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How the Earnings Growth of U.S. Immigrants Was Underestimated*

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ABSTRACT

Two radically different descriptions of immigrant earnings trajectories in the U.S. have emerged. One asserts that immigrant men following the 1965 Immigration and Nationality Act have low initial earnings and high earnings growth. Another asserts that post-1965 immigrants have low initial earnings and low earnings growth. We describe the methodological issues that create this divide and show that low earnings growth becomes high earnings growth when immigrants are followed from their initial years in the U.S., earnings growth is allowed to vary with entry earnings, and—when following cohorts instead of individuals—sample restrictions commonly used by labor economists are avoided.

JEL Classification: J1, J2, J3, C1

Keywords: Sample restrictions, immigrant earning growth, human capital investment, the U.S. census

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I. Introduction and Background

The 1965 Immigration and Nationality Act eliminated country-specific quotas, which discriminated against particular groups, and introduced a system of admission categories favoring immigrants with family members in the United States. It also authorized employment-based immigration, allocating twenty percent of the numerically restricted visas to applicants based on their occupational skills.¹

Among economists, a dominant view is that the “quality” of U.S. immigrants fell following the 1965 Act. Supporting this view are the low initial earnings of post-1965 immigrant men coupled with their apparently low earnings growth relative to U.S. natives of similar age and education. This perspective is argued most prominently and forcefully by Georges Borjas in almost all of his writings, including journal articles,² textbooks,³ and popular books.⁴

In “Assimilation, Changes in Cohort Quality, and the Earnings of Immigrants,” Borjas (1985) uses public use microdata samples (PUMS) from the 1970 and 1980 censuses to show much slower earnings growth for immigrants relative to U.S. natives over the 1970 to 1980 period than measured in cross-sectional analyses. In “Assimilation and Changes in Cohort Quality Revisited: What Happened to Immigrant Earnings in the 1980s?,” Borjas (1995) uses the 1970-1990 Census PUMS to show that the decline in immigrant wages relative to U.S. natives continued across consecutive immigrant waves into the 1980s, with little or no compensating earnings growth. In “The Slowdown in the Economic Assimilation of Immigrants: Aging and

¹ The employment-based classification embraced two components: workers, skilled and unskilled, in occupations where labor is deemed scarce, and professionals, scientists, and artists of exceptional ability.

² Examples include, Borjas (1985, 1994, 1995, 2015) and Borjas and Tienda (1987).

³ Examples include Borjas (2014, 2016, 2020).

⁴ Examples include Borjas (1999, 2000).

Cohort Effects Revisited Again,” Borjas (2015) analyzes 1970-2000 Census PUMS and pooled 2009-2011 American Community Survey (ACS) data. Relative to U.S. natives, he finds low entry wages and lower wage growth for recent immigrant cohorts than for earlier cohorts.

In sharp contrast, analysts who follow individual immigrants from their initial years in the U.S., and do not constrain the relationship between entry earnings and earnings growth, find high earnings growth relative to U.S. natives for post-1965 immigrant men.⁵ Moreover, using the same census data as Borjas, analysts who follow immigrant entry cohorts, let earnings growth vary with entry earnings, and do not exclude zero earners, students, or the self-employed find high earnings growth for post-1965 immigrants.⁶

This paper illuminates the methodological issues that create such starkly different pictures of post-1965 immigrant economic assimilation. We show that methods often used by economists understate the earnings growth of immigrants who begin their U.S. journeys with low initial earnings relative to U.S. natives of similar age and education. These methods include:

—following immigrants in all possible year-of-entry cohorts versus following only those who can be tracked from their initial years in the U.S.,

—combining all possible year-of-entry immigrant cohorts to estimate earnings growth, versus separately following each entry cohort,

—constraining the relationship between immigrant entry earnings and earnings growth, versus letting earnings growth vary with entry earnings,

⁵ Refer to Duleep and Regets, (1997), Duleep and Dowhan (2002), Hall and Farkas (2008), and Villarreal and Tamborini (2018). Analyses of occupational mobility with longitudinal data further suggest economic assimilation for post-1965 U.S. immigrants (e.g., Akresh 2006, 2008; Chiswick 1978b), and for Australian immigrants (Chiswick, Lee, and Miller 2005).

⁶ Refer to Duleep and Regets (1994, 2002), Duleep, Liu, and Regets (2018), and Duleep et al. (2021).

—controlling for education using the schooling levels of immigrants years after their U.S. entry,

— and, in studies that do not follow the same individuals, excluding students, the self-employed, and zero earners.

Part II shows how constraining the relationship between entry earnings and earnings growth influences estimates of immigrant earnings growth. Part III describes the effect on earnings-growth estimates of using current levels of schooling to control for education. Part IV reveals how commonly used sample restrictions distort earnings growth estimates of immigrants, relative to natives, when following cohorts across censuses (or other cross-sectional datasets). Part V broaches how to measure immigrant skill transferability and whether earnings growth represents human capital investment. The paper ends with Part VI summarizing our results and noting societal ramifications of high immigrant earnings growth.

II. The Importance of the Inverse Relationship between Entry Earnings and Earnings Growth

The first labor-economics studies, initiated by Chiswick (1978a, 1979), used a single year of census data to measure immigrant earnings growth. Adjusting for demographic and human capital characteristics, the difference between the initial earnings of recent immigrants and the earnings of earlier immigrants measures earnings growth. The cross-sectional approach suggests that immigrant men had low initial earnings but high earnings growth, substantially exceeding the earnings growth of U.S. natives.

This optimistic picture was shattered when Borjas (1985, 1987, 1992a, 1992b, 1994), using multiple censuses, measured a steep decline in the entry earnings of immigrant men that persists controlling for immigrant-native differences in schooling and age. The decline is most

apparent since 1965 when U.S. immigration policy shifted from a national-origins system, favoring West European immigration, to a family-based policy that altered the source-country composition of U.S. immigration.

Borjas found that immigrants who entered the U.S. in the decades immediately preceding the 1965 Act did not start their U.S. journeys with lower earnings than natives of similar age and education. Nor did they experience higher earnings growth than natives. Evidently, the cross-sectional estimates of high earnings growth stemmed from pairing the low entry earnings of more recent immigrants with the unvaryingly high earnings of earlier immigrants.

The dramatic decline in the adjusted entry earnings of immigrant men inspired a new methodology for measuring immigrant earnings growth. Now standard practice, the fixed-cohort-effect method (Borjas, 1985) is in labor economics textbooks (e.g., Benjamin et al., 2017, Bodvarsson and Van den Berg, 2013, Borjas, 1999, 2016, 2020) and used by most economists with various types of data, including longitudinal data on individuals.

As in the cross-sectional approach, the fixed-cohort-effect method estimates immigrant earnings growth in an earnings regression by statistically measuring the relationship between years since migration and immigrant earnings, controlling for age and years of schooling. It adds to the equation categorical (zero-one) variables for each year-of-entry cohort thereby allowing the estimated relationship between years since migration and earnings to begin at different levels of initial earnings, while implicitly assuming that earnings growth does not change with changing levels of initial earnings. The fixed-cohort-effect method thus abandons one assumption of the cross-sectional method—inter-cohort constancy in immigrants’ adjusted entry earnings—while retaining another—inter-cohort constancy in earnings growth.⁷

⁷ Borjas introduced the fixed cohort-effect methodology to address cross-sectional bias, as first articulated and analyzed in Chiswick (1980). In what appears to be the first use of longitudinal individual data to study immigrant

Had Borjas' empirical methodology followed the human-capital-investment perspective introduced by Chiswick (1978a, 1979), the discovery of a decline in immigrant initial earnings might have inspired a methodology permitting both entry earnings and earnings growth to vary. Instead, Borjas (1987, 1992a–c, 1994) proposed that the decline in adjusted entry earnings stems from a decline in immigrant ability.⁸ Conceivably, changes in immigrant ability could affect entry earnings without affecting earnings growth.

In contrast, a human-capital-investment perspective predicts that (controlling for initial levels of human capital) decreases in initial earnings *systematically* accompany increases in earnings growth, and vice versa.⁹ Empirical evidence supports this prediction. Following nearly a hundred country-of-origin cohorts, first with the 1960-1980 censuses, and then with the 1970-1990 censuses, Duleep and Regets (1994, 2002) document a strong inverse relationship between entry earnings and earnings growth, controlling for years of schooling and age. This finding, which resonates with other research that predicts or finds an inverse relationship,¹⁰ suggests specific strategies for measuring immigrant earnings growth.

earnings, Chiswick measured earnings growth with the National Longitudinal Survey of Adult Males, following the earnings of immigrants who had entered the U.S. in various years. He then compared this earnings growth with a cross-sectional estimation on the same data. The two estimates were similar; Chiswick concluded that cross-sectional bias was not a problem. What Chiswick did not consider was that immigrants' initial earnings, controlling for education and experience, had declined over time. If earnings growth increases as entry earnings decrease then earnings growth estimates from cross-sectional and longitudinal data will be similar.

⁸ Borjas (1987) suggests that a post-1965 increase in the income inequality of the countries contributing to U.S. immigration fueled a decline in immigrant ability. It is theoretically ambiguous whether lower ability leads to lower initial earnings: *ceteris paribus*, high ability individuals should invest in more human capital than low ability individuals thereby lowering the former group's initial earnings. Under any human capital model, however, a decline in labor-market ability would not increase earnings growth. According to the income-distribution/immigrant-ability explanation, immigrant earnings growth should have declined or stayed constant as immigrant entry earnings declined. The latter is consistent with the fixed-cohort-effect methodology (Borjas, 1985).

⁹ Refer to Chiswick (1978a, 1979), Duleep and Regets (1994, 2002), Duleep, Liu, and Regets (2018), and Duleep et al. (2021).

¹⁰ Chiswick (1978a, 1979) first predicted the inverse relationship with supporting evidence that U.S. immigrants from non-English-speaking countries had lower initial earnings but higher earnings growth than immigrants from English-speaking countries. Other confirmatory evidence includes Lalonde and Topel (1991, 1997), Duleep and Regets (1994, 1997, 1999, 2002), Jasso and Rosenzweig (1995), DeSilva (1997), Schoeni (1997), Demombynes (2002), Aydemir and Skuterud (2005), Akresh (2007), Green and Worswick (2012), Lin (2013), Villarreal and Tamborini, (2018), Duleep, Liu, and Regets (2018), and Duleep et al. (2021).

Economists typically use all possible year-of-entry cohorts in their analyses, including those for which the available earnings information begins years after immigrants' initial year of entry. Yet, to avoid implicit assumptions about the relationship between entry earnings and earnings growth, analysts should (ideally) include only year-of-entry cohorts that can be followed from immigrants' initial host-country years. Though reducing the information that is used, this approach insures that results do not reflect an assumed relationship between immigrants' initial earnings and earnings growth.¹¹

Economists also typically combine all year-of-entry cohorts into a single estimation. Since cohorts that vary in their entry-level earnings systematically vary in their earnings growth, analysts should (at least initially) examine each year-of-entry cohort separately.

Limiting their analysis to entry cohorts, following each year-of-entry cohort separately, allowing both entry earnings and earnings growth to vary, and avoiding sample restrictions, the census-based studies of Duleep and Regets (1994, 2002) find that the earnings trajectories of immigrant men who entered the U.S. prior to or just after the 1965 Immigration Act resemble the earnings trajectories of natives.¹² Subsequent cohorts start at lower earnings and have higher earnings growth than natives.

Using longitudinal earnings data matched to the 1994 Current Population Survey (CPS), Duleep and Dowhan (2002) follow immigrant men from their initial U.S. years relative to natives weighted to have the same age and education distribution as immigrants. Immigrants entering in 1960-64 and 1965-69 have initial earnings that are higher than subsequent cohorts and earnings

¹¹ It also avoids confounding effects of age and assimilation and the choice of an appropriate reference group (Kossoudji, 1989, Lalonde and Topel, 1991, and Friedberg 1992).

¹² Immigrants coming from countries whose immigration was severely restricted prior to 1965 lacked families in the U.S. The initial post-1965 entrants were thus more likely to enter the U.S. on employment-based visas than was subsequently the case (Duleep et al., 2021). This likely explains their higher entry earnings and lower earnings growth than subsequent cohorts.

growth resembling that of U.S. natives. Starting with the 1970-74 cohort, the initial earnings of immigrant men fall relative to their U.S.-born statistical twins, and their earnings growth grows.

The blue lines of Figure 1 show, for each year-of-entry cohort, median immigrant earnings divided by median U.S.-born earnings measured in the immigrants' initial year and ten years later.¹³ These studies confirm Borjas' finding that when the initial adjusted earnings of immigrants fall over time, the cross-sectional method overstates the earnings growth of *earlier* immigrant cohorts. It does not follow, however, that the earnings growth of earlier cohorts predicts the earnings growth of more recent cohorts.

In estimating the earnings growth of immigrants, the fixed-cohort-effect method averages the earnings-growth rates of all of the year-of-entry cohorts. This is not a bias issue; the estimate may be unbiased. Nevertheless, with over time decreases in immigrants' adjusted initial earnings, averaging earnings growth across cohorts misrepresents the earnings growth of cohorts that start with relatively low entry earnings.

Figure 1 shows this. The orange lines project the earnings trajectories of immigrants relative to natives using each cohort's earnings growth averaged with the earnings growth of preceding cohorts. The average earnings-growth projections understate actual earnings growth, shown in blue, for all of the post-1965-69 cohorts. For instance, in ten years, the median earnings of immigrants who reported coming to the U.S. in 1982-83 go from 60% of the median earnings of natives, weighted to have the same age and education distribution as the foreign born, to 92% of U.S. natives' earnings. With earnings growth averaged over the current and preceding cohorts, their earnings in ten years go from 60% to 69% of U.S. natives' earnings.¹⁴

¹³ The blue-line estimates are based on Table 2, panel C, adjusted results of Duleep and Dowhan (2002, p. 491). To be in the sample, positive earnings are required in the first and tenth year.

¹⁴ Borjas (1985, 1995, 1999, 2016, 2020) uses the fixed cohort-effect model with census data; Hu (2000) and Lubotsky (2007) use it with longitudinal data on individuals. Borjas (2015) allows earning growth to vary across

Because of the inverse relationship between entry earnings and earnings growth, when immigrant entry earnings fall over time, cross-sectional analyses may outperform fixed-cohort-effect estimates of the earnings growth of more recent immigrant cohorts. Note the similarity in the cross-sectional and cohort-based estimates in Chiswick (1986). This is because increasing earnings growth partially or fully compensates for lower entry earnings. When earnings growth fully compensates, immigrants starting with lower entry earnings achieve the same earnings as earlier cohorts starting at higher entry earnings. This does not mean that the cross-sectional method is right. Borjas (1985) shows it is not. The correspondence occurs because of the inverse relationship between the age- and education-adjusted entry earnings of immigrants and their earnings growth.

Figure 1 measures “initial” earnings one to five years after the period in which immigrants reported to the CPS that they had come to the U.S. Figure 2 defines year-of-entry cohorts by when earnings first appear in Social Security’s record system (Duleep and Dowhan, 2002). Although starting at various beginning points, the year-of-entry cohorts converge to the same relative earnings point.¹⁵

III. Controlling for Immigrant Education Levels Measured Years after U.S. Entry

In measuring the earnings growth of immigrants relative to natives, economists typically control for the level of education reported in the survey year. Controlling for current education likely depresses estimates of immigrant earnings growth. Immigrants who invested in more education have their post-education earnings compared not to the previous earnings of a similar

immigrant cohorts and still finds slow earnings growth for recent immigrants relative to U.S. natives of similar age and education levels. Part IV explains why.

¹⁵ A concern may be that variation in the initial-year earnings reflects variation in the extent to which there are partial-year earnings from newly arrived immigrants. Nevertheless, the finding of convergence holds when defining the cohort from the first year, and measuring earnings starting with the second year.

person holding their old education level, but to someone who already held the new education level in the previous period. The earnings growth estimate for individuals who did not get more education will also be biased downwards since their later earnings will be grouped with those who had just recently obtained the same education level.

As an example, analysts who want to measure the earnings growth of immigrants admitted with high-school degrees will be misled by comparing the average earnings of an entry cohort with high-school degrees in one period with those of high school graduates from the same cohort ten years later. Some of the high-school graduates in the second period will have earned their diploma during those ten years and many high school graduates in the first period will now have higher education.

Chiswick (1986, p. 188) noted the bias in earning growth estimates when controlling for education and comparing two groups with different rates of school attendance:

... the earnings analyses here and in Borjas (1985) bias downward the cohort increase in earnings over the decade by controlling for schooling level in the same year as the earnings data, rather than schooling level in 1970. While this downward bias occurs for all groups, it is likely to be more intense for Cuban and other refugees as they invest in more post immigration schooling. Thus, the downward bias in the estimated growth of earnings would be greater for the Cubans than for other whites.

To the extent that immigrants invest more in schooling than natives, controlling for education using schooling levels immigrants achieved years after their immigration will understate immigrant earnings growth. Using the 1990 Census PUMS (Figure 3) and the 2018 American Community Survey (Figure 4), we estimate the percent in school by age for U.S.-born men and for immigrants, one to five years after their immigration. Both the 1990 and 2018 data show higher levels of school attendance for immigrants than for natives.¹⁶

¹⁶ The 2018 comparison shows recently arrived immigrants closer to U.S. natives in their school attendance than in 1990. This difference does not necessarily mean anything since the 2018 question differs from the 1990 question. For instance, the 1990 question did not include enrollment in a trade or business school, company training, or

Because of the downward bias that using current education levels likely causes, analysts who want to control for education in their earnings estimations should ideally use education levels near immigrants' year of entry. It is possible to do this with longitudinal data when following the same individuals. When following cohorts, the best one can do to mitigate the downward bias is to control for education in broad categories.

IV. Sample Restrictions: The Effect of Excluding Students, Zero Earners, and the Self-Employed

Scholars agree that in following cohorts, immigrant emigration can bias measures of immigrant earnings growth by altering the composition of the samples being compared (e.g. Lubotsky, 2007).¹⁷ Yet, no similar concern is voiced about commonly used sample restrictions—excluding students, zero-earners, and the self-employed.

This is not a problem limited to studies of immigrants. Whenever sample restrictions are applied to individuals in two different periods, it is likely that many individuals included in one period will be excluded in the other. In longitudinal data one can exclude an individual from the data for both periods—technically maintaining a consistent sample universe, but possibly excluding people of analytic and policy interest. When comparing a cohort of individuals across two censuses, there is usually no way to know if a person would have qualified for the universe being analyzed in both periods.

Consider the case of individuals excluded from an initial sample because they are unemployed or out of the labor force (perhaps because of job search or time spent in school), but fully employed and hence included, in a subsequent sample. Individuals may also move from

tutoring; no similar restriction exists in the 2018 ACS question. The changes could differentially affect measurement of immigrant versus native school attendance.

¹⁷ Following the same individuals, as in Duleep and Regets (1997) and Duleep and Dowhan (2002), eliminates selective emigration as a spurious contributor to earnings-growth estimates.

wage and salary jobs (thereby included in the first sample) to self-employment (thereby excluded). While these issues apply to any cohort followed between censuses (or other data sources), they may be particularly important for the study of immigrant earnings growth because immigrants have high occupational mobility, high in-school rates and, for some groups, high self-employment rates.¹⁸

Beyond the effect of sample restrictions on directly measuring immigrant earnings growth, labor economists usually measure the earnings growth of immigrants *relative* to natives. Dividing foreign-born by native-born earnings controls for price movements and changes in the economy. Nevertheless, sample restrictions that differentially affect the estimated earnings growth of immigrants and natives distort the estimated relative earnings growth of immigrants. We illustrate this problem with three hypothetical examples based on theoretical expectations and follow with empirical evidence.

How Sample Restrictions Could Downward Bias the Estimated Relative Earnings Growth of Immigrants: Theoretical Expectations

Figure 5 presents three hypothetical (and simplified) earnings trajectories measuring earnings growth over ten years from immigrants' initial years in the U.S. (year 1) to ten years later (year 10); it assumes that, sans sample restrictions, natives and immigrants have the same earnings growth. Median earnings measured from the unrestricted and restricted samples are Y_u and Y_r , respectively. $(Y_r - Y_u)_1$ is the difference in estimated median earnings between the restricted and unrestricted samples at year 1; $(Y_r - Y_u)_{10}$ is the difference at year 10.¹⁹ The

¹⁸ For occupational mobility see, for instance, Akresh (2006, 2008), Chiswick (1978b), Chiswick, Lee, and Miller (2005), Chiswick and Miller (2008), Green (1999), Jasso and Rosenzweig (1990, 1995), and Zorlu (2013). For educational investment see, for instance, Duleep and Regets (1999, 2002), Chiswick and DebBurman (2004), Van Tubergen and van de Werfhorst (2007), and Jasso and Rosenzweig (1990). For self-employment see, for instance, Lofstrom (2000, 2002, 2011).

¹⁹ Note that if $(Y_r - Y_u)_1 = (Y_r - Y_u)_{10}$ then the estimated earnings growth is unaffected by the sample restriction. In Figure 5, this is the case for the top and middle tier graphs for natives and the bottom tier graph for immigrants.

earnings trajectories estimated with unrestricted samples are blue lines; those estimated from restricted samples are orange.

Hypothetical Scenario 1

Ceteris paribus, the earnings growth of immigrants relative to natives will be underestimated if a sample exclusion increases earnings in year 1 relative to year 10 more for immigrants than for natives. In other words, immigrant relative earnings growth is biased downwards to the extent that $[(Y_r - Y_u)_1 - (Y_r - Y_u)_{10}]$ is greater for immigrants than for natives. This situation, depicted in Figure 5's top tier, could occur when students are excluded from samples.

Excluding students from any sample will increase median (or average) earnings to the extent that students do not work, or work in lower-paying part-time jobs. Because immigrants are initially more likely to attend school than natives, we would expect that excluding students would increase initial earnings relative to year-10 earnings more for immigrants than for natives, thus biasing downward foreign-born relative earnings growth.

We would further expect this downward bias to be most prevalent for more highly educated immigrants. Although the effect of education on human capital investment is ambiguous in most human capital investment models (by increasing both the productivity and opportunity cost of human capital investment), in the Immigrant Human Capital Investment model (Duleep and Regets, 2002) education that does not transfer to the labor market (and thus does not raise the opportunity cost of human capital investment) *is* useful for learning new skills.

We would therefore expect school attendance by immigrants, relative to natives, to increase with initial education levels.²⁰ The greater the immigrant/native differential in attending

²⁰ For empirical evidence on this, refer to Betts and Lofstrom (2000).

school, the greater the likelihood that excluding students from the sample negatively biases the relative earnings growth of immigrants.

Hypothetical Scenario 2

Foreign-born relative earnings growth will be underestimated if a sample exclusion increases earnings in year 10 relative to year 1 less for immigrants than for natives. In other words, immigrant relative earnings growth is biased downwards to the extent that $[(Y_r - Y_u)_{10} - (Y_r - Y_u)_1]$ is less for immigrants than it is for natives. This situation, depicted in Figure 5's middle tier, could occur when zero earners are excluded from samples.

Excluding zero earners will raise average earnings in year 1 and year 10 for both immigrants and natives. Yet, holding other factors constant, we would expect the year-10 increase relative to the year-1 increase to be less for immigrants than for natives. When researchers exclude zero earners from their samples, they are very likely to include in the second-period sample individuals who previously had zero earnings in the first period. Even if the typical ten years between earnings measurements is enough for immigrants with initial skill-transferability problems to earn the same as natives, their exclusion from the first period downward biases earnings-growth estimates. Immigrants will be more likely than natives to go from zero earnings in the first period to positive earnings in the second period, particularly among those with the highest propensity to invest in human capital—immigrants who initially lack transferable skills but have relatively high levels of schooling and are young (Duleep and Regets, 2002).

Hypothetical Scenario 3

Foreign-born relative earnings growth will be underestimated if a sample exclusion increases earnings in year 10 relative to year 1 more for natives than for immigrants. This

situation, depicted in Figure 5's bottom tier, is another way excluding zero earners could lower estimates of foreign-born relative earnings growth.

If, controlling for age and education, those who stop working had lower wages prior to leaving the labor force than men who continue working, then excluding zero earners would, in ten years, create a group with higher wages. For a variety of reasons, we would expect less of a labor force dropout for immigrant men than for U.S. natives. To the extent that this is true, studies that exclude zero earners will be comparing immigrants to a more select group of natives at the ten-year point than at the first-year point thereby depressing the estimated relative earnings growth of immigrants.

The Effect on Immigrant Relative Earnings Growth of Excluding Students, Zero Earners, and the Self-employed: Empirical Evidence

To explore empirically the effect of excluding students, zero earners, and the self-employed on the relative earnings growth of immigrant men we use the Census data that Borjas (2015) uses—the public use micro Census data extracted from the 1970, 1980, 1990, and 2000 censuses and the 2010 American Community Survey (ACS).²¹ We define immigrants as persons born outside of U.S. soil who are either naturalized citizens or non-citizens²² and U.S. natives as persons born on U.S. soil who are U.S. citizens.²³

²¹ Our 1970 census data include the following three random samples: 1% state sample + 1% metro sample + 1% neighborhood sample (all from form 1). 1980 census data include: 5% state sample + 1% metro sample + 1% urban/rural sample + 1% labor market areas sample + 1% metro/non-metro sample. 1990 census data include: 5% state sample + 1% metro sample. 2000 census data include: 5% sample + 1% sample. Finally, we use the 2010 American Community Survey (ACS). Sample weights are used in all analyses, in particular, for example, for the 1980 sample, the individual sample weight is constructed by dividing the census weight by the number of representative samples used in each census year: 5 samples from 1980 are used.

²² U.S. soil includes U.S. outlying areas/territories, for example, American Samoa, Guam, Puerto Rico, U.S. Virgin Islands and other U.S. possessions. Non-citizen includes those who had begun the naturalization process but not those who were born abroad of American parents.

²³ Our definition of U.S. natives does not include those who were born abroad of American parents. Therefore, neither immigrants nor natives include people born abroad of American parents according to our definition.

As in Borjas (2015), we measure earnings with weekly earnings calculated by dividing annual earnings by weeks of work. Annual earnings include wage and salary income, non-farm business income, and farm income.²⁴ Weeks of work measure number of weeks the respondent worked for pay during the previous year.²⁵ (For those who report zero weeks of work, weekly earnings are defined as zeros and included in our unrestricted sample described below.)²⁶

Limiting our analysis to immigrant cohorts that can be followed from their initial years in the U.S., we analyze each entry cohort separately. Our entry cohorts for immigrants are persons who came to live in the U.S. within the last 5 years of each census year. We follow immigrant cohorts, defined by year of arrival, by consecutive censuses for ten years.

We separate each cohort by age: a younger group, 25 to 39 years old in the initial census and 35 to 49 in the next census, and an older group, 40 to 54 in the initial census and 50 to 64 in the next census. We further separate by education level: above high school (persons who completed thirteen or more school years) and high school and below (persons who completed less than thirteen school years). For instance, for the 1970 census the younger, more educated cohort includes immigrants who were 25-39 years old in 1970, arrived between 1965 and 1970,

²⁴ Earning data were top coded for census years 1970 and 1980 at \$50,000 and \$75,000, respectively. Following Borjas (2015), we multiply the top-coded earnings by 1.5. On the 1990 and 2000 IPUMS data file, income above the top codes (\$140,000 and \$175,000 respectively) is reported as the median of all income in an individual's state who are above the top code. Lastly, for 2010 ACS, earnings above the 99.5th percentile in the state are replaced by the state means above that value.

²⁵ Only categorical data were available for "weeks worked last year" in the 1970 census and the 2010 ACS. Following Borjas (2015), we use the median value within each interval. In particular, for people who report working between 1-13 weeks, we use 8 weeks.

²⁶ In constructing weekly earnings, we closely follow Borjas (2015). There are, however, a few differences. Borjas (2015) uses proportion of working weeks in a year to further weight individual observations. Specifically, Borjas (2015) takes the product of "the census provided individual sample weight" and "the share of the working weeks" as the weight to generate empirical results. Other than using the census provided sample weight, we do not further weight our sample by working intensity. Borjas (2015) uses log of weekly earnings, which automatically removes zero earnings. In our unconstrained samples, we do not remove individuals with zero earnings or zero weeks of work. We only use the census weight even when zero earners are excluded.

with thirteen or more school years in 1970; using 1980 census data, the same cohort is 35-49 in 1980, arrived within the last 10-15 years, with thirteen or more school years in 1980.

Within age and education groups, we measure weekly earnings of foreign-born men relative to U.S.-born men in one census and then for the same cohort ten years later, in the subsequent census. Immigrant relative weekly earnings are calculated by dividing the median weekly earnings of immigrants in an age/education cell by the median weekly earnings for U.S. natives from the same age/education cell. Our approach is nonparametric—the relationship between immigrant entry earnings and earnings growth is not constrained in any way. We also, initially, do not restrict the sample in any way. These earnings trajectories provide unconstrained baselines.

We then exclude students, the self-employed, and zero earners from our samples and again measure initial median earnings and median earnings ten years hence. We learn the effect of sample restrictions by comparing the earnings trajectories based on unrestricted versus restricted samples. Since we compare the estimates for each cohort over the same ten years, all earnings' data are in current U.S. dollars.

The blue line in each year-of-entry/age/education graph of Figure 6 represents the estimated growth of median weekly earnings for foreign-born men relative to U.S.-born men with no sample restrictions. The orange line shows the relative earnings of immigrants when we exclude the self-employed, students, and zero earners. Figure 6 reveals that in almost all age/education/year-of-entry cohorts, the relative earnings growth of immigrants based on unrestricted samples exceeds the earnings growth based on restricted samples.

The Separate Effects of Excluding Students, Zero Earners, and the Self-employed

In results not shown, we examine the individual effect of each sample restriction on immigrants' relative earnings growth. That is, what is the effect on foreign-born relative earnings growth of only excluding the self-employed, or only students, or only zero earners?

Excluding the self-employed barely nudges the relative weekly earnings growth of immigrants in most age/education/year-of-entry cohorts²⁷; when it does have an effect, it slightly increases relative foreign-born earnings growth. Borjas (1985, 1994, and 1995) excludes students, the self-employed, and zero earners; Borjas (2015) excludes students and zero earners.

Excluding students decreases foreign-born relative earnings growth for the more highly educated in both age groups. With the exception of the 1965-70 cohort, it does not decrease the relative earnings growth for the less educated. Excluding zero earners decreases foreign-born relative earnings growth for all age/education/year-of-entry groups with a single exception—the young/more-educated in the 1965-70 cohort.

To understand how excluding students downward biases estimates of immigrant relative earnings growth for men with at least thirteen years of schooling and how excluding zero earners does this for almost all age/education/year-of-entry groups, we examine the separate effects of these two restrictions for immigrants and natives.

The effects of excluding students for immigrants and natives with at least 13 years of schooling

As pictured in Figure 5's top tier, we hypothesized that excluding students would (for the more educated) raise initial earnings relative to earnings ten years later more for immigrants than

²⁷ The fact that excluding the self-employed has little effect on our measures of immigrant relative earnings growth may reflect the fact that our analyses include all immigrants. Immigrant groups vary in their self-employment rates. The effects of excluding the self-employed on immigrant earnings growth measured for particular groups, relative to all natives, may differ dramatically from our all-immigrant/all-native analyses.

for natives: $[(Y_r - Y_u)_1 - (Y_r - Y_u)_{10}]_{FB} > [(Y_r - Y_u)_1 - (Y_r - Y_u)_{10}]_{NB}$ wherein Y_u and Y_r denote the median weekly earnings estimated from unrestricted and restricted samples and FB and NB denote the foreign born and native born. As shown in Table 1, $[(Y_r - Y_u)_1 - (Y_r - Y_u)_{10}]$ is greater for immigrants than it is for natives for all year-of-entry/age groups with at least 13 years of schooling. For the younger group in the three most recent immigrant cohorts, $[(Y_r - Y_u)_1 - (Y_r - Y_u)_{10}]$ is at least three times the analogous difference for U.S. natives. For the older group in the two most recent immigrant cohorts, $[(Y_r - Y_u)_1 - (Y_r - Y_u)_{10}]$ is at least four times that for natives.

The effects of excluding zero earners for immigrants and natives

We proposed two paths for $[(Y_r - Y_u)_{10} - (Y_r - Y_u)_1]$ being greater for natives than for immigrants, thereby biasing downward the relative earnings growth of immigrants. One, pictured in Figure 5's middle tier, occurs if excluding zero earners raises earnings at year 10 less than at year 1 for immigrants than for natives. As shown in Table 2, there is not a single case where $[(Y_r - Y_u)_{10} - (Y_r - Y_u)_1] < 0$ for natives whereas we consistently find this result for the young, more educated immigrants.

Another path is if excluding zero earners raises the earnings in year 10 relative to year 1 more for natives than for immigrants, as depicted in Figure 5's bottom tier. As shown in Table 2, excluding zero earners increases the earnings at year 10 relative to year 1 for all age/education/year-of-entry groups for natives, but does so to a much lesser degree or, not at all, for immigrants.

The dramatically different effects for natives and immigrants of excluding zero earners distort measures of immigrant relative earnings growth. Not excluding zero earners, immigrants have high relative earnings growth. Excluding zero earners, immigrants have low relative

earnings growth because they are being compared to a more select sample of U.S.-born men at the ten-year point than at the first-year point. These results suggest that the U.S.-born composition to whom immigrants are compared in Borjas (1985, 1994, 1995, and 2015) changes *radically* over ten years creating the illusion that post-1965 immigrants have low earnings growth relative to U.S. natives.

V. Skill Transferability, Human Capital Investment, and Earnings Growth

The preceding analyses show that U.S. immigrant men in the decades following the 1965 Immigration and Nationality Act experience high earnings growth relative to U.S. natives.²⁸ Chiswick (1978a, 1979, 1986), Duleep and Regets (1999, 2002), and Duleep, Liu, and Regets (2018) assume that human capital investment underlies earnings growth and that differences in human capital investment between immigrants and natives, and among immigrants, stem from differences in the degree to which immigrants' source-country skills transfer to the U.S. labor market.²⁹

We would argue that the adjusted earnings gap—the difference between an immigrant's earnings and that of natives of similar age and education level—is a strong indicator of the level of skill transferability. We would also argue that human capital investment is best measured by earnings growth. Human capital investment takes myriad difficult or impossible-to-measure forms. Moreover, groups vary in their human capital investment strategies.

²⁸ We limit our analyses to immigrant men in part because of less complex labor force participation patterns for males, particularly over the multiple decades of our data. However, interruptions in women's full-time work experience for family reasons such as having primary responsibility for child rearing (Blau and Kahn 2013) or to aid the formation and running of a business (Duleep et al., 2021) may actually make our measurement concerns more important for measuring immigrant earning growth.

²⁹ Cortes (2004), Duleep and Regets (1999), and Duleep et al. (2021) also emphasize the importance of permanence.

Seeking a more tangible measure of skill transferability, scholars such as Akresh (2007) and Borjas (2015) use English proficiency. English proficiency, however, captures but one aspect of skill transferability whereas the adjusted earnings gap embodies all aspects of skill-transferability, measured and unmeasured. Also, the relationship between English proficiency and skill transferability does not occur in a vacuum. Because English proficiency affects immigrants' decisions about what work to pursue and where to live, the source-country skills of immigrants who speak English poorly may transfer well. Korean immigrants with poor English fluency have high self-employment rates³⁰; the initial earnings of Japanese immigrants, who have low English proficiency, are high³¹; Mexican immigrants who speak English poorly move to areas with large Spanish-speaking enclaves.³² Conversely, the skills of immigrants who speak English perfectly may not transfer well: persons leaving their original countries to escape constraints will come to the U.S. (or other economically developed countries) even if it means starting a new career.

The program under which immigrants enter the U.S. may provide a “tangible” but less problematic measure of skill transferability than English proficiency. By its very nature—based on an employer's willingness to participate in a labor certification process—employment-based immigrants have skills immediately valued by employers, in contrast to persons entering the U.S. via a family-based visa. Studies in Canada and the U.S. find that, controlling for age and education, immigrants who enter on a family visa have lower entry earnings but higher earnings growth than persons who enter via an employment visa.³³

³⁰ Refer to Duleep et al. (2021).

³¹ Duleep et al. (2021) explore why.

³² Refer to Bauer et al. (2005).

³³ Using different data sets, Jasso and Rosenzweig (1995), Duleep and Regets (1996a, b), and DeSilva (1997) find that family-based immigrants start their host-country lives with lower earnings than their employment-based statistical twins but have higher earnings growth; with time, their earnings converge.

Updating previous results, we used immigrant admissions' data from the 1993-1998 Immigration and Naturalization Service public use files to estimate multinomial logits of the probabilities that an immigrant would be in each of 14 different admission categories based upon their age, sex, and country of birth. Using these logit parameter estimates, we calculated probabilities of admission type for individuals on the 1995-2000 entry cohort on the 2000 decennial census and the 2008 American Community Survey. The probability of family was calculated as the sum of the probabilities of the four family admissions categories. Confirming earlier studies, immigrants who are likely family-based have lower initial earnings and higher earnings growth than immigrants who are more likely to be employment based.³⁴

Does this higher earnings growth represent human capital investment? And, if so, does it simply reflect learning English?

To answer these questions, we used the census school-attendance question. This question does not measure participation in English classes per se since it asks only about courses in degree or high school diploma programs. Since it asks whether individuals *currently* attend school, it avoids measuring changes in educational achievement by following cohorts, with the attendant problem of emigration.

Relating school attendance to the probability that an immigrant was admitted by a family versus employment-based visa reveals that family-based immigrants are more likely to attend school than employment-based immigrants (Figure 7).³⁵ With the caveat that attending school is

³⁴ While we focus in Part V on likely differences between immigrants admitted via work visas versus those with family sponsorship, many other factors can affect the incentives and the ability to work or invest in human capital. For example, the spouses of most types of work-visa holders are either forbidden to work or find work authorization difficult. Duleep et al. (2021) find that the larger the initial adjusted earnings gap for men, the more likely that women work. Yet, this relationship may reflect, at least in part, work restrictions for spouses of work-visa holders. To untangle these issues, analysts need data that more closely links visa information with work history. One exciting possibility would be to link information from individual visas to earnings data in the Social Security administrative record systems.

³⁵ Figure 7 shows school attendance as a function of age and the probability of being a family-based immigrant

only one form of human capital investment (and not necessarily the most important one), this result supports the thesis that because of skill-transferability issues, immigrants with low initial earnings (given their age and at-entry education levels) will be more likely to invest in human capital than other immigrants or natives of similar age and education.

VI: CONCLUSION

The success of immigrants in the U.S., or other receiving countries, has long been the focus of important policy discussions. The 1965 Immigration and Nationality Act, emphasizing family sponsorship, allowed immigrants from a more diverse set of countries than had characterized U.S. immigration in the four decades preceding it. Whereas the earlier immigrants earned on par with U.S. natives of comparable age and education, the initial earnings of post-1965 immigrants lie far below the earnings of their U.S.-born statistical twins.

Initial earnings, however, poorly predict the economic contribution of immigrants to the U.S. economy, or their eventual social and economic status. Rapid earnings growth accompanies low initial earnings that are due to low initial skill transferability. High earnings growth affects more than immigrants' lifetime earnings: a pattern of increasing earnings mitigates social pathologies often associated with low incomes.

High earnings growth may also signify something different about the economic contribution of immigrants to receiving countries. Human capital that is not immediately valued in the host-country's labor market is useful for learning new skills. To find a niche in their new economy, immigrants adapt and increase their human capital thereby fueling innovation and

(which is a function of country of birth, age, and year of immigration). The graphical results are evaluated at the mean level of schooling for immigrants. In other work, we find a positive relationship between family admissions and school attendance at all schooling levels. However, the higher the education level, the greater the effect of family-based admission on school attendance, consistent with the IHCI model described in Duleep and Regets (2002).

flexibility.³⁶ The social and economic consequences of low earnings associated with high human-capital investment differ profoundly from the social and economic consequences of low earnings associated with low ability.

The predominant methodologies economists use to measure immigrant economic assimilation tend to underestimate the earnings growth of immigrants who start with low earnings relative to U.S. natives. One problem stems from not allowing earning growth to vary with entry earnings. Human capital theory and much empirical work suggest that the initial earnings of immigrants, adjusted for age and education, inversely relate to earnings growth rates.

Another problem, for studies that follow cohorts across censuses, is when a control or stratification variable (such as years of schooling) can only be measured at the time of each census.³⁷ Finally, when following cohorts, sample exclusions used by labor economists bias downward the relative earnings growth of immigrants who begin with low initial earnings.

Excluding zero earners is a staple of labor economics given its reliance on Mincer earnings equations wherein the dependent variable is the logarithm of earnings. Their exclusion does not cause a bias problem in analyses that follow the same individuals: Duleep and Dowhan (2002) and Duleep and Regets (1997) exclude zero earners. If, however, despite having longitudinal individual data, analysts do not follow the same individuals then excluding zero earners likely understates the earnings growth of U.S. immigrants relative to that of U.S. natives.

Our suggestions for estimating immigrant earnings growth are summarized in the following guidelines. Each of these guidelines will have exceptions for specific analytic

³⁶ On this topic, see Green (1999) and Duleep, Jaeger, and McHenry (2019).

³⁷ This will also be a problem with longitudinal data if analysts do not or cannot carry information on the same individual from one observation period to the next.

purposes, but are a good starting point for measuring immigrant economic adaptation and earnings growth:

—When analyzing cohorts over time, include only those who can be tracked from near their initial year of migration.

—Follow each entry cohort separately, not constraining growth rates to be the same across cohorts.

—Be aware that education measured years after migration is not the same as at entry.

—Minimize sample exclusions when following cohorts. Include students, the self-employed, and zero earners.

While these recommendations generally apply to all immigrants, their importance for accurately measuring immigrant earnings growth for a given group depends on the propensity to invest in human capital for that group, the higher this propensity, the greater the relevance of our suggested strategies. Key determinants of the propensity to invest in human capital appear to be the level of economic development of the country of origin relative to the U.S., the degree of permanence of its U.S. immigrants, and the education level of immigrants (Duleep et al., 2021).³⁸ Our analysis focuses on men only. A challenge of extending our work to immigrant versus U.S.-born women is that experience is on average overstated for women given interruptions in their work histories thus leading to smaller than true earnings function gradients (Polachek, 2008).³⁹

³⁸ The effect of allowing earnings growth to vary with entry earnings on estimates of immigrant earnings growth will depend on how immigrant entry earnings, within age and education groups, have varied over time. With a decrease in entry earnings, the fixed-cohort-effect methodology underestimates the actual earnings growth of the more recent cohorts. This is the case for immigrants originating from economically developing Asian countries following the 1965 Immigration and Nationality Act (Duleep et al., 2021). Conversely, when the (adjusted) entry earnings increase from earlier to more recent cohorts, the fixed-cohort-effect methodology overestimates actual earnings growth. This is the case for certain age-education groups of European immigrants (Duleep et al., 2021).

³⁹ For the participation decision, Blau et al. (2011) highlight the role of country-of-origin culture in determining the U.S. labor force participation of immigrant women. In contrast, examining fewer countries, Duleep et al. (2021) document startling disconnects between the cultures of source countries and the U.S. labor force participation of immigrant women. These varying results highlight the need for further efforts to untangle underlying causalities in

In conclusion, following immigrant cohorts from their beginning years in the U.S., imposing no sample restrictions, and letting earnings growth vary with entry earnings reveals high earnings growth of immigrant men relative to U.S. natives in the decades following the Immigration and Nationality Act.

the role of women in the economic assimilation of immigrants.

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Table 1: The effect of excluding students on median weekly earnings for immigrants, measured one to five years after U.S. immigration and ten years later, and for U.S. natives:
Men with thirteen or more years of schooling

| <i>Men who completed thirteen or more school years and were 25-39 years old at the time of the initial census</i> | | | | | | | | | |
|---|---|-----------------------|---------|-----------------------|---------|-----------------------|---------|-----------------------|---------|
| | | 1965-70 cohort | | 1975-80 cohort | | 1985-90 cohort | | 1995-00 cohort | |
| | | Year 1 | Year 10 | Year 1 | Year 10 | Year 1 | Year 10 | Year 1 | Year 10 |
| Foreign born | | | | | | | | | |
| | Yu=median earnings for unconstrained sample | 140 | 442 | 208 | 636 | 317 | 769 | 560 | 1079 |
| | Yr=median earnings for constrained sample | 156 | 458 | 270 | 673 | 404 | 775 | 673 | 1156 |
| | Yr-Yu | 16 | 16 | 62 | 37 | 87 | 6 | 113 | 77 |
| | (Yr-Yu)1 - (Yr-Yu)10 | 0 | | 24 | | 81 | | 36 | |
| Native born | | | | | | | | | |
| | Yu=median earnings for unconstrained sample | 194 | 442 | 335 | 686 | 538 | 885 | 719 | 1060 |
| | Yr=median earnings for constrained sample | 194 | 450 | 346 | 692 | 563 | 900 | 750 | 1080 |
| | Yr-Yu | 0 | 8 | 12 | 6 | 24 | 15 | 31 | 20 |
| | (Yr-Yu)1 - (Yr-Yu)10 | -8 | | 6 | | 9 | | 11 | |
| <i>Men who completed thirteen or more school years and were 40-54 years old at the time of the initial census</i> | | | | | | | | | |
| | | 1965-70 cohort | | 1975-80 cohort | | 1985-90 cohort | | 1995-00 cohort | |
| | | Year 1 | Year 10 | Year 1 | Year 10 | Year 1 | Year 10 | Year 1 | Year 10 |
| Foreign born | | | | | | | | | |
| | Yu=median earnings for unconstrained sample | 174 | 369 | 289 | 538 | 446 | 665 | 579 | 676 |
| | Yr=median earnings for constrained sample | 181 | 374 | 308 | 553 | 500 | 669 | 615 | 694 |
| | Yr-Yu | 7 | 4 | 19 | 14 | 54 | 4 | 37 | 18 |
| | (Yr-Yu)1 - (Yr-Yu)10 | 2 | | 5 | | 50 | | 19 | |
| Native born | | | | | | | | | |
| | Yu=median earnings for unconstrained sample | 242 | 426 | 462 | 690 | 738 | 865 | 923 | 934 |
| | Yr=median earnings for constrained sample | 242 | 430 | 467 | 692 | 750 | 865 | 930 | 939 |
| | Yr-Yu | 0 | 4 | 5 | 2 | 12 | 0 | 7 | 5 |
| | (Yr-Yu)1 - (Yr-Yu)10 | -4 | | 3 | | 12 | | 2 | |

Notes: (Yr-Yu)1 - (Yr-Yu)10 is the difference between the median earnings estimated from the restricted sample (the sample excluding students) and the unrestricted sample at year 1 minus the difference between the median earnings from the restricted and unrestricted samples measured at year 10.

Table 2: The effect of excluding zero earners on median weekly earnings for immigrants, measured one to five years after U.S. immigration and ten years later, and for U.S. natives

| <i>Men who completed thirteen or more school years and were 25-39 years old at the time of the initial census</i> | | | | | | | | | |
|---|--|----------------|---------|----------------|---------|----------------|---------|----------------|---------|
| | | 1965-70 cohort | | 1975-80 cohort | | 1985-90 cohort | | 1995-00 cohort | |
| | | Year 1 | Year 10 | Year 1 | Year 10 | Year 1 | Year 10 | Year 1 | Year 10 |
| Foreign born | | | | | | | | | |
| | Yu=median earnings for unrestricted sample | 140 | 442 | 208 | 636 | 317 | 769 | 560 | 1079 |
| | Yr=median earnings for restricted sample | 155 | 456 | 250 | 672 | 385 | 800 | 675 | 1156 |
| | Yr-Yu | 15 | 13 | 42 | 37 | 67 | 31 | 115 | 77 |
| | (Yr-Yu)10 - (Yr-Yu)1 | | -2 | | -5 | | -37 | | -38 |
| Native born | | | | | | | | | |
| | Yu=median earnings for unrestricted sample | 194 | 442 | 335 | 686 | 538 | 885 | 719 | 1060 |
| | Yr=median earnings for restricted sample | 194 | 456 | 346 | 706 | 558 | 923 | 746 | 1156 |
| | Yr-Yu | 0 | 14 | 12 | 20 | 19 | 38 | 27 | 96 |
| | (Yr-Yu)10 - (Yr-Yu)1 | | 14 | | 8 | | 19 | | 69 |
| <i>Men who completed thirteen or more school years and were 40-54 years old at the time of the initial census</i> | | | | | | | | | |
| | | 1965-70 cohort | | 1975-80 cohort | | 1985-90 cohort | | 1995-00 cohort | |
| | | Year 1 | Year 10 | Year 1 | Year 10 | Year 1 | Year 10 | Year 1 | Year 10 |
| Foreign born | | | | | | | | | |
| | Yu=median earnings for unrestricted sample | 174 | 369 | 289 | 538 | 446 | 665 | 579 | 676 |
| | Yr=median earnings for restricted sample | 186 | 390 | 330 | 596 | 572 | 737 | 712 | 809 |
| | Yr-Yu | 12 | 21 | 42 | 58 | 126 | 71 | 133 | 133 |
| | (Yr-Yu)10 - (Yr-Yu)1 | | 9 | | 16 | | -55 | | 0 |
| Native born | | | | | | | | | |
| | Yu=median earnings for unrestricted sample | 242 | 426 | 462 | 690 | 738 | 865 | 923 | 934 |
| | Yr=median earnings for restricted sample | 248 | 470 | 480 | 769 | 769 | 981 | 962 | 1156 |
| | Yr-Yu | 6 | 44 | 18 | 79 | 31 | 115 | 38 | 222 |
| | (Yr-Yu)10 - (Yr-Yu)1 | | 38 | | 61 | | 84 | | 184 |
| <i>Men who completed less than thirteen school years and were 25-39 years old at the time of the initial census</i> | | | | | | | | | |
| | | 1965-70 cohort | | 1975-80 cohort | | 1985-90 cohort | | 1995-00 cohort | |
| | | Year 1 | Year 10 | Year 1 | Year 10 | Year 1 | Year 10 | Year 1 | Year 10 |
| Foreign born | | | | | | | | | |
| | Yu=earnings for unrestricted sample | 117 | 250 | 158 | 337 | 214 | 400 | 292 | 443 |
| | Yr=earnings for restricted sample | 119 | 269 | 182 | 360 | 250 | 442 | 346 | 482 |
| | Yr-Yu | 2 | 19 | 24 | 23 | 36 | 42 | 54 | 39 |
| | (Yr-Yu)10 - (Yr-Yu)1 | | 17 | | -1 | | 7 | | -15 |
| Native born | | | | | | | | | |
| | Yu=earnings for unrestricted sample | 145 | 308 | 269 | 442 | 365 | 538 | 480 | 540 |
| | Yr=earnings for restricted sample | 148 | 330 | 285 | 481 | 396 | 604 | 529 | 694 |
| | Yr-Yu | 3 | 22 | 16 | 38 | 30 | 66 | 49 | 154 |
| | (Yr-Yu)10 - (Yr-Yu)1 | | 19 | | 22 | | 35 | | 105 |
| <i>Men who completed less than thirteen school years and were 40-54 years old at the time of the initial census</i> | | | | | | | | | |
| | | 1965-70 cohort | | 1975-80 cohort | | 1985-90 cohort | | 1995-00 cohort | |
| | | Year 1 | Year 10 | Year 1 | Year 10 | Year 1 | Year 10 | Year 1 | Year 10 |
| Foreign born | | | | | | | | | |
| | Yu=earnings for unrestricted sample | 113 | 215 | 165 | 288 | 222 | 333 | 308 | 385 |
| | Yr=earnings for restricted sample | 117 | 236 | 194 | 346 | 269 | 404 | 365 | 462 |
| | Yr-Yu | 4 | 22 | 30 | 58 | 47 | 71 | 58 | 77 |
| | (Yr-Yu)10 - (Yr-Yu)1 | | 18 | | 28 | | 24 | | 19 |
| Native born | | | | | | | | | |
| | Yu=earnings for unrestricted sample | 155 | 250 | 308 | 365 | 462 | 442 | 538 | 423 |
| | Yr=earnings for restricted sample | 155 | 317 | 334 | 500 | 506 | 635 | 625 | 732 |
| | Yr-Yu | 0 | 67 | 26 | 135 | 45 | 192 | 87 | 309 |
| | (Yr-Yu)10 - (Yr-Yu)1 | | 67 | | 108 | | 147 | | 223 |

Notes: (Yr-Yu)10 - (Yr-Yu)1 is the difference between the median earnings estimated from the restricted sample (the sample excluding zero earners) and the unrestricted sample at year 10 minus the difference between the median earnings from the restricted and unrestricted samples measured at year 1.

Figure 1: Adjusted foreign-born annual earnings relative to native-born annual earnings measured at the median in the first and tenth year following immigrants' CPS reported year of entry. Men are at least 25 years old in the first year and no more than 60 years old in the tenth year. Immigrant cohorts are defined by the 1994 CPS-reported year of entry. The solid blue lines represent the earnings growth estimated for each cohort. The dashed orange lines represent earnings growth averaged across current and prior cohorts.

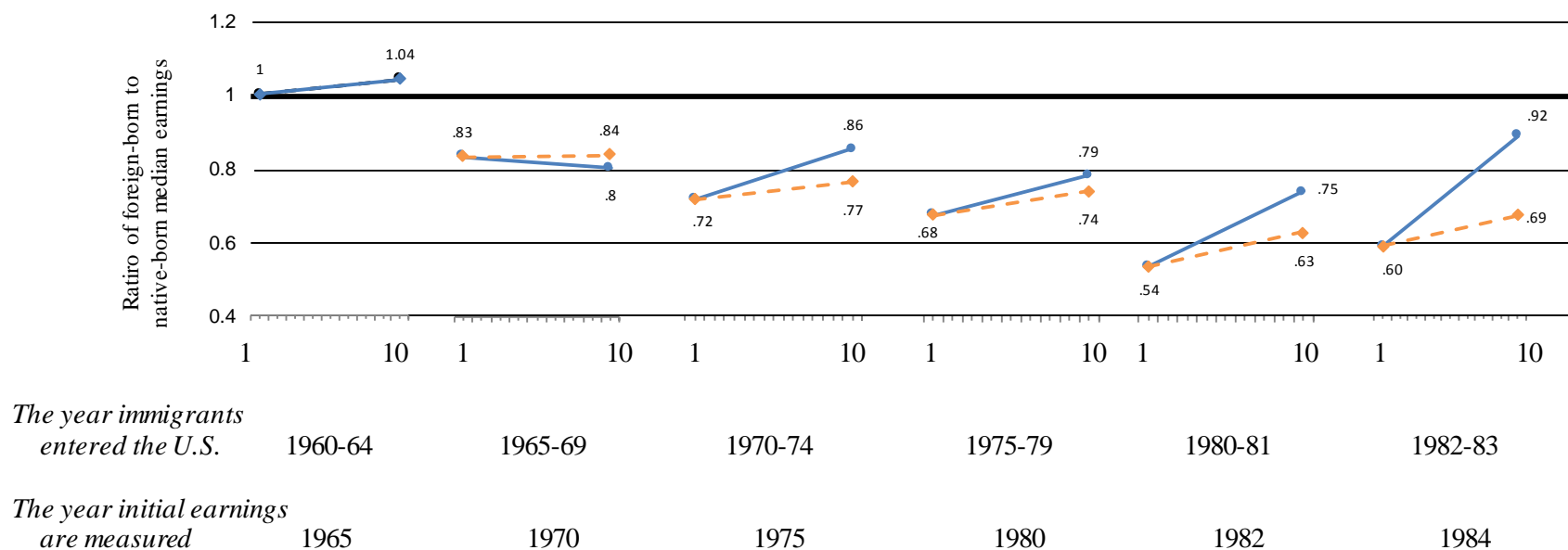
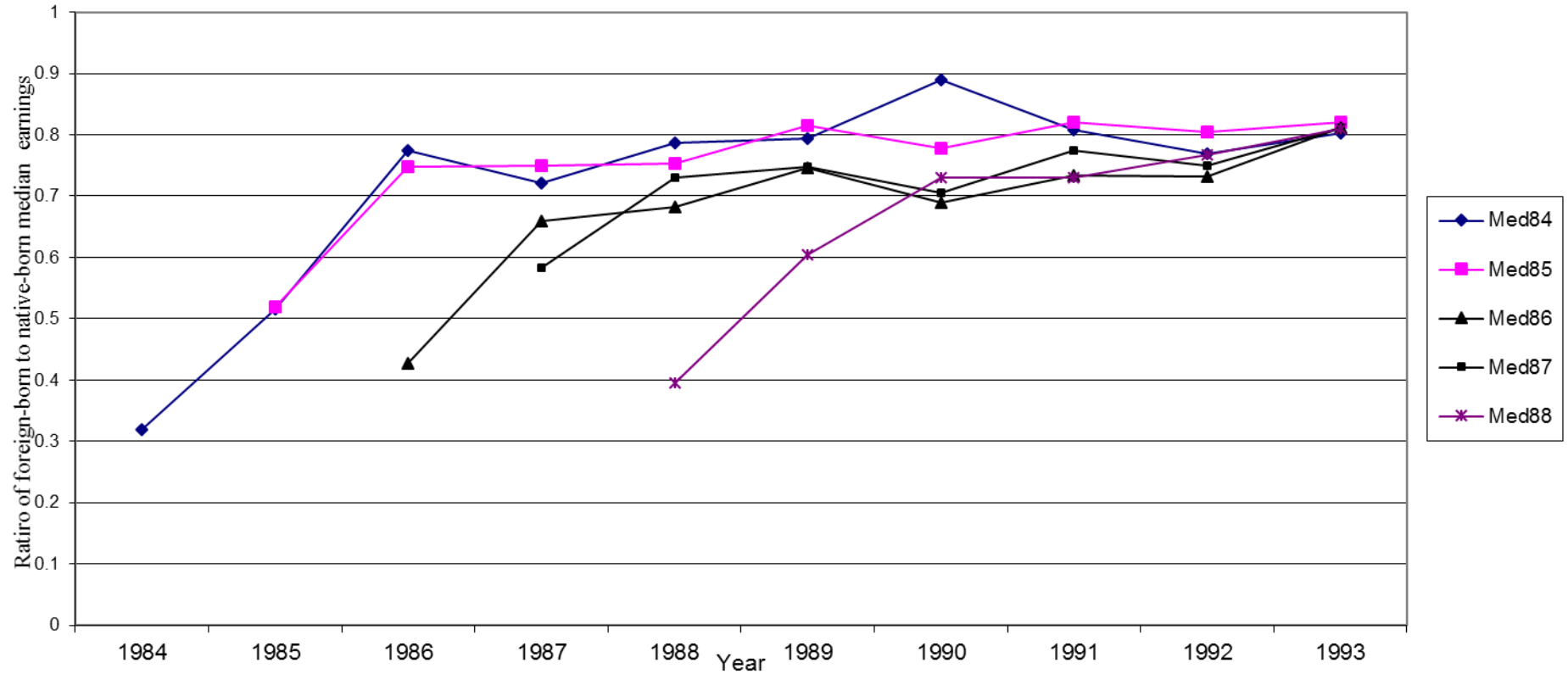


Figure 2: Adjusted foreign-born relative to native-born annual median earnings: cohorts defined by first evidence of earnings in Social Security administrative record data. Men are at least 25 years old in the first year and no more than 60 years old in 1993.



Source: Duleep and Dowhan (2002, Figure 6, p. 503)

Figure 3: Percent in School by Age: Immigrants 1 to 5 years after entry and U.S.-born men.
Estimates based on 1990 Census PUMS 5% sample.

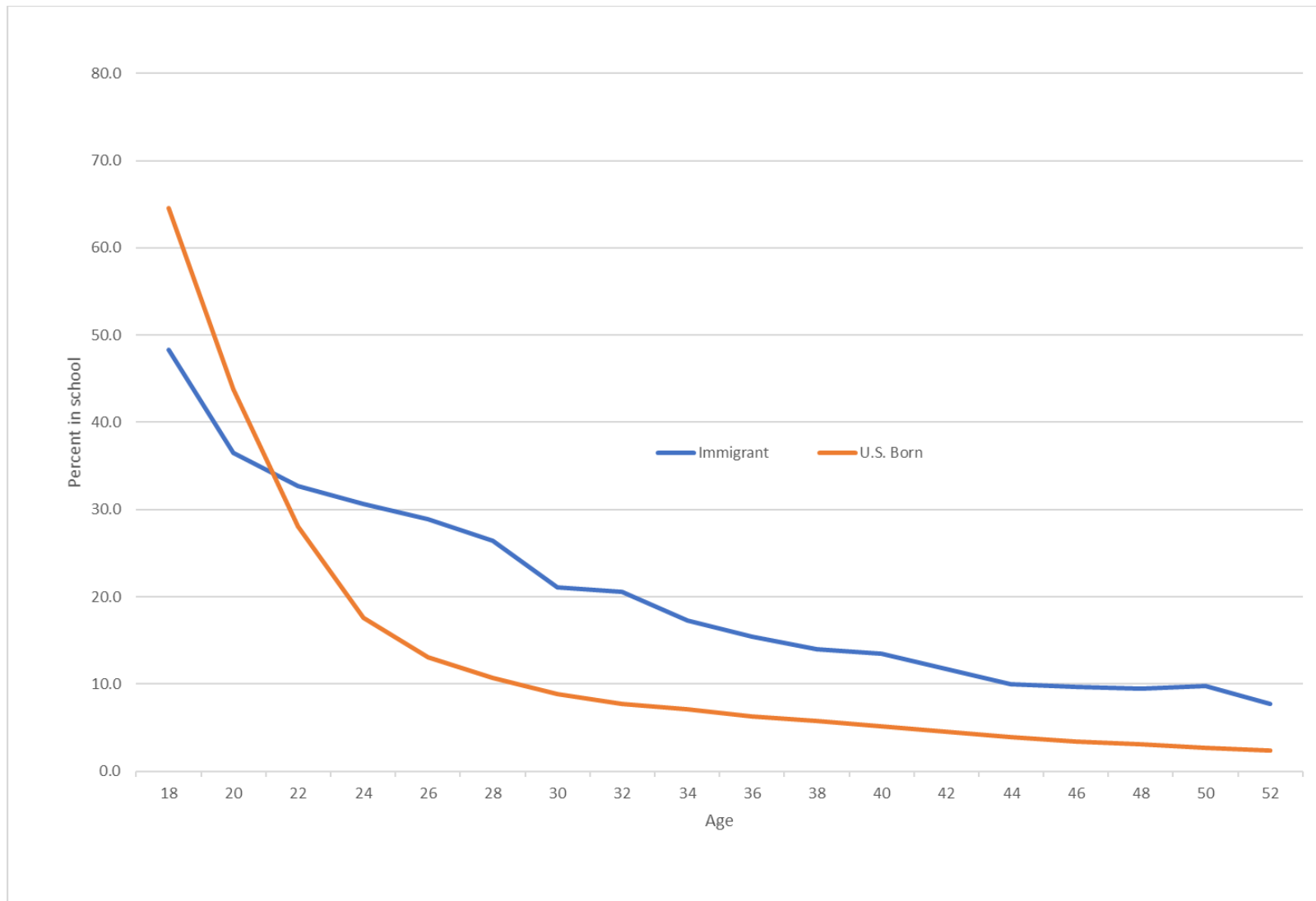


Figure 4: Percent in School by Age: Immigrants 1 to 5 Years After Entry and U.S.-born men. Estimates based on 2018 ACS

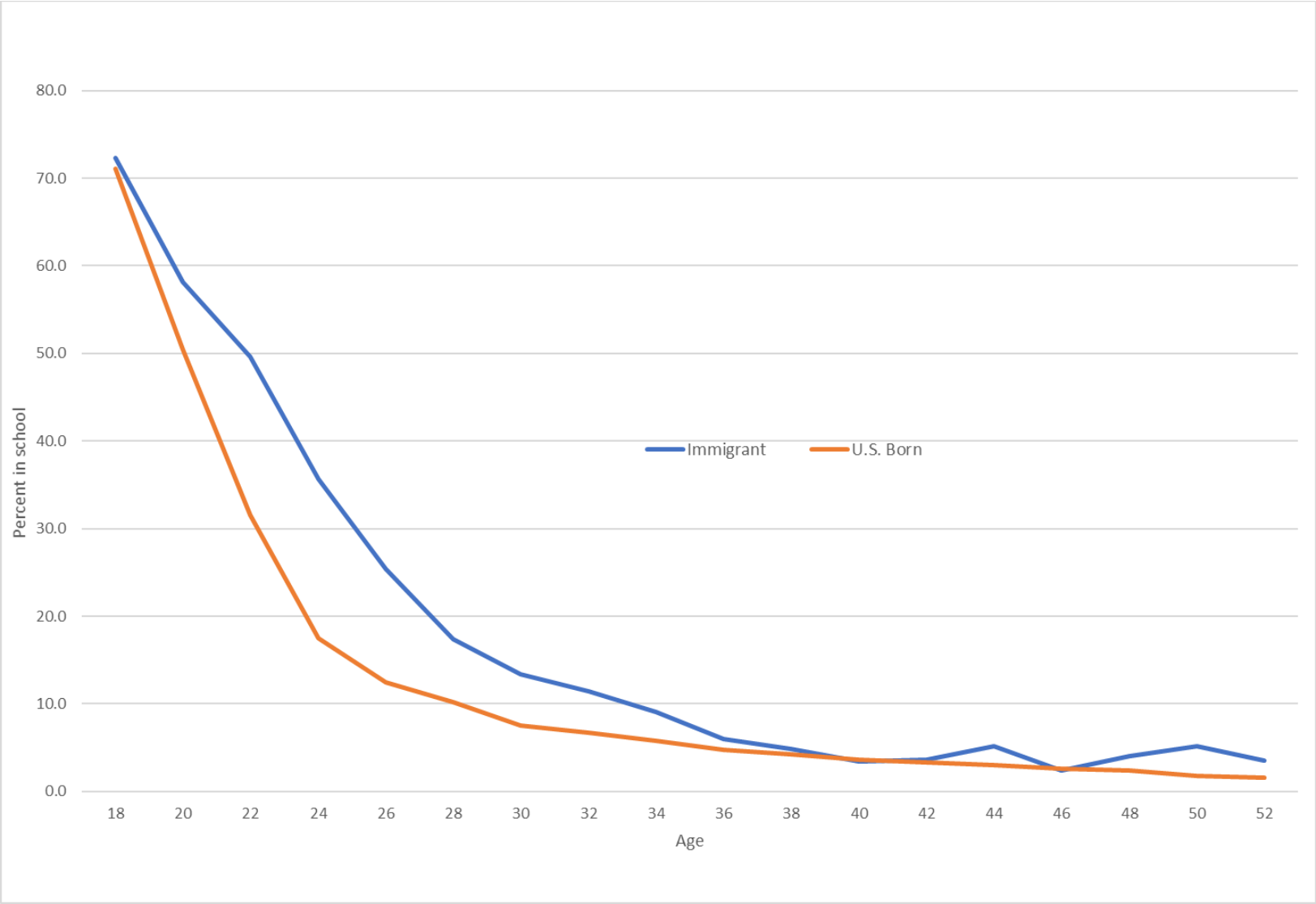
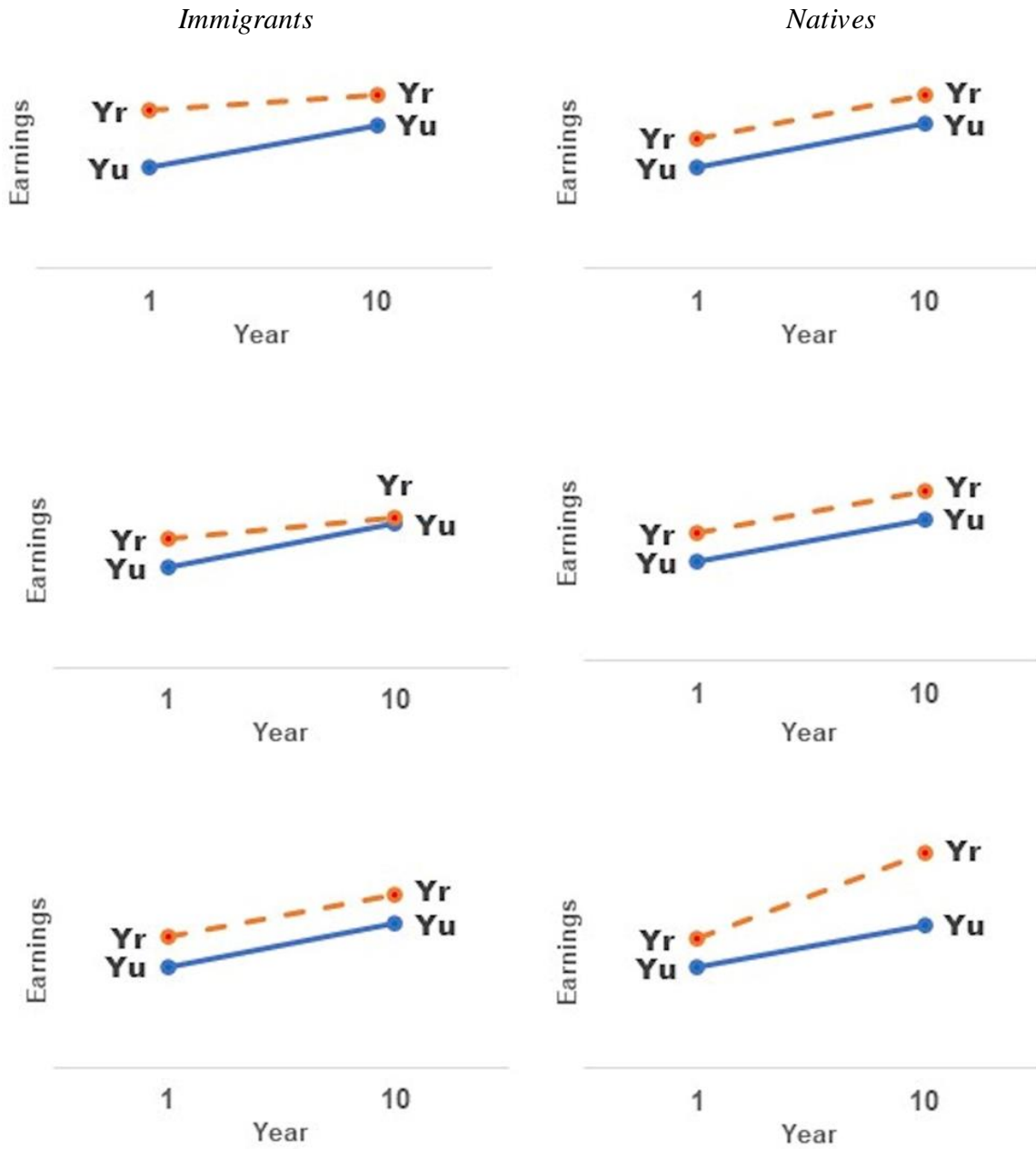


Figure 5: How Excluding Students and Zero Earners from the Sample Could Bias Estimates of Immigrant Relative Earnings Growth: Three Hypothetical Scenarios



Notes: Yu are earnings estimated on unrestricted samples; Yr are earnings estimated on restricted samples. The blue solid lines denote hypothetical earnings trajectories based on unrestricted samples; the orange dashed lines represent earnings trajectories based on restricted samples.

Figure 6: The Ratio of Foreign-born to Native-born Median Weekly Earnings Measured at One to Five Years after Migration and Ten Years Later: No Sample Restrictions (solid line) versus All Sample Restrictions (dashed line)

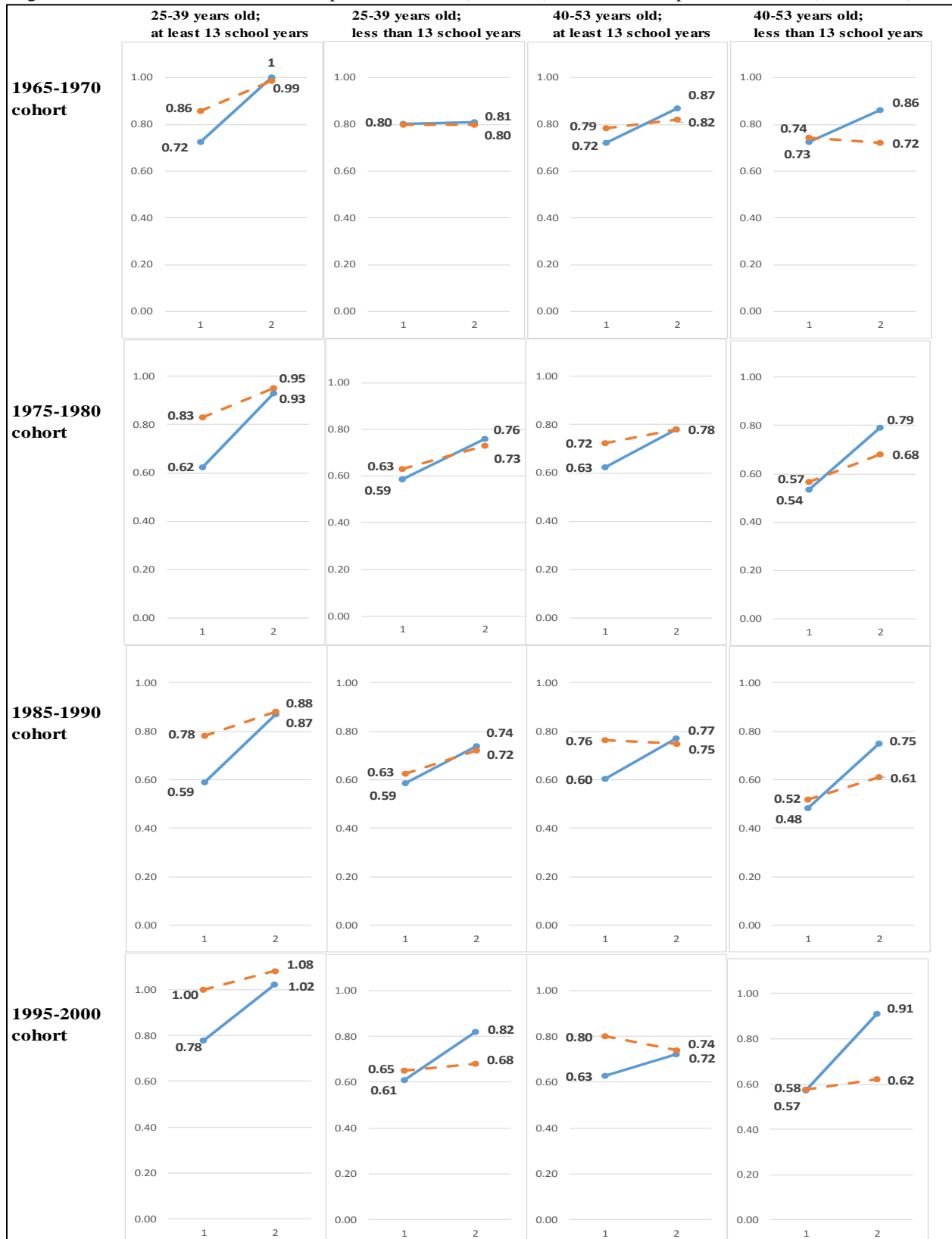


Figure 7: Estimated In-School Rate in 2000 for Immigrants Entering the U.S. in 1993-1998 by Age and Probability of Family Visa

