

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

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The ability to speak and understand a host country's primary language is strongly associated with measures of immigrant integration. We estimate the causal effects of English language training for adult immigrants on participants' civic and economic outcomes using randomized enrollment lotteries from a public adult education program in Massachusetts. Participation doubles voter participation and increases annual earnings by \$2,400 (56 percent). Increased tax revenue from earnings gains cover program costs over time, generating a 6 percent return for taxpayers. Ours is the first randomized evaluation of adult English language training as a standalone intervention in the United States. (JEL D72, H75, I21, I26, J15, J24, J31)

C 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 and 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.

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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, and Siniver 2003; Chiswick and 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 percent wage premium, conditional on other observable characteristics. Language skills are also related to measures of social and civic incorporation (Cho 1999; Bleakley and Chin 2010).

Despite these benefits, more than 23 million adults in the United States lack proficiency in the English language (US Census Bureau 2018a). Since 1990, the limited English proficient (LEP) population in the country has grown by over 80 percent, representing about 9 percent of the adult population today (Zong and 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 English language instruction to adult learners outside the traditional K–12 and higher education systems at no or low cost to participants. Every year, these programs serve about 600,000 students in English for Speakers of Other Languages (ESOL¹) classes, a small fraction of the population that could benefit from English language training (US Department of Education 2018a). 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 been largely ignored by policymakers as a tool for immigrant integration, remaining “a neglected backwater of our education system” (Chisman, Wrigley, and Ewen 1993, 1). Since 1990, public funding for adult education has declined by about 22 percent in real dollars despite the near doubling of the LEP population (US Department of Education 2018b).² Over that same period, real public spending on elementary and secondary education grew by over 60 percent (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

¹ ESOL 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.

² In 2015–2016, total public spending on adult education in the United States was just under \$2 billion (US Department of Education 2018b).

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 and Green 2000; Gerber, Green, and Larimer 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, the observed increase in voter registration among lottery winners may partially reflect the program's impact on the probability a participant becomes a naturalized citizen.

Attending ESOL classes also has large, positive, and 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 percent 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 preexisting 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 also present exploratory evidence assessing the mechanisms that could explain our findings. Taking point estimates at face value, we find that increases in English ability are associated with earnings gains among participants but not increased rates of civic engagement, consistent with ESOL services increasing earnings via their effect on English language skills and other channels (such as information effects) driving civic behavior. We also consider the role of enrollment duration and social networks.

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 6 percent internal rate of return (IRR) over participants' working lives, suggesting a positive net return to taxpayers from public investments in adult ESOL programs and an infinite marginal value of public funds (MVPF) at or below a 6 percent discount rate (Hendren and Sprung-Keyser 2020).⁴ This rate of return—which likely underestimates the full social benefits of adult ESOL by ignoring differences

³From 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 United States. 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.).

⁴MVPF “measures the ‘bang for the buck’ of spending on a given policy. The MVPF is calculated as the ratio of two numbers: the benefits that a policy provides to its recipients divided by the policy’s net cost to the government” (Policy Impacts 2021). Since we estimate that increased tax revenues on participants’ higher reported earnings

in outcomes other than tax payments and omitting earnings that are not reported to the Massachusetts Department of Unemployment Assistance (MA DUA)—is similar to the historical returns to equity and just below the estimated returns to investments in early childhood education (Heckman et al. 2010).

Ours is the first study we are aware of to use randomization to study the effects of language training as a stand-alone intervention in the United States. Outside of the United States, several recent well-identified studies from Europe estimate the effects of integration programs that include language training using regression discontinuity designs based on test scores (Lochmann, Rapoport, and Speciale 2019) or date of arrival (Arendt et al. 2020; Sarvimäki and Hämäläinen 2016). There is little quantitative research on adult ESOL or public adult education programs in general in the United States. Two studies of adult education programs in the 1980s and 1990s that used random assignment to assign individuals to job training paired with adult education classes (including ESOL classes) found positive effects on earnings and employment, but did not estimate impacts separately for ESOL students (Zambrowski and Gordon 1993; Hamilton et al. 2001; Wrigley et al. 2003). Recently, preliminary results were published from an evaluation of a program that uses randomization to identify the effect of ESOL services paired with career coaching in the Greater Boston area, finding positive effects on earnings that are consistent with our findings (Roder and Elliott 2020). As shown in Figure 1, our estimated effects on earnings are broadly aligned with estimates of the returns to language training in Europe and the United States. Our study adds to this nascent literature by using random assignment to study the impact of ESOL services delivered in a contemporary, business-as-usual setting—that is, operating under typical conditions without additional resources, without selectively screening participants, and without being paired with additional interventions.

Our study also adds to the quasi-experimental literature on the effects of language skills on immigrant integration. Our results linking ESOL participation to increases in reported earnings are consistent with estimates that use age-at-arrival instrumental variable approaches to estimate the economic returns to language skills (e.g., Bleakley and Chin 2004; Bleakley and Chin 2008; Bleakley and Chin 2010; Isphording and Sinning 2012; Yao and van Ours 2015). In addition, our investigation of the relationship between ESOL participation and measures of civic engagement contributes to the literature on the determinants of immigrant political participation (Ramakrishnan and Epenshade 2001; Hochschild et al. 2013; Fraga 2018).

The remainder of this paper is organized as follows. In Section I, we provide background on the ESOL program we study. In Section II, we describe our data sources and present key descriptive statistics for our sample. In Section III, we present our empirical strategy and econometric models. We present our main results and robustness checks in Section IV, followed by a discussion of potential mechanisms to explain these results in Section V, and a cost-benefit analysis in Section VI. We conclude in Section VII.

cover program costs over time, the denominator of this ratio is zero (or negative) and any positive benefits for participants implies infinite MVPF.

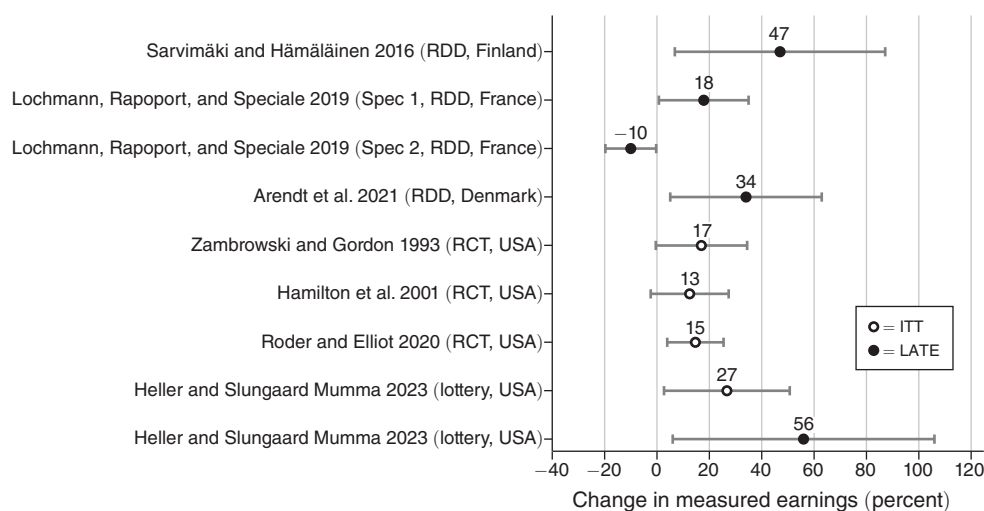


FIGURE 1. CAUSAL EFFECTS OF INTERVENTIONS THAT INCLUDE IMMIGRANT LANGUAGE TRAINING

Notes: Estimated treatment effects along with their 95 percent confidence intervals are shown. ITT estimates are plotted as outlined points, while LATE estimates are plotted as solid points. Studies are identified by their authors, date of publication, identification strategy, and country. RDD denotes a regression discontinuity design evaluation exploiting quasi-random variation in language-training policy exposure (e.g., by date of immigration or baseline language proficiency). RCT refers to randomized experiments designed to evaluate programs that included or focused on immigrant language training. Lottery refers to identification strategies exploiting randomized admission lotteries for oversubscribed language training programs. Country refers to the country where the policy change, program, or intervention took place. Where earnings estimates were reported in dollars or other currencies, effects have been converted to percentages by dividing treatment effects by the earnings of the relevant comparison group. Sarvimäki and Hämäläinen (2016) report ten-year earnings estimates in Table 3 (p. 492). Lochmann, Rapoport, and Speciale (2019) report impacts on earnings per household individual three years post-treatment (p. 282; Table 7, panel E, columns 2 and 6). Baseline income per household individual is based on authors' calculations from provided replication data, using the variable $v3_{itv}3$. Zambrowski and Gordon (1993) report effects on earnings 17–20 quarters post-treatment (p. 17). Arendt et al. (2020) report 18-year earnings estimates in Table 4 (p. 47). Hamilton et al. (2001) report 5-year earnings impacts from the Riverside, CA program (p. 87), where an explicit focus on English language training is noted (p. 33, pp. 35–36). Roder and Elliot (2020) report earnings effects two years post-treatment assignment (p. 29, Table A4). Heller and Slungaard Mumma (2023) LATE estimates correspond to the average earnings impacts in years 2 through 10 post-lottery that are presented in panel B of Table 5 below; full ITT results are available by request.

I. The Framingham Adult ESL Plus Program

In 2017, there were 103 public adult education programs serving over 18,000 students in Massachusetts, 58 percent 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 in terms of enrollment, serving over 750 students each year in Framingham, Massachusetts, a midsize city with a large Brazilian immigrant community. In addition to ESOL classes, the program also offers high school equivalency exam preparation⁵ and citizenship classes that help individuals prepare

⁵While 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 nonwinners, so we do not think differential access to high school equivalency preparation courses could explain the observed impacts on reported earnings or civic engagement.

for naturalization. While the program has a large Brazilian presence, it 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 nonprofit organization.

The FAESL+ curriculum focuses 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. The curriculum of the FAESL+ program develops English skills through content strands on US culture, civics, work, and family life. Classroom activities could include learning how to share an email address, talking about the weather and days of the week, or practicing making 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 three 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 percent 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 Once accepted into the FAESL+ program, students are guaranteed a spot in the next level course⁸ the following semester provided that they maintain good attendance. Students who do not win a spot in the FAESL+ program are encouraged to reapply and are given information about other adult ESOL programs and volunteer-led classes in the area.⁹ 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.

⁶See <http://www.faesl.org/about.html> for more program details.

⁷We identify students who are offered a seat in the volunteer-led prep class as “nonwinners.”

⁸Most students advance to the next level after completing a course; however, in some cases a student may repeat a level.

⁹We observe less than 1 percent (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 nonprofit 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.

TABLE 1—SUMMARY STATISTICS FOR 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 nonmissing date of birth and initial-level information. Asian, Black, Hispanic, and White surname are indicator variables that take on a value of 1 if 80 percent of respondents to the 2010 US census with that surname were of that racial or ethnic group and 0 otherwise. Brazilian surname is an indicator variable that takes on a value of 1 if an individual's surname was among the 100 most common surnames in Brazil, per Forebears (2019), and 0 otherwise. The indicator for having a Brazilian surname is not mutually exclusive with other racial or ethnic indicators: 31.7 percent of Brazilian surnames are classified as White, 9.9 percent are classified as Hispanic, and < 1 percent 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.

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.

II. Data and Descriptive Statistics

A. 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

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 nonmissing DOB and nonmissing level information. Lottery and enrolled student samples for 2008 include only fall applicants. Lottery and enrolled student samples for 2016 include only spring applicants.

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 (AM/PM) 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.¹⁰ 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 nonwinners) who applied to this program between fall 2008 and spring 2016 and have nonmissing date-of-birth and initial level information (see Section IVD 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 US 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 (US 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 percent of respondents to the US census with that name belong to that racial or ethnic group.¹¹ We create an indicator for having

¹⁰The 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 class levels based on Massachusetts Adult and Community Learning Services standards (see <http://www.doe.mass.edu/acsls/assessment/EFL-FAQ.html>).

¹¹Only 0.17 percent 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; therefore we do not include these

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 percent 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 percent of all registrations in the *Relacao Annual de Informacoes Sociais* (Monasterio 2017).

Gender is coded consistently across data sources but is missing for 11.4 percent 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 US Census Bureau that contains a sample of first names covering 90 percent of male and female respondents to the 1990 US census (US Census Bureau 2014; Sonnet 2015).¹²

B. 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 percent 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 percent), perhaps because the FAESL+ program serves a larger share of unauthorized 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 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 DOB 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 percent of quarterly earnings observations. The mean annual reported earnings for individuals with nonzero reported earnings in our sample is \$27,140. Overall, we match 24 percent 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 percent, 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 percent of enrolled students who responded to an

racial/ethnic groups in our final control variables..

¹²We assign individuals with missing gender data to male (female) status if 90 percent 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.

entry questionnaire reported being employed at baseline, we matched only 29 percent of enrolled students to MA DUA records. DUA-reported earnings do not cover all types of income, including income earned from self-employment, contract labor, small farms, the federal government, or working for one's spouse or child.¹³ 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.¹⁴ Finally, MA DUA earnings records do not include wages paid "under the table" (i.e., without being reported for tax or unemployment insurance purposes). This includes most wages paid to unauthorized 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).¹⁵ For these reasons, we are careful to interpret effects on earnings as effects on employer-reported earnings and not total income.

C. 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 online 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 percent 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

¹³For more information, see <https://www.mass.gov/service-details/check-eligibility-for-unemployment-benefits>.

¹⁴We 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. Unauthorized immigrants in Massachusetts were unable to get a driver's license as of 2019.

¹⁵National estimates suggest that about 25 percent of all immigrants in the United States lack authorization (Budiman 2019). While research suggests that around 50 percent of unauthorized immigrants in the United States pay taxes using an Individual Tax Identification Number, since unauthorized immigrants are unable to get driver's licenses in Massachusetts, we would not match to these earnings (Gee, Gardner, and Wiehe 2016).

TABLE 3—SAMPLE BALANCE AND FIRST-STAGE ESTIMATES

	Control mean (1)	Won lottery (2)
<i>Panel A. Baseline characteristics</i>		
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)
<i>F</i> -statistic from test of joint probability		0.634
<i>P</i> -value from joint <i>F</i> -test		0.750
Observations	3,513	4,761
<i>Panel B. First-stage estimates</i>		
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.

hours of ESOL classes. The first-stage effects reflect the fact that some applicants reapply 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 percent of the control group eventually enrolled in the FAESL+ program, and 19.6 percent of first-time lottery winners never appeared in enrollment records.¹⁶

¹⁶Individuals who attend the FAESL+ program for <12 hours of instruction are not reported as enrolled students in the state adult education reporting system; these students would be classified as “no shows” in our results.

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. Local average treatment effect (LATE) estimates imply that individuals who are induced to enroll by winning their first lottery enroll for just over three semesters on average.¹⁷ 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, and Fix 2007).¹⁸ For a student beginning at Level 1, the lowest level of English proficiency, with no ability to read, write, or speak in English, advancing to Level 3 on the Massachusetts 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).

II. Empirical Strategy

We want to measure the effects of FAESL+ attendance on voter registration, voter participation, and whether or not an individual matches to employer-reported earnings, which we express as follows:

$$(1) \quad Y_i = \beta_0 + \beta_1 \text{Attend}_i + \beta_2 X_i + \theta_{clt} + \epsilon_{iclt},$$

where Y_i is the outcome of individual i ; Attend_i is an indicator that is equal to 1 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).¹⁹ 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 1 if an individual won his or her first lottery attempt (Won). The first-stage equation is

$$(2) \quad \text{Attend}_i = \delta_0 + \delta_1 \text{Won}_i + \delta_2 X_i + \nu_{clt} + v_{iclt}.$$

¹⁷ 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).

¹⁸ McHugh and coauthors 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.

¹⁹ While courses are offered at more granular sublevels of English ability (e.g., "low beginner" or "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—and assuming that positive lottery outcomes weakly increase all applicants' probability of FAESL+ enrollment and negative lottery outcomes weakly reduce all applicants' probability of enrollment (i.e., no defiers)—we obtain unbiased LATE 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

$$(3) \quad Y_i = \beta_0 + \beta_1 \widehat{Attend}_i + \beta_2 X_i + \theta_{clt} + \epsilon_{icl},$$

where $\widehat{Attend}_i$ is the predicted value of $Attend_i$, estimated from equation (2).²⁰

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

$$(4) \quad Y_{ip} = \lambda_0 + \lambda_1 Attend_i + \lambda_2 X_i + \xi_{clt} + \psi_p + e_{iclp},$$

where Y_{ip} is individual i 's total earnings for period p , and ψ_p is a vector of period relative to the first lottery.

Using a longitudinal dataset of individual-by-year observations, we instrument for FAESL+ attendance and obtain unbiased LATE estimates of λ_1 for compliers from the second-stage equation

$$(5) \quad Y_{ip} = \lambda_0 + \lambda_1 \widehat{Attend}_i + \lambda_2 X_i + \xi_{clt} + \psi_p + e_{iclp},$$

where $\widehat{Attend}_i$ is the predicted value of $Attend_i$, estimated from equation (2), adapted to include period fixed effects. 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.

IV. Results

A. 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, 7 percent of individuals were registered to vote in the state of Massachusetts, as shown in column 2.²¹ 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 9.0 (SE 2.2)²² percentage points, more than double the control mean. The estimated effect on ever participating in a post-lottery

²⁰ 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 online Appendix Table A11.

²¹ We define voter registration date as reported in the NationBuilder file.

²² Hereafter, we present standard errors in parentheses following each point estimate.

TABLE 4—EFFECTS ON CIVIC OUTCOMES

	Control mean (1)	Ever enrolled (2)	Sample (3)
<i>Panel A. Voting and voter registration</i>			
Registered to vote	0.07	0.090 (0.022) 4,761	F2008–S2016
Voted	0.06	0.078 (0.021)	F2008–S2016
Observations		4,761	
<i>Panel B. Voting by general election</i>			
Voted in 2010	0.01	0.000 (0.008) 1,897	F2008–F2010
Voted in 2012	0.02	0.016 (0.016) 2,827	F2008–F2012
Voted in 2014	0.01	0.017 (0.010) 3,739	F2008–F2014
Voted in 2016	0.05	0.072 (0.019)	F2008–S2016
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.

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 reelection 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 2 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. We note that the voting effect may emerge over time in part due to restrictions on when a green card holder is eligible to naturalize, a prerequisite to registering to

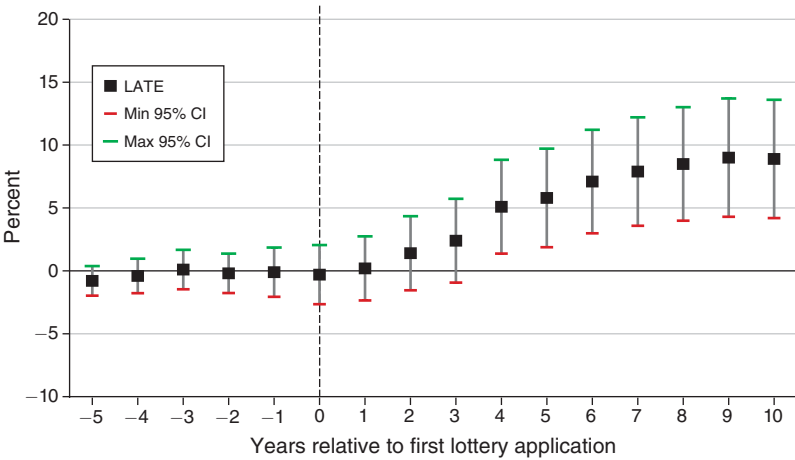


FIGURE 2. CUMULATIVE EFFECTS ON PROBABILITY OF HAVING REGISTERED TO VOTE, BY YEAR SINCE FIRST LOTTERY

Notes: Year of voting is defined relative to first lottery (year = 0). LATE 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 registered to vote by the indicated period. Online Appendix Table A2 records the point estimates plotted here.

vote, since most green card holders are eligible to become citizens only after living continuously in the country for at least five years. Additionally, the four-year US general election cycle may influence the timing of voter engagement relative to an individual’s first lottery, as some participants may become eligible to vote during time periods where political processes and voting are particularly salient (e.g., in the run-up to a presidential election). For instance, we see the highest overall rates of turnout in our sample in presidential election years (i.e., 2012 and 2016), where point estimates and baseline levels of voter participation are at least twice as large as the preceding midterm years (i.e., 2010 and 2014).

B. Employer-Reported Earnings

Attending adult ESOL courses substantially increases 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

TABLE 5—EFFECTS ON EMPLOYER-REPORTED EARNINGS

	Control mean (1)	Ever enrolled [earnings in \$] (2)	Ever enrolled [Ln(earnings in \$)] (3)
<i>Panel A. Matched to employer-reported earnings</i>			
Ever matched	0.21	0.042 (0.028) 4,761	—
Quarters matched	3.78	1.64 (0.67)	—
Observations		4,761	
<i>Panel B. Average annual earnings</i>			
Annual earnings, through Y_{10}	\$4,022	1,843 (771) 32,770	0.464 (0.223) 32,770
Annual earnings, Y_2 – Y_{10}	\$4,147	2,388 (911)	0.557 (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 period fixed effects.

on when an individual first applied to the FAESL+ program.²³ 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 2 to 10, 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 2, as shown in Figure 3.²⁴ Over the full post-lottery period, enrollees report an additional \$1,843 (\$771) annually, and from years 2 to 10, enrollees report an additional \$2,388 (\$911) in earnings each year. The change in annual reported earnings represents a 46–56 percent increase for enrollees relative to their peers who did not enroll at FAESL+ because of their lottery outcome (We interpret the LATEs on $\ln(\text{Earnings})$ in column 3 as percentage changes in reported earnings.

²³ 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).

²⁴ 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 ($p = 0.556$).

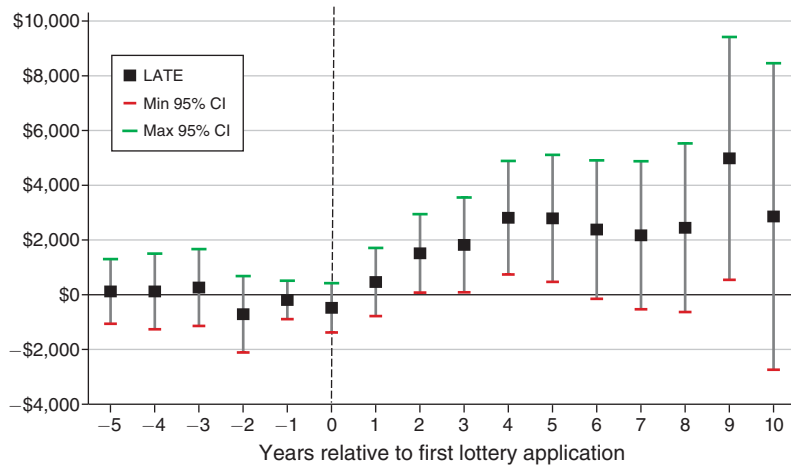


FIGURE 3. ANNUAL EFFECTS ON REPORTED EARNINGS, BY YEAR SINCE LOTTERY

Notes: Year of reported earnings is defined relative to first lottery (year = 0). LATE 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. Online Appendix Table A3 records the point estimates plotted here.

Dividing the LATEs from column 2, which estimates impacts on earnings in dollars by the control means in column 1, yields similar results).

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 online Appendix Table A5, we present alternative specifications that address this issue by estimating effects over a series of balanced panels (panel A) and reweighting 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 3 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 nonparticipants from the pre-period 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 4 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)

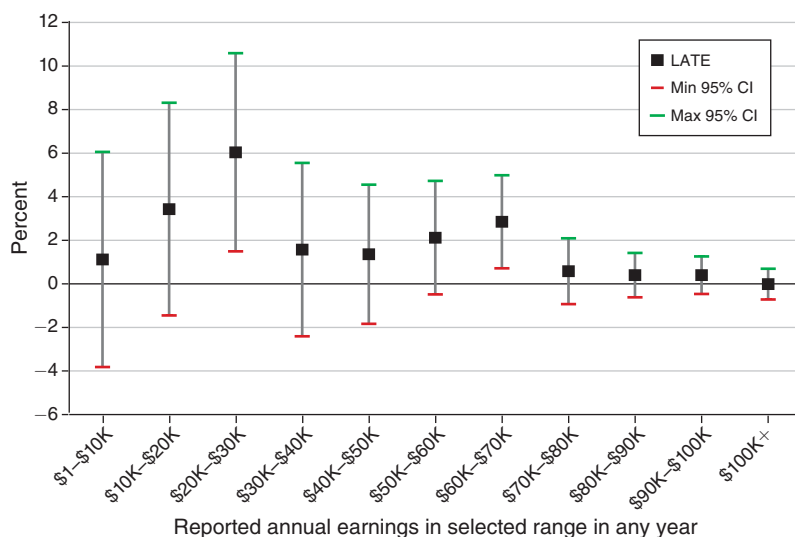


FIGURE 4. CUMULATIVE EFFECTS ON PROBABILITY OF EVER REPORTING EARNINGS IN SELECTED RANGES

Notes: LATE 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. Online Appendix Table A4 records the point estimates plotted here.

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.

C. 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. 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.²⁵ 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

²⁵ Results for individuals who ever report earnings during the period of our study are similar to estimates for individuals with positive pre-period earnings.

TABLE 6—HETEROGENEITY OF EFFECTS

	Male	Female	Beginner	Intermediate or advanced	Brazilian surname	Non- Brazilian surname	Pre-period earnings > \$0	Pre-period earnings = \$0
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A. Voting and voter registration</i>								
Registered to vote	0.049 (0.029) 1,929	0.117 (0.032) 2,832	0.086 (0.024) 4,191	0.139 (0.063) 570	0.084 (0.032) 2,093	0.099 (0.031) 2,668	—	—
Voted	0.057 (0.029)	0.089 (0.030)	0.074 (0.022)	0.107 (0.065)	0.082 (0.030)	0.080 (0.030)	—	—
Observations	1,929	2,832	4,191	570	2,093	2,668		
<i>Panel B. Matched to earnings data</i>								
Ever matched	0.030 (0.042) 1,929	0.052 (0.038) 2,832	0.033 (0.031) 4,191	0.055 (0.077) 570	0.012 (0.037) 2,093	0.043 (0.041) 2,668	−0.018 (0.051)	0.054 (0.032) 2,643
Quarters matched	1.71 (1.05)	1.51 (0.85)	1.32 (0.72)	3.21 (1.88)	0.20 (0.76)	2.35 (1.01)	4.24 (1.87)	0.62 (0.52)
Observations	1,929	2,832	4,191	570	2,093	2,668	531	2,643
<i>Panel C. Average annual earnings</i>								
Annual earnings, through Y_{10}	2,055 (1,379) 13,452	1,627 (805) 19,318	922 (779) 28,686	7,036 (2,521) 4,084	−549 (842) 14,722	3,423 (1,208) 18,048	6,167 (3,278) 3,187	662 (654) 15,300
Annual earnings, $Y_2 - Y_{10}$	2,777 (1,650)	2,009 (937)	1,281 (927)	8,658 (2,935)	−457 (990)	4,343 (1,457)	9,803 (4,340)	896 (873)
Observations	10,263	14,557	21,691	3,129	11,362	13,458	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 period effects. Beginner and intermediate/advanced subgroups are identified based on initial (entry) level of English. 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.

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 nonbeginners, 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.

In columns 5 and 6, we compare impacts by whether an applicant has a Brazilian surname. We find no differences in impacts on civic outcomes, but we find that non-Brazilian applicants drive the earnings results. While it is unclear whether and how social, cultural, or baseline skill differences between these groups influence our results, we consider the implications of this dimension of treatment effect heterogeneity in the mechanisms section below.

D. Placebo and Robustness Checks

To assess the validity of our identification strategy, we present results from a number of falsification tests in online Appendix Table A6. In panel A, we consider whether lottery winners are more likely than nonwinners 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 2 and 3—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.

For a small minority of applicants to FAESL+, we are missing data on DOB, class level, or time preference that are necessary to match observations to outcomes or identify the lottery an individual participated in. (See online Appendix Table A7 for detail on the incidence of missing data.) In online Appendix A, we discuss a number of sensitivity tests we conduct to determine whether missing data are driving our results. We find that missing data are unlikely to bias our estimates (see online Appendix Table A8).

Since our outcome data are limited to the state of Massachusetts, we may also be concerned that differential out-of-state mobility for lottery winners and nonwinners could bias our results. In online Appendix B, we discuss the tests we conduct to assess this possibility by merging to out-of-state voting records, looking at patterns of within-state mobility, and examining patterns in earnings data reporting over time. We determine that out-of-state mobility is unlikely to be a substantial source of bias (see online Appendix Tables A9 and A10).

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 online Appendix Table A11, we present results from an alternative specification where we define treatment as completing a semester at the FAESL+ program.²⁶ These estimates rescale our effect on ever enrolling by the inverse of the LATE 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

²⁶We 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.

vote by 2.8 percentage points and increases average annual reported earnings by \$540 to \$682.

V. Mechanisms

We have demonstrated that attending ESOL courses positively impacts voting and employer-reported earnings. Until now, we have implicitly assumed that attending ESOL classes affects these outcomes because attendance improves participants' English language skills. However, attending ESOL classes at FAESL+ may affect participants in other ways, such as by changing their social network or increasing access to information. In this section, we consider the available evidence on mechanisms that could explain our findings.

Improved English language skills—the focus of ESOL courses—is the most obvious mechanism to explain changes in participants' outcomes. One challenge to exploring whether language skills drive treatment effects is that we do not observe a post-lottery measure of English ability for applicants who do not attend FAESL+. Because of this, we cannot leverage our lottery strategy to estimate the causal effect of attending FAESL+ on English ability. Nonetheless, back-of-the-envelope calculations indicate that attending ESOL classes does meaningfully increase English ability. Among the subsample of FAESL+ participants who took the same test of English ability more than once,²⁷ their English language skills improved by 0.163 standard deviations (0.228 Educational Functional Levels, or EFLs)²⁸ for every 100 hours they were enrolled.

We find suggestive evidence that improvements in language skills are positively associated with earnings growth but not with voting outcomes. In column 1 of Table 7, we report the coefficient on standardized growth in English language proficiency²⁹ from a regression where we use proficiency growth to predict the outcome listed in each row, controlling for individual demographics and lottery fixed effects. In column 2, we replace our standardized measure of proficiency growth with the student's growth in EFL levels.³⁰ Panels B and C of Table 7 show that, in general, growth in language proficiency is positively related to reported earnings (although we are underpowered to rule out null effects). In contrast, panel A shows that there

²⁷ Most FAESL+ students are initially assessed using the Basic English Skills Test Plus (BEST+), which uses a scripted interview process to measure an individual's oral fluency in English. However, students who are identified as having advanced skills or who advance beyond beginner classes may later take the Comprehensive Language Assessment System-English (CLAS-E) test, which assesses oral fluency, reading ability, and writing ability in English, as a post-test (early cohorts also used the Arlington Education and Employment Program (REEP) exam to assess advanced students' English writing skills). Since these tests measure different dimensions of English language proficiency, we omit students who change tests from the descriptive analyses in Table 7.

²⁸ An EFL is a standardized measure of English ability used nationally to measure language skills for adult learners. The FAESL+ program used three different skill assessments over the period of our study (BEST+, CLAS-E, and REEP). Student scores on these tests were equated to EFL levels based on National Reporting System for Adult Education guidelines (see <https://www.nrsweb.org/>).

²⁹ Among students who take the same test of English Ability, we standardize score growth (the simple difference in scores between a student's pre- and post-tests) by assessment (BEST+, CLAS-E, and REEP) to have a mean of 0 and standard deviation of 1 within our sample.

³⁰ A student's EFL level is calculated by mapping their pre- and post-test scores to EFL levels, per National Reporting System for Adult Education guidelines. EFL growth is defined as the simple difference in levels between a student's pre- and post-tests.

TABLE 7—CORRELATION BETWEEN GROWTH IN LANGUAGE SKILLS AND OUTCOMES

	Standardized score growth (1)	Level growth (2)
<i>Panel A. Voting and voter registration</i>		
Registered to vote	−0.003 (0.01) 1,017	0.002 (0.007) 1,017
Voted ever	0.002 (0.009) 1,017	0.005 (0.007) 1,017
<i>Panel B. Matched to employer-reported earnings</i>		
Ever matched	−0.009 (0.013) 1,017	−0.008 (0.010) 1,017
Quarters matched	0.255 (0.316) 1,017	0.064 (0.225) 1,017
<i>Panel C. Average annual earnings</i>		
Annual earnings, through Y_{10}	562 (388) 7,066	101 (245) 7,066
Annual earnings, $Y_2 - Y_{10}$	745 (465) 5,349	190 (299) 5,349

Notes: Results are estimated by regressing the outcome in each row on the measure of English language skill growth in each column in a longitudinal 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. 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. Sample is restricted to FAESL+ participants who took the same test of English proficiency two or more times, as described in Section V of the text. 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.

is no association between growth in English ability and voting outcomes, suggesting improvements in English skills are not driving these effects. However, we interpret these results with caution, given the limitations in our measure of language growth and the subsample we can include in this analysis.

A related mechanism that could explain differences in treatment effects is the quantity of additional language training that lottery winners are induced to complete. First, we assess whether there are differences in effects on enrollment intensity among subgroups where we observed treatment effect heterogeneity in Table 6. Even among subgroup pairs with large differences in earnings effects, we can rule out modest differences in effects on enrollment intensity (see online Appendix Table A13). Second, we estimate first-stage impacts and treatment effects by lottery cohort to assess whether these estimates covary. We find that cohort-level first-stage impacts on enrollment intensity are unrelated to impacts on reported earnings ($\rho = -0.010$; see online Appendix Figure A2.A), but positively related to impacts on voting ($\rho = 0.459$; see online Appendix Figure A2.B). Interpreting these results is complicated by the fact that the drivers of enrollment duration

could be positively correlated with participants' underlying economic and civic outcomes in unobserved ways (e.g., if longer enrollment spells reflected qualities like determination, taste for education, motivation to learn English, or propensity to naturalize), but they could also be negatively correlated (e.g., if longer enrollment spells are associated with lower baseline skills, poor economic conditions, limited job prospects, or low opportunity cost of time). While we lack the data to directly test why a stronger contrast in enrollment intensity would translate to stronger voting effects but not earnings effects, the contrast in results suggests that different mechanisms may explain changes in immigrants' civic and economic integration.

Another potential explanation is that attending ESOL classes impacts an individual via network effects that arise from social interactions with fellow students. Putnam (2007) notes the vital role of social capital in immigrant integration and the contrast between "bonding" social capital—developed by building relationships *within* one's national, ethnic, or linguistic group—and "bridging" social capital, developed by building relationships *between* groups. One way to assess the relative roles of inter- and intragroup relationships would be to identify how students' social networks change as a result of winning a lottery to attend FAESL+ and compare treatment effects for students who experience larger or smaller changes. While we do not directly observe these changes, we can think about the proportion of classmates who do not share the student's race, ethnicity, or nationality as a proxy for intergroup exposure. Individuals in larger racial, national, language, or ethnic groups may experience smaller changes in intergroup exposure—and more opportunities to build relationships within their own group—than individuals in smaller groups.

We find that participants with more potential intergroup exposure experience larger effects on earnings, but not voting outcomes. When we divide our sample by Brazilian nationality (see columns 5 and 6 of Table 6)—the largest immigrant group in the FAESL+ program—we find that attending FAESL+ has strong, positive, and similarly sized effects on voting outcomes for both groups. However, we find that our earnings results are driven by increases in reported earnings among non-Brazilians.³¹ While these results are consistent with the hypothesis that intergroup (as opposed to intragroup) relationships drive some of the economic effects, we note that other differences between minority and majority groups within FAESL+ may also explain this treatment effect heterogeneity. For example, students in minority language groups may perceive larger payoffs to developing English fluency and work harder to develop their language skills, or participants from larger groups may have more opportunities for informal or contract employment that does not appear as employer-reported earnings. Overall, we see that Brazilian participants have lower rates of baseline reported earnings (10.7 percent) than other FAESL+ participants (20.8 percent). Thus, the differences in earnings we observe for these groups may be specific to the relatively low rates of formal labor market participation for Brazilians in our sample and may not

³¹ The results are remarkably similar if we instead split the sample by whether an applicant is part of the modal racial or ethnic group in their lottery cohort, consistent with the fact that 93 percent of the "modal ethnic group" sample across all lotteries is Brazilian.

generalize to other contexts or immigrant communities. The contrast between the earnings and voting results provides further evidence that different mechanisms drive the observed changes in civic and economic integration.

A final explanation we consider for the effects of the FAESL+ program is that they reflect the impact of attending FAESL+ on participants' access to information. While we are unable to address this possibility empirically, we find some evidence of this mechanism anecdotally. For example, current students in ESOL classes offered by the FAESL+ program are more likely to be aware of and take advantage of the FAESL+ program's Citizenship Prep classes that help students prepare for the citizenship exam.³² Reports from program directors also indicate that teachers, administrators, and fellow students in this program act as resources to participants, sharing information or directing them to services to help with day-to-day challenges such as tax preparation, finding a job, securing childcare, or getting a driver's license.

In summary, we find empirical or anecdotal evidence to support a role for language skills, social networks, and informational effects as potential mechanisms to explain the impacts of ESOL participation on civic and economic outcomes that we observe. We find suggestive evidence that language skills as well as intergroup social networks may influence earnings. We find that voting outcomes are not related to language growth or group membership, suggesting that other explanations—such as enrollment duration or information effects—may drive these results.

VI. 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 nonpecuniary 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 MVPF spent on ESOL services (Hendren and 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 percent 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 online Appendix Table A12 and informed by population-level data from the American Community Survey (Feenberg and Coutts 1993). Next, we regress an individual's estimated annual tax obligations on predicted program attendance, as in equation

³² Enrollment in FAESL+ Citizenship Prep classes is not limited to FAESL+ ESOL students. We are unable to comprehensively recreate records for Citizenship Prep classes to identify what portion of participants enroll in these classes each semester. However, conversations with program administrators suggest that these classes are relatively small (about 20 students a semester) and about 80 percent of students are former or current ESOL students.

(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 LATE estimates from each family structure model by the approximate proportion of the sample each family structure represents (see column (6) of online Appendix Table A12).

On average, adult ESOL classes substantially increase participants' state income tax, federal income tax, and FICA payments. In Table 8, we present estimates of the net present value of investments in ESOL services as well as the 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 assume there is no change in tax liabilities for the first two years. We then assume that the estimated annual change in post-enrollment tax payments kick in two years after the first lottery application and 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 and 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 percent 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 percent ($NPV = \$7,987$ at $r = 3\%$). In our preferred estimates, we include 19 percent of FICA contributions as benefits to taxpayers, corresponding to the portion of FICA that funds Medicare.³³ These assumptions imply an IRR of 6.0 percent ($NPV = \$1,731$ 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 percent), approximately double the 3 percent rate used in similar analyses, our tax simulation implies that as long as aggregate willingness to pay (WTP) for services is positive,³⁴ the MVPF invested in adult ESOL services is infinite (i.e., $MVPF_{ESOL} = \frac{\sum_i WTP_i}{NetCosttoGov't} = \frac{+}{0} = \infty$) (Hendren and Sprung-Keyser 2020). Table 8 and online Appendix Table A12 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.

³³It 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 percent of FICA contributions fund the Social Security Administration (6.2 percent of the 7.65 percent tax on employee wages), we treat the remaining 19 percent (funding Medicare) as a social benefit.

³⁴The program's excess demand, large impacts on reported earnings, and the presence of private, for-profit ESOL providers strongly suggests this is the case.

TABLE 8—COST-BENEFIT ANALYSIS

Tax	IRR	NPV at r = 0%	NPV at r = 1%	NPV at r = 3%	NPV at r = 5%	Annual Δ in tax payments	Years before NPV > \$0 at r = 3%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
State + Federal + 19% FICA	6.0%	\$5,022	\$3,618	\$1,731	\$428	\$347	20
State + Federal income tax	3.5%	\$2,657	\$1,701	\$269	−\$718	\$265	27
Federal income tax	−0.2%	−\$110	−\$689	−\$1,555	−\$2,148	\$162	n/a
State income tax	−3.1%	−\$1,724	−\$2,084	−\$2,618	−\$2,981	\$103	n/a
Federal income tax + FICA	11.7%	\$11,592	\$9,420	\$6,157	\$3,897	\$596	11
State + Federal + FICA	13.9%	\$14,369	\$11,819	\$7,987	\$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 online Appendix Table A12. 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 LATE estimate forward for a total of 27 years. The 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.

VII. Discussion

We leverage the randomized enrollment lottery of one of the largest public adult ESOL programs 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 nonwinners. 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 prerequisite for registering to vote. While we are unable to observe citizenship status in our data, we note that our voting results are consistent with ESOL participation increasing rates of naturalization or increasing civic engagement among immigrants who are or become eligible 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 percent. Extrapolating from our tax liability estimates, this incremental \$2,388 a year in employer-reported earnings translates into an incremental

\$55,107 in take-home earnings over the working life of an average participant.³⁵ 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 preexisting human capital (Chiswick and 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 percent. 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).

Our findings suggest that the current rationing of adult ESOL services in Massachusetts is inefficient from a long-run budgetary perspective and that the federal government would benefit from subsidizing an expansion of services to meet demand. Additionally, under conditions of perfect information, based on the high earnings returns we observe, we would expect individuals who do not win a chance to enroll to seek out private adult ESOL service providers or to be willing to travel to undersubscribed public programs in other cities. The fact that very few applicants (< 1%) enroll in other public ESOL programs could mean that applicants underestimate the returns to participation, face transportation challenges that make it difficult to attend other programs, or view competing public programs as inferior in quality. Anecdotally, applicants do not appear to view private ESOL services as a substitute for public classes. This may be because applicants face liquidity constraints that force them to rule out unsubsidized programs or that government-subsidized classes have crowded out private providers. There is an active private market for adult ESOL services and tutoring in Framingham,³⁶ but other low-cost, nonprofit, or volunteer-led programs are generally very small, and conversations with FAESL+ program staff suggest that higher-cost, for-profit providers offer services that are financially out of reach for most FAESL+ applicants. If the current environment of rationed services is due to governmental capacity constraints, the state might benefit from working with private providers to expand services to meet demand.

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 state median 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

³⁵ We assume that the increase in annual reported earnings and corresponding increase in annual tax obligations both begin two years post-lottery and continue for 27 years on average.

³⁶ FAESL+ shares the names and contact information for 23 volunteer, nonprofit, and private tutoring services in Framingham and surrounding communities with individuals who do not win a lottery to enroll in their program. Information and links to these providers are also posted on the FAESL+ website.

may attenuate estimated effects on earnings and tax revenue. Although immigrants select into applying to these services, the long waitlists for ESOL programs across Massachusetts suggest that it is likely that these programs could be substantially expanded while continuing to serve a population of similarly motivated English learners. Nationwide, public adult ESOL programs served roughly 600,000 participants in 2016, representing only a small fraction ($< 3.2\%$) of the United States' 19.2 million working-age immigrants who report speaking English less than very well (Wilson 2014; US Department of Education 2018a). It is unclear how much total unmet demand for adult ESOL services exists or how such demand is geographically distributed.

One limitation of this current study is our inability to fully isolate the mechanisms that explain the effects we observe. ESOL services may influence participants' civic and economic outcomes primarily through improving their English language skills—which could conceivably be replicated through lower-cost interventions like language learning software—or differences in outcomes could be driven primarily through social or informational aspects of the program—which perhaps could not. While we find suggestive evidence that language is an important pathway for economic (but not civic) outcomes, future work in this area should focus on mechanisms given their implications for policymakers seeking to effectively scale up adult ESOL services.

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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