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No. 9170

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LABOUR ECONOMICS

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ISSN 0265-8003

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Discussion Paper No. 9170
October 2012

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CEPR Discussion Paper No. 9170
October 2012

ABSTRACT

The Impact of Immigration on the Educational Attainment of Natives*

Using a state panel based on census data from 1940-2010, I examine the impact of immigration on the high school completion of natives in the United States. Immigrant children could compete for schooling resources with native children, lowering the return to native education and discouraging native high school completion. Conversely, native children might be encouraged to complete high school in order to avoid competing with immigrant high-school dropouts in the labor market. I find evidence that both channels are operative and that the net effect is positive, particularly for native-born blacks, though not for native-born Hispanics. An increase of one percentage point in the share of immigrants in the population aged 11-64 increases the probability that natives aged 11-17 eventually complete 12 years of schooling by 0.3 percentage points, and increases the probability for native-born blacks by 0.4 percentage points. I account for the endogeneity of immigrant flows by using instruments based on 1940 settlement patterns.

JEL Classification: I21 and J15
Keywords: education and immigration

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*I thank, without implicating, Daniel Parent, Leah Brooks, David Figlio, Tommaso Frattini, James Heckman, John Eric Humphries, Ethan Lewis, Marguerite Lukes, James MacKinnon, Steve Pischke and participants in numerous seminars for comments and data advice. I am grateful to the Social Science and Humanities Research Council of Canada for financial support. I am also affiliated with the CEPR (London) and DIW (Berlin).

Submitted 01 October 2012

The extent to which the children of low–education or low–income parents are able to achieve their full potential in the United States is a cause for concern. Contrary to popular mythology, there is less intergenerational mobility in earnings and education in the United States than in continental Europe and Canada, and no more than in the United Kingdom.[1] An important step upward for many children from low socio–economic status families is graduation from high school, yet U.S. high school graduation rates are no longer increasing.[2] In this paper, I contribute to our understanding of the determinants of high school educational attainment by investigating the role of immigration. Increasing immigration in recent decades has led to popular concern that immigration is reducing the quality of K–12 education. If this concern is well founded, rising immigration could reduce native high school graduation rates. Conversely, immigration–induced changes in labor market incentives for educational attainment could have the opposite effect. I seek evidence for these two channels and assess their net effect.

Immigrants and the young children of immigrants generally have a more limited command of English than natives. If immigrants and natives are taught in the same classes, teachers of some subjects may slow the pace of instruction to accommodate non–native speakers. If immigrant students have had worse quality prior education, or have less education than their native classmates, teachers may lower expectations for all students. Chin et al. (2012) provide evidence for such within–classroom negative externalities by showing that non–Spanish speaking students in Texas have higher test scores when Spanish speakers with limited English proficiency are taught in separate classes rather than integrated into the regular classes, despite an associated shift in spending towards Spanish speakers. In other settings, a diversion of financial resources from native students to support the needs of immigrants could lower the quality of native education. For example, Fix and Zimmerman (1993) find that federal Chapter I spending per economically disadvantaged student fell due to the immigration–induced expansion in the number of eligible children.

[1] Checchi, Ichino and Rustichini (1999); Corak (2006).

[2] Heckman and LaFontaine (2010).

A lower educational quality for natives will reduce their earnings capacity at a given number of years of education, and this lower return to education in turn may induce natives to complete fewer years of high school. This prediction is not unambiguous, however. If high school becomes easier, the fall in marginal cost may outweigh the fall in the marginal benefit and lead to higher native completion rates. Furthermore, if immigrant students are better educated or harder working than their native classmates, they will provide positive peer effects and may relax the resource constraint, and could increase native completion rates.

There exists a second channel through which immigration could influence natives' high school educational attainment. Incentives to complete high school are influenced by the wage structure, which is in turn affected by the entry of immigrant workers: this is modelled formally by Chiswick (1989). Immigration will affect wage inequality among natives if the distribution of immigrant skill differs from that of natives. Compared to natives, immigrants to the United States are very disproportionately poorly educated and somewhat disproportionately highly educated. Immigrants are underrepresented among workers with an intermediate level of education, such as a high school diploma. The effect of immigrants entering the labor market should therefore be to increase wage inequality in the lower half of the native distribution, particularly the wage gap between high school dropouts and high school graduates. Empirical studies confirm this.[3] The net effect of the changes in the wage structure is likely to be to increase the return to completing high school, and hence native completion rates.[4] Native–born youth are likely to be well informed about the dropout labor market even while still high school students, since this is the market in which many seek part–time jobs.[5]

[3] Borjas and Katz (2007), Ottaviano and Peri (2012). Card (2009) views the induced increase in wage inequality as small, which he attributes to high school dropouts and high school graduates being perfect substitutes.

[4] If high school dropout workers and workers with less than college are perfect substitutes, it is the return to college which will rise, which will also increase high school completion rates.

[5] Smith (2012) presents evidence that adult immigrants with high school or less reduce the employment rate of native high school students. This reduction could could provide an additional channel for immigration to affect native graduation rates.

Any negative effects on the schooling quality of natives will affect the children of low socio–economic status (SES) parents more than children of high SES parents. Families, whether immigrant or native, tend to locate near other families of similar SES, and the immigrants encountered by poorer native children in their local public school are more likely to have inadequate previous education than the immigrant classmates of richer natives. Richer parents may more easily move their child to a learning environment with either fewer immigrants or immigrants with better language skills and educational background, by using private schools (Betts and Fairlie 2003; see also Hoxby 1998) or by moving to a different school district (Cascio and Lewis 2012). Furthermore, the educational quality of a child's school is likely to have a smaller impact on the children of high SES parents, as such parents can compensate in part for a school's deficiencies by providing the child with instruction at home. At the same time, any positive effects of immigration on high–school graduation rates are likely to be larger for groups with graduation rates that leave substantial room for improvement. Thus, effects through both channels are more likely to affect low SES natives, and consequently to affect minorities more than non–Hispanic whites. Furthermore, native minorities live in closer proximity to immigrants than native non–Hispanic whites, as I show below, increasing their likely responsiveness to immigration. Minority boys, who have particularly low high–school graduation rates (Orfield et al. 2004, NCES 2008, Noguera et al. 2011, Noguera 2008), may be particularly sensitive to immigration.

I focus on the impact of immigration on natives' completion of 12 years of schooling, comparing results across ethnicity, race and gender. I use the decennial censuses of 1940–2000 and the pooled 2008–2010 American Community Surveys (ACS) to construct a state panel. I extend two closely related papers, Betts (1998) and Betts and Lofstrom (2000), in several ways. The most important extensions in practice are the distinction between immigrants of different educational attainment, the measurement of the immigrant inflows at the time natives were of school age, rather than later, and the use of a dependent variable consistent over time. The extension to the use of instrumental variables based on historical immigrant settlement patterns is important in principle but less important in practice. Some of the analysis in Smith (2012) is also closely related to my paper: he examines the effects of adult low–skill immigration on natives' high school enrollment rates. The estimates are imprecise, however, and for this reason I follow Betts (1998) and Betts and Lofstrom (2000) in examining completed education among several cohorts of older respondents.[6] Several papers have examined the impact of immigrants on native test scores in Europe and Israel, with mixed results.[7]

I measure the shares of immigrants in the population when natives are aged 11–17, and I measure native educational attainment at ages 21–27. Unlike Betts (1998) and Betts and Lofstrom (2000), who found a detrimental net effect of immigration on native high school attainment for each native racial and ethnic group, I find the net effect of immigration to be positive for natives generally, and especially for blacks: an increase of one percentage point in the share of immigrants in the population aged 11–64 (0.13 standard deviations) increases the probability natives complete 12 years of schooling by 0.3 percentage points, and increases the probability for blacks by 0.4 percentage points. I estimate a detrimental net effect for native–born Hispanics of -0.2 percentage points that is statistically insignificant. All effects are rather small compared to the average native completion rate of 87.8% (81.0% for native blacks; 81.3% for native Hispanics) and given the average immigrant share of 8.9% (8.1% for blacks; 15.5% for Hispanics). The standard error on the coefficient for native Hispanics allows moderately sized negative effects to be ruled out.

The finding that poorly educated natives upgrade their education in response to immigration adds to our understanding of why the wages of high school dropouts decline so little in the face of immigration.[8] Peri and Sparber (2009) have previously documented

[6] Jackson (2011) finds that when a greater share of adult immigrants is unskilled, native college enrollment rises; the effect on contemporaneous completed high school is mixed. Borjas (2006) finds that foreign students do not reduce native enrollment in graduate school. Llull (2010) and Eberhard (2012) find in a structural model that natives increase their years of education in response to adult immigrants. Neymotin (2009) finds native SAT scores and probability of applying to top colleges are not negatively affected by the school's share of immigrant test–takers.

[7] Brunello and Rocco (2011), Geay et al. (2012), Gould et al. (2009), Jensen and Rasmussen (2011), Ohinata and van Ours (2011).

[8] Card (2009) and Ottaviano and Peri (2012) believe the wage declines to be small. Borjas, Grogger and Hanson (2011) disagree.

that unskilled natives exploit their comparative advantage to avoid competition with immigrants, by shifting to more communication–intensive occupations. By distinguishing among immigrants by age and education, I find support for the labor market channel for education upgrading proposed by Chiswick (1989): a one percentage point increase in the share of immigrants with less than 12 years school in the population aged 18–64 (0.27 standard deviations) increases the eventual native completion rate by 0.8 percentage points, with larger effects for native–born minorities. Effects of more educated adult immigrants are not precisely estimated.

Considering the school channel, I find that immigrants have at most a small negative effect on natives as a whole. All specifications indicate that child immigrants reduce native completion rates, with the instrumental variables specification indicating a one percentage point increase in the share of immigrants in the population aged 11–17 (0.22 standard deviations) reduces the probability natives eventually complete 12 years of school by 0.2 percentage points. Moderately negative magnitudes can be ruled out, even though the instrumental variables coefficient is statistically insignificant. Conversely, the more negative point estimate for native blacks means that moderately negative magnitudes cannot be ruled out. There is no effect on non–Hispanic white natives. The results for native Hispanics are more subtle, as the effect of immigrant children depends strongly on the education of the immigrants' parents: child immigrants of more educated parents have moderately–sized positive effects, offsetting moderately–sized negative effects of child immigrants of poorly educated parents. These effects are present for male Hispanic natives only.

The evidence that some child immigrants reduce the educational attainment of some minority natives suggests the need for reform in immigrant education, though the results are also consistent with a neighborhood rather than a school mechanism. Reform could include both increased resources for schools in areas with high immigration (Singer 2008) and the implementation of best practices regarding improving language skills of non–native speakers, remedying educational deficiencies of immigrants, and integrating immigrants with native students (García, Kleifgen and Falchi 2008).

1 Data and descriptive statistics

The principal data for regression analysis are the IPUMS micro–data samples for the 1940–2000 decennial censuses and the pooled 2008–2010 American Community Surveys (which I refer to as the 2010 ACS data), from which I construct a panel of states.[9] I supplement them with data from the Bureau of Economic Analysis on state personal income per capita. I choose the census and ACS data for the large sample sizes they afford for the measurement of both state immigrant shares and the shares of native cohorts by race and ethnicity attaining a given educational level. Even with the large census samples, I am forced to consider a native cohort as one spanning several birth years. A disadvantage of the data is that they do not contain information about parental education or income, except for children living with their parents. I check the sensitivity of the analysis to using metropolitan areas instead of states (the construction of the metropolitan area sample is described in the Data Appendix).

In order to have a consistent outcome variable over all years, I define the outcome of interest as the completion of 12 years of schooling, with or without the obtention of a high school diploma, as the two may be distinguished only from 1990 onwards.[10] I focus on the native–born who were aged 11–17 in the previous census: this implies current ages 21–27 (20–26 in 2009, 19–25 in 2008). Most covariates are lagged one census, to correspond to the time when natives were aged 11–17. I construct samples of all races and ethnicities pooled, blacks, Hispanics and non–Hispanic whites. Being black and Hispanic are not mutually exclusive, so there is some overlap in the two minority samples. Immigrants are defined as those born abroad, including those born in U.S. territories. I drop the states of Alaska and Hawaii, as their absence from the 1940 and 1950 censuses complicates the use of the instruments and covariates measured in 1940.

Figure 1 depicts the shares of native–born 21–27 year olds who have completed at least

[9] Ruggles et al. (2010).

[10] Betts (1998) uses 12 years of schooling for 1980, and obtention of a high school diploma in 1990. This discrepancy drives the negative effect he finds of immigration on native Hispanics.

12 years of schooling, by race and ethnicity, for 1940–2010.[11] The shares increase strongly over the early decades then level off around 1990. Minorities begin the period with much lower education, and converge towards non–Hispanic white rates from 1960 until 1980 or 1990. At the start of the period, both blacks and Hispanics (concentrated in different regions) were educated in segregated, inferior schools. As a result of court decisions in the 1940s and 1950s, the Civil Rights Act of 1964 and the Coleman Report (Coleman et al. 1966), educational quality, integration and attainment increased for minorities.[12] Evans et al. (2012) argue persuasively that convergence ended due to the incentives for black males to drop out of high school to earn large sums and risk death selling crack cocaine.

Heckman and LaFontaine (2010) have cautioned that both the increase in high school completion observed in the census and the convergence between whites and minorities mask an increasing share of individuals receiving a General Equivalency Degree (GED). For the purposes of this paper, it is desirable to know whether any response in native education is coming through time in regular high school, or the propensity to obtain a GED. The measurement difficulty is that, unlike in the ACS, is it impossible to identify GED holders in the census micro–data. In early censuses, when GEDs were uncommon, no specific instructions concerning GEDs were given to the respondents. In 1980, GED recipients were instructed to respond they had completed 12 years of high school, while in 1990 and 2000 they were instructed to respond that they had a high school diploma. I therefore correct the 12–year completion rates using annual published tables on GEDs awarded by state and age, and using the Heckman and LaFontaine appendix for methodological guidance. However, the GED recipients I am subtracting, while not holders of regular high school diplomas, do have the possibility of attending college, so the adjusted measure understates final educational attainment. The adjustment for GEDs becomes increasingly crude as the data get older, as explained in the Data Appendix, and I do not attempt to adjust 1970 and earlier years.[13] Appendix Table 1 shows the 12–year

[11] For the purposes of the graph, I use 21–27 for all years including 2008–2010.

[12] MacDonald and Monkman (2005), Valencia et al. (2002).

[13] Heckman and LaFontaine report in their appendix that the adjustment for 1970 is small; furthermore, in 1970 GED holders may not have claimed to have completed 12 years of high school.

completion rates measured in different ways.

Figure 2 shows the evolution of the share of immigrants over the period, by age group. The share of immigrants in the population of working age, 18–64, traces out a U shape, falling from 12.1% in 1940 to 6.1% in 1970, before rising to 18.4% in 2010 (top line). The share of immigrants in the school–age population, 11–17, traces a different path, rising almost monotonically from 1.6% in 1940 to a still modest 7.2% in 2010 (bottom line).

Figure 3 shows the time paths of three additional key covariates: the shares of the population aged 18–64 composed of immigrants with less than 12 years of education, exactly 12 years of education, and more than 12 years of education. The share of the lowest education immigrants falls from a high of 10.1% in 1940 to a low of 2.8% in 1980, before rising again to 4.8% in 2010. The shares of the immigrants from the two more educated groups rise monotonically from 0.7-1.1% in 1940 to 5.4% in 2010 for those with exactly 12 years of education and 8.1% for those with more than 12 years education. Appendix Tables 2 and 3 give further means of variables measured at the individual level, while Appendix Tables 4 and 5 give means of variables measured at the state level.

The Census Bureau produces tabulations of their census data at the school district level. The 1990 tabulations, known as the School District Database (SDDB), may be used to assess which native children are most likely to interact with immigrants in school.[14] The first four columns of Table 1 are based on samples of children of kindergarten, primary or secondary school age, from which I have discarded the small number of school districts with no high school. Panel A column 1 shows that the share of immigrants among such children is 4.2%, but that a native–born child is on average in a school district with only 3.8% immigrants, with corresponding numbers of 2.5% for white non–Hispanic natives, 5.2% for black natives, 10.7% for Hispanic natives, and 14.3% for immigrants.

However, this does not necessarily show that Hispanic natives interact more with immigrants than non–Hispanic white natives within a given state (the relevant question given that my subsequent analysis will rely on within–state variation). This pattern could emerge if Hispanic immigrants and Hispanic natives were concentrated in one region of

[14] The data are available at www.nber.org/sddb/, accessed 3 April 2012.

the country and white natives in another. I therefore compute the panel A numbers for each state, calculate differences between groups for each state, and report the population–weighted average differences across states in panel B. Column 1 shows that while native blacks and native Hispanics are both more likely than native whites to be in school districts with many immigrants, black and Hispanic shares are more similar to each other than panel A indicated. On average (within state), a native black child is in a school district with 2.7 percentage points more immigrants than a native white child, while a native Hispanic child is in a school district with 3.6 percentage points more immigrants. Column 2 panel B shows that the black–Hispanic difference is larger when proximity to Hispanic immigrants is measured, but columns 3 and 4 show there is no sizeable difference in the black–Hispanic exposure to white non–Hispanic immigrants or Asian immigrants. Columns 5 and 6 are based on data on the parents of children of school age. The share of immigrants among parents is higher than among children, but generally similar patterns prevail.

These statistics suggest that any effect of child immigration and probably also adult immigration to a given state will be larger for native blacks and especially Hispanics than for non–Hispanic whites, as native minorities interact more with immigrants in their schools and neighborhoods, and probably labor markets. The implication of the high degree of contact between Hispanic natives and Hispanic immigrants is unclear: immigrants may have less impact on natives similar to themselves, or they could have more impact, for example by encouraging native–born Hispanics to speak more Spanish, possibly at the expense of English, or by straining resources directed at those native–born Hispanics who have limited English proficiency.

2 Estimation

Rather than analyze native schooling determinants at the individual level, I reduce the sample size by calculating state schooling variables adjusted for individual characteristics, and conduct the main analysis on a panel of states. Specifically, for natives aged 21–27

(20–26 in 2009 and 19–25 in 2008) at time t and born in state s, I first run the following linear probability regression for each of the samples, for 1940–2010:

$$P(E_{ist} \geq 12) = \alpha_0 + \alpha_1 F_{ist} + \sum_{a=20}^{a=27} \gamma_a A^a_{ist} + \sum_s \sum_t \lambda_{st}(\delta_s \times \nu_t) + \eta_{ist}, \tag{1}$$

where i indexes individuals and s individuals' birth state, E represents years of completed schooling, F is a gender dummy, A^a are dummy variables for age, δ_s are state dummies and ν_t are year dummies. I match individuals to their birth state to avoid endogenous moves of young adults that would plague the use of state of current residence. When I use samples of all races and ethnicities, I also control for race (Asian, black, race missing) and Hispanic ethnicity (Mexican, Puerto Rican, Cuban, other, Hispanic ethnicity missing). I control for black race and Hispanic ethnicity when using Hispanic samples, and for Hispanic ethnicity when using black samples.[15] I experimented with including three dummies for whether the individual could have left school at age 14/15, 16, or 17 given his or her birth state and birth year, but their coefficients were never jointly significant, so I report results without these controls.[16]

I weight this regression using weights based on the census weights. The census weights sum to the U.S. population of the census year, while I wish the standard errors to reflect the variation in sample sizes from year to year. I adjust the census weights so that the ratios of their sums for each year reflect the ratio of the census sample sizes, resulting in considerably more weight being put on recent years. The average year in the weighted data is 1989 for non–Hispanic whites, 1991 for blacks and 1995 for Hispanics.[17]

In a second step, I use the coefficients $\hat{\lambda}_{st}$ as the dependent variable in a state panel

[15] To the degree that controlling for natives' Hispanic ethnicity implies controlling for the effects of second generation immigrants, the impacts of immigrants estimated below will be those of the first generation only.

[16] The coefficients on the school leaving dummies are identified despite the state–year effects, as each year has several birth cohorts. The data sources are Açemoglu and Angrist (2000) and the Digest of Education Statistics, various issues.

[17] The 1940, 1960 and 1970 censuses are 1% samples, the 1980–2000 censuses are 5% samples, the ACS has a more complicated sampling scheme which results in the pooled 2008–2010 sample being smaller than the 2000 sample. The 1950 census only asked education questions of a subset of the main 1% sample.

analysis:

$$\hat{\lambda}_{st} = \beta_0 + \beta_1 I^{11-17}_{s,t-10} + \beta_2 I^{E<12}_{s,t-10} + \beta_3 I^{E=12}_{s,t-10} + \beta_4 I^{E>12}_{s,t-10} + \beta_5 X_{s,t-10} + \beta_6 Y_{s,1940} t + \gamma_s + \nu_t + \epsilon_{st}. \quad (2)$$

I weight the regressions with the inverse of the squared standard errors on the $\hat{\lambda}_{st}$ in the first step, and cluster the standard errors by state. $I^{11-17}_{s,t-10}$ represents the share of the population aged 11–17 that is foreign–born in the previous census, when the native–born cohort was itself aged 11–17, and is designed to capture natives' exposure to immigrant classmates. Ideally, an additional covariate would capture the presence of immigrants when natives were of elementary school age, but the ten–year spacing of the census precludes this. The null hypothesis to be tested is that β_1 is negative because immigrant children reduce current school quality. However, β_1 may also reflect the behavior of sophisticated native students who factor the presence of immigrant schoolmates into their predicted return to high school. Measurement error, including errors in matching individuals to the state in which they went to school, may bias the coefficient towards zero.

$I^{E<12}_{t-10}$ represents the share of the population aged 18–64 when natives were aged 11–17 that was immigrants with less than 12 years of schooling, and $I^{E=12}_{t-10}$ and $I^{E>12}_{t-10}$ are defined similarly. The null hypothesis to be tested is that β_2 is positive, because the presence of immigrants with less than 12 years education increases the return to completing 12 or more years of education, and because high school students use the return among current adults as a proxy for the return they themselves will face in the labor market. The necessity of using multi–year birth cohorts is likely to bias β_2 towards zero, since the younger members of the age range 11–17 are likely to base their years of schooling decision on the wage structure, and hence immigration rates, of later years. The signs of β_3 and β_4 are ambiguous, as the inflows of more educated immigrants have opposite effects on the return to exactly 12 years of education versus more than 12 years of education (relative to less than 12 years).

While most immigrants aged 11–17 arrive in the United States with their parents, most immigrants 18–64 do not arrive with children aged 11–17. The correlation between I^{11-17} and $I^{E<12}$ is 0.76 (weighted with the weights for all natives), and between the ten–year differences of these covariates is 0.41. It should therefore be possible to identify their effects separately, at least if least squares is an adequate estimation method.

This regression suffers from endogeneity problems, however. Native high school educational attainment and high shares of low–education immigrants in a state may be spuriously negatively correlated. What makes the state economically attractive for immigrants, such as the availability of low–skill jobs, may by the same token mean that natives have a low incentive to complete 12 years of schooling. For example, a downturn in a state's low–skill industries could deter unskilled immigrants from moving to the state and encourage its natives to graduate from high school, leading β_2 to be biased down (the same direction as the measurement error bias, if $\beta_2 > 0$). Similar reasoning suggests that β_3 and β_4 could be biased up by endogeneity. β_1 could be biased up if immigrants with children choose states with high educational attainment (the same direction as the measurement error bias, if $\beta_1 < 0$), but there may be other biases due to endogeneity in their parents' choice of state if these have not been controlled for properly.

These considerations lead me to implement an instrumental variables strategy using ten–year differences of equation (2):

$$\Delta\hat{\lambda}_{st} = \tau_0 + \beta_1 \Delta I^{11-17}_{s,t-10} + \beta_2 \Delta I^{E<12}_{s,t-10} + \beta_3 \Delta I^{E=12}_{s,t-10} + \beta_4 \Delta I^{E>12}_{s,t-10} + \beta_5 \Delta X_{s,t-10} + \beta_6 Y_{s,1940} + \nu_t + \Delta\epsilon_{st}. \tag{3}$$

I estimate this using weights $1/(1/w_{s,t} + 1/w_{s,t-10})$, where w is the weight used in equation (2). I devise instruments for the differenced immigration covariates, based on the flows of immigrants to a state that would have been expected given the 1940 geographic distribution of immigrants from different regions and the subsequent national inflows from those regions.[18] To illustrate, if immigrants from Europe prefer the northeastern United States because it is closer to home and because other Europeans are already there because of geography, and Mexican immigrants prefer the southern border states for analogous reasons, the large national increase since 1940 in the share of immigrants that are Mexican will be associated with an increase in immigration to the southern border states

[18] These instruments are similar to the instrument developed by Card (2001), and also used by Jackson (2011) and Hunt and Gauthier–Loiselle (2010).

relative to the northeast. The predicted flows captured in the instrumental variable will therefore be strongly, though not perfectly, correlated with actual immigrant flows to states. Furthermore, since the national increase in Mexican immigration appears to be the result of increasingly large birth cohorts entering the Mexican labor market,[19] and the national decrease in European immigration is due to Europe's having become richer, the decrease in immigration to the northeast relative to the border states is unrelated to non–immigration factors affecting native education choices.

I define an instrument for each of the education–specific immigration variables as follows. For a state s, the predicted change in the number of immigrants of education level E (aged 18–64), caused by changing origin regions k, can be written as

$$\Delta \hat{M}_s^E = \sum_k \frac{M_{sk}}{M_k} \Delta M_k^E = \sum_k \mu_{sk} \Delta M_k^E, \tag{4}$$

where μ_{sk} is state s's share in 1940 of the national total of immigrants who originate from region k, and ΔM_k^E is the national change in the number of immigrants with education E (aged 18–65) from that region. I use 18 source regions or countries, listed in Appendix Table 6. Because the variables to be instrumented are percentage point changes, I convert $\Delta \hat{M}_s^E$ to percentage points by dividing by the population level (aged 18–65) at the start of the period to which Δ refers, to define the final instrument as:

$$Z_s^E = \frac{\Delta \hat{M}_s^E}{POP_s}. \tag{5}$$

I deliberately base the μ_{sk} on immigrants of all educations (and ages) to emphasize the role of geography and taste and minimize the role of economic factors that might disproportionately attract workers of a specific education level. The instrument will be invalid if non–immigration shocks to high school completion are correlated with 1940 immigrant densities; for example, if improvements to the California and Texas school systems caused a national–level increase in Mexican immigration.[20] By defining an instrument for each education level, I assume that improvements to the California and Texas school systems did not cause a national–level increase in Mexican immigration of any education group.

[19] Hanson and McIntosh (2007).

[20] See Beaudry et al. (forthcoming) for a formal treatment.

I can further reduce the likelihood that the instrument is correlated with the error term by following Wozniak and Murray (2012) in removing the state's interdecadal flow of a particular immigrant type from the national flow. This corresponds to rewriting equation (4) as

$$\Delta\hat{\mathcal{M}}_s^E = \sum_k \mu_{sk}(\Delta M_k^E - \Delta M_{sk}^E). \tag{6}$$

The instrument based on this, which I refer to as the adapted instrument, will also be more weakly correlated with the endogenous variable, however, and is not my preferred instrument.

It is easy to construct instruments for different immigrant age groups, in particular for ΔI^{11-17}, by replacing the education–specific variables in equations (4) and (5) (and 6) with age–specific variables, and I do so. However, the intuition of the instruments extends less easily to subdivisions by age group, as immigrant numbers in an age group are strongly influenced by aging as well as immigration, and furthermore, for the 11–17 age group in particular, it is obvious that changes in their inflows will be closely linked to those of adult immigrants.

I choose to use a common first stage for all four race/ethnicity samples, weighting each first stage with the denominator of its dependent variable (the population 11–17 or the population 18–64) in order to improve efficiency. This approach also means the first stage always includes all states and years: some early state–year cells have no native–born blacks or Hispanics aged 21–27.

It proves useful to further distinguish among immigrants in equation (2). Immigrant students could have either positive or negative spillovers on their native classmates, depending upon the quantity and quality of their prior education, their English skills, their industriousness, and the extent to which their parents contribute to their education. Parental education is likely to be a proxy for some of these characteristics, and because most children aged 11–17 live with their parents, we can observe their parents' education in the census data. It is therefore possible to split the share of the population 11–17 that is immigrant into immigrants whose parent or parents in the household have less than 12 years schooling, immigrants with at least one parent with 12 years or more, and immigrants with neither parent living in the household. The expectation is that children of more educated parents will make better peers and require fewer resources than children of less educated parents. The difficulty with regressions distinguishing child immigrants according to parental characteristics is that the number of endogenous variables becomes too large for the use of 2SLS.[21]

Although the natural level at which to examine school quality is the school, there are some reasons to use more aggregated data beyond the limited availability of school–level data. If some natives move out of their school and neighborhood when immigrants move in, analysis at the school or school–district level will not attribute any change in schooling of the native movers to the arrival of the immigrants. If public school data are used, even natives who move to private schools in the same school district will cause the same problem. Also, it is difficult to find an instrument at the school level which accounts for immigrants' potentially endogenous choice of location (and school). I base the main analysis on states, but also repeat the estimates using metropolitan areas (cities). The advantages of using cities rather than states are that the analysis is slightly closer to the ideal school or labor–market level analysis, while still permitting the construction of instruments, that there are (or may be) enough observations to identify the effects for more recent years, and that if this is the case, the information on English proficiency of immigrants may be used. The disadvantages are that the city of birth is not known, constraining the analysis to be based on city of current residence, and that rural areas are excluded.

[21] I have experimented with using the (OLS) state–year return to completing 12 years schooling as the independent variable of interest, instrumenting it with actual or predicted immigrant flows. The coefficient on the return is always very imprecisely estimated, and the first–stage immigrant coefficients are often wrongly signed.

3 Results

I examine the impact of immigration on the probability of natives' completing 12 years of schooling, for all native–born, non–Hispanic whites, blacks and Hispanics, first assessing the net impact of immigration, then decomposing the impact into school quality versus labor market channels, distinguishing child immigrants by parental education, checking the robustness of the results to the treatment of GED holders and finally distinguishing natives by gender.

3.1 The net effect of immigrants ages 11–64

In Table 2, I investigate the impact of the immigrant share of the population aged 11–64, initially using as the dependent variable the 12–year completion rate adjusted for age and sex only. With only state and year effects in column 1, or with unemployment rates and native cohort size (see Card and Lemieux 2001) as additional controls in column 2, immigration's coefficient is small and statistically insignificant. Neither the youth unemployment rate nor the prime–age unemployment rate has a statistically significant coefficient, possibly because many of the respondents were some years from graduation when the unemployment rate was measured.[22] However, once controls allowing for convergence amongst states, and linear trends for eight BEA regions are included in column 3, immigration's coefficient becomes a statistically significant 0.20. The 1940 share of non–Hispanic whites aged 21–27 who had less than 12 years education is statistically significantly positive, capturing convergence. The trend in the 1940 share of workers in agriculture is included to capture convergence for minorities, and its coefficient is insignificant for the full sample of natives.

Changing the dependent variable to one also adjusted at the individual level for race and ethnicity in column 4 raises the immigration coefficient from 0.20 to 0.29. This indicates that increases in immigration are positively correlated with increases in native–born

[22] State–level unemployment rates are not available from other sources for earlier decades, so the unemployment rate cannot be matched to the year the respondent was aged 17, for example.

minorities, not surprisingly in the case of Hispanics, and that once this is controlled for and immigration no longer picks up the under–performance of native minorities, immigration appears a more positive force. In column 5, I take into account that one would expect richer states to be able to afford better educational systems. However, states with better educational systems should become richer, so the coefficient on a control for state income would be biased up. The results show that the correlation between log state personal income per capita and completion of 12 years of schooling is indeed positive and statistically significant, and that the coefficient on the immigrant variable, now a lower bound on the true coefficient, is reduced somewhat from 0.29 to 0.22.

I base the differenced specifications of columns 6–8 on the specification of column 4. Differencing does not change the coefficients greatly (column 6), while the preferred 2SLS coefficient in column 7 is the largest of all specifications, at a statistically significant 0.34. The instrument is strong in the first stage, as evidenced by its associated F–statistic of 25. A coefficient of 0.34 implies that a one percentage point increase in the share of immigrants in the population 11–64 increases the native probability of eventually completing 12 years of education by 0.34 percentage points. This is a small effect considering that the (weighted) mean completion rate is 87.8%, and the share of immigrants in the population 8.9% (the implied elasticity is 0.035). The results using the adapted instrument, in column 8, are very similar, with a point estimate on the share of immigrants of 0.31. As expected, the standard error is larger than in column 7, with a smaller associated F–statistic of 21.

In Table 3, I analyze natives by race and ethnicity, using the same specifications as Table 1, reporting only the coefficient on the immigrant covariate.[23] For reference, I reproduce the coefficients from the first row of Table 1, for all natives, in the first row of Table 3. The coefficients for non–Hispanic whites, in the second row, are always smaller

[23] For blacks and Hispanics, there is a large positive coefficient on the share of agricultural workers in 1940, which captures convergence among states: agricultural states in 1940 had large shares of either blacks or Hispanics in the population, who were poorly educated. I do not control for the educational attainment of blacks and Hispanics in 1940, as they are based on very small samples for many states. White educational attainment in 1940 is statistically insignificant in the regressions for minorities.

than those for all natives; the preferred 2SLS coefficient in column 7 is 0.21, significant only at the 10% level. The coefficient is even smaller using the adapted instrument in column 8: 0.13 and statistically insignificant. For blacks in the third row, once state–specific trends and convergence are controlled for in column 3, there is a robust positive coefficient. The 2SLS coefficients are 0.38 in column 7 and 0.43 in column 8, small compared to the mean black completion rate of 81.0% and a black–white completion gap of 8.5 percentage points, but statistically significant. For Hispanics, in the fourth row, an effect of 0.31–0.53 is robust in the least squares columns 3–6, but it disappears with 2SLS: the coefficients are -0.21 in column 7 and -0.63 in column 8, both statistically insignificant. I conclude that the net effect of immigration on native completion of 12 years of schooling is positive and small for natives generally, blacks and non–Hispanic whites, and is possibly negative for Hispanics, though also small, as moderately sized negative effects can be ruled out.

3.2 Decomposing the impact of immigration into school quality and labor market channels

I now turn to decomposing the impact of immigration into school quality and labor market channels. I return in Table 4 to the sample of all natives, presenting the same specifications as in Tables 2 and 3, except with immigrants split into four categories. The first row shows that the effect on natives of immigrants aged 11–17, likely to have been natives' classmates, is negative: the coefficient is in the range -0.29– -0.45 in columns 3–6. 2SLS causes the point estimate to become less negative (-0.18 in column 7 and -0.11 in column 8), confounding my expectation that least squares would be biased towards zero. The 2SLS coefficients are not statistically significant, however, since the standard error rises considerably to 0.17 in column 7 and 0.29 in column 8. While the instruments are apparently not sufficiently powerful to allow identification of small effects, it is possible to rule out moderately–sized negative effects with the preferred instrument of column 7. As the system is exactly identified, 2SLS itself introduces no bias.

The first stage information for the 2SLS regressions of column 7 is presented in Table 5, column 2 (in column 1, I present the first stage used for immigrants 11–64 in column 7 of Tables 2 and 3). Although the predicted share of the population 11–17 has a statistically significant coefficient, it is not much more significant than those of the other excluded instruments, suggesting that I have not managed successfully to instrument the share of immigrants in the population 11–17. The F–statistic for the joint significance of the excluded instruments is only 8, while the more appropriate Angrist and Pischke (2008) F–statistic (an F–statistic adapted for multiple endogenous variables) is somewhat higher at 15. Appendix Table 7 shows the first stages for the adapted instruments are qualitatively similar, though the adapted instruments are weaker than the original instruments.

The second row of Table 4 shows that the effect on natives' acquiring 12 years of schooling of immigrants aged 18–64 with less than 12 years of schooling is positive and statistically significant in every specification, with coefficients of 0.81 and 0.82 in the 2SLS in columns 7 and 8. This is consistent with the hypothesis that the presence of unskilled immigrants in the labor market alters the wage structure in such a way as to give natives an incentive to complete 12 years of schooling. A comparison of column 6 with columns 7 and 8 shows that using 2SLS does not increase the coefficient greatly, despite the expectation it would be biased down in least squares. Column 3 of Table 5 shows that the preferred predicted share of immigrants with less than 12 years' schooling in the population 18–64 is a strong predictor in the first stage, much stronger than the other excluded instruments, and the Angrist–Pischke F–statistic is very high.

The third row of Table 4 shows that the impact of adult immigrants with exactly 12 years of education is imprecisely estimated. In the fourth row, the impact of adult immigrants with more than 12 years of education appears positive and significant until 2SLS is employed, when the coefficient falls to essentially zero (column 7) or slightly negative (column 8). The instruments associated with these covariates are fairly strong in their respective first stages (Table 5 columns 4 and 5; see also Appendix Table 7).

I repeat the exercise of Table 4 for native non–Hispanic whites in Table 6. The least squares results are qualitatively similar to those for all natives, but the absolute values of the coefficients are smaller. For this sample, however, 2SLS raises the point estimate on child immigrants to to zero or a statistically insignificant positive (0.03 in column 6 and 0.11 in column 7), suggesting there is no negative effect on native non–Hispanic whites through the schooling channel (or that it is cancelled out by natives' anticipation of their immigrant classmates' future labor market effect). Nevertheless, the WLS and 2SLS coefficients are not statistically significantly different. The coefficient on the adult immigrants with less than 12 years of education is a statistically significant 0.41 under 2SLS (column 6; 0.39 in column 7), half the size for the whole sample in Table 4.

In Table 7, I turn to native–born blacks, for whom the negative effect of immigrants 11–17 is larger in absolute value than for non–Hispanic whites. As for the previous samples, 2SLS renders this coefficient statistically insignificant, although in this sample the point estimates are almost unchanged at -0.34 in column 7 and -0.35 in column 8. The positive effect of adult immigrants with less than 12 years of education is robust and larger than for non–Hispanic whites, with a moderately sized, statistically significant 2SLS coefficient of 1.03 (column 7; 1.13 in column 8). The effects of adult immigrants with exactly 12 years education and with more than 12 years are imprecisely estimated. Prompted by the Evans et al. (2012) finding of the importance of the crack epidemic for black schooling, I have also estimated regressions controlling for the murder rate. The unreported coefficient is statistically insignificant, as it is in the regressions below by gender: it is probably necessary to have yearly data to successfully identify the effect.[24]

From Tables 4–7, I conclude that the labor market channel through which immigration might operate works as expected, with a larger effect for native blacks than non–Hispanics whites, also as expected. There is evidence for the schooling channel in least squares estimation, but it is imprecisely estimated under 2SLS, and while moderately sized effects can be ruled out for natives generally and for non–Hispanic whites, this is not the case for blacks.

[24] The murder data are from the Federal Bureau of Investigation's Uniform Crime Reports at www.ucrdatatool.gov/Search/Crime/Crime.cfm, accessed 30 September 2012. The data are available from 1960 only, so the relevant regressions have fewer observations than those in Table 7.

Finally, I examine native–born Hispanics in Table 8. While the sign of the coefficient on immigrants aged 11–17 is always negative, it is imprecisely measured in many specifications (first row). The evidence of a positive effect of adult immigrants with less than 12 years of education is not robust, since in differenced specifications the sign is negative (columns 6–8). Another difference from results for other native groups is that immigrants adults with more than 12 years of education have relatively large positive effects, even under 2SLS. The results for Hispanics therefore appear to provide only weak support for the hypotheses being tested. In the next section, however, I show that this is because the Table 8 regressions are misspecified.

3.3 Distinguishing child immigrants according to parental characteristics

Distinguishing among immigrants aged 11–17 according to their parents' education proves helpful for interpreting the effects of immigration on native–born Hispanics, and these results are presented in column 2 of Table 9. Column 1 reproduces the fixed effects specification of Table 8 (column 4). As hypothesized (albeit not for Hispanics specifically), the coefficient on child immigrants with parents with less than 12 years of education is negative and statistically significant, with the relatively large magnitude (in absolute value) of -2.00 in column 2, while the coefficient on child immigrants with more educated parents is positive and statistically significant, with a relatively large coefficient of 3.46.

The magnitude of the two coefficients implies a one percentage point increase in the relevant immigrant share changes the 12–year completion rate by 2–3 percentage points. This compares to shares of immigrants in the 11–17 population of 3.3% with parents with less than 12 years schooling and 4.7% for those with a parent with at least 12 years schooling, and a Hispanic completion rate of 81.3%.[25] These numbers imply moderate elasticities of -0.08 for children of unskilled parents and 0.20 for children of more educated parents.

[25] The shares of immigrants in the population 11–17 are computed using Hispanic weights, and are hence higher than implied in Figure 2. See Appendix Table 4.

It appears that in the specification with the undifferentiated child immigrant variable, the unskilled adult immigrant coefficient was capturing the opposing effects of the unskilled adult immigrants and their children: the coefficient on adult immigrants with less than 12 years of education rises from 0.48 in column 1 to a statistically significant 1.74 in column 2, in accordance with the theory. At the same time, the effect of child immigrants with more educated parents was being picked up by the educated adult immigrant variable in the simpler specification: the coefficient on adult immigrants with more than 12 years of education falls from the anomalously high 1.87 in column 1 to -0.35 (and statistically insignificant) in column 2.

The coefficient on the small share of the 11–17 year–old population that is immigrants with no parent in the household is strongly negative (column 2). There are several possible explanations for this. The first is causal: such immigrants, who tend to have very low enrollment rates, drop out early from school and have very negative peer effects in the short time they are in school in the United States. If this is so, one might expect a similarly negative impact of children of the lowest educated immigrants. However, unreported regressions splitting parental dropouts into those with more or less than nine years of education do not point to clear differences between the two groups. Two other explanations rely on endogeneity, and imply a negative bias on the coefficient. To the extent that the young immigrants move to the United States without their parents in order to work (Oropesa and Landale 2009), they are the immigrants most likely to move to states with (imperfectly controlled) favorable labor market conditions for youth, conditions which would incite the closely substitutable native students to drop out. To the extent that some young immigrants move to the United States with their parents and initially attend school, (imperfectly controlled) favorable youth labor market conditions are likely to entice both immigrants and natives to drop out of school to work, and hence also to leave home.

A second possible way in which the regressions of Table 8 may be misspecified is through the omission of information on the presence of second–generation immigrants among children. Some such natives have limited English, and might affect other natives in a similar way to (first–generation) immigrant children. While it is not possible to identify which natives aged 21–27 whose education I am examing have immigrant parents, the state's share of 11–17 year olds who are natives with immigrant parents may be computed, and I add this control in column 3 instead of splitting the child immigrant covariate by parental education. The effect is somewhat similar to distinguishing child immigrants by parental education: the positive coefficient on adult immigrants with less than 12 years of schooling becomes larger and statistically significant (coefficient 1.07), while the coefficient on the share of the population that is immigrant age 11–17 becomes more negative, and statistically significant (coefficient -0.88; more negative than the coefficient for blacks in Table 7). The coefficient on second–generation immigrants themselves is -0.72 and statistically significant, suggesting that second–generation immigrants reduce the education of Hispanic natives. In column 4, I include all the covariates. The specification of column 4 is robust to differencing (these results are not reported), contrary to the specification in Table 8.

Since it is not possible to instrument all the immigrant variables in the expanded specification of column 2, it is not possible to make statements about causal effects. Nevertheless, Table 9 suggests that for Hispanic natives, just as for other natives, adult immigrants with less than 12 years schooling have a positive effect on completing 12 years of schooling, and that for Hispanic natives, classmates with educated parents have positive effects, while classmates with poorly educated parents have detrimental effects. It is likely that the large effect of child immigrants with no parent in the household reflects endogeneity.

I present the regressions corresponding to column 4 for native non–Hispanic whites (column 5) and native–born blacks (column 6). For these natives, there is no indication that grouping all child immigrants introduced misspecification, as the coefficients on the adult immigrant variables do not change much when the child immigrant variable is split into components. Neither is there a clear distinction between the effects of child immigrants with lower and higher education parents, and indeed, these coefficients are all statistically insignificant. Nor does the presence of second–generation children appear to matter. The coefficient on child immigrants with no parent in the household is not nearly as negative for non–Hispanic whites as for native Hispanics, and is positive for blacks. The larger correlation for native Hispanics may be related to the fact that in recent decades, such child immigrants are very disproportionately Mexican–born and are more likely to live in states with Hispanic natives.

3.4 Robustness checks and further results

For the sample of all native–born, I can test the sensitivity of the results to classifying GED holders as having less than 12 years of education. There is no apparent way to do this for the 12–year completion rate adjusted for sex and age (and race and ethnicity), so I use the unadjusted 12–year completion rate as the basis for the state panel analysis. In Table 10, I present results from regressions with a single immigration covariate (upper panel), and from regressions with immigration split by age and education (lower panel). The fixed effects results in column 1 (corresponding to column 3 in Tables 2 and 4) and column 2 show that the failure to adjust for sex and age has no effect in the upper panel, but in the lower panel reduces the absolute value of the coefficients, rendering the coefficient on child immigrants statistically insignificant.

Reclassifying the GED holders in column 3 increases the immigration impact from 0.20 to 0.31 in the upper panel; in the lower panel it generally increases slightly both the absolute value of the coefficients and the standard errors, leaving the results qualitatively the same. The use of the differenced specification in column 4 returns the upper panel coefficient to 0.23, and in the lower panel makes the coefficient on child immigrants statistically significantly negative once more. 2SLS (using the preferred instruments) in column 5 increases the coefficient in the upper panel to 0.48, larger than the coefficient of 0.34 in Tables 2 and 3 column 7, though not statistically significantly so. In the lower panel, 2SLS flips the sign of the insignificant coefficient on child immigrants and raises the point estimate on adult dropout immigrants to 0.97, a similar magnitude to the 0.81 estimated in Table 4 column 7. The coefficients on the two other immigrant variables become large in absolute value, but are statistically insignificant. The comparison of Table 10 with earlier tables indicates that natives do adjust to schooling through changes in years of regular high school, and schooling adjustment is not driven by changes in the propensity to obtain a GED.

I have also conducted further robustness checks on regressions with the original dependent variable. The student–teacher ratio for all grades in public schools always has a statistically insignificant coefficient (the sources are the Digest of Education Statistics and the Biennial Survey of Education, various years). Matching state characteristics to natives' state of current residence, rather than state of birth, does not change the qualitative picture. For all races and ethnicities together, coefficients are smaller in absolute value, while for minorities there is no clear pattern. These results are not reported.

As a check on the effects of immigrant children distinguished by the education of the parent, I can instead distinguish immigrant children by their birth region (Table 11); including both birth region and parental education causes all relevant coefficients to become insignificant). Child immigrants from Latin America have a statistically significant negative effect on natives generally (coefficient -0.49 in column 1), and this effect is statistically significant at the 10% level for each of the three native groups considered (columns 2–4). This is consistent with a negative effect of child immigrants from low SES or low education families. The effect of Asian children is negative and significiant at the 10% level for all natives (coefficient -0.56 in column 1) and negative but insignificant for native groups individually. The effect of European, Canadian, Australian and New Zealand children is small and insignificant for all natives, but has a relatively large significant positive effect for native–born Hispanics (coefficient 1.73 in column 4), whereas the coefficient for blacks is negative and statistically insignificant in column 3 (and is zero for non–Hispanic whites in column 2). This is consistent with native–born Hispanics responding positively to high–SES or high–education schoolmates or neighborhood companions.

Native males and native females could be differently affected by immigration, either due to differing degrees of labor market substitutability with immigrants, or due to differing performance in school, or different types or degrees of interaction with immigrants in school (or the neighborhood). I have therefore repeated all the analysis distinguishing natives by gender. I present two panels of results in Table 12, using a single immigration covariate in the upper panel, and distinguishing among immigrants in the lower panel. The analysis for native Hispanics shows that the subtle effects of immigrants age 11–17 come entirely from the effects on males (lower panel, columns 1 and 2). The fact that these effects are found only for males and not for females means it is unlikely that they are explained by endogeneity of location of various types of child immigrants. The table also shows that the negative sign of the net effect of immigration on native Hispanics in the 2SLS comes from native males, though standard errors remain large (upper panel, columns 3 and 4).[26]

The results for native–born blacks (columns 5–8) show coefficients for native–born black females are very imprecisely estimated, indicating that the statistical significance of earlier results was driven by males, though the general pattern of results is similar for the two genders. The analysis for all natives and white non–Hispanic natives does not reveal any noteworthy differences by gender (see Appendix Table 8).

Finally, I reestimate the regressions using 130 metropolitan areas (cities) instead of 49 states, reporting results for each race and ethnicity sample in Table 13. The upper panel shows the effect of overall immigration in the 11–64 age group. In column 1, the fixed effects specification shows a very small positive and statistically significant effect of 0.09 for all natives. 2SLS increases the coefficient substantially to a statistically significant 0.26 in column 2, similar to the 0.34 found for the states sample in Tables 2 and 3 column 7. The F–statistic for the excluded instrument in the first stage is 14. However, the 2SLS coefficients for the separate race and ethnic groups are statistically insignificant (columns 4, 6 and 8).

In the lower panel, where types of immigrant are distinguished, the odd columns show that fixed effects estimation yields small, insignificant coefficients whose point estimates do not correspond to the hypotheses of the paper. On the other hand, the even columns,

[26] In British schools, Lavy et al. (2012) find greater sensitivity of girls than boys to peer effects in British schools, while Gibbons and Telhaj (2012) find no difference.

containing 2SLS coefficients, indicate point estimates larger in absolute value than in the odd columns, and similar to those for the states sample. For all natives in column 2, the coefficient on adult immigrants with less than 12 years education is a statistically significant 0.70, compared to a corresponding coefficient of 0.81 for the states sample (Table 4 column 7), and the point estimates on child immigration are identical for cities and states (-0.18). The coefficients for the separate race and ethnic groups are statistically insignificant, however, and as the tables notes indicate, the instruments are weak in the first stage for the 2SLS regressions of the lower panel, leading to generally high standard errors in the second stage. Native Hispanics are a special case, as I do not attempt to instrument, and so cannot judge if 2SLS returns the patterns found in the state data. It is noteworthy that when the regression of column 7 (lower panel) is run with state data confined to 1980–2010, the point estimates of the coefficients are almost unchanged compared to the regression using all years of data.

It appears that analysis at the level of the city introduces more endogeneity to be repaired by 2SLS, and the 2SLS results are similar to those at the state level using all years. However, generally standard errors are large due to weak first stages when types of immigrant are distinguished. The greater need for 2SLS precludes analysis of child immigrants distinguished according to their English ability, as 2SLS would involve too many instruments; the unreported coefficients from fixed effects estimation are all small and statistically insignificant.

4 Conclusion

In this paper, I have shown that natives' probability of completing 12 years of education is increased by immigration, albeit by a small magnitude, an effect that is larger for blacks than non–Hispanic whites. The effect for Hispanic natives, on the other hand, is a small (statistically insignificant) negative one. Consistent with the hypothesis that this education upgrading is prompted by a higher return to high school due to immigration of high school dropouts, I find that natives' probability of completing 12 years of education is increased by greater presence of adult immigrants with less than 12 years of education. This effect is larger for native minorities than for non–Hispanic whites, which probably reflects a greater effect for individuals from low SES families generally. Even for minorities, however, the effects are small, though larger than the effect of immigrants of all ages.

While immigrants age 11–17 have no effect on the 12–year completion rates of non–Hispanic white natives, moderate negative effects on the completion rates of black natives cannot be excluded, though the instruments are not sufficiently powerful to identify the size of the effect precisely. For native–born Hispanics, the effects of child immigrants are more subtle, strongly dependent on the education of the child immigrants' parents: children of parents with less than 12 years of education have a deleterious impact on native completion rates, while children with a parent with 12 or more years of education have a positive effect. These effects are of moderate size, larger than any others found in the study, and are found for males only. That some native–born minorities are negatively affected by some child immigrants suggests the need for reform in accommodating immigrant students, particularly those with less educated parents and in schools with many native–born minorities.

Hispanics are the native group living in school districts with the most immigrants, and black and Hispanic males have the lowest native graduation rates, so it is not surprising that native Hispanic males are most sensitive to child immigration. The relatively large negative effect on native Hispanic males of child immigrants of poorly educated parents may be an indicator that native students are most affected in school when exposed to culturally similar immigrants. However, it is not entirely clear why black males, for example, do not also increase their 12–year completion rates in response to child immigrants of educated parents. The fact that native Hispanics increase their 12–year completion rates in response to European, Canadian, Australian and New Zealand child immigrants, while blacks do not, suggests that the explanation can be at most in part related to similarity between native and immigrant Hispanics, and at most in part due to differential exposure to immigrant children of higher SES, since native blacks and Hispanics have very similar exposure to white non–Hispanic immigrant children.

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Data Appendix

Analysis of Metropolitan Areas

I have linked metropolitan areas (cities) across the 1980–2000 censuses and the 2010 ACS using code generously provided by Ethan Lewis. It is only from 1980 onwards, when the census sample rises from 1% to 5%, that samples are large enough to perform the analysis by city. I use the 130 cities (excluding Honolulu, for consistency with the state analysis) for which there are at least 75,000 observations on native 21–27 year olds in the whole period; even using so few cities, some city–year cells are very small, especially for minorities. I retain the same specifications as for the state–level analysis, merely replacing trends based on 1940 values of certain variables with trends based on their 1980 values. I calculate the instruments in a parallel way, computing national shares of immigrants from different countries across cities in 1980, and retaining the same set of countries as for the state–level analysis. The means of the city–based samples are available upon request.

GED Analysis

I correct the 12–year completion rates using annual published tables on GEDs awarded by state and age, using the Heckman and LaFontaine (2010) appendix for methodological guidance. I scale down the published figures so as to reflect only native–born GED recipients with the help of the pooled 1999–2001 October supplement to the Current Population Survey and the 2008–2010 pooled ACSs, but the CPS samples sizes do not permit a break–down by race and ethnicity. The published GED data indicate that only a small minority (less than 5%) of GED recipients had completed 12 years of high school, so I reduce the reported number of native completers by the estimated number of native GED recipients to obtain a "true" number of native completers of 12 years of schooling.

Official GED Data

The basic data on GEDs are taken from the annual GED Statistical Reports ("Who Took the GED?", available at www.acenet.edu/Content/NavigationMenu/ged/pubs/GED_Archived_Annual_.htm, accessed 23 March 2012). From 1989 onwards, statistics are available for the age distribution by state of GEDs awarded. A few missing and implausible values are filled in with linear interpolation, and for a number of states that began reporting the statistics only later, linear extrapolation back to 1989 is used. From 1974 to 1988, the age distribution is only available for those tested, rather than those who were awarded a GED. For these years, I compute for each age group the pass rate in the most recent year for which both the distribution of test–takers and the distribution of credentials awarded are available (1989 for most states), and use the pass rates to adjust the distribution of test–takers in 1974–1988 to obtain the age distribution of GEDs awarded. New York begins reporting information by age only from 1984, and I impute 1974–1983 values by extrapolating the 1984–1988 trends. Before 1974, no information is provided by age and state, and for 1969–1973, I assume the age distribution is the same as in 1974.

The age categories are not the same in all years, and the age categories for test–takers are not always the same as those for GEDs awarded. When the age categories change (in general they become finer over time), I split the coarser categories based the distribution in the nearest year with finer categories. Even the finest divisions do not give the age distribution for each year of age (above age 19), so after I harmonize the data at the finest categorization, I assume that GEDs are uniformly distributed within an age group. I assume that recipients 16 or younger are age 16. The final age distributions are combined with statistics for total GEDs awarded by state and year to obtain GEDs awarded by state and year for each year of age.

I use these numbers to calculate how many 21–27 years olds held GEDs in 1980, 1990, and 2000, assuming that by the census date, one third of the year's GEDs had been received. For "2010", I average the age 19–25 stock in 2008, the age 20–26 stock in 2009, and the age 21–27 stock in 2010.

Birthplace of GED recipients

The official GED statistics give no breakdown by birthplace, a breakdown necessary since I seek statistics for natives only. The October supplement to the CPS allows those whose highest degree is a GED to be identified, and from 1994 onwards, the basic monthly CPSs contain birthplace. The ACS contains both the GED and birthplace information. I therefore use the pooled 1999–2001 October CPSs and the pooled 2008–2010 ACSs to estimate the immigrant share among those who completed a GED but had no college. The sample sizes are too small, however, to do this for each state (and too small to allow the calculation of the shares of minorities, which I do not attempt). I instead distinguish three groups of states in the CPS and six groups in the ACS, based on the share of immigrants in the population aged 21–27.

Combining the data sources

For 2000 and 2010, it is straightforward to adjust the total GED stocks calculated from official GED data with the share of immigrants from the census and ACS data to reflect natives only. For 1980 and 1990, I assume that the share of immigrants among GED holders aged 21–27 changes proportionately to immigrants' share in the 21–27 population. I calculate the latter shares for the three state groups using the 1980 and 1990 censuses.

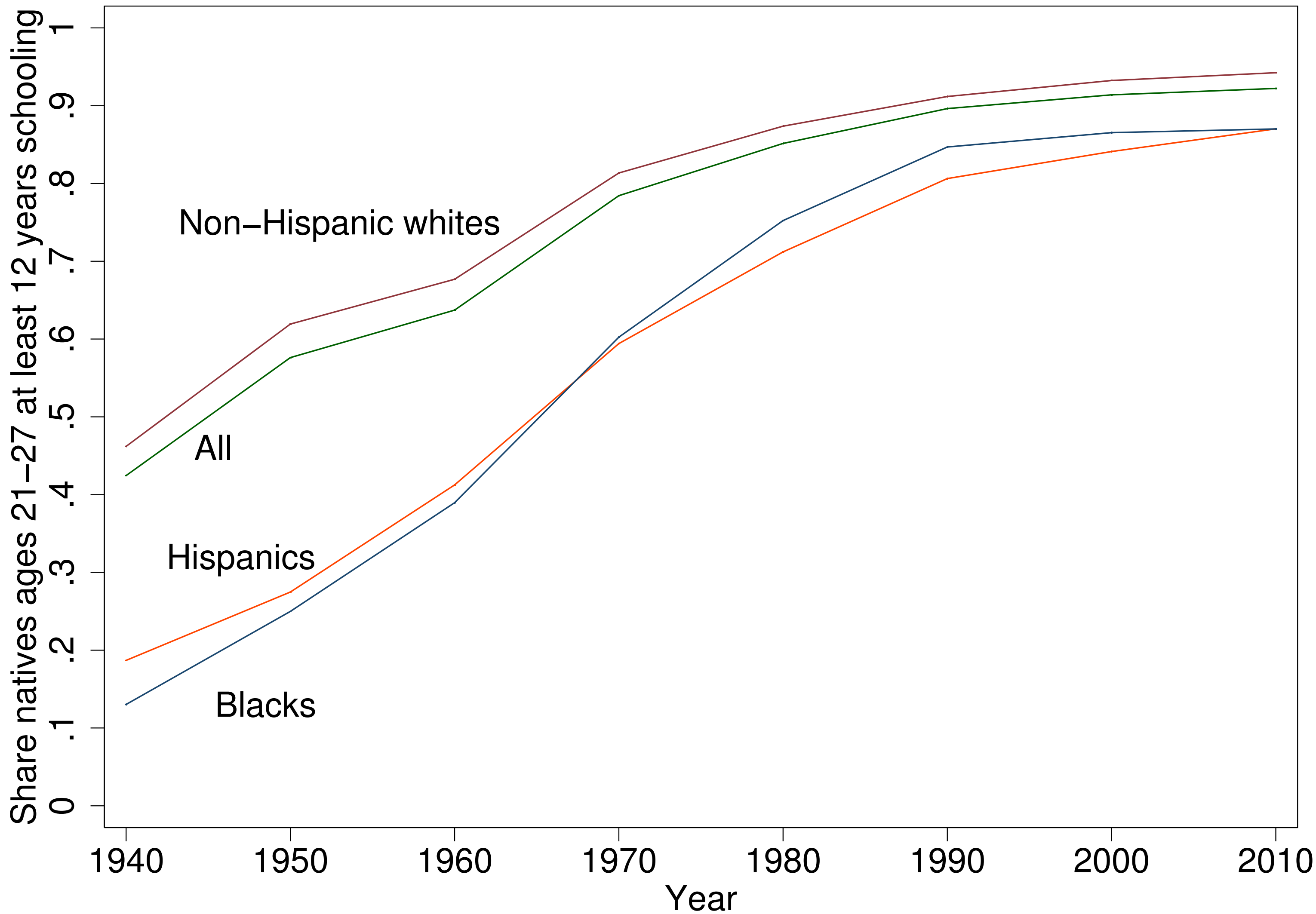
I then calculate the share of the native population aged 21–27 that holds a GED by dividing the GED stocks of natives 21–27 by the native population aged 21–27 (or the equivalent for 2008–2010), the latter computed by summing the census weights for natives, including those whose native status is imputed. I subtract this fraction from the fraction of natives with 12 years of schooling or more, which had been calculated based on a sample with non–missing (and non–imputed) education and birthplace.

GED–adjusted statistics

The first row of Appendix Table 1 shows the national raw native 12–year completion rates, while the second row shows the rates adjusted to count GED holders as non–completers. The adjustment reduces the completion rate by five percentage points in 1980, and by more in later years. Nevertheless, the adjusted completion rate is estimated to have increased between 1980 and 2010, with stagnation between 1990 and 2000. The third row provides the 12–year completion rate as directly measured in the 2010 ACS. This rate of 87.8% lies between the rates of the first two rows (84.9% adjusted, 92.3% unadjusted), indicating that too many GEDs have been subtracted in the adjustment. One reason for this would be that some of the GED holders subtracted in fact went on to obtain more education, and do not appear as GED holders in the 2010 ACS. In panel B, I consider high school graduation rates, which must be lower than 12-year completion rates. I measure this rate directly in the pooled 2008–2010 ACSs as 86.3%.

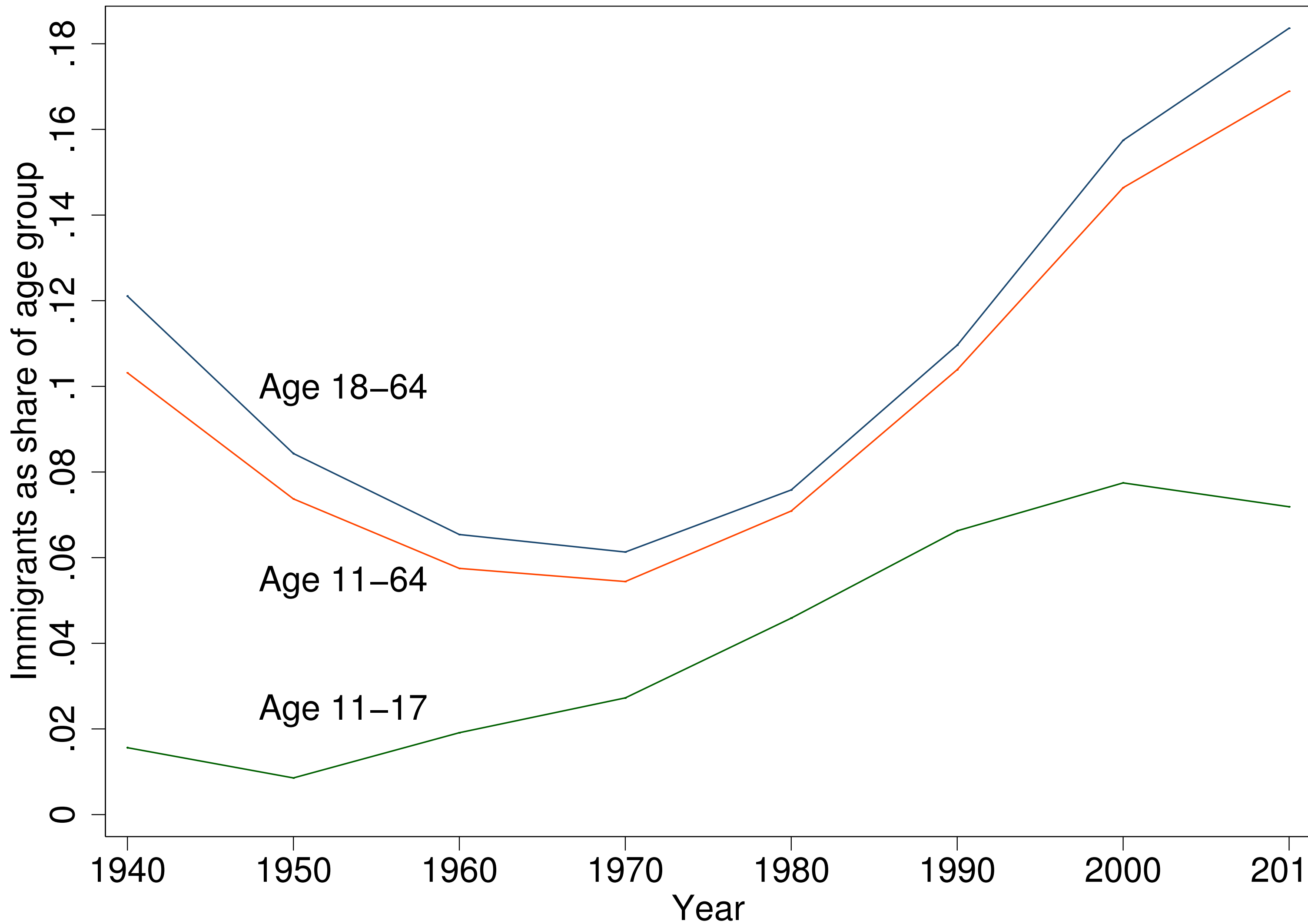
In panel C, I present for comparison the Heckman and LaFontaine (2010) high school graduation estimates for 1970–2000. They use age groups that do not quite match mine, and remove only recent immigrants from their GED counts. I assume that their raw numbers for 1970 and 1980 include 12–year completers, since they cannot be distinguished from high school graduates, and that their raw numbers from 1990 onwards exclude them. This would account for the lack of progress between 1980 and 1990 in their graduation rates, in constrast to my adjusted 12–year completion rates in panel A. Our 1980 rates should be similar, which they are. The graduation rate for 2010 measured directly (panel B) appears high compared to the estimated Heckman and LaFontaine rates for 2000.

Figure 1: Share of natives with at least 12 years of schooling, by race and ethnicity



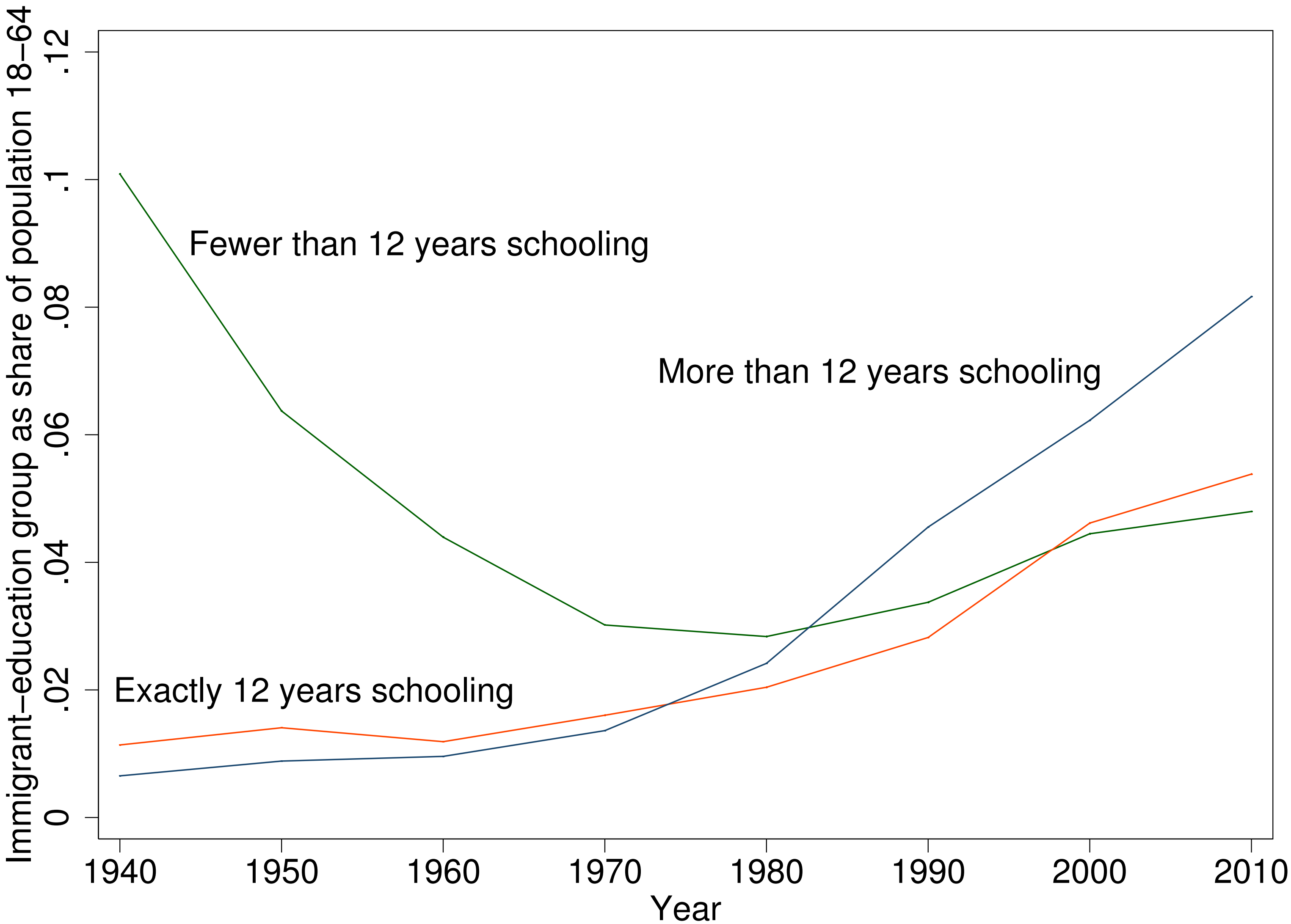
Note: The share of natives aged 21–27 who have completed at least 12 years of schooling.
Source: U.S. Census 1940–2000, American Community Survey 2008–2010.

Figure 2: Immigrant share in various age groups



Note: Immigrants as a share of each age group.
Source: U.S. Census 1940–2000, American Community Survey 2008–2010.

Figure 3: Immigrant education groups as share of the population, ages 18–64



Note: Immigrants 18–64 with various education levels as share of the total population aged 18–64.
Source: U.S. Census 1940–2000, American Community Survey 2008–2010.

Table 1: Interaction of natives by ethnicity with immigrants, school district level 1990

	Immigrants	Hispanic immigrants	White non-Hispanic immigrants	Asian immigrants	Immigrant parents	Hispanic immigrant parents
	(1)	(2)	(3)	(4)	(5)	(6)
A. National level shares						
Overall	0.042	0.023	0.0055	0.010	0.102	0.044
In native's district	0.038	0.020	0.0052	0.009	0.078	0.031
In native white's district	0.025	0.011	0.0045	0.008	0.069	0.025
In native black's district	0.052	0.028	0.0060	0.011	0.120	0.055
In native Hispanic's district	0.107	0.074	0.0084	0.017	0.201	0.139
In immigrant's district	0.143	0.088	0.0138	0.028	0.312	0.159
B. Difference in state shares						
Native black-native white	0.027	0.016	0.0014	0.005	0.061	0.033
Native Hispanic-native white	0.036	0.027	0.0017	0.004	0.072	0.049
Native Hispanic-native black	0.009	0.011	0.0003	-0.002	0.012	0.015

Source: School District Database (tabulations of the 1990 census at school district level).

Notes: Based on school districts including a high school: 10,935 school districts for children and 12,503 for parents of children (the reason for the difference is unknown). In panel B, the shares by race/ethnicity are calculated at the state level, then averaged across states weighting with total enrollment in the state. An immigrant is a child of school age born abroad; an immigrant parent is someone born abroad who is the parent of a school-aged child (born anywhere). White denotes non-Hispanic white.

Table 2: Effects of immigrants in population 11-64 on native probability of completing 12 years education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Fixed effects				10-year differences	
						WLS	2SLS 1	2SLS 2
Share population 11-64	0.03	0.07	0.20**	0.29**	0.22**	0.26**	0.34**	0.31**
which is immigrant; t-10	(0.13)	(0.08)	(0.06)	(0.06)	(0.07)	(0.05)	(0.11)	(0.15)
Unemployment rate	--	0.69*	-0.11	-0.09	-0.03	-0.07	-0.09	-0.07
age 18-24; t-10		(0.38)	(0.17)	(0.17)	(0.16)	(0.10)	(0.10)	(0.10)
Unemployment rate	--	-0.68	0.31	0.27	0.22	0.25	0.28	0.23
age 25-54; t-10		(0.79)	(0.40)	(0.41)	(0.37)	(0.24)	(0.24)	(0.23)
Share of native population	--	-1.36**	-0.24	-0.20	0.08	0.02	0.03	0.03
which is age 11-17; t-10		(0.36)	(0.25)	(0.25)	(0.22)	(0.17)	(0.18)	(0.18)
Share workers in agriculture 1940*year	--	--	0.006	0.006	0.003	0.047	0.053	0.049
			(0.003)	(0.003)	(0.003)	(0.033)	(0.034)	(0.035)
Share white natives 21-27	--	--	0.007**	0.007**	0.006**	0.067**	0.071**	0.070**
less than 12 years school 1940*year			(0.003)	(0.003)	(0.003)	(0.027)	(0.028)	(0.029)
Log personal income per capita; t-10	--	--	--	--	0.100**	--	--	--
					(0.026)			
BEA regions*year (p-value)	--	--	0.00	0.00	0.00	0.00	0.00	0.00
Outcome adjusted for race, ethnicity	--	--	--	Yes	Yes	Yes	Yes	Yes
R^2	0.91	0.92	0.97	0.97	0.98	0.85	--	--
Observations			343				294	

Notes: The dependent variable is the share of natives age 21-27 who have completed 12 years of education, adjusted at the individual level for age and sex, and also for black, Asian, missing race, Mexican, Cuban, Puerto Rican, other Hispanic, and missing Hispanic in columns 4-8. Estimation is by weighted least squares (columns 1-6), and two-stage least squares (columns 7 and 8), with weights w the inverse of the squared standard errors on the state-year interaction coefficient in the individual regression for columns 1-5, and $1/(1/w_t+1/w_{t+10})$ for columns 6-8. All specifications include year dummies; fixed effects specifications also include state dummies. The dependent variable is based on 1950-2010 data, the independent variables on data from 1940-2000. The instruments in columns 7 and 8 are based on the 1940 distribution of immigrants from different regions (see text). Standard errors are clustered by state and reported in parentheses.

Table 3: Effects of immigrants in population 11-64 on native probability of completing 12 years education, by race and ethnicity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Fixed effects				10-year differences	
						WLS	2SLS 1	2SLS 2
All natives	0.03	0.07	0.20**	0.29**	0.22**	0.26**	0.34**	0.31**
	(0.13)	(0.08)	(0.06)	(0.06)	(0.07)	(0.05)	(0.11)	(0.15)
Non-Hispanic white natives	-0.00	0.01	0.15**	--	0.11	0.14**	0.21*	0.13
	(0.08)	(0.06)	(0.07)		(0.07)	(0.06)	(0.11)	(0.11)
Black natives	0.03	0.12	0.47**	0.48**	0.39**	0.45**	0.38**	0.43**
	(0.15)	(0.11)	(0.10)	(0.10)	(0.11)	(0.10)	(0.14)	(0.17)
Hispanic natives	0.12	0.21	0.52**	0.49**	0.34**	0.31**	-0.21	-0.63
	(0.26)	(0.18)	(0.19)	(0.19)	(0.13)	(0.14)	(0.23)	(0.39)
Unemployment rates; t-10	--	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Share of native pop which is age 11-17; t-10	--	Yes	Yes	Yes	Yes	Yes	Yes	Yes
BEA regions*year	--	--	Yes	Yes	Yes	Yes	Yes	Yes
Share workers in agriculture 1940*year	--	--	Yes	Yes	Yes	Yes	Yes	Yes
Share white natives 21-27 less than 12 years school 1940*year	--	--	Yes	Yes	Yes	Yes	Yes	Yes
Outcome adjusted for race, ethnicity	--	--	--	Yes	Yes	Yes	Yes	Yes
Log personal income per capita; t-10	--	--	--	--	Yes	--	--	--
Observations (all/whites/blacks/Hispanics)			343/343/324/332				294/294/270/283	

Notes: The dependent variable is the share of natives age 21-27 of a racial/ethnic group who have completed 12 years of education, adjusted at the individual level for age and sex, and also for black, Asian, missing race, Mexican, Cuban, Puerto Rican, other Hispanic and missing Hispanic in columns 4-8. Estimation is by weighted least squares (columns 1-6), and two-stage least squares (columns 7 and 8), with weights w the inverse of the squared standard errors on the state-year interaction coefficient in the individual regression for columns 1-6, and $1/(1/w_t+1/w_{t+10})$ for columns 6-8. All specifications include year dummies; fixed effects specifications also include state dummies. The dependent variable is based on 1950-2010 data, the independent variables on data from 1940-2000. The instruments in columns 7 and 8 are based on the 1940 distribution of immigrants from different countries (see text). Standard errors are clustered by state and reported in parentheses. Each coefficient is from a different regression.

Table 4: Effects of immigrants by age and education on native probability of completing 12 years education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Fixed effects				10-year differences	
						WLS	2SLS 1	2SLS 2
Share population 11-17 which is immigrant; t-10	-0.55**	-0.62**	-0.45**	-0.43**	-0.36**	-0.29**	-0.18	-0.11
	(0.22)	(0.15)	(0.10)	(0.11)	(0.12)	(0.07)	(0.17)	(0.29)
Share population 18-64 which is								
immigrant < 12 years education; t-10	0.90**	0.98**	0.88**	0.90**	0.83**	0.74**	0.81**	0.82**
	(0.24)	(0.16)	(0.15)	(0.15)	(0.15)	(0.14)	(0.16)	(0.17)
immigrant 12 years education; t-10	0.46	0.63	-0.45	-0.37	-0.32	-0.12	0.58	0.60
	(0.62)	(0.57)	(0.28)	(0.27)	(0.26)	(0.24)	(0.72)	(0.85)
immigrant > 12 years education; t-10	-0.31	-0.29	0.67**	0.78**	0.64**	0.55**	-0.08	-0.19
	(0.63)	(0.48)	(0.28)	(0.28)	(0.30)	(0.23)	(0.66)	(0.74)
Unemployment rates; t-10	--	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Share of native pop which is age 11-17; t-10	--	Yes	Yes	Yes	Yes	Yes	Yes	Yes
BEA regions*year	--	--	Yes	Yes	Yes	Yes	Yes	Yes
Share workers in agriculture 1940*year	--	--	Yes	Yes	Yes	Yes	Yes	Yes
Share non-Hispanic whites less than 12 years education 1940*year	--	--	Yes	Yes	Yes	Yes	Yes	Yes
Outcome adjusted for race and ethnicity	--	--	--	Yes	Yes	Yes	Yes	Yes
Log personal income per capita; t-10	--	--	--	--	Yes	--	--	--
R^2	0.94	0.95	0.98	0.98	0.98	0.87	--	--
Observations			343				294	

Notes: The dependent variable is the share of natives ages 21-27 who have completed 12 years of education, adjusted at the individual level for age and sex, and also for black, Asian, missing race, Mexican, Cuban, Puerto Rican, other Hispanic and missing Hispanic in columns 4-8. Estimation is by weighted least squares (columns 1-6), and two-stage least squares (columns 7 and 8), with weights *w* the inverse of the squared standard errors on the state-year interaction coefficient in the individual regression for columns 1-5, and $1/(1/w_t+1/w_{t+10})$ for columns 6-8. All specifications include year dummies; fixed effects specifications also include state dummies. The dependent variable is based on 1950-2010 data, the independent variables on data from 1940-2000. The instruments in columns 7 and 8 are based on the 1940 distribution of immigrants from different countries (see text). Standard errors are clustered by state and reported in parentheses.

Table 5: First stage of two-stage least squares (preferred instruments)

	(1)	(2)	(3)	(4)	(5)
	Share of immigrants in age group		Share of population aged 18-64 which is immigrant with		
	11-64	11-17	less than 12 years education	exactly 12 years education	more than 12 years education
Predicted share population 11-64 which is immigrant	0.27** (0.05)	--	--	--	--
Predicted share population 11-17 which is immigrant	--	0.68** (0.26)	-0.00 (0.07)	-0.04 (0.02)	0.05 (0.06)
Predicted share population 18-64					
which is immigrant < 12 years education	--	0.00 (0.09)	0.72** (0.07)	0.04 (0.04)	0.10 (0.07)
which is immigrant 12 years education	--	-1.40** (0.52)	-0.82** (0.22)	0.24** (0.09)	-0.24 (0.14)
which is immigrant > 12 years education	--	0.67** (0.25)	0.44** (0.11)	0.09 (0.06)	0.43** (0.09)
F statistic for joint significance	24.9	8.3	65.5	43.4	29.3
Angrist-Pischke F statistic	--	15.1	103.7	55.0	18.8
Partial R^2	0.40	0.39	0.56	0.36	0.42
Observations	294	294	294	294	294

Notes: Estimation is by weighted least squares on 10-year differenced data, with weights $1/(1/h_t+1/h_{t+10})$, where h is the denominator of the dependent variable in each regression: population age 11-64 in column 1, population age 11-17 in column 2, population age 18-64 with less than 12 years education column 3, population age 18-64 with exactly 12 years education column 3, population age 18-64 with more than 12 years education column 5. All specifications include year dummies and the (differenced) non-immigrant covariates of Table 2 column 7. The excluded instruments (predicted shares) are based on historical patterns of settlement by age and education. The dependent variable and excluded instruments are based on data from 1940-2000 (i.e. from t-10), while other covariates are based on data from 1950-2010. Standard errors are clustered by state and reported in parentheses. This tables underlies the 2SLS 1 estimates and unspecified 2SLS estimates.

Table 6: Effects of immigrants by age and education on native non-Hispanic whites' probability of completing 12 years education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Fixed effects				10-year differences	
					WLS	2SLS 1	2SLS 2
Share population 11-17 which is immigrant; t-10	-0.49**	-0.50**	-0.29**	-0.27**	-0.16**	0.03	0.11
	(0.17)	(0.14)	(0.10)	(0.11)	(0.07)	(0.17)	(0.22)
Share population 18-64 which is							
immigrant < 12 years education; t-10	0.59**	0.62**	0.53**	0.50**	0.45**	0.41**	0.39**
	(0.17)	(0.13)	(0.15)	(0.16)	(0.14)	(0.13)	(0.13)
immigrant 12 years education; t-10	0.34	0.47	-0.41**	-0.39*	-0.22	0.66	0.83
	(0.49)	(0.45)	(0.22)	(0.21)	(0.17)	(0.48)	(0.55)
immigrant > 12 years education; t-10	-0.11	-0.15	0.58**	0.54**	0.37	-0.29	-0.49
	(0.47)	(0.41)	(0.26)	(0.29)	(0.23)	(0.52)	(0.51)
Unemployment rates; t-10	--	Yes	Yes	Yes	Yes	Yes	Yes
Share of native population age 11-17; t-10	--	Yes	Yes	Yes	Yes	Yes	Yes
BEA regions*year	--	--	Yes	Yes	Yes	Yes	Yes
Share workers in agriculture 1940*year	--	--	Yes	Yes	Yes	Yes	Yes
Share white natives 21-27 less than 12 years school 1940*year	--	--	Yes	Yes	Yes	Yes	Yes
Log personal income per capita; t-10	--	--	--	Yes	--	--	--
R^2	0.94	0.95	0.98	0.98	0.88	--	--
Observations		343				294	

Notes: The dependent variable is the share of native-born non-Hispanic whites ages 21-27 who have completed 12 years of education, adjusted at the individual level for age and sex. Estimation is by weighted least squares (columns 1-5), and two-stage least squares (columns 6 and 7), with weights w the inverse of the squared standard errors on the state-year interaction coefficient in the individual regression for columns 1-4, and $1/(1/w_t+1/w_{t+10})$ for columns 5-7. All specifications include year dummies; fixed effects specifications also include state dummies. The dependent variable is based on 1950-2010 data, the independent variables on data from 1940-2000. The instruments in columns 6 and 7 are based on the 1940 distribution of immigrants from different countries (see text). Standard errors are clustered by state and reported in parentheses.

Table 7: Effects of immigrants by age and education on native blacks' probability of completing 12 years education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Fixed effects				10-year differences	
						WLS	2SLS 1	2SLS 2
Share population 11-17 which is immigrant; t-10	-0.73**	-0.74**	-0.56**	-0.58**	-0.55**	-0.35**	-0.33	-0.35
	(0.29)	(0.19)	(0.16)	(0.17)	(0.17)	(0.15)	(0.28)	(0.58)
Share population 18-64 which is								
immigrant <12 yrs education; t-10	0.64**	0.73**	1.09**	1.11**	1.07**	0.88**	1.05**	1.13**
	(0.34)	(0.23)	(0.21)	(0.22)	(0.25)	(0.20)	(0.23)	(0.24)
immigrant 12 yrs education; t-10	-0.26	0.18	0.04	0.06	0.06	0.38	1.39	1.64
	(0.62)	(0.68)	(0.64)	(0.67)	(0.67)	(0.59)	(1.28)	(1.59)
immigrant > 12 yrs education; t-10	0.65	0.57	1.00*	1.02*	0.97	0.67	-0.51	-0.81
	(0.70)	(0.62)	(0.57)	(0.59)	(0.60)	(0.48)	(1.04)	(1.27)
Unemployment rates; t-10	--	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Share native population age 11-17; t-10	--	Yes	Yes	Yes	Yes	Yes	Yes	Yes
BEA regions*year	--	--	Yes	Yes	Yes	Yes	Yes	Yes
Share workers in agriculture 1940*year	--	--	Yes	Yes	Yes	Yes	Yes	Yes
Share white natives 21-27 less than 12 years school 1940*year	--	--	Yes	Yes	Yes	Yes	Yes	Yes
Outcome adjusted for race and ethnicity	--	--	--	Yes	Yes	Yes	Yes	Yes
Log personal income per capita; t-10	--	--	--		Yes	--	--	--
R^2	0.96	0.97	0.98	0.98	0.98	0.85	--	--
Observations			324				270	

Notes: The dependent variable is the share of native-born blacks age 21-27 who have completed 12 years of education, adjusted at the individual level for age and sex, and also for Mexican, Cuban, Puerto Rican and other Hispanic in columns 4-8. Estimation is by weighted least squares (columns 1-6), and two-stage least squares (columns 7 and 8), with weights w the inverse of the squared standard errors on the state-year interaction coefficient in the individual regression for columns 1-5, and $1/(1/w_t+1/w_{t+10})$ for columns 6-8. All specifications include year dummies; fixed effects specifications also include state dummies. The dependent variable is based on 1950-2010 data, the independent variables on data from 1940-2000. The instruments in columns 7 and 8 are based on the 1940 distribution of immigrants from different countries (see text). Standard errors are clustered by state and reported in parentheses.

Table 8: Effects of immigrants by age and education on native Hispanics' probability of completing 12 years education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Fixed effects				10-year differences	
						WLS	2SLS 1	2SLS 2
Share population 11-17 which is immigrant; t-10	-0.18	-0.39	-0.54**	-0.50*	-0.26	-0.27	-0.59	-0.83
	(0.27)	(0.29)	(0.27)	(0.26)	(0.30)	(0.20)	(0.41)	(1.04)
Share population 18-64 which is								
immigrant < 12 years education; t-10	0.74	1.18**	0.35	0.49	0.30	-0.36	-0.11	-0.36
	(0.48)	(0.51)	(0.46)	(0.48)	(0.49)	(0.42)	(0.51)	(0.51)
immigrant 12 years education; t-10	2.40	1.15	-0.67	-0.49	-0.36	0.10	-3.00*	-4.13*
	(1.35)	(1.17)	(0.83)	(0.79)	(0.84)	(0.86)	(1.77)	(2.34)
immigrant > 12 years education; t-10	-1.68	-0.74	2.27**	1.89**	1.32	1.70**	2.61**	2.81*
	(1.28)	(1.06)	(0.79)	(0.75)	(0.79)	(0.80)	(1.23)	(1.66)
Unemployment rates; t-10	--	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Share of native pop age 11-17; t-10	--	Yes	Yes	Yes	Yes	Yes	Yes	Yes
BEA region trends	--	--	Yes	Yes	Yes	Yes	Yes	Yes
Share agricultural workers 1940*year	--	--	Yes	Yes	Yes	Yes	Yes	Yes
Share white natives 21-27 less than 12 years school 1940*year	--	--	Yes	Yes	Yes	Yes	Yes	Yes
Outcome adjusted for race and ethnicity	--	--	--	Yes	Yes	Yes	Yes	Yes
Log personal income per capita; t-10	--	--	--	--	Yes	--	--	--
R^2	0.91	0.91	0.94	0.94	0.94	0.57	--	--
Observations			332				283	

Notes: The dependent variable is the share of native-born Hispanics age 21-27 who have completed 12 years of education, adjusted at the individual level for age and sex, and also for black, Mexican, Cuban, Puerto Rican and other Hispanic in columns 4-8. Estimation is by weighted least squares (columns 1-6), and two-stage least squares (columns 7 and 8), with weights w the inverse of the squared standard errors on the state-year interaction coefficient in the individual regression for columns 1-5, and $1/(1/w_t+1/w_{t+10})$ for columns 6-8. All specifications include year dummies; fixed effects specifications also include state dummies. The dependent variable is based on 1950-2010 data, the independent variables on data from 1940-2000. The instruments in columns 7 and 8 are based on the 1940 distribution of immigrants from different countries (see text). Standard errors are clustered by state and reported in parentheses.

Table 9: Effects of child immigrants by parental education on natives' probability of completing 12 years education

	Hispanic natives				Non-Hispanic white natives	Black natives
	(1)	(2)	(3)	(4)	(5)	(6)
Share population 11-17 which is immigrant; t-10	-0.50*	--	-0.88**	--	--	--
	(0.26)		(0.28)			
Parents less than 12 years education	--	-2.00**	--	-1.90**	0.06	-1.07
		(0.71)		(0.79)	(0.20)	(0.64)
One parent 12 or more years education	--	3.46**	--	3.01**	-0.29	-0.18
		(0.82)		(0.93)	(0.28)	(0.68)
No parent in household	--	-4.94**	--	-6.64**	-1.99**	0.15
		(1.85)		(2.12)	(0.57)	(1.64)
Share population 18-64 which is immigrant; t-10						
Less than 12 years education	0.49	1.74**	1.07**	2.25**	0.64**	1.19**
	(0.48)	(0.49)	(0.48)	(0.49)	(0.18)	(0.30)
12 years education	-0.49	-1.07*	0.28	-0.32	-0.32	-0.07
	(0.79)	(0.67)	(0.99)	(0.82)	(0.23)	(0.76)
More than 12 years education	1.89**	-0.35	2.60**	0.51	0.79**	0.74
	(0.75)	(0.72)	(0.71)	(0.73)	(0.28)	(0.61)
Share population 11-17 which is 2nd generation immigrant; t-10			-0.72**	-0.74**	-0.12	-0.03
			(0.34)	(0.24)	(0.12)	(0.24)
Other covariates	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.93	0.94	0.94	0.94	0.98	0.98
Observations	332				343	324

Notes: The dependent variable is the share of native—born Hispanics (columns 1-2), non-white Hispanics (column 3) or blacks (column 4) age 21-27, who have completed 12 years of education, adjusted at the individual level for age and sex (all columns); Mexican, Cuban, Puerto Rican and other Hispanic (columns 1-5, 6) and black (columns 1-4). Estimation is by weighted least squares, with weights w the inverse of the squared standard errors on the state-year interaction coefficient in the individual regression. All specifications include year dummies and state dummies. Other covariates are those in Table 2: unemployment rates, cohort size, BEA region trends, 1940 agriculture share trend, 1940 share white natives with less than 12 years education trend. The dependent variable is based on 1950-2010 data, the independent variables on data from 1940-2000. Standard errors are clustered by state and reported in parentheses.

Table 10: Sensitivity of results to treatment of GED holders

	GED holders have 12 years schooling		GED holders have less than 12 years schooling		
	Sex-age adjusted	Not sex-age adjusted	Not sex-age adjusted		
	Fixed effects	Fixed effects	Fixed effects	10-year diffs	10-year diffs 2SLS
	(1)	(2)	(3)	(4)	(5)
Share population 11-64 which is immigrant; t-10	0.20**	0.20**	0.31**	0.23**	0.48**
	(0.06)	(0.07)	(0.10)	(0.10)	(0.18)
Share population 11-17 which is immigrant t-10	-0.45**	-0.18	-0.15	-0.26**	0.46
	(0.10)	(0.16)	(0.15)	(0.12)	(0.41)
Share population 18-64 which is					
immigrant < 12 years education; t-10	0.88**	0.44**	0.56**	0.51**	0.97**
	(0.15)	(0.13)	(0.18)	(0.18)	(0.31)
immigrant 12 years education; t-10	-0.45	-0.39	-0.57	0.40	4.18
	(0.28)	(0.30)	(0.43)	(0.55)	(2.75)
immigrant > 12 years education; t-10	0.67**	0.53*	0.81**	0.05	-3.30
	(0.28)	(0.29)	(0.40)	(0.52)	(2.34)
Average age of natives	--	Yes	Yes	Yes	Yes
Other covariates	Yes	Yes	Yes	Yes	Yes
Observations	343		294		

Notes: The dependent variable is the share of native—born age 21-27 who have completed 12 years of schooling; in column 1 only this is adjusted at the individual level for age and sex. In columns 1 and 2, GED holders are considered to have completed 12 years of schooling, while in columns 3-5 they are not. Estimation is by weighted least squares: column 1 weights k are the inverse of the squared standard errors on the state-year interaction coefficient in the individual regression, columns 2-3 weights are the native population aged 21-27, column 4-5 weights are $1/(1/k_t+1/k_{t+10})$. All specifications include year dummies; fixed effects specifications also include state dummies. Other covariates are those in Table 2: unemployment rates, cohort size, BEA region trends, 1940 agriculture share trend, 1940 share white natives with less than 12 years education trend. The dependent variables are based on 1950-2010 data, adjusted in columns 3-5 with official GED statistics, the independent variables on data from 1940-2000. The instruments in column 5 are based on the 1940 distribution of immigrants from different regions (see text) and correspond to those in 2SLS 1 in earlier tables. Standard errors are clustered by state and reported in parentheses.

Table 11: Effects of child immigrants by birth region on natives' probability of completing 12 years education

	All Natives (1)	Native non-Hispanic whites (2)	Native blacks (3)	Native Hispanics (4)
Share population 11-17 which is immigrant; t-10				
Born in Asia	-0.56*	-0.14	-0.53	-0.32
	(0.29)	(0.27)	(0.36)	(0.63)
Born in Latin America	-0.49**	-0.38*	-0.70*	-0.85*
	(0.23)	(0.21)	(0.36)	(0.43)
Born in Europe/Canada/Australia/New Zealand	0.21	0.10	-0.57	1.73**
	(0.29)	(0.31)	(0.58)	(0.73)
Born in other region	0.70	-0.48	2.36	2.07
	(1.11)	(1.10)	(1.94)	(4.06)
Share population 18-64 which is immigrant; t-10				
Less than 12 years education	0.89**	0.54**	1.06**	0.49
	(0.15)	(0.26)	(0.24)	(0.37)
12 years education	-0.30	-0.33	0.58	-0.19
	(0.30)	(0.20)	(0.73)	(1.11)
More than 12 years education	0.59**	0.55	0.60	1.78*
	(0.27)	(0.26)	(0.59)	(0.97)
Other covariates	Yes	Yes	Yes	Yes
R^2	0.98	0.98	0.98	0.94
Observations	343	343	324	332

Notes: The dependent variable is the share of natives (column 1), native non-white Hispanics (column 2) or blacks (column 3) or Hispanics (column 4) age 21-27, who have completed 12 years of education, adjusted at the individual level for age, and sex; black (columns 1 and 4), Mexican, Cuban, Puerto Rican and other Hispanic (columns 1, 3 and 4). Estimation is by weighted least squares, with weights *w* the inverse of the squared standard errors on the state-year interaction coefficient in the individual regression. All specifications include year dummies and state dummies. Other covariates are those in Table 2: unemployment rates, cohort size, BEA region trends, 1940 agriculture share trend, 1940 share white natives with less than 12 years education trend. The dependent variable is based on 1950-2010 data, the independent variables on data from 1940-2000. Standard errors are clustered by state and reported in parentheses.

Table 12: Results by gender for native-born Hispanics and blacks

	Hispanic Natives				Black natives			
	Fixed effects		10-year differences 2SLS		Fixed effects		10-year differences 2SLS	
	Women	Men	Women	Men	Women	Men	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share pop 11-64	1.67**	0.48**	0.06	-0.31	0.42	0.42**	0.52	0.48**
which is immigrant; t-10	(0.61)	(0.22)	(0.72)	(0.27)	(0.35)	(0.12)	(0.62)	(0.15)
Share population 11-17	--		--	--	-0.52	-0.79**	-1.03	-0.15
which is immigrant; t-10					(0.79)	(0.22)	(1.64)	(0.40)
Parents less than 12 years edu	0.54	-2.27**	--	--	--	--	--	--
	(2.50)	(0.72)						
One parent 12 or more years	1.08	4.10**	--	--	--	--	--	--
	(2.94)	(0.98)						
No parent in household	5.55	-5.80**	--	--	--	--	--	--
	(13.08)	(2.03)						
Share population 18-64								
which is immigrant; t-10								
Less than 12 years education	1.98	1.88**	--	--	0.52	1.44**	0.97	1.23**
	(1.29)	(0.61)			(0.67)	(0.29)	(1.18)	(0.33)
12 years education	-2.77	-1.02	--	--	1.24	-0.52	-4.18	2.81
	(2.69)	(0.81)			(1.96)	(0.86)	(6.50)	(2.04)
More than 12 years edu	2.51	-0.76	--	--	0.01	1.38*	4.23	-1.55
	(2.18)	(0.84)			(1.91)	(0.74)	(5.43)	(1.69)
Other covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	326	326	276	276	314	317	263	264

Notes: The dependent variable is the share of native-born Hispanics (columns 1-4) or blacks (columns 4-8) age 21-27 of the gender specified who have completed 12 years of education, adjusted at the individual level for age and sex, and also for Mexican, Cuban, Puerto Rican, and other Hispanic, and (in columns 1-4) also for black. Estimation is by weighted least squares, with weights *w* the inverse of the squared standard errors on the state-year interaction coefficient in the gender-specific individual regression for columns 1, 2, 5 and 6 and $1/(1/w_t+1/w_{t+10})$ for columns 3, 4, 7 and 8. All specifications include year dummies; fixed effects specifications also include state dummies. Other covariates are those listed in Table 2. The dependent variable is based on 1950-2010 data, the independent variables on data from 1940-2000. The instruments in columns 3,4,7 and 8 are based on the 1940 distribution of immigrants from different regions (see text), and correspond to those in 2SLS 1 in earlier tables. Standard errors are clustered by state and reported in parentheses.

Table 13: Sensitivity of results to using metro areas and restricting the sample to 1980-2010

	All Natives		Native non-Hispanic whites		Native blacks		Native Hispanics	
	Fixed effects	10-year differences 2SLS	Fixed effects	10-year differences 2SLS	Fixed effects	10-year differences 2SLS	Fixed effects	10-year differences 2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share pop 11-64	0.09**	0.26**	0.03	0.10	0.18**	0.41	0.34**	0.12
which is immigrant; t-10	(0.04)	(0.09)	(0.03)	(0.08)	(0.08)	(0.27)	(0.12)	(0.15)
Share population 11-17	-0.04	-0.18	0.02	0.16	-0.10	-0.49	--	--
which is immigrant; t-10	(0.05)	(0.32)	(0.04)	(0.28)	(0.18)	(0.95)		
Parents less than 12 years edu	--	--	--	--	--	--	-0.06	--
							(0.18)	
One parent 12 or more years	--	--	--	--	--	--	0.07	--
							(0.14)	
No parent in household	--	--	--	--	--	--	-0.33	--
							(0.42)	
Share population 18-64								
which is immigrant; t-10								
Less than 12 years education	0.10	0.70**	0.06	0.39*	0.19	1.39	0.13	--
	(0.12)	(0.30)	(0.08)	(0.23)	(0.26)	(0.91)	(0.17)	
12 years education	0.13	0.12	0.01	0.58	-0.11	-0.53	0.08	--
	(0.15)	(0.86)	(0.13)	(0.71)	(0.31)	(2.54)	(0.19)	
More than 12 years edu	0.12	0.32	0.00	-0.50	0.52**	1.38	0.09	--
	(0.11)	(0.88)	(0.10)	(0.77)	(0.25)	(2.62)	(0.12)	
Other covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	390	260	390	260	390	260	390	260

Notes: The dependent variable is the share of natives age 21-27 of a racial/ethnic group who have completed 12 years of education, adjusted at the individual level for age and sex, as well as race and ethnicity (except columns 3 and 4). Estimation is by weighted least squares, with weights w the inverse of the squared standard errors on the metro area-year interaction coefficients in the individual regression (odd columns), or with weights $1/(1/w_t+1/w_{t+10})$ (even columns). All specifications include year dummies and metro area dummies. Other covariates are those listed in Table 2, or their equivalent based where relevant on 1980 rather than 1940. The dependent variable is based on 1990-2010 data, the independent variables on data from 1980-2000. Standard errors are clustered by metro area and reported in parentheses. The instruments are based on the 1940 distribution of immigrants from different regions (see text), and correspond to those in 2SLS 1 in earlier tables. The F-statistic for the excluded instrument in the first stage is 13.9 in the upper panel (even columns). The Angrist-Pischke F-statistics for the four instruments in the first stage of the lower panel are 1.0, 3.9, 2.1 and 3.5 (even columns).

Appendix Table 1: Native high school completion and graduation rates 1970-2010

	1970	1980	1990	2000	2010
A. 12-year completion rate, age 21-27					
Unadjusted – GEDs have 12 years education	0.784	0.852	0.897	0.914	0.923
Adjusted with official GED statistics	--	0.803	0.831	0.830	0.849
Adjusted using micro-data to exclude GEDs	--	--	--	--	0.878
B. High-school graduation rate, age 21-27					
Only regular diploma-holders are graduates	--	--	--	--	0.863
C. High-school graduation rate (Heckman and LaFontaine)					
Adjusted with official GED statistics, age 20-23	0.807				
Adjusted with official GED statistics, age 20-24	--	0.786	0.794	0.771	--
Adjusted with official GED statistics, age 25-29	--	0.810	0.779	0.792	--

Notes: 1970-2000 statistics are based on census data, while 2010 statistics are based the pooled ACS years 2008-2010. For 2008 both statistics and the GED adjustment are based on ages 19-25, for 2009 ages 20-26 (panels A and B). Hawaii and Alaska are included. Heckman and LaFontaine's (2010) high school graduation rates in panel C refer to natives and immigrants in the United States for more than 10 years (for ages 20-24) or 15 years (for ages 25-29). In 1990-2000 they do not include completers of 12 years of education who did not receive a diploma; in 1970-1980, they presumably do, since the distinction cannot be made in the micro-data.

Appendix Table 2: Means of individual level variables – all natives and non-Hispanic white natives

	All natives			Non-Hispanic white natives		
	1950	2010	1950-2010	1950	2010	1950-2010
12 or more years education completed	0.576	0.919	0.878	0.619	0.940	0.895
Female	0.49	0.50	0.51	0.49	0.49	0.50
Age	24.0	22.9	23.8	24.0	23.0	23.8
	(2.0)	(2.2)	(2.1)	(2.0)	(2.2)	(2.1)
Black	0.098	0.148	0.128	0	0	0
Asian	0.002	0.025	0.010	0	0	0
Race missing	0.009	0.009	0.010	0	0	0
Mexican	0.013	0.090	0.048	0	0	0
Puerto Rican	0.000	0.015	0.009	0	0	0
Cuban	0.000	0.004	0.002	0	0	0
Other Hispanic	0.002	0.023	0.013	0	0	0
Hispanic missing	0	0.009	0.014	0	0	0
Year	1950	2010	1991.0	1950	2009.0	1990.0
Observations	47,225	568,840	3,978,143	40,948	397,184	3,095,866
12 years completed – males	0.545	0.906	0.870	0.585	0.932	0.889
Observations	22,621	284,710	1,953,509	19,718	198,450	1,531,932
12 years completed – females	0.608	0.933	0.887	0.654	0.949	0.902
Observations	24,604	284,130	2,024,634	21,230	198,734	1,563,934

Notes: Weighted with census weights adjusted so that the sum of weights for each year reflects the sample size of the census in that year. 2010 refers to the pooled 2008-2010 ACSs. The sample contains natives aged 21-27, except for 2008, when they are aged 19-25, and 2009, when they are aged 20-26. Standard deviations are in parentheses.

Appendix Table 3: Means of individual level variables – black and Hispanic natives

	Black natives			Hispanic natives		
	1950	2010	1950-2010	1950	2010	1950-2010
12 or more years education completed	0.250	0.865	0.810	0.273	0.869	0.813
Female	0.53	0.51	0.54	0.52	0.50	0.51
Age	24.1	22.8	23.7	23.9	22.8	23.5
	(2.0)	(2.2)	(2.1)	(1.9)	(2.2)	(2.1)
Black	1	1	1	0.016	0.023	0.020
Asian	0	0	0	--	--	--
Race missing	0	0	0	0.059	0.035	0.055
Mexican	0.001	0.005	0.003	0.805	0.682	0.670
Puerto Rican	0.000	0.008	0.004	0.031	0.114	0.121
Cuban	0.000	0.001	0.001	0.014	0.030	0.026
Other Hispanic	0.001	0.006	0.004	0.150	0.174	0.184
Hispanic missing	0	0.015	0.026	0	0	0
Year	1950	2009.0	1992.4	1950	2009.0	1996.8
Observations	4936	68,652	463,844	956	67,613	263,446
12 years completed – males	0.216	0.830	0.786	0.252	0.850	0.804
Observations	2236	34,556	214,262	435	33,795	128,890
12 years completed – females	0.281	0.899	0.831	0.292	0.889	0.823
Observations	2700	34,096	249,582	521	33,818	134,556

Notes: Weighted with census weights adjusted so that the sum of weights for each year reflects the sample size of the census in that year. 2010 refers to the pooled 2008-2010 ACSs. The sample contains natives age 21-27, except for 2008, when they are age 19-25, and 2009, when they are age 20-26. Standard deviations are in parentheses.

Appendix Table 4: Means of immigration-related state-level covariates

	All native weights			White weights	Black weights	Hispanic weights
	1940	2000	1940-2000	1940-2000	1940-2000	1940-2000
Share population 11-64	0.092	0.132	0.089	0.085	0.081	0.155
which is immigrant; t-10	(0.078)	(0.098)	(0.080)	(0.076)	(0.078)	(0.092)
Share pop 11-17 which is immigrant; t-10						
All	0.015	0.071	0.051	0.048	0.048	0.091
	(0.009)	(0.046)	(0.045)	(0.043)	(0.043)	(0.051)
Parents < 12 years education	0.011	0.019	0.016	0.015	0.013	0.034
	(0.007)	(0.018)	(0.019)	(0.018)	(0.017)	(0.022)
One parent >= 12 years education	0.003	0.044	0.029	0.028	0.030	0.047
	(0.002)	(0.026)	(0.024)	(0.023)	(0.025)	(0.026)
No parent in household	0.001	0.007	0.005	0.004	0.004	0.009
	(0.001)	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)
Share pop 18-64 which is immigrant; t-10						
Less than 12 years education	0.089	0.039	0.033	0.032	0.026	0.059
	(0.076)	(0.036)	(0.037)	(0.037)	(0.029)	(0.035)
12 years education	0.010	0.041	0.025	0.024	0.024	0.043
	(0.009)	(0.032)	(0.024)	(0.023)	(0.025)	(0.029)
More than 12 years education	0.006	0.057	0.035	0.032	0.034	0.058
	(0.005)	(0.040)	(0.032)	(0.030)	(0.033)	(0.039)
Share population 11-17 which is	0.114	0.068	0.046	0.044	0.038	0.084
2nd generation immigrant; t-10	(0.107)	(0.067)	(0.055)	(0.054)	(0.048)	(0.063)
Observations	49	49	343	343	320	330

Notes: The weights are the inverse of the squared standard errors on the state-year interaction coefficient in the individual regression for attainment of 12 years of education, by race/ethnicity. The individual regressions are weighted with census weights adjusted so that the sum of weights for each year reflects the sample size of the census in that year. State personal income per capita is measured in nominal dollars. Whites are non-Hispanic whites.

Appendix Table 5: Means of other state-level covariates

	All native weights			White weights	Black weights	Hispanic weights
	1940	2000	1940-2000	1940-2000	1940-2000	1940-2000
Unemployment rate ages 18-24	0.155	0.108	0.107	0.107	0.108	0.106
	(0.055)	(0.019)	(0.029)	(0.030)	(0.026)	(0.022)
Unemployment rate ages 25-54	0.069	0.039	0.046	0.047	0.046	0.044
	(0.019)	(0.007)	(0.013)	(0.014)	(0.012)	(0.010)
Share native population age 11-17	0.141	0.106	0.113	0.113	0.112	0.111
	(0.011)	(0.008)	(0.016)	(0.016)	(0.017)	(0.013)
Share employment in agriculture 1940	0.010	0.094	0.093	0.090	0.116	0.086
	(0.071)	(0.063)	(0.064)	(0.063)	(0.075)	(0.051)
Share native whites	0.554	0.529	0.534	0.532	0.574	0.488
with <12 years education 1940	(0.088)	(0.097)	(0.094)	(0.092)	(0.094)	(0.082)
State personal income per capita	561	30,014	16,744	16,252	17,148	20,622
	(202)	(4211)	(9882)	(9801)	(9939)	(9927)
Observations	49	49	343	343	320	330

Notes: The weights are the inverse of the squared standard errors on the state-year interaction coefficient in the individual regression for attainment of 12 years of education, by race/ethnicity. The individual regressions are weighted with census weights adjusted so that the sum of weights for each year reflects the sample size of the census in that year. State personal income per capita is measured in nominal dollars. Whites are non-Hispanic whites.

Appendix Table 6: 1940 shares of national-level immigrants from various origins, all ages and educations

Origin	Share
United Kingdom	0.050
Ireland	0.059
Italy	0.062
Germany	0.052
Poland	0.060
Russia	0.064
Other Europe	0.050
Mexico	0.043
Puerto Rico	0.096
Canada	0.038
Central America	0.052
South America	0.069
Other Caribbean	0.073
Cuba	0.047
China	0.058
India	0.047
Other Asia	0.045
Rest of world	0.054

Appendix Table 7: First stage of two-stage least squares (adapted instruments)

	(1)	(2)	(3)	(4)	(5)
	Share of immigrants in age group		Share of population aged 18-64 which is immigrant with		
	11-64	11-17	less than 12 years education	exactly 12 years education	more than 12 years education
Predicted share population 11-64 which is immigrant	0.27**	--	--	--	--
	(0.06)				
Predicted share population 11-17 which is immigrant	--	0.62**	-0.1	-0.04	0.05
		(0.26)	(0.14)	(0.05)	(0.09)
Predicted share population 18-64					
which is immigrant < 12 years education	--	-0.10	0.79**	0.04	0.09
		(0.14)	(0.12)	(0.05)	(0.08)
which is immigrant 12 years education	--	-1.20**	-0.85**	0.28**	-0.20
		(0.57)	(0.37)	(0.12)	(0.19)
which is immigrant > 12 years education	--	0.44	0.41**	0.10	0.42**
		(0.29)	(0.18)	(0.96)	(0.13)
F statistic for joint significance	21.5	2.5	24.7	17.8	16.3
Angrist-Pischke F statistic	--	6.6	66.9	14.7	15.1
Partial R^2	0.20	0.11	0.40	0.24	0.24
Observations	294	294	294	294	294

Notes: Estimation is by weighted least squares on 10-year differenced data, with weights $1/(1/h_t+1/h_{t+10})$, where h is the denominator of the dependent variable in each regression: population age 11-64 in column 1, population age 11-17 in column 2, population age 18-64 with less than 12 years education column 3, population age 18-64 with exactly 12 years education column 3, population age 18-64 with more than 12 years education column 5. All specifications include year dummies and the (differenced) non-immigrant covariates of Table 2 column 7. The excluded instruments (predicted shares) are based on historical patterns of settlement by age and education. The dependent variable and excluded instruments are based on data from 1940-2000 (i.e. from t-10), while other covariates are based on data from 1950-2010. Standard errors are clustered by state and reported in parentheses. This tables underlies the 2SLS 2 estimates.

Appendix Table 8: Results by gender for native-born non-Hispanic whites and all races and ethnicities

	Native non-Hispanic whites				Natives of all races and ethnicities			
	Fixed effects		10-year differences 2SLS		Fixed effects		10-year differences 2SLS	
	Women	Men	Women	Men	Women	Men	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share population 11-64	0.23**	0.16**	0.35*	0.22**	0.29**	0.28**	0.35**	0.32**
which is immigrant; t-10	(0.10)	(0.07)	(0.19)	(0.11)	(0.07)	(0.06)	(0.12)	(0.11)
Share population 11-17	0.06	-0.25**	-0.03	0.14	-0.42**	-0.46**	-0.25	-0.11
which is immigrant; t-10	(0.27)	(0.11)	(0.29)	(0.18)	(0.11)	(0.12)	(0.18)	(0.21)
Share population 18-64								
which is immigrant; t-10								
Less than 12 years education	0.43**	0.42**	0.30	0.33**	0.97**	0.83**	0.91**	0.71**
	(0.17)	(0.15)	(0.23)	(0.14)	(0.17)	(0.15)	(0.16)	(0.17)
12 years education	-0.37	-0.55**	0.75	0.97	-0.17	-0.56*	0.35	0.84
	(0.50)	(0.26)	(1.26)	(0.51)	(0.29)	(0.31)	(0.85)	(0.77)
More than 12 years edu	0.20	0.75**	0.18	-0.51	0.60**	0.96**	0.12	-0.28
	(0.39)	(0.28)	(1.09)	(0.52)	(0.30)	(0.31)	(0.72)	(0.74)
Other covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	343	343	294	294	343	343	294	294

Notes: The dependent variable is the share of native—born non-white Hispanics (columns 1-4) or all races and ethnicities (columns 4-8) age 21-27 of the gender specified who have completed 12 years of education, adjusted at the individual level for age and sex, and also for black, Asian, Mexican, Cuban, Puerto Rican and other Hispanic in columns 5-8. Estimation is by weighted least squares, with weights w the inverse of the squared standard errors on the state-year interaction coefficients in the gender-specific individual regression for columns 1, 2, 5 and 6 and $1/(1/w_t+1/w_{t+10})$ for columns 3, 4, 7 and 8. All specifications include year dummies; fixed effects specifications also include state dummies. Other covariates are those listed in Table 2. The dependent variable is based on 1950-2010 data, the independent variables on data from 1940-2000. The instruments in columns 3,4,7 and 8 are based on the 1940 distribution of immigrants from different regions (see text), and correspond to those in 2SLS 1 in earlier tables. Standard errors are clustered by state and reported in parentheses.