~ 1.6) divided by the first-stage effect of Won on our binary measure of attendance (~ 0.5) or roughly a factor of 3.2. See appendix Table A5.

[18] Hereafter, we present standard errors in parentheses following each point estimate.

double the control mean. The estimated effect on ever participating in a post-lottery election, 7.8 (2.1) percentage points, is practically indistinguishable from the effect on registration, consistent with the increase in civic engagement being driven by newly registered voters. In Panel B of Table 4, we report estimated effects on the probability of voting in each federal general election from 2010 to 2016, including two presidential elections, the re-election of President Barack Obama (2012) and the election of President Donald Trump (2016). Point estimates for the 2010, 2012, and 2014 elections are insignificant. Estimates are large and significant for the 2016 election, when immigration policy featured prominently in then-Republican-candidate Trump's campaign platform.

Impacts on voting results take several years to emerge. Figure 1 provides a graphical representation of the estimated effect of enrolling at FAESL+ on the cumulative probability of having registered to vote by each year relative to the first lottery (year=0). The effect of program participation on the probability of having registered to vote is flat in the pre-period. The difference in the probability of having registered to vote becomes significant four years after an individual's first lottery attempt.

5.2 Employer-Reported Earnings

Adult ESOL courses substantially increase participants' employer-reported earnings. Panel A of Table 5 summarizes the effect of attending the FAESL+ program on the probability of matching to any employer-reported earnings in the MA DUA data. Over the three to ten years of post-lottery earning data we observe—the average applicant is observed for 6.9 years—FAESL+ enrollees report an additional 1.64 (0.67) quarters of earnings. Our estimated impact of ESOL enrollment on ever matching to reported earnings data is positive at 4.2 (2.8) percentage points, but statistically insignificant.

Panel B of Table 5 summarizes the effects of participating in the FAESL+ program on average annual employer-reported earnings and their natural logarithm. We estimate these effects using an unbalanced panel of data that is long at the individual-by-year level, with coverage over pre- and post-lottery years depending on when an individual first applied to the FAESL+ program.[19] We present estimates of average effects on annual employer-reported earnings that pool data from across all post-lottery years or restrict the sample to post-lottery years two to ten, after the average enrollee has completed three semesters of coursework and stopped participating in the program. We prefer estimates that pool data from years 2 through 10 because we find evidence of heterogeneity in treatment effects over time that become constant beginning in year two, as shown in Figure 2.[20] Over the full post-lottery period, enrollees report an additional \$1,843 (\$771) annually, and from

[19] We assign a value of \$0 for all pre- and post-lottery measures of reported earnings to individuals who do not match to any employer-reported earnings in years covered by our data (or \$1, when taking the natural logarithm).

[20] We conduct an F-test to test the hypothesis that the estimated effects on annual earnings are constant in years 0-2 ($p = 0.006$) or 0-3 ($p = 0.015$), which we reject, but fail to reject the hypothesis that annual effects from years 2 through 10 are equal ($p = 0.556$).

years two to ten, enrollees report an additional \$2,388 (\$911) in earnings each year. The change in annual reported earnings represents a 46-56% increase for enrollees relative to their peers who did not enroll at FAESL+ because of their lottery outcome.

The unbalanced nature of our panel means that some years and some cohorts will contribute more observations to our estimates of the effect on average annual earnings than others. In appendix Table A6, we present alternative specifications that address this issue by estimating effects over a series of balanced panels (Panel A) and re-weighting estimates to give equal weight to each post-lottery year (Panel B). Our estimates are qualitatively similar using these alternative specifications.

Substantial positive impacts in reported earnings emerge after participants complete ESOL courses. Figure 2 plots coefficients estimating the effect of attending the FAESL+ program on annual earnings reported from five years before an individual's first lottery attempt through ten years after, where year=0 in the year of the first lottery. While FAESL+ participants' employer-reported earnings are indistinguishable from those of non-participants through the first two years of the post period (while the average participant is still enrolled in classes), a considerable gap in annual earnings emerges two to three years after the first lottery attempt. Ten years after an individual's first lottery application, the difference in annual employer-reported earnings appears to be sustained, suggesting that program participation may permanently increase reported earnings.

We also find that program participation affects the probability of reporting income at different levels. Figure 3 plots the estimated effects on reporting earnings within selected ranges of the earnings distribution. We find economically meaningful and statistically significant impacts on the probability that FAESL+ enrollees ever report annual earnings between \$20,000-\$30,000 or \$60,000-\$70,000 during the first ten years after winning an enrollment lottery. Enrollees are 6.0 (2.3) percentage points more likely to ever report between \$20,000-\$30,000 in earnings, and 2.9 (1.1) percentage points more likely to report \$60,000-\$70,000 in earnings. The change in likelihood of reporting earnings in other ranges are generally positive below \$80,000, but not statistically significant.

5.3 Heterogeneity of Effects

Estimating average effects of adult ESOL attendance on our outcomes of interest may obscure important variation in treatment effects by subgroup. Table 6 presents estimated effects for selected subgroups of students. We note strong effects on voting for females and for beginners. The effect on ever-reporting earnings is strongest for individuals who applied to an evening class, which is scheduled to accommodate applicants who are working or plan to work during regular business hours. In columns (7) and (8), we disaggregate effects for individuals with and without pre-lottery earnings, noting that this limits our sample to lotteries that occurred in fall 2010 or later, since 2010 is the first year we observe reported earnings.

The effect on average annual reported earnings is disproportionately large for individuals with pre-period reported earnings; for individuals in this group, the estimated annual effect on earnings is nearly \$10,000 per year, while estimates for individuals without pre-period earnings are indistinguishable from zero.[21] This suggests that the returns to English language training may operate primarily by increasing the productivity of individuals with existing ties to the formal labor market, rather than by pushing individuals to transfer income from the informal to formal labor market or pushing individuals who are unemployed or do not work to find a job, though we do note a marginally significant positive effect on the probability of reporting earnings for individuals with no baseline earnings.

Estimates in columns (3) and (4) test whether program impacts vary by incoming levels of English proficiency. We find that labor market impacts are driven by non-beginners, which is consistent with a model of increasing returns to skill, where higher baseline levels of English proficiency may best position participants to profit from improved language skills in the formal economy. These results may also reflect labor market constraints facing recent immigrants with limited English skills, particularly individuals working in industries where paying wages under-the-table is common or whose immigration status prohibits formal paid work.

5.4 Robustness Checks

5.4.1 Placebo Tests

To assess the validity of our identification strategy, we present results from a number of falsification tests in Table 7. In Panel A, we consider whether lottery winners are more likely than non-winners to have been registered to vote or to have voted *before* their first lottery attempt. Panel B tests whether the probability of having reported earnings in the pre-period varies by lottery outcome. Panel C considers whether pre-lottery annual earnings vary by lottery outcome. Reassuringly, we find insignificant effects across all pre-lottery outcomes. In addition, Figures 1 and 2—which plot effects on voter registration and reported earnings by year—show a flat trend in the pre-period, with no significant differences by lottery outcome in any pre-lottery year.

5.4.2 Missing Date of Birth, Level, or Class Time Preference

For a small minority of applicants to FAESL+, we are missing data that is necessary to match observations to outcomes or identify the lottery an individual participated in. In Table 8, we conduct a series of bounding exercises to determine whether covariates or missing data affect our main results. Column (1) presents our main results, for comparison. Column (2) presents estimates from a model that omits individual-level controls. Results are substantively the same.

[21] Results for individuals who ever report earnings during the period of our study are similar to estimates for individuals with positive pre-period earnings.

In column (3), we consider whether missing DOB data biases our results. We are missing DOB for 2.8% of all individuals who we observe as first-time lottery applicants between fall 2008 and spring 2016. (See appendix Table A7 for information missing data and incidences of names). Since DOB is required to match to outcome data, these individuals are dropped from our sample in our main results. As a sensitivity test, we impute favorable outcomes for lottery non-winners with missing DOB and unfavorable outcomes for lottery winners' observations with missing DOB. Specifically, we impute that treatment observations with missing DOB never register to vote, and that control observations have a 20% rate of voting and voter registration. For reported earnings outcomes, we impute that all treatment observations with missing DOB data did not report any earnings, but that control observations with missing DOB data reported earnings at the median of the distribution of a given earnings outcome for the sample of control individuals with a positive value for that outcome (e.g., the 50th percentile of the control group earnings distribution for year three reported earnings, conditional on having positive reported earnings in year three). We present estimates using these assumptions in Column (3) of Table 8. Estimates are statistically indistinguishable from our main results.

We are missing baseline English proficiency level for 2.9% of individuals. Since baseline English level is required to identify an individual's lottery group, we drop these observations from our main results. In columns (4) and (5) of Table 8, we estimate treatment effects under the assumption that all of these observations are beginners (Column 4) or advanced (Column 5) students. Classifying applicants with missing levels as beginners, the most common observed category, increases our estimated effects. Classifying all applicants with missing level data as advanced students yields estimates that are indistinguishable from our main results.

We are missing time availability data for 15.1% of our analytic sample. In our main results, we classify these applicants as participating in evening lotteries, since the ratio of evening to morning applications is over four to one for individuals with known preferences. Estimates in column (5) show that our results are not sensitive to whether or not we include individuals with missing availability. In column (7), we consider an alternative test where we impute morning availability to individuals with unknown time preferences instead of evening availability. Again, results are similar.

Finally, since we match to outcome data using every combination of name and DOB observed in our three administrative datasets, we consider whether incidences of name and DOB differ by lottery outcome. In our analytic sample, we find no statistically significant differences in incidences of names or dates of birth by lottery outcome. (See results in appendix Table A7).

5.4.3 Out-of-State Mobility

If winning access to the FAESL+ program impacts the probability an individual remains in the state—for instance, by creating stronger ties to the local community—inter-state migration could bias our results since outcome data are only measured in the state of Massachusetts. In Table 9, we assess this possibility in three ways. First, we obtain public voting records from four of the top six destinations of Massachusetts residents who move within the United States, including three of the five states that share a border with Massachusetts (Rhode Island, Connecticut, and New York) and Florida (U.S. Census Bureau, 2018b).[22] We match individuals to these records using name and DOB.[23] We match 26 lottery winners and 85 lottery non-winners to out-of-state voting records. In column (1) of Table 9, we test whether winning an ESOL enrollment lottery predicts being a registered voter in any of these four destination states. We find no evidence that winning the lottery is related to out-of-state voter registration.

Second, we consider whether we find evidence of differences in *intra*-state migration. In columns (2) and (3), we estimate the effect of winning a lottery to attend the FAESL+ program on registering to vote or reporting earnings within Massachusetts, but outside of Framingham.[24] Effects are insignificant and point estimates are positive in both cases; if anything, this suggests lottery winners are more likely to appear outside the Framingham area than those who do not.

Finally, we consider whether we find patterns in earnings data that are consistent with differences in out-of-state mobility. In our employer-reported earnings records, we test whether winning the lottery predicts that individuals with stable earning histories (defined as ever reporting earnings for four consecutive quarters) suddenly and permanently stop reporting earnings in a future quarter. In column (4), we show that winning the lottery does not predict that individuals fit this pattern of reported earnings overall. In column (5), we find that winning the lottery does predict this pattern of reported earnings when we restrict our sample to the smaller group of individuals with stable earnings histories. Lottery winners with a stable prior earnings history are 9.9 percentage points less likely to suddenly stop reporting earnings than non-winners.

While there are many reasons an individual may stop reporting earnings, including a positive effect of ESOL services on stable employment, we conduct a series of robustness checks to bound the influence of possible out-of-state mobility on our estimates and present these in Table 10. First, we test whether the difference in rates of attrition from the reported earnings data can explain the main results. To do this, we identify the final post-lottery quarter an individual reported earnings for everyone in our sample with stable post-lottery earnings histories and carry that quarter's earnings forward through the end of the panel. This imposes the assumption that all "stopping out" from stable earners is due to out-of-state mobility and that individuals who "stop out" would be earning as much as they did before if we were able to observe their out-of-state earnings. Since more control

[22] Voting records from California and New Hampshire, the two other top destination states, are not readily available to the public.

[23] Records include fist name, last name, and DOB for currently registered voters. For Rhode Island records, we use name and year of birth since DOB is not made available in public files. Sources: `https://www.connvoters.com/` (CT, accessed May 30, 2020); `https://rivoters.com/` (RI, accessed May 30,2020); `https://www.elections.ny.gov/FoilRequests.html` (NY, received January, 2020); `https://flvoters.com/` (FL, accessed August 11, 2020).

[24] We define Framingham as the area including the following five zip codes: 01701, 01702, 01703, 01704, or 01705. Results are similar if we also include voting or earnings in zip codes of all cities and towns contiguous to Framingham.

observations suddenly and permanently stop reporting earnings, this imputation affects more control observations, "correcting" for differential attrition. In column (2) of Table 10, we conduct a more conservative test by carrying forward earnings for our control group only, imposing the assumption that all "stopping out" in the control group is due to out-of-state mobility but all "stopping out" in the treatment group are quarters with no earnings. Under these tests, the estimated effects on reported earnings attenuate by 25-55% but remain statistically indistinguishable from the main results.

5.4.4 Alternative Specification

In equation (2), we use lottery outcomes to predict the extensive margin of program enrollment, but policymakers may also be interested in measuring effects based on enrollment intensity. In appendix Table A5, we present results from an alternative specification where we define treatment as completing a semester at the FAESL+ program.[25] These estimates rescale our effect on ever enrolling by the inverse of the TOT effects on semesters completed (3.2), and can be interpreted as the effect of completing an additional semester for compliers. Completing a term at FAESL+ increases the probability an individual registers to vote by 2.8 percentage points and increases average annual reported earnings by \$540 to \$682.

6 Cost-Benefit Analysis

We use reported earnings data to conduct a cost-benefit exercise, calculating the estimated change in taxes paid by FAESL+ enrollees to measure the net return to taxpayers of funding adult ESOL services. Since we do not observe unreported earnings (including taxed earnings from self-employment, etc.) or non-pecuniary outcomes outside of voting behavior that may have social benefits (such as reduced reliance on public services), our calculation of "net-benefit to tax-payers" is a partial estimate of the social benefits of FAESL+ based only on increased tax revenue, and can be thought of as one component of a full accounting of the marginal value of public funds (MVPF) spent on ESOL services (Hendren & Sprung-Keyser, 2020). In 2019, the FAESL+ program received \$2,323 in direct state and federal appropriations for each seat. The program raised an additional 20% in revenue from local government and philanthropic sources for annual costs of approximately \$2,788 per seat.

To conduct our cost-benefit analysis, we use the NBER TAXSIM 27 tool to estimate state and federal tax liabilities based on applicants' reported earnings under the range of assumptions about family structure and spousal income described in appendix Table A8 and informed by population-level data from the American Community Survey. Next, we regress an individual's estimated

[25] We estimate these results by adapting our IV specification to replace *Attend* in equations (1)–(5) with *Terms*, the number of semester completed at the FAESL+ program.

annual tax obligations on predicted program attendance, as in equation (4), to generate estimates of program impacts on tax liabilities by year under each set of family assumptions. We then create an aggregate estimate of the program's impact on each type of tax payment (state taxes, federal income taxes, and FICA payments) for each tax year by weighting the TOT estimates from each family structure model by the approximate proportion of the sample each family structure represents (see column (6) of Table A8).

On average, adult ESOL classes substantially increase participants' state income tax, federal income tax, and FICA payments. In Table 11, we present estimates of the net present value of investments in ESOL services as well as the internal rate of return (IRR) of the program's impact on tax receipts. To calculate the IRR, we assume that during the first two years after an individual's first lottery application, taxpayers incur $4,500 in costs associated with the additional 3.2 semesters of ESOL classes the average enrollee is induced to attend by winning the lottery; we then assume that the estimated annual changes in post-enrollment tax payments (beginning, on average, two years after an individual's first lottery application) are sustained through 27 years post-lottery, or the average time before a FAESL+ applicant turns 65. We estimate that on average, participants pay an additional $162 per year in federal income tax, an additional $103 per year in state income tax, and make an additional $434 in FICA contributions.

Carrying changes in state and federal tax payments forward through the working lives of participants, and subtracting the costs of the program from this stream of tax payments, implies a 3.0% IRR for ESOL investments (NPV=$269 at r=3%), excluding changes to FICA contributions. The IRR increases substantially if full FICA contributions are included as social benefits, to 13.9% (NPV=$7,987 at r=3%). In our preferred estimates, we include 19% of FICA contributions as benefits to taxpayers, corresponding to the portion of FICA that funds Medicare.[26] These assumptions imply an IRR of 6.0% (NPV=$1,731 at r=3%), with a net positive benefit to taxpayers beginning in year 20 at r=3%. Since program costs are fully recovered by the government through tax payments (with a discount rate below 6%), approximately double the 3% rate used in similar analyses, our tax simulation implies that as long as aggregate willingness to pay (WTP) for services is positive[27], the MVPF invested in adult ESOL services is infinite, (i.e., $MVPF_{ESOL} = \frac{\sum_i WTP_i}{Net\,Cost\,to\,Gov't} = \frac{+}{0} = \infty$) (Hendren & Sprung-Keyser, 2020). Table 11 and appendix Table A8 show how the rate of return varies under a range of reasonable assumptions about participants' marital status, family size, spousal income, and the discount rate.

[26]It is unclear what proportion of FICA contributions should be viewed as social benefits, since individuals who make FICA contributions may reap benefits from Medicare and Social Security in retirement. Social security benefits increase with FICA payments, so the increased cash flow to the government in the short and medium-run increases the government's long-term fiscal liabilities. However, if individuals would qualify for Medicare with lower reported earnings, the portion of their increased FICA contributions that funds Medicare is a social benefit. Since 81% of FICA contributions fund the Social Security Administration (6.2% of the 7.65% tax on employee wages), we treat the remaining 19% (funding Medicare) as a social benefit.

[27]The program's excess demand, large impacts on reported earnings, and the presence of private, for-profit ESOL providers strongly suggests this is the case.

7 Discussion

We leverage the randomized enrollment lottery of one of the largest public adult ESOL program in Massachusetts to estimate the effect of attending publicly funded English language courses on voter registration, voter participation, and employer-reported earnings. We find that lottery winners who enroll in adult ESOL are twice as likely to register to vote or cast a vote as non-winners. These effects are consistent with research that finds host country language skills contribute to increased political knowledge and civic engagement for immigrants (Cho, 1999). Program impacts on new voter registration may also reflect a positive effect of attending the FAESL+ program on naturalization, a pre-requisite for registering to vote.

We find positive effects of ESOL courses on reported earnings that become significant and economically meaningful during the third year after an individual applies to a lottery to attend the program, and remain large and positive through at least ten years post-lottery. Overall, attending FAESL+ increases average annual reported earnings by 46-56%. These effects are strongest for individuals with pre-lottery earnings and for individuals with higher levels of initial English proficiency, in line with the theory that the returns to language skills are highest for individuals with higher levels of pre-existing human capital (Chiswick & Miller 2007).

A simple cost-benefit analysis implies that every dollar invested in immigrant language skills is paid back by increased tax revenue within 20 years after an individual's first lottery application, with an estimated lifetime social rate of return of 6.0%. The net benefit to taxpayers is similar to the long-run return to equity of 5.8 percent, and is slightly below the estimated IRR of investments in early childhood education that account for not only social benefits attributable to increased tax revenue but also private economic benefits and social benefits associated with decreased criminal activity, lower rates of special education classification, and reduced use of public welfare (Heckman et al., 2010).

While this study uses data from a single program serving a particular population, there are reasons to believe our effects generalize or even underestimate the average returns to ESOL programs in Massachusetts and elsewhere. Compared to other programs, the FAESL+ program requires a modest commitment of time from students (six hours per week) and is near the middle of the distribution in terms of per-pupil expenditure. Moreover, we find evidence that a smaller share of FAESL+ applicants participate in the formal workforce than at other ESOL programs, which may attenuate estimated effects on earnings and tax revenue.

Adult education programs in the United States serve some of the country's most marginalized and vulnerable residents, including immigrants seeking to improve their English skills. Our results suggest reason for optimism regarding the private and social returns to investments in immigrant language skills and highlight the potential of adult ESOL programs as a cost-effective tool for facilitating immigrant integration.

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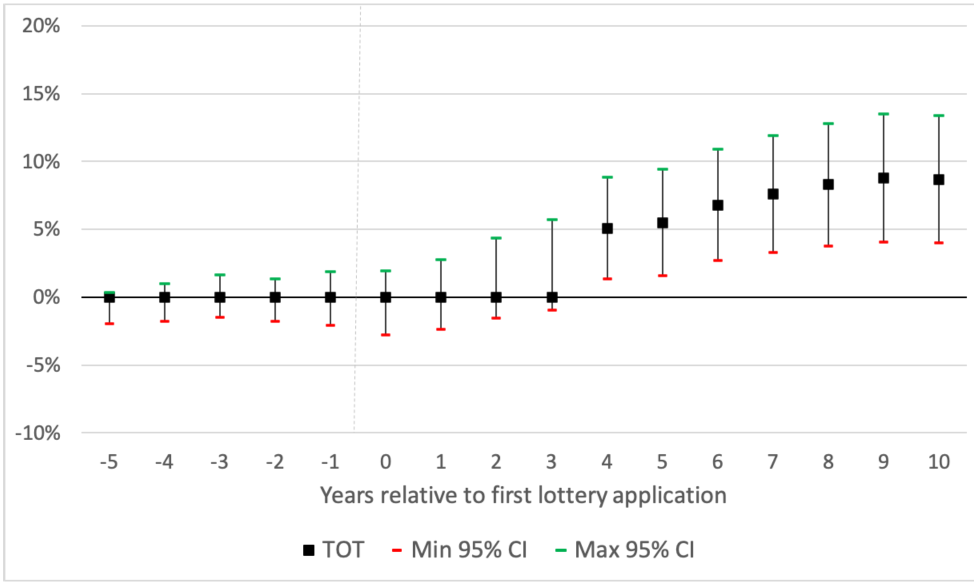
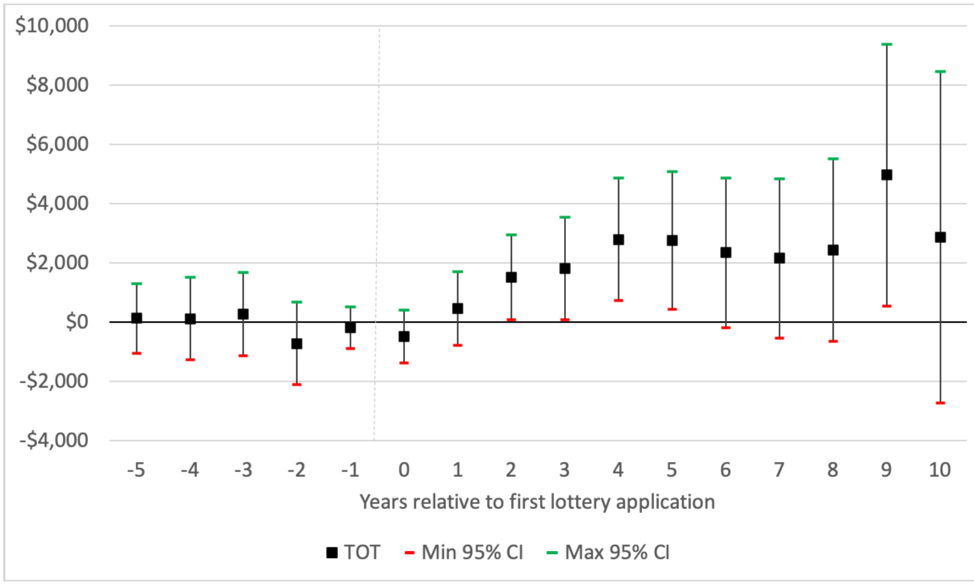
Figure 1: Cumulative effects on probability of having registered to vote, by year since first lottery



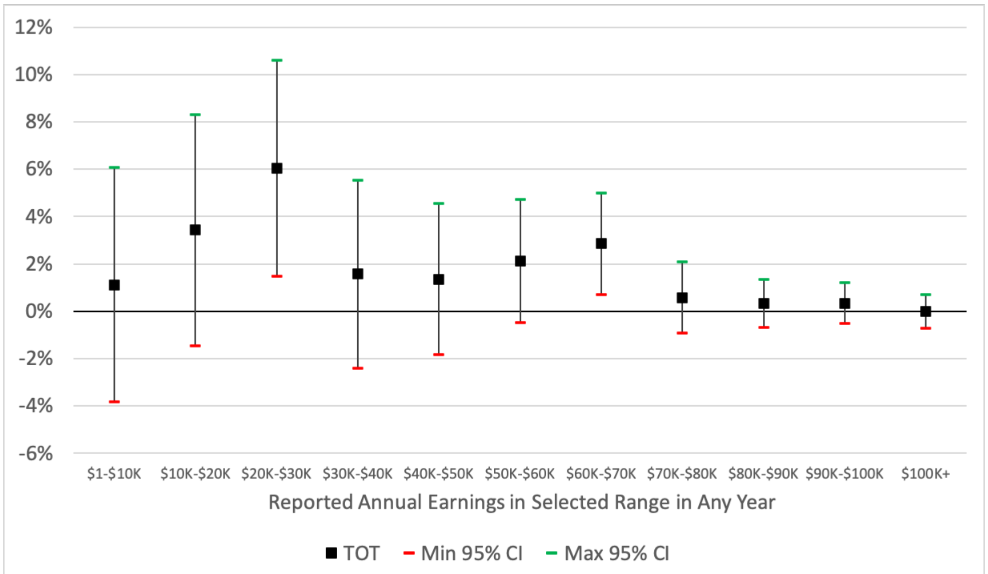
Notes: Year of voting is defined relative to first lottery (year=0). TOT point estimates and heteroskedasticity-robust confidence intervals are calculated from 2SLS IV estimates using equation (5) of the effect of enrolling in the FAESL+ program on having registered to vote by the indicated period. Appendix Table A2 records the point estimates plotted here.

Figure 2: Annual effects on reported earnings, by year since lottery



Notes: Year of reported earnings is defined relative to first lottery (year=0). TOT point estimates and heteroskedasticity-robust confidence intervals are calculated from 2SLS IV estimates using equation (5) of the effect of enrolling in the FAESL+ program on reported income in the indicated year. Appendix Table A3 records the point estimates plotted here.

Figure 3: Cumulative effects on probability of ever reporting earnings in selected ranges



Notes: TOT point estimates and heteroskedasticity-robust confidence intervals are calculated from 2SLS IV estimates using equation (3) of the effect of enrolling in the FAESL+ program on having ever reported annual income in the indicated range. Appendix Table A4 records the point estimates plotted here.

Table 1: Summary statistics: characteristics of students in Massachusetts adult ESOL programs

	All ESOL Students (1)	FAESL+ Students (2)	Lottery Sample (3)
Male	0.34	0.37	0.41
Age at Lottery	38.85	40.04	36.70
Asian Surname	0.12	0.05	0.04
Black Surname	0.04	0.00	0.00
Hispanic Surname	0.34	0.21	0.24
White Surname	0.08	0.20	0.23
Brazilian Surname	0.11	0.31	0.44
Surname Not Attributed to Any Group	0.39	0.34	0.23
Matched to Voting Records	0.22	0.10	0.10
Matched to Earnings	0.45	0.29	0.24
Observations	52,797	2,384	4,761

Notes: Column (1) includes all students who enrolled in a public adult ESOL class in Massachusetts between fall 2008 and spring 2016. Column (2) includes all students who enrolled in a FAESL+ ESOL class between fall 2008 and spring 2016, including continuing students and first-time enrollees. Column (3) is limited to first-time lottery applicants who applied to ESOL classes at the FAESL+ program between fall 2008 and spring 2016 and is limited to individuals with non-missing date-of-birth and initial level information. Asian, Black, Hispanic, and white surname are indicator variables that take on a value of one if 80% of respondents to the 2010 U.S. Census with that surname were of that racial or ethnic group and zero otherwise. Brazilian surname is an indicator variable that takes on a value of one if an individual's surname was among the 100 most common surnames in Brazil, per Forebears (2019), and zero otherwise. The indicator for having a Brazilian surname is not mutually exclusive with other racial or ethnic indicators: 31.7% of Brazilian surnames are classified as white, 9.9% are classified as Hispanic, and <1% are classified as Asian. Age refers to age at the start of first observed ESOL enrollment for Columns (1) and (2) and age at first lottery for the lottery sample in Column (3).

Table 2: Distribution of Students and Lottery Applicants by Year

Year	Enrolled Students (1)	First-Time Lottery Applicants (2)	Won (3)	Did Not Win (4)
2008	534	408	132	276
2009	680	756	198	558
2010	686	733	177	556
2011	674	501	156	345
2012	683	429	147	282
2013	687	454	136	318
2014	686	458	135	323
2015	693	606	125	481
2016	541	416	42	374

Notes: First-time lottery sample is limited to individuals with non-missing DOB and non-missing level information. Lottery and enrolled student samples for 2008 include only fall applicants. Lottery and enrolled student samples for 2016 include only spring applicants.

Table 3: Sample Balance and First-Stage Estimates

	Control Mean (1)	Won Lottery (2)
A. Baseline Characteristics		
Age at Lottery	36.3	0.287
		(0.455)
Male	0.419	0.004
		(0.019)
Asian Surname	0.034	-0.009
		(0.008)
Hispanic Surname	0.233	0.020
		(0.016)
White Surname	0.236	0.001
		(0.015)
Brazilian Surname	0.462	-0.017
		(0.018)
Surname not attributed to any group	0.316	0.002
		(0.017)
Baseline Quarterly Earnings	$804	9
		(138)
F-statistic from test of joint probability		0.634
P-value from joint F-test		0.750
Observations	3,513	4,761
B. First-Stage Estimates		
Ever Enrolled at FAESL+	0.244	0.503**
		(0.015)
Number of Terms Enrolled	0.875	1.62**
		(0.097)
Total Hours Enrolled	69.4	125**
		(8)
Observations	3,513	4,761

Notes: Column (1) presents the mean of each characteristic for individuals in our sample who did not win their first lottery attempt. Column (2) in Panel A reports the coefficient on an indicator for winning an individual's first lottery attempt in separate regressions testing whether lottery results predict each of the listed characteristics, controlling for lottery group fixed-effects, with heteroskedasticity-robust standard errors in parentheses. At the bottom of Panel A, we report results from an F-test of joint significance from a regression testing whether all characteristics in Panel A jointly predict lottery outcomes, conditional on lottery group fixed-effects. Due to the terms of our data use agreement, we are unable to combine indicators for baseline voting with reported earnings data; F-test results are similar if we include an indicator for being a registered voter at baseline instead of baseline earnings. In Panel B, we report first-stage effects estimated from equation (2) with the indicated measures of program participation as the dependent variable. $*p < 0.05$, $**p < 0.01$.

Table 4: Main Effects on Civic Outcomes

	Control Mean (1)	Ever Enrolled (2)	Sample (3)
A. Voting and Voter Registration			
Registered to Vote	0.07	0.089***	F2008-S2016
		(0.022)	
		4,761	
Voted	0.06	0.078***	F2008-S2016
		(0.021)	
Observations		4,761	
B. Voting by General Election			
Voted in 2010	0.01	0.000	F2008-F2010
		(0.008)	
		1,897	
Voted in 2012	0.02	0.017	F2008-F2012
		(0.016)	
		2,827	
Voted in 2014	0.01	0.017	F2008-F2014
		(0.010)	
		3,739	
Voted in 2016	0.04	0.072***	F2008-S2016
		(0.019)	
Observations		4,761	

Notes: Column (1) presents the mean of each outcome for individuals in our sample who did not win their first lottery attempt. All outcomes defined over post-lottery periods only. Column (2) presents 2SLS IV estimates of the impact of ever enrolling at FAESL+ on the outcomes listed in each row, with heteroskedasticity-robust standard errors in parentheses followed by the number of observations that contribute to each estimate. Estimates calculated using a dataset unique at the applicant-level. All estimates include covariates and lottery fixed effects that interact incoming level with time-of-day preferences and semester of first lottery application. Covariates include gender; Asian, Hispanic, or white surname; Brazilian surname; surname not attributed to any racial or ethnic group; age at lottery; and an indicator for missing gender. $*p < 0.05$, $**p < 0.01$.

Table 5: Effects on Employer-Reported Earnings

	Control Mean (1)	Ever Enrolled [Earnings in $] (2)	Ever Enrolled [Ln(Earnings in $)] (3)
A. Matched to Employer-Reported Earnings			
Ever Matched	0.21	0.042	–
		(0.028)	
		4,761	
Quarters Matched	3.78	1.64*	–
		(0.67)	
Observations		4,761	
B. Average Annual Earnings (All)			
Annual Earnings, through Y_{10}	$4,022	1,843*	0.464*
		(771)	(0.223)
		32,770	32,770
Annual Earnings, Y_2–Y_{10}	$4,147	2,388**	0.557*
		(911)	(0.255)
Observations		24,820	24,820

Notes: Column (1) presents the mean of each outcome for individuals in our sample who did not win their first lottery attempt. All outcomes defined over post-lottery periods only. Columns (2) and (3) present 2SLS IV estimates of the impact of ever enrolling at FAESL+ on the outcomes listed in each row, with heteroskedasticity-robust standard errors in parentheses followed by the number of observations that contribute to each estimate. Estimates in Panel A are calculated from equation (3) using a dataset unique at the applicant-level. Estimates in Panel B are calculated by equation (5) using a longitudinal dataset of applicant-by-year observations (unbalanced panel), with standard-errors clustered at the individual level, with outcomes measured in unadjusted dollars (Column 2) or their natural logarithm plus $1 (Column 3). All estimates include covariates and lottery fixed effects that interact incoming level with time-of-day preferences and semester of first lottery application. Covariates include gender; Asian, Hispanic or white surname; Brazilian surname; surname not attributed to any racial or ethnic group; age at lottery; baseline quarterly earnings and an indicator for missing gender. Panel B adds year fixed effects. $*p < 0.05$, $**p < 0.01$.

Table 6: Heterogeneity of Effects

	Male (1)	Female (2)	Beginner (3)	Intermediate or Advanced (4)	AM Lottery (5)	PM Lottery (6)	Pre-Period Earnings > \$0 (7)	Pre-Period Earnings = \$0 (8)
A. Voting and Voter Registration								
Registered to Vote	0.050	0.114**	0.084**	0.137*	0.163*	0.079**	–	–
	(0.029)	(0.031)	(0.023)	(0.063)	(0.074)	(0.023)		
	1,929	2,832	4,191	570	705	4,056		
Voted	0.058*	0.088**	0.074**	0.107	0.116	0.072**	–	–
	(0.029)	(0.029)	(0.022)	(0.065)	(0.071)	(0.022)		
Observations	1,929	2,832	4,191	570	705	4,056		
B. Matched to Earnings Data								
Ever Matched	0.030	0.052	0.033	0.055	-0.132	0.070*+	-0.018	0.054
	(0.042)	(0.038)	(0.031)	(0.077)	(0.104)	(0.029)	(0.051)	(0.032)
	1,929	2,832	4,191	570	705	4,056	531	2,643
Quarters Matched	1.71	1.51	1.32	3.21	1.64	1.74*	4.24*	0.62+
	(1.05)	(0.85)	(0.72)	(1.88)	(2.09)	(0.70)	(1.87)	(0.52)
Observations	1,929	2,832	4,191	570	705	4,056	531	2,643
C. Average Annual Earnings								
Annual Earnings, through Y_{10}	2,055	1,627*	922	7,036**+	3,521	1,651*	6,167	662+
	(1,379)	(805)	(779)	(2,521)	(2,271)	(816)	(3,278)	(654)
	13,452	19,318	28,686	4,084	4,766	28,004	3,187	15,300
Annual Earnings, Y_2–Y_{10}	2,777	2,009*	1,281	8,658**+	5,004	2,106*	9,803*	896+
	(1,650)	(937)	(927)	(2,935)	(3,020)	(951)	(4,340)	(873)
Observations	10,263	14,557	21,691	3,129	3,546	21,274	2,125	10,014

Notes: Results in Panels A and B are estimated using equation (3) in a dataset that is unique at the individual-level, with heteroskedasticity-robust standard errors in parentheses followed by the number of observations that contribute to each estimate. Results in Panel C are estimated using equation (5) in a longitudinal dataset that is unique at the individual-by-year level, with standard errors clustered at the individual level. All outcomes defined over post-lottery periods only. All estimates include covariates and lottery fixed effects that interact incoming level with time-of-day preferences and semester of first lottery application. Covariates include gender; Asian, Hispanic or white surname; Brazilian surname; surname not attributed to any racial or ethnic group; age at lottery; and an indicator for missing gender. Panels B and C add baseline earnings as a covariate. Panel C adds year fixed effects. Beginner and Intermediate/Advanced subgroups are identified based on initial (entry) level of English. AM/PM lottery are identified based on preferences from first application/enrollment. The full analytic sample of first-time lottery applicants from fall 2008 to spring 2016 contributes to columns (1)-(6); the sample in columns (7) and (8) is limited to first-time lottery applicants from fall 2010 to spring 2016, representing cohorts with observed pre-lottery earnings data. The symbol "+" denotes that a given subgroup estimate is statistically distinguishable from its complement at the 5% level. $*p < 0.05$, $**\,p < 0.01$.

Table 7: Placebo Tests

	Sample (1)	Control Mean (2)	Ever Enrolled (3)
A. Pre-Lottery Voting and Voter Registration			
Registered to Vote	F2008-S2016	0.02	-0.002
			(0.011)
			4,761
Voted	F2008-S2016	0.01	0.001
			(0.010)
Observations			4,761
B. Pre-Lottery Matched to Earnings			
Ever Matched	F2010-S2016	0.15	0.022
			(0.027)
			3,174
Quarters Matched	F2010-S2016	1.04	0.206
			(0.251)
Observations			3,174
C. Pre-Lottery Average Annual Earnings			
Annual Earnings, through Y_{-5}	S2011-S2016	$1,900	-106
			(723
Observations			9,663

Notes: All outcomes defined over pre-lottery periods only. Column (2) presents the mean of each pre-lottery outcome for individuals in the analysis sample indicated in column (1) who did not win their first lottery attempt. Column (3) presents 2SLS IV estimates assessing whether ever enrolling at FAESL+ predicts the pre-lottery outcomes listed in each row, with heteroskedasticity-robust standard errors in parentheses followed by the number of observations that contribute to each estimate. Results in Panels A and B are estimated using equation (3) in a dataset that is unique at the individual-level. Results in Panel C are estimated using equation (5) in a longitudinal dataset that is unique at the individual-by-year level, with standard errors clustered at the individual level. All estimates include covariates and lottery fixed effects that interact incoming level with time-of-day preferences and semester of first lottery application. Covariates include gender; Asian, Hispanic or white surname; Brazilian surname; surname not attributed to any racial or ethnic group; age at lottery; and an indicator for missing gender. Panels B and C add baseline earnings as a covariate. Panel C adds year fixed effects. Descriptions of placebo tests are presented in the section 5.4.1 of the text. $*p < 0.05$, $**p < 0.01$.

Table 8: Robustness Checks

	Main Results (1)	No Covariates (2)	Impute Outcomes (Missing DOB) (3)	Impute Level (Beginner) (4)	Impute Level (Advanced) (5)	Exclude Missing Availability (6)	Impute Availability (AM) (7)
A. Voting and Voter Registration							
Registered to Vote	0.078***	0.083***	0.066**	0.127***	0.086***	0.076***	0.074***
	(0.021)	(0.019)	(0.021)	(0.026)	(0.022)	(0.023)	(0.021)
	4,761	4,761	4,884	4,890	4,890	4,040	4,761
Voted	0.089***	0.092***	0.078***	0.155***	0.103***	0.086***	0.089***
	(0.022)	(0.020)	(0.022)	(0.027)	(0.023)	(0.024)	(0.021)
Observations	4,761	4,761	4,884	4,890	4,890	4,040	4,761
B. Matched to Earnings Data							
Ever Matched	0.042	0.048	0.028	0.095**	0.048	0.032	0.054
	(0.028)	(0.032)	(0.029)	(0.034)	(0.029)	(0.031)	(0.028)
	4,761	4,761	4,884	4,890	4,890	4,040	4,761
Quarters Matched	1.640*	1.836*	1.407*	2.655***	1.810**	1.586*	1.744**
	(0.670)	(0.756)	(0.674)	(0.786)	(0.687)	(0.724)	(0.663)
Observations	4,761	4,761	4,884	4,890	4,890	4,040	4,761
C. Average Annual Earnings							
Annual Earnings, through Y_{10}	1,843*	1,807	1,415	2,517**	1,880*	1,825*	1,844*
	(771)	(939)	(790)	(886)	(778)	(823)	(749)
Observations	32,770	32,770	33,649	33,571	33,571	29,097	32,770
Annual Earnings, Y_2–Y_{10}	2,388**	2,301*	1,891*	3,144**	2,423**	2,459*	2,392**
	(911)	(1029)	(932)	(1035)	(922)	(978)	(884)
Observations	24,820	24,820	25,514	25,370	25,370	22,385	24,820

Notes: Results in Panels A and B are estimated using equation (2) in a dataset that is unique at the individual-level, with heteroskedasticity-robust standard errors in parentheses followed by the number of observations that contribute to each estimate. Results in Panel C are estimated using equation (5) in a longitudinal dataset that is unique at the individual-by-year level, with standard errors clustered at the individual level. All outcomes defined over post-lottery periods only. All estimates include lottery fixed effects that interact incoming level with time-of-day preferences and semester of first lottery application. All estimates except those in column (2) add covariates, including gender; Asian, Hispanic or white surname; Brazilian surname; surname not attributed to any racial or ethnic group; age at lottery; and an indicator for missing gender. Panels B and C add baseline earnings as a covariate. Panel C adds year fixed effects. Descriptions of each robustness test are presented in the section 5.4.2 of the text. $*p < 0.05$, $**\,p < 0.01$.

Table 9: Mobility Tests

	Registered to Vote in RI, CT, NY, or FL (1)	Registered to Vote in MA Outside Framingham (2)	Reported Earnings Outside Framingham (3)	Stopped Reporting After 4Q > \$0 (4)	Stopped Reporting After 4Q > \$0 (5)
Won Lottery	-0.004	0.015	0.016	-0.014	-0.099**
	(0.005)	(0.008)	(0.014)	(0.009)	(0.036)
Observations	4,761	4,761	4,761	4,761	926
Sample Restriction	None	None	None	None	4Q>\$0

Notes: Results are estimated using equation (3) in a dataset that is unique at the individual-level, with heteroskedasticity-robust standard errors in parentheses. In columns (2) and (3), "Outside Framingham" is defined by exluding observations from the five zipcodes that comprise the city. In columns (4) and (5), the outcome variable, "stopped reporting earnings" is a binary indicator that takes on a value of one for any individual who is never again observed reporting earnings after being observed reporting earnings in any four past consecutive quarters, and zero otherwise. All outcomes defined over post-lottery periods only. All estimates include covariates and lottery fixed effects that interact incoming level with time-of-day preferences and semester of first lottery application. Covariates include gender; Asian, Hispanic or white surname; Brazilian surname; surname not attributed to any racial or ethnic group; age at lottery; and an indicator for missing gender. Columns (3), (4), and (5) add baseline quarterly earnings as a covariate. Descriptions of each test are presented in the section 5.4.3 of the text. $*p < 0.05$, $**p < 0.01$.

Table 10: Mobility Robustness Checks

		Carry Forward Last Earnings	
	Main Results (1)	Full Sample with 4Q>\$0 (2)	Control Only with 4Q>\$0 (3)
Average Annual Earnings, through Y_{10}	1,843*	1,394	866
	(771)	(988)	(972)
Observations	32,770	32,770	32,770
Annual Earnings, Y_2–Y_{10}	2,388**	1,778	1,095
	(911)	(1,077)	(1,059)
Observations	24,820	24,820	24,820

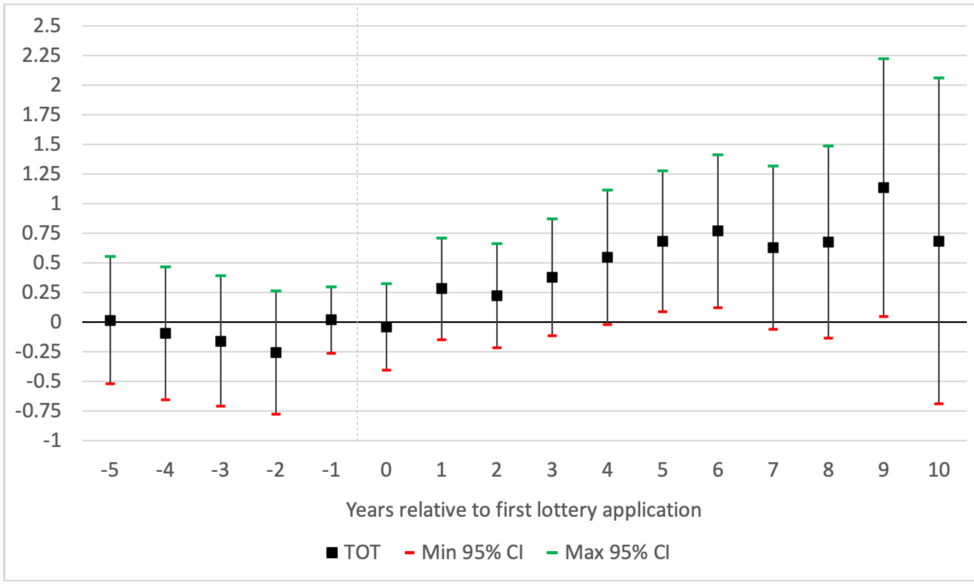
Notes: Results are estimated using equation (5) in a longitudinal dataset that is unique at the individual-by-year level, with heteroskedasticity-robust standard errors clustered at the individual level. All outcomes defined over post-lottery periods only. All estimates include covariates and lottery fixed effects that interact incoming level with time-of-day preferences and semester of first lottery application. Covariates include gender; Asian, Hispanic or white surname; Brazilian surname; surname not attributed to any racial or ethnic group; age at lottery; baseline quarterly earnings and an indicator for missing gender. Descriptions of each test are presented in section 5.4.3 of the text. ($*p < 0.05$, $**p < 0.01$).

Table 11: Cost-Benefit Analysis

Tax	IRR (1)	NPV at r=0% (2)	NPV at r=1% (3)	NPV at r=2% (4)	NPV at r=3% (5)	NPV at r=4% (6)	NPV at r=5% (7)	Annual Δ in Tax Payments (8)	Years before NPV> $0 at r=3% (9)
State + Federal + 19% FICA	6.0%	$5,022	$3,618	$2,585	$1,731	$1,022	$428	$347	20
State + Federal Income Tax	3.5%	$2,657	$1,701	$917	$269	-$269	-$718	$265	27
Federal Income Tax	-0.2%	-$110	-$689	-$1,164	-$1,555	-$1,878	-$2,148	$162	n/a
State Income Tax	-3.1%	-$1,724	-$2,084	-$2,377	-$2,618	-$2,817	-$2,981	$103	n/a
Federal Income Tax + FICA	11.7%	$11,592	$9,420	$7,635	$6,157	$4,927	$3,897	$596	11
State + Federal + FICA	13.9%	$14,369	$11,819	$9,722	$7,987	$6,542	$5,331	$699	10

Notes: Tax liabilities are estimated using NBER TAXSIM 27 software under the assumptions about family structure and spousal income described in Table A8. Annual changes in tax payments in column (8) were calculated by estimating the impact of program enrollment on tax liabilities under each set of family structure assumptions using equation (5) with estimated tax liabilities as the dependent variable for each post-lottery year, imputing the average annual post-lottery TOT estimate forward for a total of 27 years. The internal rate of return (IRR) for each stream of tax payments is calculated under the assumption that changes in earnings and tax payments are sustained for 27 years, after two years of no change in tax payments during which program costs of $4,492 are incurred. The IRR represents the interest rate at which the net present value (NPV) of the stream of tax payments less program costs equals zero. Tax liabilities are estimated in a longitudinal dataset that is unique at the individual-by-year level, with heteroskedasticity-robust standard errors clustered at the individual level. Data restricted to 2010 to 2018 observations, the only years where full annual earnings are available (earnings data is only observed through quarter 3 of 2019). All estimates include covariates and lottery fixed effects that interact incoming level with time-of-day preferences and semester of first lottery application. Covariates include gender; Asian, Hispanic or white surname; Brazilian surname; surname not attributed to any racial or ethnic group; age at lottery; baseline quarterly earnings and an indicator for missing gender. N=20,059 annual earnings observations.

Figure A1: Annual effects on ln(reported earnings), by year since lottery



Notes: Year of reported earnings is defined relative to first lottery (year=0). TOT point estimates and heteroskedasticity-robust confidence intervals are calculated from 2SLS IV estimates using equation (5) of the effect of enrolling in the FAESL+ program on the natural logarithm of reported income in the indicated year. All estimates include covariates and lottery fixed effects that interact incoming level with time-of-day preferences and semester of first lottery application. Covariates include gender; Asian, Hispanic or white surname; Brazilian surname; surname not attributed to any racial or ethnic group; age at lottery; baseline quarterly earnings and an indicator for missing gender. Appendix Table A3 records the point estimates plotted here.

Table A1: Lottery Balance by Semester

	P-Value from Joint F-Test (1)
Fall 2008	0.119
Spring 2009	0.735
Fall 2009	0.156
Spring 2010	0.684
Fall 2010	0.063
Spring 2011	0.015
Fall 2011	0.797
Spring 2012	0.842
Fall 2012	0.010
Spring 2013	0.311
Fall 2013	0.219
Spring 2014	0.161
Fall 2014	0.707
Spring 2015	0.350
Fall 2015	0.316
Spring 2016	0.900

Notes: Column (1) reports the p-value from a joint test of the significance of differences between treatment and control group means of all covariates in Panel A of Table 2 for each semester's lottery. In individual lotteries, imputed race characteristics are included for all race and ethnicity subgroups with at least five observations. All estimates include covariates and lottery fixed effects that interact incoming level with time-of-day preferences and semester of first lottery application. Covariates include gender; Asian, Hispanic or white surname; Brazilian surname; surname not attributed to any racial or ethnic group; age at lottery; baseline quarterly earnings and an indicator for missing gender. Baseline earnings are available and included in balance tests beginning in fall 2010.

Table A2: Effect on Probability of Having Registered to Vote by Year Since Lottery

Year (1)	Control Mean (2)	Ever Enrolled (3)
-5	0.01	-0.008
		(0.006)
-4	0.01	-0.004
		(0.007)
-3	0.01	0.001
		(0.008)
-2	0.01	-0.002
		(0.008)
-1	0.01	-0.001
		(0.010)
0	0.02	-0.004
		(0.012)
1	0.03	0.002
		(0.013)
2	0.03	0.014
		(0.015)
3	0.04	0.024
		(0.017)
4	0.05	0.051**
		(0.019)
5	0.06	0.055**
		(0.020)
6	0.06	0.068**
		(0.021)
7	0.07	0.076**
		(0.022)
8	0.08	0.083**
		(0.023)
9	0.08	0.088**
		(0.024)
10	0.08	0.087**
		(0.024)

Notes: Year is defined relative to first lottery (year=0). Column (1) reports the proportion who had registered to vote by the indicated year among individuals in our sample who did not win their first lottery attempt. Column (2) reports 2SLS IV estimates using equation (3) of the effect of enrolling in the FAESL+ program on having registered to vote by the indicated period, with heteroskedasticity-robust standard errors in parentheses. All estimates include covariates and lottery fixed effects that interact incoming level with time-of-day preferences and semester of first lottery application. Covariates include gender; Asian, Hispanic or white surname; Brazilian surname; surname not attributed to any racial or ethnic group; age at lottery; baseline quarterly earnings and an indicator for missing gender. The coefficients reported here are plotted in Figure 1. N=4,761. $*p < 0.05$, $**p < 0.01$.

Table A3: Effects on Annual earnings by Year Since Lottery

Year	Control Mean (1)	Earnings \$ (2)	Earnings Ln(\$) (3)	Observations (4)
-5	\$949	123	0.016	1,022
		(603)	(0.273)	
-4	\$1,129	120	-0.093	1,480
		(705)	(0.286)	
-3	\$1,424	264	-0.160	1,934
		(716)	(0.281)	
-2	\$2,162	-713	-0.258	2,363
		(713)	(0.265)	
-1	\$2,826	-189	0.019	2,864
		(359)	(0.143)	
0	\$3,549	-478	-0.038	3,597
		(459)	(0.186)	
1	\$3,720	467	0.281	4,353
		(635)	(0.218)	
2	\$3,765	1,514*	0.224	4,761
		(731)	(0.224)	
3	\$4,070	1,822*	0.379	4,345
		(884)	(0.253)	
4	\$4,058	2,814**	0.548	3,739
		(1,057)	(0.290)	
5	\$4,354	2,791*	0.687*	3,281
		(1,184)	(0.303)	
6	\$4,590	2.381	0.773*	2,827
		(1,290)	(0.331)	
7	\$4,570	2,173	0.634	2,398
		(1,379)	(0.351)	
8	\$4,331	2,449	0.680	1,897
		(1,571)	(0.412)	
9	\$3,894	4,981*	1.133*	1,164
		(2,262)	(0.557)	
10	\$2,943	2,859	0.684	408
		(2,857)	(0.702)	

Notes: Year is defined relative to first lottery (year=0). Column (1) reports mean reported earnings in the indicated year among individuals in our sample who did not win their first lottery attempt. Columns (2) and (3) report 2SLS IV estimates using equation (5) of the effect of enrolling in the FAESL+ program on reported earnings (Column 2) and their natural logarithm (Column 3) in the indicated year, with heteroskedasticity-robust standard errors in parentheses. All estimates include covariates and lottery fixed effects that interact incoming level with time-of-day preferences and semester of first lottery application. Covariates include gender; Asian, Hispanic or white surname; Brazilian surname; surname not attributed to any racial or ethnic group; age at lottery; baseline quarterly earnings and an indicator for missing gender. The coefficients reported in column (2) are plotted in Figure 2 and coefficients reported in column (3) are plotted in appendix Figure A1. $*p < 0.05$, $**p < 0.01$.

Table A4: Impact on P(Ever reporting earnings within selected ranges)

Earnings Range	Control Mean (1)	TOT (2)
\$0 – \$10,000	0.114	0.011
		(0.025)
\$10,000 – \$20,000	0.103	0.034
		(0.025)
\$20,000 – \$30,000	0.091	0.060**
		(0.023)
\$30,000 – \$40,000	0.077	0.016
		(0.020)
\$40,000 – \$50,000	0.046	0.014
		(0.016)
\$50,000 – \$60,000	0.030	0.021
		(0.013)
\$60,000 – \$70,000	0.014	0.029**
		(0.011)
\$70,000 – \$80,000	0.009	0.006
		(0.008)
\$80,000 – \$90,000	0.003	0.004
		(0.005)
\$90,000 – \$100,000	0.003	0.004
		(0.004)
Over \$100,000	0.003	-0.0001
		(0.004)
Observations		4,761

Notes: Column (1) reports the proportion who ever reported annual earnings in the indicated range among individuals in our sample who did not win their first lottery attempt. Column (2) reports 2SLS IV estimates using equation (3) of the effect of enrolling in the FAESL+ program on reporting earnings in the indicated range, with heteroskedasticity-robust standard errors in parentheses. All estimates include covariates and lottery fixed effects that interact incoming level with time-of-day preferences and semester of first lottery application. Covariates include gender; Asian, Hispanic or white surname; Brazilian surname; surname not attributed to any racial or ethnic group; age at lottery; baseline quarterly earnings and an indicator for missing gender. The coefficients reported here are plotted in Figure 3. $*p < 0.05$, $**p < 0.01$.

Table A5: Alternative IV Estimates: Number of Terms as Treatment

	Control Mean (1)	Number of Terms (2)
A. Voting and Voter Registration		
Ever Registered to Vote	0.07	0.028**
		(0.007)
		4,761
Ever Voted	0.06	0.024**
		(0.007)
Observations		4,761
B. Matched to Earnings Data		
Ever Reported Earnings	0.21	0.013
		(0.009)
		4,761
Quarters with Earnings	3.78	0.509*
		(0.209)
Observations		4,761
C. Average Annual Earnings		
Annual Earnings, through Y_{10}	$4,022	540*
		(228)
		32,770
Annual Earnings, Y_2–Y_{10}	$4,147	682**
		(263)
Observations		24,820

Notes: Column (1) presents the mean of each outcome for individuals in our sample who did not win their first lottery attempt. All outcomes defined over post-lottery periods only. Column (2) presents 2SLS IV estimates of the impact of enrolling at FAESL+ for one term/semester on the outcomes listed in each row, with heteroskedasticity-robust standard errors in parentheses. Results in Panels A and B are estimated using an adaptation of equation (3) that replaces the binary indicator for program attendance with the number of terms an individual attended FAESL+ in a dataset that is unique at the individual-level. Results in Panel C are estimated using an adaptation of equation (5) that replaces the binary indicator for program attendance with the number of terms an individual attended FAESL+ in a longitudinal dataset that is unique at the individual-by-year level. Standard errors in Panel C are clustered at the individual level. All estimates include covariates and lottery fixed effects that interact incoming level with time-of-day preferences and semester of first lottery application. Covariates include gender; Asian, Hispanic or white surname; Brazilian surname; surname not attributed to any racial or ethnic group; age at lottery; and an indicator for missing gender. Panels B and C add baseline earnings as a covariate. Panel C adds year fixed effects. $*p < 0.05$, $**p < 0.01$.

Table A6: Alternative Specifications, Effects on Annual Employer-Reported Earnings

	Control Mean (1)	Ever Enrolled [Earnings in $] (2)	Ever Enrolled [Ln(Earnings in $)] (3)
A. Balanced Panels			
Annual Reported Earnings, through Y_1	$3,687	-150	0.072
		(529)	(0.192)
		7,194	7,194
Annual Reported Earnings, Y_2–Y_5	$4,002	2,131*	0.520
		(960)	(0.278)
		13,124	13,124
Annual Reported Earnings, Y_6–Y_9	$3,694	3,948*	0.870
		(1,961)	(0.532)
Observations		4,656	4,656
B. Reweighted Estimates			
Annual Reported Earnings, through Y_{10}	$3,989	2,240*	0.570*
		(935)	(0.258)
		32,770	32,770
Annual Reported Earnings, Y_2–Y_{10}	$4,071	2,692*	0.653*
		(1,086)	(0.294)
Observations		24,820	24,820

Notes: Results in Panel A are estimated in balanced panels where the sample is restricted to individuals whose reported earnings over the range of post-lottery years indicated in each row could be observed in reported earnings data from 2010–2019. In Panel B, an unbalanced panel is used to generate reweighted estimates where observations are weighted by the inverse of the number of observations in the sample in a given year, where an observation's year is defined relative to the date of an individual's first lottery application (year-0). Column (1) presents the mean of each outcome for individuals in our sample who did not win their first lottery attempt (weighted as described above for Panel A). All outcomes defined over post-lottery periods only. Columns (2) and (3) present 2SLS IV estimates of the impact of ever enrolling at FAESL+ on the outcomes listed in each row, with heteroskedasticity-robust standard errors in parentheses followed by the number of observations that contribute to each estimate. Estimates in each panel are calculated by equation (5) using a longitudinal dataset of applicant-by-year observations (unbalanced panel), with standard-errors clustered at the individual level, with outcomes measured in unadjusted dollars (Column 2) or their natural logarithm plus $1 (Column 3). All estimates include covariates, year fixed effects, and lottery fixed effects that interact incoming level with time-of-day preferences and semester of first lottery application. Covariates include gender; Asian, Hispanic or white surname; Brazilian surname; surname not attributed to any racial or ethnic group; age at lottery; baseline quarterly earnings and an indicator for missing gender. $*p < 0.05$, $**p < 0.01$.

Table A7: Missingness and Incidence of Names and Date of Birth

	All (1)	Won (2)	Did not win (3)	t-stat (p-value) (4)	Observations (5)
Missing DOB	0.028	0.017	0.032	2.88	5,031
				(0.004)	
Missing Level	0.029	0.100	0.002	19.02	5,031
				(0.000)	
Unknown Availability	0.151	0.025	0.196	14.85	4,761
				(0.000)	
Variations of First Names	1.523	1.52	1.525	0.160	4,761
				(0.871)	
Variations of surnames	1.739	1.778	1.725	1.19	4,761
				(0.235)	
Variations of DOBs	1.06	1.061	1.059	0.17	4,761
				(0.862)	

Notes: Missing DOB and Missing Level samples include all individuals who applied to the FAESL+ program for the first-time between fall 2008 and spring 2016. An incident of a name or date-of-birth is defined as a unique iteration of that name or date-of-birth as observed in an administrative dataset. These combinations include iterations of first names with and without middle name (e.g., "Oprah Gail" and "Oprah"), iterations of surnames with and without middle name (e.g., "Gail Winfrey" and 'Winfrey"). If we observe an individual with multiple first, last, and/or middle names, we iterate all possible name combinations (e.g., an individual who appears as both "Carlos Irwin Estévez" and "Charlie Sheen" would generate additional observations for "Carlos Sheen," "Carlos Irwin Sheen," "Charlie Irwin Sheen," "Charlie Estévez," and "Charlie Irwin Estévez"). All other samples are limited to individuals in our analytic sample, which is restricted to individuals who applied to FAESL+ for the first-time between fall 2008 and spring 2016 who have non-missing date-of-birth and initial English level information.

Table A8: Tax Simulation Details

Tax	Marital Status (1)	Number of Dependents (2)	Spousal Income (3)	Control Mean (4)	TOT (5)	Estimated Proportion (6)
State + Federal + 19% FICA	Single	0	N/A	654	398	0.361
State + Federal + 19% FICA	Single	1	N/A	77	112	0.022
State + Federal + 19% FICA	Single	2	N/A	-290	-86	0.026
State + Federal + 19% FICA	Single	3	N/A	-510	-199	0.013
State + Federal + 19% FICA	Married	0	None	423	259	0.103
State + Federal + 19% FICA	Married	0	$15,000	805	434	0.103
State + Federal + 19% FICA	Married	0	Same	1,327	793	0.103
State + Federal + 19% FICA	Married	1	None	-81	18	0.032
State + Federal + 19% FICA	Married	1	$15,000	322	294	0.032
State + Federal + 19% FICA	Married	1	Same	930	662	0.032
State + Federal + 19% FICA	Married	2	None	-465	-171	0.039
State + Federal + 19% FICA	Married	2	$15,000	-125	136	0.039
State + Federal + 19% FICA	Married	2	Same	586	520	0.039
State + Federal + 19% FICA	Married	3	None	-679	-276	0.019
State + Federal + 19% FICA	Married	3	$15,000	-408	5	0.019
State + Federal + 19% FICA	Married	3	Same	344	401	0.019

Notes: Column (4) reports estimated tax liabilities simulated from NBER TAXSIM 27 under the family structure and spousal income assumptions in columns (1) through (3). Estimates in column (5) report the impact of program enrollment on annual tax liabilities calculated from reported earnings under each set of family structure and spousal income assumptions. TOT estimates are calculated using equation (5) with estimated tax liabilities as the dependent variable in a longitudinal dataset that is unique at the individual-by-year level, with heteroskedasticity-robust standard errors clustered at the individual level. Proportions in column (6) are authors' calculations from ACS data describing the population of Framingham, MA (using 2017 ACS tables B05009, B09005, and S0501), assuming that spousal income is evenly split between the three categories for individuals who are married. Data restricted to 2010 to 2018 observations, the only years where full annual earnings are available (earnings data is only observed through quarter 3 of 2019). Spousal income categories of "None", "$15,000", and "Same" calculate household tax liabilities under the assumption that married couples file jointly and that household taxable earnings are equal to individual earnings ("None"), individual earnings plus $15,000 ("$15,000"), or twice individual earnings ("Same"). Alternative specifications that censor "Same" spousal earnings at $50,000 produce qualitatively similar results. All estimates include covariates and lottery fixed effects that interact incoming level with time-of-day preferences and semester of first lottery application. Covariates include gender; Asian, Hispanic or white surname; Brazilian surname; surname not attributed to any racial or ethnic group; age at lottery; baseline quarterly earnings and an indicator for missing gender. N=20,059 annual earnings observations.