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Did the Americanization Movement Succeed? An Evaluation of the Effect of English-Only and Compulsory Schools Laws on Immigrants' Education

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ABSTRACT

In the early twentieth century, education legislation was often passed based on arguments that new laws were needed to force immigrants to learn English and "Americanize." 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 from 1910 to 1930. English schooling statutes did increase the literacy of foreign-born children, though only modestly. Compulsory schooling and continuation school laws raised immigrants' enrollment and the effects were much larger for children born abroad than for native-born children.

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"Everyone should speak English or just shut up, that is what I say," Calvin, Calvin and Hobbes

I - Introduction

During the last decades of the twentieth century, immigration rates to the United States increased substantially, reaching their highest levels since 1924.[1] A majority of these new immigrants come from Spanish-speaking countries. Perhaps as a result, recent years have also seen a resurgence of legislation geared towards making English the official language of the states; for example between 1980 and 1990 sixteen states passed such laws (Arington, 1991). The proponents of these English-only laws also perceive bilingual education programs as being detrimental to immigrants and support legislation to make English 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, 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 socio-economic 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 and decreased federal funds for bilingual education, emphasizing instead 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.

[1] In 1910, foreign born constituted about 15% of the population, a historical maximum since 1900. In 2000 the share of foreign born was about 12.5% (http://www.census.gov/population/www/documentation/twps0029/tab01.html, http://pewhispanic.org/files/factsheets/foreignborn2008/Table%201.pdf)

Though there is still an active debate on the costs and benefits of making English the main language of instruction, this is in fact not the first time that that these kinds of laws have been proposed and enacted. This paper investigates the impact of earlier assimilation policies known as the Americanization Movement, the name given to the diverse attempts of states to assimilate the millions of new immigrants from southern and eastern Europe through education policy between 1910 and 1930. Nativist legislators perceived new immigrants as a threat to society for a host of reasons. In addition to their differences in religion these new immigrants had low literacy rates, were not fluent in English, and were generally unfamiliar with American customs and the workings of democratic government. In contrast to the largely literate non-South native white population, in 1910 about 23 percent of the foreign born over 10 years of age were unable to speak English (Edwards, 1923). 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, p. 12). Furthermore, nativists could not rely on the existence of public schools to Americanize the children of these immigrants. Poor parents often placed their children in the labor force or enrolled them in parochial schools where they would have no opportunity to learn English since classes were often taught in their native language. Many saw public schools not only as a mechanism for assimilation, but also an institution which would instill patriotism (Ross, 1994).

The onset of World War I made immigration issues even more salient. Although laws that regulated language use had been in place in some states since the nineteenth century, legislators many changes in the 1910s and 1920s, particularly in the years immediately following the war. For example, in 1919 alone 21 states passed some legislation making English the only or the main language of instruction (Edwards, 1923). States also made compulsory laws more

stringent, in part as an attempt to force immigrants to attend school. We look at whether these education laws, which often targeted immigrants, were effective at increasing their English fluency, literacy and overall education levels in the 1910 to 1930 period. To our knowledge this question has not been studied before.

Previous work has examined the effects of compulsory schooling and child labor laws on the educational attainment of natives, and this work has found that this legislation had positive but modest effects on their education (Lleras-Muney, 2002; Goldin and Katz, 2008). But no work documents whether these laws affected immigrants – as legislators intended. We ask if the set of education laws affected immigrants and natives differentially and whether English laws affected immigrants at all.

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 that English-only laws did in fact increase the literacy of immigrants during this period, particularly for children from non-English speaking countries and of illiterate parents, but that these laws did not by themselves or in combination with compulsory schooling laws affect the school enrollment or employment of immigrants or natives. Their effects were also modest in magnitude. We also find that compulsory schooling laws and continuation school laws had a large impact on the enrollment of immigrants. The effects we estimate are about twice the size as the effect these laws had on natives.

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 period and empirically analyze what state characteristics can account for the passing of these laws (Section II). Then we describe the methodology and data

that we use to investigate immigrant outcomes (Section III). In Section IV we estimate the effect of education laws on immigrants. We then assess at whether the effect of the legislation was larger for immigrants then for natives (section V). We close by discussing the magnitudes of the effects and the overall findings (Section VI).

II - The Americanization Movement and Education Laws

II. 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 immigrants from Europe between 1880 and 1915, and by 1900, 15 percent of the American population was foreign-born. New immigrant flows had also grown more diverse and no longer consisted primarily of Protestant Anglo-Saxons. The influx of German and Irish Catholic immigrants beginning in the 1840s sparked the "Know Nothing" nativist movement, and the arrival of Russian Jews, Catholic Italians, and other southern and eastern Europeans after 1885 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 U.S. to further immigration from countries outside northwestern Europe.

Legal and historical accounts of English-only legislation emphasize concerns over immigration as the primary driver of the laws' passage. For 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."

Tensions over the origin and customs of new immigrants peaked with the outbreak of World War I, after which Americans became suspicious of all things German (O'Brian, 1961). Many 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. In 1914, the use of the German language was prevalent in German-American social clubs, newspapers, churches, and parochial schools (and those public schools that offered instruction in German). In fact, by the onset of World War I, there were approximately 522 German-language periodicals across the U.S. and over two thousand Roman Catholic churches conducting services in German. But German became unpopular: the proportion of high school students studying German fell from 25 percent in 1915 to 1 percent in 1922 (Ross, 1994, p. 65).

Additionally, Americans often saw the term "Lutheran" as synonymous with "German" because two-thirds of the German population was Lutheran, and approximately half of the Lutherans in America were of German descent. Accordingly, the use of German in Lutheran parochial schools and churches aroused great suspicion, making the use of the German language in schools a main target of the Nativist movement. In fact, several of the laws that were passed specifically prohibited the use of German in schools. The anti-German movement believed that while they could not control the use of German by adults, they "hoped that the schools would break the German language cycle" (Ross, 1994, p. 45).

State legislators responded to these challenges with bundles of laws aimed at assimilating children through required English-language schooling. Proponents of the Americanization-by-education theory saw primary schools as a highly

desirable policy target because enrolled pupils would be reached during their most formative years. 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 language is an elective or a prescribed subject of the curriculum, not to exceed on hour each day."[2]

This law is careful to state that schools needed to be conducted in English, so a German-speaking parochial school would not count. Statutes often included a minimum age at which a child could obtain a work permit as well.

We collected laws on English language instruction for each state during 1910-1930 from various sources (see the Data Appendix). There are significant differences across states in how these laws were specified; in some states the law applied to all schools but in others they referred only to public schools, and in some states the laws affected only primary but not secondary schools. We coded a state as having an "English only" law if it required all instruction to be in English except for the teaching of foreign languages themselves in all schools, including parochial (Appendix Table 1 lists education laws for each state and each

[2] Minnesota, Laws 1919, ch. 320, amending Gen. Stat. 1914, sec. 2979 as described in Ruppenthal (1920).

census year).[3] Figure 1 shows that in 1910 about 12 percent of states had strict language laws, and this number rose to more than 50 percent by 1920. Almost all of the changes occurred in the 1910-1920 decade with only a few after 1920. The closing of the frontier in 1924 and the 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 most likely responsible for the lack of further legislation. In the next section we empirically analyze the determinants of the passage of these laws.

Compulsory schooling and child labor laws regulated the ages when a child needed to be in school and the conditions under which and ages when a child could work. These laws were already in place by 1910 in most states, and by 1920 all states had such statutes. But these laws changed many times during the 1900-1940 period. There was also a large political effort to increase the education levels of the population in general, and the education of immigrants in particular. Education was believed to further a common American culture and a function as a bulwark against crime, inequality and economic stagnation. There were also concerns about idle youths, and these education laws were in part "anti-truancy" laws. On the other hand, states that employed large fraction of children were opposed to laws that prohibited or restricted the employment of youths, and there was also opposition to compulsory laws from those that argued that such laws restricted individual freedoms and those who believed that not all children would benefit from additional education (Goldin and Katz, 2008). Another important piece of legislation required that children who were granted permission to work and were below the mandated age for dropping out of school attend school on a part-time basis. These "continuation school laws" increased the cost of employing children, since the law required that children attend school during the work week.

[3] We also coded less stringent laws (only pertaining to public schools) and examine them as a robustness check.

The compulsory schooling data were compiled by Goldin and Katz (2008). They report laws in place from 1910 to 1939 for each of the 48 states (Alaska, Hawaii and Washington, D.C. are excluded in all years). Following previous work (Lleras-Muney, 2002; Goldin and Katz, 2008), 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 drop out of school, and whether the state required children with work permits to continue attending school on a part-time basis. Figure 1 shows the trends in these laws. The age at which children had to enter school increased slightly in the 1910s but remained stable in the 1920s. On the other hand, the work permit age rose each decade and thus the implicit number of years that a child had to stay in school increased by about two years over the period. Finally, continuation school laws became widespread during this period. No state required working teens to continue going to school part-time in 1910, but such laws existed in more than half of the states by 1930.

II. b - What explains the passage of Americanization laws?

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 in states where education levels were high, these laws were more likely to be passed. These results support the hypothesis that immigration was one impetus for compulsory education legislation. But the results also suggest that these laws were passed in part because they would affect a small number of individuals: education levels among natives were already relatively high and trending upwards. We do

not reproduce these results here and instead focus on the factors that predict the passage of English-only laws.

English-only laws were often included in the same text as compulsory schooling laws and to some extent the historical accounts suggest that they were driven by the same underlying forces as these other education laws, namely the level of immigration and its determinants, such as income. But other factors may have also mattered for the passage of English-only laws. In particular the timing of the laws appears to be driven by the onset of the war, and the fact there were few changes after 1920 is consistent with the closing of the border in 1924 having ended the interest in immigration-related laws.

The stringency of the laws might have been a function of the number of German immigrants instead of the total number of immigrants. In addition the religious affiliation of natives may have also been important since religious differences (in addition to language) were seen as threatening. Finally, the political power of immigrants could have affected the legislation: in areas where immigrants constituted a large share of the population, immigrants might have successfully blocked English-only laws.

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

$$\boldsymbol{I}(English=1)_{st} = \boldsymbol{f}\left(\boldsymbol{c} + \boldsymbol{b}X_{st} + \boldsymbol{\alpha_R} + \boldsymbol{\eta_t}\right), \quad (1)$$

where I(English=1) is a dummy equal to one if states passed an English-only law and zero otherwise. X_{st} is a vector of state characteristics at the start of the decade, and we also include four region dummies. In the simplest specification we check 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. Six states that already had such laws in place in 1910 are therefore not included in the estimation.

We collected data from various sources (see Data Appendix) on the state characteristics that are suggested above as predictors of the laws. These include baseline education characteristics (measured using enrollment levels and number of schools), demographic composition of the state (share foreign born, share German immigrant, share black, share of the population over 65 and share under 14, share urban), economic and political measures (state income, share in manufacturing, share voted Republican and share Catholic). Finally we also consider whether states with more stringent education laws were also more likely to pass English-only laws. Because we have very few observations we consider explanations one at a time and then include all regressors at once.

The results are in Table 1. In general, we do not find any statistically significant predictors of the passage of these laws, outside the share of Catholics in the population, which makes it less likely to have passed English-only laws. No other variable is statistically significant, even when it is the only regressor. Furthermore several covariates have signs that change depending on the specification. Overall, in spite of our data collection efforts, we are unable to find many statistically significant predictors of the legislation, although this is perhaps due to the small sample size. Most importantly, we do not find any evidence that the enrollment of natives or the foreign born predict legislation. But our results must be taken with caution because we do not have a lot of power to detect these effects. In our main empirical work, we will test how sensitive our results are to adding these state-level controls.

III-The effect of education laws legislation on immigrant education

III. a-Empirical strategy

The main purpose of this paper is to assess whether the education laws that were 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 (more details on the data are given in the next section), we estimate the following probit model:

$$\boldsymbol{I}(y=1)_{ist} = \boldsymbol{f}\left(\boldsymbol{c} + \boldsymbol{b}L_{st} + \boldsymbol{X_i}\boldsymbol{\delta} + \boldsymbol{\alpha_s} + \boldsymbol{\eta_t} + \boldsymbol{\alpha_s} * \boldsymbol{year}\right) \quad (2)$$

where $I(y=1)_{ist}$ 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 and parental characteristics (literacy and place of birth); and the specification includes 47 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 et al, 2004). 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: it measures 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 in educational attainment. To interpret b as the causal effect of legislation on schooling outcomes, we require that changes within states in legislation not be correlated with changes in other determinants of schooling, and that changes in legislation not result from changes in schooling within states. We test whether the results are robust to including region-specific trends or state-specific trends because both enrollments and legislation have

strong trends which could differ by geographic area (see Figures 1 and 2). We also test how robust our results are to including additional state covariates that we used to predict legislation. Finally we perform a series of basic falsification exercises such as checking whether native English speakers are affected by English-only laws.

III. b - Data

We use the 1910, 1920, and 1930 1% random samples of the censuses available through IPUMS.[4] The data contain individual characteristics (age, gender, race, and ethnicity), place of birth, and place of residence, as well as characteristics of the individual's parents (literacy, English fluency, and birth place). We restrict the sample to non-black individuals aged six to sixteen living in the 48 contiguous states.[5] Children whose place of residence, place of birth or whose 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 US, "first-generation" immigrants if children were born abroad, and "second-generation" immigrants if they were born in the US but either of their parents was not.

Table 2 shows summary statistics for our samples. Our key variables of interest are 1-whether the child is reported to be in school (any time since September 1st[6]), 2-whether the child works (which we code as one if the respondent reports an occupation), 3-whether the child is literate (can read and write in any language) and 4-whether the child speaks English. Literacy and

[4] See http://usa.ipums.org/usa/

[5] We exclude blacks because previous work did not find legislation to be effective for them. Also since there are no laws requiring 5-year olds or those 17 and above to attend school, these ages are excluded.

[6] The 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 in 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 4 months. In 1930, the census date is April 1 and the reference period 6 months.

English fluency were only asked of children 10 years and older, whereas enrollment and work are available for all ages. Figures 2-5 show the trends in these variables in the various samples we study.

Enrollments were rising during the 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 and thereafter appears stable. Although foreign-born children were less likely than natives to be in school in 1910 (only 79 percent reported being in school compared to 87 percent of the native born), their attendance rates grew more quickly (in part because they began the period with lower enrollment rates), and in 1930 the gap between natives and foreign born was only 1.7 percentage points. Figure 3 shows trends in employment. In 1910, 18 percent of immigrant children and about 10 to 13 percent of natives and second-generation children were working. But these fractions fall over the period, and more so for immigrants.

Not surprisingly, almost all second-generation and native children reported speaking English throughout the period.[7] For immigrant children there is a quantitatively large, 14 percentage point increase in the fraction speaking English from 1910 to 1930 (Figure 4).[8] 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 our last variable of interest, literacy. Literacy rates for second-generation children were reported as being almost 100% 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

[7] Technically the report is made by the parent or household member that answered the census questionnaire.

[8] There 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.

increased significantly during the period, with the largest increases again seen for immigrant children. Changes in our four outcomes of interest over the 1910-1930 period were clearly largest for immigrant children, who appear to have converged towards 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 discuss later in the paper.

IV - The effects of Americanization laws on the education of immigrant children

IV. a - Main results

We begin by investigating whether compulsory schooling laws and English-only laws had any impact on (first-generation) immigrant children. Table 3 presents the results. All the specifications include state and year fixed effects as in equation (2).

Column 1 reports the results for school enrollment. Compulsory schooling laws had a positive and statistically significant effect on enrollment. The coefficients imply that if immigrant children aged 6 to 16 lived in a state that required them to be in school, they were 5 percentage points more likely to be in school, an increase of 6 percent relative to the mean. If we further restrict attention to children ages 10 to 16 (column 4), the coefficient is larger, about 9 percent, a sizeable effect. The coefficients on continuation laws are also positive and statistically significant. The magnitudes are about half the size of the compulsory schooling laws effect. Thus, if state laws required children to be in

school and further required those who worked to attend school part-time, enrollments increased by 9.5 percent for those aged 6 to 16, and 13 percent for those aged 10 to 16. On the other hand, the coefficients on English-only laws are negative, very small and statistically insignificant – perhaps because the focus of these laws was not enrollment per se.

In columns 3 and 4 we assess the effects of the laws on the likelihood of employment. We find that compulsory schooling laws have negative and statistically significant effects, but the coefficients on continuation school laws and English laws are small and statistically insignificant. Forcing children to be in school lowered the probability that they worked by 35 to 40 percent and the effects are of similar magnitude for both age groups relative to the mean. These are very large effects. In columns 5 and 6 we look at how these laws affected the likelihood that children were both in school and not working (a measure that has been used to capture "full-time enrollment", see Goldin and Katz 2008). All laws here appear to have positive effects though only compulsory schooling laws appear significant – they imply an increase in full enrollment of 5.4 for all children and 7.3 percent for the 10-16 age group.

In column 7 we consider whether children were more likely to be literate as a result of these laws. English-only laws increased literacy by about 3 percent, and this effect is statistically significant though only marginally (at the 10 percent level). The effects of compulsory schooling and continuation school laws are positive but small (about half of the size of the English law coefficient) and not statistically significant. Column 8 reports the effects of the laws on whether or not immigrant children spoke English. None of the coefficients is statistically significant by itself, and a test of joint significance also finds that the laws appear to have no effect.

These results suggests that immigrant children were affected by these laws, and the magnitudes of these effects were substantial in the case of

compulsory schooling and continuation school, but there is only a marginal effect of English-language laws on literacy and no effect of on speaking English. English laws did not in principle aim at increasing attendance, so perhaps the fact they did not is to be expected. But the weak results for literacy and fluency are more surprising. We investigate the robustness of these findings next.

The first row of results of Table 4 reproduces the coefficients on the "English law" variable from Table 3 for reference. We first drop the individual level controls for whether parents are literate, fluent in English and the year of immigration of the child. The coefficients are unchanged, suggesting that the omitted parental characteristics were not biasing the results presented in Table 3. We then restrict attention to immigrant children from non-English speaking countries, the group that was targeted by the laws, and again the effects are positive and significant for literacy and positive but insignificant for fluency. The effect of the English law on literacy is the largest for immigrant children who have been in the U.S. for two or fewer years, but with the small sample size this effect is only significant at the 10 percent level. The literacy effect is also larger for the sample of immigrant children who have been in the U.S. for five or fewer years than for the full sample. But again there are no significant effects on fluency for either of the newly arrived subsamples. Finally, the effects on literacy are positive, significant, and larger for children whose parents were illiterate or did not speak English, but again there is no statistically significant effect of the laws on fluency for either of these groups.[9]

Overall Table 3 and 4 suggest that immigrant children were affected by these laws, and the magnitudes were substantial in the case of compulsory schooling and continuation school laws, which increased enrollment likelihood by about ten percent and decreased the likelihood of an immigrant child working by

[9] The table also shows that the results are unchanged when we control for state covariates.

about 35 percent. English-only laws also had significant effects on literacy but these effects were small (about a three percent increase), though larger for children from non-English speaking countries (four percent), those with illiterate parents (eight percent) or the most recently arrived (up to 23 percent). The effects on English fluency are never statistically significant, though they are positive and similar in magnitude to the effects on literacy.

IV.b - Further robustness checks

In Appendix Table II we further investigate the effect of these laws. Panel A uses an alternative definition of whether a state has a law that requires English: we define it now as equal to one if any schools were regulated (whereas our usual measure is a more stringent one, equal to one only if all schools were required to follow the law). This alternative measure makes the coefficient on literacy now insignificant and does not change any other qualitative result.[10] This suggests that the main effect of the English-only laws was through their effect on parochial schools.

Since the passage of English-only laws was correlated with the passage of compulsory schooling laws, and there is less variation in English-only laws, we might be underestimating the effects of English laws by considering all three laws together. Nonetheless, in Panel B we show that our estimates are identical to those in Table 3 when we include English-only laws by themselves.

We also investigated whether there were any interactions between English-only laws and compulsory schooling and continuation school laws; these could be complementary since requiring that English be taught all schools is effective only if children are in school.[11] But it is also possible if the laws were perceived as a way to discriminate against different immigrant groups, then

[10] We also estimated a specification where we used the number of years that a child would have been in school while the English law was effect if he had gone when mandated. There was no statistically significant effect.

[11] Results available upon request.

English-only laws could have dissuaded some from attending school. Indeed, we find that these interactions are negative, but they are statistically insignificant. The effects are also statistically indistinguishable for boys and girls though surprisingly they are larger for rural areas in comparison with urban areas.

Our results suggest that the English-only laws affected immigrant children's literacy. However, this result relies on the assumption that the laws are not correlated with other unobserved determinants of these children's outcomes. In Appendix Table II Panel C we conduct another test of this assumption: we check to see if the English laws affected the literacy or fluency of second-generation immigrants whose native language is English. We find no effect of the English laws for them, regardless of their mother's year of arrival (Panel D), in contrast to what we found for first-generation immigrants.

In summary we find that education laws did have a significant impact on immigrant children. We now investigate whether the effects were similar on immigrants and natives.

V - 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, these effects were rather modest, especially when compared to the large increases that took place during the period these laws were passed (Lleras-Muney, 2002; Goldin and Katz, 2008). However, as Table 1 indicates, native children (of native or foreign-born parents) had higher enrollment rates than immigrant children and significantly lower employment rates. And, as the historical record suggests, many of these laws were passed specifically to target immigrants. We test to see whether, for enrollment and work, there are statistically significant differences in the effects of these laws

between natives, first-generation and second-generation children.[12] To do so, we pool all groups together and add dummies for first generation, second generation, and the interaction of each of these dummies with education laws, urban residence, gender, and age.

The results for the full sample are in Table 5. The coefficient of compulsory schooling for natives is positive, statistically significant, and consistent with what previous literature has found. The interaction with second generation is small and statistically insignificant. Although the interaction with foreign born is not statistically significant, the magnitude of the interaction implies an effect that is about 50 percent larger than the effect for natives, a very large difference.

The results for continuation school laws are surprising. The main effect of the laws on natives is not significant and is very small. On the other hand, the coefficients on the interactions for both first and second generation are positive and statistically significant, though the coefficient is twice as large for first-generation children. These results suggest that in previous work, the effect of continuation school laws on children's educational attainment was driven by the laws' effects on second-generation children. The effects on employment are in the second column. They show the compulsory school lowered the employment of all groups equally. There is a large negative coefficient on second generation children, but it is not statistically significant. Neither continuation school laws nor English laws appear to have an effect on any group's employment.

Table 6 further explores the results on enrollment and shows a number of additional checks. To increase sample sizes we expand our sample and include a 1% sample from 1940. We also drop English-only laws since they are never significant. Column 1 reports the results for enrollments using our main

[12] We do not look at the differential effect of these laws on literacy and English proficiency since the means of these variables for natives are essentially 100 percent.

specification which includes just state and year fixed effects as in previous tables. We find the same results as in Table 5, though the magnitudes change a bit in this larger and later sample. Column 2 adds region-specific trends and column 3 adds state-specific trends. In columns 4-6 we show the identical set of specifications including state-level controls. The qualitative results are the same, and importantly we now do find a statistically significantly different effect for foreign-born children: the effect of the laws was twice as large for them.[13] Thus the results all suggest that the enrollment rates of immigrants were substantially more affected by compulsory schooling and continuation laws than those of their native counterparts.

VI - Discussion

We find that English-only laws increased the literacy of immigrant children but had no effect on their enrollment or likelihood of work. English-only laws had no effect on outcomes for second-generation children or natives, and their effects are concentrated on immigrants from non-English speaking countries. These results are consistent with a causal effect of the legislation on outcomes. The magnitudes suggest that passing an English-only law affected literacy by 3 percent, a small effect. The change in literacy rates between 1910 and 1930 for the foreign-born is about 4 percentage points and our estimated effect of English-only laws is 2.7 percentage points. Hence, our results imply that about 70 percent of the increase in literacy over the period is explained by the English-only laws. However, literacy rates were high and rising so the level effects are modest.

Though we find effects of the English laws on literacy, we can't rule out that people are simply more likely to report that their children were literate after

[13] We also ran the specification with non-Southern states only since there were few immigrants in Southern states and the non-Southern states with controls. The results are quantitatively unchanged (table available upon request).

these laws were passed. We also don't know if the laws had some other unobserved (unintended) effects on immigrants, for example it might make them less likely to benefit from immigrant networks in the labor market. Thus it would be useful to be able to estimate the return of the legislation in terms of the outcomes such as labor market success and political integration. Unfortunately, this exercise is generally not possible since there are no data that allow us to know where immigrants first lived in the United States and where they grew up. The only possibility then is to match individuals to the laws in place in their state of residence at the time of the census. However, Appendix Table III shows that the effect of compulsory schooling and continuation laws on native-born individuals' years of schooling in 1940 is positive and significant if we match using state of birth, but not if we match using state of residence. This difference is explained by the fact that the laws appear to have affected those who in 1940 are not living in their state of birth (in other words, the laws affected mobility).

Laws that forced children to be in school can account for 74% of the increases in immigrant enrollment over the period. We also find that compulsory schooling and child labor laws had a substantially larger effect on the enrollment of immigrant children than on natives (about twice as large). The gap in enrollment between natives and the foreign born was about 8 percentage points in 1910, and our estimates imply that forcing children to go to school closed about 40% of this difference due to the laws' proportionally larger effect on immigrants. Furthermore, our estimates of the effects of continuation school laws suggest that, ceteris paribus, these two laws in combination closed almost the entire gap in enrollment between natives and immigrants. The effects are less striking for second-generation children, who are remarkably similar to natives in their levels and trends of schooling.

Although these results suggest that these laws had large effects, the data also show that immigrants appear to have converged within a generation. Thus the

question is the extent to which these laws accelerated the process of convergence. The gap in enrollment in 1910 between natives and foreign-born was about 7.7 percentage points. If we assume that this gap would be close within a generation (25 years) then CSLs are equivalent to about eight years of progress. The effects of these laws on employment are different. Forcing children to be in school appears to have the same negative effect on employment for all children. And continuation school laws appear to have had no effects on employment for any group.

Overall our results suggest these laws had positive effects on the targeted immigrant groups but overall their magnitudes in levels suggest that their effects were modest. An important question for future research would look at whether these laws had any effect on the long run economic and social outcomes of immigrants.

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DATA APPENDIX

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 Laws of 1919, Ch. 19, Sec. 1, p. 34
Oregon General Laws p. 281 Ch. 72 § 2 (1909)
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
West Virginia Laws of 1919, Ch. 2, Sec. 9, Lines 12-14
Wisconsin's Bennett Law of 1889

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Bartels v. State of Iowa, 262 U.S. 404, 410 (1923)
Farrington v. Tokushige 273 U.S. 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 U.S. 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)
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William G. Ross. "A Judicial Janus: Meyer v. Nebraska in Historical Perspective," The University of Cincinnati Law Review (57) p. 125-204 (1988): Indiana (1889), New York (1889, p. 57) , Wisconsin (1854).

Barbara Bennett Woodhouse, "Who Owns the Child?: *Meyer* and *Pierce* and the Child as Property," 33 William & Mary Law Review (998) (1992): Minnesota (1894).

State-Level Data

Share Republican ballots was taken from ICPSR Dataset 8611, Electoral Data for Counties in the United States: Presidential and Congressional Races, 1840-1972, PIs Jerome M. Clubb, William H. Flanigan, and Nancy H. Zingale. We used turnout for congressional races in 1910, 1920, and 1930.

Data on state per capita income were taken from the BAE and indexed to 2000 dollars. State per capita income was interpolated for 1910.

Share of population Catholic was taken from ICPSR Dataset 8, Censuses of Religious Bodies, 1906-1936, United States Department of Commerce, Bureau of the Census. Values for 1910, 1920, and 1930 were interpolated/extrapolated using the censuses from 1906, 1916, and 1926.

Variables on enrollments, the foreign-born, Germans, non-German foreign born, blacks, individuals over the age of 65, individuals under the age of 14, and population in urban areas were computed from the respective IPUMS samples for each year.

Data on schools per square mile was taken from Adriana Lleras Muney's work on compulsory schooling; see her 2002 paper for details on the construction of these variables. We extrapolated to estimate the 1910 values for schools per square mile.

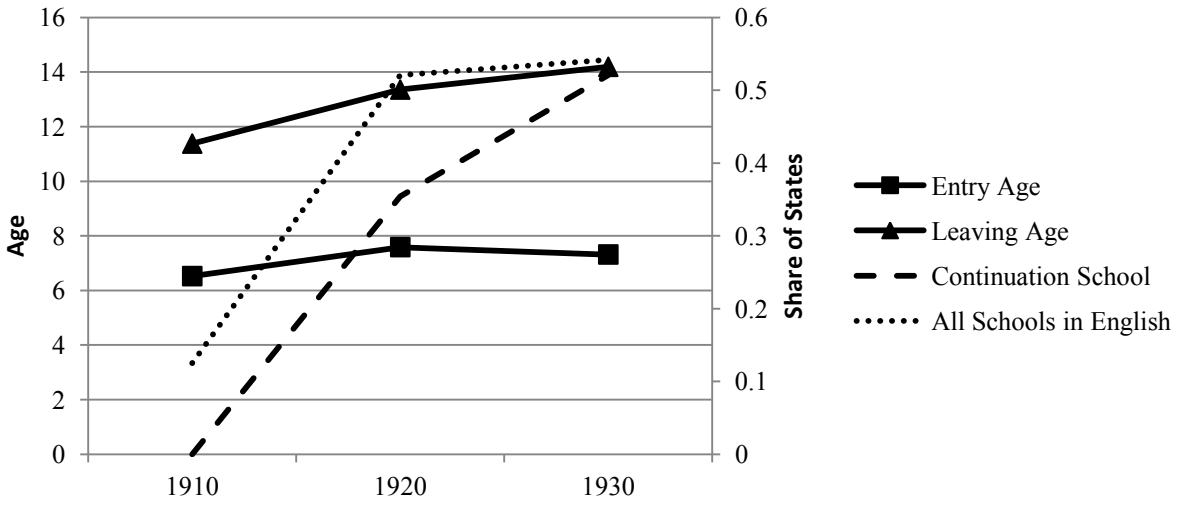
Data on private enrollment shares were collected from several editions of the Biennial Survey of Education prepared by the U.S. Bureau of Education. 1930 values were interpolated using values from 1924 and 1936.

Data on manufacturing employment per capita were provided by Claudia Goldin and Lawrence Katz from their work on the high school movement.

Compulsory Schooling and Child Labor Laws

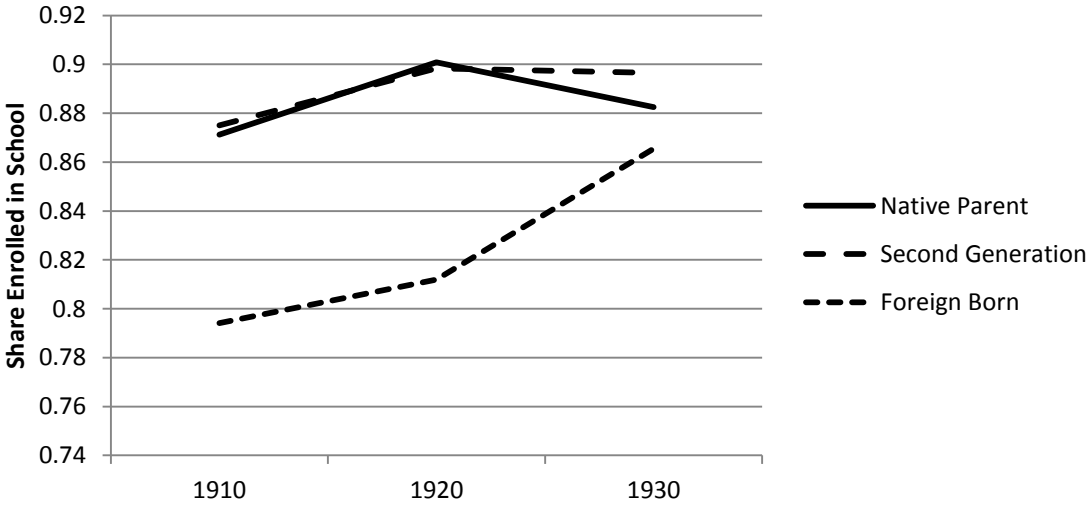
The compulsory schooling and child labor law data used in the paper were provided by Claudia Goldin and Lawrence Katz from their work on the role of state compulsion on the high school movement. See their 2008 paper for details on the classification of the entry age, work permit age, and continuation school variables.

Figure 1. Trends in Education Laws, 1910-1930



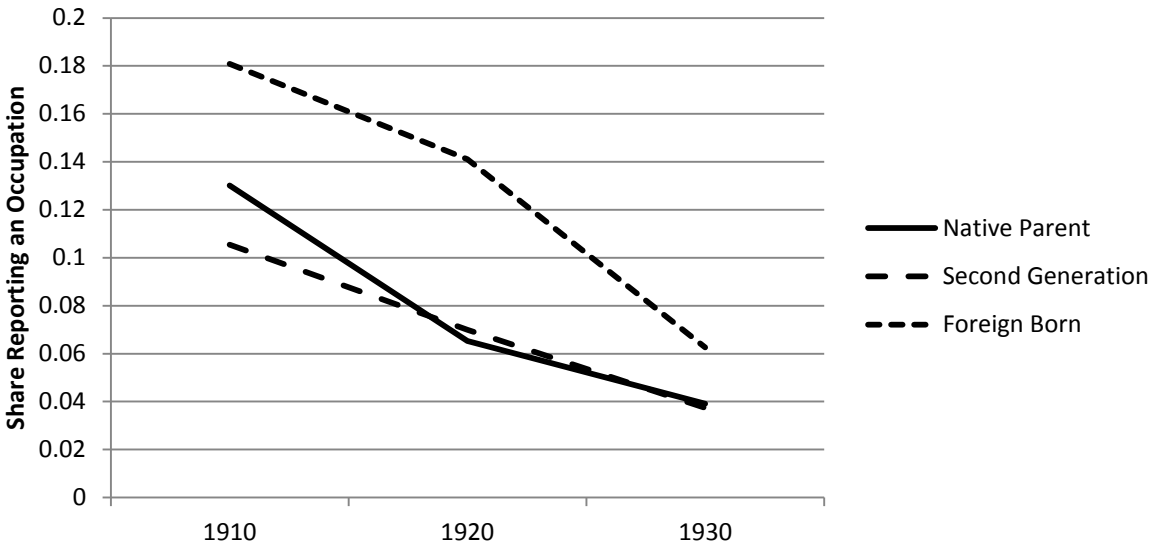
Sample includes the 48 continental states. Average entry and leaving age are given for states with such a law in place. The continuation school law and English law trends are the share of states with the respective type of law in place. See the Data Appendix for details on the sources and construction of the various law variables.

Figure 2. Percent of Children Ages 6-16 in School, 1910-1930



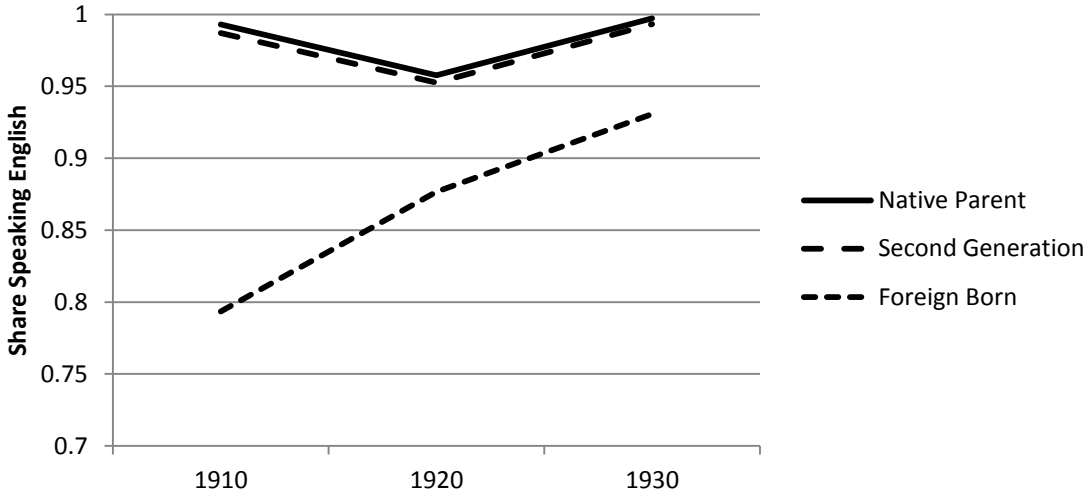
Sample includes the 48 continental states. Data come from the 1910, 1920 and 1930 IPUMS samples.

Figure 3. Percent of Children Ages 6-16 Reporting an Occupation, 1910-1930



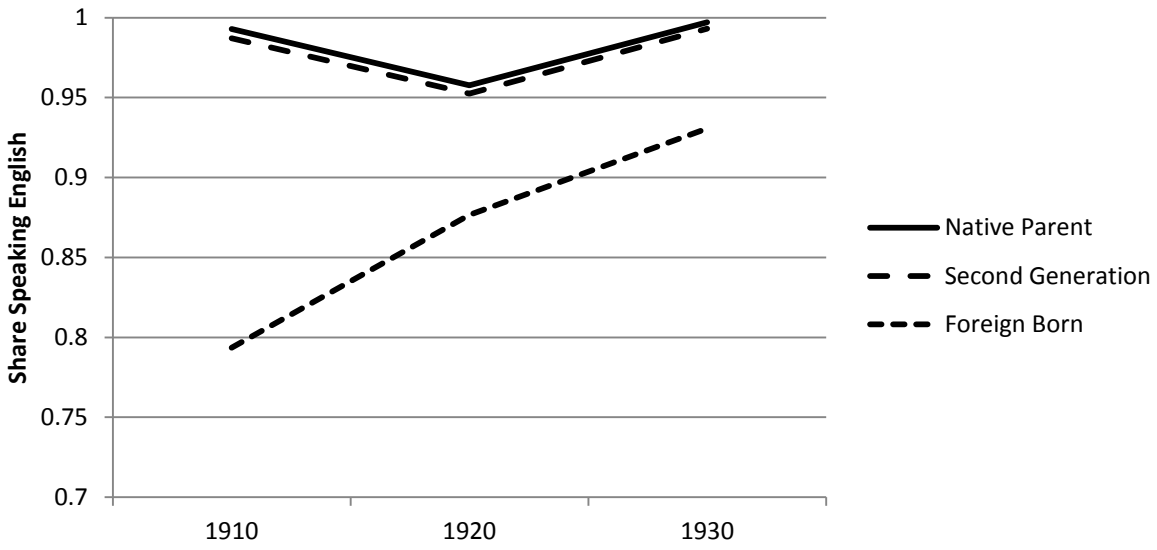
Sample includes the 48 continental states. Data come from the 1910, 1920 and 1930 IPUMS samples.

Figure 4. Percent of Children Ages 10-16 Speaking English, 1910-1930



Sample includes the 48 continental states. Data come from the 1910, 1920 and 1930 IPUMS samples.

Figure 5. Percent of Children Ages 10-16 Literate, 1910-1930



Sample includes the 48 continental states. Data come from the 1910, 1920 and 1930 IPUMS samples.

TABLE 1: PREDICTORS OF COMPULSORY SCHOOLING, CHILD LABOR, AND ENGLISH AS LANGUAGE OF INSTRUCTION LAWS 1910-1930 (probit regression with marginal effects reported)

Dependent variable:	English Law Passed after 1910		English Law in the Next Decade	
	correlates entered individually	correlates entered jointly	correlates entered individually	correlates entered jointly
Presence and level of education of foreign born				
% Foreign-Born Aged 6-16 Enrolled	-0.347	-0.182	-0.119	0.13
	(0.556)	(1.166)	(0.295)	(0.385)
% Native-Born Aged 6-16 Enrolled	-0.516	-2.161	-0.246	-1.132
	(1.937)	(2.923)	(1.050)	(1.453)
% Students Enrolled in Private Schools	-0.477	-0.669	-0.026	0.196
	(1.574)	(2.536)	(0.801)	(1.269)
% Foreign Born	1.318	2.606	0.728	1.463
	(1.356)	(2.991)	(0.700)	(1.498)
% German Descent	-0.302	-1.904	-0.208	2.739
	(1.795)	(9.737)	(0.941)	(4.623)
Demographic, political and economic determinants				
% Black	-0.072	0.41	-0.016	0.755
	(0.750)	(1.465)	(0.399)	(0.624)
% age > 65	-5.946	-9.576	-4.022	-9.54
	(6.300)	(11.646)	(3.151)	(5.755)
% age < 14	0.78	-1.688	0.488	-2.676
	(2.041)	(4.486)	(1.065)	(2.066)
% Residing in Urban Areas	-0.412	(0.614)	-0.167	-0.408
	(0.320)	-0.566	(0.166)	(0.293)
% Catholic	-0.529	-2.156	-0.078	-1.216
	(0.612)	(1.001)*	(0.294)	(0.579)*
% Republican Congressional Election Votes	0.003	0.006	0.002	0.007
	(0.006)	(0.011)	(0.003)	(0.006)
Per Capita Income	-0.003	-0.003	-0.001	-0.001
	(0.002)	(0.002)	(0.001)	(0.001)
Per Capita Manufacturing Jobs	-1.232	3.796	-0.510	2.075
	(2.673)	(4.097)	(1.352)	(2.213)

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TABLE 1, con't

Other education laws and supply of schools				
Schools/Square Mile	-0.935	0.611	-0.317	0.315
	(1.108)	(1.496)	(0.556)	(0.757)
Work Permit Age - Entry Age	0.036	0.063	0.019	0.029
	(0.020)	(0.043)	(0.012)	(0.016)
Continuation School Law			0.851	0.751
			(93.647)	(50.514)
1920 Dummy	~	~	~	-0.721
				(50.514)
Observations[+]	42	39	39	80

The sample includes all states without an existing English law for all schools in 1910. The passage of a law in any year after 1910 (and before 1930) is predicted using 1910 characteristics. The sample in second column uses 1910 and 1920 characteristics to predict the passage of an English law in the subsequent decade. See the Data Appendix for details on the construction of the predictor variables. All specifications include four census region fixed effects. * $p<0.1$

[+] Due to missing voting data at the state level, there are only 39 observations in 1910 for regressions using the Congressional voting data and 80 observations in 1910 and 1920.

TABLE 2: SUMMARY STATISTICS. INDIVIDUALS AGES 6-16, 1910, 1920, AND 1930 CENSUSES. Mean and standard deviation (in parenthesis)

	All (N=622,173)	Native born, Native parents (N=431,488)	Second Generation (N=171,200)	Foreign Born (N=19,485)
Outcome variables				
In School	0.88	0.89	0.89	0.82
	(0.32)	(0.32)	(0.31)	(0.39)
In School and Not Employed	0.85	0.84	0.87	0.78
	(0.36)	(0.37)	(0.34)	(0.42)
Employed	0.07	0.07	0.07	0.14
	(0.26)	(0.26)	(0.25)	(0.35)
Literate*	0.98	0.98	0.99	0.94
	(0.14)	(0.15)	(0.09)	(0.23)
Speaks English*	0.98	0.98	0.98	0.86
	(0.15)	(0.13)	(0.15)	(0.35)
Laws				
Should be in school by law	0.58	0.57	0.60	0.56
	(0.49)	(0.49)	(0.49)	(0.50)
Continuation school	0.40	0.35	0.52	0.42
	(0.49)	(0.48)	(0.50)	(0.49)
English law for all schools	0.47	0.43	0.55	0.51
	(0.50)	(0.50)	(0.50)	(0.50)
Individual Characteristics				
Female	0.50	0.49	0.50	0.50
	(0.50)	(0.50)	(0.50)	(0.50)
Resides in Urban Area	0.41	0.30	0.66	0.71
	(0.49)	(0.46)	(0.47)	(0.45)
Age	10.88	10.81	10.96	11.68
	(3.17)	(3.16)	(3.17)	(3.12)
Neither Parent Speaks English	0.09	0.07	0.09	0.34
	(0.28)	(0.26)	(0.29)	(0.47)
Neither Parent Literate	0.08	0.07	0.11	0.22
	(0.28)	(0.25)	(0.31)	(0.41)
Mean Year of Immigration				1912
				(8.01)

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TABLE 2, con't

State Characteristics				
State Fraction < 14 Yrs Old	0.31	0.32	0.29	0.29
	(0.04)	(0.04)	(0.03)	(0.03)
State Fraction > 65 Yrs Old	0.05	0.05	0.05	0.05
	(0.01)	(0.01)	(0.01)	(0.01)
State Foreign-Born Fraction	0.13	0.11	0.20	0.21
	(0.09)	(0.09)	(0.07)	(0.08)
State Black Fraction	0.09	0.11	0.03	0.04
	(0.13)	(0.14)	(0.05)	(0.06)
Schools per Square Mile	0.18	0.17	0.21	0.21
	(0.10)	(0.09)	(0.10)	(0.11)
State Manufacturing Per Capita Jobs	0.07	0.06	0.09	0.09
	(0.04)	(0.04)	(0.04)	(0.04)

"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. "English law for all schools" is a dummy variable equal to one if the state required instruction in both private and public schools to be in English (see Data Appendix for details). 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 or whose parents' place of birth is missing are dropped. Data on compulsory schooling laws was provided by Goldin and Katz (2008). Data on English laws were collected from state records (see Data Appendix for details). See the Data Appendix for details on the sources of the state-level characteristics. Person weights used in computing population mean and standard deviation.
* Ages 10-16 only.

TABLE 3: EFFECTS OF COMPULSORY SCHOOLING, CHILD LABOR, AND ENGLISH AS LANGUAGE OF INSTRUCTION LAWS ON IMMIGRANT CHILDREN OUTCOMES 1910-1930
(probit estimation with mean marginal effects reported)

Dependent variable:	In school=1		Employed=1		In school and Not Employed=1		Literate=1	Speaks English=1
	Ages 6-16	Ages 10-16	Ages 6-16	Ages 10-16	Ages 6-16	Ages 10-16	Ages 10-16	Ages 10-16
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Should be in school by law=1	0.05	0.072	-0.049	-0.06	0.043	0.056	0.011	0.006
	(0.015)**	(0.018)**	(0.013)**	(0.022)**	(0.014)**	(0.020)**	(0.013)	(0.012)
Continuation law=1	0.029	0.034	0.005	0.009	0.014	0.012	0.014	0.02
	(0.011)**	(0.013)**	(0.013)	(0.018)	(0.015)	(0.018)	(0.013)	(0.017)
English law for all schools=1	-0.008	-0.005	-0.006	-0.009	0.005	0.013	0.024	0.029
	(0.012)	(0.013)	(0.010)	(0.013)	(0.013)	(0.015)	(0.010)*	(0.020)
Both Parents Speak English	0.049	0.051	-0.038	-0.05	0.059	0.064	0.015	0.182
	(0.007)**	(0.010)**	(0.007)**	(0.008)**	(0.008)**	(0.009)**	(0.009)	(0.018)**
One Parent Speaks English	0.021	0.024	-0.021	-0.026	0.03	0.034	0.005	0.076
	(0.005)**	(0.008)**	(0.005)**	(0.006)**	(0.007)**	(0.008)**	(0.007)	(0.009)**
Both Parents Literate	0.03	0.031	-0.028	-0.04	0.037	0.039	0.076	-0.009
	(0.007)**	(0.008)**	(0.007)**	(0.009)**	(0.009)**	(0.010)**	(0.005)**	(0.007)
Only One Parent Literate	-0.005	-0.009	-0.015	-0.021	0.003	0	0.021	-0.022
	(0.010)	(0.013)	(0.007)*	(0.010)*	(0.010)	(0.011)	(0.005)**	(0.008)*
N	19230	13709	19123	13712	19293	13751	12281	13100
Mean of Y	0.827	0.808	0.123	0.17	0.791	0.759	0.942	0.855

The dependent variable in columns 1-2 is equal to one if the child is reported to have been in school at any time in the reference period; in columns 3-4 it is equal to one if the child reports having an occupation; in columns 5-6 it is equal to one if the child is reported to have been in school and does not report an occupation; in column 7 it is equal to one if the child is reported to be literate; in column 8 it is equal to one if the child is reported to speak English. See Table 2 for sample details and law definitions. All regressions include dummies for female, urban residence, year of immigration, single year of age, and state and year fixed effects. The omitted category for parents' literacy is either both illiterate or missing data, and similarly for parents' English ability. Standard errors are clustered at the state level (in parentheses). Person weights used in all estimations. ** $p<0.05$, * $p<0.1$

TABLE 4: EFFECT OF ENGLISH LAW ON LITERACY AND ABILITY TO SPEAK ENGLISH FOR SUBSAMPLES OF IMMIGRANT CHILDREN AGED 10-16, 1910-1930 (probit regression with marginal effects reported)

Dependent variable:	Literate=1				Speaks English=1			
Sample:	No State Controls	With State Controls	N	Mean Literacy	No State Controls	With State Controls	N	Mean English
All foreign-born ages 10-16 with controls+	0.024	0.025	12281	0.942	0.029	0.008	13100	0.855
	(0.010)*	(0.011)*			(0.020)	(0.019)		
All foreign-born ages 10-16	0.027	0.029	12299	0.942	0.042	0.02	13120	0.855
	(0.009)**	(0.012)*			(0.022)	(0.020)		
Non-English Speaking Country of Origin	0.033	0.032	9747	0.928	0.044	0.013	10527	0.827
	(0.010)**	(0.014)*			(0.027)	(0.026)		
and Immigrated Less than Two Years Ago	0.19	0.327	1203	0.84	0.056	-0.154	1397	0.551
	(0.083)*	(0.096)**			(0.072)	(0.101)		
and Immigrated Less than Five Years Ago	0.051	0.025	3310	0.893	0.048	-0.009	3676	0.703
	(0.027)	(0.036)			(0.042)	(0.050)		
and Neither Parent Literate	0.073	0.075	5210	0.875	0.059	0.008	5454	0.764
	(0.018)**	(0.024)**			(0.043)	(0.036)		
and Neither Parent Speaks English	0.035	0.056	6350	0.896	0.046	-0.005	6897	0.742
	(0.017)*	(0.025)*			(0.038)	(0.038)		

+ The first row reports the main effect of the English law (equal to one if the state required instruction in both private and public schools to be in English) from Table 3 (controls for parents' literacy, parents' English-speaking ability, and the child's year of immigration). All regressions control for whether the child should have been in school given the state's compulsory schooling law and the presence of a continuation school law. They also include dummies for female, urban residence, single year of age, and state and year fixed effects. The dependent variable in the first column is equal to one if the child is reported to be literate; in column two it is equal to one if the child is reported to speak English. Sample includes non-black foreign-born individuals ages 10-16 living in the 48 states (excluding Alaska, Hawaii and Washington DC) in the 1910, 1920, and 1930 censuses. Children whose place of residence or whose parents' place of birth is missing are dropped. Standard errors are clustered at the state level (in parentheses). Person weights used in all estimations. ** $p<0.05$, * $p<0.1$

TABLE 5: EFFECTS OF EDUCATION LAWS ON THE ENROLLMENT OF NATIVE AND IMMIGRANT CHILDREN
1910-1930 (probit regression with marginal effects reported)

Dependent variable:	In School=1	Not Employed=1
	(1)	(2)
Should be in school by law=1	0.021	-0.03
	(0.005)***	(0.004)***
Should be in school * Foreign born	0.016	0.002
	(0.012)	(0.009)
Should be in school * Second gen.	0.006	-0.019
	(0.012)	(0.015)
Continuation law=1	-0.002	0.005
	(0.005)	(0.004)
Continuation law * Foreign born	0.022	0.001
	(0.009)*	(0.006)
Continuation law * Second gen.	0.022	-0.004
	(0.007)**	(0.006)
English law for all schools=1	0.008	-0.005
	(0.006)	(0.003)
English law * Foreign born	0.005	-0.005
	(0.007)	(0.005)
English law * Second gen.	0.002	-0.003
	(0.005)	(0.003)
Foreign born	0.055	-0.003
	(0.014)***	(0.022)
Second generation	0.027	-0.036
	(0.018)	(0.014)**
N	622102	622107
P value (Joint Significance of Laws on Foreign-Born)	0.001	0.714
P-value (Joint Significance of Laws on Second-Generation)	0.014	0.356
P-value Joint Significance of English Laws and English Law Interactions	0.372	0.15
Mean of Y	0.885	0.885

The dependent variable in column 1 is equal to one if the child is reported to be have been in school at any time during in reference period. The dependent variable in column 2 is equal to one if the child reports an occupation. See Table 2 for sample details and law definitions. All regressions include a dummy for female, a dummy for urban, dummies for each single year of age, and group x female, group x urban, and group x year of age interactions, as well as state and year fixed effects. Standard errors are clustered at the state level (in parentheses). Person weights used in all estimations. ** $p<0.05$, * $p<0.1$

TABLE 6: EFFECTS OF COMPULSORY SCHOOLING AND CHILD LABOR LAWS ON ENROLLMENT, 1910-1940. ROBUSTNESS CHECKS (probit regression with marginal effects reported)

Dep variable: Enrolled=1 (mean: 0.88)	All States			With state-level controls		
	(1)	(2)	(3)	(4)	(5)	(6)
Should be in school by law=1	0.036	0.035	0.034	0.035	0.035	0.034
	(0.006)**	(0.006)**	(0.006)**	(0.006)**	(0.006)**	(0.005)**
Should be in school * Foreign born	0.03	0.032	0.033	0.032	0.033	0.034
	(0.013)*	(0.013)*	(0.012)**	(0.013)*	(0.013)**	(0.012)**
Should be in school * Second gen.	0.008	0.009	0.01	0.009	0.01	0.01
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Continuation law=1	-0.011	-0.011	-0.01	-0.007	-0.005	-0.009
	(0.006)	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)
Continuation law * Foreign born	0.026	0.028	0.027	0.028	0.027	0.025
	(0.009)**	(0.010)**	(0.010)**	(0.010)**	(0.009)**	(0.009)**
Continuation law * Second gen.	0.024	0.026	0.025	0.026	0.023	0.025
	(0.008)**	(0.009)**	(0.008)**	(0.009)**	(0.008)**	(0.007)**
Foreign born	0.064	0.06	0.06	0.061	0.059	0.06
	(0.018)**	(0.017)**	(0.017)**	(0.018)**	(0.017)**	(0.017)**
Second generation	0.081	0.071	0.071	0.07	0.068	0.069
	(0.015)**	(0.013)**	(0.013)**	(0.014)**	(0.013)**	(0.013)**
Census Region-Specific Trends	No	Yes	No	No	Yes	No
State-Specific Trends	No	No	Yes	No	No	Yes
P-value (Joint Sig of Laws on Foreign-Born)	0.000	0.000	0.000	0.000	0.000	0.000
P-value (Joint Significance of Laws on 2nd Gen)	0.012	0.009	0.012	0.011	0.015	0.002

N=857,483. Sample includes non-black individuals ages 6-16 living in the 48 states (excluding Alaska, Hawaii and Washington DC) in the 1910, 1920, 1930 and 1940 censuses, with no missing values for place of residence or parents' place of birth. 1939 laws are used for 1940. All regressions include dummies for female, urban, single year of age, and group x female, group x urban, and group x year of age interactions, as well as state and year fixed effects. Standard errors are clustered at the state-group level (in parentheses). The state-specific characteristics included in Panels C and D are the state characteristics listed in Table 2. Person weights used in all estimations. ** $p<0.05$, * $p<0.1$

Appendix I: Compulsory Schooling Laws in the United States, 1910-1939
(entry age to work permit eligibility age with continuation school in parentheses in A columns, English law in parentheses in B columns)

State	1910		1920		1930		1939
	A	B	A	B	A	B	A
Alabama	.-., (no)	no	8-14, (no)	yes	8-14, (no)	yes	7-14, (no)
Arizona	8-14, (no)	no	8-14, (yes)	yes	8-14, (yes)	yes	8-14, (yes)
Arkansas	8-14, (no)	no	7-14, (no)	yes	7-14, (no)	yes	7-14, (no)
California	8-14, (no)	yes	8-14, (yes)	yes	8-15, (yes)	yes	8-15, (yes)
Colorado	8-14, (no)	no	8-14, (no)	yes	8-14, (no)	yes	8-14, (no)
Connecticut	7-14, (no)	no	7-14, (yes)	no	7-14, (yes)	yes	7-16, (yes)
Delaware	7-14, (no)	no	7-14, (no)	yes	7-14, (yes)	yes	7-14, (yes)
Florida	.-., (no)	no	8-12, (no)	no	7-14, (yes)	no	7-14, (yes)
Georgia	.-., (no)	no	8-12, (no)	no	8-14, (no)	no	7-14, (no)
Idaho	8-14, (no)	no	7-14, (no)	yes	8-14, (no)	yes	7-14, (no)
Illinois	7-14, (no)	no	7-14, (no)	yes	7-14, (yes)	yes	7-14, (yes)
Indiana	7-14, (no)	no	7-14, (no)	yes	7-14, (no)	yes	7-14, (no)
Iowa	7-14, (no)	yes	7-14, (no)	yes	7-14, (yes)	yes	7-14, (yes)
Kansas	8-14, (no)	no	8-14, (no)	yes	7-14, (no)	yes	7-14, (no)
Kentucky	7-14, (no)	no	7-14, (no)	no	7-14, (no)	no	7-14, (no)
Louisiana	.-14, (no)	no	7-14, (no)	yes	7-14, (no)	yes	7-14, (yes)
Maine	7-14, (no)	no	7-14, (no)	no	7-15, (no)	no	7-15, (no)
Maryland	8-12, (no)	no	7-13, (no)	no	7-14, (no)	no	7-14, (no)
Massachusetts	7-14, (no)	no	7-14, (yes)	no	7-14, (yes)	no	7-16, (yes)
Michigan	7-14, (no)	no	7-15, (no)	no	7-15, (yes)	no	7-15, (yes)
Minnesota	8-14, (no)	no	8-14, (no)	no	8-14, (no)	no	8-14, (no)
Mississippi	.-., (no)	no	7-12, (no)	no	7-14, (no)	no	7-14, (no)
Missouri	8-14, (no)	no	8-14, (yes)	no	7-14, (yes)	no	7-14, (yes)
Montana	8-14, (no)	no	8-14, (yes)	yes	8-16, (yes)	yes	8-16, (yes)
Nebraska	7-14, (no)	no	7-14, (yes)	yes	7-14, (yes)	yes	7-14, (yes)

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Appendix Table I,
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Nevada	8-14, (no)	no	8-14, (yes)	yes	7-14, (yes)	yes	7-14, (yes)
New Hampshire	8-14, (no)	no	8-14, (no)	yes	8-14, (no)	yes	8-14, (no)
New Jersey	7-14, (no)	no	7-14, (yes)	no	7-14, (yes)	no	7-14, (yes)
New Mexico	7-., (no)	yes	7-., (yes)	no	6-14, (yes)	no	6-14, (yes)
New York	7-14, (no)	yes	8-14, (yes)	yes	7-14, (yes)	yes	7-16, (yes)
North Carolina	8-12, (no)	no	8-14, (no)	no	7-14, (no)	no	7-16, (no)
North Dakota	8-14, (no)	no	7-14, (no)	yes	7-14, (no)	yes	7-14, (no)
Ohio	8-14, (no)	no	8-15, (no)	yes	6-16, (yes)	yes	6-16, (yes)
Oklahoma	8-14, (no)	no	8-16, (yes)	yes	8-14, (yes)	yes	7-16, (yes)
Oregon	9-14, (no)	no	9-14, (yes)	no	7-14, (yes)	no	8-14, (yes)
Pennsylvania	8-14, (no)	no	8-14, (yes)	yes	8-14, (yes)	yes	8-16, (yes)
Rhode Island	7-14, (no)	yes	8-14, (no)	yes	7-15, (no)	yes	7-16, (no)
South Carolina	.-12, (no)	no	8-14, (no)	yes	8-14, (no)	yes	7-16, (no)
South Dakota	8-14, (no)	no	8-15, (no)	yes	8-14, (no)	yes	8-14, (no)
Tennessee	8-14, (no)	no	8-14, (no)	no	7-14, (yes)	no	8-14, (yes)
Texas	.-., (no)	no	8-12, (no)	no	8-15, (no)	no	7-15, (no)
Utah	8-., (no)	no	7-14, (yes)	no	8-14, (yes)	no	8-16, (yes)
Vermont	8-12, (no)	no	8-15, (no)	no	8-14, (no)	no	8-14, (no)
Virginia	8-12, (no)	no	8-14, (no)	no	7-14, (no)	no	7-14, (no)
Washington	8-14, (no)	no	8-14, (yes)	no	8-14, (yes)	no	8-14, (yes)
West Virginia	8-12, (no)	no	8-14, (no)	no	7-14, (yes)	no	7-16, (yes)
Wisconsin	7-12, (no)	yes	7-14, (yes)	yes	7-14, (yes)	yes	7-16, (yes)
Wyoming	7-., (no)	no	7-., (no)	no	7-14, (no)	no	7-14, (no)

Notes: Data on compulsory schooling laws was provided by Goldin and Katz (2008). Data on English laws were collected from state records (see Data Appendix for details).

APPENDIX TABLE II: EFFECTS OF COMPULSORY SCHOOLING, CHILD LABOR, AND ENGLISH AS LANGUAGE OF INSTRUCTION LAWS ON IMMIGRANT CHILDREN OUTCOMES 1910-1930 (probit regression with mean marginal effects reported)

Dependent variable:	In school=1		Employed and Not in School=1		In school and Not Employed=1		Literate=1	Speaks English=1
	Ages 6-16	Ages 10-16	Ages 6-16	Ages 10-16	Ages 6-16	Ages 10-16	Ages 10-16	Ages 10-16
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Table 3 results reproduced								
Should be in school by law=1	0.05	0.072	-0.049	-0.06	0.043	0.056	0.011	0.006
	(0.015)***	(0.018)***	(0.013)***	(0.022)**	(0.014)**	(0.020)**	(0.013)	(0.012)
Continuation law=1	0.029	0.034	0.005	0.009	0.014	0.012	0.014	0.02
	(0.011)**	(0.013)**	(0.013)	(0.018)	(0.015)	(0.018)	(0.013)	(0.017)
English law for all schools=1	-0.008	-0.005	-0.006	-0.009	0.005	0.013	0.024	0.029
	(0.012)	(0.013)	(0.010)	(0.013)	(0.013)	(0.015)	(0.010)*	(0.020)
Panel A: English law targets public schools only, not all schools								
Should be in school by law=1	0.052	0.074	-0.048	-0.058	0.043	0.055	0.01	0.003
	(0.016)***	(0.018)***	(0.013)***	(0.021)**	(0.014)**	(0.019)**	(0.014)	(0.012)
Continuation law=1	0.028	0.033	0.006	0.01	0.012	0.009	0.011	0.019
	(0.010)**	(0.012)**	(0.013)	(0.018)	(0.015)	(0.018)	(0.014)	(0.020)
English law for Public schools=1	-0.016	-0.015	0	-0.005	-0.006	0.000	-0.005	0.025
	(0.011)	(0.013)	(0.010)	(0.013)	(0.015)	(0.017)	(0.015)	(0.022)
Mean of Y	0.827	0.808	0.123	0.17	0.791	0.759	0.942	0.855
Panel B: English Law alone (without other education laws)								
English law for All schools=1	-0.014	-0.013	-0.006	-0.009	0.001	0.009	0.023	0.025
	(0.014)	(0.015)	(0.010)	(0.014)	(0.014)	(0.015)	(0.010)*	(0.022)
Mean of Y	0.827	0.808	0.123	0.17	0.791	0.759	0.942	0.855

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Table Appendix II, con't

Panel C: Results for second generation children only								
Should be in school by law=1	0.02	0.038	-0.048	-0.072	0.034	0.066	0	0
	(0.009)*	(0.016)*	(0.014)***	(0.025)**	(0.013)*	(0.023)**	(0.001)	(0.004)
Continuation law=1	0.021	0.031	0.001	-0.001	0.009	0.014	-0.002	-0.01
	(0.005)***	(0.008)***	(0.008)	(0.012)	(0.007)	(0.008)	(0.002)	(0.005)
English law for all schools=1	0.004	0.007	-0.004	-0.008	0.003	0.007	0	-0.001
	(0.005)	(0.009)	(0.004)	(0.006)	(0.005)	(0.007)	(0.002)	(0.006)
Mean of Y	0.892	0.882	0.066	0.103	0.87	0.847	0.991	0.978
Panel D: Second-Generation children with mothers having later than median year of arrival								
Should be in school by law=1	0.02	0.049	-0.039	-0.069	0.032	0.074	0.001	-0.002
	(0.008)*	(0.016)**	(0.009)***	(0.019)***	(0.012)**	(0.022)***	(0.002)	(0.004)
Continuation law=1	0.022	0.035	-0.005	-0.013	0.016	0.025	0.000	-0.009
	(0.007)***	(0.008)***	(0.005)	(0.009)	(0.008)*	(0.008)***	(0.002)	(0.006)
English law for All schools=1	-0.002	0.002	-0.002	-0.005	-0.004	0.000	-0.001	0.003
	(0.005)	(0.008)	(0.004)	(0.007)	(0.005)	(0.007)	(0.002)	(0.006)
Mean of Y	0.892	0.882	0.066	0.103	0.87	0.847	0.991	0.978

Notes: See Table 3 for specification details and variable definitions. Standard errors are clustered at the state level (in parentheses). Person weights used in all estimations. *** p<0.01, ** p<0.05, * p<0.1

APPENDIX TABLE III: THE IMPACT OF LAWS ON IMMIGRANTS BASED ON STATE OF RESIDENCE OR STATE OF BIRTH MATCHING (OLS REGRESSION)

Dependent variable: years of schooling	All Native-Born Individuals Born and Living in Cont. U.S.		Not Living in State of Birth		Living in State of Birth
	(1)	(2)	(3)	(4)	(5)
CSL Years in State of Birth		0.029*		0.059***	
		(0.017)		(0.016)	
Continuation School Law in State of Birth		0.112		0.221***	
		(0.091)		(0.065)	
CSL Years in State of Res.	-0.007		-0.026		-0.001
	(0.015)		(0.019)		(0.020)
Continuation School Law in State of Res.	0.021		0.127		-0.019
	(0.062)		(0.098)		(0.120)
Female	0.281***	0.282***	0.177***	0.179***	0.308***
	(0.051)	(0.051)	(0.044)	(0.045)	(0.058)
Constant	7.581***	7.357***	8.281***	7.688***	7.497***
	(0.095)	(0.103)	(0.119)	(0.096)	(0.124)
Observations	364759	364759	78401	78401	286358
R-squared	0.072	0.072	0.036	0.038	0.087

Notes: Standard errors are clustered at state level. All regressions include year of birth or state of residence FE. Individuals are matched to entry age when they were six and age to obtain a work permit and presence of a continuation school law when they were 14.