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Is Spanish-only schooling responsible for the Puerto Rican language gap?

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Abstract

Between 1898 and 1948, English was the language of instruction for most post-primary grades in Puerto Rican public schools. Since 1949, the language of instruction in all grades has been Spanish. We use these shifts in language of instruction policy to estimate the effect of English-intensive instruction on the English-language skills of Puerto Ricans. Although naïve estimates suggest that English instruction increased English-speaking ability among Puerto Rican natives, estimates that allow for education-specific cohort trends show no effect. This result is surprising in light of the strong presumption by American policymakers at the time that instruction in English is the best way to raise English proficiency. This has implications for school language policies in former colonies as well as for U.S. education policy toward immigrant children.

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On the morning of January 2, 1949, the first elected governor of Puerto Rico took office. In the afternoon, he appointed Mariano Villaronga as the Commissioner of Education. Villaronga had been appointed to the same post by President Truman in December 1946, but resigned in June 1947 because the U.S. Senate refused to confirm his appointment. The Senate had stalled Villaronga's confirmation indefinitely because he had said that if confirmed, he would make Spanish the medium of instruction in Puerto Rican public schools. Philleo Nash, an advisor to President Truman on issues related to U.S. territories, recalled that "all previous incumbents [in the

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Commissioner of Education post] had had a condition set on them that they would have English be the language in the schools, or they wouldn't get confirmed by the United States Senate. The Senate was standing firm, at least the Senate committee [on Territories and Insular Possessions] was, and was refusing to approve Villaronga" (Hess, 1966, p. 320). Upon returning as Commissioner of Education in 1949, Villaronga made Spanish the language of instruction in all grades in public schools, with English taught as a subject. The Villaronga policy remains in effect today.

The 1949 language reform ended half a century of English instruction in at least some Puerto Rican grades. In this paper, we use shifts in language of instruction policy between 1930 and 1949, and especially the dramatic 1949 reform, to gauge the importance of English-intensive instruction for Puerto Ricans' ability to speak English. Variation in years of exposure to English-intensive instruction arises from differences in the timing and amount of schooling. Among individuals growing up in Puerto Rico, cohorts educated entirely in the post-reform period were taught in Spanish while earlier cohorts could have received some English-intensive instruction. At the same time, these policy shifts changed the language of instruction only for those completing five or more years of schooling because lower grades were taught in Spanish even before the 1949 reform. Use of interactions between year of birth and years of schooling to capture language policy effects leads to a difference-in-differences identification strategy across cohort and schooling groups. As it turns out, however, a simple differences-in-differences approach is confounded by strong education-specific cohort trends in Puerto Rican English-ability. We therefore focus on a triple differences identification strategy using later cohorts of Puerto Ricans (who never experienced a change in language of instruction) to adjust for differential trends.

Our statistical analysis exploits the fact that the U.S. Census covers Puerto Rico as well as the U.S. mainland. Thus, we can analyze samples of island-born individuals regardless of where they chose to live or when or how often they moved to the mainland. In particular, we use data from the 1980 and 1990 U.S. Census of Population and Housing Public Use Microdata Samples (PUMS) for Puerto Rico and the mainland.

As far as we know, ours is the first rigorous evaluation of the 1949 language reform.[1] An assessment of the consequences of this reform should be of interest for a number of reasons. First, some observers see the 1949 reform as contributing to relatively low levels of English proficiency among Puerto Ricans today, and favor bringing back English-language instruction in some grades and subjects in order to raise English proficiency (see, e.g., Barreto, 1998). Cohort data on the English proficiency of the Puerto Rican-born provide some support for this view. Fig. 1, which plots cohort trends in English proficiency as observed in the 1980 and 1990 PUMS, shows a continuous increase in English proficiency that flattens with the last cohort instructed in English (born 1933). Among cohorts born 1934 and later, there is a persistent "language gap," in that one-third of these cohorts do not speak English at all. Since the language gap stopped narrowing after Spanish-only schooling was introduced, it is natural to ask whether the policies regarding language of instruction explain this.[2]

[1] Osuna (1949) and Pousada (1999) describe early studies of the effectiveness of English instruction in Puerto Rico. The general finding is that Puerto Rican students were less proficient in English than the evaluators thought they should be, or compared with students on the mainland. These studies do not address the counterfactual question of what English proficiency would have been without the English-intensive instruction then in use.

[2] Not all of these cohort differences can be attributed to the language reform, as evidenced by the fact that English proficiency follows the same pattern (flattening after the 1933 cohort) for those with 0–4 years of schooling.

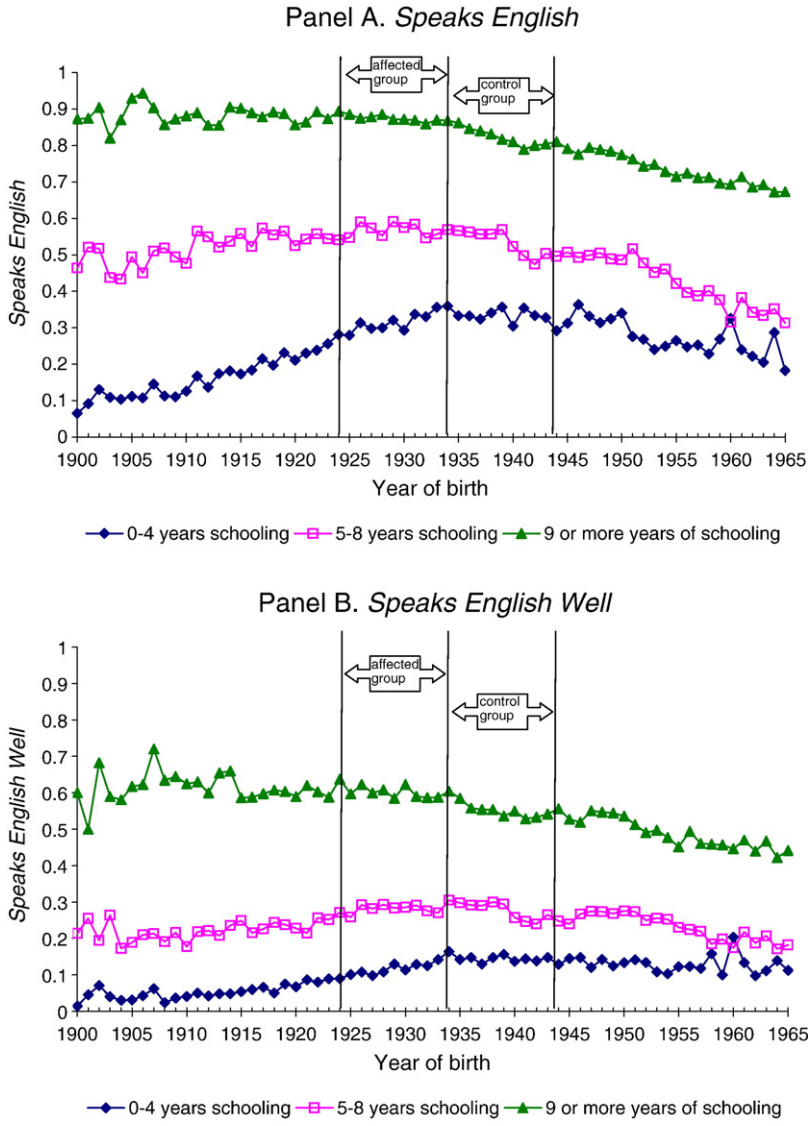

Fig. 1. English-speaking ability by year of birth and educational attainment for the Puerto Rican-born. The sample includes 237,297 Puerto Rican-born men and women aged 25-80 from the 1980 and 1990 PUMS files for Puerto Rico and the mainland.

In addition to the implications of language reform for Puerto Ricans themselves, the Puerto Rican experience should also be of more general interest. Many former European and American colonies have struggled with language policy (see, e.g., Tollefson and Tsui, 2004; United Nations Development Programme, 2004). Some former colonies have opted for native-language instruction in public schools (e.g., Morocco, Malaysia, Pakistan, and India) while others have continued with the colonial language (e.g., much of sub-Saharan Africa and the Philippines). On one hand, native-language instruction might reinforce national identity and make schooling more accessible. On the other hand, since top jobs in government and business often continue to use the

colonial language, native-language instruction might reduce economic opportunities for the poor (see, e.g., Angrist and Lavy, 1997; Munshi and Rosenzweig, 2005).

The Puerto Rican experience may also be relevant for contemporary U.S. education policy. The proper extent and timing of English-only instruction for non-native English speakers remains highly controversial. Over 8% of students enrolled in U.S. public schools are classified as limited-English-proficient (LEP), of whom three-quarters are Hispanic.[3] From 1980 to 1999, enrollment of LEP students doubled but total enrollment grew by only 25%. Recent years have seen a move away from bilingual instruction for LEP students towards English-only instruction and a "sink or swim" approach (Zehler et al., 2003). Although a large literature attempts to evaluate programs for LEP students, few studies address the endogeneity of program participation or other sources of omitted variables bias.[4] The variation in exposure to English-intensive instruction used in this paper arises from sharp shifts in language of instruction policy, thereby facilitating evaluation.

The paper is organized as follows. Section 1 describes Puerto Rico's language of instruction policies and outlines our identification strategy. Section 2 describes the data sources and presents some descriptive statistics. Section 3 discusses the empirical results. Section 4 concludes.

1. Background and identification strategy

1.1. Background

After 400 years as a Spanish colony, Puerto Rico became an American possession in 1898 as a result of the Treaty of Paris which ended the Spanish-American War. The U.S. took an active role in the island's administration, particularly in education.[5] One American goal was to expand the public school system. Under Spanish rule, educational opportunities had, for the most part, been reserved for the elite. A second goal was to teach Puerto Ricans English as part of a process of Americanization.

The American administration set up a U.S.-style school system providing free education through 12th grade.[6] Schooling was compulsory for those aged 8–14, though in practice the compulsory schooling law was of little consequence since many rural communities had no school offering grades beyond 4th. To increase access, spending on public education was increased from half a million to 21.4 million dollars between 1900 and 1948, while the number of public school teachers increased from 897 to 9101 (Osuna, 1949, p. 607, Table 2). These efforts generated sizable gains in educational attainment. Individuals born 1914–1923 had an average of 6.4 years of schooling, but those born 10, 20 and 30 years later had 7.9, 9.3 and 10.7 years of schooling, respectively. Much of the increase in attainment came from a shift in the distribution of years of schooling from four or fewer to more than four years. This can be

[3] Zehler et al. (2003), using data provided by school districts, estimate that there were 4.0 million LEP students in grades K-12 in U.S. public schools in the 2001-02 school year. Different school districts have different standards for classifying a student as LEP, but all LEP students are deemed to have inadequate English skills.

[4] See, for example, Baker and de Kanter (1981), Willig (1985), Rossell and Baker (1996) and Greene (1998) for reviews. An exception is Matsudaira (2004), who uses a regression-discontinuity design to estimate the impact of participating in bilingual education and English as a Second Language (ESL) programs compared to a mainstream, English-only classroom. Matsudaira finds no effect of bilingual and ESL program participation on math scores, and weak positive effects on reading scores.

[5] This subsection provides only a brief description of education in Puerto Rico. For more detail, see Osuna (1949) and Solís (1994).

[6] Elementary education consisted of four years of primary and four years of middle school. Beginning with the 1941–1942 school year, Puerto Rico switched to a 6-year elementary school+3-year junior high school+3-year senior high school system, mirroring changes in the U.S. mainland.

seen in Fig. 2, which plots the cumulative distribution of educational attainment for the Puerto Rican-born population by cohort. Forty-two percent of those born 1914–1923 had zero to four years of schooling, compared with 29% of those born 1924–1933, 16% of those born 1934–1943, and 8% of those born 1944–1953.

The effort to increase English proficiency proved to be at least as much of a challenge as increasing access to public education. One difficulty was the lack of consensus over the appropriate pedagogical method for achieving this goal. Some educators favored the use of English as the only language of instruction in all grades, but others favored Spanish in the early grades and English in later grades. Between 1898 and 1949, language policy changed several times, reflecting the views of different Commissioners of Education. The last change – the 1949 reform described in the introduction – completely eliminated English instruction whereas earlier changes merely changed which grades received English instruction. These policy shifts are summarized in Table 1. In this paper, we focus on cohorts born 1924–1943 because they straddle the 1949 reform, containing affected cohorts (born 1924–1933) who could have received English-intensive instruction and control cohorts (1934–1943) who would have been just too young to receive it. For these cohorts, the main source of variation in exposure to English-intensive instruction is the 1949 reform, although some variation arises from the policy changes from 1930 to 1948.

Importantly for our analysis, and in spite of the relative scarcity of English-speaking teachers, most students in pre-reform cohorts indeed appear to have studied in schools that complied with the English instruction policy. Describing high schools around 1920, Osuna (1949) observes: "With the exception of Spanish, which was taught as a subject, the official language of the high

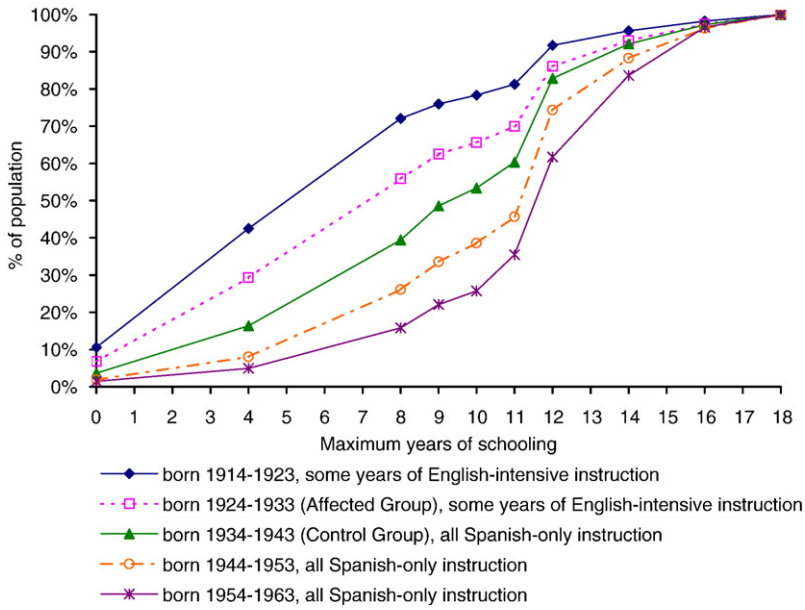

Fig. 2. Puerto Rican education distribution by cohorts. The sample includes Puerto Rican-born men and women from the 1980 and 1990 PUMS files for Puerto Rico and the mainland. Each point represents the fraction of the total number of people in that cohort at or below the indicated schooling level. The following educational attainment categories are used: 0=no schooling, 4=1-4 grades, 8=5-8 grades, 9= 9 grades, 10=10 grades, 11=11 grades or 12 grades but no diploma, 12=high school graduate or GED, 14=some college, 16=college degree and 18=more than college degree. Data for the 1954-63 cohort came from the 1990 Census only.

Table 1
Language of instruction policies in Puerto Rican public schools

Years	Policy
1493–1898	Puerto Rico was a Spanish colony throughout the period. Spanish was the medium of instruction.
1898–1900	In 1898, Puerto Rico became a U.S. territory. 1898–1900 was a transitional period in which Puerto Rico was run by military government. The official policy was English instruction, but little changed from the Spanish period.
1900–1905	Brumbaugh policy: Spanish instruction in elementary school (grades 1–8) and English in secondary school (grades 9–12).
1905–1916	Falkner policy: English instruction in all grades.
1916–1934	Miller policy: Spanish instruction in grades 1–4, half Spanish and half English in grade 5, and English in grades 6–12.
1934–1937	Padín policy: Spanish instruction in elementary school (grades 1–8) and English in secondary school (grades 9–12).
1937–1942	Gallardo policy: Spanish instruction in grades 1–2, both Spanish and English in grades 3–8 with progressive increase in English, and English in grades 9–12.
1942–1945	Revert to Padín policy: Spanish instruction in elementary school (now grades 1–6) and English in secondary school (now grades 7–12).
1945–1949	No official policy change but a gradual transition to Spanish instruction in all grades.
1949–present	Villaronga policy: Spanish instruction in all grades.

Sources were Osuna (1949) and Cafferty and Rivera-Martínez (1981). Policy names refer to Commissioners of Education. A given calendar year may have two different policies since the school year begins with the fall semester and ends with the spring semester.

school was English" (p. 248). Similarly, the *Report of the Commissioner of Education* for the 1947–1948 school year states that English "is the medium of instruction in the senior high school in all classes except the Spanish class and the class in Puerto Rican history"(Puerto Rico Department of Education, 1948, p. 25). The report for the 1948–1949 school year, halfway through which Villaronga began serving as the Commissioner of Education, mentions plans for "the introduction of Spanish as the medium of instruction…in the senior high schools in the following year" (Puerto Rico Department of Education, 1949, p. 24).

In earlier grades, English instruction was nearly complete in urban areas since English training was an enforced prerequisite for teaching positions in urban elementary schools.[7] Moreover, even if English-intensive instruction had never been used in the countryside, compliance rates would have remained high since most post-primary enrollment was in cities.[8] For example, in 1940, 57% of 5th graders, 77% of 8th graders and 100% of 10th graders were attending schools in urban

[7] The *Report of the Commissioner of Education* for the 1926–1927 school year notes: "The preparation now required for an elementary urban-school teacher is a 2-year normal course after graduation from high school. These teachers hold an English graded license issued by the Department of Education, which is also attainable through free examination and University summer courses by experienced rural teachers who have attained a high standing in the profession" (Puerto Rico Department of Education, 1927, pp. 14–15).

[8] In the 1911–1912 school year 98% of 771 urban elementary schools and 17% of 1097 rural elementary schools used English (Osuna, 1949, p. 346). The cohorts we will be analyzing would have attended school at least two decades later, but this information for the 1911–1912 school year supports the idea that even at an earlier stage of the Puerto Rican school system's development, high compliance was achieved in urban areas. Various issues of the *Report of the Commissioner of Education* provide evidence that English continued to be used as required in urban schools. The reports for the 1919–1920 and 1930–1931 school years note that English was used to teach arithmetic in grades five and up. In the 1941–1942 report, which describes changes in curricula in concert with the shift to a 6-3-3 school system, English instruction is mentioned for urban junior high schools and grade 9 of rural junior high schools. The 1947–1948 report mentions the end of English instruction in junior high, beginning with the 1948–1949 school year.

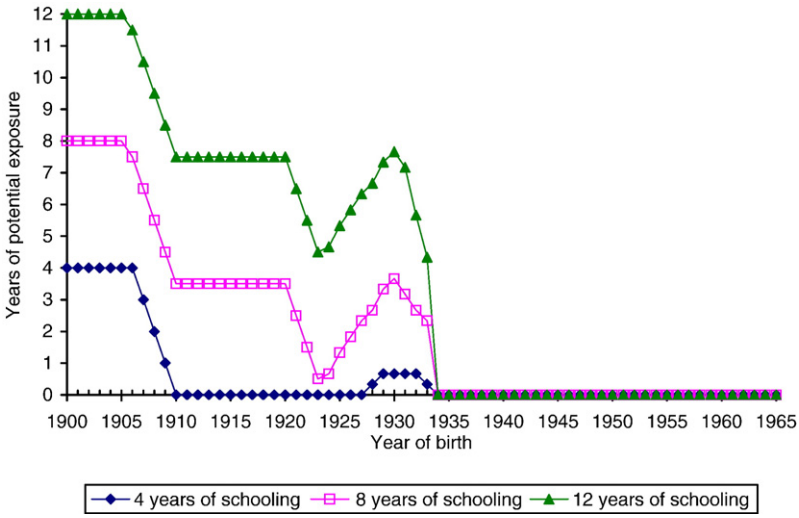

Fig. 3. Potential exposure to English-intensive instruction in Puerto Rican public schools by year of birth for three illustrative levels of educational attainment. The diamond-marker, square-marker and triangle-marker lines display the years of exposure to English-intensive instruction by year of birth for people with exactly 4, 8 and 12 years of schooling, respectively, assuming they began first grade at age 6 and were promoted yearly up through the last grade completed. The years of exposure for these people, as well as for people with other levels of educational attainment, are provided in the appendix (Table A1). Variation from cohort to cohort comes from the policy shifts described in Table 1.

areas.[9] Thus, the English-intensive instruction called for by official language of instruction policies from 1930 to 1949 was widely delivered.[10]

1.2. Identification strategy

Exposure to English-intensive instruction varies across cohorts and schooling levels. This variation can be exploited using a difference-in-differences strategy as in Angrist and Lavy's (1997) study of the effects of a change in language of instruction from French to Arabic in Morocco. In the context of a rapidly expanding school system, however, education-specific cohort trends are likely to bias the resulting estimates. For example, high school graduates from recent cohorts may be less able than high school graduates from earlier cohorts, when access to education was more restricted. We therefore employ a triple-differences strategy that uses younger cohorts to remove cohort trends that differ by schooling group.

Our empirical work focuses on the 1924–1943 birth cohorts, who would have started grade 1 as early as 1930 and started grade 12 as late as 1961, assuming school entry at age 6 and annual

[9] Authors' calculations based on enrollment data by year, grade and urban/rural status from Osuna (1949), pp. 624–625, Appendix VIII, Table 1. In 1930, schools offering post-primary grades were less prevalent in rural areas and thus the percentage of students attending schools in urban areas was even higher: 66% of 5th graders, 93% of 8th graders and 100% of 10th graders. Even by the 1951–1952 school year (the last year for which we have the commissioner's annual report), there were no public senior high schools in rural areas.

[10] We ignore private schools, which were not required to follow the language of instruction policies set by the Department of Education, since private schools accounted for a low and fairly stable share of enrollment during the time the cohorts analyzed in this paper would have attended school. For example, the share of grade K-12 enrollment in private schools was 3% in the 1919–1920 school year, 4% in 1940–1941, and 5% in 1945–1946 (Osuna, 1949, pp. 475–476 and Appendix VI).

grade promotion. Policy changes from 1930 onwards therefore determine treatment intensity for these cohorts. Fig. 3 shows treatment intensity by cohort for three levels of educational attainment, 4, 8 and 12 years of schooling.[11] Among those with 4 years of schooling, exposure is zero for all those born after 1909—see the diamond-marker line.[12] Among the 1924–1943 cohorts, the largest source of variation in exposure to English instruction is along the extensive margin, from some English-intensive instruction to none; this is caused primarily by the 1949 reform, though there is some variation due to policy changes in the earlier period as well.

The empirical analysis below allows the effect of English-intensive instruction to vary by years of exposure to English-intensive instruction, although for ease of interpretation we also estimate specifications with a single treatment variable, years of exposure.[13] Among cohorts affected by the 1949 reform (born 1924–1933), exposure varied from 0 to 8 years. A natural control group for these affected cohorts is the cohorts born 1934–1943. Differences-in-differences-type estimates using the sample born 1924–1943 can be estimated using the equation

$$y_{ijk} = \alpha + \sum_{m=1}^{8} \beta_m I(E_{jk} = m) + \theta_j + \rho_k + \varepsilon_{ijk} \quad (1)$$

for individual i born in year j with k years of schooling where $I(\cdot)$ is the indicator function. The dependent variable, y_{ijk}, is a measure of English proficiency and E_{jk} is years of potential exposure to English-intensive instruction. The parameters β_1, β_2, …, β_8 in Eq. (1) are the difference-in-differences estimates, while θ_j is a cohort effect, controlling for cohort trends common across schooling groups, and ρ_k is an educational attainment-specific effect, controlling for the fact that less-educated people probably have weaker English-language skills.

The interpretation of β_m in Eq. (1) as the causal effect of receiving m years of English-intensive instruction turns on the assumption that the coefficients for interactions between birth cohort and years of schooling would be zero had language policies been unchanged. Fig. 1, which plots cohort trends in English proficiency by education group, does not appear to support this assumption. In each panel, the diamond-marked line shows the English proficiency of those with four or fewer years of schooling, who did not experience a change in language of instruction. The slope of this line differs from the slopes of the lines for more educated people, even among the cohorts who received Spanish-only schooling.

To correct the difference-in-differences-type estimates for education-cohort trends, we used a triple-differences-type strategy. Triple differences estimates are based on the following equation, estimated using data on cohorts born 1924–1963:

$$y_{ijk} = \alpha + \sum_{m=1}^{8} \beta_m I(E_{jk} = m) + \sum_{m=1}^{8} \delta_m I(ET_{jk} = m) + \theta_j + \rho_k + \pi_k I(j \geq 1944) + \varepsilon_{ijk} \quad (2)$$

Here, ET_{jk} is a term capturing spurious exposure effects. For those born 1924–1943, ET_{jk} equals E_{jk}, while for those born 1944–1963, ET_{jk} equals years of pseudo-treatment, i.e., treatment status

[11] Table A1 shows potential years of exposure to English-intensive schooling by year of birth and years of completed schooling for all levels of educational attainment, not just these three. It is worth emphasizing that Fig. 3 reflects *potential* exposure since some children start school at different ages, repeat grades, or withdraw temporarily. The results discussed below are unchanged when the exposure variable is coded allowing for modest levels of delayed school entry and grade repetition.

[12] For the 1924–1943 cohorts, grades 1–4 always had Spanish instruction. However, the Gallardo policy called for both English and Spanish to be used as languages of instruction in grades 3 and 4, with English used 1/3 of the time. Thus, someone with 4 years of schooling could have as much as 2/3 of a year of exposure.

[13] We use dummies for years of exposure because English proficiency is not necessarily a linear function of years of exposure, especially for low levels of proficiency.

assigned *as if* these cohorts had been born 20 years earlier. The parameters $\beta_1, \beta_2, \ldots, \beta_8$ in Eq. (2) are the triple differences estimates, implicitly equal to the treatment effects from the real experiment minus the pseudo-treatment effects estimated using the younger cohorts born 1944–1963. The triple differences estimates capture the effects of English-intensive instruction assuming that the education-specific cohort trends that applied to the 1944–1963 cohorts would have applied to the 1924–1943 cohorts in the absence of treatment. This assumption appears reasonable, since the rapid expansion of the Puerto Rican school system affected younger cohorts as well.

2. Data and descriptive statistics

The empirical analysis pools individual-level data from the 1980 and 1990 U.S. Census long forms in Puerto Rico and on the mainland.[14] Similar questionnaires were fielded in both locations so we can assemble a data set of consistently defined variables for Puerto Ricans regardless of whether they live on the island or the mainland. Most importantly, self-reported information on English-speaking ability has been solicited on Puerto Rico's census form for decades and was added to the mainland census form in 1980. Although the language question is asked differently on the two forms, we were able to construct a uniform set of dummy variables indicating English-speaking proficiency.[15]

A natural concern is the extent to which self-reported English-speaking ability is a meaningful measure of English-language skills. The Census language question has been validated in two ways, both described by Kominski (1989). First, the English Language Proficiency Study, conducted in 1982 by the Census Bureau for the Department of Education, incorporated standardized tests of English-language skills. The results on this test were shown to be highly correlated with Census self-reported English-speaking ability; for example, those responding "speaks English very well" in the census questionnaire had standardized test scores similar to a native English-speaking control population, while score levels fell markedly when self-reported English-speaking ability was lower. A second validation effort compared Census self-reported English-speaking ability with other measures of English-language skills taken from the National Content Test administered by the Census Bureau in 1986. These results showed Census self-reports to be highly correlated with functional measures of language skills such as English reading and writing skills and whether respondents used English at work.

Our analysis uses samples of people born in Puerto Rico. Since passage of the Jones Act in 1917, which granted U.S. citizenship to all Puerto Ricans, islanders have been able to travel freely and settle anywhere in the U.S. mainland and possessions. Significant migration to the mainland began in

[14] For 1980, we have a 6% sample for both Puerto Rico and mainland residents (the 5% A+1% B PUMS samples). For 1990, we also have a 6% sample for each (5% State+1% Metro PUMS samples). Data files for mainland residents were obtained from the IPUMS website (Ruggles et al., 2004), while data files for Puerto Rico residents were obtained from the ICPSR (U.S. Department of Commerce, Bureau of Census, 1980, 1990a,b).

[15] The Puerto Rican census form asks: "Do you know how to speak English?" with three possible responses: "yes, with ease", "yes, with difficulty" and "no, I do not speak English". This question is asked of every respondent. The 1980 and 1990 mainland census form asks: "How well does this person speak English?" with four possible responses "very well", "well", "not well" and "not at all". This question is asked only of those responding affirmatively to "Does this person speak a language other than English at home?" We coded mainland residents speaking only English as speaking English very well. Our indicator for speaking English well is 1 for Puerto Rican residents who speak English with ease or mainland residents who speak English well or very well. Our indicator for speaking English is 1 for Puerto Rican residents who speak English with difficulty or mainland residents who speak English not well or anyone for whom the variable *Speaks English Well* equals 1.

the 1950s, and by 1970 approximately one-third of those born in Puerto Rico lived on the mainland. Because virtually all Puerto Ricans live either in Puerto Rico or on the mainland, the combination of mainland and Puerto Rican census data provides a representative sample of all people born in Puerto Rico. This allows us to sidestep the problem of selective migration when examining the impact of the English-intensive instruction induced by the language of instruction policies.

Our initial analysis uses Puerto Ricans born 1924–1943 labeled as "affected" and "control" cohorts in Fig. 1.[16] As discussed in Section 1, the 1924–1933 cohorts were the last to be schooled in English, while those born 1934–1943 were the first educated in the post-reform regime. To construct falsification tests and triple differences estimates, we added one 10-year cohort born earlier and two 10-year cohorts born later, for a total of five cohorts used in the analysis. Those in the affected cohort with at least 5 years of schooling were exposed to almost 4 years of English instruction, on average. Exposure was zero among those in the affected group with 0–4 years of schooling and in both younger cohorts. The oldest cohort had an exposure level close to that in the affected cohort, about 3.8 years for those with 5 or more years of schooling. A complete set of descriptive statistics for all five cohorts appear in the appendix (Table A2).[17] The appendix also gives years of exposure for each year of birth and schooling level (Table A1).

3. Results

3.1. Difference-in-differences estimates

Simple differences-in-differences-type estimates suggest that those who received English-intensive instruction were significantly more likely to speak English. This can be seen in Panel A of Table 2, which reports ordinary least squares estimates of Eq. (1) using the sample born 1924–1943.[18] For example, the effect of receiving 6 years of English-intensive instruction on the probability of speaking English is estimated to be .035 (column 1). Those receiving more than 1 year of treatment are more likely to speak English than those receiving only 1 year, however, the exposure effects do not increase monotonically. Column 2 shows that those who received English-intensive instruction are also significantly more likely to speak English well. The estimates in this case mostly increase with years of exposure, consistent with the notion that stronger English-language skills take time to develop. Panel B shows the results when we allow the years of exposure to enter linearly rather than as eight dummies, and similarly suggests significant positive effects.

Control for mainland residence has little effect on the results. This can be seen in columns 3 and 4, which report results from models that include a dummy for mainland residence. The robustness to inclusion of the control for residence is encouraging since, although residence is

[16] Because we pool 1980 and 1990 data, the sample includes those aged 37–56 in 1980 and 47–66 in 1990. The results are invariant to modest changes in these age ranges.

[17] Schooling is measured slightly differently across the PUMS samples. The 1990 samples measure educational attainment while the 1980 samples measure years of schooling completed, and additionally the 1990 mainland sample has less detail for the lower grades (grades 1–4 form one group and grades 5–8 form another). Treatment intensity is assigned based on the original measure of schooling provided by each sample. For the 1990 mainland sample, people with grades 1–4 are assigned grade 3's treatment intensity and people with grades 5–8 are assigned grade 7's.

[18] The regressions used to construct these estimates also control for a full set of schooling, year of birth, age and sex dummies, as well as for potential experience and year of observation. The corresponding logit marginal effects are reported in the working paper version of this paper (Angrist et al., 2006). These estimates differ little from the linear probability model estimates.

Table 2
Difference-in-differences estimates of effect of English-intensive instruction

	All individuals born in Puerto Rico				Born and living in PR	
	Speaks English (1)	Speaks English Well (2)	Speaks English (3)	Speaks English Well (4)	Speaks English (5)	Speaks English Well (6)
Panel A. Dummies for years of exposure						
One	0.0124	0.0080	0.0313	0.0286	0.0481	0.0282
	(0.0265)	(0.0199)	(0.0113)	(0.0092)	(0.0140)	(0.0127)
Two	0.0760	0.0608	0.0550	0.0379	0.0725	0.0392
	(0.0267)	(0.0239)	(0.0128)	(0.0101)	(0.0182)	(0.0110)
Three	0.0557	0.0373	0.0691	0.0520	0.1033	0.0595
	(0.0249)	(0.0204)	(0.0109)	(0.0104)	(0.0148)	(0.0106)
Four	0.0496	0.0470	0.0595	0.0578	0.0943	0.0683
	(0.0141)	(0.0120)	(0.0108)	(0.0085)	(0.0150)	(0.0103)
Five	0.0536	0.0719	0.0542	0.0725	0.0769	0.0752
	(0.0149)	(0.0160)	(0.0109)	(0.0122)	(0.0148)	(0.0147)
Six	0.0354	0.0698	0.0421	0.0771	0.0595	0.0826
	(0.0157)	(0.0153)	(0.0105)	(0.0114)	(0.0138)	(0.0143)
Seven	0.0305	0.0662	0.0441	0.0810	0.0630	0.0923
	(0.0163)	(0.0143)	(0.0108)	(0.0097)	(0.0140)	(0.0127)
Eight	0.0524	0.0892	0.0660	0.1041	0.0915	0.1218
	(0.0179)	(0.0173)	(0.0118)	(0.0147)	(0.0160)	(0.0161)
Panel B. Linear years of exposure	0.0049	0.0106	0.0055	0.0113	0.0079	0.0131
	(0.0016)	(0.0015)	(0.0013)	(0.0013)	(0.0016)	(0.0015)
Dummy for lives on the mainland	No	No	Yes	Yes	No	No
Number of observations	92,430	92,430	92,430	92,430	62,597	62,597

The sample consists of individuals born 1924–1943 in Puerto Rico from the 1980 and 1990 PUMS files. Each column of each panel is from a separate OLS regression controlling for year of birth dummies, educational attainment dummies (using the ten categories defined in Fig. 2 notes), age dummies, census year dummies, female dummy and a quadratic in potential experience (age−years of schooling−6). Years of exposure to English-intensive instruction are from Table A1 and are rounded to the nearest whole number when the eight years of exposure dummies are used. Standard errors adjusted for year of birth-educational attainment clusters are shown in parentheses.

potentially endogenous (with language skills affecting the decision to migrate), the fact that the island and mainland language questions differ is a potential concern in pooled island/mainland samples. We would therefore like to look at estimates for island residents only. As it turns out, estimates using Puerto Rico residents only, reported in columns 5 and 6, resemble the estimates generated using the sample of all Puerto Rican-born.

3.2. Controlling for differential trends

The estimates in Table 2, which point to better English-language skills for those who were schooled before the Spanish-only period, may be confounded by education-specific cohort trends in English proficiency. Such trends could have arisen through several channels, primarily as a by-product of the rapid expansion of the Puerto Rican public education system for the 1924–1943 cohorts. First, as access to education spread, the average ability of more educated people may have fallen. A related point is that the education distribution – and unobserved ability conditional on schooling and cohort – may have changed as a consequence of the 1949 language reform. Second, rapid expansion of the public school system may have led to a decline in the quality of upper-grade classrooms relative to lower-grade classrooms. Third, although most schools with more than the first four grades were located in cities and towns, access to upper grades was becoming increasingly common in rural areas. Finally, some Puerto Ricans may have developed English-language skills when they served in the U.S. military, and service was more prevalent among older and more educated cohorts. All of these channels may generate spurious treatment effects, since the treated group consists of relatively old and more educated cohorts. We examine these specific sources of bias below; here, we discuss the results of a test for bias, followed by estimates that adjust for differential trends using Eq. (2).

As a first pass at a formal falsification test, we repeated a Table 2-type analysis using pairs of 10-year birth cohorts unaffected by the reform. These results are shown in Table 3. For comparison, results using the original treatment and control cohorts are reported in columns 1 and 2. Columns 3 and 4 show results for two cohorts that we think of as *always* treated (i.e., both were born 1933 or earlier), while columns 5 and 6 use two cohorts that were *never* treated (i.e., both were born 1934 or later). Finally, columns 7 and 8 show results for a second pair of never-treated cohorts, born 1944–1953 and 1954–1963. This falsification test generates spurious treatment effects. In Panel A, the hypothesis that the eight pseudo-treatment effects are jointly zero can be rejected at the 99% level in each column. In Panel B, the coefficient for the years of exposure variable is positive and significant in each column.

Table 3 also generates clear evidence of confounding trends, though it remains possible that the treatment-control contrasts for the cohorts actually experiencing a change in language policy exceed those for the falsification cohorts, perhaps significantly. In practice, however, estimates of Eq. (2), reported in Table 4, show that controlling for differential trends eliminates any significant positive effects of English-intensive instruction on English proficiency. For example, in Panel A, the smallest p-value for joint significance of the eight years of exposure dummies is over 0.2.

An important consideration in assessing the results in Table 4 is statistical power. To assess the precision of the estimates, and our ability to detect effects of a plausible magnitude, we focus on the results in Panel B, which come from a specification in which treatment is measured by a single variable, years of exposure. The largest standard error associated with the triple differences estimate for the *Speaks English* dependent variable is 0.0026, so the effect must be at least 0.0052 (about twice the standard error) to show up as significant with 95% confidence. If the effect per year exposed were 0.0052, then the average effect would be 2.1 percentage points (since conditional on receiving treatment, average years of treatment is almost four), which is 3% of the

Table 3
Control experiments using younger and older cohorts born and living in Puerto Rico

	From Table 2, columns 5 and 6 Born 1924–1933 (affected) vs. born 1934–1943 (control)		Always treated Born 1914–1923 (placebo) vs. born 1924–1933 (control)		Never treated Born 1934–1943 (placebo) vs. born 1944–1953 (control)		Never treated Born 1944–1953 (placebo) vs. born 1954–1963 (control)	
	Speaks English (1)	Speaks English Well (2)	Speaks English (3)	Speaks English Well (4)	Speaks English (5)	Speaks English Well (6)	Speaks English (7)	Speaks English Well (8)
Panel A. Dummies for years of exposure or pseudo-exposure								
One	0.0481	0.0282	0.0271	−0.0011	0.0371	0.0173	0.0562	0.0259
	(0.0140)	(0.0127)	(0.0153)	(0.0080)	(0.0126)	(0.0101)	(0.0128)	(0.0095)
Two	0.0725	0.0392	0.1105	0.0185	0.0540	0.0216	0.0883	0.0290
	(0.0182)	(0.0110)	(0.0207)	(0.0170)	(0.0151)	(0.0093)	(0.0156)	(0.0090)
Three	0.1033	0.0595	0.1191	0.0269	0.0902	0.0451	0.1243	0.0513
	(0.0148)	(0.0106)	(0.0190)	(0.0214)	(0.0161)	(0.0142)	(0.0153)	(0.0116)
Four	0.0943	0.0683	0.0917	0.0301	0.0824	0.0419	0.0996	0.0575
	(0.0150)	(0.0103)	(0.0212)	(0.0144)	(0.0159)	(0.0128)	(0.0174)	(0.0117)
Five	0.0769	0.0752	0.0668	0.0496	0.1033	0.0812	0.1165	0.0932
	(0.0148)	(0.0147)	(0.0187)	(0.0196)	(0.0205)	(0.0154)	(0.0167)	(0.0111)
Six	0.0595	0.0826	0.0935	0.0512	0.0949	0.0717	0.1128	0.0875
	(0.0138)	(0.0143)	(0.0130)	(0.0174)	(0.0176)	(0.0133)	(0.0166)	(0.0116)
Seven	0.0630	0.0923	0.0553	0.0731	0.0704	0.0541	0.1132	0.1054
	(0.0140)	(0.0127)	(0.0110)	(0.0106)	(0.0168)	(0.0141)	(0.0164)	(0.0138)
Eight	0.0915	0.1218	0.0807	0.0681	0.1081	0.0936	0.1237	0.1155
	(0.0160)	(0.0161)	(0.0233)	(0.0165)	(0.0228)	(0.0198)	(0.0193)	(0.0126)
Panel B. Linear years	0.0079	0.0131	0.0119	0.0097	0.0109	0.0099	0.0116	0.0142
	(0.0016)	(0.0015)	(0.0020)	(0.0016)	(0.0020)	(0.0016)	(0.0020)	(0.0015)
N	62,597	62,597	48,478	48,478	81,454	81,454	96,302	96,302

The sample consists of individuals born and currently living in Puerto Rico from the 1980 and 1990 PUMS files. Each column of each panel is from a separate OLS regression controlling for year of birth dummies, educational attainment dummies (using categories defined in Fig. 2 notes), age dummies, census year dummies, female dummy and a quadratic in potential experience (age−years of schooling−6). Years of pseudo-exposure equal actual years of exposure as if the older cohort were born 1924–1933 (which is the older cohort in Columns 1 and 2) and the younger cohort were born 1934–1943 (which is the younger cohort in Columns 1 and 2). Standard errors adjusted for year of birth-educational attainment clusters are shown in parentheses.

Table 4
Estimates of effect of English-intensive instruction using younger cohorts to control for differential trend

	All individuals born in Puerto Rico				Born and living in PR	
	Speaks English (1)	Speaks English Well (2)	Speaks English (3)	Speaks English Well (4)	Speaks English (5)	Speaks English Well (6)
Panel A. Dummies for years of exposure						
One	−0.0020	0.0027	−0.0018	0.0029	−0.0081	0.0023
	(0.0438)	(0.0328)	(0.0160)	(0.0150)	(0.0189)	(0.0159)
Two	−0.0102	0.0021	−0.0214	−0.0114	−0.0158	0.0102
	(0.0381)	(0.0318)	(0.0186)	(0.0137)	(0.0240)	(0.0142)
Three	−0.0110	−0.0129	−0.0132	−0.0156	−0.0209	0.0082
	(0.0339)	(0.0276)	(0.0158)	(0.0169)	(0.0213)	(0.0157)
Four	0.0080	−0.0047	−0.0015	−0.0162	−0.0053	0.0107
	(0.0229)	(0.0194)	(0.0168)	(0.0144)	(0.0229)	(0.0155)
Five	0.0067	0.0005	−0.0196	−0.0314	−0.0396	−0.0180
	(0.0240)	(0.0221)	(0.0164)	(0.0158)	(0.0223)	(0.0184)
Six	−0.0040	0.0007	−0.0258	−0.0256	−0.0533	−0.0049
	(0.0265)	(0.0228)	(0.0166)	(0.0164)	(0.0215)	(0.0184)
Seven	−0.0065	−0.0189	−0.0197	−0.0348	−0.0502	−0.0130
	(0.0271)	(0.0226)	(0.0171)	(0.0156)	(0.0215)	(0.0187)
Eight	0.0205	0.0053	0.0003	−0.0191	−0.0323	0.0063
	(0.0303)	(0.0246)	(0.0199)	(0.0191)	(0.0251)	(0.0204)
F-test *p*-value for dummies	0.8132	0.8488	0.4451	0.2845	0.2051	0.7019
Panel B. Linear years of exposure	0.0035	−0.0003	0.0002	−0.0042	−0.0038	−0.0011
	(0.0026)	(0.0021)	(0.0022)	(0.0019)	(0.0026)	(0.0022)
Dummy for lives on the mainland	No	No	Yes	Yes	No	No
Number of observations	233,990	233,990	233,990	233,990	158,899	158,899

The sample consists of individuals born 1924–1963 in Puerto Rico from the 1980 and 1990 PUMS files. Each column of each panel is from a separate OLS regression which contains as controls years of pseudo-exposure dummies and the same covariates as in Table 2. The education, age, census year, female and potential experience coefficients are allowed to vary by two groupings of year of birth, born 1924–1943 and born 1944–1963. Years of pseudo-exposure are equal to actual years of exposure for individuals born 1924–1943. On the other hand, individuals born 1944–1963 are assigned the actual years of exposure as if they were born 20 years earlier. Standard errors adjusted for year of birth-educational attainment clusters are shown in parentheses. The *F*-test *p*-value reported in Panel A is for a test of the joint significance of the eight years of exposure dummies.

control group mean and 4% of the control group standard deviation. Similarly, the largest standard error associated with the triple differences estimate for the outcome *Speaks English Well* is 0.0022, so the effect must be at least 0.0044 to be significant. If the effect per year were 0.0044, then the average effect would be 1.8 percentage points, 4% of the control group mean and standard deviation. These calculations suggest the research design that lies behind Table 4 has the power to detect even modest effects.

3.3. Controlling for education-related selection

A likely source of education-specific cohort trends is a decrease in positive selection into higher levels of education over time. As education spread and compulsory schooling laws were increasingly enforced, children with less ability or from a more disadvantaged family background increasingly entered higher grades. As a result, the well-educated from more recent cohorts might have been less likely to speak English than the well-educated from earlier cohorts.

A related point is the possibility of an endogenous schooling response to the 1949 language reform. That is, the reform itself could be responsible for increasing educational attainment, since instruction in English might have been a barrier for some children in school. This is a concern here because we are relying on differences between schooling groups across cohorts to identify the effects of the reform. Still, our results suggest the cohort-schooling strategy should allow us to learn something about reform effects. This is because a (sharp) endogenous increase in negative selection in the post-reform period should generate positive triple differences estimates, since these estimates control for (presumably smooth) trends using non-reform cohorts. In practice, however, the estimates in Table 4 are close to zero or negative. This suggests that a sharp endogenous schooling response is not a confounding factor, though the placebo experiment does indicate the presence of a relatively smooth selection trend.

As a further check on the selection hypothesis, we added a quadratic function of a measure of the education cumulative distribution function (CDF) by cohort and educational attainment level to Eq. (1). Specifically, the "education CDF" measure for each respondent is the fraction of people in the Census born the same year with lower educational attainment than the respondent.[19] The results, reported in columns 3 and 4 of Table 5, show treatment effects that are on average lower by 40% and 17%, respectively, relative to the original results not controlling for education CDF (redisplayed in columns 1 and 2). Moreover, in column 3, the positive coefficients for both the education CDF and its square imply that as the proportion of one's cohort with less schooling increases, ability to speak English increases. In column 4, the negative coefficient for education CDF and positive coefficient of greater magnitude for its square imply that at high levels of educational attainment, the higher the proportion of a cohort with less schooling, the higher is the propensity to speak English very well.

In addition to exploring the impact of CDF controls in the basic differences-in-differences setup without trends, we added the schooling-CDF quadratic to Eq. (2) as well; these results are shown in columns 5–8 of Table 5. Only 1990 data are used for triple differences estimation with CDF controls since some in the youngest cohorts would not have completed schooling by 1980. These triple differences estimates of the treatment effects are (jointly) insignificantly different from zero, as in Table 4. Thus, while changing selection bias based on unobservable characteristics appears to be an important source of education-specific cohort trends, controlling

[19] Note that controlling for the education CDF also helps control for a possible endogenous schooling response to the reform, since any jump in educational attainment for treated cohorts should be reflected in the education CDF.

Table 5
Specifications controlling for education distribution

	Table 2, columns 5 and 6, 1980 and 1990 sample born 1924–1943 (DD using PR-born and resident)		Table 2, columns 5 and 6, adding education CDF controls, 1980 and 1990 sample born 1924–1943 (DD using PR-born and resident)		Table 4, columns 5 and 6, adding education CDF controls, 1990 sample born 1924–1963 (DDD using PR-born and resident)		Table 4, columns 1 and 2, adding education CDF controls, 1990 sample born 1924–1963 (DDD using all PR-born)	
	Speaks English (1)	Speaks English Well (2)	Speaks English (3)	Speaks English Well (4)	Speaks English (5)	Speaks English Well (6)	Speaks English (7)	Speaks English Well (8)
Years of exposure								
One	0.0481	0.0282	0.0200	0.0301	0.0108	0.0095	−0.0141	−0.0158
	(0.0140)	(0.0127)	(0.0157)	(0.0129)	(0.0245)	(0.0176)	(0.0932)	(0.0683)
Two	0.0725	0.0392	0.0305	0.0377	0.0043	0.0093	−0.0233	−0.0045
	(0.0182)	(0.0110)	(0.0195)	(0.0126)	(0.0315)	(0.0223)	(0.0694)	(0.0582)
Three	0.1033	0.0595	0.0558	0.0513	0.0068	0.0094	0.0009	0.0009
	(0.0148)	(0.0106)	(0.0134)	(0.0116)	(0.0272)	(0.0222)	(0.0883)	(0.0739)
Four	0.0943	0.0683	0.0579	0.0538	−0.0043	−0.0095	−0.0151	−0.0247
	(0.0150)	(0.0103)	(0.0130)	(0.0111)	(0.0259)	(0.0219)	(0.0483)	(0.0391)
Five	0.0769	0.0752	0.0502	0.0483	0.0152	−0.0311	0.0137	−0.0242
	(0.0148)	(0.0147)	(0.0151)	(0.0165)	(0.0332)	(0.0296)	(0.0544)	(0.0446)
Six	0.0595	0.0826	0.0367	0.0607	−0.0097	−0.0147	−0.0115	−0.0230
	(0.0138)	(0.0143)	(0.0142)	(0.0150)	(0.0304)	(0.0260)	(0.0578)	(0.0458)
Seven	0.0630	0.0923	0.0434	0.0715	−0.0201	−0.0291	−0.0284	−0.0513
	(0.0140)	(0.0127)	(0.0137)	(0.0138)	(0.0293)	(0.0274)	(0.0583)	(0.0459)
Eight	0.0915	0.1218	0.0794	0.1029	0.0169	−0.0020	0.0221	−0.0030
	(0.0160)	(0.0161)	(0.0140)	(0.0179)	(0.0375)	(0.0301)	(0.0627)	(0.0487)
Education CDF measure	No	No	0.3833	−0.3000	0.5690	−0.1962	0.3704	−0.2044
			(0.1060)	(0.1039)	(0.1214)	(0.0997)	(0.1830)	(0.1545)
Education CDF measure squared	No	No	0.2169	0.3981	0.0769	0.4687	0.0688	0.4337
			(0.1160)	(0.1110)	(0.1103)	(0.1130)	(0.1396)	(0.1481)
F-test *p*-value	0.0000	0.0000	0.0000	0.0000	0.7575	0.7897	0.2495	0.4119
N	62,597	62,597	62,597	62,597	77,398	77,398	113,578	113,578

The sample consists of individuals born in Puerto Rico from the PUMS files, with Columns 1–4 using both 1980 and 1990 data and Columns 5–8 using only 1990 data. Each column is from a separate OLS regression. The education cumulative distribution function (CDF) measure gives the fraction of people of the same year of birth with less education than the individual. Standard errors adjusted for year of birth-educational attainment clusters are shown in parentheses. The *F*-test *p*-value reported is for a test of the joint significance of the eight years of exposure dummies. DD denotes difference-in-differences estimation and DDD denotes triple differences estimation.

for this source of bias does not change the finding that more English-intensive instruction does not raise English-speaking ability.

3.4. Other possible sources of differential trends

Our analysis of education-specific cohort trends concludes with a brief look at a few other possible explanations. First, the positive difference-in-differences estimates observed among cohorts that did not experience a change in language of instruction may be caused by changes in school quality. Large increases in public expenditures on education financed additional teachers, higher teacher salaries and more classrooms (Osuna, 1949, p. 607, Table 2). Moreover, double enrollment – a practice in which teachers teach two half-day sessions to different groups of students to relieve overcrowding – was gradually eliminated. Double enrollment was most common in the first two grades and in rural areas; in the 1943–1944 school year, for example, 78% of the total enrollment of rural schools attended half-day sessions compared with 44% of urban schools.[20] Elimination of double enrollment meant more instruction time in the early grades, including in English class. This may have reduced the gap between upper- and lower-grade English proficiency among younger cohorts, thereby contributing to spurious reform effects.

As a rough check on the school quality story involving double enrollment, we dropped people with 1–4 years of schooling. The remaining members of the low-education control group are people with no schooling. Those with 1–4 years of schooling were probably most affected by the gradual elimination of double enrollment, while people with no schooling were unaffected. The results of estimating Eq. (1) with this restricted sample, which are available upon request, are similar to the original results reported in columns 5 and 6 of Table 2. Likewise, results from the control experiments without grades 1–4 are similar to those from the full sample. Thus, the gradual elimination of double enrollment does not appear to be behind the education-specific cohort trends.

Second, education-specific cohort trends may have been induced by the gradual spread of higher-grade schools to rural areas. In the first decades of the American occupation, few rural communities offered schooling beyond the 4th grade. Later, however, the number of schools with post-primary grades in rural areas multiplied. As a result, the urban proportion of 5th grade enrollment fell from 66% in 1930 to 57% in 1940, and the urban proportion of 8th grade enrollment fell from 93% in 1930 to 77% in 1940.[21] Our cohort-schooling differences-in-differences strategy may be biased by the increased likelihood that more educated individuals from more recent cohorts came from rural areas and therefore had reduced English proficiency (since cities and towns present more opportunities for exposure to English in daily life).

The effects of increased schooling in the countryside are difficult to control for in practice since the Census records urban/rural status for current residence but not childhood residence (in fact, not even birthplace). Still, an analysis based on current residence may provide useful information so long as urban residents are more likely than rural residents to have grown up in cities and towns. The most consistent definition of urban status that can be used for both the 1980 and 1990 censuses is residence in the San Juan-Bayamón primary metropolitan statistical area (PMSA).[22] We estimated Eq. (1) restricting the sample to residents of the San Juan-Bayamón

[20] Osuna (1949), p. 291. In 1920, 90% of rural schools were on double enrollment (Osuna, 1949, p. 213).

[21] Authors' calculations based on enrollment data by year, grade and urban/rural status from Osuna (1949, pp. 624-25, Appendix VIII, Table 1).

[22] This area contains the largest and oldest cities of Puerto Rico, containing over 30% of the total population and over 60% of the urban population. Estimates using other definitions of urban status generate similar results.

PMSA as well as for a subsample who lived in the same house 5 years ago; probably people in this subsample are more likely to live where they were born. In practice, both sets of estimates show the same strong effects observed in Table 2. Thus, changes in the likelihood of urban residence for the more educated do not appear to account for the positive difference-in-differences estimates in Table 2.

A third possible explanation for education-specific cohort trends is changes in the probability of military service. Many Puerto Ricans served in the U.S. military, especially among the older cohorts in our sample. For example, 30% of men born and living in Puerto Rico from the 1924–1933 cohorts had served compared with 18% from the 1934–1943 cohorts. Veterans from these cohorts are also more educated than non-veterans. Among men born 1924–1933, average schooling is 12.5 for veterans and 6.5 for non-veterans. Military service may have increased English-speaking ability. Given the strong education differences by veteran status, this in turn may have induced an education-specific cohort trend in English proficiency.

To determine whether military service accounts for education-specific cohort trends, we re-estimated Eq. (1) restricting the sample to non-veteran men. The results, which are available upon request, again show significant positive difference-in-differences estimates in both the real and control experiments. We also see similar results in a sample restricted to women, in spite of the fact that almost no Puerto Rican women served in the military. Thus, changes in the likelihood of serving in the U.S. military for the more educated do not appear to account for patterns observed in Table 2.

3.5. An alternative identification strategy

As an alternative to strategies that control for unobservables using unaffected Puerto Rican cohorts, we also experimented with an identification strategy that compares Puerto Ricans living on the mainland with immigrants from former Spanish colonies. Especially relevant comparison groups are immigrants from Cuba and the Philippines since, like Puerto Rico, these territories became U.S. possessions in 1898. Unlike Puerto Rico, however, the language of instruction in Cuba and the Philippines has been unchanged since the American occupation. We also look at two other comparison countries: the Dominican Republic, a Spanish-speaking neighbor, and Mexico, the largest source of Hispanic immigrants to the U.S. A drawback of the cross-country strategy, not shared by our first strategy, is that immigrants are self-selected and subject to U.S. eligibility rules, some of which are country-specific. An advantage, however, is that we need not rely on comparisons across schooling groups, since schooling itself is a potentially endogenous variable.

A triple-differences-type estimation strategy was again used to allow for country-specific trends in English proficiency. In particular, we estimated the following equation using a sample of adult migrants born 1924–1963 from the 1990 Census:

$$y_{ijk} = \alpha + \beta PR_k \times E_j + \delta PR_k \times ET_j + \theta_j + \rho_k + \pi_k I(j \geq 1944) + X_{ijk}\Gamma_k + \varepsilon_{ijk} \quad (3)$$

for individual i born in year j in country k. Here, PR_k is a dummy variable indicating the Puerto Rican born, E_j is dummy indicating the 1924–1933 cohorts, ET_j is dummy indicating either the 1924–1933 or 1944–1953 cohorts, and X_{ijk} is a vector of individual covariates.[23] The coefficient

[23] We focus on adult migrants because they would have obtained their grades 1–12 education in their place of birth. We use only 1990 data because year of arrival to the mainland is not available for Puerto Ricans in the 1980 census, making it impossible to drop people who would have been educated in the mainland. A similar analysis using 2000 PUMS data and pooled 1990 and 2000 PUMS data generates similar results.

Table 6
Estimation using Puerto Rican adult migrants and Hispanic and Filipino adult immigrants to the mainland

	Sample composed of adult migrants from Puerto Rico and immigrants from			
	Cuba (1)	Philippines (2)	Mexico (3)	Dominican Republic (4)
Panel A. Dependent variable is Speaks English				
Triple differences estimate	−0.0451	−0.0347	0.0178	−0.0631
	(0.0172)	(0.0109)	(0.0152)	(0.0238)
Differential trend estimate	0.0747	−0.0080	0.0351	0.0783
	(0.0124)	(0.0065)	(0.0089)	(0.0127)
Number of observations	30,284	38,001	81,919	18,883
Panel B. Dependent variable is Speaks English Well				
Triple differences estimate	−0.0487	−0.0025	0.0047	−0.0553
	(0.0294)	(0.0187)	(0.0209)	(0.0239)
Differential trend estimate	0.1203	0.0057	0.0097	0.0872
	(0.0246)	(0.0116)	(0.0148)	(0.0132)
Number of observations	30,284	38,001	81,919	18,883
Panel C. Dependent variable is Speaks English Very Well				
Triple differences estimate	−0.0510	−0.0349	0.0076	−0.0097
	(0.0267)	(0.0235)	(0.0229)	(0.0235)
Differential trend estimate	0.0840	0.0688	−0.0303	0.0179
	(0.0214)	(0.0142)	(0.0145)	(0.0139)
Number of observations	30,284	38,001	81,919	18,883

This analysis uses 1990 Census data on Puerto Rican adult migrants and Hispanic and Filipino adult immigrants born 1924–1963 arriving to the mainland 1950–1986 and currently living in the mainland. Adult migrants and adult immigrants are defined as individuals who arrived to the mainland at age 18 or above. Each column of each panel reports the results of a separate OLS regression controlling for year of birth dummies, place of birth dummies, educational attainment dummies (using categories defined in Fig. 2 notes), female dummy, year of arrival dummies and a quadratic in potential experience (age − years of schooling − 6). The coefficients for the last five variables are allowed to vary by two groupings of year of birth, born 1924–1943 and born 1944–1963. Additionally, the coefficients for all education, sex and year of arrival variables are allowed to differ for Puerto Ricans. Standard errors adjusted for year of birth-country of birth clusters are shown in parentheses.

β is the triple differences estimate, implicitly constructed by subtracting the country-of-birth trend estimated using people born 1944–1963 (with those born 1944–1953 in Puerto Rico taken to be pseudo-treated) from the difference-in-differences estimate for those born 1924–1943 (with those born 1924–1933 in Puerto Rico receiving treatment).

The results of estimating Eq. (3) are presented in Table 6, which reports estimates of country-specific cohort trends, δ, as well as the triple differences parameter, β. The differential trends for all three English proficiency measures are mostly positive, suggesting that the improvement in English proficiency across cohorts has been smaller for Puerto Rican migrants than for other groups (or the decline for Puerto Ricans has been larger). But because this decline relative to the immigrant groups occurred for both treated and pseudo-treated cohorts, it does not point to an adverse effect of Puerto Rican language policy shifts. In fact, the triple differences estimates of English-intensive instruction on all three English proficiency measures are either statistically insignificant or negative. Thus, estimates using immigrants as a control group are consistent with the earlier findings using a Puerto Rican-only identification strategy.

4. Conclusions

Changes in Puerto Rican language of instruction policies, culminating in the 1949 language reform eliminating English instruction, provide a unique opportunity to assess the long-run consequences of English-intensive instruction for the English-language skills of a Spanish-speaking population. Perhaps surprisingly, our results suggest that the change from English to Spanish as the medium of instruction in public schools had little effect on Puerto Rican English proficiency, at least as far as self-reported English-speaking skills are concerned. These results are especially unexpected given the presumption by American policymakers at the time that English-only instruction is the best way to raise English proficiency among Puerto Ricans.

Our findings also contrast with those reported by Angrist and Lavy (1997), who evaluated the effects of a similar language reform in Morocco—in their case, a change from the colonial language (French) to Arabic in middle and secondary schools. The Angrist and Lavy results show a marked decline in French-language skills among affected groups, though it should be noted that they found a significant effect on French *writing* skills, but not on French *reading* skills. A more detailed analysis might show a similar pattern in Puerto Rico. Another difference between the Puerto Rican and Moroccan experiences is the relatively abundant supply of French speakers in Morocco, including French citizens and an educated workforce comfortable with a French-speaking milieu.[24]

While our results suggest English-intensive instruction is not sufficient for improved English-language skills, there is good circumstantial evidence that English-intensive instruction is not necessary for good English-language skills either. For example, in a 2000 survey, 41% of Europeans said they knew English even though their language of instruction was a non-English mother tongue, with English taught only as a foreign language.[25] Moreover, 80% of those surveyed in Denmark, the Netherlands, and Sweden knew some English, and 60% of respondents in these countries reported a "good" or "very good" level of English. Both the continued use of colonial language instruction in many former colonies and the American movement away from native-language instruction for immigrant children are partially motivated by the belief that children instructed in a non-native language will have better non-native language skills. For the Puerto Rican-born, at least, this does not appear to be true.

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[24] Angrist and Lavy also found negative earnings effects. We briefly explored models for wages; consistent with our results for English-language skills, after adjusting for education-specific cohort trends, these models show no effects. It is also worth noting that Angrist and Lavy relied on a less comprehensive specification check than our triple differences models with full nonparametric control for education-specific cohort trends.

[25] INRA (Europe) (2001). Along these lines, in 2003, Chile launched a program called English Opens Doors, designed to raise English proficiency. This program focuses on teaching English as a second language in public schools (Rohter, 2004).

Appendix A

Table A1
Potential exposure to English-intensive instruction in Puerto Rican public schools

Year of birth	Year of entry into grade 1	Potential years of exposure to English-intensive instruction if highest grade completed is												
		<Grade 1	Grade 1	Grade 2	Grade 3	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8	Grade 9	Grade 10	Grade 11	≥Grade 12
1900	1906	0.0	1.0	2.0	3.0	4.0	5.0	6.0	7.0	8.0	9.0	10.0	11.0	12.0
1901	1907	0.0	1.0	2.0	3.0	4.0	5.0	6.0	7.0	8.0	9.0	10.0	11.0	12.0
1902	1908	0.0	1.0	2.0	3.0	4.0	5.0	6.0	7.0	8.0	9.0	10.0	11.0	12.0
1903	1909	0.0	1.0	2.0	3.0	4.0	5.0	6.0	7.0	8.0	9.0	10.0	11.0	12.0
1904	1910	0.0	1.0	2.0	3.0	4.0	5.0	6.0	7.0	8.0	9.0	10.0	11.0	12.0
1905	1911	0.0	1.0	2.0	3.0	4.0	5.0	6.0	7.0	8.0	9.0	10.0	11.0	12.0
1906	1912	0.0	1.0	2.0	3.0	4.0	4.5	5.5	6.5	7.5	8.5	9.5	10.5	11.5
1907	1913	0.0	1.0	2.0	3.0	3.0	3.5	4.5	5.5	6.5	7.5	8.5	9.5	10.5
1908	1914	0.0	1.0	2.0	2.0	2.0	2.5	3.5	4.5	5.5	6.5	7.5	8.5	9.5
1909	1915	0.0	1.0	1.0	1.0	1.0	1.5	2.5	3.5	4.5	5.5	6.5	7.5	8.5
1910	1916	0.0	0.0	0.0	0.0	0.0	0.5	1.5	2.5	3.5	4.5	5.5	6.5	7.5
1911	1917	0.0	0.0	0.0	0.0	0.0	0.5	1.5	2.5	3.5	4.5	5.5	6.5	7.5
1912	1918	0.0	0.0	0.0	0.0	0.0	0.5	1.5	2.5	3.5	4.5	5.5	6.5	7.5
1913	1919	0.0	0.0	0.0	0.0	0.0	0.5	1.5	2.5	3.5	4.5	5.5	6.5	7.5
1914	1920	0.0	0.0	0.0	0.0	0.0	0.5	1.5	2.5	3.5	4.5	5.5	6.5	7.5
1915	1921	0.0	0.0	0.0	0.0	0.0	0.5	1.5	2.5	3.5	4.5	5.5	6.5	7.5
1916	1922	0.0	0.0	0.0	0.0	0.0	0.5	1.5	2.5	3.5	4.5	5.5	6.5	7.5
1917	1923	0.0	0.0	0.0	0.0	0.0	0.5	1.5	2.5	3.5	4.5	5.5	6.5	7.5
1918	1924	0.0	0.0	0.0	0.0	0.0	0.5	1.5	2.5	3.5	4.5	5.5	6.5	7.5
1919	1925	0.0	0.0	0.0	0.0	0.0	0.5	1.5	2.5	3.5	4.5	5.5	6.5	7.5
1920	1926	0.0	0.0	0.0	0.0	0.0	0.5	1.5	2.5	3.5	4.5	5.5	6.5	7.5
1921	1927	0.0	0.0	0.0	0.0	0.0	0.5	1.5	2.5	2.5	3.5	4.5	5.5	6.5
1922	1928	0.0	0.0	0.0	0.0	0.0	0.5	1.5	1.5	1.5	2.5	3.5	4.5	5.5
1923	1929	0.0	0.0	0.0	0.0	0.0	0.5	0.5	0.5	0.5	1.5	2.5	3.5	4.5
1924	1930	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.7	1.7	2.7	3.7	4.7
1925	1931	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.7	1.3	2.3	3.3	4.3	5.3
1926	1932	0.0	0.0	0.0	0.0	0.0	0.0	0.5	1.2	1.8	2.8	3.8	4.8	5.8
1927	1933	0.0	0.0	0.0	0.0	0.0	0.5	1.0	1.7	2.3	3.3	4.3	5.3	6.3
1928	1934	0.0	0.0	0.0	0.0	0.3	0.8	1.3	2.0	2.7	3.7	4.7	5.7	6.7

(continued on next page)

Table A1 (*continued*)

Year of birth	Year of entry into grade 1	Potential years of exposure to English-intensive instruction if highest grade completed is												
		<Grade 1	Grade 1	Grade 2	Grade 3	Grade 4	Grade 5	Grade 6	Grade 7	Grade 8	Grade 9	Grade 10	Grade 11	≥Grade 12
1929	1935	0.0	0.0	0.0	0.3	0.7	1.2	1.7	2.3	3.3	4.3	5.3	6.3	7.3
1930	1936	0.0	0.0	0.0	0.3	0.7	1.2	1.7	2.7	3.7	4.7	5.7	6.7	7.7
1931	1937	0.0	0.0	0.0	0.3	0.7	1.2	1.2	2.2	3.2	4.2	5.2	6.2	7.2
1932	1938	0.0	0.0	0.0	0.3	0.7	0.7	0.7	1.7	2.7	3.7	4.7	5.7	5.7
1933	1939	0.0	0.0	0.0	0.3	0.3	0.3	0.3	1.3	2.3	3.3	4.3	4.3	4.3
1934 and later	1940 and later	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0

For each year of birth and level of educational attainment, the years of exposure to English-intensive instruction is given assuming that individuals began first grade at age 6 and were promoted yearly up through the last grade completed. Variation from cohort to cohort comes from the policy shifts described in Table 1. We coded grade-years in which both English and Spanish were used as languages of instruction as a fraction of a whole year of treatment. Under the Miller policy, grade 5 was half Spanish and half English. Under the Gallardo policy, grades 3–8 used both Spanish and English with a gradual increase in English.

Table A2
Descriptive statistics for Puerto Rican-born

	Born 1914–1923			Born 1924–1933 (Affected cohort)			Born 1934–1943 (Control cohort)			Born 1944–1953			Born 1954–1963		
	Total (1)	4 or less yrs educ (2)	5 or more yrs educ (3)	Total (4)	4 or less yrs educ (5)	5 or more yrs educ (6)	Total (7)	4 or less yrs educ (8)	5 or more yrs educ (9)	Total (10)	4 or less yrs educ (11)	5 or more yrs educ (12)	Total (13)	4 or less yrs educ (14)	5 or more yrs educ (15)
Years of exposure to English-intensive instruction	2.20	0.00	3.82	2.85	0.13	3.98	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(2.66)	(0.00)	(2.47)	(2.66)	(0.22)	(2.37)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Speaks English (not well, well or very well)	0.50	0.21	0.71	0.63	0.31	0.76	0.68	0.34	0.74	0.69	0.31	0.72	0.62	0.22	0.64
	(0.50)	(0.41)	(0.45)	(0.48)	(0.46)	(0.43)	(0.47)	(0.47)	(0.44)	(0.46)	(0.46)	(0.45)	(0.49)	(0.42)	(0.48)
Speaks English well (well or very well)	0.27	0.07	0.41	0.37	0.11	0.48	0.42	0.15	0.48	0.45	0.13	0.48	0.38	0.11	0.39
	(0.44)	(0.25)	(0.49)	(0.48)	(0.32)	(0.50)	(0.49)	(0.35)	(0.50)	(0.50)	(0.34)	(0.50)	(0.48)	(0.31)	(0.49)
Age	65.65	66.03	65.37	56.14	56.66	55.93	46.03	46.77	45.89	36.32	36.74	36.29	26.31	6.74	26.29
	(5.66)	(5.65)	(5.65)	(5.75)	(5.73)	(5.75)	(5.75)	(5.76)	(5.74)	(5.73)	(5.66)	(5.73)	(5.80)	(5.88)	(5.80)
Proportion female	0.53	0.57	0.50	0.53	0.59	0.51	0.53	0.58	0.53	0.53	0.49	0.54	0.52	0.42	0.53
	(0.50)	(0.49)	(0.50)	(0.50)	(0.49)	(0.50)	(0.50)	(0.49)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.49)	(0.50)
Years of schooling	6.36	2.20	9.42	7.89	2.26	10.22	9.30	2.29	10.67	10.71	2.32	11.44	11.23	2.17	11.68
	(4.52)	(1.50)	(3.43)	(4.72)	(1.48)	(3.48)	(4.33)	(1.48)	(3.24)	(3.93)	(1.53)	(3.15)	(3.43)	(1.58)	(2.81)
Proportion with no schooling	0.11	0.25	0.00	0.07	0.23	0.00	0.04	0.22	0.00	0.02	0.23	0.00	0.01	0.28	0.00
	(0.31)	(0.43)	(0.00)	(0.25)	(0.42)	(0.00)	(0.19)	(0.42)	(0.00)	(0.14)	(0.42)	(0.00)	(0.12)	(0.45)	(0.00)
Proportion with 1–4 years schooling	0.32	0.75	0.00	0.23	0.77	0.00	0.13	0.78	0.00	0.06	0.77	0.00	0.03	0.72	0.00
	(0.47)	(0.43)	(0.00)	(0.42)	(0.42)	(0.00)	(0.33)	(0.42)	(0.00)	(0.24)	(0.42)	(0.00)	(0.18)	(0.45)	(0.00)
Proportion with 5–8 years schooling	0.30	0.00	0.51	0.27	0.00	0.38	0.23	0.00	0.28	0.18	0.00	0.20	0.13	0.00	0.14
	(0.46)	(0.00)	(0.50)	(0.44)	(0.00)	(0.48)	(0.42)	(0.00)	(0.45)	(0.38)	(0.00)	(0.40)	(0.34)	(0.00)	(0.35)
Proportion with 9 or more years schooling	0.28	0.00	0.49	0.44	0.00	0.62	0.61	0.00	0.72	0.74	0.00	0.80	0.82	0.00	0.86
	(0.45)	(0.00)	(0.50)	(0.50)	(0.00)	(0.48)	(0.49)	(0.00)	(0.45)	(0.44)	(0.00)	(0.40)	(0.39)	(0.00)	(0.35)
Lives on the mainland	0.23	0.18	0.27	0.30	0.26	0.32	0.34	0.30	0.35	0.35	0.29	0.36	0.29	0.20	0.29
	(0.42)	(0.38)	(0.45)	(0.46)	(0.44)	(0.47)	(0.47)	(0.46)	(0.48)	(0.48)	(0.45)	(0.48)	(0.45)	(0.40)	(0.45)
Number of observations	27,554	11,692	15,862	39,059	11,425	27,634	53,371	8731	44,640	71,422	5716	65,706	70,138	3382	66,756

The sample consists of individuals born in Puerto Rico 1924–1963 from the 1980 and 1990 PUMS files with non-missing and non-allocated values for age, education, place of birth and English-speaking ability variables. Standard deviations are shown in parentheses. Years of exposure to English-intensive instruction is from Table A1.

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