

Did the Americanization Movement Succeed? An Evaluation of the Effect of English-Only and Compulsory Schooling Laws on Immigrants[†]

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We provide the first estimates of the effect of statutes requiring English as the language of instruction and compulsory schooling laws on the school enrollment, work, literacy, and English fluency of immigrant children during the Americanization period (1910–1930). English-only statutes moderately increased the literacy of certain foreign-born children, particularly those living in cities or whose parents were not fluent in English. However, these laws had no impact on immigrants' eventual labor market outcomes or measures of social integration (from 1940 census and WWII enlistment records). Only laws regulating the age when children could work significantly affected immigrant outcomes. (JEL I21, I26, I28, J13, J15, N31, N32)

“Everyone should speak English or just shut up, that’s what I say.”

—Calvin, *Calvin and Hobbes*

During the last decades of the twentieth century, immigration rates to the United States increased substantially, reaching their highest levels since the closing of the border in 1924.¹ A significant portion of these immigrants arrived from Spanish-speaking countries such as Mexico and El Salvador. A resurgence of legislation geared towards making English the official language of the states has accompanied this migration; sixteen states passed such laws between 1980 and 1990 alone (Arington 1991). The proponents of official language laws also perceive bilingual education programs as detrimental to immigrants and support legislation designating English as the main language of instruction in public schools. Arington summarizes supporters’ views as arguing that “current bilingual programs maintain native languages and cultures rather than teach English as quickly as possible,

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¹The US foreign-born share ranged from 13 to 15 percent between 1870 and 1910. In 2000 the foreign-born share was about 12.5 percent (calculations use published census tabulations).

and that English-only laws would ensure that all citizens of this country gain the fluency in English needed for full participation in America's political process and socioeconomic lifestyle."

In a similar spirit, the 2002 No Child Left Behind legislation changed the name of the Bilingual Education Act (enacted in 1968) to the English Language Acquisition, Language Enhancement, and Academic Achievement Act. The No Child Left Behind law also decreased federal funds for bilingual education, focusing instead on English acquisition. States with large fractions of immigrants had ended their bilingual education programs even earlier: for instance, California passed Proposition 227 in 1998, which ended bilingual education and instead emphasized English language immersion. As a result there has been a large decline in the fraction of limited English proficiency students instructed using their native language (Zehler et al. 2003).

Although the costs and benefits of bilingual education are still being debated, this is not the first time in American history that states have experimented with education legislation aimed at assimilating immigrants. This paper investigates the impact of an earlier set of policies known as the Americanization Movement, the term given to the diverse attempts of states to assimilate the millions of Europeans who came to the United States in the late nineteenth and early twentieth centuries. Nativist legislators of the day perceived these immigrants, mainly from southern and eastern Europe, as a threat to society for a host of reasons. In addition to differences in religion—most recent immigrants were Catholic or Jewish instead of Protestant—the newcomers had low literacy rates, often could not speak English, and were generally unfamiliar with American customs. Poorly educated immigrants were seen as vulnerable to exploitation by political parties since they "lacked any conception of democracy and willingly would submit to the authoritarian yoke that had bound them in Europe" (Ross 1994, 12). Furthermore, poor parents often placed their children in the labor force or enrolled them in parochial schools where they would be taught in their native language, instead of sending them to public schools where they would learn English and be exposed to American culture.

The onset of World War I magnified concerns over immigrant assimilation. Although laws that regulated language of instruction in schools had been in place in some states since the nineteenth century, there were many changes after 1910, particularly in the years immediately following the war. For example, 16 states passed legislation making English the sole language of instruction in schools in 1919 alone. States also made compulsory schooling laws more stringent. In this paper we ask whether these education laws, which often targeted immigrants, were effective at increasing their English fluency, literacy, and overall education levels between 1910 and 1930.

Previous work has examined the effects of compulsory schooling and child labor laws on the educational attainment of natives and found that this legislation had positive but modest effects on their education (Lleras-Muney 2002; Goldin and Katz 2011; Clay, Lingwall, and Stephens 2012). However, no work documents whether these laws affected *immigrants* or whether the impact on them was larger, as legislators intended. To our knowledge this is the first paper to empirically assess the impact of English-only laws from this period.

We collected data on the passage of English-only laws between 1910 and 1930 and make use of previously collected laws on compulsory schooling and child labor during the same period. Using the 1910, 1920, and 1930 censuses, we assess how these laws affected the enrollment, literacy, employment, and English fluency of children. We find that English-only laws did in fact increase the literacy of certain foreign-born populations, including children who resided in central cities or had parents who could not speak English, but the effects are very modest in size. Compulsory schooling laws also raised immigrants' enrollment, and these effects were larger for children born abroad than for native-born children.

We then investigate whether these laws affected immigrant adult outcomes using the 1940 census and enlistment records from WWII. Overall we find that keeping children in school by increasing the age required to obtain a work permit resulted in higher educational attainment and earnings, and the effects are larger for immigrant children than for natives. English-only laws, on the other hand, were not associated with better outcomes among immigrants later in life, including wages, becoming a citizen, or serving in the Army. We find no robust effects of continuation school laws or laws regulating the age at which children must start attending school.

Our paper contributes to the growing literature on the role of language of instruction on the educational outcomes of immigrant children. Slavin et al. (2011) find no difference in the English language skills of students randomly assigned to either bilingual education or English immersion programs. Similarly Matsudaira (2005) finds zero to small differences in the educational achievement of children who were eligible to participate in either English immersion or bilingual education programs. Chin, Daysal, and Imberman (2013) study the effects of bilingual education programs in Texas and find small and statistically insignificant effects on the test scores of students for whom English is a second language.² Also related to our paper is Angrist, Chin, and Godoy (2008)'s study showing that the move from English instruction to Spanish in Puerto Rico had no adverse consequences on the English-speaking abilities of adults, calling into question the importance of English instruction for English proficiency.

On the other hand, a study by Zadovny (2000) reports that making English the official language of the state adversely affects workers with limited English proficiency. Thus, a reasonable conclusion from the extant literature is that English language school programs do not help or hurt immigrant children, though official language laws might adversely affect immigrant adults. Our results from the Americanization Movement are largely consistent with the conclusions from the contemporary literature: forcing immigrants to receive instruction in English does not appear to have had economically meaningful impacts on their educational attainment or labor market outcomes.

This paper is organized as follows. We begin by reviewing in more detail the historical accounts of why English language legislation was passed during the first decades of the twentieth century, and we empirically analyze what state characteristics can account for the passage of these laws (Section I). Then we describe the

²However, they find spillover effects for other students.

methodology and data that we use to examine immigrant outcomes (Section II). We estimate the effect of education laws on immigrants and ask whether the contemporaneous effect of the legislation was larger for immigrants than for natives (Section III). We then investigate the long-term effects of the laws on immigrants using data from the 1940 census and enlistment records from World War II (Section IV). We close by discussing the magnitudes of the effects and explanations for the overall findings (Section V).

I. The Americanization Movement and Education Laws

A. *Historical Background of Americanization Laws*

The Americanization Movement was spurred by concerns about the assimilation of large numbers of immigrants who were markedly different from the existing population. The United States absorbed nearly 23 million European immigrants between 1880 and 1915, and by 1910 nearly 15 percent of the American population was foreign born. New immigrant flows had also grown more diverse and no longer consisted primarily of northwestern Europeans. The influx of German and Irish Catholic immigrants in the 1840s had already sparked the “Know Nothing” nativist movement, and the arrival of Russian Jews, Catholic Italians, and other southern and eastern Europeans after 1880 was met with further alarm by the Protestant establishment. A series of attempts to restrict these immigrant flows at the national level culminated in the National Origins Act of 1924, which effectively closed the US to further immigration from countries outside northwestern Europe.

Legal and historical accounts of the Americanization Movement emphasize concerns over the diversity of immigration during this period and the associated need for legislation to promote assimilation.³ The question of how to educate foreigners in the English language and convey “American values” had been the subject of debate since at least the late 1880s, and in fact many states passed laws regulating the language of instruction in schools in the late nineteenth century. City initiatives were also launched in the early twentieth century: between 1913 and 1917, the cities of Detroit, New York, and Los Angeles all established and funded special programs to educate their foreign-born populations. Many private groups also participated in the effort to teach foreigners English and American customs. The D.E. Sicher Company in New York, the Ford Motor Company in Detroit, and the Pennsylvania Railroad System offered English courses to their foreign employees as early as 1913 (Hill 1919). The Young Men’s Christian Association is estimated to have taught English to more than 55,000 immigrants by 1912 (Hartman 1967).

Despite these private and early public efforts, the onset of World War I magnified concern over unassimilated immigrants living in the United States, particularly those of German descent (O’Brien 1961). In 1914 the use of the German language

³For instance, Frank Trumbull of the National Americanization Committee was quoted in 1915 in the New York Times as saying, “It has come to us that we are a country full of unassimilated groups within groups with varying social ideals, varying ideas of American citizenship and loyalty to America (...) Americanization is a complex matter (...). But there can be no doubt about the first steps – the English language and the principles of American citizenship.”

was still prevalent in German-American social clubs, newspapers, churches, and parochial schools. Even some public schools offered instruction in German. Concerned Americans believed that the continued use of German by immigrants and their children preserved loyalty to Germany, prevented assimilation, and undermined the absorption of American values and good citizenship. Lack of English literacy also undermined the readiness of the military: the draft revealed that 700,000 out of 10,000,000 registrants for the war could not read or write in English (Hartman 1967). The spirit of the Americanization movement is well captured by the following quote from the Americanization Bill hearings in 1919:

*A BILL to promote the education of native illiterates, of persons unable to understand and use the English language, and of other resident persons of foreign birth; to provide for cooperation with the States in the education of such persons in the English language, the fundamental principles of government and citizenship, the elements of knowledge pertaining to self-support and home making, and in such other work as will assist in preparing such illiterates and foreign-born persons for successful living and intelligent American citizenship.*⁴

While the use of the German language in schools was a primary driver of Americanization efforts, other immigrant groups also faced a backlash during and after the war. For instance, Mexicans in Texas were accused of supporting Germans, and individuals of Japanese descent in Hawaii were thought to support the Japanese Empire (Tamura 1993). Immigrants from Eastern Europe were also the subject of nativist concern. Oregon's 1919 English-only law mentions "[...] the appalling turbulence of the world's chaotic political and social conditions particularly in the Bolshevik and soviet countries of Eastern Europe and the probability of such contagion extending over and permeating our own American government..."⁵

State legislators responded to the concern over these immigrant groups with bundles of laws aimed at assimilating children through required English-language schooling. Nativist legislators believed that while they could not control the use of German by adults, they "hoped that the schools would break the German language cycle" and reach children of all nationalities in their formative years (Ross 1994, 45). Common components of laws included English as the required language of instruction, compulsory schooling for children by age, and limits on child labor. For example, a 1919 law in Minnesota reads:

Every child between 8 and 16 years of age shall attend a public school, or a private school, in each year during the entire time the public schools of the district in which the child resides are in session; ... A school, to satisfy the requirements of compulsory attendance, must be one in which all the common branches are taught in the English language, from textbooks written in the English language and taught by teachers qualified to teach in the English language. A foreign language may be taught when such

⁴United States Education and Labor Committee (Senate, 66:1).

⁵Oregon Laws of 1919, Ch. 19, Sec. 1, p. 34.

*language is an elective or a prescribed subject of the curriculum, not to exceed one hour each day.*⁶

This law is careful to state that schools needed to be conducted in English, so a German-speaking parochial school would not count; bilingual programs or schools with a large fraction of instructional time in a foreign language would not meet the law's requirement either. Statutes often included a minimum age at which a child could obtain a work permit as well. Laws differed across states somewhat: some covered all schools (private or public), whereas others only applied to public schools. Some laws explicitly banned instruction in foreign languages, often specifically German. Although this was in part a reaction to the war, it was also due to the fact that German was by far the most commonly studied foreign language in secondary public schools. About 20 percent of students enrolled in public secondary schools in the 1909 to 1910 school year were learning German, compared with 8 percent learning French. German was also very popular in private secondary schools, with 15 percent of pupils studying the language and 20 percent learning French in the same year.⁷

Efforts to teach foreigners English were often coupled with "Americanization courses." For instance, Iowa required all public and private schools to teach American citizenship and for high schools to teach American history and civics. Kansas, New Jersey, South Dakota, and Nebraska also passed similar laws requiring instruction in "American values." For example, New Jersey established courses to teach "the form of government and the laws of this state and of the United States" (Rider 1920).⁸ Thus, English-only laws were sometimes associated with a shift in the curriculum of instruction. However, most historical accounts emphasize that both public and private efforts focused on "English first" as the main objective of the Americanization movement (Hartman 1964). English-only programs effectively ended nascent efforts to provide bilingual education, as many were termed "un-American" (Pavlenko 2002). Direct evidence on whether these laws were enforced is unavailable, but states did appropriate monies to fund Americanization programs.⁹ Some states even went as far as imposing fines on noncompliant aliens (Hartmann 1948).

We collected data on English language instruction laws for each state during the 1910 to 1930 period from various sources (see Data Appendix I). States were coded as having an "English only" law if instruction was required to be in English in all schools, including parochial schools, save for the teaching of foreign languages.

⁶Minnesota, Laws 1919, Ch. 320, amending Gen. Stat. 1914, sec. 2979 as described in Ruppenthal (1920).

⁷Statistics taken from the 1910 Report of the Commissioner of Education, Secondary School chapter (Bureau of Education; Department of the Interior).

⁸Similar courses were designed for adults. In Detroit in 1917 an eight-week course meeting twice a week included lessons on naturalization proceedings, US history, geography, government and the Constitution (Cody 1918). Two courses on English skills and citizenship were designed by federal agencies for immigrants by the Bureau of Education and the Bureau of Naturalization.

⁹We found no data on enforcement of these laws. However, there is evidence that states did appropriate funds to devote to Americanization purposes. Data for all years is not available but details of appropriations for the 1919 laws are reported in Hartman (1964) for a few states: Connecticut appropriated \$50,000 for a two year Americanization campaign, Delaware appropriated \$15,000 for two years, Maine \$25,000, Minnesota \$25,000, Massachusetts \$10,000 per year, New Hampshire \$162,000 per year, Rhode Island \$5,000, Arizona \$25,000, Utah \$20,000 (1919), Wyoming \$8,000, South Dakota \$15,000, and finally New York appropriated \$40,000 for training teachers to instruct the foreign born and \$100,000 for Americanization courses.

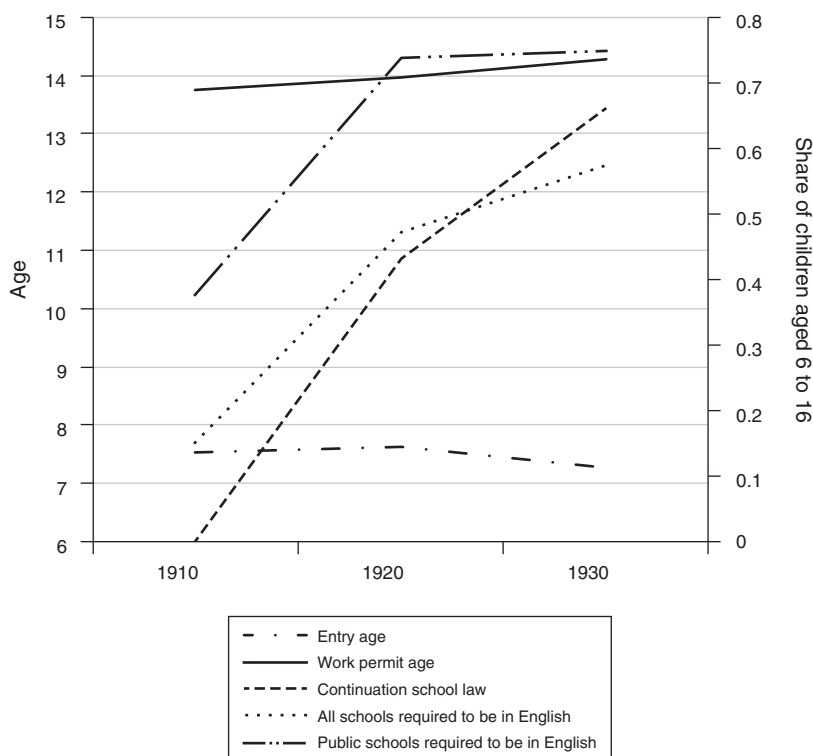


FIGURE 1. TRENDS IN EDUCATION LAWS, 1910–1930

Notes: Sample includes white children aged 6 to 16 in the 48 continental states and shows the respective law applying to each child according to her home state and the census year. Children whose place of residence, place of birth, or parents' place of birth is missing are dropped from the sample. See Data Appendix I for details on the sources and construction of the English law variables. States with no entry age or leaving age are dropped from the respective calculation.

(Appendix Table I in the online Appendix lists education laws for each state and each census year.) We also coded less stringent laws that required *only* public schools to use English as the language of instruction. Figure 1 shows that in 1910, about 15 percent of students were subject to strict English language instruction laws; this number rose to 47 percent by 1920. Almost all of the changes occurred in the 1910 to 1920 decade with only a few after 1920. The closing of the border in 1924 and the 1923 Supreme Court case of *Meyer v. Nebraska* (which found laws requiring all instruction to be in English *and* forbidding foreign language instruction to be unconstitutional) are likely responsible for the lack of further legislation.

Compulsory schooling laws were already in place by 1910 in most states, and by 1920 all states had such statutes. However, states made many changes to their existing laws between 1900 and 1930. According to Progressive Era ideals of the day, education would further a common American culture and function as a bulwark against crime, inequality, and economic stagnation.¹⁰ There was also concern about

¹⁰On the other hand, states that employed a large fraction of youth were opposed to laws that prohibited or restricted child labor. There was also opposition to compulsory laws from those that argued that such laws restricted

idle youth, and these education laws were in part anti-truancy laws intended to keep children off the streets. During this time, obtaining a work permit was equivalent to being exempt from going to school because it allowed children to drop out even if they were below the minimum age for leaving school. Another important component of legislation required that children who were granted permission to work attend school at least part-time. Since the law required that children attend school during the work week, these “continuation school” laws increased the cost of employing children. Although there is no data on enforcement for all states, historical accounts emphasize that starting in the twentieth century, compulsory schooling laws and child labor laws were enforced: states started counting the student population, hiring truant officers, and instituting fines for noncompliant families (see Tyack and Berkowitz 1977; Lleras-Muney 2002).¹¹

The compulsory schooling data we use were compiled by Goldin and Katz (2011). They report laws in place from 1910 to 1939 for each of the 48 states.¹² Following previous work (Lleras-Muney 2002; Goldin and Katz 2011), we focus on the three laws that have been found to affect the educational attainment of natives: the age at which individuals were required to enter school, the age at which they could obtain a work permit, and whether the state allowed children with work permits to drop out completely or forced them to remain in school at least part-time. Figure 1 shows the trends in the schooling laws we study. The entry age faced by the average nonblack student in states with an entry age law was relatively stable during the period. However, all seven states without an entry age law in 1910 adopted one by 1930. On the other hand, the age required to obtain a work permit increased each decade, even among states with such a law, from 13.7 to 14.3. The eight states without a work permit age in 1910 all adopted one by 1930 as well. Finally, continuation school laws became widespread. No state required working teens to continue going to school part-time in 1910, but two-thirds of students faced a continuation school law by 1930.¹³

B. What Explains the Passage of Americanization Laws?

English-only laws were often included in the same text as compulsory schooling laws, and historical accounts suggest that they were driven by the same underlying forces: the level of immigration and determinants of immigration such as income. However, other factors may have also mattered for the passage of English-only laws. In particular, the timing of the laws appears to have been driven by the onset of World War I, and the fact that there were few changes after 1921 is consistent with the closing of the border in 1924, which diminished interest in immigration-related laws.

individual freedoms, or those who believed that not all children would benefit from additional education (Goldin and Katz 2008).

¹¹For instance in 1913–1914 Chicago investigated 58,064 cases, prosecuted 67 parents and brought 826 children to juvenile court. Nevertheless even by 1925 there was only one truant officer for every 7,500 children suggesting enforcement was not great in many locations.

¹²Alaska, Hawaii, and Washington, DC are excluded in all years.

¹³Trends in entry age, work permit age, continuation school, and English laws by state are shown in Appendix Table VII in the online Appendix.

Previous work has investigated the determinants of compulsory and child labor laws during this period, although to our knowledge no empirical studies of English-only laws exist. Lleras-Muney (2002) finds that states with higher incomes, larger shares of immigrants, and smaller shares of blacks were more likely to pass stringent compulsory schooling laws. She also reports that such laws were more likely to be passed in states where education levels were high. These results support the hypothesis that immigration was one impetus for compulsory education legislation; however, the results also suggest that these laws were passed in part because they would affect only a small number of individuals (for instance, in states where education levels were already high; see Clay, Lingwall, and Stephens 2012). We do not reproduce these results here and instead focus on the factors that predict the passage of English-only laws.

We collected data from various sources (see Data Appendix II in the online Appendix) on the state characteristics that could have predicted the passage of English-only laws. To test the notion that immigration was the driving force behind the passage of Americanization legislation, we include the shares of the foreign-born population, including the percent of recent immigrants (percent immigrants, percent recent immigrants).¹⁴ We also ask whether the presence of immigrant children in school spurred English-only law passage and consider recent immigrants separately (share of enrolled children who are immigrants, share of enrolled children who are recent immigrants). To test the German backlash theory, we also look at the share of the population composed of individuals who were either first or second-generation German immigrants (percent German descent). We also include the share of the state population identifying as Catholic and Lutheran to capture the role of religious conflict related to immigration (percent Catholic). To determine whether other education policies mattered, we include proxies for the supply of education in the state (state enrollment level among natives, the share of students in private schools, and the number of schools per square mile) and compulsory education laws.

We also account for the demographic composition of the state (percent black, percent of the population over 65, percent under 14, percent urban), and economic measures (state per capita income, per capita manufacturing jobs). Richer states and more homogenous states had greater secondary schooling expansions and greater expenditures on post-secondary schooling during this period (Goldin and Katz 2009). Lastly we use the percent Republican as a proxy for political preferences. The Americanization movement and the Progressive movement were supported by the Republican Party and the Progressive Party (a splinter of the Republican Party, extant between 1912 and 1918).

To empirically determine which factors explain the passage of the laws, we estimate the following probit model:

$$(1) \quad P(\text{English} = 1)_{st} = f(c + bX_{st} + \eta_t),$$

¹⁴We thank an anonymous referee for this suggestion.

where $P(\text{English} = 1)$ is a dummy equal to one if states passed an English-only law and zero otherwise. We consider which variables predicted laws affecting all schools and just public schools. X_{st} is a vector of state characteristics at the start of the decade. In the simplest specification we ask if 1910 covariates predict the passage of legislation after 1910. Alternatively we create a panel of states, with two observations per state, and predict whether a state passed an English-only law sometime between 1910 and 1920 (1920 and 1930) using 1910 (1920) characteristics. The four states that already had stringent English-only laws in place for all schools (both public and private) in 1910 are not included in the estimation to predict passage of these laws; likewise the fourteen states that already had English-only laws for public schools in place in 1910 are not included in the estimation to predict passage of these laws.

The results where we consider each covariate by itself are reported in Table 1. Consistent with the timing of the English-only laws, two indicators are correlated with the passage of “all schools” legislation: the share of immigrants and recent immigrants (either in the population or in school) and the years a child had to attend school by law. In addition, native enrollment share and black population share negatively predict “public schools” laws. All of the other state characteristics (including German origin, percent Catholic, and per capita income) are insignificant. The results are qualitatively similar for both specifications. However, in spite of our data collection efforts, we were unable to find other statistically significant predictors of the legislation.¹⁵ This outcome is perhaps due to the small sample size or to the fact that some measures are represented by poor proxies. We nevertheless test how sensitive our results are to adding these state-level controls.

II. Empirical Approach

A. Empirical Strategy

The main purpose of this paper is to assess whether the education laws passed with the intention of forcing immigrant children to attend school and learn English were successful. Using three cross-sections from the censuses of 1910, 1920, and 1930, we estimate the following probit model:

$$(2) \quad P(y_{ist} = 1) = f(c + bL_{st} + \delta X_{ist} + \alpha_s + \eta_t),$$

¹⁵We also looked at proxies for unionization and the number of women's organizations by state. But again we found no statistically significant effect of either. Historical accounts have sometimes emphasized their importance in passing Americanization laws. Many women's organizations were very active in the Americanization efforts and published pamphlets like “Americanization through Women's Organizations” (Hartman 1964). The prohibition/temperance movement, also strongly supported by women, stated in its platform “We stand for compulsory education with instruction in the English language, which, if given in private or parochial schools, must be equivalent to that afforded by the public schools, and be under state supervision.” In the other side, prior to WWI unions were openly opposed to Americanization efforts. Though they recognized that basic knowledge of the English language and of American institutions were needed by immigrants, they doubted the motives of the movements and also insisted that success could not be achieved without additional labor reforms to improve the working (and living) conditions of the immigrants (Gompers 1916). However, by 1918 unions' attitudes had changed and they actively supported the Americanization program of the Bureau of Education (Hartman 1964).

TABLE 1—PREDICTORS OF ENGLISH AS LANGUAGE OF INSTRUCTION LAWS 1910–1930
(*Probit regression with marginal effects reported*)

Dependent variable:	English law passed after 1910		English law passed in the next decade	
Law applied to:	All schools	Public schools	All schools	Public schools
<i>Presence and level of education of foreign born</i>				
Share of enrolled children who are immigrants	3.775 (2.430)	6.339 (2.471)*	1.303 (1.309)	2.403 (1.215)*
Share of enrolled children who are recent immigrants	3.950 (2.669)	6.822 (2.598)*	1.263 (1.424)	2.471 (1.282)
Share of population foreign born	0.698 (0.729)	1.168 (0.640)**	0.421 (0.405)	0.905 (0.377)*
Share of population recent foreign born	1.867 (1.571)	3.894 (1.445)**	0.761 (0.910)	1.801 (0.832)*
Share of population of German descent	0.300 (0.899)	0.287 (0.906)	0.179 (0.527)	0.722 (0.635)
<i>Level of education and education laws</i>				
Share of enrolled children who are native born	−3.775 (2.430)	−6.339 (2.471)	−1.303 (1.309)	−2.403 (1.215)*
Share of students enrolled in private schools	1.253 (1.388)	1.030 (1.548)	0.646 (0.684)	0.697 (0.715)
Schools per square mile	−0.445 (0.670)	−0.867 (0.776)	−0.060 (0.370)	−0.273 (0.441)
Work permit age–entry age	0.080 (0.024)***	0.030 (0.019)	0.047 (0.017)**	0.016 (0.012)
Continuation school law	—	—	1.130 (171.966)	0.974 (100.501)
<i>Demographic, political, and economic determinants</i>				
Share of population black	−0.41 (0.432)	−0.850 (0.371)*	−0.222 (0.251)	−0.470 (0.236)*
Share of population > 65 years of age	0.143 (5.167)	−2.096 (5.458)	−0.442 (2.848)	−1.627 (3.003)
Share of population < 14 years of age	−1.862 (1.342)	−1.582 (1.488)	−0.975 (0.799)	−0.817 (0.844)
Share of population residing in urban areas	−0.109 (0.311)	−0.073 (0.352)	0.021 (0.158)	0.081 (0.173)
Catholic share of population	0.136 (0.382)	0.737 (0.445)	0.16 (0.183)	0.364 (0.197)
Republican share of congressional election votes	0.003 (0.004)	0.005 (0.004)	0.002 (0.002)	0.003 (0.002)
Per capita income	0.000 (0.002)	0.000 (0.002)	0.001 (0.000)	0.000 (0.001)
Per capita manufacturing jobs	0.048 (1.657)	−0.022 (1.873)	0.263 (0.842)	0.254 (0.905)
Observations	44	34	88	68

Notes: The sample includes all states without an existing respective English law in 1910. The passage of a law in any year after 1910 is predicted using 1910 characteristics in the first two columns. The sample in the third and fourth columns use 1910 and 1920 characteristics to predict the passage of an English law in the subsequent decade, and these specifications include a dummy for 1920. See online Data Appendix II for details on the construction of the predictor variables. Due to missing voting data at the state level, there are only 42 (all schools) and 32 (public schools) observations in 1910 and 85 (all schools) and 65 (public schools) observations in 1910 and 1920 for regressions using the congressional voting data.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

where $P(y_{ist} = 1)$ is an indicator for whether a given child i living in state s in year t is in school (or some other binary outcome); X_i contains individual characteristics such as age, gender, place of birth, years lived in the US, and parental characteristics (literacy and ability to speak English); the specification also includes 48 state dummies (α) and three year dummies (η). L_{st} contains the education laws in place in state s and census year t (whether the child is supposed to be in school given her age and the current compulsory schooling laws, whether there is a continuation school law in place, and whether there is an English-only law). The errors are clustered at the state level to account for correlations within a state in a given year and over time (Bertrand, Duflo, and Mullainathan 2004). Integrated Public Use Microdata Series (IPUMS) survey weights are used in the estimations. We repeat these estimations using literacy, English fluency, and employment status as alternative outcomes.

The coefficient b in equation (2) is our main coefficient of interest, informing the question of whether legislation affected outcomes. We report average marginal effects for ease of interpretation. Since we include state and year fixed effects, this coefficient is identified using changes within states over time, beyond those that are predicted by national trends. To interpret b as the causal effect of legislation on outcomes, we require that changes within states in legislation not be correlated with changes in other determinants of outcomes, and that changes in legislation not result from changes in outcomes within states. We test how robust our results are to including additional state covariates that we found predictive of legislation to test this assumption.

B. Data

We use the 1910, 1920, and 1930 1 percent random samples of the censuses available through IPUMS.¹⁶ The data contain individual characteristics (age, gender, race, ethnicity, place of birth, place of residence), as well as characteristics of the individual's parents (literacy, English fluency, place of birth). We restrict the sample to nonblack individuals aged six to sixteen living in the 48 contiguous states.¹⁷ Children whose place of residence, place of birth, or parents' place of birth is missing are dropped from the sample. We define children as "native" if they and both of their parents were born in the United States; as "first-generation" immigrants if they were born abroad; and as "second-generation" immigrants if they were born in the United States but at least one of their parents was not.

Table 2 shows summary statistics for our samples. Our key variables of interest are: whether the child is reported to be in school (any time since September first), whether the child works (which we code as one if the child is reported to have an occupation), whether the child is literate (can read and write in any language), and whether the child speaks English. Literacy and English fluency were only asked of children ten years and older, whereas enrollment and work are available for all ages

¹⁶See <http://usa.ipums.org/usa/> (Ruggles et al. 2010).

¹⁷We exclude blacks because previous work did not find legislation to be effective for them. Also since there are no laws requiring five-year-olds or those 17 and above to attend school, these ages are excluded.

TABLE 2—SUMMARY STATISTICS. INDIVIDUALS AGES 6–16, 1910, 1920, AND 1930 CENSUSES.
MEAN AND STANDARD DEVIATION (*in parentheses*)

	All	Native born, native parents	Second generation	Foreign born
<i>Outcome variables</i>				
In school	0.88 (0.32)	0.89 (0.32)	0.89 (0.31)	0.82 (0.39)
Employed	0.07 (0.26)	0.07 (0.26)	0.07 (0.25)	0.14 (0.35)
Literate ^a	0.98 (0.14)	0.98 (0.15)	0.99 (0.09)	0.94 (0.23)
Speaks English ^a	0.98 (0.15)	0.98 (0.13)	0.98 (0.15)	0.86 (0.35)
<i>Laws</i>				
Should be in school by law	0.58 (0.49)	0.57 (0.49)	0.60 (0.49)	0.56 (0.50)
Continuation school	0.39 (0.49)	0.34 (0.48)	0.51 (0.50)	0.42 (0.49)
All schools required to be in English	0.42 (0.49)	0.39 (0.49)	0.48 (0.50)	0.47 (0.50)
Years exposed to all schools English law	3.78 (4.55)	3.57 (4.52)	4.47 (4.66)	2.53 (3.29)
<i>Individual characteristics</i>				
Female	0.49 (0.50)	0.49 (0.50)	0.50 (0.50)	0.50 (0.50)
Resides in urban area	0.41 (0.49)	0.29 (0.46)	0.66 (0.47)	0.71 (0.45)
Age	10.85 (3.17)	10.79 (3.17)	10.91 (3.17)	11.67 (3.11)
Both parents speak English	0.79 (0.41)	0.83 (0.38)	0.73 (0.44)	0.45 (0.50)
Both parents literate	0.77 (0.42)	0.80 (0.40)	0.71 (0.45)	0.57 (0.50)
Mean years lived in United States				6.40 (3.68)
<i>State characteristics</i>				
State fraction < 14 yrs old	0.31 (0.04)	0.32 (0.04)	0.29 (0.03)	0.29 (0.03)
State fraction > 65 yrs old	0.05 (0.01)	0.05 (0.01)	0.05 (0.01)	0.05 (0.01)
State foreign-born fraction	0.13 (0.10)	0.11 (0.09)	0.20 (0.08)	0.21 (0.08)
State recent foreign-born fraction	0.04 (0.04)	0.03 (0.03)	0.06 (0.04)	0.07 (0.04)
State urban resident share	0.43 (0.28)	0.37 (0.26)	0.58 (0.27)	0.58 (0.27)
Fraction population owning autos	0.11 (0.10)	0.11 (0.10)	0.11 (0.10)	0.09 (0.10)
State black fraction	0.09 (0.13)	0.11 (0.14)	0.03 (0.05)	0.04 (0.06)
Schools per square mile	0.18 (0.10)	0.17 (0.09)	0.21 (0.10)	0.21 (0.11)
Per capita doctors	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
Lagged recent foreign-born enrollment	0.03 (0.03)	0.02 (0.02)	0.04 (0.03)	0.05 (0.03)
State manufacturing per capita jobs	0.07 (0.04)	0.06 (0.04)	0.09 (0.04)	0.09 (0.04)
Observations	610,423	423,252	167,722	167,722

Notes: “Should be in school by law” is a dummy variable equal to one if a child’s age is greater than or equal to the compulsory starting age and less than the age required for a work permit. “Continuation school” is a dummy equal to one if the state required those with a work permit to continue school on a part-time basis. Sample includes non-black individuals ages 6–16 living in the 48 states (excluding Alaska, Hawaii, and Washington, DC) in the 1910, 1920, and 1930 censuses. Children whose place of residence, parents’ place of birth, literacy, or English ability is missing are dropped. Data on compulsory schooling laws was provided by Golding and Katz (2008). Data on English laws were collected from state records (see Data Appendix I for details). See online Data Appendix II for details on the sources of the state-level characteristics. Person weights used in computing population mean and standard deviation.

^aAges 10–16 only.

^bComputed as share of 6–16-year-olds in the state.

in our sample.¹⁸ Figures 2 through 5 show the trends in these variables in the various samples we study.

Enrollments were rising during the relevant period for all groups (Figure 2). The trends for second-generation children closely follow that of natives: their attendance rose a bit between 1910 and 1920, but appears stable thereafter. Although foreign-born children were less likely than natives to be in school in 1910 (only

¹⁸The approximate period during which a child could have attended school varies in each census because of differences in the date of the census survey year. In 1910, the census took place on April 15 and thus children could have attended school in any of the previous 7.5 months. In 1920, the census date was January 1, thus the reference period is four months. In 1930, the census date is April 1 and the reference period six months.

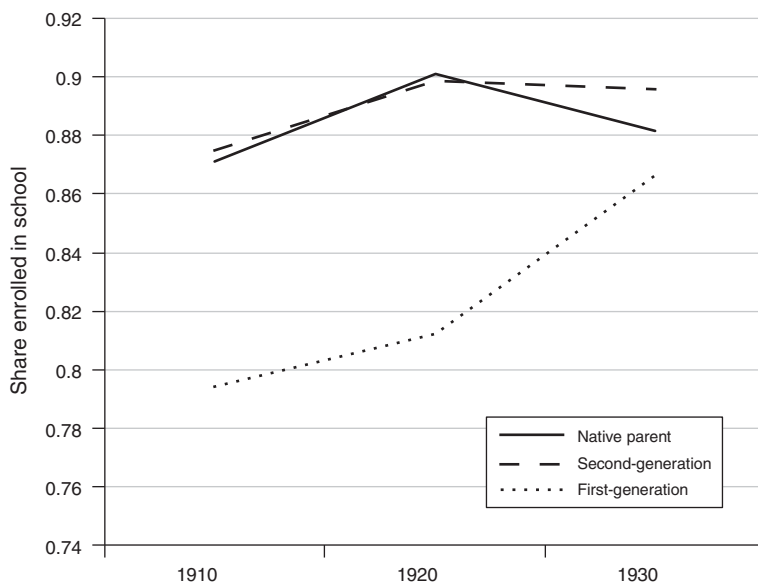


FIGURE 2. PERCENT OF CHILDREN AGES 6–16 IN SCHOOL, 1910–1930

Notes: Sample includes nonblack children aged 6 to 16 in the 48 continental states. Children whose place of residence, place of birth, or parents' place of birth is missing are dropped from the sample. We define children as "native parent" if they and both of their parents were born in the United States; as "first-generation" immigrants if they were born abroad; and as "second-generation" immigrants if they were born in the United States but one or both of their parents were not. Data come from the 1910, 1920, and 1930 1 percent IPUMS samples.

79 percent reported being in school compared to 87 percent of the native-born), their attendance rates grew more quickly. By 1930 the gap between natives and foreign born was only 1.5 percentage points. Figure 3 shows trends in employment. In 1910, 18 percent of immigrant children and about 10 and 13 percent of natives and second-generation children were working, respectively. But these fractions fall over the period, and more so for immigrants.

Not surprisingly, almost all second-generation and native children reported being able to speak English throughout the period.¹⁹ For immigrant children, there is a quantitatively large (15 percentage point) increase in the fraction speaking English from 1910 to 1930 (Figure 4).²⁰ Overall, second-generation children look very similar to natives in terms of their level and trends in enrollment, work, and English fluency, while immigrant children lag behind the other two groups. That is not the case for literacy. Literacy rates for second-generation children were reported as being almost 100 percent in 1910, 1920, and 1930. This is the highest literacy rate of all three groups, followed by natives and first-generation children. The literacy rate of native and first-generation children increased significantly during the period, with the largest increases again seen for immigrant children.

¹⁹Technically the report is made by the parent or household member that answered the census questionnaire.

²⁰There is a strange decrease in fluency in the 1920 census for natives and second generation children, but it is small so we ignore it here. There were no changes in the way the question was asked.

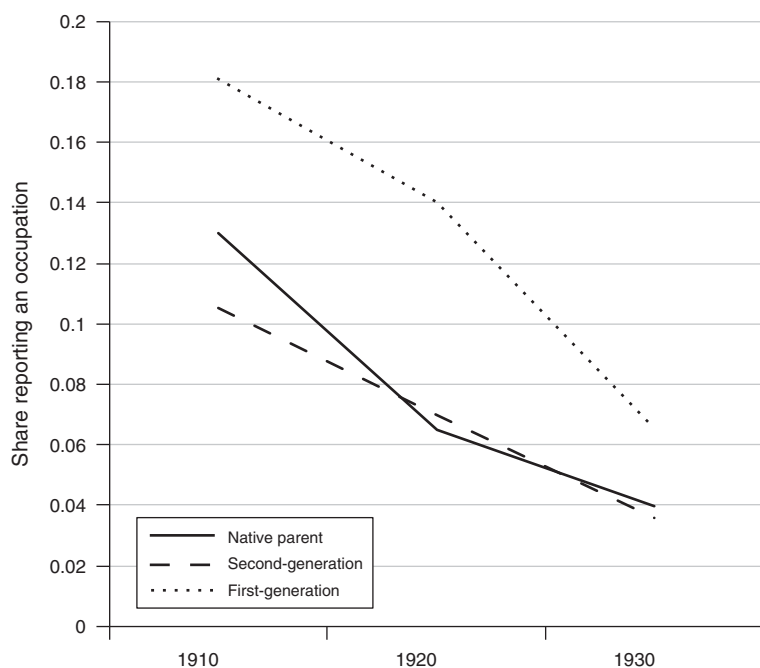


FIGURE 3. PERCENT OF CHILDREN AGES 6–16 REPORTING AN OCCUPATION, 1910–1930

Note: See Figure 2 notes for sample description.

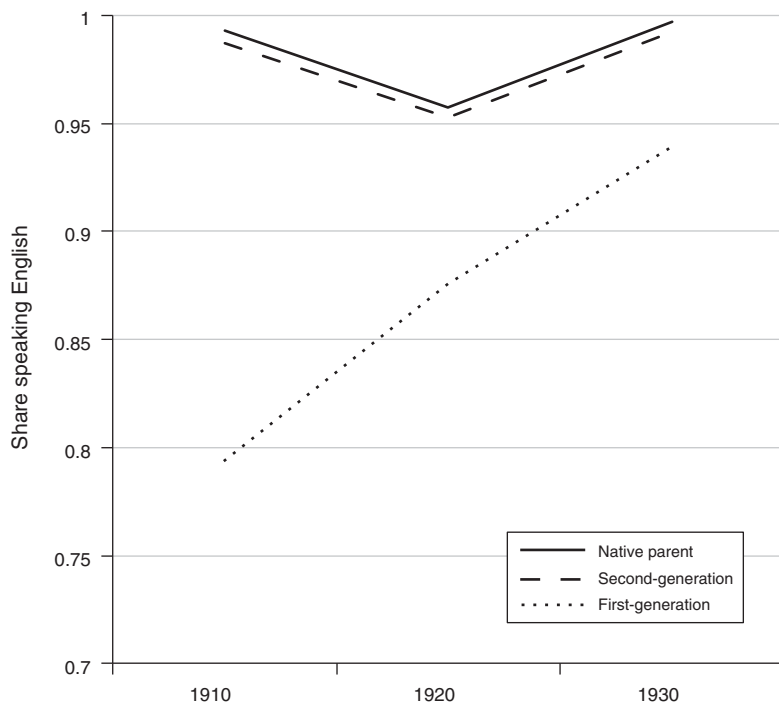


FIGURE 4. PERCENT OF CHILDREN AGES 10–16 SPEAKING ENGLISH, 1910–1930

Note: See Figure 2 notes for sample description.

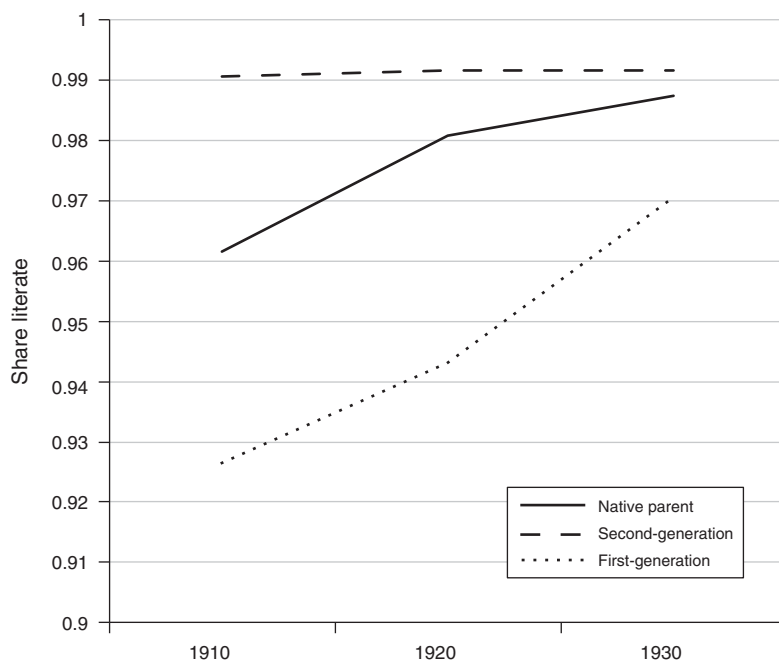


FIGURE 5. PERCENT OF CHILDREN AGES 10–16 LITERATE, 1910–1930

Note: See Figure 2 notes for sample description.

Changes in our four outcomes of interest over the 1910 to 1930 period were clearly largest for immigrant children, who appear to have converged toward native levels. We are interested in assessing the extent to which this immigrant “catch-up” was driven by education laws. Given that second-generation children are almost indistinguishable from natives, it would appear that convergence would have occurred even in the absence of education legislation (although the patterns could also be driven by changes in immigrant cohort characteristics). Thus, the question in this case is whether these laws substantially accelerated the convergence process, a question we return to later.

III. The Effects of Americanization Laws on Immigrant Children

A. Main Results

We begin by considering whether compulsory schooling laws and English-only laws had any impact on first-generation immigrant children. Table 3 presents the results for enrollment, employment, literacy, and fluency. The main results are in Panel A. The first column reports the results for school enrollment. Only compulsory schooling has a statistically significant coefficient, implying that laws forcing children to stay in school increased enrollment by about 5 percent overall and about 6.5 percent for children aged 10 to 16. In the next two columns we find that forcing children to be in school lowered the probability that they worked by about

TABLE 3—EFFECTS OF ENGLISH AS LANGUAGE OF INSTRUCTION, COMPULSORY SCHOOLING, AND CHILD LABOR LAWS ON IMMIGRANT CHILDREN OUTCOMES (*Probit estimation with mean marginal effects reported*)

Dependent variable	In school=1		Employed=1	Literate=1	Speaks English=1
Sample: Ages	6–16	10–16	10–16	10–16	10–16
<i>Panel A. Main results</i>					
All schools English law	−0.018 (0.013)	−0.007 (0.016)	−0.020 (0.022)	0.019 (0.015)	0.022 (0.016)
Compulsory schooling law	0.048 (0.018)**	0.065 (0.021)**	−0.077 (0.034)*	0.012 (0.014)	0.011 (0.018)
Continuation school law	0.009 (0.013)	0.018 (0.015)	−0.004 (0.027)	−0.025 (0.015)	−0.001 (0.017)
<i>Panel B. No state controls</i>					
All schools English law	−0.013 (0.014)	−0.003 (0.015)	−0.034 (0.022)	0.034 (0.016)*	0.041 (0.021)
Compulsory schooling law	0.049 (0.014)***	0.068 (0.017)***	−0.075 (0.027)**	0.011 (0.014)	0.007 (0.015)
Continuation school law	0.021 (0.014)	0.031 (0.015)*	−0.008 (0.020)	0.014 (0.014)	0.026 (0.018)
<i>Panel C. Without CSL</i>					
All schools English law	−0.022 (0.013)	−0.015 (0.016)	−0.017 (0.017)	0.030 (0.012)*	0.023 (0.017)
<i>Panel D. By stringency of English law</i>					
All schools English law	−0.014 (0.014)	−0.002 (0.016)	−0.018 (0.022)	0.019 (0.015)	0.018 (0.018)
Public school English law	−0.013 (0.013)	−0.016 (0.015)	−0.006 (0.018)	0.003 (0.012)	0.021 (0.016)
<i>Panel E. Using variation in exposure</i>					
Years exposed to English law	0.004 (0.001)***	0.003 (0.001)*	−0.004 (0.002)	0.003 (0.002)	0.002 (0.003)
Observations	19,356	13,800	13,820	12,211	13,560
Mean outcome	0.818	0.795	0.193	0.943	0.857

Notes: All regressions include dummies for female, urban residence, year of immigration, years lived in the United States, year of age, place of birth, and state and year fixed effects. The specifications also include indicators for both parents literate, one parent literate, both parents speak English, and one parent speaks English where the omitted category for parents' literacy is either both illiterate or missing data, and similarly for parents' English ability. Panels A, C, D, and E also include a vector of state controls; see Table 2 for the list. Standard errors are clustered at the state level (in parentheses). Person weights used in all estimations.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

40 percent for both age groups, and the effects are statistically significant. Again the coefficients on continuation school laws and English-only laws are small and statistically insignificant. The last two columns assess the effect on language acquisition. English-only laws and compulsory schooling laws have small and insignificant effects on literacy or English fluency. The coefficients are statistically insignificant for both measures of language skills, and a test of joint significance also finds that the laws appear to have no effect.

Panels B through E investigate the robustness of the “English-only” law nonresult. In panel B we drop our state controls. In panel C we drop compulsory schooling and continuation school laws since they are highly correlated with English-only

laws. In both cases we find no effects of the laws on enrollment, employment or fluency, and a statistically significant effect of English-only laws on literacy of about 3 percent. This likely represents an upper bound of the effect of English-only laws. On the other hand, the coefficients for compulsory schooling laws are mostly unaffected by dropping state controls. Panel D, which again includes state controls, shows that the small literacy effects are if anything driven by laws that affected all schools; laws targeted only at public schools had even smaller effects.

The previous literature examining education laws has emphasized the importance of controlling for state (or at least region)-specific trends (Stephens and Yang 2014). The variation in our outcomes is small: most children are in school, literate and fluent. For this reason we cannot estimate state-specific trends using our three cross sections.²¹ However, we investigate the robustness of the long-term results to the inclusion of state-specific trends in the next section. These trends generally decrease the magnitude of our estimates. Thus, we conclude that the small effects we estimate are likely to be upwards biased. A simple regression controlling for birthplace and year of immigration only indicates that the English laws only raised literacy by 2.7 percentage points, further suggesting that the likely upper bound is small.

Finally, we use years exposed to English-only law as an alternative measure to identify the effect of the laws. To compute exposure, we must use current state of residence as a proxy for the state of residence between ages 6 and 16.²² We compute the number of years between ages 6 and 16 that an individual was exposed to the law based on her state of residence, her age, and the year in which that state passed a law. Individuals in states with no laws, and individuals who were older than 16 when the law passed, are given an exposure value of zero. We further correct this using information on the year of arrival for immigrants, capping exposure at the total number of years an immigrant had lived in the United States. This variable ranges from 0 to 11.

Panel E of Table 3 shows the results from implementing this strategy. We do find significant effects on enrollment and employment, but they are very small: the coefficients imply that an increase of ten years in exposure (the maximum) would increase enrollment by less than 5 percent. We also find positive but statistically insignificant effects of additional years exposed to the English-only law on literacy: the implied effect of a standard deviation in exposure among immigrants is about 0.01, similar to but somewhat smaller than the coefficient on the presence of a law. Although this measure contains somewhat more variation, it also contains more measurement error.

Enrollment and employment were not targeted by English-only laws, so it is not surprising to find that the laws did not significantly affect these outcomes. On the other hand, we do find suggestive evidence of small effects of English-only laws on foreign-born children's language abilities, which were the specific target of the legislation. Table 4 focuses on samples for which we might expect a larger impact of the legislation. The first row of results of Table 4 reproduces the coefficients on the "English-only law" variable from Table 3 for reference. The second row shows

²¹We were able to estimate a few models including these trends. When the models converge, the estimates are not very different from those reported here.

²²The census did not ask questions about internal migration until 1940.

TABLE 4—EFFECT OF ALL-SCHOOL ENGLISH LAW ON LITERACY AND ABILITY TO SPEAK ENGLISH FOR
SUBSAMPLES OF IMMIGRANT CHILDREN AGED 10–16, 1910–1930
(*Probit regression with mean marginal effects reported*)

Dependent variable:	Literacy=1			Fluency=1		
	Effect	N	Mean literacy	Effect	N	Mean fluency
<i>Panel A. All foreign born</i>						
Baseline	0.019 (0.015)	12,211	0.943	0.022 (0.016)	13,560	0.855
Center cities residents only	0.071 (0.015)***	5,780	0.968	0.022 (0.017)	6,877	0.907
Non-German only	0.017 (0.016)	11,544	0.941	0.031 (0.018)	12,876	0.854
<i>Panel B. Non-English speaking origin</i>						
Baseline	0.021 (0.018)	11,049	0.934	0.014 (0.019)	11,980	0.837
and in US ≤ 5 years	−0.040 (0.054)	3,675	0.905	0.036 (0.039)	4,134	0.719
and in US > 5 years	0.043 (0.015)**	6,361	0.950	0.007 (0.017)	7,633	0.900
and parents illiterate	0.075 (0.028)**	5,530	0.881	−0.011 (0.028)	5,808	0.767
and parents not fluent	0.089 (0.030)**	6,739	0.900	0.005 (0.029)	7,352	0.744

Notes: See Table 2 for sample restrictions and English law variable definition. See Table 3 for specification details. Both panels A and B include state controls; see Table 2 for the list. Standard errors are clustered at the state level (in parentheses). Person weights used in all estimations.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

that these effects were larger in central cities, where most immigrants were located and where enforcement was likely better. Indeed, we find a statistically significant 7 percent increase in literacy, but no effect on fluency. We also drop German-born children since the legislation was accompanied by anti-German feelings and may have had different effects on those children. Our results are unaffected.

Panel B restricts attention to immigrants from non-English-speaking countries. On average the effects are not larger for this group, although it does appear that the effects are positive for those children who have been in the United States for at least five years. There is a more sizeable 8 to 10 percent increase in literacy for children whose parents were themselves illiterate or not fluent in English. But the laws never have a statistically significant or large effect on fluency. We also estimated these models using years of exposure to an English law and the results are similar: we find strong effects of the law on literacy only for children living in center cities. There is no effect of public school English laws on any subsample.²³

²³Results provided in the online Appendix.

B. Did Compulsory Schooling and Continuation School Laws Have Larger Effects on Immigrant Children than on Natives?

Previous work found that although compulsory schooling laws had a statistically significant effect on natives' education, those effects were rather modest, especially when compared to the large increases in educational attainment that took place when these laws were passed. Perhaps these small effects are not surprising: as Table 1 indicates, in 1910 native children already had higher enrollment rates and significantly lower employment rates than immigrant children. And because many of these laws targeted immigrants specifically, the effects of the legislation on immigrants could be larger than for natives.²⁴

Table 5 shows the results for natives, first-generation, and second-generation children separately. For the second-generation specifications, we control for mother's place of birth and mother's year of immigration to capture heterogeneity within this population. Panel A shows compulsory schooling statistically significantly increased enrollment of first-generation immigrant children by about 6 percent, but only by 2 percent for native-born children. Compulsory schooling laws had significant and sizeable effects on employment for all groups, ranging from 40 percent for foreign-born children to 50 percent for second-generation children. Panels B and C show these effects are entirely driven by 10–16-year-old children, suggesting that laws regulating entry ages were not effective. Laws changing the working age from younger to older teens were effective at keeping children in school and out of the labor force. On the other hand, there are no substantial, statistically significant effects of continuation school or English-only laws on enrollment or employment for any of the groups we study.

The effect of the work permit age on enrollment in Table 5 can be expected to translate into an increase in years of schooling of about 0.23 years of school for immigrants ($0.048 \times 11 \times 0.44$, assuming the increase in enrollment was equally spread over ages 6 to 16, and multiplying by the share of children who were not required to be in school in 1910), and about 0.10 years for natives. We now consider the long-term effects of these laws on outcomes and particularly whether these short-term effects persist or decline as children age into adulthood.

IV. Effects of Education Laws on Immigrant Outcomes in Adulthood

The intent of compulsory schooling and English-only laws was to improve immigrants' labor market success and increase their political and social integration. There are several reasons why the short-term estimates from the 1910 to 1930 censuses could underestimate (or overestimate) the impact of the legislation on outcomes. First of all, the laws might have impacted outcomes we cannot measure. English-only laws, and the Americanization efforts that accompanied them, could have directly affected children in areas beyond school enrollment, work, and fluency; for instance, these policies may have adversely affected the social integration of foreign-born

²⁴We do not consider the differential effect of these laws on literacy and English proficiency since the means of these variables for natives are essentially 100 percent.

TABLE 5—EFFECTS OF EDUCATION LAWS ON THE ENROLLMENT AND EMPLOYMENT OF NATIVE AND IMMIGRANT CHILDREN AGED 6–16, 1910–1930 (*Probit regression with marginal effects reported*)

Dependent variable:	In school=1			Employed=1		
Sample:	Foreign born	Second generation	Native parents	Foreign born	Second generation	Native parents
<i>Panel A. All ages 6–16</i>						
Should be in school by law=1	0.048 (0.018)**	0.020 (0.008)*	0.022 (0.005)***	−0.053 (0.021)*	−0.033 (0.010)**	−0.033 (0.005)***
Continuation law=1	0.012 (0.012)	0.010 (0.005)	0.002 (0.006)	0.002 (0.020)	−0.006 (0.006)	0.003 (0.004)
English law for all schools=1	−0.014 (0.013)	0.006 (0.007)	0.010 (0.006)	−0.010 (0.015)	−0.006 (0.003)*	−0.005 (0.004)
Observations	19,356	121,947	423,017	19,320	156,427	423,000
Mean of <i>Y</i>	0.818	0.891	0.885	0.139	0.068	0.074
<i>Panel B. Ages 10–16</i>						
Should be in school by law=1	0.065 (0.021)**	0.037 (0.017)**	0.038 (0.007)***	−0.077 (0.034)*	−0.056 (0.018)**	−0.052 (0.008)***
Continuation law=1	0.018 (0.015)	0.019 (0.007)**	0.003 (0.008)	−0.004 (0.027)	−0.010 (0.010)	0.005 (0.007)
English law for all schools=1	−0.007 (0.016)	0.011 (0.007)	0.012 (0.007)	−0.020 (0.022)	−0.007 (0.006)	−0.008 (0.007)
Observations	13,800	96,232	256,255	13,800	96,243	256,247
Mean of <i>Y</i>	0.795	0.879	0.897	0.795	0.109	0.117
<i>Panel C. Ages 6–9</i>						
Should be in school by law=1	0.012 (0.015)	−0.004 (0.004)	0.000 (0.008)			
Continuation law=1	0.003 (0.020)	−0.004 (0.008)	0.003 (0.010)			
English law for all schools=1	−0.028 (0.021)	0.005 (0.007)	0.003 (0.009)			
Observations	5,360	60,301	166,762			
Mean of <i>Y</i>	0.875	0.910	0.867			

Notes: All regressions include a dummy for female, a dummy for urban residence, dummies for each single year of age, and dummies for place of birth, a vector of state controls, and state and year fixed effects. The specifications also include indicators for both parents literate, one parent literate, both parents speak English, and one parent speaks English where the omitted category for parents' literacy is either both illiterate or missing data, and similarly for parents' English ability. The foreign born specifications include year of immigration and years lived in the United States. The second generation specifications include mother's place of birth and mother's year of immigration. Standard errors are clustered at the state level (in parentheses). Person weights used in all estimations.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

children in schools. Heckman (1995) and Heckman, Stixrud, and Urzua (2006) document that many early education interventions have large effects on noncognitive abilities with social and labor market returns without affecting schooling attendance and test scores. Thus it is possible that Americanization efforts positively affected immigrant children in ways we cannot observe in the 1910 to 1930 data.

The second set of concerns with the contemporary results relates to measurement. School attendance, labor force participation, literacy, and fluency as measured by the census questionnaire are very rough measures. Our results also suggest that the

impact of the legislation was not immediate; for instance the effect of English-only laws on literacy is larger for immigrants who have been in the United States for longer. For these reasons we wish to assess the long-term impact of these policies using a broader set of outcomes than we were able to use in the contemporaneous setting.

A. Data

To evaluate long-term outcomes of the Americanization Movement, we make use of two datasets containing information on immigrants: the 1940 census and World War II army enlistment records. The 1940 census was the first to collect information on years of education, labor force status, occupation, and annual earnings. The questionnaire also asked about veteran status and citizenship status, two relevant integration markers for “Americanization.” We drop the top and bottom percentile of annual earnings and estimate log earnings regressions.

The WWII army data contain information on about nine million individuals (about 85 percent of those serving in the army) who enlisted between 1938 and 1945. The records report educational attainment and occupation prior to enlistment. We convert occupation into wages by imputing the mean 1950 wage from the census to each occupation.

For both samples we make a few restrictions. We concentrate on men because of their closer attachment to the labor force during this time period. We study cohorts born between 1904 and 1924. We only have legislation for states from 1910 to 1940, so the first cohort that we can match to laws are those born in or after 1904. Since we are interested in adult outcomes we limit our investigation to those ages 16 and over (born 1924 or earlier). These men are also the last cohort before the closing of the border.

Both datasets have limitations. To study the effect of Americanization laws on adults, we rely on variation in exposure to the laws during childhood. As in Table 3, we impute years under the English-only law based on state of residence in 1940 or at the time of enlistment (1938–1945). For natives we can also match individuals to their state of birth to assess the extent to which state law assignment matters (or conversely, the extent to which early-life mobility affects our results). Neither data source reports the year of immigration, so we cannot use this variable as a control, nor can we use it to correct our years of exposure to English laws measures as in the contemporaneous analysis. Finally, the WWII data is selected since not all men were equally likely to serve.²⁵

Both data also have some known measurement problems with education. The 1940 census overreports educational attainment and this overreporting is likely to inflate our estimates (see Goldin 1998). The WWII records never report years of schooling below eight years, possibly because literacy was a requirement for serving. Nevertheless, Figure 6 shows very similar trends in educational attainment among immigrants in both datasets. Consistent with contemporary evidence on enrollment, educational attainment rose very substantially among immigrant males

²⁵ See Bleakley, Costa, and Lleras-Muney (2014) for a more detailed discussion of these data.

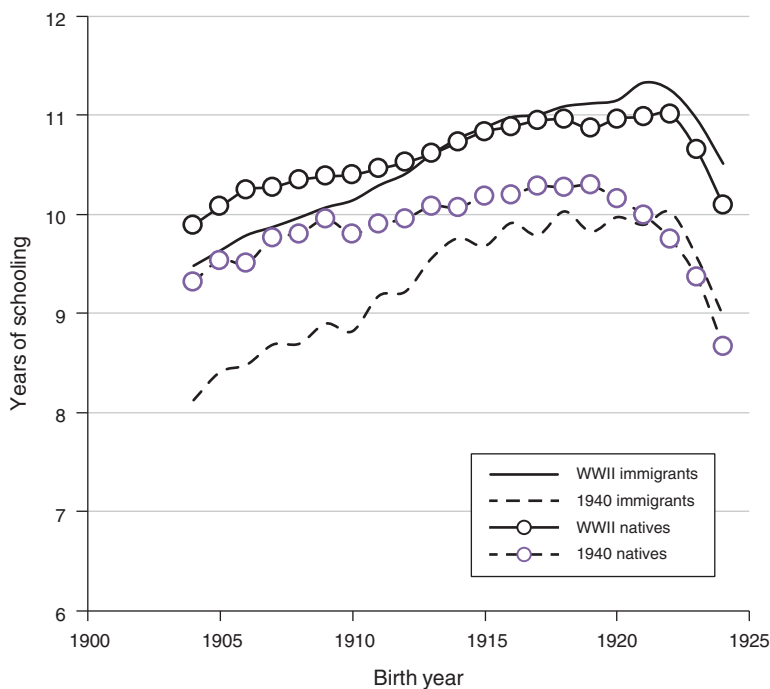


FIGURE 6. TRENDS IN IMMIGRANT EDUCATION IN 1940 AND WWII ENLISTMENT RECORDS

Note: Sample includes white males born between 1904 and 1924 living in the 48 continental states in the respective dataset.

during this period. However, the figure also shows a dip for the last few cohorts, possibly because WWII disrupted the educational attainment of these youngest cohorts. We check that our results are not driven by these young cohorts.

Despite these limitations, these are the only two sources of data that allow us to study long-term outcomes. Later censuses could be used, but no census reports past state of residence for immigrants while they were children. We would thus have to match individuals to laws in their state of residence, but the measurement error induced by migration gets larger as cohorts age and move. The main advantage of the WWII enlistment records is the substantially larger sample size of immigrants.

B. Empirical Strategy and Results

Using individual data from the census and from WWII records, we estimate the following linear models:

$$(3) \quad Y_{isc} = c + bL_{sc} + X_i \delta + \alpha_s + \eta_c + \alpha_s \times YOB_c + \varepsilon_{ics},$$

where the Y_{isc} is the outcome of interest for individual i living in state s and belonging to cohort c . We include state-of-residence fixed effects (α), cohort fixed effects (η) and state-specific cohort trends ($\alpha \times YOB$). We also control for country-of-birth fixed effects. In the WWII specifications we also include age at enlistment dummies.

TABLE 6—EFFECT OF LAWS ON MALE IMMIGRANT OUTCOMES IN THE 1940 CENSUS AND WWII ENLISTMENT RECORDS 1904–1924 BIRTH COHORTS

Outcome:	1940 census				WWII records		
	Years of school	Employed = 1	log income in 1939	Naturalized = 1	Veteran = 1	Years of school	log occ score
<i>Panel A. All immigrants</i>							
Age for work permit	0.127** (0.055)	0.005 (0.007)	0.003 (0.037)	−0.000 (0.011)	0.009 (0.016)	0.109*** (0.040)	0.015** (0.006)
Age must enter school	0.077 (0.126)	0.004 (0.016)	−0.020 (0.038)	0.010 (0.012)	0.005 (0.007)	−0.047 (0.060)	0.000 (0.003)
Continuation school law	0.151 (0.125)	−0.003 (0.019)	0.005 (0.049)	0.004 (0.032)	0.006 (0.011)	0.091 (0.062)	−0.003 (0.009)
Years exposed to EL	−0.053 (0.035)	0.004 (0.005)	0.010 (0.016)	−0.003 (0.005)	0.004 (0.004)	0.008 (0.006)	0.001** (0.001)
Mean outcome	9.07	0.76	6.79	0.50	0.02	10.66	3.18
Observations	10,900	10,900	8,166	10,900	3,221	274,964	246,242
<i>Panel B. Nonnative English country of origin</i>							
Age for work permit	0.092 (0.074)	0.002 (0.009)	0.016 (0.039)	−0.004 (0.013)	0.012 (0.016)	0.110** (0.044)	0.018*** (0.006)
Age must enter school	0.043 (0.163)	−0.013 (0.013)	−0.049 (0.043)	−0.008 (0.016)	0.002 (0.011)	−0.022 (0.059)	0.003 (0.004)
Continuation school law	0.244 (0.164)	0.010 (0.026)	−0.017 (0.046)	−0.011 (0.037)	−0.006 (0.015)	0.082 (0.081)	−0.001 (0.012)
Years exposed to EL	−0.054 (0.052)	0.007 (0.005)	0.031* (0.017)	−0.000 (0.007)	0.008 (0.006)	0.007 (0.006)	0.001 (0.001)
Mean outcome	8.68	0.76	6.77	0.51	0.02	10.66	3.17
Observations	7,592	7,592	5,576	7,592	2,235	214,026	189,858

Notes: All 1940 specifications use the 1940 survey weights. Regressions with both samples include state of residence dummies, country of birth dummies, year of birth dummies, a dummy for work permit age missing, a dummy for entry age missing and state-of-residence cohort-specific trends. Regression with WWII data control for age at enlistment dummies as well as year of enlistment. Sample includes white males born between 1904 and 1924; individuals born or living in Hawaii, Alaska, or Washington, DC were dropped. Standard errors are clustered at the state level (in parentheses).

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

The standard errors are clustered at the state of residence level and the 1940 survey weights are used in census estimations.

The variable L_{sc} includes our measure of years of exposure to English-only and education laws. To measure compulsory schooling law exposure, we follow previous literature and include three variables: the age required for a work permit (at age 14), the age at which a child must start school (at age 6), and a dummy for whether a continuation school law is in place (at age 14). If the state had no law for the age of school entry or exit for a given cohort, we impute the sample average age. We also include a dummy for missing values. Thus, the effects for these two laws are identified only through changes in the required ages.

Table 6 presents the results. The results for years of school are consistent with the 1910 to 1930 results: higher work permit ages increased years of school but no other law affected educational attainment. The magnitudes are surprisingly similar for the 1940 census and the WWII samples: increasing the work permit age by one year increased schooling by about 0.1 years (a tenth of a year). These magnitudes are very similar for nonnative speakers and similar to those reported by Clay, Lingwall,

and Stephens (2012). The effects on completed education are lower than the contemporary enrollment results suggest though (about half the magnitude).

There appears to be an associated wage gain of about 1 to 2 percentage points in the WWII data. Assuming this gain is fully driven by the increases in education, the implied returns to school in panel A are about 14 percent per year of school, identical to what Clay, Lingwall, and Stephens (2012) estimate. The point estimates for 1940 are statistically insignificant, but the standard errors are large and we cannot reject a 1 to 2 percentage point effect on wages in this smaller sample (in other words, the WWII effect is within the confidence interval).

On the other hand, the work permit age has no effects on employment, naturalization status, or veteran status. Furthermore, there are no robust effects of age at entry, continuation school or English-only laws on education, wages, or employment.²⁶ None of the education laws we consider have an effect on “social integration” measured by veteran status or citizenship status. This is true for all immigrants and also for nonnative speakers.

Table 7 repeats the estimations using the 1940 census, dropping the last few cohorts, whose educational attainment appears to have been interrupted by the war (Figure 6). The coefficients on education are similar and the effects on wages are now statistically significant. The results also show that estimates based on the log of the occupation score underestimate the labor market returns to these laws, which appear to accrue within occupations as well as across occupations. But even among these cohorts we see no effect of the English-only laws and no robust effect of any other education laws except for work permit age laws.

Online Appendix Table IV shows the results for natives.²⁷ The effects of the work permit ages are positive and significant in both the 1940 and the WWII records, although the estimates are lower in the WWII data. These results are robust to matching based on state of birth or state of residence. Although we do not find statistically significant effects on wages, the WWII results are consistent with an 8 percent return to each additional year of school. On the other hand, we find no effects of the age at which children were required to enter school for natives or immigrants, which is consistent with contemporary evidence on the effects of age-based entry laws (Dobkin and Ferreira 2010). The estimates of continuation school laws are unstable across specifications and generally insignificant as are the effects of exposure to English-only laws.

Finally we note that the long-term results are estimated including state-specific cohort trends. As shown by Stephens and Yang (2014), online Appendix Table V documents that adding these controls substantially lowers the coefficients on our estimates of the effect of work permit ages.²⁸ Our estimates are therefore conservative since these trends absorb much of the variation in the laws of interest. However, even in regressions without state-specific trends, the coefficients on English-only laws are insignificant.

²⁶ Although entry age and continuation school have statistically significant effects in the WWII data, these effects are smaller and insignificant in the non-English speaking sample and they are dramatically different in size in the 1940 census.

²⁷ See the online Appendix.

²⁸ See the online Appendix.

TABLE 7—EFFECT OF LAWS ON MALE IMMIGRANT OUTCOMES IN THE 1940 CENSUS AND WWII ENLISTMENT RECORDS 1904–1920 BIRTH COHORTS

Outcome:	1940 census						WWII records	
	Years of school	Employed =1	log income in 1939	log occ score	Naturalized =1	Veteran =1	Years of school	log occ score
<i>Panel A. All immigrants</i>								
Age for work permit	0.124 (0.121)	−0.015 (0.014)	0.048** (0.023)	0.030 (0.045)	−0.008 (0.016)	0.020 (0.017)	0.118* (0.060)	0.006 (0.006)
Age must enter school	0.058 (0.140)	0.014 (0.017)	−0.014 (0.037)	−0.002 (0.037)	0.011* (0.006)	0.003 (0.012)	−0.046 (0.055)	0.000 (0.003)
Continuation school law	0.086 (0.141)	−0.015 (0.021)	−0.007 (0.046)	−0.026 (0.041)	0.002 (0.011)	0.008 (0.033)	0.068 (0.063)	−0.001 (0.010)
Years exposed to EL	−0.088 (0.070)	0.003 (0.008)	−0.012 (0.017)	0.034 (0.032)	−0.003 (0.004)	−0.005 (0.010)	0.008 (0.005)	0.002** (0.001)
Observations	9,756	9,756	7,627	9,756	3,079	9,756	274,964	246,242
<i>Panel B. Nonnative English country of origin</i>								
Age for work permit	0.168 (0.166)	−0.016 (0.021)	0.068** (0.027)	0.047 (0.042)	−0.006 (0.016)	0.025* (0.014)	0.125* (0.067)	0.008 (0.008)
Age must enter school	0.038 (0.180)	−0.004 (0.012)	−0.060 (0.037)	0.009 (0.049)	0.014 (0.009)	−0.008 (0.017)	−0.019 (0.054)	0.003 (0.004)
Continuation school law	0.192 (0.175)	−0.004 (0.029)	−0.012 (0.052)	−0.016 (0.046)	−0.012 (0.014)	−0.006 (0.040)	0.056 (0.081)	0.001 (0.012)
Years exposed to EL	−0.057 (0.105)	0.002 (0.009)	0.007 (0.016)	0.032 (0.038)	0.001 (0.004)	0.001 (0.011)	0.006 (0.006)	0.001 (0.001)
Observations	6,841	6,841	5,221	6,841	2,137	6,841	214,026	189,858

Notes: All 1940 specifications use the 1940 survey weights. Regressions with both samples include state of residence dummies, country of birth dummies, year of birth dummies, a dummy for work permit age missing, a dummy for entry age missing and state-of-residence cohort-specific trends. Regression with WWII data control for age at enlistment dummies as well as year of enlistment. Sample includes white males born between 1904 and 1920; individuals born or living in Hawaii, Alaska, or Washington, DC were dropped. Standard errors are clustered at the state level (in parentheses).

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

V. Discussion

These results allow us to draw several conclusions. English-only laws had no impact on the school enrollment of children or in their eventual years of education. This is true of immigrants and natives. Although the laws increased immigrants' literacy measured contemporaneously, it did so only among a subset of nonnative speakers. Furthermore we found no other measurable improvements in the adult labor market outcomes of immigrants exposed to these language laws as children. There are also no effects on likelihood of being a veteran or being a citizen.

The evidence consistently points to very moderate impacts of the language policy.²⁹ We consider various explanations for this result. One possibility is that our contemporary measures, literacy and fluency, are too coarse to detect an effect on

²⁹ We considered the possibility that the small effects of the law on literacy were due to changes in reporting of children's language ability to census enumerators after an English-only law was passed instead of actual increases

language skills. To test this possibility we estimated occupational score regressions for the 1910 to 1930 decades (since wages are not available until 1940) and included literacy and fluency as regressors.³⁰ We find that both have a positive return in the labor market, though the returns to reported literacy are larger than those for reported fluency. The results suggest that these measures do contain important information related to labor market success, but the laws did not affect them much. However, it is also possible that the results are driven by measurement error: exposure to English-only laws is measured with considerable error for immigrants because data on their residential history since migrating is unavailable. However, the contemporary evidence suggests otherwise because there is no measurement error in matching in the short term.

Another possibility is that individuals responded to the laws by switching schools. Figure 7 shows that the share of students attending private schools was rising between 1910 and 1930 in states that did not pass any English-only laws.³¹ But in states where the laws applied only to public schools, we observe a sharp increase in private school attendance after 1920 consistent with avoidance behavior in those states. We also observe that in states where English-only laws applied to all schools, there was a decline in the share attending private schools. This is consistent with the view that private schools lost their “comparative advantage” as a result of the law, since they could no longer cater to foreign students by teaching in their native language. Because the data is not available separately for foreign-born children, it is impossible to know whether these changes in enrollment were driven by the switching behavior of immigrant children. However, this evidence does suggest that the public school English-only laws were ineffective in part due to switching. However, even the laws that applied to all schools did not appear to have a long-term effect on immigrants.

Finally we consider the possibility that the laws had no effect because the nation as a whole was moving away from using foreign languages voluntarily. Although there is no data on language of instruction in primary schools, we found tabulations of the number of all students learning foreign languages for public and private high schools in the 1909 to 1910 and 1921 to 1922 school years. Figure 8 panel A shows that the share of students learning any foreign language declined very rapidly in the 1910s, at the same rate in states with and without English-only laws. Figure 8 panel B further shows that by 1922 German had essentially disappeared, going from being studied by 20 percent of public high school students to less than 1 percent.³² German disappeared faster in states without English-only laws. This evidence suggests that other trends were driving the decline in the use of foreign languages everywhere and is consistent with a very small effect of the language laws. Historical evidence supports this view: there were many voluntary changes that resulted in greater use of English in schools. For example, American bishops pushed for English-only policies in parochial schools (Galush 2000), and in many

in English literacy. However, a falsification test performed on adults shows no effect of the passage of English-only laws on the literacy or English-speaking of individuals aged 18 to 50.

³⁰ See the online Appendix.

³¹ We thank Claudia Goldin for providing us with this data.

³² The rapid decrease in the use of German has been noted by many (e.g., Zimmerman 2002).

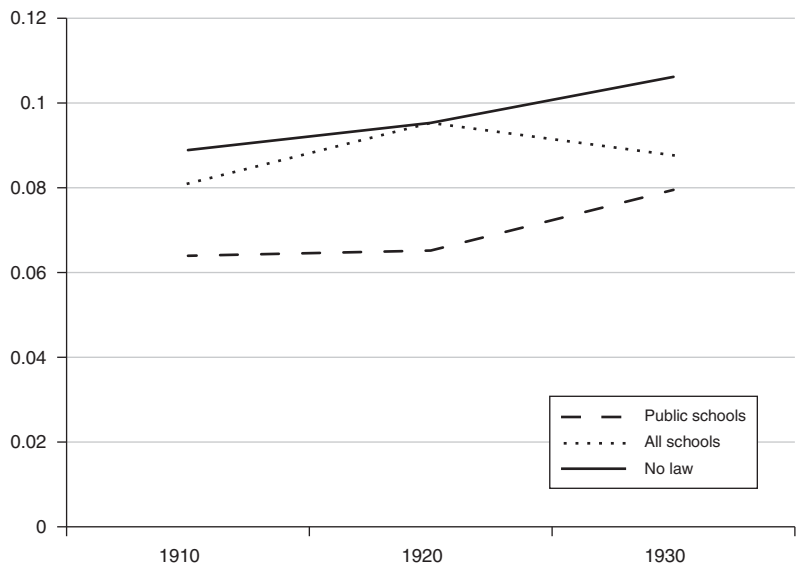


FIGURE 7. PERCENTAGE OF STUDENTS ENROLLED IN PRIVATE SCHOOLS BY TYPE OF ENGLISH-ONLY LAW

Notes: Figure reports (unweighted) average across states. Claudia Goldin and Larry Katz provided the enrollment data, which were compiled from various reports from the Commissioner of Education. See the Appendix to Goldin and Katz (2011) for details.

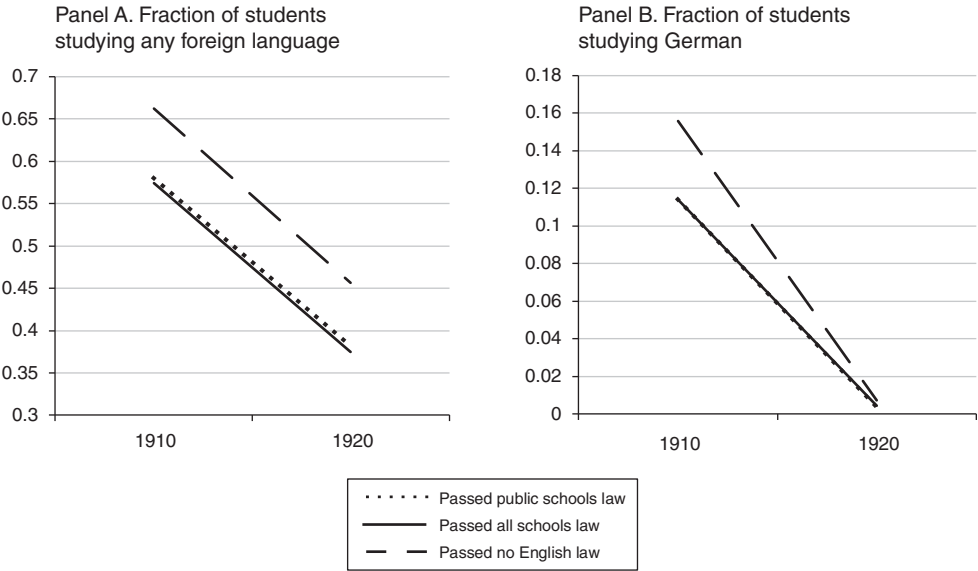


FIGURE 8

Notes: Graphs report the (unweighted) average across states. Data on foreign language use by state and year was reported in the Report of the Commissioner of Education in 1911 and Biennial Survey of Education for 1920–1922 (Bureau of Education, Department of the Interior).

cities “diocesan authorities ordered national parishes to limit non-English instruction” (Zimmerman 2001). Thus, even in the absence of legislation, children’s exposure to the English language was large and growing. Alternatively, the laws might have not worked because they were not enforced. However, the historical record does not allow us to assess this possibility.

Although language laws had no economically large impacts, laws that forced children to be in school as adolescents did have measurable impacts. Immigrant enrollments increased by 6 percent as a result of the work permit laws. This accounts for 74 percent of the increase in immigrant enrollment over the period. The gap in enrollment between natives and foreign-born was about 8 percentage points in 1910, and our estimates imply that forcing children to go to school closed about 40 percent of this difference due to the laws’ proportionally larger effect on immigrants. Consistent with these findings, the 1940 census and the WWII enlistment records show that laws forcing children to stay in school during their adolescence (by requiring a work permit) had a positive impact, both on immigrants’ educational attainment and on their subsequent wages. The effects of these laws on education were larger for immigrants than for natives. On the other hand, we find no robust effects of laws regulating the age at which a child had to enter school or laws forcing working adolescents to continue attending school part-time.

None of the laws we examine had a positive impact on the likelihood of being a veteran or a citizen among immigrants, so along this dimension the Americanization movement did not achieve its goals. However, it is clear that there were strong trends among immigrants towards integration, and their outcomes were converging towards those of natives relatively fast. It is possible the Americanization movement contributed to voluntary integration, but we find no evidence that the laws regulating English in schools contributed significantly to that process.

DATA APPENDIX I: ENGLISH LAWS

The data on English-only laws were compiled from the legal and historical sources listed below. No mention of English laws could be found for the following states: Alabama, Florida, Kentucky, Maryland, Michigan, Mississippi, New Jersey, North Carolina, Tennessee, and Wyoming.

State Statutes

- California Political Code § 1664 (1915)
- Colorado Session Laws § 6010 (1908)
- Colorado Session Laws Ch. 179 § 6010 (1919)
- Delaware Rev. Code 2283 Ch. 157 §11 (1919)
- Idaho Session Laws Ch. 153 p. 493 (1919)
- Illinois Laws of 1919, Ch. 917, § 1
- Indiana Statute § 6582 (1913)
- Indiana Session Laws, Ch. 18 §1 (1919)
- Iowa Code § 2749 (1897)
- Act of General Assembly of Iowa, Ch. 198, I.C.A. § 1 (1919)
- Kansas General Statute Sec. 8985 (1915)

Kansas Law Ch. 272, Amending Sec. 9415 of 1915 General Statute (1919)
 Louisiana State Constitution of 1898, Article 251
 Louisiana State Laws of 1918, Sec. 1, Act 114, p. 188
 Maine Laws of 1919, Ch. 146, Amending R, S, Ch. 16, Sec. 122, Part 7
 Massachusetts Laws of 1902 (First Rev. Laws 1902), Ch. 44, Sec. 2, p. 478
 Minnesota General Statute Sec. 2797 (1913)
 Minnesota Laws of 1919, Ch. 320
 Montana Laws of 1907 (as cited in 1 Rev. Codes 1915 § 912)
 Montana Laws of 1913 Ch. 76, p. 237
 Montana Laws of 1915 (3 Rev. Codes 1915, Supplement Sec. 824)
 Montana Laws of 1917 (Rev. Codes 1917, Sec. 912)
 Nebraska Mockett Law (1913)
 Siman Act, Session Laws of Nebraska 1019, Chapter 249 § 7 (1919)
 Nebraska Laws of 1921, Ch. 61 § 6457-62
 Nevada Laws of 1919, Ch. 133, Sec. 1, p. 247
 New Hampshire Laws of 1919, Ch. 84, Amending Pub Statute, Ch. 93, Sec. 14
 The New Mexico Enabling Act, Ch. 310 §2 (June 29, 1910); 35 Statute 559
 New Mexico Constitution, Art. 21 Sec. 4 (1911)
 NY Education Law Ch. 140 § 945 (1909)
 NY Education Law Art. 23 Sec 620 (in existence in 1920)
 North Dakota Laws of 1918 (Laws 1918, Ch. 41)
 Ohio 3 Ann General Code 1910 Sec. 7729 (1910)
 108 Ohio Laws 614 § 7762 (1)-(3) (1919)
 Oklahoma 1 Rev. Laws, Constitution of 1907, Art. 1, Sec. 5 (1910)
 Oklahoma Laws of 1919, Ch. 141, Sec. 1, p. 201
 Oregon General Laws p. 281 Ch. 72 § 2 (1909)
 Oregon Laws of 1919, Ch. 19, Sec. 1, p. 34
 South Carolina Laws of 1919, Sec. 5, Ch. 135
 South Dakota Laws of 1918 Ch. 42, Sec. 1, and Ch. 41, Sec. 1
 Texas 2 Civil Statute of 1914, Art. 2782
 Texas Acts, 4th C.S.P. 179 (1918)
 Texas Comp. Laws § 1850 (1907)
 Washington Laws 1912 (Pierce's Code, Ann. Title 413 § 215)
 Washington Laws of 1919, Sec. 4889
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Bartels v. State of Iowa, 262 US 404, 410 (1923)
Farrington v. Tokushige 273 US 284 (1927)
Pohl v. State, 102 Ohio St. 474 (1921)
Nebraska Dist. of Evangelical Lutheran Synod of Missouri, Ohio, and other States v. McKelvie 104 Neb. 93 (1919)
Pierce v. Society of Sisters, 268 US 510 (1925)
Hardwick v. Board of School Trustees of Fruitridge School Dist., Sacramento County, 54 Cal.App. 696 (1921)

Pohl v. State, 102 Ohio St. 474 (1921)
Hughes v. Caddo Parish School Bd., 57 F.Supp. 508 (D.C. LA 1945)
Westminster School Dist. of Orange County v. Mendez, 161 F.2d 774 (Cal. 1947)
City of Mobile v. Rouse, 173 So. 266 (Ala. 1937)
State v. Sisters of Mercy, 115 So. 323 (Miss. 1928)
Busboom v. State, 110 Neb. 629 (1923)
Nebraska Dist. v. McKelvie, 262 US 404 (1923)

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