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DO IMMIGRANTS AFFECT THE EDUCATIONAL ATTAINMENT OF AMERICAN MINORITIES?

BY

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Abstract

The paper studies whether immigration affects the probability of high school graduation of American-born minorities. Since both the costs and the benefits of education are likely to rise due to immigration, the direction of the impact is ambiguous. The paper uses pooled 1980 and 1990 Census data to test for a link. State fixed effect estimates suggest a negative and significant impact of immigrants on the probability of completing high school for native-born blacks and Hispanics. The results are robust to use of metropolitan fixed effects, controlling for the pupil-teacher ratio in the state, and removing those who have recently moved. The results for blacks are also robust to removal of observations from California. However, the results for Hispanic natives depend crucially on the inclusion of the Californian subsample.

JEL Code: I2, J15, J61

1. Introduction

A large body of literature addresses the important question of how rapidly immigrants to the United States adapt to their new home, in terms of labor-market performance. Perhaps an equally important question is how the presence of immigrants affects the economic well being of American-born residents of the United States. This issue has received much less attention. To date, most research in this area has tested for an impact of immigrant flows on the wages of native-born Americans. Examples include Altonji and Card (1991), Bean, Lowell and Taylor (1988), Borjas (1990), Grossman (1982) and LaLonde and Topel (1991). The typical conclusion from these studies has been that the presence of a large number of immigrants tends to lower the wages of natives, but by only a very small percentage. In a review of the literature, Borjas (1994) estimates that the average native wage elasticity with respect to the number of immigrants is -0.01 to -0.02.

To the best of my knowledge, no paper has yet studied whether the presence of immigrants in the local area affects the educational attainment of American-born workers. This link, if it exists, could provide an important mechanism through which immigrants affect the economic well-being of natives. Unfortunately, economic theory provides ambiguous predictions about the direction of the effect of immigrants on natives' educational attainment. The reason is simple: immigrants are likely to increase both the costs and benefits of education to natives. The marginal cost of education for natives may rise due to competition between immigrant and American-born students for school resources; the marginal benefit of education for natives may rise if the arrival of relatively unskilled immigrants increases the returns to education.

The goal of the present paper is to test whether immigration has affected the probability that American-born minority students complete high school. The analysis will proceed using 1980 and 1990 Census data. [1]

I concentrate on estimating the impact of immigrants on educational attainment of two minority groups: native-born blacks and native-born Hispanics. There are three good reasons for doing so. First, families of higher income and socioeconomic status can "vote with their feet" if an influx of immigrants into the local area puts a strain on public services such as schooling. They can do this by moving to more affluent areas, or by enrolling their children in private schools. It follows that the main impact of immigrants on natives may be felt by American-born minorities, simply because minorities are less likely to have the financial resources to move to affluent areas or to place their children in private school. Second, *within* schools, minority students are more likely to be placed in classes with recent immigrants due to the grouping of students by initial achievement.[2] That is, since minority students are more likely to be placed in classrooms with lower average achievement, they are more likely to have immigrants in their classes than are students with higher levels of achievement. Third, disadvantaged immigrant students are eligible to participate in federally financed Chapter I programs. The goal of this spending

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[1] See Hoxby (1997) for a careful study which tests whether immigrants crowd minority students out of colleges.

[2] The use of tracking to group together students who are at similar levels of achievement is a widespread practice in American public schools. Betts and Shkolnik (1996), using a representative national panel of 6000 students from 1987 to 1992, find that approximately three quarters of the schools' principals reported that a formal tracking policy was used in math classes. Yet the range of mean academic achievement across classes was virtually identical in schools which reported using tracking and those which claimed that they

is to provide remedial education to disadvantaged children. In a study of school districts in Oakland, Houston, Boston and Washington, D.C., Fix and Zimmerman (1993) conclude that an influx of immigrants to each of these school districts in the 1980's did not crowd native-born students out of participation in Chapter I programs. But they do find evidence that the influx of immigrant schoolchildren during this time expanded the number of children receiving Chapter I services, which had the effect of reducing spending per pupil on remediation. This reduction in remedial spending should have been felt most strongly by minority students.[3] For all three of these reasons, it is plausible that the effects of immigrants on educational attainment of natives should be greatest on minorities.

Given the focus in this paper on minorities, I further focus on high-school completion, because of the large and growing gap in earnings between high school graduates and high school dropouts.[4] The distinction between those with and without a high school diploma is becoming one of the great divides of American society. This is especially true for minorities. In 1992, among whites, blacks and Hispanics aged 18 to 24, 12.2%, 16.3% and 33.9% had failed to graduate from high school respectively. (U.S. Bureau of the Census, 1994, p. 172)

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did not. This finding suggests that virtually all American schools track students, even if no formal tracking policy is in place.

[3] For instance, Fix and Zimmerman (p. 49, 1993) report that in 1989-90 29% of Chapter I enrollees were black and 23% were Hispanic. These proportions are far larger than the proportions of blacks and Hispanics in the overall population.

[4] Blackburn, Bloom and Freeman (Table 1, 1990) document that between 1979 and 1987 the ratio of annual earnings of high school dropouts to high school graduates among white male full-year workers aged 25 to 34 fell from 0.797 to just 0.743.

The next section discusses in more detail why economic theory suggests an ambiguous impact of immigration on the educational attainment of American-born minorities. Section 3 describes the data, and Section 4 presents the results.

2. The Impact of Immigrants on the Marginal Costs and Benefits of Education for Natives

As outlined above, the presence of a large number of immigrants in a city or state could induce native-born Americans to stay in school for either longer or shorter periods, depending on whether the marginal benefits of education or the marginal costs of education rise more in response.

Consider first the costs. Young immigrants to the United States are typically not perfectly acculturated to American public schools. In particular, they often lack the ability to speak English fluently. The presence of a significant number of students in a school who cannot speak English fluently, or who are otherwise not fully assimilated culturally, can reduce the effectiveness of educational spending for *all* students. If separate classes are established for students with "Limited English Proficiency" (LEP), this drains resources away from native-born students. If on the other hand immigrant students are placed in classes with native-born students, the large gap in language proficiency between the two groups of students will make it difficult for the teacher to teach as effectively as if he or she had a more homogeneous class. Similarly, a large body of research indicates that parents have a large influence on the quality of the schools which their children attend. It thus becomes possible that immigrant parents, to the extent that their educational attainment and attitudes about schooling differ from those of native-born parents, could influence the effectiveness of schooling for both their own children and native-born children in the school.[5]

The idea that the presence of immigrants in the classroom can diminish schools' effectiveness is by now widely accepted in the United States. Congress passed the Emergency Immigrant Education Act of 1984 in a bid to provide supplemental funding to school districts that had a large fraction of immigrant students.[6] Although the existence of this Act illustrates public recognition that additional funding is needed to cope with inflows of immigrant school-children, the funding disbursed under the law makes at best modest contributions to solving the problem. In the 1989-1990 school year, average disbursements under the Act were only $62 per eligible immigrant student. (General Accounting Office, 1991) This sum is slightly more than 1% of the current expenditures per pupil in average daily attendance in public schools in that year, which totaled $4939 (National Center for Education Statistics, 1991, p. 155) [7] [8]

If it is more expensive to teach a given set of skills and knowledge to immigrant students than to native-born students, and if, as appears to be the case, government funding has not risen to reflect fully these additional costs, it is likely that native-born students' rate of learning at school will fall. The cost to American students of acquiring a

[5] For a review of the impact of family background and the characteristics of the student body on the achievement of individual students, see Hanushek (1986, p. 1163).
[6] See U.S. Government Printing Office (1984) for text of the original bill.
[7] For recent Congressional testimony which confirms that funding from the Federal government has not adequately accounted for increases in the numbers of immigrant school children, see Morra (1994).
[8] A larger program directed toward immigrant schoolchildren is Title VII funding for bilingual education. In 1990-91 spending in this program amounted to $158.5 million, or about $70 per Limited English Proficiency student in the country. Even though the number of LEP students rose by 51.8% between 1985 and 1990, between 1980 and 1990 total Title VII spending on bilingual education fell by 5.1% in nominal

given set of skills thus rises. Once immigrant students arrive in a school, native-born students would either have to exert more independent effort or stay in school longer in order to acquire a given amount of human capital. The induced rise in the marginal costs of schooling suggests that American-born students will on average reduce the amount of education which they acquire. In other words, "educational crowding out" might reduce educational attainment of natives.

But it is also possible that the presence of immigrants in the area might increase the marginal benefits of education to natives, thus rendering the overall direction of the impact on natives' educational attainment ambiguous. To see why the marginal benefits of education to natives might rise after an inflow of immigrants, consider a simple model with two types of workers, skilled and unskilled. Borjas (1994) outlines such a model. The wages of skilled and unskilled workers are both likely to change after immigrants arrive in the country. If the proportion of immigrants who are unskilled is greater than the proportion of natives who are unskilled, then it is likely that the wage of skilled workers will rise and the wage of unskilled workers will fall. Since, as documented in Borjas (1995), about 37% of immigrants in both the 1980 and 1990 Census were high school dropouts, compared to just 23% of natives in 1980 and 15% of natives in 1990, it seems fair to argue that immigrants are "unskilled" relative to natives. Consequently, the presence of immigrants in the American labor market should increase the gap between the earnings of high school graduates and high school dropouts. In this way, immigrants can increase the marginal benefits of education for natives. Indeed, Borjas, Freeman and Katz (1992) estimate that the arrival of less skilled immigrants could have decreased the wages

terms and 47.8% after accounting for inflation. Most of the drop occurred in the first half of the 1980's.

of high school dropouts relative to high school graduates by about 3% between 1980 and 1988.

If both of these forces are at work: educational crowding out which increases the marginal cost of education to natives, and rising returns to education due to an influx of less skilled immigrants, the average educational attainment of natives could either rise or fall in response to immigration. Given the aforementioned set of papers, which tend to show no or small effects of immigrants on the wages of local natives, the crowding out effect may dominate in the real world, thus leading natives to obtain less education.

3. Data

The regressions use a pooled sample of young people taken from the 5% samples of the 1980 and 1990 versions of the Census of Population and Housing. The regression samples include all American-born blacks and Hispanics aged 19 to 25 at the time of the Census.[9] [10] In the regressions, the dependent variable is a dummy indicating whether the person has obtained a high school diploma or higher, on one hand, or less than a high school diploma on the other. In the case of people from the 1980 Census, if a person reported twelve years of education, but indicated that he or she had not completed the given year of education, I set the graduation variable to zero.

I choose young people aged 19-25 in order to ensure that the person is likely to have attended school in the state of current residence. In other words, given that I seek to estimate the immigrant-to-population ratio in the area where each person spent his or her

((Author's calculations based on Tables 3.2 and 3.3 of Fix and Zimmerman (pp. 22-23, 1993).)
[9] By "American-born" I mean those born in the United States or born abroad of American parents.

childhood and adolescence, using the person's current state of residence will produce more accurate estimates for younger workers than for older workers. Studying the group of people who attended grade school in the 1980's is especially interesting because of large increases in immigration during the 1980's.

Based on each person's place of residence, measures of the immigrant to population ratio were calculated. In the main regressions, these and other demographic variables were calculated for the state of residence. But additional regressions were run in which immigration rates were calculated for the metropolitan area of residence. In these latter regressions, people were assigned to one of 132 metropolitan areas, based on code generously provided by Susanna Loeb, Sarah Turner and David Jaeger. See Loeb, Turner and Jaeger (1996) for a summary of their method of creating consistent definitions of metropolitan areas across the 1980 and 1990 editions of the Census. In 1980, metropolitan area was derived from the person's county group; in 1990 the metropolitan area was derived from the PUMA code for each person.

Regressors include a dummy for whether the person at the time of the Census was living in a city, which was determined using the four-digit SMSA codes in the 1980 Census and the four-digit MSA/PMSA codes in the 1990 Census. Other regressors are described in the next section.

Unlike the 1980 Census, the 1990 Census is not self-weighting. That is, each person in the sample did not have an equal probability of being included in the Census. Therefore, all model estimates are weighted, using the person weights contained in the

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[10] Hispanics are defined as those who state that their ancestry is Spanish (in 1980) or Hispanic (in 1990) and that their race is white or 'other' (and not Asian, native American or black).

1990 Census data (which have a mean of roughly 20, given that the sample is a 5% sample), and using a constant weight of 20 in the 1980 sample.

4. Results

4.1 Basic Results

This section begins by presenting simple OLS results, and then proceeds to more complex difference-in-difference models which use variations over time in each state to identify the impact of immigrants on educational attainment of minorities.

The basic estimates are linear probability models for the probability that a given individual has obtained twelve years of schooling by the time of the Census. The simple OLS and the fixed effect models are given by the following equations, where i, s and t are subscripts for the individual, his or her state of residence and the year of the Census observation (1980 or 1990):

(1) $$GRAD_{ist} = c + \alpha CENSUS90_{ist} + \beta IMM_{ist} + X_{ist}\Gamma + \varepsilon_{ist}$$

and

(2) $$GRAD_{ist} = \sum_{j=1}^{51} STATE_{ist}\gamma_j + \alpha CENSUS90_{ist} + \beta IMM_{ist} + X_{ist}\Gamma + \varepsilon_{ist}$$

respectively.

The dependent variable is a dummy variable set to one if the person has finished at least twelve years of schooling. In the OLS regressions without state dummies, regressors include a constant, $CENSUS90_{ist}$, which is a dummy variable for whether the observation derives from the 1990 Census, IMM_{ist}, which is a measure of the proportion of the state population which consists of immigrants, and X_{ist} is a vector of other variables. This vector initially includes a dummy for whether the person was living in a city at the time of the Census, calculated as described in the previous section, a dummy for whether the person was female, and age at the time of the interview. Equation (2) is a fixed effect estimator which replaces the constant with a set of dummy variables for each state (and the District of Columbia).

A variety of measures of the proportion of immigrants in the local population were calculated. The first was the proportion of people aged 19 to 25 in the area who reported being born abroad of foreign parents. Note that this age range corresponds exactly to the members of minority groups whose educational attainment is being modeled. This measure should capture any "crowding out" effects experienced by these young blacks and Hispanics while they were in school, and at the same time will control for any changes in the returns to education for young workers due to immigration. But it makes sense to calculate a second immigration ratio over a broader age group for three reasons. First, the quality of grade-school education which was enjoyed by natives aged 19-25 could have been adversely affected if a large number of younger immigrant children had attended the school district at the same time, as it might have diverted resources from upper grade levels to the lower levels. (Conversely, when these minority students were in primary school, a large influx of immigrants of high school age could have diverted the resources of the school district away from primary schools.) Second, to the extent that younger and older workers of a given level of education are substitutes in the labor market, the returns to education for young workers should depend on the overall ratio of immigrants in the population, not just in the population aged 19-25. Third, to the extent that adult immigrants affect the quality of local schooling, through indirect neighborhood effects, it may be relevant to include older people in the calculation of immigrant ratios. Therefore a second measure of the extent of immigration was calculated: the proportion of the entire population aged 6 to 64 who were born abroad of foreign parents.

The OLS results without state fixed effects are presented for young blacks and Hispanics in Tables 1 and 2 respectively. Although, as will be discussed below, these models appear to be misspecified due the neglect of significantly different intercepts by state, these regressions provide a logical starting place.

Table 1 shows that black women were significantly more likely to have completed high school than black men, by about 6-7%. Similarly, blacks living in cities, and older blacks, were more likely to have graduated. In column #1, which includes the proportion of the state's population aged 19-25 who are immigrants, this variable enters positively and significantly. Column #2 instead uses the overall immigrant ratio among the population aged 6 through 64. The coefficient is still positive and significant, but both the coefficient and its t-statistic fall by over half.

A very common finding throughout the tables to be presented is that when the immigrant ratio for ages 19-25 is replaced by the ratio for the population aged 6-64, the coefficient falls. There are two ways to interpret this pattern. The first is that immigrants affect the educational attainment of natives of their own age only, so that using broader age groupings introduces measurement error. But this explanation does not seem wholly accurate, since measurement error should bias the coefficient toward zero. As will be shown below, in the typical case the use of the broader age grouping leads to a larger *negative* coefficient on the immigration variable, which is not consistent with the explanation of measurement error. A second, and more reasonable explanation, is that the educational attainment of young natives aged 19-25 is affected by immigrants of a much broader age group than just ages 19-25. Since the effect of immigrants becomes smaller or more negative when using this measure, it suggests that the presence of adult (or very young) immigrants may decrease educational attainment of native-born minorities, perhaps by inducing reallocation of the school district's resources between grade levels.

The two specifications presented so far are rather sparse. It seems quite likely that omitted variable bias could be biasing the coefficients. It seems particularly likely that if immigrants tend to settle in economically vibrant states, and if minorities tend to obtain relatively more education in such states, then the coefficients on immigrant ratio will be biased upward.

As a first attempt to solve this problem, consider other demographic variables that might influence the educational attainment of young blacks. Research has consistently shown that the socioeconomic status of a person's parents is a significant determinant of the person's educational attainment. See for instance Taubman (1989). It is not possible to obtain such information from the Census unless the person is still living in the same residence as his or her parents. But proxies can be obtained by measuring the average socioeconomic status of people in the local area who are in the age group likely to have offspring who would be in the regression sample (aged 19-25 and of the given race). Therefore, in some regressions, two measures were included to capture the characteristics of the older population of the given race in the person's area of residence. The first was the average total income of people in the local area of the given race, who were American-born, and who were aged 35 to 64 at the time of the Census. The second measure was the proportion of members of the given race, who were American-born and aged 35-64, who had at least twelve years of schooling. Separate measures were obtained for blacks and Hispanics.

Columns #3 and #4 replicate the first two models with these two proxies for the income and education of parents of young blacks added. Strikingly, the coefficients on the immigrant ratios reverse, and are significantly different from zero. This reversal suggests that omitted variable problems of the sort discussed above may have biased the coefficients of the immigration variable upward. As expected, if a larger proportion of the older black population in the state has at least twelve years of schooling, young blacks are more likely to have completed high school. However, a somewhat troubling result is that income per capita of this older generation is significantly *negatively* correlated with the probability of high school completion of young blacks.

One indicator of a potential reason for this puzzling result is shown in the bottom row of the table, which lists the probability value for the hypothesis that model (1) is correctly specified, with model (2), which adds state fixed effects, as the alternative. In all cases the null is overwhelmingly rejected. I will return to this issue below.

Table 2 presents the same four specifications using the sample of young Hispanics. As for young blacks, those who are female, older, or who live in a city are significantly more likely to have completed at least twelve years of schooling. Column #1 indicates a positive correlation between high school completion and the proportion of immigrants in the state's young population, but the link is not significant. In column #2, the coefficient reverses sign, but remains insignificant. In columns #3 and #4, which add measures of the socioeconomic status of older Hispanics in the state, the immigrant ratios become negative and highly significant, just as was found for blacks. Unlike for blacks, both proxies for parents' socioeconomic status are positively and significantly related to educational attainment of the younger generation. A notable difference between the results for Hispanic and black youth is that the estimated (signed) impact of immigrants on the probability of graduating is uniformly lower for Hispanics. In other words, there is greater evidence of an adverse impact on outcomes for Hispanics than for blacks. [11]

An immediate concern in the search for an impact of immigrants in the local area on the educational attainment of minorities is that a correlation between these two variables might not be causal, but instead might reflect correlation with an underlying trait of the local area. One plausible example is a situation in which immigrants tend to settle in thriving areas of the country. If the returns to education are higher in such areas, young American-born citizens are more likely to obtain their high school diplomas. In such a situation, a spurious positive correlation will emerge between the ratio of immigrants to total population and the probability that an American-born citizen will have completed high school. To the extent that any such unobserved traits of the local area are fixed over time, this spurious correlation can be purged from the model by including a fixed effect for each area.

Indeed, for both blacks and Hispanics, the hypothesis of a common intercept across states was uniformly rejected with a p-value < 0.00001. Accordingly, Tables 3 and

[11] The models in Tables 1 and 2 were re-estimated using a probit instead of a linear probability model. The signs and level of significance of the coefficients were extremely close to the OLS results presented in Tables 1 and 2.

4 show the results when dummies for state of residence are added, as in model (2) above. In this specification, which as before includes a time dummy, variations across states in educational attainment no longer contribute to the identification of the parameters; rather, this model uses changes between 1980 and 1990 within each state to identify the coefficients. The inclusion of a time dummy means that the identification is further limited to the parts of these within-state changes which vary from the average changes over time at the national level.

In these tables, the ratio of immigrants to total population is in all cases negatively and significantly related to the probability that the young person finishes at least 12 years of schooling. The models also seem better specified in that the troubling negative and significant coefficient on mean income per capita of the older black generation in Table 1 is reversed in Table 3, once one controls for unobserved differences across states.[12]

Recall that the coefficient on the immigration variables changes radically in the OLS specifications in Tables 1 and 2 once proxies for parental socioeconomic status are added. This appears to indicate that in the simpler models in Tables 1 and 2, the immigration ratio was biased upward by correlation with these and other measures of the socioeconomic level of state residents. But once the state fixed effect is added in Tables 3 and 4, the coefficients on the immigration ratios are much more robust to addition of the two socioeconomic variables. This suggests that the addition of the state fixed effects does a good job of controlling for socioeconomic determinants of the educational attainment of young blacks and Hispanics.

[12] In Table 4 the proxy for parental education becomes negative, but it is insignificant.

Note that in the fixed effect estimates, as was the case in the simpler models, the adverse impact of immigrants appears to be larger for native-born Hispanics than for native-born blacks. For instance, based on columns #3 and #4, an increase of 0.1 in the share of immigrants in the state population aged 19-25 is predicted to lower the probability of high school completion for blacks by 0.024, or 2.4%. For Hispanics, the predicted drop is much larger at 5.8%. Using the overall immigration ratio, the predicted drop in graduation probabilities after a 10% increase in the immigrant ratio is 4.7% for blacks and 8.8% for Hispanics.

4.2 Interpreting the Size of the Coefficients

What is the economic import of the coefficients on the immigration variables? Table 5 presents estimates of how the probability of graduation would change when the immigrant-to-population ratio changes. Given that the hypothesis of no fixed effects was strongly rejected, and that the two proxies for parental socioeconomic status are in general significant, the table uses the estimated impact of immigration from columns #3 and #4 of Tables 3 and 4. The first row shows the proportion of the regression sample which had graduated from high school. The next three rows in the table show the immigrant-to-population ratio for blacks and Hispanics in 1980 and 1990. These are based on the immigrant-to-population ratios that were calculated for each state using the population of each state aged between 19 and 25, or between 6 and 64, which are the ratios used in the previous regressions. Separate estimates of the "average" exposure of blacks and Hispanics to immigrants were then obtained by taking a weighted average of these state immigrant ratios, using the numbers of native born blacks and Hispanics in each state. The immigrant share in total population rose substantially during the 1980's, with both the initial level and the subsequent increase proving especially large for Hispanics. (As will become apparent from the later analysis, this in large part reflects a major increase in the immigrant-to-population ratio in California, a state in which slightly over one quarter of the Hispanic natives in the regression sample resided.)

The first "experiment" simply estimates the predicted drop in the probability of graduating from high school for blacks and Hispanics, based on the coefficient estimates and the observed changes in immigration. For blacks, the probability of high school graduation is predicted to have dropped by approximately 1% during the 1980's due to immigrant inflows. For American-born Hispanics, the estimates are much higher, on the order of a 3-4% drop. These estimates are higher for Hispanics than for blacks both due to higher estimated marginal effects and a greater increase in exposure to immigrants. These predicted drops in graduation rates are meaningful when compared to the actual sample averages of the graduation rate, which as shown in Row 1 of Table 5, are roughly 70% for both blacks and Hispanics.

The second experiment is outlined in the bottom box of the table. It shows that if the immigrant-to-population ratio rose from 0 to its actual level for each race in 1990, the predicted drop in the probability of high school graduation would be 1.8-3.4% for blacks, and an astonishing 8.5-11.5% for Hispanics. These estimates should of course be treated with some caution. Since the coefficients underlying this analysis are identified using actual changes between 1980 and 1990, the estimates from this second bolder experiment are probably less accurate than are the estimates based on the actual, more limited, changes in immigration, in the 1980's.

4.3 Robustness

The remaining tables probe the robustness of the results. Four sorts of re-specifications are carried out. First, I re-run the fixed effect models while conditioning on a measure of public school resources in each state. Second, I attempt to reduce measurement error caused by young people migrating from the area in which they attended school. Third, I re-run the difference-in-difference model, this time using the metropolitan area as the unit of observation for gathering geographic data such as the immigrant ratios. Finally, given that the immigrant-to-population ratio rose particularly strongly in California during the 1980's, I re-run the key models without observations from California.

Controlling for School Resources

Models 3 and 4 in Tables 3 and 4 control for unobserved but constant factors in each state through use of a state fixed effect. They also control for the socioeconomic status of the parents' generation in the state, by including measures of the older generation's level of education and income, both specific to race. It is possible that not only family background but also school spending influences the probability that a student will graduate from high school. Betts (1996) reviews the literature on the link between school resources and educational attainment of students. Evidence is mixed, with the strongest positive result being the finding in 5 of 7 studies that there exists a negative and statistically significant link between the pupil-teacher ratio and students' years of schooling. Class size is also one of the school inputs which has changed the most over the last 30 years: at the national level the average pupil-teacher ratio declined from 26.4 in 1960 to just 16.8 in 1990 (National Center for Education Statistics, 1991, p. 70).

To test whether the models suffer from omitted variable bias of this sort, the average pupil-teacher ratio in each state was calculated based on data published by the National Center for Education Statistics (various years) in the *Digest of Education Statistics* and the *Biennial Survey of Education in the United States* (Federal Security Agency, various years). For several years in the 1960's, data were linearly interpolated using surrounding observations. In the sample of natives aged 19-25 in 1980, the oldest would have attended Grades 1 through 12 between fall 1961 and spring 1973, while the youngest would have graduated from Grade 12 in spring 1979. So for this cohort, the simple average of the pupil-teacher ratio in each state was taken over the school years 1961-2 through 1978-9. Similarly, for the sample of people aged 19-25 in 1990, the pupil-teacher ratio was calculated as the mean for all school years between 1971-72 and 1988-89.[13]

I then repeated models #3 and #4 from Tables 3 and 4 for blacks and Hispanics, adding the mean pupil-teacher ratio to the models which already contain controls for education and income among the older generation of the given race, as well as a fixed effect for each state. The results appear in the top panel of Table 6. Several conclusions emerge from the table. First, the pupil-teacher ratio does not appear to be significantly related to the probability of graduation. Second, the coefficients and level of significance on the immigration variables and measures of socioeconomic status are highly similar to

[13] Pupil-teacher ratios were calculated for all states except Hawaii and Alaska, so that the regressions to be discussed below which use this variable have slightly fewer observations than those in the earlier part of the paper.

those reported in the earlier tables. The t-statistics and the coefficients on the immigration variables are slightly smaller in absolute size.

In the fixed effect specification the coefficients on the immigration variables and the pupil-teacher ratio are identified by changes in these variables between the two cohorts represented in the 1980 and 1990 Census. It is useful to examine the difference in the immigration ratios and the pupil-teacher ratio between the 1990 and 1980 Census periods. Figure 1 shows the change in the immigration-to-population ratio among the population aged 19-25 between 1980 and 1990 for each state, plotted against the change in the calculated pupil-teacher ratio for this age group. The figure reveals three facts. First, the states varied considerably in how they changed the pupil-teacher ratio over time, with some states decreasing class size by only 1 or 2 and many others decreasing class size by 4 or 5 pupils. This finding suggests that a lack of variation in the pupil-teacher ratio cannot explain why this measure of school resources is not significantly related to the probability that black or Hispanic students graduate from high school. Second, the figure reveals a positive correlation between changes in the immigration ratio and the pupil-teacher ratio. This may explain why the t-statistics and the coefficients on the immigration-to-population ratio decline somewhat when the pupil-teacher ratio is added, even though the latter does not enter significantly itself. (The correlation between the changes in the immigration ratio and the pupil-teacher ratio is 0.32). Third, the figure reveals that California was very much an outlier over the 1980's, with its immigrant-to-population ratio rising by 0.132, far above the simple mean for the remaining states, of just 0.015. This is an important issue which will be addressed below.[14]

The conclusion from this analysis is that the observed negative relation between the immigration-to-population ratio and educational attainment is robust to controls for the pupil-teacher ratio among both blacks and Hispanics.

Replication of Results Using Sub-samples of Non-Movers

In the state-level regressions measurement error in the state-level variables, including the immigration-to-population ratios, could be biasing the estimated effects of immigrants on natives' probability of finishing high school upward (toward zero). One way to control for this is to use data available in the Census indicating the location in which the person was living five years before the Census year in order to eliminate those who have moved. In theory, this should increase the absolute size of the coefficients on the variables measured at the state level, such as the immigration ratio, since these variables are calculated based on current location, not the actual location in which the person grew up. But removing movers is not without costs. First, the sample drops, in part because movers are removed from the sample, but also because in the 1980 Census questions about the person's location in 1975 were made available for only one half of the 5% sample. Thus the precision of the estimates will drop. Second, it is possible that movers are a non-randomly selected sample, so that regressions on this subsample could be biased in an unknown direction.

[14] When California was omitted, the correlation between the changes in the immigration ratio and the pupil-student remains positive, but is much smaller at 0.190. A similar analysis using the immigration ratio

I repeated models #3 and #4 from Tables 3 and 4 after dropping those who reported living in a different state five years before the Census year, as well as the 50% of 1980 observations for which location five years earlier was not available. Accordingly, in the regressions, the weights on those remaining in the 1980 Census sample were doubled to 40, while weights on observations from the 1990 Census were left unchanged, with an average value of approximately 20. This has the effect of representing both years equally in the regression sample.

The results appear in the bottom panel of Table 6 (regressions #5-8). About one third of the observations are lost due to the sample restriction. The table indicates that for Hispanics the results are very little changed when people who lived in a different state five years prior to the Census year are excluded, with the coefficients on the immigration variables becoming slightly more negative. In the regressions for blacks, the coefficients on the immigration variables remain negative and significant but become about one third larger in absolute value. The direction of these changes is consistent with the presence of some measurement error in the full sample due to internal migration of natives.

Replicating the Results using Metropolitan Areas

A problem with using state-level estimates of the immigrant-to-population ratio is that immigrants are not evenly distributed within the typical state. It would be reassuring if the flavor of the above estimates were retained in analyses based on less geographically aggregated areas. For this reason, the regressions in Tables 3 and 4 were repeated for the

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among the entire population aged 6-64 yields a correlation of 0.218 for all states, and 0.066 without California.

subsample of blacks and Hispanics who reported living in one of the 132 metropolitan areas for which I could obtain consistent coding between 1980 and 1990.

Using less geographically aggregated measures has both advantages and disadvantages. The advantage is that more precise estimates can result. The disadvantages are two-fold. First, it is likely that the metropolitan area in which members of the 19-25 cohort live at the time of the Census differs from the metropolitan area in which they attended school. This is of course an issue in the state-level analysis as well, but is necessarily of less importance in the state-level analysis since inter-state migration is less frequent than is inter-urban migration. Second, the matching of metropolitan areas across Census years is an inexact science, as borders of cities and groups of cities within metropolitan areas evolve over time. For both of these reasons, the metropolitan-level regressions could prove to be *less* precise than the state-level regressions, and possibly biased in an unknown regression.

Table 7 shows the metropolitan-level regressions for blacks and Hispanics. The regressions are identical in form to those in Tables 3 and 4 with three exceptions. First, the state dummies are replaced by dummies for metropolitan areas. Second, the dummy for whether the person lives in a city is dropped in this metropolitan-level sample. Third, the immigrant ratios, and in the replications of models #3 and #4 the proxies for the education and income of older blacks and Hispanics, were calculated at the level of the metropolitan area rather than at the level of the state. Of course, the sample size is lower since only those living in one of the 132 metropolitan areas are included.

For blacks, in all four specifications the link between the immigrant-to-population ratio and the probability of high school graduation remains negative and highly significant. The coefficients using the immigration ratio among young people are quite close to those reported in Table 3. The coefficients on the immigration ratio for all people aged 6-64 drop by about 60% but remain highly significant.

For Hispanics, the coefficients on the immigration variables remain negative and highly significant. The coefficients fall considerably in all cases though. One potential explanation for the lower coefficients is that in these metropolitan area regressions there is greater measurement error in assigning people to geographic locations than occurred in the state-level analysis, both due to changes in the metropolitan boundaries and changes in the place where people live, so that my assignment of immigration ratios based on current geographic location mismeasures the immigration ratio in the location(s) where the person attended school.

Overall, this metropolitan-area analysis bolsters the conclusions in the state-level analysis, suggesting that a higher proportion of immigrants in the local area over time is associated with declining probabilities of high school graduation among blacks and Hispanics. Recall that in the state-level analysis, the estimated effects on graduation probabilities were much higher among Hispanics than blacks; the metropolitan-level models yield the same pattern, but the crowding-out effects are now only slightly higher for native-born Hispanics than for native-born blacks.

Table 8 addresses the valid concern that migration between cities has introduced measurement error in the immigrant ratios and other demographic variables, biasing the coefficients towards zero. The table presents the results when models #3 and #4 from Table 7, that used metropolitan-level fixed effects, are repeated on a subsample of natives who reported that they lived in the same metropolitan area five years before the Census.

Again, half the 1980 sample drops out because the information about the respondent's location in 1975 is provided by the Bureau of the Census for only one half of the 5% sample in 1980. In the 1980 sample, the weight of those for whom this information is available is doubled accordingly. For blacks, both the level of significance and the size of the immigration coefficients are quite similar to the full sample regression in Table 7. Similarly, in the regressions for Hispanics, the results are little changed. In all cases, the coefficients on the immigration variables become somewhat larger (more negative), as would be expected if those who have moved results in measurement error.

The conclusion from these robustness tests is that the observed impact of immigrants on the probability that minorities graduate from high school is robust to the level of geographical detail, and to removal of people who have moved between metropolitan areas in the five years before the Census.

Are the Observed Effects a Purely Californian Phenomenon?

As Figure 1 revealed, the rise in the immigrant-to-population ratio among the age group 19-25 in California between 1980 and 1990 was 0.132, more than double that of any other state. It therefore becomes important to test whether the results presented above depend crucially upon the extraordinary rise in the proportion of immigrants in California's population. Table 9 replicates the state-level and metropolitan-level fixed effect models in Tables 3, 4 and 7 (models #3 and #4) once those living in California at the time of the Census are excluded. In the state-level sample, this restriction drops the sample of blacks by 7.2%, but the sample of Hispanics drops by fully 27.7%.

Consider first the results for blacks. In the models with state-level fixed effects, shown in the top panel of the table, both the coefficients and t-statistics are very similar in the regressions to those reported in Table 3, although in model #3 in Table 9 the immigration ratio becomes marginally significant. The bottom panel of Table 9 shows the results in the models with fixed effects for metropolitan areas. The coefficient on the immigration to population ratio in the age group 19-25 drops considerably both in size and in significance once Californians are excluded. But in specification #4, the coefficient on the overall immigrant ratio is highly similar to that in Table 7, and it remains highly significant. The evidence among the non-Californian population is clearly stronger when the overall immigration ratio is used, rather than the immigration ratio in the age group 19-25. Overall, the conclusion that immigrants crowd out blacks from secondary school seems to be a national phenomenon, and not restricted solely to California.

Although the results for blacks seem quite robust, the same cannot be said for the results for Hispanics. In none of the regressions in Table 9 is immigration negatively and significantly related to the probability that Hispanics graduated from high school. The change in the results in the models with metropolitan fixed effects once California residents are dropped is particularly dramatic. The overall immigrant ratio becomes insignificantly related to the probability of graduation for Hispanics, and the immigrant ratio in the age group 19-25 becomes positively and significantly related to the probability of graduation. This last result should probably be viewed with some caution, as it is the only specification in the paper which yields such a result.

What do these regressions suggest? For blacks, they indicate that the observed negative impact of immigration on the educational attainment of native blacks is a national phenomenon. Among Hispanics, the observed negative impact appears to be almost purely a Californian phenomenon. This finding does not necessarily mean that the observed negative impact of immigration on the probability that native Hispanics graduate is spurious. Indeed, it in not particularly surprising that the effect weakens so much once Hispanics from California are removed from the regression sample: over one quarter of all young native Hispanics in the sample live in California.

5. Conclusion

Using pooled Census data from 1980 and 1990, combined with state-level estimates of the share of immigrants in the local population, the paper uses a difference-in-differences strategy to identify the effect of immigration on the educational attainment of American-born minorities. Evidence emerges that there is a negative link between immigration and the probability of high school graduation for both blacks and Hispanics. The effects are meaningful. Based on the leading specifications, the rise in immigrants' share in the population observed in the 1980's is predicted to have decreased the probability that blacks graduate from high school by roughly 1%; for American-born Hispanics, the predicted drop is closer to 3.5-4%. This compares with graduation rates of 73.7% and 69.5% among blacks and Hispanics in the regression sample. These results are similar to findings by Hoxby (1997) who finds strong evidence that immigrants crowd native-born minorities out of American universities.

Economic theory is ambiguous as to whether immigration should decrease the educational attainment of natives, since both the marginal benefits and the marginal costs of education are likely to rise when less skilled immigrants enter the country. The state-level evidence presented here supports the "educational crowding out" hypothesis, whereby an influx of immigrants reduces the effectiveness of public education for minorities who attend the same schools, discouraging them from completing school. This effect appears to outweigh any increase in high school graduation which theory suggests could result among natives if immigration serves to boost the returns to education. This is not particularly surprising given that earlier research suggests that immigration has only small effects on the wages of natives.

The conclusions of the state-level analysis are largely supported when subjected to a number of tests for robustness. The results are virtually unchanged when controls for the pupil-teacher ratio in each state are added. I repeated the analysis using the metropolitan area as the unit of observation for the immigrant-to-population ratio, and again obtained highly similar results. I also re-analyzed the models on a subsample which excluded people who reported moving between states (or metropolitan areas) in the five years before the Census year. This was done in an attempt to reduce measurement error in the variables designed to characterize the immigrant ratio in the area in which each individual grew up. The immigration variables remained negative and highly significant. Finally, I re-estimated the models with a sample which excluded people residing in California, on the grounds that the immigrant-to-population ratio had grown over twice as fast in the 1980's in California as in any other state. The results for blacks weakened somewhat but continued to indicate the existence of a crowding-out effect. In contrast, the negative link between immigration and the probability of finishing high school among Hispanics appears to derive solely from trends in California. Given that over one quarter of young native-born Hispanics in the sample lived in California, the finding of a negative impact of immigration on the educational achievement of Hispanics within California is still important from a policy perspective.

The results have important implications for public policy. If schools have become less effective as a result of immigrant inflows during the 1980's, it raises the question of whether federal aid for school districts with large numbers of immigrants, mandated under the Emergency Immigrant Education Act of 1984 and under Title VII spending on bilingual education, is sufficient to meet the rising need. Recent trends in immigration also raise the question of whether native-born children who are disadvantaged have seen a dilution in Chapter I spending, as suggested by the work of Fix and Zimmerman (1993). This could provide a direct mechanism for the "crowding-out" phenomenon documented above.

Overall, the results of this paper are strongly suggestive of the hypothesis that immigrants "crowd out" investments in public education by American-born minorities. But the results are not definitive. The state-level analysis identifies the effect of immigrants using, in effect, 51 observations by state (and the District of Columbia) on changes in the ratio of immigrants to population between 1980 and 1990. The metropolitan-level analysis involves far less geographical aggregation, but again identification of the effect of immigrants derives from changes in the immigrant to population ratio in 132 metropolitan areas. In spite of my attempt to control for changes in the demographic traits of each area, and for changes in the resources devoted to schools, it is certainly a possibility that the strong "crowding out" effect detected in this analysis is a proxy for other unmeasured changes within states and cities during the 1980's.

Clearly, further research, possibly conducted at the level of the actual school attended, would be advisable to confirm the finding of a crowding-out effect. Of greatest importance to this task, but arguably also of greatest difficulty, is research to understand how resources are reallocated within schools and school districts when schools experience large inflows of immigrant students.

Figure 1
Changes by State in the Immigration-to-Population Ratio and the Pupil-Teacher Ratio, 1980-90.

Note: The immigration ratio is calculated among people aged 19-25 in each state. The pupil-teacher ratio is calculated as an average for those aged 19-25 in the given Census year during the time they were attending Grades 1-12.

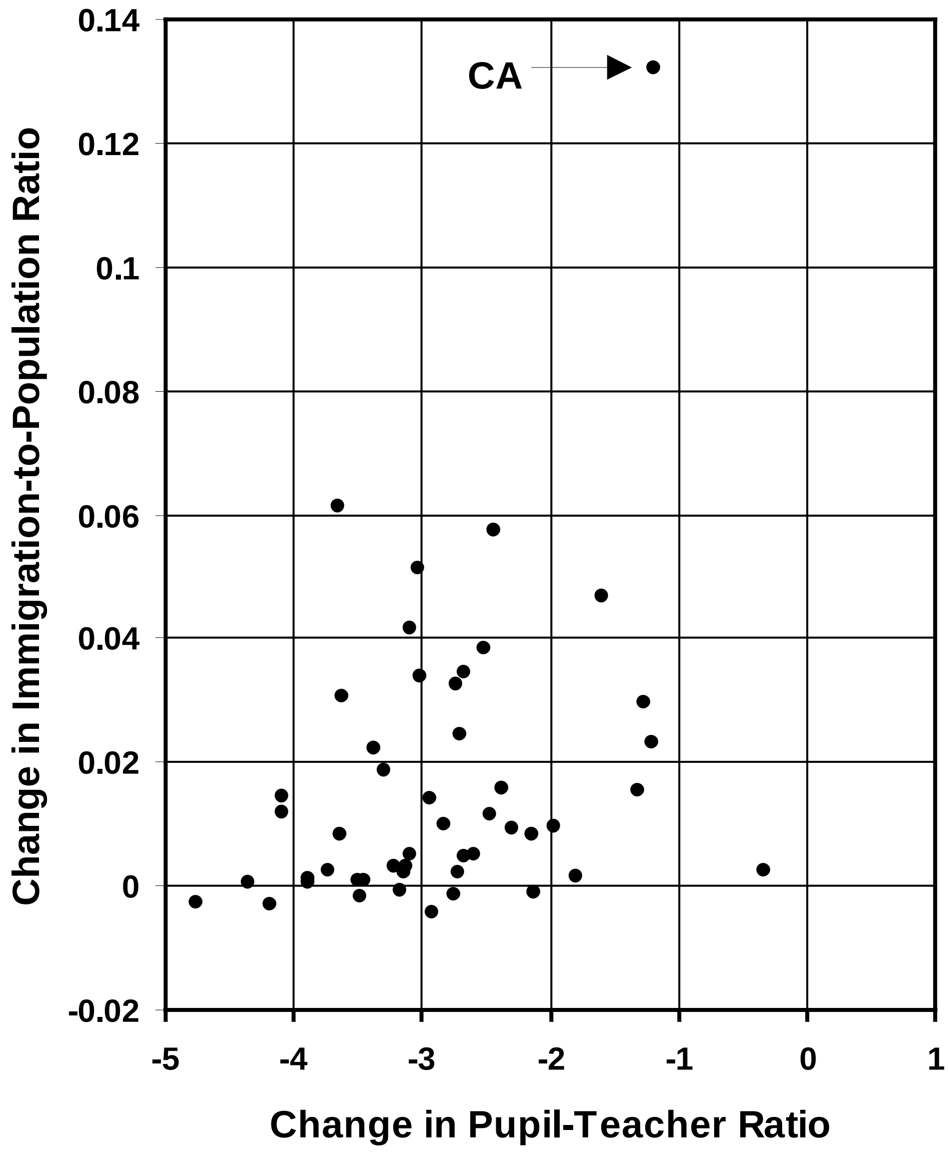

Table 1
Linear Probability Models of the Probability of Attaining at Least Twelve Years of Schooling for Native-Born Blacks Aged 19-25

Note: T-statistics appear in parentheses.

Variable	#1	#2	#3	#4
Constant	0.4643	0.4646	0.3825	0.3697
	(50.20)	(50.22)	(37.70)	(36.40)
Female	0.0663	0.0661	0.0672	0.0672
	(39.82)	(39.69)	(40.39)	(40.38)
Age	0.0093	0.0093	0.0094	0.0094
	(22.73)	(22.80)	(22.91)	(22.95)
Live in City	0.0423	0.0446	0.0319	0.0327
	(17.25)	(18.07)	(12.49)	(12.78)
90 Census	-0.0199	-0.0177	-0.0669	-0.0701
	(-11.64)	(-10.43)	(-26.05)	(-27.22)
Proportion Age 35-64 with HS Dip.			0.3953	0.4114
			(23.86)	(25.10)
Mean income/1000 (35-64)			-0.0102	-0.0090
			(-13.18)	(-11.50)
Immig./Popul. (19-25)	0.1139		-0.0473	
	(8.07)		(-2.77)	
Immig./Popul. (6-64)		0.0480		-0.1604
		(3.08)		(-8.30)
R-square	0.0096	0.0094	0.0118	0.0120
Adj R-square	0.0096	0.0094	0.0118	0.0120
Number of Obs.	278282	278282	278282	278282
P-value: F-test for Exclusion of State Dummies	<0.00001	<0.00001	<0.00001	<0.00001

Table 2
Linear Probability Models of the Probability of Attaining at Least Twelve Years of Schooling for Native-Born Hispanics Aged 19-25

Note: T-statistics appear in parentheses.

Variable	#1	#2	#3	#4
Constant	0.4661	0.4692	0.2049	0.2099
	(31.64)	(31.82)	(12.66)	(13.03)
Female	0.0282	0.0282	0.0307	0.0308
	(10.85)	(10.86)	(11.91)	(11.94)
Age	0.0083	0.0083	0.0084	0.0084
	(12.89)	(12.89)	(13.16)	(13.19)
Live in City	0.0252	0.0280	0.0443	0.0469
	(5.69)	(6.29)	(9.92)	(10.46)
90 Census	0.0140	0.0174	-0.0780	-0.0772
	(4.96)	(6.29)	(-20.24)	(-20.03)
Proportion Age 35-64 with HS Dip.			0.5258	0.5090
			(23.78)	(22.89)
Mean income/1000 (35-64)			0.0046	0.0052
			(4.05)	(4.62)
Immig./Popul. (19-25)	0.0292		-0.2359	
	(1.69)		(-11.90)	
Immig./Popul. (6-64)		-0.0351		-0.3146
		(-1.66)		(-13.14)
R-square	0.0029	0.0029	0.0153	0.0155
Adj R-square	0.0029	0.0029	0.0152	0.0155
Number of Obs.	125664	125664	125664	125664
P-value: F-test for Exclusion of State Dummies	<0.00001	<0.00001	<0.00001	<0.00001

Table 3
Linear Probability Models of the Probability of Attaining at Least Twelve Years of Schooling for Native-Born Blacks Aged 19-25, with State Fixed Effects

Note: T-statistics appear in parentheses.

Variable	#1	#2	#3	#4
Female	0.0691	0.0691	0.0691	0.0691
	(41.62)	(41.61)	(41.61)	(41.61)
Age	0.0093	0.0093	0.0093	0.0093
	(22.87)	(22.88)	(22.85)	(22.86)
Live in City	0.0535	0.0536	0.0530	0.0530
	(19.95)	(19.97)	(19.76)	(19.77)
90 Census	-0.0083	-0.0072	-0.0696	-0.0658
	(-3.95)	(-3.27)	(-6.32)	(-6.09)
Proportion Age 35-64 with HS Dip.			0.2473	0.2196
			(4.45)	(4.00)
Mean income/1000 (35-64)			0.0064	0.0085
			(3.36)	(4.17)
Immig./Popul. (19-25)	-0.3026		-0.2449	
	(-6.34)		(-3.75)	
Immig./Popul. (6-64)		-0.4561		-0.4718
		(-6.57)		(-4.78)
R-square	0.0102	0.0102	0.0103	0.0103
Adj R-square	0.0101	0.0102	0.0103	0.0103
Number of Obs.	278282	278282	278282	278282

Table 4
Linear Probability Models of the Probability of Attaining at Least Twelve Years of Schooling for Native-Born Hispanics Aged 19-25, with State Fixed Effects

Note: T-statistics appear in parentheses.

Variable	#1	#2	#3	#4
Female	0.0317	0.0317	0.0318	0.0317
	(12.30)	(12.29)	(12.33)	(12.32)
Age	0.0084	0.0084	0.0084	0.0084
	(13.21)	(13.21)	(13.21)	(13.21)
Live in City	0.0571	0.0572	0.0569	0.0569
	(12.21)	(12.22)	(12.17)	(12.16)
90 Census	0.0426	0.0424	0.0330	0.0384
	(9.73)	(9.12)	(2.40)	(2.75)
Proportion Age 35-64 with HS Dip.			-0.0509	-0.0742
			(-0.84)	(-1.22)
Mean income/1000 (35-64)			0.0129	0.0135
			(4.67)	(4.79)
Immig./Popul. (19-25)	-0.4263		-0.5758	
	(-7.51)		(-8.93)	
Immig./Popul. (6-64)		-0.6045		-0.8831
		(-6.82)		(-8.46)
R-square	0.0046	0.0045	0.0048	0.0047
Adj R-square	0.0045	0.0045	0.0047	0.0046
Number of Obs.	125664	125664	125664	125664

Table 5
Estimates of the Predicted Drop in the Probability of Graduation Given a Rise in the Ratio of Immigrants to the Overall Population

Estimates are based on the coefficients in columns #3 and #4 in Tables 3 and 4. Mean immigration ratios are calculated using weighted means for blacks and Hispanics aged 19-25 in 1980 and 1990.

ROW	**RACE:** **IMMIGRATION MEASURE:**	**BLACK** **Age 19-25**	**BLACK** **Age 6-64**	**HISPAN-IC** **Age 19-25**	**HISPAN-IC** **Age 6-64**
1	Proportion of Regression Sample of Given Race Which Graduated:	0.737	0.737	0.695	0.695
2	Mean Immigration Ratio, 1980	0.045774	0.04931	0.085048	0.085705
3	Mean Immigration Ratio, 1990	0.075413	0.072448	0.148837	0.130947
4	Change, 1980-1990	0.029639	0.023138	0.063789	0.045242
	Experiment #1: Effect of 1980-90 Increase in Imm. Ratio:				
5	Coefficient on Immigration	-0.2449	-0.4718	-0.5758	-0.8831
	X Change, 1980-1990 (Row 4)				
6	= Predicted Change in Prob. Graduate	-0.00726	-0.01092	-0.03673	-0.03995
	Experiment #2: Effect of Increasing Imm. Ratio from 0 to 1990 Level: Row 3 X Row 5				
7	= Predicted Change in Prob. Graduate	-0.01847	-0.03418	-0.0857	-0.11564

Table 6
Robustness Tests of the State Fixed Effect Models

Note: T-statistics appear in parentheses. The regressions are identical to models #3 and #4 in Tables 3 and 4 except as noted in the rows indicating added regressors and changes in sample.

Added Regressors:	**Pupil-Teacher Ratio**			
Sample:	**Dropped Observations from Hawaii and Alaska**			
	Blacks	**Blacks**	**Hispanics**	**Hispanics**
Variable	**#1**	**#2**	**#3**	**#4**
Pupil-Teacher Ratio	-0.0023	-0.0009	-0.0001	-0.0030
	(-0.85)	(-0.33)	(-0.03)	(-0.72)
Immig./Popul.(19-25)	-0.2039		-0.5706	
	(-2.71)		(-6.47)	
Immig./Popul.(6-64)		-0.4440		-0.8111
		(-3.94)		(-5.84)
R-square	0.0103	0.0104	0.0048	0.0047
Adj R-square	0.0103	0.0103	0.0047	0.0046
Number of Obs	277464	277464	125043	125043
Added Regressors:	**None**			
Sample:	**Subsample Reporting Lived in Same State 5 Years before Census Year**			
	Blacks	**Blacks**	**Hispanics**	**Hispanics**
Variable	**#5**	**#6**	**#7**	**#8**
Immig./Popul.(19-25)	-0.3353		-0.5949	
	(-3.82)		(-6.96)	
Immig./Popul.(6-64)		-0.6339		-0.9343
		(-4.78)		(-6.65)
R-square	0.0122	0.0123	0.0043	0.0042
Adj R-square	0.0122	0.0123	0.0042	0.0041
Number of Obs	175286	175286	83093	83093

Table 7
Estimates of the Probability of Attaining at Least Twelve Years of Schooling for Native-Born Blacks and Hispanics Aged 19-25, with Metropolitan Area Fixed Effects

Note: T-statistics appear in parentheses. The four specifications for each group are identical to the four specifications given in Tables 3 and 4, except that the regressors describing the level of education and income of those aged 35-64 in the given group, which appear in the third and fourth regressions, are now calculated at the metropolitan level rather than the state level. Also, the dummy variable for people who live in a city is dropped.

Group:	**Blacks**			
Variable	**#1**	**#2**	**#3**	**#4**
Immig./Popul. (19-25)	-0.2443		-0.2555	
	(-5.53)		(-4.47)	
Immig./Popul. (6-64)		-0.1767		-0.2068
		(-5.93)		(-5.58)
R-square	0.0102	0.0102	0.0103	0.0103
Adj R-square	0.0102	0.0102	0.0103	0.0103
Number of Obs.	211039	211039	211038	211038
Group:	**Hispanics**			
Variable	**#1**	**#2**	**#3**	**#4**
Immig./Popul. (19-25)	-0.2647		-0.4147	
	(-5.12)		(-6.98)	
Immig./Popul. (6-64)		-0.2075		-0.3470
		(-5.64)		(-7.95)
R-square	0.0038	0.0039	0.0041	0.0042
Adj R-square	0.0038	0.0038	0.0040	0.0042
Number of Obs.	102749	102749	102749	102749

Table 8
Replication of Metropolitan Fixed Effect Models in Table 7, Models #3 and #4, with Subsample Reporting They Lived in Same Metropolitan Area Five Years before Census Year

Note: T-statistics appear in parentheses. This table replicates models #3 and #4 from Table 7. Column numbers refer to the model from Table 7 being replicated.

	Blacks	**Blacks**	**Hispanics**	**Hispanics**
Variable	**#3**	**#4**	**#3**	**#4**
Immig./Popul.(19-25)	-0.2590		-0.4274	
	(-2.86)		(-4.73)	
Immig./Popul.(6-64)		-0.2410		-0.3758
		(-4.16)		(-5.55)
R-square	0.0113	0.0113	0.0038	0.0040
Adj R-square	0.0112	0.0113	0.0037	0.0039
Number of Obs	101918	101918	47123	47123

Table 9
Replication of State and Metropolitan Fixed Effect Models in Tables 3, 4 and 7 with Subsample Living Outside California

Note: T-statistics appear in parentheses. This table replicates models #3 and #4 from Tables 3, 4 and 7. Column numbers refer to the model from Table 3, 4 or 7 being replicated.

Type of Fixed Effect Added:	**State**			
	Blacks	**Blacks**	**Hispanics**	**Hispanics**
Variable	**#3**	**#4**	**#3**	**#4**
Immig./Popul.(19-25)	-0.2052		0.0811	
	(-1.83)		(0.45)	
Immig./Popul.(6-64)		-0.4915		0.1176
		(-3.44)		(0.51)
R-square	0.0105	0.0105	0.0056	0.0056
Adj R-square	0.0105	0.0105	0.0056	0.0056
Number of Obs	258212	258212	90857	90859
Type of Fixed Effect Added:	**Metropolitan**			
	Blacks	**Blacks**	**Hispanics**	**Hispanics**
Variable	**#3**	**#4**	**#3**	**#4**
Immig./Popul.(19-25)	-0.1374		0.5445	
	(-1.62)		(3.85)	
Immig./Popul.(6-64)		-0.1830		-0.0338
		(-3.68)		(-0.44)
R-square	0.0106	0.0106	0.0053	0.0051
Adj R-square	0.0105	0.0106	0.0052	0.0050
Number of Obs	191343	191343	69284	69284

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