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ABSTRACT

Research on the effect of language skills on earnings is complicated by the endogeneity of language skills. This study exploits the phenomenon that younger children learn languages more easily than older children to construct an instrumental variable for language proficiency. We find a significant positive effect of English proficiency on wages among adults who immigrated to the U.S. as children. Much of this impact appears to be mediated through education. Differences between non-English-speaking origin countries and English-speaking ones that might make immigrants from the latter a poor control group for non-language age-at-arrival effects do not drive these findings. (JEL J61, J24, J31)

I. Introduction

For both social and economic reasons, language is a barrier that separates many immigrants from natives. On the social side, immigrants who speak English poorly are more visibly foreign than others. This may facilitate discrimination on the part of natives, and contribute to social isolation and ghettoization. On the economic side, weak language skills probably reduce productivity and therefore increase the immigrant-native earnings gap. Moreover, strong language skills almost certainly increase the range and quality of jobs that immigrants can get. This view is supported by numerous empirical studies which suggest a positive association between English-language ability and earnings.[1]

Interest in the language skills of immigrants has been fostered in part by the upsurge in immigration to the United States in recent decades. The 2000 U.S. Census showed that 10.4 percent of the U.S. population is foreign born, up from 7.9 percent in 1990. Most of these recent immigrants are from non-English-speaking countries. In fact, the 2000 U.S. Census also showed that 47 million U.S. residents aged 5 and over spoke a language other than English at home and 21 million spoke English less than "very well".

Although language is central to the process of immigrant assimilation, and the relationship between language and earnings has been the subject of considerable research, the problem of measuring the causal effect of language skills on earnings is complicated by the fact that immigrants with stronger language skills may earn more for reasons other than these skills. Studies to date have relied primarily on simple regression strategies to control for confounding factors.

The contribution of this paper is the implementation of an identification strategy for the causal effect of language skills that is motivated by research on language acquisition. Younger

[1] See Section II for an overview of these studies.

children tend to learn languages easily while adolescents and adults do not. This psychobiological phenomenon leads us to use an instrumental variable derived from immigrants' age at arrival to their country of destination. As we show below, there is a powerful association between immigrants' age at arrival and language skills in the 1990 U.S. Census. On the other hand, age at arrival probably affects immigrant earnings through channels other than language. For example, immigrants who arrive earlier may adapt better to American institutions. We therefore use immigrants from English-speaking countries to control for non-language effects of age at arrival. The result is an instrumental variable (IV) strategy using age at arrival interacted with a dummy for non-English-speaking country as the identifying instrument.

To make this idea concrete, consider four immigrants, each brought to the U.S. as a child. Two are from Jamaica (an English-speaking country), one aged five at arrival and the other aged fifteen. The other two are from Mexico (a non-English-speaking country), with parallel ages of arrival. If we observe a difference between the wages of the two Jamaicans, we could attribute it to secular age-at-arrival effects. But all of these effects are also present in the case of the two Mexicans, *in addition* to the fact that the Mexicans had substantially less exposure to the English language before immigrating. As such, the Jamaicans can be used to control for the non-language age-at-arrival effects. Any differences between the Mexicans in excess of the differences between the Jamaicans can be attributed to *language* effects, *i.e.*, that the child who immigrated to the U.S. at an older age had a higher cost of acquiring a second language, and thus attained a lower level of proficiency in English.

Using individual-level data from the 1990 U.S. Census, we find that English-language skills have substantial, positive effects on wages and educational attainment. The IV estimates are higher than the ordinary least squares (OLS) estimates; the latter are subject to upward bias resulting from ability bias that is obscured by severe downward bias resulting from measurement error in the language skills variable. Most of the effect of language skills on wages appears to be mediated by the effect on years of schooling. This suggests that the role of language proficiency as an input to the production of human capital is far more important than the direct effect of language on the marginal product of labor.

The rest of the paper is organized as follows. Section II discusses the literature on the returns to language skills on the one hand and language acquisition on the other, and describes the data used in our empirical analysis. Section III presents the base results. Section IV performs some robustness checks and discusses some implications of the findings. Section V concludes.

II. Background and Data

A. Previous Research on Language Skills and Earnings

This study has several antecedents in the literature. One set of studies focuses on how long it takes for immigrant workers to achieve earnings parity with native-born workers (see Schultz (1998) and Borjas (1999) for reviews; also Friedberg (1993, 2000)). Their finding of an initial earnings disadvantage for immigrants that decreases with years in the host country is certainly consistent with the language skills hypothesis; however it is also consistent with numerous other explanations.

A second, related set of studies seeks to explicitly test the language skills hypothesis. Earlier studies tend to regress log earnings on some measure of language skills and interpret the OLS coefficient for the language variable as the labor market return to language skills (*e.g.*, McManus, Gould and Welch (1983), Kassoudji (1988), Tanier (1988) and Chiswick (1991)). More recent studies have attempted to address the problem of endogeneity in the relationship between language and earnings (*e.g.*, Chiswick and Miller (1995, 1998), Angrist and Lavy (1997), and Dustmann and van Soest (2002)).

Angrist and Lavy use an IV strategy based on a policy change in the schooling system of Morocco. However, the context of their "natural experiment" is quite different from ours: they estimate the return to speaking French in Morocco, an Arabic-speaking country, among native Moroccans. It is unclear that the lessons learned in their study can be readily extrapolated to the situation of immigrants in the U.S. labor market.[2]

Dustmann and van Soest as well as Chiswick and Miller analyze the returns to proficiency in the dominant language. Chiswick and Miller's identifying instruments include minority-language concentration of the place of residence, veteran status, whether married overseas and number of children. However, the excludability of their instruments from the wage equation has been debated (Borjas (1994)).[3] Dustmann and van Soest approach the potential problems of endogeneity and measurement error in the language skills measure using several instrumental variables techniques. Because they have panel data, they are able to correct for measurement error that is independent over time by using leads and lags of the language measure as instruments. To correct for endogeneity, they use parents' education as the identifying instruments; these exclusion restrictions on the wage equation might also be considered onerous.

A third set of studies has documented the low educational attainment among childhood immigrants. Individuals who immigrated from Mexico and Central America as children are much less likely than natives to complete high school and indeed even junior high school (Hispanic Dropout Project (1998) and Urban Institute (2000)). We are unaware of studies that

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[2] French is not the predominant language of Morocco, although as a vestige of the country's colonial history it continues to be used in the civil service and trade-oriented sectors. On the other hand, English is the dominant language of the U.S., and the lack of English-language skills impedes participation in a much broader range of jobs and sectors.

[3] For example, the concentration ratio is a region-of-residence variable, but region of residence is a choice variable, and regions with higher concentrations differ from regions with lower concentrations in a variety of ways, one of which is language. Moreover, regional characteristics correlated with the concentration ratio (*e.g.*, industrial composition, extent of ethnic businesses, extent of poverty) have direct effects on earnings. In general, one's region of residence, household composition, human capital investment and labor market decisions are jointly determined, *i.e.*, all outcomes of the same household utility maximization problem.

rigorously identify the determinants of the immigrant-native gap in educational attainment. Furthermore, we believe that the present study is the first to identify the contribution of language proficiency to earnings through pre-market factors such as education.

B. Language Acquisition Theory and Empirical Research

Our choice of instrument is motivated by the well-documented relationship between language acquisition and age in the psychobiological literature. Younger children learn languages more easily than adolescents and adults. Cognitive scientists refer to this as the "Critical Period Hypothesis". There is believed to be a critical age range in which individuals learn languages more easily and after which language acquisition is more difficult. If exposure to the language begins during the critical period, acquisition of the language up to "native" ability is almost automatic. If exposed afterwards, the individual's performance is less certain.

Behavioral evidence has been supportive of this hypothesis: late learners tend to attain a lower level of language proficiency (see Newport (1990) for a review). This appears to be linked to physiological changes in the brain (Lenneberg (1967)). Maturational changes starting just before puberty precipitate a sharp reduction in a child's ability to acquire second languages, especially with respect to sound production and grammatical structure, and to lesser extent vocabulary.

Applied to immigrants to the U.S., the Critical Period Hypothesis predicts that those who arrive at an earlier age will develop better English-language skills than those who arrive at a later age. We test this prediction after describing our data.

C. Data and Descriptive Statistics

We implement our empirical strategy using microdata from the 1990 U.S. Census, specifically the Integrated Public Use Microsample Series (IPUMS) files (Ruggles, *et al.* (1997)). We combine the 5 percent State sample with the 1 percent Metro sample. These samples contain information on each individual's age at arrival to the U.S., English-language proficiency[4], educational attainment, and labor earnings.

We restrict our attention to childhood immigrants, which we define as those immigrants who were under age 18 upon arrival to the U.S. For these individuals, age at arrival is not a choice variable since they did not time their own immigration but merely followed their parents to the U.S.[5] Year of arrival to the U.S. is reported in multi-year intervals, with more detailed intervals for the recent past.[6] Our definition of age at arrival is [current age – (1990 – maximum year of arrival)], so we are using the maximum possible age at arrival. We choose this conservative definition of age at arrival so as not to mistakenly include adult immigrants in our sample. Using this definition, over 35% of the foreign-born population in the U.S. are childhood immigrants.

We further restrict our sample to individuals arriving to the U.S. between 1960 and 1974, or equivalently, individuals who have been living in the U.S. for 16 to 30 years. We ignore more recent childhood immigrants because many of them would still be school-aged and not fully participating in the labor force by 1990. Our final restriction is that the individuals are between age 25 and 38 in 1990. Our age at arrival and year of arrival restrictions alone would limit the age range to 16-47. We narrow this range to get individuals on a more similar part of the experience-earnings profile. Our results are not sensitive to these particular sample-selection

[4] The Census question based on which the English-ability measures in this paper are constructed is: "How well does this person speak English? " with the four possible responses "very well," "well," "not well" and "not at all." This question is only asked of individuals responding affirmatively to "Does this person speak a language other than English at home?" We have coded immigrants who do not answer "Yes" to speaking another language as speaking English "very well." Other studies have used this question to study English proficiency, and have likewise coded immigrants who speak only English as speaking English very well (*e.g.*, Chiswick and Miller (1992, 1995)). The English-speaking ability measure is coded as 0 for not speaking English at all, 1 for speaking English not well, 2 for speaking English well and 3 for speaking English very well.

[5] According to the U.S. Immigration and Naturalization Service, immigrating parents may bring any unmarried children under age 21. We use a more restricted set of childhood immigrants: immigrants who were under 18 upon arrival (i.e., maximum age at arrival is 17). The results below are robust to excluding those who arrived in the United States after age 14 (who may have migrated of their on volition).

[6] Year of arrival to the U.S. data is reported in intervals, i.e., before 1950, 1950-1959, 1960-1964, 1965-1969, 1970-1974, 1975-1979, 1980-1981, 1982-1984, 1985-1986 and 1987-1990.

criteria.

We divide our sample into three mutually exclusive language categories: non-English-speaking countries of birth; countries of birth with English as an official language that have English as the predominant language; and other countries of birth with English as an official language.[7] The first category is our "treatment" group and the second is our "control" group. The last category is omitted from the main analysis, since we are not sure how much exposure to the English language immigrants from these countries would have had before immigrating.[8] Appendix Table 1 displays the categorization of countries, as well as the composition of our sample by national origin. Table 1 provides the descriptive statistics for the treatment and control groups, with decompositions by age at arrival.

III. Estimation Results

A. Reduced-form Estimation

Simple statistical techniques can be used to illustrate how the IV strategy based on age at arrival identifies the effect of English-language skills on wages. Consider the regression model,

(1) $y_{ija} = \alpha + \beta x_{ija} + \delta A_a + \gamma N_j + \varepsilon_{ija}$

for individual *i* born in country *j* arriving to the U.S. at age *a*. y_{ija} is log wages, x_{ija} is a measure of English-language skills (the endogenous regressor), A_a is a dummy for arrived young (age at arrival $\leq$ 11) and N_j is a dummy for born in a non-English-speaking country. Let z_{ija} denote the

[7] We used *The World Almanac and Book of Facts, 1999*, to determine whether English was an official language of each country. Recent adult immigrants from the 1980 Census were used to provide empirical evidence of the prevalence of English in countries with English as an official language. English-speaking countries are defined as those countries from which more than half the recent adult immigrants did not speak a language other than English at home. The remaining countries with English as an official language are excluded from the main analysis. We made two exceptions to this procedure. First, despite the fact that Great Britain was not listed as having an official language, we included it in the list of English-speaking countries. Second, we classified Puerto Rico as non-English speaking even though English is an official language due to its colonial history.

[8] Our results do not change when we include these omitted English-official countries. Because this group has had some intermediate level of exposure to English prior to arrival, when we estimate the regressions in Section III using it as the control and using the non-English-speaking countries as the treatment, the first stage and reduced-form coefficients are lower in magnitude, but the 2SLS coefficients are about the same.

binary instrument, the interaction between arrived young and born in a non-English-speaking country, *i.e.*, $z_{ija} = A_a * N_j$. The IV estimate of β in this equation is

$$(2) \qquad \beta_{IV} = \frac{(\bar{y}_{1,1} - \bar{y}_{0,1}) - (\bar{y}_{1,0} - \bar{y}_{0,0})}{(\bar{x}_{1,1} - \bar{x}_{0,1}) - (\bar{x}_{1,0} - \bar{x}_{0,0})}$$

where $\bar{y}_{1,0}$ is the mean of y_{ija} for those observations with $A_a = 1$ and $N_j = 0$; other terms are similarly defined. The numerator is the reduced-form relationship between y_{ija} and z_{ija}: the difference-in-difference of mean log earnings. The denominator is the reduced-form relationship between x_{ija} and z_{ija}: the difference-in-difference of mean English ability. The β_{IV} obtained from estimating Equation 1 using two-stage least squares (2SLS) is identical to the indirect least squares estimate obtained from taking the ratio of the reduced-form coefficients since Equation 1 is just-identified.

We emphasize that the identifying instrument is not age at arrival itself. Such an exclusion restriction seems difficult to justify *a priori*, since younger arrivers likely differ from older arrivers along non-language dimensions that also affect earnings. For example, in addition to having earlier exposure to English, younger arrivers are matriculated into the U.S. educational system at an earlier age. To the extent that human capital acquired in U.S. schools is better suited to the U.S. labor market, the younger arrivers would have an advantage that has nothing to do with language human capital (Friedberg (2000)). Also, younger children may face lower costs of assimilation along cultural dimensions that also have nothing to do with language *per se*. Furthermore, families that migrate with younger children may differ along some important margin from those that migrate with older children.

Instead, the identifying instrument is an interaction of age at arrival with country of birth. Incorporating immigrants from English-speaking countries into the analysis enables us to partial out the non-language effects of age at arrival. This is because upon arrival to the U.S., immigrants originating from English-speaking countries encounter everything that immigrants from non-English-speaking countries encounter except a new language. Thus, any difference in wages between young and old arrivers in non-English-speaking countries that is over and above the difference in English-speaking countries can plausibly be attributed to language.

The relationship between age at arrival and English-language skills is shown graphically in Figure 1. The diamond-marker line in Panel A displays the mean English-speaking ability for immigrants from non-English-speaking countries. Consistent with the research on language acquisition, children who received their first exposure to English at an earlier age attain a higher level of English-language proficiency than those who received it later. In fact, immigrants from non-English-speaking countries who arrive quite young (up until age 8 or 9) attain English-language skills comparable to those of immigrants from English-speaking countries. For later ages of arrival, however, their English-language skills are markedly lower. The square-marker line in Panel A displays the mean English-speaking ability of the immigrants from English-speaking countries. It is flat: nearly every immigrant from English-speaking countries speaks English very well.[9] This result is as predicted by the theory, since their first exposure to English does not depend on when they migrated to the U.S.

Older arrivers have statistically significantly lower English-speaking ability. Figure 1, Panel B displays the difference in mean English-speaking ability between immigrants from English- and non-English-speaking countries. This same result is summarized in Table 2. Early arrival from a non-English-speaking country translates into increases at each point in the cumulative distribution function (CDF) of English-speaking ability. The ordinal measure of English-speaking ability is 0.3124 units higher for early arrivers (Column 4).

Figure 2 shows the relationship between age at arrival and wages. Panel A shows the

[9] This line is not mechanically pinned at three because some of these countries have large non-English-speaking communities, e.g., the Quebecois in Canada.

mean log annual wages as a function of age at arrival for immigrants from non-English-speaking countries and for those from English-speaking countries. As in Figure 1, Panel A, the lines corresponding to the means of the two groups are similar at earlier ages at arrival and diverge for later ages. Among the younger arrivers, whether they come from non-English-speaking countries makes no significant difference in their wages. Among the adolescent arrivers, however, wages tend to be lower for the immigrants from non-English-speaking countries. The line for immigrants from English-speaking countries is nearly flat, suggesting that the non-language effects of age at arrival are small.[10] Panel B shows the difference in mean between the two groups. This differential drop in wages for older arrivers closely parallels the differential drop in English-speaking ability for older arrivers shown in Figure 1, Panel B.

The reduced-form effects of the binary instrument z_{ija} on language proficiency and earnings graphically depicted in Figures 1 and 2 can be used to construct a simple instrumental variables estimate of the returns to language. The average reduced-form effects are given in Table 2, Columns 4 and 5. Substituting these into Equation 2, we obtain an indirect least squares estimate of the returns to language: a one unit increase in English-speaking ability raises earnings 39 percent.[11]

In Table 2, note that the effect of the "arrived young" dummy variable is consistently positive. Simple-difference estimates with just immigrants from non-English-speaking countries would have overstated the effect of English-language skills by neglecting non-language age-at-arrival effects. Nevertheless, the non-language effects are much lower in magnitude than the language effects, suggesting that much of the assimilation process is through developing

[10] Alternatively, this might suggest that immigrants from English-speaking countries are a poor control group, since they do not capture all the non-language age-at-arrival effects that immigrants from non-English-speaking countries experience. In Section IV, we will attempt to enhance comparability between English- and non-English-speaking countries in a variety of ways.
[11] Numerator is from Column 5: 0.1221. Denominator is from Column 4: 0.3124. This estimate is merely illustrative, and in the next subsection we will regression-adjust for more variables.

destination-country language skills.

Investment in education may be an important intervening factor in the effect of language skills on earnings, as suggested by Figure 3. The pattern of years of schooling completed by age at arrival bears remarkable resemblance to the pattern of earnings by age at arrival. In examining the economic returns to language skills, therefore, it is essential to recognize that language can affect earnings through direct as well as indirect channels.

B. Two-Stage-Least-Squares Estimation

In this subsection, we drop the assumption that age at arrival is binary, and proceed to use age at arrival in a way that better captures the pattern of second-language acquisition in children. We use a parameterization that admits a degradation in language-learning ability that starts at age twelve and grows linearly: $\max(0, a_i - 11)$, in which a_i continues to be individual i's age at arrival. Of course, the key prediction is that the immigrants from English- and non-English-speaking countries have increasingly divergent language and wage outcomes starting at age-at-arrival twelve, so the instrument excluded from the second stage is $k_{ija} = \max(0, a_i - 11)*N_j$.[12] This piecewise-linear variable allows the difference between the control (English-speaking country of birth) and treatment (non-English-speaking country of birth) groups to grow starting just before the onset of puberty.

The aforesaid procedure is summarized by the following two-equation system. The second-stage equation relates the outcome of interest, wages, to the endogenous regressor, English-language skills. This is just Equation 1, which is modified here by the inclusion of a vector of exogenous explanatory variables w_{ija}:

(3) $y_{ija} = \alpha + \beta x_{ija} + \delta_a + \gamma_j + w_{ija}'\rho + \varepsilon_{ija}$.

The first-stage equation relates the endogenous regressor to the instrument k_{ija}:

[12] Results are not dependent on our particular parameterization of age at arrival. Appendix Table 2 presents results using alternative ways of defining the instrument.

(4) $x_{ija} = \alpha_1 + \beta_1 k_{ija} + \delta_{1a} + \gamma_{1j} + w_{ija}'\rho_1 + \varepsilon_{1ija}$.

This system is just-identified. δ_a is a full set of age-at-arrival fixed effects; this controls for non-language age-at-arrival effects in a finer way than just having a dummy for arriving young. The γ_j are a full set of country-of-birth fixed effects; this controls for cross-country differences more precisely than a single dummy for non-English-speaking origin. The first-stage regression results (from estimating Equation 4) are displayed in Table 3, Columns 1 and 2. There is a strong, negative relationship between the instrument k_{ija} and English-speaking ability. Immigrants who arrived from non-English-speaking countries have progressively poorer English skills for each year of arrival past age 11.

1. Effect of language skills on earnings

The results from estimating Equation 3 are displayed in the last four columns of Table 3. Columns 3 and 4 show the results using OLS and Columns 5 and 6 show the results using 2SLS. Column 6 suggests that on average, improving English-speaking ability by one unit increases log wages by 0.3335. Compared to a person who speaks English poorly (x_{ija} = 1), a person who speaks English well (x_{ija} = 2) earns 33 percent more and a person who speaks English very well (x_{ija} = 3) earns 67 percent more. This 2SLS estimate of the return to one unit of English-speaking ability is higher than its OLS counterpart (22.19 percent in Column 4). The OLS estimate appears to be downward biased, although it should be noted that its 95 percent confidence interval overlaps with the 95 percent confidence interval of the 2SLS estimate. This is nevertheless somewhat surprising, since the ability bias story implies higher OLS estimates than IV estimates; this issue is discussed in Section IV.C.

These results are robust to the exclusion of immigrants from Canada, who account for the largest share (40 percent) of immigrants from English-speaking countries. The concern is that immigrants from Canada are poor controls for the non-language age-at-arrival effects experienced by immigrants from non-English-speaking countries because of Canada's cultural and institutional similarity to the U.S. However, the IV estimate of the effect of language on earnings does not differ when we exclude Canadian immigrants from the analysis (compare panels A and B of Table 4). If, in addition, we excluded immigrants from England, Scotland, Wales, Australia and New Zealand – other countries that seem culturally similar to the U.S. – the IV wage effect is basically the same (see panel C). Further, we obtain similar estimates even if we restrict our analysis exclusively to immigrants from the Caribbean region (see panel D), where we find countries with more homogeneous social, economic and historical backgrounds. The results of Table 4 lend support to our difference-in-difference identification strategy and our interpretation of the 2SLS estimate as the return to language. We defer presenting additional robustness checks until Section IV.A.

2. Effect of language skills on educational attainment

Since instruction in U.S. classrooms is almost exclusively conducted in English, English-language skills can be expected to affect not only the quality of learning at each stage of schooling and but also the probability of progression to the next stage of schooling. Individuals who have poorer English-language skills effectively face a higher cost of education – it may be impossible to master the materials, or at the very least it requires more effort to do so.

The OLS estimate of the effect of English-language skills on educational attainment might be biased for the same reasons that the OLS estimate of their effect on wages might be biased. By using the exogenous variation provided by language-learning theory, we obtain a consistent estimate of the effect of English-language skills on educational attainment. The right-most columns Table 4 contain these results. We have estimated Equations 3 with years of schooling as the dependent variable. The OLS estimate (Column 3) suggests that increasing English-speaking ability by one unit raises years of completed schooling by two years. The

2SLS estimate (Column 4) is twice the OLS estimate: on average, a one unit increase in English-speaking ability raises educational attainment by four years.[13]

A complication for this interpretation is that many low-educated young men migrate on their own to the U.S. from Mexico and Central America to look for work. Among the older children from non-English-speaking countries, there may be a disproportionate number of low-educated immigrants who never intended (or were never able) to attend school in the U.S., and moreover who differ along other dimensions as well since they chose to migrate on their own. To address the problem of loner immigrants, we restrict our analysis to those who arrived to the U.S. at age fourteen or younger, *i.e.*, we drop the fifteen to seventeen-year-olds. Our results are qualitatively similar for both earnings and schooling, although the point estimate is 18% smaller for schooling. This suggests that what we observe is truly an effect of language and not due to the independent and self-selected migration of young adults.

IV. Interpretation

In this section, we discuss the interpretation of our findings. Section A addresses further the concern that the differential age-at-arrival effects for non-English-speaking countries may not be due to language, but to some omitted factor that co-varies with age at arrival in the same way. Our findings survive a variety of robustness checks. We proceed in Section B to discuss the role of investments in education human capital in the effect of language proficiency on wages. Finally, Section C explores the "puzzle" of why the IV estimates are higher than the OLS estimates of the return to language skills.

A. Additional Specification Checks

We have been interpreting the age-at-arrival effect for immigrants from non-English-

[13] In addition to affecting the mean years of schooling completed, language proficiency had a concentrated impact on particular points of the distribution of educational attainment, notably drop-out behavior in secondary school. We treat the effects on the whole distribution of educational attainment in an earlier version (Bleakley and Chin, 2002) of the present study.

speaking countries that is in excess of the age-at-arrival effect for immigrants from English-speaking countries as the causal effect of English-language proficiency. However, if non-language age-at-arrival effects differ between the two groups of immigrants, then our strategy to identify the effect of English-language proficiency is invalid. In this subsection, we consider two hypotheses for differential age-at-arrival effects between the two groups of immigrants that have nothing to do with the causal effect of language skills.

1. How comparable are treatment and control countries?

The first alternative hypothesis is that immigrants from non-English-speaking countries exhibit a stronger age-at-arrival effect simply because immigrants from poorer countries face additional barriers to adaptation and that these barriers increase in severity as a function of age at arrival. This is plausible because non-English-speaking countries tend to be poorer than English-speaking countries (see in Appendix Table 1). Richer countries might have better school systems. If there are different returns associated with the schooling obtained in a non-English-speaking country versus an English-speaking one, the 2SLS estimate may reflect not only differential English-language skills but also differential returns to origin-country schooling.[14]

To assess this hypothesis, we control explicitly for characteristics of the country of birth in the regression models. The country data that we employ are the 1965 levels of GDP per capita, per pupil school expenditures and teacher-pupil ratio.[15] These correlates of origin-country school quality are included in the regression specification as interactions with age at arrival. Table 5, panels B, C and D show the estimation results from adding these school quality interactions one by one. The principal finding is that although the school quality interactions

[14] Immigrants who arrived at a younger age systematically receive a lower share of their schooling in their origin country. Friedberg (2000) finds that, among immigrants to Israel, there is a lower return to schooling obtained abroad than to schooling obtained in Israel. This, in and of itself, provides a strong additional justification for including a main effect of age at arrival. However, for this to impact our strategy, the effect has to vary between the control and treatment groups.

[15] These are from the data sets constructed and described by Barro and Lee (1997) and Summers and Heston (1988).

enter significantly in the first stage (displayed in Column 1) and reduced-form (not reported) equations, the coefficient for our identifying instrument k_{ija} remains significant. The 2SLS estimates of the return to English-speaking ability remain around 30 percent. (We perform the same analysis with years of schooling instead of earnings as the dependent variable, and the estimated effect remains around four years.) In Panel E, we allow the treatment effect and the effect of the control variables to differ between immigrants from countries with below-median GDP and immigrants from countries with above-median GDP. The first stage results in Column 1 indicate that the instrument has a weaker effect on immigrants from richer countries. (The reduced-form effect (not reported) is also smaller.) It is possible that in richer countries compulsory schooling laws and better school quality help offset some of the disadvantages of arriving in the U.S. at a later age. The OLS and 2SLS estimates of the return to English proficiency are lower among immigrants from richer countries, as shown in Columns 2 and 3. However, this differential return between poorer and rich countries is not significantly different in the case of the 2SLS estimates.

2. Do parents factor in child's language-learning ability in the migration decision?

The second alternative hypothesis is that parents from non-English-speaking countries may factor their children's ages into the migration decision in a way that is different from parents from English-speaking countries. For example, the former may systematically enter when their children are younger because they realize the language-learning disadvantage their children would suffer if they do otherwise. Because of this, the distribution of parental characteristics across age at arrival may differ between English- and non-English-speaking countries. The 2SLS estimate may reflect not only the true effect of English-language proficiency, but also, the effects of differences in parental characteristics.

To assess this, we compare the age-at-arrival distribution of the treatment and control groups. Figure 4 displays this distribution. Each point on the diamond-marker (square-marker) line gives the proportion of the immigrants from non-English-speaking countries (English-speaking countries) that arrived in the U.S. at that particular age. It is not the case that parents from non-English speaking countries are *more* likely than parents from English-speaking countries to migrate when their children are very young, understanding that older children have a language-learning disadvantage. Had this been the case, there would have been more mass in the younger ages for the immigrants from non-English-speaking countries. Figure 4 shows that the reverse is true in our sample. Indeed, a regression of arriving young (up to age 11) on non-English speaking country and controls reveals that childhood immigrants from non-English speaking countries are 1.8% less likely to arrive young compared to those from English-speaking countries.[16] This difference is statistically significant at the 99 percent level of confidence. If we dropped the fifteen to seventeen-year-olds from the analysis – these immigrants may have come on their own accord, without their parents – there is no statistically significant difference.

B. Contribution of Education to the Effect of English-Language Skills on Wages

To what extent is the large and positive effect of English-language proficiency on education generating the large and positive effect of English-language proficiency on wages? In this subsection, we address this issue by incorporating education directly into the wage regressions from above.

Table 6 displays the analysis incorporating education. As a reference point, we report in Column 1 the 2SLS coefficient for the English-language measure in our base specification: a one unit increase in English-speaking ability brings about a 0.33 increase in log wages. In Columns 2 to 5, we partial out the effect of schooling on wages using rates of return suggested by previous research. In Columns 6 and 7, we treat educational attainment as an exogenous control variable.

[16] The controls are age, race/ethnicity and female dummies.

We obtain coefficients for the English-language measure that are lower by at least a factor of three. Using returns to schooling closer to those favored by our data, we find the estimated effect is lower by about a factor of ten (see Column 4). That is, approximately 90 percent of the effect of English-language skills on wages works through changing educational attainment. The remaining 10 percent may be due to other channels, such as the improved ability to communicate with customers and co-workers, although we cannot reject the hypothesis that *all* of the wage effect is mediated by schooling.

C. Comparing the OLS and IV Estimates

One puzzle regarding our results is that IV estimate of the return to language skills is higher than the OLS estimate; a model in which omitted ability affects both earnings capacity and language acquisition suggests the reverse. In this subsection, we discuss two potential explanations: measurement error in the language skills measure and differences in the weighting function underlying the OLS and IV estimates.

1. Is IV capturing individuals at a different part of the distribution than OLS?

First, the IV estimate uses only the variation in language skills that is induced by the instrument whereas the OLS estimate uses all the variation. If the marginal return to language skills for individuals affected by the instrument differs systematically from that of the population, then the coefficient estimated using OLS will differ from that using IV (Angrist and Imbens (1995)). It may be that the return to moving from speaking English "not at all" to speaking "not well" is different from the return from moving from "well" to "very well". Recall from Table 2 that the binary instrument shifts the CDF up (towards higher English-language proficiency) at every point in the distribution. Nevertheless, the largest effect of arriving in the U.S. at a young age is to bring individuals who speak English well across the margin to very well. Thus, IV would yield a higher estimate than OLS if the greatest gains from language proficiency come from later steps towards proficiency. However, in our sample, OLS estimates of the marginal return at each point of English-language proficiency do not suggest nonlinearities in the returns to language skills.[17] Thus there is no direct support for the idea that the higher IV estimate is due to a simple reweighting of heterogeneous effects.

2. What is the extent of measurement error?

Second, measurement error in the language skills measure may affect the OLS and IV estimates differentially. The language measure used in this paper is an ordinal measure with four categories (0 to 3), which we denote as x (in this subsection, we suppress subscript i). It is likely measured with error because it is based on each individual's self-assessment of his/her English-speaking ability, and measured in only a few discrete categories. Let x^* be the individual's true, latent language skills. Suppose the true relationship between log wages (y) and language skills is

(5) $y = \alpha + \beta x^* + \varepsilon$

(for expositional convenience, we present a bivariate form of Equation 3). Further suppose that Equation 5 satisfies the assumptions of the classical regression model. However, the researcher estimates the model using x instead of x^*. The OLS estimate of β will tend to be biased. To see this, we first write down a linear relationship between x and x^*:

(6) $x = \lambda (x^* + u)$,

where the means have been removed. The λ is merely a scale factor allowing for x and x^* to be measured in different units. Then we can calculate the asymptotic value of the OLS estimate of β in Equation 5:

[17] We estimate the specification in Table 3, Column 4 but replace the English ability ordinal measure with dummies for each value of the ordinal measure. The OLS coefficients are 0.1911 (standard error of 0.0524) for moving from no English to speaks English not well, 0.2661 (0.0264) for moving from not well to well, and 0.2031 (0.0153) for moving from well to very well. An F-test cannot reject the null hypothesis that the three coefficients are equal.

(7) plim $b_{OLS} = \frac{\beta}{\lambda}\left[\frac{Var(x^*) + Cov(x^*,u)}{Var(x^*) + Var(u) + 2Cov(x^*,u)}\right]$.[18]

In the case of classical measurement error (*i.e.*, Cov(x*,u) = 0), we get the standard result of attenuation bias in the OLS estimate. The greater the noise (Var(u)), the farther the term in brackets is from one, and thus the greater the bias towards zero. On the other hand, when we instrument for the language measure, we eliminate the attenuation bias, thus leading to a higher IV estimate. Thus, classical measurement error can explain why our IV estimate of the returns to language is higher than our OLS estimate.

To get an idea of the magnitude of the attenuation bias, we turn to an external data source that has a higher quality measure of English-language skills. The 1992 National Adult Literacy Survey (NALS) was designed to study the nature and extent of literacy among adults in the U.S. (see National Center for Educational Statistics (1997)).[19] Respondents answered background questions (including the Census language question verbatim) and took a 45-minute literacy test. The literacy test score is an appealing measure of English-language skills because it is based on an objective test (instead of a self-assessment), and also because it is measured in finer gradations (instead of four broad categories). To proceed, we treat the ordinal Census-style measure of language skills as the noisy measure of language skills – this is x. The range is 0 to 3 (integer values only), mean is 2.44 and variance is 0.715. We treat the literacy test score as the true measure of language skills – this is x^*.[20] To correct the OLS estimate of the effect of

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[18] So $u = x/\lambda - x^*$. Note that in cases where x and x^* are measured in the same units (e.g., multiple reportings of years of schooling), $\lambda = 1$, which leads to the more familiar $u = x - x^*$. There is no obvious scale to measure English-language skills, as opposed to, say, years of schooling, hence we introduce the parameter λ.

[19] We do not use the NALS for all our analysis because of the paucity of observations. The NALS surveyed approximately 13,000 individuals, but less than 300 satisfy all the data restrictions described in Section II. The NALS data used below has 266 observations. They are immigrants from non-English-speaking countries who arrived to the U.S. between 1962 and 1981 and are currently aged 23 to 38. We require non-missing literacy test score and self-assessment of English-speaking ability, but not non-missing wages.

[20] We can also let the test score measure be a noisy measure of true language skills. If the measurement errors are correlated with each other or the error in the wage regression, *e.g.*, if the two language variables measure different

language on earnings for classical measurement error, we use Equation 7, setting Cov(x*,u) = 0. We calculate the moments used in Equation 7 with NALS data: Var(λx*) = 0.298, V(λu) = 0.417 and λ=0.008.[21] The term in brackets is 0.42. In Table 2, the OLS estimate of the return to language was 22% – this is λb_{OLS}. Thus the corrected OLS estimate (in the same units as *x*) is 53% (22% = 53%*0.42). Attenuation bias appears to be severe, so much so that the corrected OLS estimate is more than twice the original OLS estimate and higher than the IV estimate. This is consistent with Dustmann and van Soest (2002), who find that estimates of the effect of language on earnings that account for classical measurement error are two to three times larger than the uncorrected OLS estimate.

Non-classical measurement error, with Cov(x*,u) $\neq$ 0, might also be a concern when using Census-based language variables. On one hand, a positive Cov(x*,u) might arise from rounding a continuous measure of language ability to the nearest discrete category (Berman, Lang and Siniver (2000)). It is plausible that *x** is a continuous variable, or at least takes on more than four values. If the distribution of *x** has a central tendency, then above the mean there will be a higher frequency of rounding down than rounding up (and thus, more positive residuals than negative ones), and the reverse is true below the mean. On the other hand, self-reporting can lead to misreporting of language skills. First, individuals with better language skills may simply be better able to accurately assess their proficiency, leading to an inverse relationship between *x** and measurement error. Second, to the extent that there are many people at the bounds, then there will be a negative relationship between *x** and *u*: at the lower bound, measurement error will more likely be too positive (individuals have less room to under-report)

abilities, then even the "corrected" estimates would still have bias. We emphasize that the following analysis using NALS data should be viewed as suggestive rather than definitive evidence on the role of measurement error. What is important is the test score appears to be a higher quality measure of language skills. In our sample, the literacy test score ranges from 68 to 390.

[21] The 0.008 is the coefficient on *x** when Equation 6 is estimated using OLS. Note that when Cov(x*,u) = 0, OLS provides the best linear unbiased estimate of λ.

and at the upper bound, it will more likely be too negative (individuals have less room to over-report).[22] This might be a serious concern considering that 83% of immigrants from non-English-speaking countries in our sample report having the highest category of English-language skills.

When Cov(x*,u) ≠ 0, the OLS estimate will biased as shown in Equation 7. In addition, the IV estimate will also be biased. Let *k* be an instrument for language skills, satisfying the criteria Cov(k,x*) ≠ 0 and Cov(k,ε) = 0. Write *k* as

(8) $$k = \pi x^* + q$$

and let the error terms (ε, u and q) be uncorrelated. The IV estimate is just the indirect least squares estimate (*i.e.*, the ratio of the reduced-form effect on earnings and the reduced-form effect on language), and it can be shown that

(9) $$\text{plim } b_{IV} = \frac{\beta}{\lambda}\left[\frac{\pi Var(x^*) + Cov(x^*,q)}{\pi Var(x^*) + \pi Cov(x^*,u) + Cov(x^*,q)}\right].$$

If the misreporting induces a negative correlation between x* and u that exceeds the positive correlation induced by the rounding, then non-classical measurement error can help explain why the IV estimate is higher than the OLS estimate – OLS is downward biased and IV is upward biased. To get a rough idea of the magnitude of the bias in the OLS and IV estimates caused by the non-classical measurement error, we again use the NALS data. Since there is no obvious way to map the continuous literacy test score (x^*) into qualitative categories, we tried several different methods. One simple but plausible method would be to rescale x^* by assigning the value of zero (lowest level of English-language proficiency according to the scale of our ordinal measure) to the lowest test score and three (highest proficiency) to the highest test score

[22] Naturally, if misreporting tends to occur only in particular parts of the language distribution or in a particular direction, then the sign of the bias on the IV estimate is ambiguous (for example, see Kane *et al.* (1999) and Black *et al.* (2000)).

while preserving the relative values of x^*. This results in a continuous measure of true English-language skills that ranges from 0 to 3. We obtain Var(λx^*) = 0.384, V(λu) = 0.423, Cov($\lambda x^*,\lambda u$) = –0.046 and λ=0.009.[23] Using Equation 7, the corrected OLS estimate (in the same units as x) would be 47%. It turns out that classical measurement error dominates non-classical measurement error as a source of bias in the original OLS estimate.[24] To correct the IV estimate for non-classical measurement error (recall that when there is only classical measurement error, IV provides a consistent estimate), we use Equation 9. For this particular rescaling of x^*, the term in brackets is 1.1, and thus the corrected IV estimate is 30%. Results are similar when we use a method of scaling x^* that leads to the same proportion of individuals being placed in the top and bottom integer value for x^* as for x.[25] Our analysis suggests that, even accounting for non-classical measurement error, there is still a substantial effect of English-language proficiency on earnings.

Removing biases caused by measurement error, we find that the IV estimate is lower than the OLS estimate by ten to twenty percentage points. This difference may be attributable to the fact that the OLS estimate does not correct for endogeneity while the IV estimate does. The upward bias of the OLS estimate is consistent with a significant role for the ability bias story. This upward bias is apparently masked by the severe downward bias associated with measurement error in the language variable based on the Census language question. Since many

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[23] We set Cov(x^*,q) = 0, that is the projection of the true measure of language skills on the instrument is uncorrelated with the error term. This seems valid, as the instrument is an interaction between age at arrival and country of origin.

[24] The OLS estimate corrected for classical but not non-classical measurement error is lower by only a few tenths of a percent compared to the fully corrected estimate. This is because Cov(x^*,u) is so small in magnitude relative to the total noise.

[25] In the NALS data, 63% of the individuals have $x = 3$ and 4% have x=0. We calculate the test score at the 27th percentile, and assign the individual with this test score an x^* of 2.5 – this is the lowest x^* that can be rounded into the top reported category, $x = 3$. We calculate the test score at the 5th percentile, and assign the individual with this test score an x^* of 0.5 – this is the lowest test score that can be rounded into $x = 1$, with even lower scores placed into x = 0. Based the linear relationship implied by these two points, we can map every single test score into x^*. Then for any predicted x^* below 0, we assign $x^* = 0$, and for any predicted x^* above 3, we assign $x^* = 3$. For this particular rescaling of x^*, the corrected OLS estimate would be 35% and the corrected IV estimate would be 25%.

researchers studying the effects of language skills rely on data sets with the same survey instrument to measure language, this finding has widespread implications.[26] In particular, it would be difficult to make inferences about the effects of language skills without addressing both endogeneity and errors-in-variable.

V. Conclusions

We find a significant positive effect of English-language skills on wages among individuals from the 1990 Census who immigrated to the U.S. as children. The estimated effect using our IV strategy is greater in magnitude than that suggested by regression strategies that do not address endogeneity and measurement error. We find evidence of substantial downward bias in the OLS estimate due to measurement error and somewhat smaller upward bias due to endogeneity.

Much of the effect of English-language skills appears to be mediated by years of schooling. Better English-language skills induce immigrants who would otherwise drop out with the equivalent of junior high or some high school education to at least complete their high school degree.

Our findings suggest that timing of migration and its effect on English-language skills are critical to a variety of important outcomes, and policymakers should be cognizant of this. Since much of the effect of English-language skills is through increased years of schooling, adult English-language classes may be insufficient to help these immigrants' wages to converge to those of natives. Instead, programs aimed at junior-high-school-aged and high-school-aged children may be more effective. Future work will explore in greater detail the policies and programs that may be most effective in mitigating the effect of poor English skills on the school-drop-out rates of immigrants.

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[26] The censuses of various other countries use the U.S. Census language question, including Australia, Canada and Israel. Additionally, the Current Population Survey in the U.S. also uses the Census language question.

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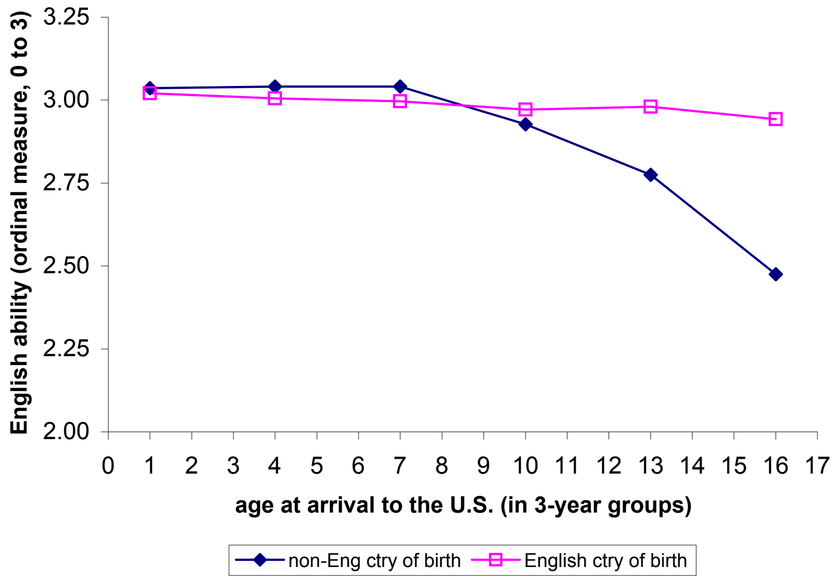
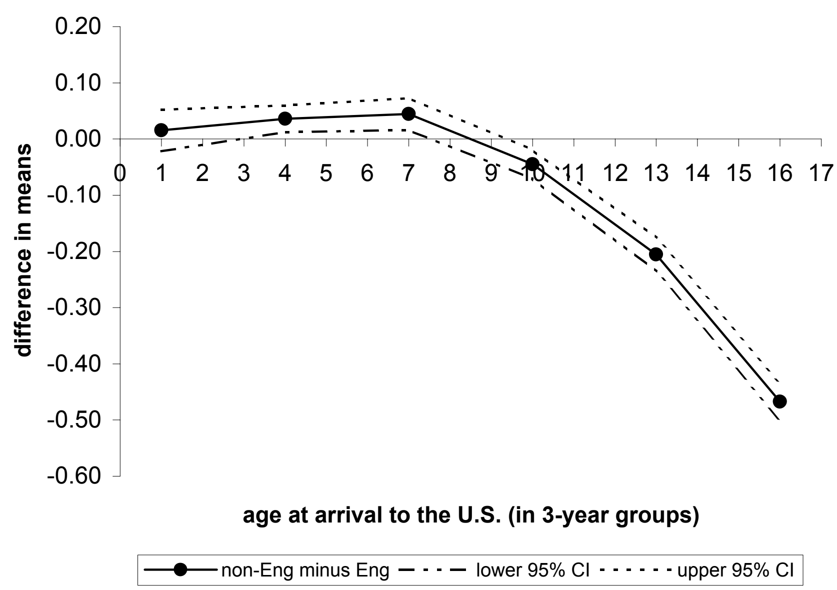
Figure 1. English-Speaking Ability by Age at Arrival

Panel A. Regression-Adjusted Means



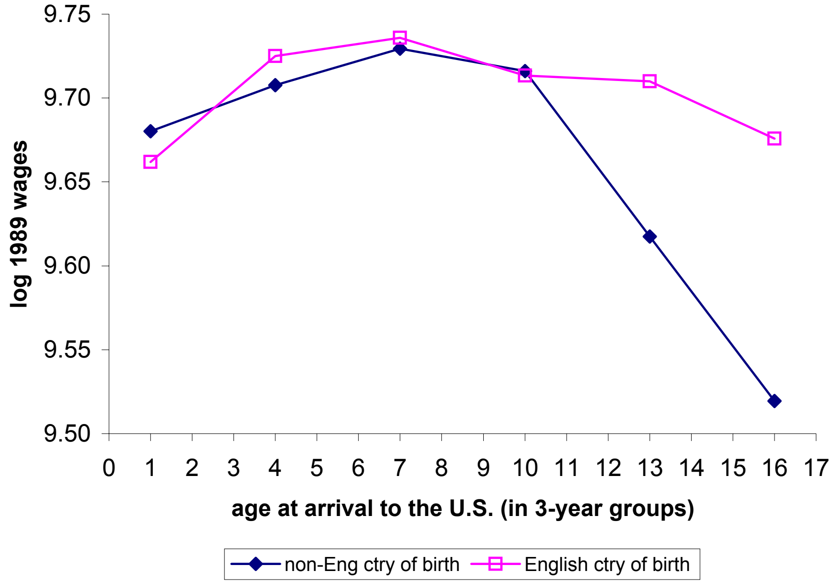
Panel B. Difference in Means



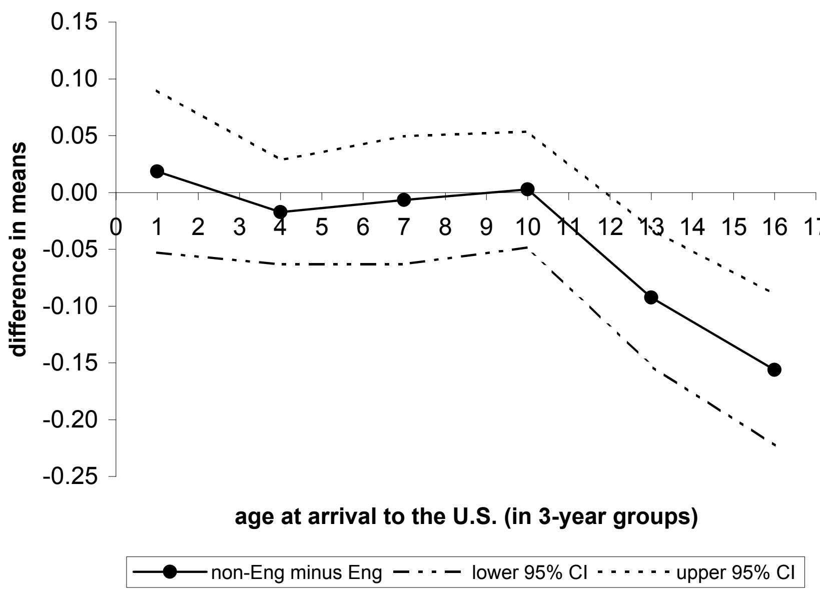
Notes: Data from 1990 IPUMS. Sample size is 66,584 (comprised of individuals who arrived to the U.S. by age 17 between 1960 and 1974 and currently aged 25 to 38). English ordinal measure: 0 = no English, 1 = not well, 2 = well and 3 = very well. Means have been regression-adjusted for age, race/ethnicity and female dummies.

Figure 2. Log Annual Wages by Age at Arrival

Panel A. Regression-Adjusted Means



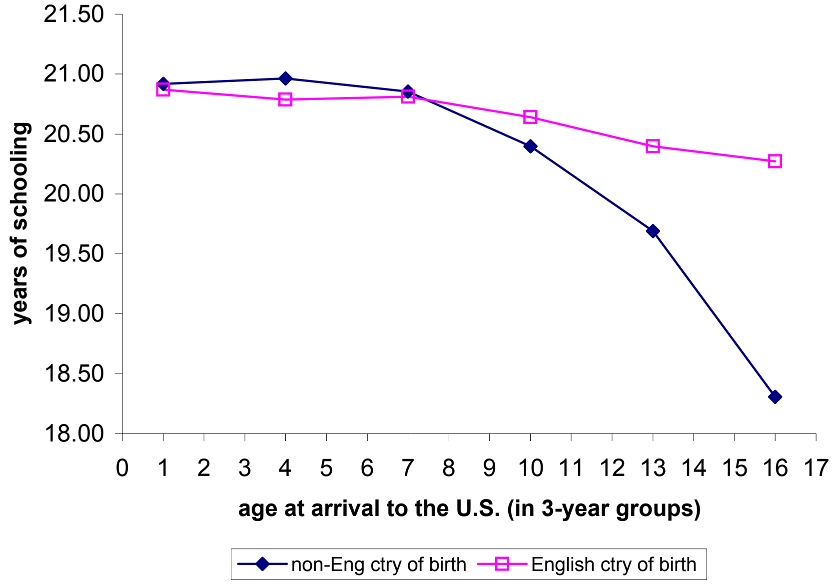
Panel B. Difference in Means



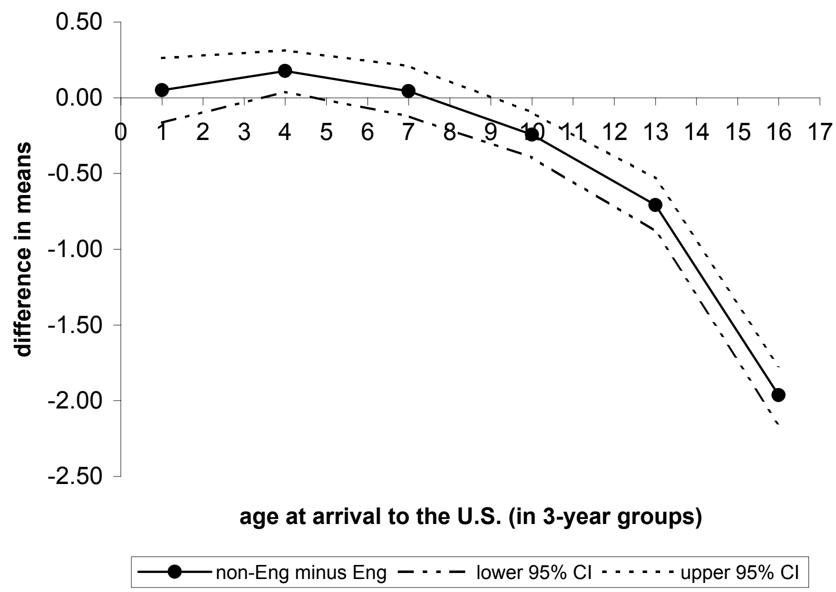
Notes: Data from 1990 IPUMS. Sample size is 47,422 (comprised of individuals who arrived to the U.S. by age 17 between 1960 and 1974 and currently aged 25 to 38). Means have been regression-adjusted for age, race/ethnicity and female dummies.

Figure 3. Years of Schooling by Age at Arrival

Panel A. Regression-Adjusted Means

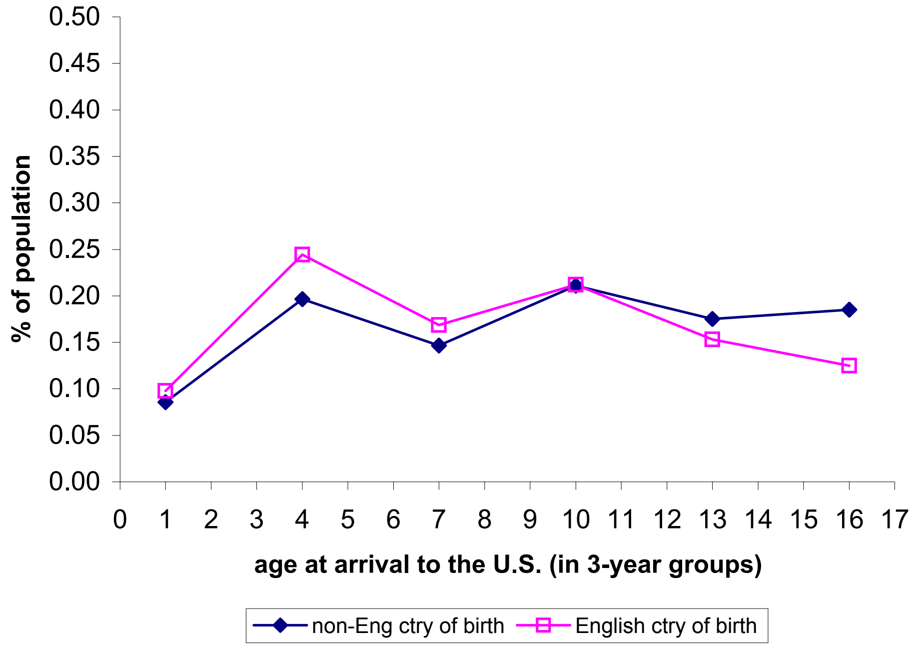


Panel B. Difference in Means



Notes: Data from 1990 IPUMS. Sample size is 65,214 (comprised of individuals who arrived to the U.S. by age 17 between 1960 and 1974 and currently aged 25 to 38). Means have been regression-adjusted for age, race/ethnicity and female dummies.

Figure 4. Probability Distribution Function of Age at Arrival



Notes: Data from 1990 IPUMS. Sample size is 66,584 (comprised of individuals who arrived to the U.S. by age 17 between 1960 and 1974 and currently aged 25 to 38), of which 57,106 are from a non-English-speaking country of birth and the remaining 9,478 are from an English-speaking country of birth.

Table 1. Descriptive Statistics

	immig from non-English-spking ctries			immig from English-spking ctries		
	overall	arrived aged 0-11	arrived aged 12-17	overall	arrived aged 0-11	arrived aged 12-17
	(1)	(2)	(3)	(4)	(5)	(6)
log annual wages	9.6699	9.6723	9.6652	9.7648	9.7363	9.8426
	(0.9449)	(0.9424)	(0.9499)	(0.9537)	(0.9573)	(0.9397)
English-speaking ability variables						
ordinal measure (scale of 0 to 3, 3=best)	2.7693	2.8928	2.5259	2.9863	2.9858	2.9878
	(0.5545)	(0.3746)	(0.7397)	(0.1323)	(0.1383)	(0.1143)
speaks English not at all (0)	0.0083	0.0024	0.0200	0.0000	0.0000	0.0000
	(0.0909)	(0.0491)	(0.1400)	(0.0000)	(0.0000)	(0.0000)
speaks English not well (1)	0.0399	0.0151	0.0889	0.0020	0.0026	0.0005
	(0.1958)	(0.1219)	(0.2846)	(0.0448)	(0.0507)	(0.0222)
speaks English well (2)	0.1258	0.0698	0.2363	0.0096	0.0090	0.0112
	(0.3317)	(0.2548)	(0.4248)	(0.0977)	(0.0947)	(0.1054)
speaks English very well (3)	0.8259	0.9127	0.6548	0.9884	0.9884	0.9883
	(0.3792)	(0.2822)	(0.4754)	(0.1073)	(0.1072)	(0.1077)
control variables						
age at arrival	8.9789	6.1663	14.5168	8.2438	6.0229	14.3058
	(4.8341)	(3.1853)	(1.7770)	(4.6251)	(3.1179)	(1.7415)
age	30.4483	29.1236	33.0567	30.1490	29.1121	32.9793
	(3.6630)	(3.1822)	(3.1048)	(3.5596)	(3.1151)	(3.1408)
white	0.8893	0.8927	0.8825	0.7243	0.8163	0.4732
	(0.3138)	(0.3095)	(0.3220)	(0.4469)	(0.3873)	(0.4994)
black	0.0425	0.0429	0.0418	0.2478	0.1603	0.4864
	(0.2017)	(0.2025)	(0.2002)	(0.4317)	(0.3670)	(0.4999)
Asian/other non-white race	0.0682	0.0644	0.0757	0.0279	0.0234	0.0405
	(0.2521)	(0.2455)	(0.2645)	(0.1648)	(0.1511)	(0.1971)
Hispanic	0.5394	0.4744	0.6674	0.0170	0.0149	0.0227
	(0.4985)	(0.4994)	(0.4711)	(0.1293)	(0.1213)	(0.1489)
female	0.4559	0.4657	0.4367	0.4937	0.4801	0.5309
	(0.4981)	(0.4988)	(0.4960)	(0.5000)	(0.4997)	(0.4992)
schooling variables						
years of schooling	13.0773	13.6567	11.9282	14.2124	14.2324	14.1576
	(3.2525)	(2.6293)	(3.9828)	(2.2605)	(2.2370)	(2.3233)
completed high school	0.7979	0.8718	0.6514	0.9432	0.9433	0.9430
	(0.4016)	(0.3343)	(0.4765)	(0.2314)	(0.2313)	(0.2319)
completed college	0.2391	0.2684	0.1812	0.3276	0.3380	0.2991
	(0.4266)	(0.4431)	(0.3852)	(0.4694)	(0.4731)	(0.4580)
Number of observations	40,258	26,490	13,768	7,164	5,309	1,855
N for schooling variables	39,647	26,154	13,493	7,097	5,260	1,837

Notes: Means weighted by IPUMS weights. Standard deviation in paretheses. Sample is as follows: 1990 IPUMS, arrived to the U.S. by age 17, between 1960 and 1974, is currently aged 25 to 38 and with nonmissing language and wage variables.

Table 2. Difference-in-Differences with Binary Treatment Variable

dependent variable *mean for old & non-Eng. ctry.*	Speaks English not well, well or very well *0.9800*		Speaks English well or very well *0.8911*		Speaks English very well *0.6548*		English Ability ordinal measure *2.5259*		Log Annual Wages *9.6652*	
	(1)		(2)		(3)		(4)		(5)	
arrived young*non-English-speaking country of birth	**0.0142**	***	**0.0794**	***	**0.2188**	***	**0.3124**	***	**0.1221**	***
	(0.0012)		**(0.0031)**		**(0.0060)**		**(0.0082)**		**(0.0302)**	
arrived young (aged 0 to 11)	0.0018	***	0.0007		0.0101	**	0.0125	**	0.0206	
	(0.0006)		(0.0021)		(0.0045)		(0.0059)		(0.0295)	
non-English-speaking country of birth	-0.0109	***	-0.0681	***	-0.2178	***	-0.2968	***	-0.1277	***
	(0.0009)		(0.0027)		(0.0058)		(0.0075)		(0.0271)	
Adjusted R-squared	0.0149		0.0680		0.1851		0.1618		0.0796	

Notes: Weighted by IPUMS weights. Robust standard errors in parentheses. Single asterisk denotes statistical significance at the at the 90% level of confidence, double 95%, triple 99%. English-speaking ability ordinal measure is defined as: 0 = no English, 1 = not well, 2 = well and 3 = very well. Sample is as follows: 1990 IPUMS, arrived to the U.S. by age 17 between 1960 and 1974, is currently aged 25 to 38 and with nonmissing language and wage variables. The number of observations is 47,422 for each column. In addition to the regressors listed above, all specifications also include age, race/ethnicity (White, Black, Hispanic, Asian & Other) and sex dummies.

Table 3. Effect on Log Annual Wages -- Base Results

dependent variable	**English Ability**				**Log Annual Wages**							
mean for non-English-spking ctry.	*2.7693*				*9.6699*							
	OLS		OLS		OLS		OLS		2SLS		2SLS	
	(1)		(2)		(3)		(4)		(5)		(6)	
endogenous regressor												
English-speaking ability					0.2225	***	0.2219	***	0.3286	***	0.3335	***
(scale of 0 to 3, 3=best)					(0.0093)		(0.0093)		(0.1060)		(0.1054)	
identifying instrument												
max (0, age at arrival - 11) * non-English-speaking country of birth	-0.0771	***	-0.0776	***								
	(0.0021)		(0.0021)									
controls												
max (0, age at arrival - 11)	-0.0022	***			-0.0121	***			-0.0047			
	(0.0014)				(0.0030)				(0.0082)			
Age at arrival dummies	No		Yes		No		Yes		No		Yes	
Country of birth dummies	Yes		Yes		Yes		Yes		Yes		Yes	
Adjusted R-squared	0.2328		0.2352		0.1125		0.1125					

Notes: See notes for Table 2.

Table 4. Effect on Wages and Schooling -- Alternative Countries in Sample

dependent variable *mean for non-English-spking ctry.*	**Log Annual Wages** *2.7693*		**Years of Schooling** *13.0773*	
	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)
Panel A. All Countries (Base)				
English-speaking ability	0.2219 ***	0.3335 ***	2.0044 ***	3.9572 ***
	(0.0093)	(0.1054)	(0.0359)	(0.2798)
N	47,422	47,422	46,744	46,744
Panel B. Excluding immigrants from Canada				
English-speaking ability	0.2220 ***	0.3285 ***	2.0077 ***	4.1359 ***
	(0.0094)	(0.1274)	(0.0360)	(0.3043)
N	44,567	44,567	43,920	43,920
Panel C. Excluding immigrants from Canada, England, Scotland, Wales, Australia and New Zealand				
English-speaking ability	0.2226 ***	0.2853 *	2.0085 ***	4.2052 ***
	(0.0094)	(0.1608)	(0.0360)	(0.3780)
N	42,163	42,163	41,531	41,531
Panel D. Caribbean Countries Only				
English-speaking ability	0.2204 ***	0.4393 *	1.5825 ***	2.2961 ***
	(0.0258)	(0.2350)	(0.0788)	(0.5469)
N	9,953	9,953	9,782	9,782

Notes: See notes for Table 2. In addition to the regressors listed above, all specifications also include age at arrival, country of birth, age, race/ethnicity and sex dummies. Also, the 2SLS estimate is obtained using the variable max (0, age at arrival - 11) * non-English-speaking country of birth to instrument for the endogenous regressor, English-speaking ability.

Table 5. Effect on Log Annual Wages -- School Quality Controls

	1st stage	OLS	2SLS	N
	(1)	(2)	(3)	(4)
Panel A. Base (from Table 3)				
English-speaking ability		0.2219 ***	0.3335 ***	47,422
(scale of 0 to 3, 3=best)		(0.0093)	(0.1054)	
max (0, age at arrival - 11) * non-English-speaking country of birth	-0.0776 ***			
	(0.0021)			
Panel B. Control for GDP in Country of Birth				
English-speaking ability		0.2208 ***	0.3317 ***	40,552
(scale of 0 to 3, 3=best)		(0.0097)	(0.0986)	
max (0, age at arrival - 11) * non-English-speaking country of birth	-0.0908 ***			
	(0.0029)			
max (0, age at arrival - 11) * ln(per capita PPP GDP)	-0.0146 ***	0.0032	0.0031	
	(0.0025)	(0.0046)	(0.0046)	
Panel C. Control for School Expenditures in Country of Birth				
English-speaking ability		0.2173 ***	0.3628 **	36,272
(scale of 0 to 3, 3=best)		(0.0101)	(0.1755)	
max (0, age at arrival - 11) * non-English-speaking country of birth	-0.0543 ***			
	(0.0026)			
max (0, age at arrival - 11) * ln(school exp per child)	0.0362 ***	0.0064 *	-0.0004	
	(0.0020)	(0.0036)	(0.0088)	
Panel D. Control for Teacher-Pupil Ratio in Country of Birth				
English-speaking ability		0.2174 ***	0.4031 ***	38,563
(scale of 0 to 3, 3=best)		(0.0100)	(0.1344)	
max (0, age at arrival - 11) * non-English-speaking country of birth	-0.0647 ***			
	(0.0024)			
max (0, age at arrival - 11) * ln(teacher-pupil ratio)	0.1094 ***	0.0046	-0.0185	
	(0.0053)	(0.0095)	(0.0200)	
Panel E. Allow Coefficients to Differ between High- and Low-GDP Countries				
English-speaking ability		0.2326 ***	0.3369 ***	40,552
		(0.0105)	(0.1230)	
English-speaking ability * I(Above-median-GDP country of birth)		-0.0872 ***	-0.0669	
		(0.0281)	(0.2010)	
max (0, age at arrival - 11) * non-English-speaking country of birth	-0.0834 ***			
	(0.0035)			
max (0, age at arrival - 11) * non-English-speaking country of birth * I(Above-median-GDP ctry)	0.0338 ***			
	(0.0054)			

Notes: See notes for Table 2. In addition to the regressors listed above, all specifications also include age at arrival, country of birth, age, race/ethnicity and sex dummies. Also, the 2SLS estimate is obtained using the variable max (0, age at arrival - 11) * non-English-speaking country of birth to instrument for the endogenous regressor, English-speaking ability. Finally, the specification in Panel E has the aforementioned regressors as well as their interactions with a dummy equal to one if the country of origin had above-median GDP in 1965.

Table 6. Effect of English-Speaking Ability on Log Annual Wages, As Mediated by Years of Schooling

	Base Result	Returns to Schooling Constrained to a Particular Value				Returns to Schooling Estimated	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
endogenous regressor							
English-speaking ability	0.3335 ***	0.1018	0.0622	0.0226	-0.0169	0.0183	0.0309
	(0.1054)	(0.1031)	(0.1028)	(0.1026)	(0.1024)	(0.1312)	(0.2511)
controls							
years of schooling	--	0.060 #	0.070 #	0.080 #	0.090 #	0.0811 ***	--
						(0.0075)	
dummies for years of schooling							Yes
potential years of schooling in country of birth							-0.0058
							(0.0113)
potential years of schooling in country of birth* school quality measures							Yes

Notes: See notes for Table 2. "#" indicates that the coefficient is constrained to reported value.

Appendix Table 1. Immigrants by Country of Birth

Panel A. Countries with English as an official language

1. English-speaking countries (=Control Group)

Rank by N	country	N	share of total N
1	Canada	3,775	39.8%
2	UK - England	2,242	23.7%
3	Jamaica	1,040	11.0%
4	Trinidad and Tobago	451	4.8%
5	UK - not specified	312	3.3%
6	UK - Scotland	298	3.1%
7	Guyana/British Guyana	180	1.9%
8	Australia	167	1.8%
9	Ireland	163	1.7%
10	Bermuda	162	1.7%
11	Barbados	113	1.2%
12	Belize/British Honduras	94	1.0%
13	South Africa	70	0.7%
14	Bahamas	60	0.6%
15	U.S. Virgin Islands	54	0.6%
16	New Zealand	48	0.5%
17	Grenada	37	0.4%
18	Northern Ireland	36	0.4%
19	St. Kitts-Nevis	32	0.3%
20	Liberia	31	0.3%
21	Antigua-Barbuda	30	0.3%
22	St. Vincent	18	0.2%
23	UK - Wales	16	0.2%
24	St. Lucia	16	0.2%
25	Zimbabwe	11	0.1%
26	British West Indies - n.s.	7	0.1%
27	Cayman Islands	6	0.1%
28	British Virgin Islands	5	0.1%
29	UK - Jersey	2	0.0%
30	Anguilla	2	0.0%
	Total English-spking obs	*9,478*	*100.0%*
	As % of total obs		*13.2%*

2. Other English-official countries (excluded from main analysis)

Rank by N	country	N	share of total N
1	Philippines	2,474	48.3%
2	Hong Kong	1,260	24.6%
3	India	493	9.6%
4	Guam	263	5.1%
5	Pakistan	102	2.0%
6	US territory - not specified	99	1.9%
7	Nigeria	63	1.2%
8	American Samoa	59	1.2%
9	Fiji	50	1.0%
10	Dominica	42	0.8%
11	Kenya	31	0.6%
12	Malta	31	0.6%
13	Tonga	25	0.5%
14	Singapore	23	0.4%
15	Uganda	23	0.4%
16	Tanzania	17	0.3%
17	Ghana	15	0.3%
18	Papua New Guinea	13	0.3%
19	Micronesia	9	0.2%
20	Zambia	6	0.1%
21	Marshall Islands	6	0.1%
22	Sierra Leone	6	0.1%
23	Mauritius	3	0.1%
24	Palau	3	0.1%
25	Gambia	2	0.0%
26	Gibraltar	2	0.0%
27	Seychelles	2	0.0%
28	Senegal	2	0.0%
29	Kiribati	1	0.0%
	Total other Eng-official obs	*5,125*	*100.0%*
	As % of total obs		*7.1%*

Notes: Table 1 continued on next page.

Appendix Table 1. Immigrants by Country of Birth (continued)

Panel B. Non-English-speaking countries (=Treatment Group)

Rank by N	country	N	share of total N	Rank by N	country	N	share of total N
1	Mexico	15,035	26.3%	31	Israel/Palestine	278	0.5%
2	Cuba	6,554	11.5%	32	Costa Rica	263	0.5%
3	Germany excl. West G.	4,660	8.2%	33	Honduras	259	0.5%
4	Puerto Rico	4,404	7.7%	34	Iran	257	0.5%
5	Italy	2,358	4.1%	35	Turkey	244	0.4%
6	Japan	2,044	3.6%	36	South Korea	199	0.3%
7	West Germany	1,915	3.4%	37	Egypt	192	0.3%
8	Portugal	1,398	2.4%	38	Nicaragua	183	0.3%
9	France	1,265	2.2%	39	Lebanon	182	0.3%
10	Dominican Republic	1,238	2.2%	40	Vietnam	173	0.3%
11	Outside US, not specified	1,059	1.9%	41	Thailand	158	0.3%
12	Colombia	1,027	1.8%	42	Chile	153	0.3%
13	Korea excl. South Korea	962	1.7%	43	Indonesia	133	0.2%
14	Greece	832	1.5%	44	Morocco	130	0.2%
15	Poland	798	1.4%	45	Iraq	117	0.2%
16	Ecuador	588	1.0%	46	Africa - not specified	113	0.2%
17	Haiti	542	0.9%	47	Jordan	109	0.2%
18	Yugoslavia	515	0.9%	48	Czechoslovakia	107	0.2%
19	Spain	508	0.9%	49	Libya	105	0.2%
20	Taiwan	493	0.9%	50	Switzerland	97	0.2%
21	Argentina	484	0.8%	51	Romania	90	0.2%
22	El Salvador	434	0.8%	52	Austria	90	0.2%
23	Panama	412	0.7%	53	Bolivia	84	0.1%
24	China	390	0.7%	54	Russia	81	0.1%
25	Brazil	343	0.6%	55	Hungary	80	0.1%
26	Netherlands	337	0.6%	56	Uruguay	79	0.1%
27	Guatemala	326	0.6%	57	Subtotal, top 60 countries	55,765	97.7%
28	Peru	322	0.6%	58	Subtotal, other (87) countries	1,341	2.3%
29	Azores	285	0.5%	59	*Total non-Eng-spking obs*	*57,106*	*100.0%*
30	Venzuela	281	0.5%	60	*As % of total obs*		*79.6%*

Notes: Information on each country's official languages from the World Almanac. Recent adult immigrants from the 1980 IPUMS were used to divide English-official countries into English-speaking (at least 50% of recent adult immigrants did not speak a language other than English at home) or Other. Above tabulations by country of birth use following sample: 1990 IPUMS, arrived to the U.S. by age 17 between 1960 and 1974, is currently aged 25 to 38 and has non-missing value for English-speaking ability. "Countries" correspond to IPUMS detailed birthplace codes.

Appendix Table 2. Effect on Log Annual Wages -- Alternative Instruments

	1st stage (1)	OLS (2)	2SLS (3)
Panel A. Base (from Table 3)			
English-speaking ability		0.2219 ***	0.3335 ***
(scale of 0 to 3, 3=best)		(0.0093)	(0.1054)
max (0, age at arrival - 11) * non-English speaking country of birth	-0.0776 ***		
	(0.0021)		
Panel B. Linear Age at Arrival			
English-speaking ability		0.2219 ***	0.4519 ***
(scale of 0 to 3, 3=best)		(0.0093)	(0.1257)
Age at arrival * non-English speaking country of birth	-0.0255 ***		
	(0.0008)		
Panel C. Dummy Variable for Arrival when Young			
English-speaking ability		0.2219 ***	0.4257 ***
(scale of 0 to 3, 3=best)		(0.0093)	(0.1218)
(Age at arrival ≤ 11) * non-English speaking country of birth	0.2649 ***		
	(0.0084)		
Panel D. All Three Instruments			
English-speaking ability		0.2219 ***	0.3571 ***
(scale of 0 to 3, 3=best)		(0.0093)	(0.1046)
max (0, age at arrival - 11) * non-English speaking country of birth	-0.0627 ***		
	(0.0039)		
Age at arrival * non-English speaking country of birth	-0.0061 ***		
	(0.0011)		
(Age at arrival ≤ 11) * non-English speaking country of birth	0.0156		
	(0.0151)		
Panel E. Age-at-Arrival Dummies			
English-speaking ability		0.2219 ***	0.3435 ***
(scale of 0 to 3, 3=best)		(0.0093)	(0.1045)
Age-of-Arrival Dummies * non-English speaking country of birth	Yes		

Notes: See notes for Table 2.