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ABSTRACT

A Historical Note on the Assimilation Rates of Foreign-Born Men and Women in the U.S.*

Fueling debates about the "quality" of immigrants from economically developing countries, empirical studies based on a well-respected methodology conclude that post-1965 immigrant men have low initial earnings and sluggish earnings growth. This methodology is based on flawed assumptions (Duleep, Liu, and Regets, 2022). Removing these assumptions reveals high earnings growth for post-1965 immigrant men in accordance with the Immigrant Human Capital Investment Model (Duleep and Regets, 1999). A similar story emerges for immigrant women, contradicting the Family Investment Hypothesis first put forth by Long (1980) and Duleep and Sanders (1993). It appears a pre-1965/post-1965 transition occurred in the earnings profiles of U.S. immigrants, from earnings resembling those of U.S. natives to low initial earnings but much higher earnings growth than their U.S.-born statistical twins. The transition underlies the overtime success story of immigrant families from economically developing countries (Duleep, Regets, Sanders, and Wunnava, 2021); the high earnings growth reflects human capital investment that invigorates the economy (Duleep, Jaeger, and McHenry, 2018; Green, 1999, Green and Worswick, 2012).

JEL Classification: J15, J16, J24, J31, C1

Keywords: immigrant earning growth, human capital investment, skill transferability, immigrant quality, sample restrictions, family investment hypothesis, nonparametric estimation

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A Historical Note on the Assimilation Rates of Foreign-Born Men and Women in the U.S.

I. Background

The U.S. 1924 Immigration Act favored immigrants from Western Europe and discriminated against immigrants from economically developing countries (Reimers, 2023).[1] Forty years later, the 1965 Immigration and Nationality Act abolished the 1924 country-of-origin preferences and emphasized family reunification, while also adding an employment-based admission policy. Immigration from economically developing countries soared (Figure 1), and the education- and age-adjusted entry earnings of immigrant men, relative to U.S. natives, plummeted. Table 1 and Figure 2 contrast the entry earnings of immigrants from economically developing versus developed countries.

Table 1: Median Entry Earnings in 1989 of Immigrant Men, Aged 25-54, Who Entered the United States Between 1985 and 1990 as a Percent of the Earnings of U.S.-Born Men, by Immigrant Region of Origin

	All	25-39 years old; 1-12 years of school	25-39 years old; more than 12 years of school	40-54 years old; 1-12 years of school	40-54 years old; more than 12 years of school
All immigrants	41%	53	48	38	50
Asia	44	59	43	32	44
Central/South America	36	51	45	38	40
Western Europe	101	115	93	84	137

Source: Estimates based on a 6% microdata sample created by combining and reweighting the 1990 Census of Population 5% and 1% Public Use samples. Native born are persons born in the U.S.; foreign born are defined as persons born outside of the U.S. excluding those with U.S. parents. The sample includes everyone including students, the self-employed, and persons with zero earnings.

[1] Separate provisions banned Asian immigration (Duleep et al. 2021, Chapter 2). Western Hemisphere immigration continued to be unrestricted, but the same 1924 Act created the Border Patrol and different types of restrictions upon legal Mexican immigration later in the decade.

Figure 1: Over Time National Origin Composition of Legal Immigrants

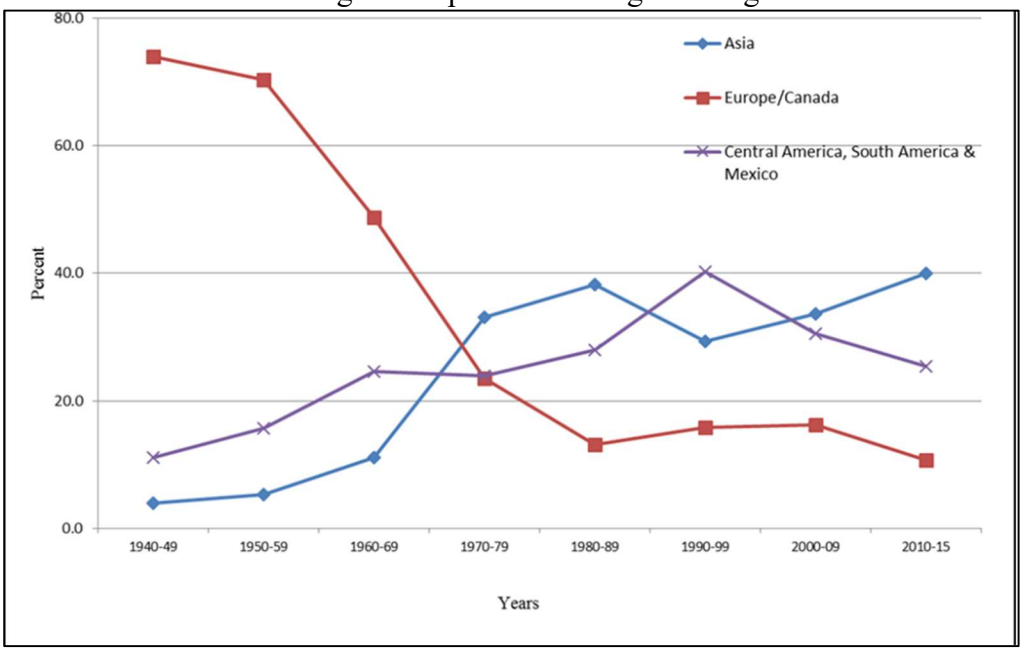
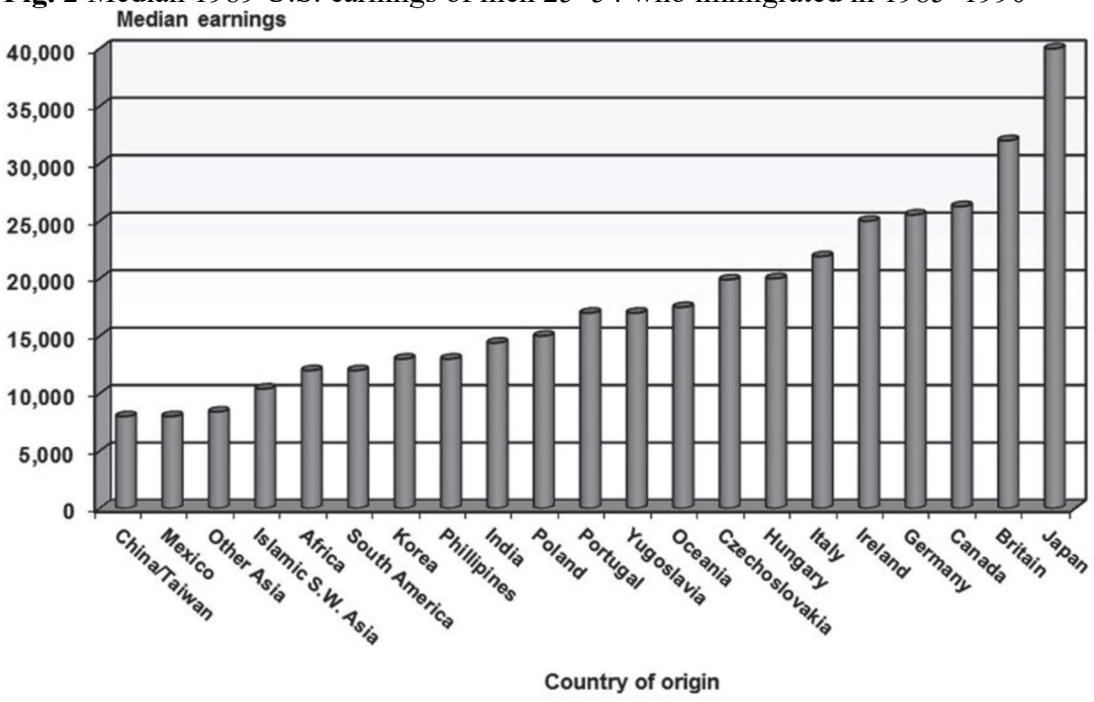


Fig. 2 Median 1989 U.S. earnings of men 25–54 who immigrated in 1985–1990



Source: Estimates based on the 1990 Census of Population 5% and 1% public-use samples. Notes: The foreign born are defined as persons born outside of the United States excluding those with U.S. parents

Among economists, the most famous hypothesis for the post-1965 decline in immigrant entry earnings for men is that immigrant "quality" declined. Published in the *American Economic Review*, Borjas (1987) notes that accompanying the change in immigrant admissions, more U.S. immigration came from countries with high levels of income inequality. Theoretically, individuals at the bottom of their home country's age and education-adjusted income distribution, hence "quality" spectrum, have the most to gain by migrating to the U.S.; with the increased income inequality, the quality of U.S. immigrants should have decreased. Measuring quality by the difference between immigrants' initial earnings and the earnings of U.S. natives of similar age and education, Borjas (1987, 1992, 2015) documents a profound post-1965 decrease in immigrant quality, worsening with time, and accompanied by low earnings growth for immigrant men.

Chiswick (1978, 1979) proposes an alternative hypothesis: The post-1965 decline in immigrant entry earnings reflects the lower transferability to the U.S. of the skills learned growing up and working in economically developing countries. In numerous papers, Chiswick also emphasizes the importance of English proficiency to skill transferability.

Yet, skills learned in developing countries may easily transfer to the U.S. Mathematical skills acquired at *given* levels of schooling in developing countries can exceed those acquired in U.S. schools (Rivera-Batiz and Sechzer 1991). Moreover, immigrants from some economically developing countries with low initial earnings in the U.S. are more English proficient than immigrants from some West European countries with high initial earnings. Japanese immigrants have low English proficiency but high initial earnings (Duleep, Regets, Sanders and Wunnava, 2021).

Building on Chiswick's insights and analyses, we propose a more general hypothesis for the post-1965 decline in immigrant entry earnings that accommodates these disparities and explains why immigrants from economically developing countries (and refugees) have lower skill transferability, hence lower initial earnings, than immigrants from economically developed countries. In countries with opportunities and conditions resembling those in the U.S., few will migrate if it involves initial earnings loss and the need for substantial investment in new human capital. In contrast, people who face constraints in their home countries will migrate even if it requires pursuing new training and education, changing careers or, in a word, starting all over. Compared with people in developed countries, individuals in developing countries face more constraints such as war, gang violence, repressive governments, discrimination, limited professional opportunities, less capitalized businesses and research facilities, inflexible labor markets, restricted or non-existent opportunities for adult education or for changing professions, rigid social structures, and limited opportunities for their children. With such constraints, people in developing countries face not just lower earnings than statistically similar individuals in economically developed countries, but fewer opportunities for growth.

According to the Immigrant Human Capital Investment (IHCI) model developed by Duleep and Regets (2002, 1999, 1992) lower skill transferability creates a lower opportunity cost for human capital investment and a higher return to such investment. The higher return to new human capital investment stems from both the utility of otherwise untransferable human capital in learning new skills, and the complementarity of new human capital with these older skills. To

the extent that immigrants from economically developing countries have lower initial skill transferability, the IHCI model predicts low entry earnings and high earnings growth.[2]

These three hypotheses—differences in immigrant quality, skill transferability, and the IHCI model—were developed to explain the earnings of immigrant men. A fourth hypothesis—the Family Investment Hypothesis (FIH)—seeks to illuminate the earnings of immigrant women.

As first articulated by Long (1980) and Duleep and Sanders (1993), the FIH suggests that immigrant women finance the human-capital investment of husbands who lack transferable skills to their new country. Initially, when their husbands' U.S.-specific skills are lowest and their human capital investment most intense, immigrant women are more likely to work, work more hours, and forego human capital investment in favor of jobs with higher wages. As their husbands' host-country human capital grows, the propensity of immigrant women to work decreases. Relative to U.S. natives of similar age and education, their initial earnings are higher than would otherwise be the case, and their earnings growth (by foregoing human capital investment and eventually working less) is lower. The FIH model predicts immigrant women will have low earnings growth (versus U.S.-born women) when immigrant men have high earnings growth (versus U.S.-born men).

Keeping in mind the various predictions about immigrant earnings growth, we explore historical trends with data described in Part II. In contrast to the typical regression approach for measuring immigrant earnings growth, we pursue a nonparametric methodology, described in Part III. With this methodology, Parts IV and V estimate historical trends in the earnings growth

[2] If these immigrants were negatively selected and of lower quality, they would likely have less incentive to invest in U.S.-specific skills as such learning might come harder and have a lesser return. Alternatively, immigrants may be positively selected since the act of migration requires at least a modicum of motivation and personal organization. This would, however, apply to immigrants from all source countries and all admission visa types. The test of the IHCI model comes from connecting differences in immigrant behavior to likely differences in degrees of skill transferability.

of immigrant men and women, respectively. We conclude, citing issues for further research and two natural experiments.

II. The 1994 CPS-SSA Matched Data

In January 1994, the Current Population Survey (CPS) began to identify immigrants in each monthly survey. We use longitudinal Social Security Administration (SSA) earnings data matched to the 1994 March CPS to track the annual earnings of working-age foreign- and native-born men and women.[3] The longitudinal individual earnings data circumvent potential cohort biases associated with cross-sectional analyses, biases due to immigrant emigration, and other factors that potentially affect both cohort and cross-sectional analyses.

We identify immigrants as persons born abroad of non-U.S. parents. An important aspect of our study is the historical nature of the Social Security earnings data. This permits us to separately follow numerous year-of-entry immigrant cohorts over time. For each cohort, immigrant earnings are first measured in the year following a CPS defined year-of-immigration period. We only follow individuals (whether U.S. or foreign-born) who are at least 25 years old in the initial year of earnings measurement, and no more than 60 in the final year of earnings measurement.

The samples we track do not change because of emigration—the earnings of the same people are measured at the beginning and end of each year-of-entry, cohort-specific analysis. Nevertheless, who is in these samples is affected by emigration. We observe immigrants who were in the U.S. in 1994 (as evidenced by their response to the 1994 CPS) but not immigrants who immigrated before 1994 and emigrated prior to the 1994 CPS. Our study provides

[3] Refer to Duleep and Dowhan (2002) for comprehensive detailed information about these data.

information on the earnings profiles of persons who immigrated in the past and stayed. This statement is particularly true for the pre-1984 cohorts since emigration declines steeply with time in the U.S., with most occurring in the first ten years following immigration (Warren and Peck, 1980; Warren and Kraly, 1985; Duleep, 1994). Thus, our ten-year earnings-growth findings for the 1960-1983 cohorts present accurate earnings profiles for immigrant stayers. As the year of immigration approaches 1994, the cohorts will increasingly include persons who emigrate, and this affects the earnings profiles to an unknown degree. Our theoretical perspective is that permanence increases the propensity to invest, hence earnings growth (Duleep and Regets, 1999); were it possible to limit the more recent cohorts to stayers, the measured earnings growth might be higher.

III. Our Methodology versus Regression Approaches for Estimating Immigrant Earnings Growth

The typical regression approach underestimates the earnings growth of immigrants who start their U.S. journeys at low earnings relative to U.S. natives of similar age and education. This occurs for two reasons:

(1) The fixed-cohort-effect regression specification generally used by economists ignores a pronounced inverse relationship between entry earning and earnings growth.

(2) Most regression specifications in economics use ln earnings as the dependent variable thereby excluding zero earners and creating inconsistent sample universes over time.

1. *The fixed-cohort-effect methodology*

Whether following year-of-entry immigrant cohorts across cross-sections from multiple years or individuals with longitudinal data, economists typically pool immigrants who have entered the host country in different years and estimate a variant of the following stylized model:

$$\ln(y_i) = X'\beta + \alpha \cdot F \cdot YSM + \gamma' F \cdot C_j + \varepsilon_i \quad (1)$$

where y_i denotes the earnings of immigrant i; X is a vector of variables measuring education and experience, and β the corresponding coefficients; $F = 1$ if the individual is an immigrant or 0 if the individual is a U.S. native; YSM measures years since migration. C_j is a set of 0-1 variables representing each year-of-immigration category, j. The coefficients on C_j measure how high initial earnings are for each cohort j; the coefficient on YSM measures earnings growth.

Multiple studies document a systematic inverse relationship between entry earnings and earnings growth.[4] Yet, the fixed-cohort-effect methodology (taught in all economic textbooks) only lets the initial earnings of each cohort change. It assumes that immigrant entry earnings and earnings growth (controlling for age and education) are not systematically related. Averaging earnings growth rates across year-of-entry cohorts hides earnings-growth changes that occur when entry earnings change. Moreover, it understates the earnings growth of immigrants who start with relatively low earnings while overstating the earnings growth of immigrants who start with relatively high earnings.[5]

Following individuals over time, versus cohorts, solves many problems. Nevertheless, the fixed-cohort-effect methodology dominates studies by economists even when researchers use longitudinal data on individuals. Important exceptions among economists include Green and Worswick (2012) and Lin (2013). Using a multivariate analysis that models intercepts and slopes separately, sociologists Hall and Farkas (2008) find significant wage growth for poorly educated

[4] Empirical evidence following cohorts or individuals includes Akresh (2007), Demombynes (2002), DeSilva (1997), Duleep and Dowhan (2002), Duleep, et al. (2018), Duleep et al. (2021), Duleep and Regets (1992, 1994, 1996a, b; 1997; 1999; 2002), Green and Worswick (2012), Hall and Farkas (2008), Jasso and Rosenzweig (1990a, 1995), Lalonde and Topel (1991, 1997), Schoeni (1997), and Villarreal and Tamborini (2018). Lin (2013) and Duleep et al. (2021) show this for immigrants from specific countries.

[5] Duleep et al. (2022) illustrate this for all-immigrants and Duleep et al. (2021) for country-specific groups. Also refer to Jasso and Rosenzweig (1990b).

immigrants, casting doubt on a segmented labor market theory prediction that low-skill immigrants are permanently consigned to dead-end jobs with no wage appreciation.

In estimating the earnings profiles of immigrants, the simplest and most informative way to let earnings growth change with entry earnings is to examine each year-of-entry cohort separately.

2. *Using ln Earnings as the Dependent Variable*

A staple of labor economics is to use ln earnings for the dependent variable in earnings equations. Economists relate age and education to the logarithm of earnings because using the log allows them to interpret the relationship as a percentage change in earnings for a given change in age or education. An oft forgotten fact, however, is that log of earnings is undefined when earnings are zero. Hence, using ln earnings in any statistical analysis excludes zero earners from the sample. This issue, in addition to ignoring an inverse relationship between entry earnings and earnings growth, underestimates immigrant earnings growth.

Even in cross-sectional regressions using data from a single point in time, excluding zero-earners poses a problem for some analytic purposes since non-earners systematically differ from earners. Nevertheless, while this sample selectivity may bias estimates of the return to education, a sample of "all earners" is at least consistently defined.

A much bigger problem occurs when pooling data across more than one time period. Very different individuals may qualify for the sample in each period. Yet, any estimate of earnings growth from censored pooled data, done by regression or other means, will be based upon the over time difference of these inconsistent samples. Consider one hypothetical individual who is out of the labor market in period 1 because of low earnings potential and is instead in school. If that person works in period 2, they will qualify for the sample in period 2

even if they were ineligible in period 1. Earning growth would be measured, not by looking at the period 2 earnings of those in the sample in period 1, but by comparison of period 1 earnings to the period 2 earnings of anyone qualifying for the period 2 sample. Many such scenarios can be posited where sample restrictions lead to people with very different earnings histories included in different periods of pooled data. In each case this will lead to biased estimates of earning growth.

Although biases from sample restrictions causing inconsistent sample universes across periods will bias the earnings growth estimate for any group, the problem becomes much worse when comparing the earnings growth of two groups: immigrant earnings are often standardized by the earnings of natives in the same data. Excluding zero earners,[6] as immigrants and natives are followed across censuses over say ten-year periods, U.S. natives become an increasingly select group as the cohort ages and lower wage individuals leave the labor force, a phenomenon that has worsened for natives over time. In comparison, immigrant labor force participation remains high at older ages.

The estimated low relative earnings growth of immigrant men for the post-1970 cohorts (reported in all of the studies of Borjas), and its apparent worsening in recent years (Borjas, 2015) occurs because of the differential native/immigrant selection bias from sample restrictions, not because immigrant earnings growth is lower than that of U.S. natives or has worsened. The exclusion of zero earners is the main reason why all of the empirical analyses of Borjas show low earnings growth for immigrant men relative to natives. Using the exact same census data

[6] The same type of issue occurs with many types of sample restriction, such as excluding the self-employed. But the zero-earner exclusion is pervasive since it occurs routinely by the common practice of using ln (earnings) as the dependent variable.

sans sample restrictions reveals high earnings growth for immigrant men relative to natives (Duleep et al. (2022), Duleep and Regets (1992, 2002), Duleep et al. (2021)).

A simple solution to avoid a differential sample selection between immigrants and natives when following cohorts is not to impose *any* sample selections. This eliminates the problem and incorporates labor force participation into measures of immigrant economic assimilation. Duleep, Liu, and Regets (2022) show that the effect of imposing sample selections is enormous, changing the high earnings growth that we find for the post-1965 immigrants relative to U.S. natives to low earnings growth. It accounts for the low earnings growth measured by Borjas in all of his works that suggest the quality of U.S. immigrant men fell following the 1965 Immigration Act.

Most often this type of analysis is done between different years of census data. With pseudo cohorts, it is impossible to know if an individual being included in the sample for one period would have qualified in the other periods being pooled. However, even with longitudinal data on individuals, where one can require an individual to qualify in each period, it is not clear that analysts do this: use of the natural log of earnings will still reject an individual from one period's data without removing them from others.[7]

In longitudinal data this differential sample selection problem can be avoided by linking individual earnings at one point in time to later earnings for the same individual and measuring earnings growth by the difference between the two points. Duleep and Regets (1997) and Duleep and Dowhan (2002) do this and show high earnings growth for immigrant men relative to natives. This, the simplest and most straightforward approach, avoids selection bias from sample

[7] Forcing longitudinal data into a functional form developed for regressions on pseudo cohorts also misses the opportunity to measure actual earnings growth over the period for each specific individual and directly relate that to individual characteristics.

restrictions and from emigration. Nevertheless, it is not commonly pursued despite the problematic effects of age-related changes in immigrant/native sample selections on the measurement of immigrant earnings growth relative to natives, as shown in Duleep, Liu, and Regets (2022).

Avoiding these problems

Problems with fixed cohort-effect functional forms and with sample restrictions imposed by using the log of earnings can be avoided using simple nonparametric models. Analytic tradeoffs are implicit in any model choice. In this case, nonparametric models make sense because of the statistical problems they solve, and because of the analytic question being asked wherein neither the return to education nor the return to experience is a primary focus.

With large foreign-born cohort-specific sample sizes, an attractive nonparametric option for controlling for age/education differences between immigrants and natives is to use cross-tabs—compare immigrants and natives within age/education subsets (Duleep and Regets 1992, 1999, 2002). Constrained by small cohort-specific foreign-born sample sizes we pursue another nonparametric alternative that uses all of the foreign-born observations in each year-of-entry cohort. Specifically, we compare native and immigrant earnings controlling for age and education by reweighting each native-born observation so that the age and education distribution of each native-born cohort matches the corresponding foreign-born cohort's distribution.[8]

To do this, we describe the age-education distribution of each immigrant and native cohort sample, labeling the percent in each age-education cell $f(i,j)$ for the foreign born and $n(i,j)$ for the native born. We then weight each native-born observation in cell i,j by

[8] Kitagawa and Hauser (1973) in their classic study of differential mortality weighted observations to compare groups. Some economists have embraced this approach. See DiNardo et al. (1996) for an elegant extension, and Duleep (1988), Duleep and Regets (1997), Duleep and Dowhan (2002), and Duleep et al. (2021) for applications.

$f(i,j)/n(i,j)$. With this approach we estimate the median earnings of immigrants at their own age-education distributions while exploiting the numerous native-born observations to estimate their median earnings at alternative age-education distributions. This approach controls for immigrant/native age and education differences without assuming how education and age affect immigrant and native earnings, or as DiNardo and Tobias (2001, p. 11) quip, "…without the straitjacket of a specific functional form."

Our analyses of immigrant men and women use this approach to track the annual earnings of nine year-of-entry cohorts of immigrants who report coming to the U.S. in 1960-64, 1965-69, 1970-74, 1975-79, 1980-81, 1982-83, 1984-85, 1986-87, and 1988-89, and compare their earnings with those of U.S. natives. For the analyses presented in this paper, we first describe the age-education distribution of each immigrant and native cohort sample with a 36-element matrix composed of six age and six education categories: the age categories begin with 25-29 year olds, covering 5 years each; the education categories are 8th grade or less, 8-12 years with no high school diploma, high school diploma, some college short of a bachelor's degree, bachelor's degree, and a graduate degree. Each cell $f(i,j)_k$ of each foreign-born cohort k matrix is the percent of the foreign-born cohort's sample in age category i and education category j. Each cell $n(i, j)_k$ of each U.S.-born cohort k matrix is the percent of the U.S.-born cohort's sample in age category i and education category j. For each native-born cohort k, we weight each native-born age i/education j observation by $f(i,j)_k /n(i,j)_k$.

As noted before, we reweight each cohort of the U.S. born with the corresponding foreign-born cohort's age-education distribution rather than the reverse. Given small, cohort-specific, foreign-born sample sizes, this permits estimating the median earnings (or other earnings percentile) of immigrants at their own age-education distributions, while taking

advantage of the plenitude of native-born observations to reliably estimate the median earnings of natives at alternative detailed age-education distributions. Prediction error increases the smaller the sample size and the farther away the forecast is from the sampling experience. Since the sample size for natives exceeds that for immigrants, it makes sense to predict the U.S. native values at the foreign-born schooling and age distribution, rather than the reverse.

With this nonparametric methodology we use the 1994 CPS-SSA Matched data to measure the earnings growth of immigrant men, as reported in Duleep and Dowhan (2002) and discussed in Part IV. We use the same methodology and data to measure the earnings growth of immigrant women as discussed in Part V.

IV. Immigrant Men

In 1965, U.S. immigrant admissions changed from a policy that discriminated against persons from economically developing countries to one that promoted family-sponsored admissions. The 1965 Immigration and Nationality Act also allowed 20 percent of numerically restricted visas for employer-sponsored immigrants.

According to our analyses of the 1994 CPS-SSA Matched data, the earnings profiles of men coming to the U.S. in 1960-64 and in 1965-69 (before and after the 1965 Immigration and Nationality Act) resemble the earnings profiles of U.S. natives of similar age and education. Then, starting with the 1970-74 cohort, the entry earnings of immigrant men fell relative to their U.S.-born statistical twins and their earnings growth grew. Indeed, earnings growth exceeding that of U.S. natives characterizes all male immigrant cohorts beginning with the 1970-74 cohort. Similar results emerge using census data to follow cohorts that do not exclude zero earners (Duleep and Regets (2002).

The entry-earnings decline of immigrant men likely occurred because of a decline in the proportion of immigrants with transferable skills to the U.S. accompanying greater immigration from less-developed countries. But, since immigration policy changed in 1965, why did initial earnings not drop until the 1970-74 cohort? Two factors may explain this.

By 1965, the 1924 Immigration Act had existed for over four decades. Potential migrants from previously excluded economically developing countries were unlikely to have U.S. family members to sponsor them via the 1965 family admission provisions. Unable to migrate via family ties, migrants with highly transferable skills in the 1965-69 cohort from economically developing countries likely entered the U.S. via the employment categories, and had initial earnings resembling those of U.S. natives. Once established, their relatives, with less transferable skills, could immigrate through family ties.

Using different datasets, Jasso and Rosenzweig (1995), Duleep and Regets (1996a, b), and DeSilva (1997) find that although family-based immigrants start their host-country lives with lower earnings than their employment-based statistical twins, they have higher earnings growth. This narrative likely explains why the earnings of immigrant men in the 1965-1969 entry cohort resembled the earnings of U.S. natives and why—starting with the 1970-1974 cohort—immigrant entry earnings fell relative to natives' earnings but earnings growth grew.

V. Immigrant Women

The Family Investment Hypothesis (FIH) predicts immigrant women will have low earnings growth (relative to U.S.-born women) when immigrant men (investing in human capital with the assistance of their spouses) have high earnings growth. The three panels of Figure 3 show, for each year-of-entry cohort, the median annual earnings of immigrant women in a

specific year, divided by the median annual earnings of U.S.-born women. The native-born observations are weighted to match the age and schooling distribution of the corresponding immigrant cohort.

Earnings are first measured in the year following a CPS defined year-of-immigration period. For instance, earnings are measured in 1965 for women who came to the U.S. in 1960-64. Earnings are last measured nine years later for cohorts followed 10 years, and in 1992, for cohorts followed less than ten years. Positive earnings are required in the year following the last-year earnings to guard against partial-year earnings from emigration or individuals leaving Social Security covered employment. These same requirements were followed for the analyses of men in Part IV.[9]

Besides the final-year-plus-one requirement, the top panel of Figure 3 requires earnings only in the first and last years, which defines the top panel's lines. The middle panel, based on samples requiring positive earnings each year, measures earnings each year. Using the points of the middle panel the bottom panel estimates, for each cohort, $\frac{Y_{Ft}}{Y_{Nt}} = \alpha + \beta Year_t$, where Y_{Ft} is the median earnings of immigrant women for year t and Y_{Nt} is the median earnings of the corresponding weighted native sample.[10] These estimates, evaluated at first and last years, determine the bottom panel's lines and help compare the top and middle panels.

The top panel suggests that immigrant women went from having earnings profiles resembling those of U.S. natives (of similar age and education) to earning profiles with low initial earnings and high earnings growth, relative to their U.S.-born statistical twins. The

[9] For immigrant men, several ways to identify initial earnings are examined in Duleep and Dowhan (2002).

[10] For each cohort, the estimated coefficients and t-statistics (in parentheses) for $\frac{Y_{Ft}}{Y_{Nt}} = \alpha + \beta Year_t$, are: 1960-64 -0.017 (-2.26); 1965-69 -0.020 (-2.35); 1970-74 0.010 (2.07); 1975-79 0.006 (0.64); 1975-79 0.006 (0.64); 1980-81 0.021 (4.02); 1982-83 0.026 (2.28); 1984-85 0.058 (6.10); 1986-87 0.040 (3.79); and 1988-89 0.149 (2.00).

transformation parallels that for immigrant men except the shift from high-entry-earnings/low-earnings-growth to low-entry-earnings/high-earnings-growth begins with the 1970-74 cohort for men; for women, it begins with the 1980-81 cohort.

The male/female divergent earnings patterns for the 1970-74 and 1975-79 cohorts might suggest a family investment strategy. However, the middle and bottom panels show that women who work every year have the same transition as men: flat earnings profiles (relative to natives) to low initial earnings and high earnings growth starting with the 1970-74 cohort. Figure 4 emphasizes this similarity with cohort-specific ratios of immigrant to native ten-year growth rates for women and men.[11]

Perhaps the high earnings growth of recent cohorts reflects high earnings growth of unmarried immigrant women, while married women—particularly those married to immigrants—have relatively low earnings growth reflecting the FIH. Decreasing marriage rates among immigrants in recent years could yield the overall pattern we observe. If so, then restricting our sample for the most recent cohorts from "all immigrant women" to "married immigrant women" to "married immigrant women with immigrant husbands" should decrease the earnings growth of immigrant women relative to natives.[12] Figure 5 shows this is not the case.

VI. Caveats

A. *Why do we not find stronger support for the Family Investment Hypothesis?*

The Family Investment Hypothesis suggests that the earnings of immigrant women are influenced by their efforts to finance their husbands' human capital investment. Perhaps our

[11] The earnings growth rates shown in Figure 4 are defined as $[(Y_{END} - Y_1)/Y_1]_F / [(Y_{END} - Y_1)/Y_1]_N$ where Y_1 and Y_{END} denote the beginning- and end-year earnings, and F and N denote foreign and native born.

[12] For our most recent cohorts, we assume that women who report to the CPS ever having been married in 1994 were married by the year we first measure each cohort's earnings.

results are not more supportive of the FIH because most women in our study are not married by the year we first measure their earnings?

The CPS asks when immigrants migrated to the U.S. and whether they are married at the time of the survey, but not when they first married. Age of marriage and year of immigration on the 1980 5% Census public use sample permits estimating the percent of immigrant women who were married by or in the first year we measure each cohort's earnings. For women 25-51 years old, 79% of the 1960-64 cohort (those who immigrated in 1960-64) were married before 1966; 82% of the 1965-69 cohort, were married before 1971; 82% of the 1970-74 sample were married before 1976; and 79% of the 1975-79 sample were married before 1981[13] These statistics suggest that most women in the cohorts we study were married by the year we first measure their earnings.

The FIH predictions pertain to immigrant women who marry immigrant men. Perhaps most immigrant women in our study did not marry immigrant men. CPS data from 1994 to 2024 reveal that, of married immigrant women aged 25 to 51 who had been in America for 1 to 5 years, more than 85% had an immigrant spouse.

The FIH assumptions are less likely to apply if either the woman or their spouse entered with an employment visa. There would then be less reason for either spouse to invest in the career of the other since the employment principal's skills are already highly valued in the U.S. labor market. Our data lack information on the type of visa women, or their spouse entered with. However, only about 20% of new green card recipients are employer-sponsored.

Although we fail to find evidence for the FIH, other work underscores the role women play in immigrant family earnings (e.g., Reimers 1984, 1985 and Blau et al. 2011), including

[13] A similar analysis is not possible for the 1980-81, 1982-83, 1984-85, 1986-87, and 1988-89 cohorts because the 1990 census does not ask age of marriage.

family efforts to pursue self-employment (Duleep et al. 2021). In this vein, there are multiple ways our work could be enhanced via variations in marriage patterns and U.S. arrival times. Indeed, one such variation is at the heart of the research of Baker and Benjamin (1997).

B. *Transition Puzzles*

Resonating with the work of Blau et al. (2003) we have stressed the similarity of the earnings profiles of immigrant men and women. There are, however, differences warranting exploration.

An obvious route for further investigation is how children affect the results. A large literature (Mincer and Polachek 1974, 1978, O'Neil and Polachek 1993, Polachek and Siebert 1996, Polachek 2004) relates optimal human capital investment by women to the skill depreciation that occurs during time taken out of the labor force for child rearing. To the extent this pattern is the same for immigrant women, it is an additional reason we might have expected a flatter earnings profile for immigrant women than for immigrant men. On the other hand, difficulties in the transfer of skills for immigrants may in many ways resemble the effect of skill depreciation during labor market absences, yet equally affect both men and women. Whether this helps explain similarities and differences in male and female immigrant earnings calls for further research. It may illuminate why the "transition" from earnings patterns resembling those of U.S. natives to low initial earnings and high earnings growth occurs earlier for men than for women.

C. *Labor Force Restrictions*

Another key issue with policy implications is the effect of labor force restrictions. Our analyses assume that work—type of work and hours of work—are unrestricted. But that is not universally the case. Employment is barred in some of the temporary visa classes and for the unauthorized foreign born. Further, some employment opportunities are for citizens only.

For example, the spouses of those here on temporary work visas can only work under limited circumstances. Spouses of J-1 visa holders (an odd visa class that includes summer carnival workers and biomedical postdocs) can apply for authorization to work (an Employment Authorization Document, or EAD) only if they can demonstrate that their family does not need the money. Spouses of H-1B visa holders (specialty occupations requiring a college degree, or fashion models) can apply for an EAD only after their spouse has an approved application for a permanent visa. Those on student visas are allowed limited off-campus work, but generally their spouses are not allowed to work at all. As confusing as these variations are, their analysis could help elucidate the economic assimilation of immigrants as individuals and as family members.

Of course, whether immigrants have legal status or not affects their employment. We limited our longitudinal earnings analyses to individuals with a CPS-SSA match on Social Security Number, name, age, and gender. The exact match nature of our sample makes it very likely that the earnings we follow are for the correct persons, albeit at the cost of a smaller sample. Reporting errors, name confusions, and other issues cause mismatches between the CPS and Social Security records. These mismatches would affect both the U.S.- and foreign-born samples. In addition, the CPS foreign-born sample includes illegal aliens, who either lack a Social Security number or have a fraudulent one. The percent of the 1994 CPS's foreign-born sample who fail the CPS-SSA exact match test exceeds the corresponding percent for the U.S. born by the percent of the CPS foreign-born sample that is estimated to be illegal (Passel, 1999). This suggests that the foreign-born sample we analyze provides a good representation of the legal foreign-born population.[14]

D. ***Permanence and Naturalization***

[14] For further information on measuring the unauthorized via administrative and survey data matches and mismatches refer to Duleep et al, 2025 and Tamborini et al, 2025.

Empirical evidence suggests that the U.S. permanence of immigrants from economically developing countries exceeds that of immigrants from economically developed countries, and that the decision to invest in human capital and the decision to stay permanently in the U.S. are jointly determined and positively associated (Duleep and Dowhan, 2008; Duleep et al., 2021).

Another issue is naturalization. Those who seek to be naturalized are more permanently attached to the U.S. and naturalization increases opportunities for Federal employment, employment that requires security clearances, and with employers that discriminate on citizenship status (something illegal only since 1986). To become naturalized, individuals generally have to be in the U.S. for 5 years, or two years if married to a U.S. citizen. The interplay between permanence and naturalization could be better understood with information about when individuals apply for naturalization. Although the naturalization date is not included on Census or CPS files, it might be possible to add this information in future matches of Social Security and Census data: USCIS (United States Citizenship and Immigration Services) has provided these data to the Social Security Administration when a person's citizenship status changes.

VII. Concluding Notes and Evidence from Natural Experiments

An evolving picture of the role women play in the economic adjustment of immigrants continues to unfurl. Earlier research, mostly based on cross-sectional data, supported the Family Investment Hypothesis (FIH). More recent evidence[15] including our analyses of year-of-entry cohorts in this paper contradict the FIH.

[15] See, for instance, Adserà and Ferrer (2014, 2016), Blau et al. (2003), Cohen-Goldner et al., (2009), Cortes (2004), Rashid (2004), and Worswick (1996). Also refer to Tamborini, and Villarreal (2024) for information by generation and ethnicity.

When compared with similar U.S.-born workers, the early cohorts of immigrant women and men have relatively flat earnings growth; more recent cohorts have low initial earnings but high earnings growth. These analyses support Blau et al. (2003) who find similar earnings behavior of U.S. immigrant women and men. Our study also illuminates why scholars found some previous support for diverging earnings patterns for immigrant men and women. Yet this disappears when we examine women who work every year.

To understand the earnings of immigrant women, Blau et al. (2003) encourage international comparisons. Such pursuits might be aided by using the "rules" guiding our exploration: Follow immigrant cohorts from the beginning of their host-country years. Follow each year-of-entry cohort separately thus allowing both entry earnings and earnings growth to vary. With large sample sizes consider education and age-defined crosstabs to facilitate international comparisons; with smaller sample sizes consider using weights to control for variables that affect earnings such as education and age. Finally, in studies that do not measure earnings growth by linking individual earnings, consider how results are affected by changes in immigrant and native sample universes caused not only by immigrant emigration but by dramatic immigrant/native differences in the effects of sample restrictions as cohorts age (Duleep, Liu, and Regets, 2022).

Two natural experiments are embedded in the broad swath of time we analyze. With a distinct policy change, the U.S. went from excluding immigrants from economically developing countries to including them. Concomitantly, two programs to admit immigrants were initiated: one where U.S. family members sponsor their relatives, and another where employers sponsor immigrants. Almost by definition, immigrants sponsored by employers have skills that are immediately valued in the U.S. labor market and thus have very high skill-transferability.

Consistent with the IHCI model, both natural experiments find high earnings growth associated with low initial earnings. Relative to immigrants from economically developed countries, immigrants from economically developing countries have low initial earnings and high earnings growth (Duleep, Liu, and Regets, 2018, Duleep et al. 2021). Relative to employment-based immigrants, family admitted immigrants have low initial earnings and high earnings growth (Jasso and Rosenzweig, 1995, DeSilva, 1997, Duleep and Regets, 1996a).

Several studies provide evidence of immigrants with low initial earnings investing in specific forms of human capital, such as schooling or learning English (Akresh, 2007, Duleep and Regets, 2017, Tubergen and Werfhorst 2007, Duleep, Jaeger, and McHenry 2018, and Duleep et al. 2021.)[16] Yet, human capital investment takes myriad forms not easily captured by any one type of human capital investment. One group may invest in English proficiency, another in creating businesses not requiring English; Korean immigrants with poor English fluency have high self-employment rates (Duleep et al. 2021). Human capital investment strategies also vary with time and place and reflect how permanently attached groups are to the U.S. For all of these reasons, the best indicator of human capital investment is earnings growth, which is affected by all forms of human capital investment observable and unobservable.

In conclusion, after 1965, America experienced a historic transformation in the countries of origin of her immigrants. As previously found for men, recent cohorts of U.S. immigrant women with low entry earnings relative to similar U.S. natives have high earnings growth. Though limited to a subset of the immigrant population—individuals who are more permanent (given their presence in the U.S. over a ten-year period)—the high earnings growth of the post-

[16] For a detailed discussion of the difficulties associated with measuring human capital investment with a single form of human capital investment versus earnings growth, representing multiple and varied forms of human capital investment, refer to Part V of Duleep, Liu, and Regets (2022)

1965 immigrants refutes the idea that post-1965 immigrants, who mostly come from economically developing countries, are of lower quality than immigrants from economically developed countries.

Over the time period we have examined, a transition in immigrant earnings profiles occurred. The earnings profiles of U.S. immigrant men and women changed from earnings resembling those of U.S. natives before the 1965 Immigration Act, to lower initial earnings but much higher earnings growth than their U.S.-born statistical twins. This transition underlies the overtime success of immigrant families from economically developing countries (Duleep, Regets, Sanders, and Wunnava, 2021). The high earnings growth signifies human capital investment that invigorates the economy (Duleep, Jaeger, and McHenry, 2018; Green, 1999, Green and Worswick, 2012).

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Table 1: Median Entry Earnings in 1989 of Immigrant Men, Aged 25-54, Who Entered the United States Between 1985 and 1990 as a Percent of the Earnings of U.S.-Born Men, by Immigrant Region of Origin					
	All	25-39 years old; 1-12 years of school	25-39 years old; more than 12 years of school	40-54 years old; 1-12 years of school	40-54 years old; more than 12 years of school
All immigrants	41%	53	48	38	50
Asia	44	59	43	32	44
Central/South America	36	51	45	38	40
Western Europe	101	115	93	84	137
Source: Estimates based on a 6% microdata sample created by combining and reweighting the 1990 Census of Population 5% and 1% Public Use samples. Native born are persons born in the U.S.; foreign born are defined as persons born outside of the U.S. excluding those with U.S. parents. The sample includes everyone including students, the self-employed, and persons with zero earnings.					

Figure 1: Over Time National Origin Composition of Legal Immigrants

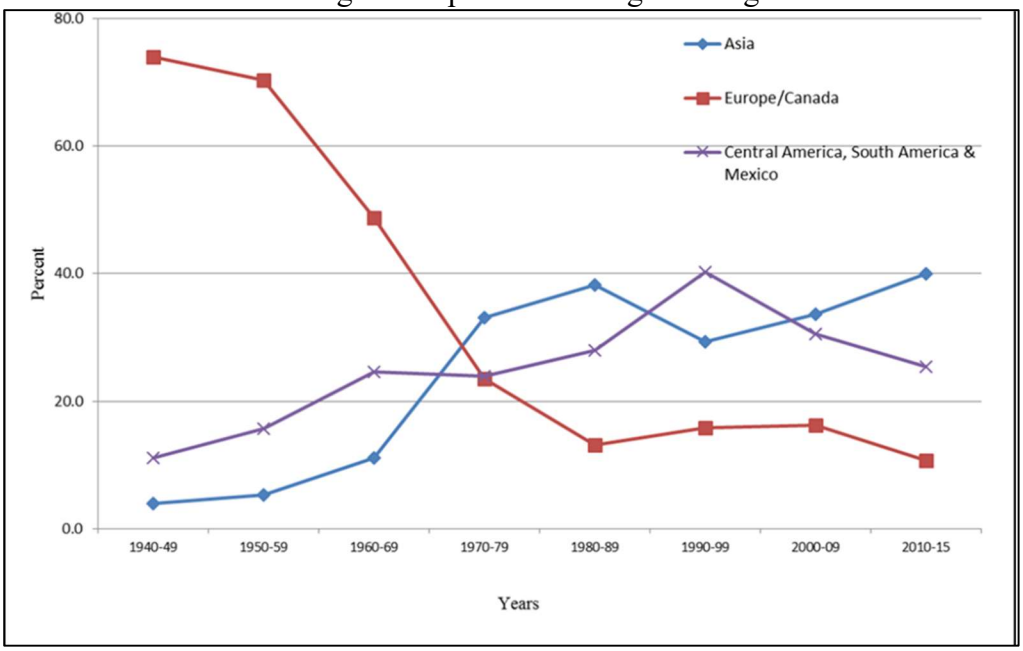
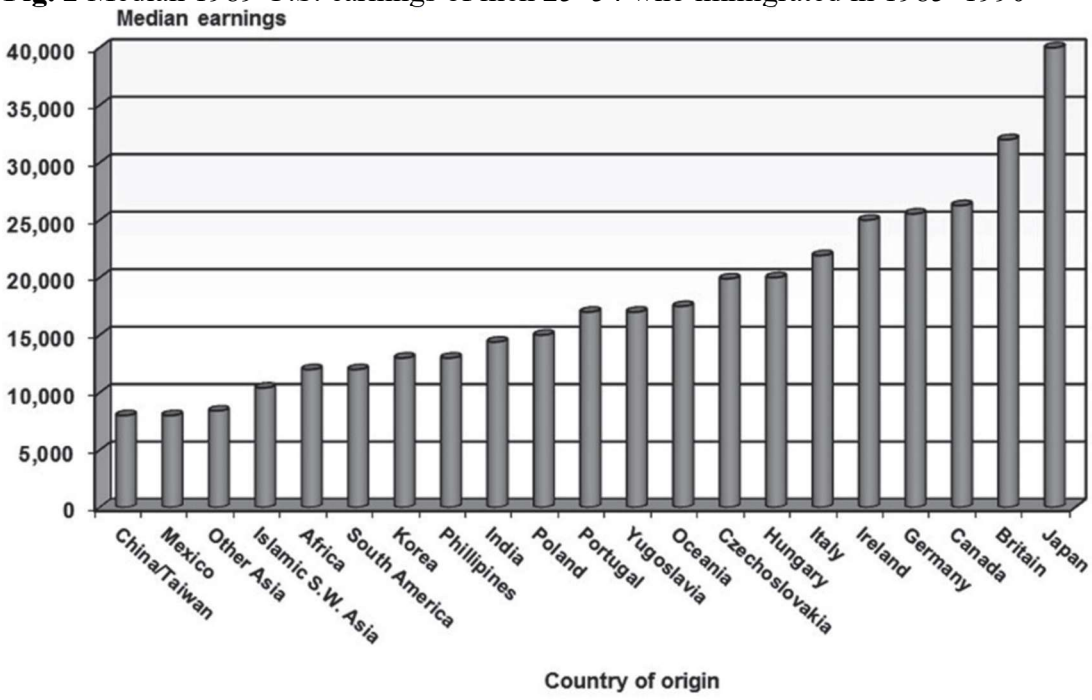
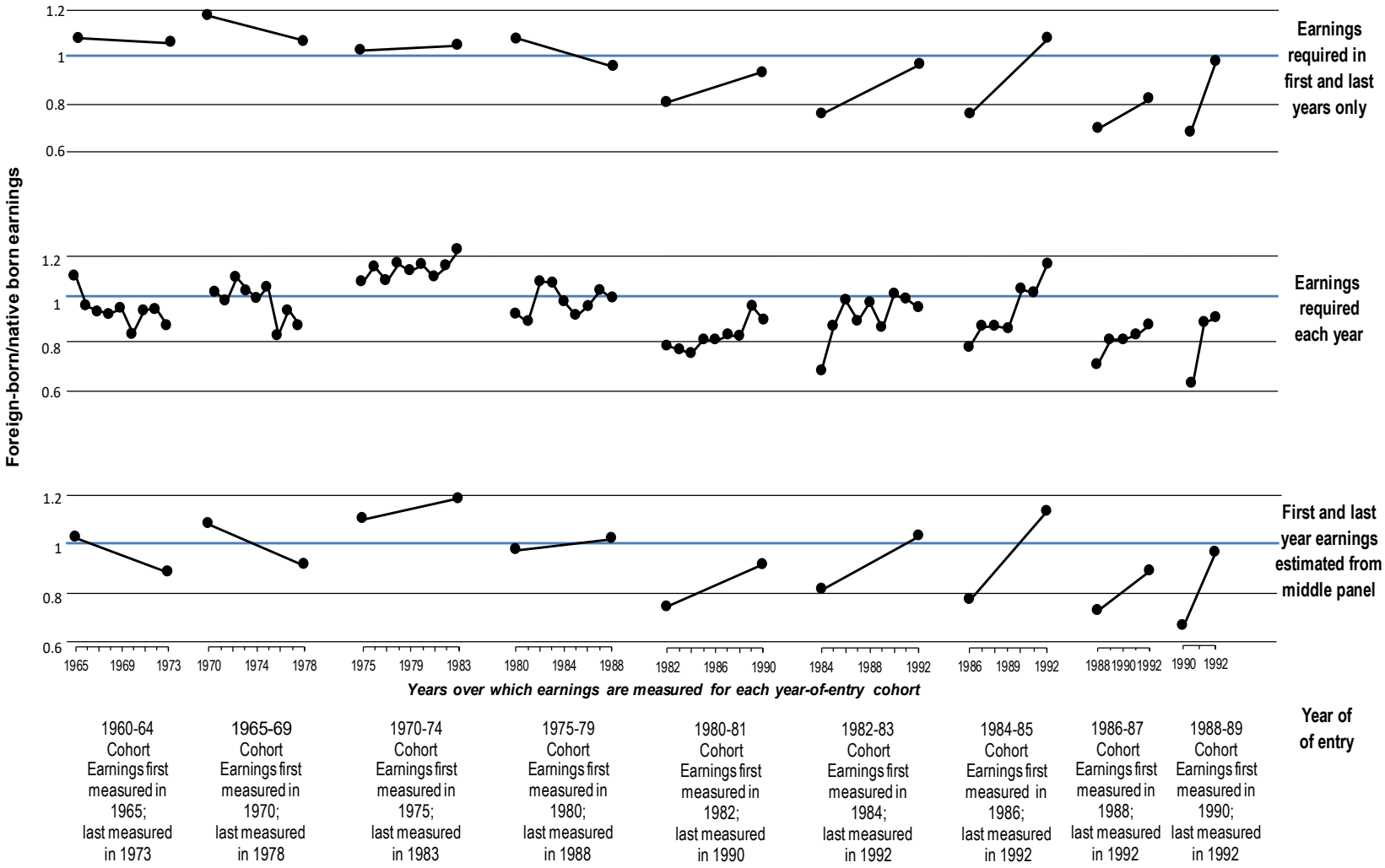


Fig. 2 Median 1989 U.S. earnings of men 25–54 who immigrated in 1985–1990



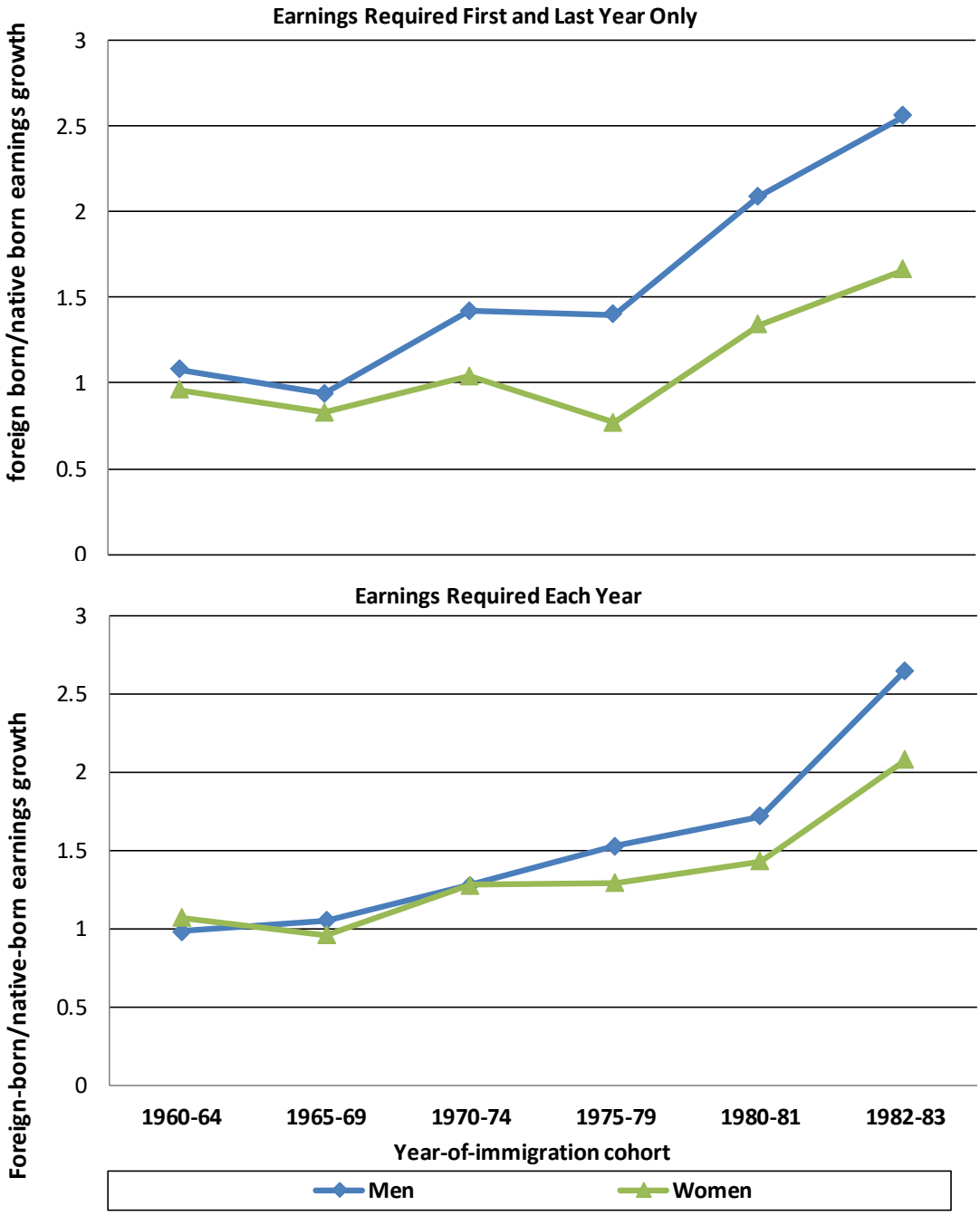
Source: Estimates based on the 1990 Census of Population 5% and 1% public-use samples. Notes: The foreign born are defined as persons born outside of the United States excluding those with U.S. parents

Figure 3: The Annual Earnings of Foreign-Born Women Relative to U.S.-Born Women



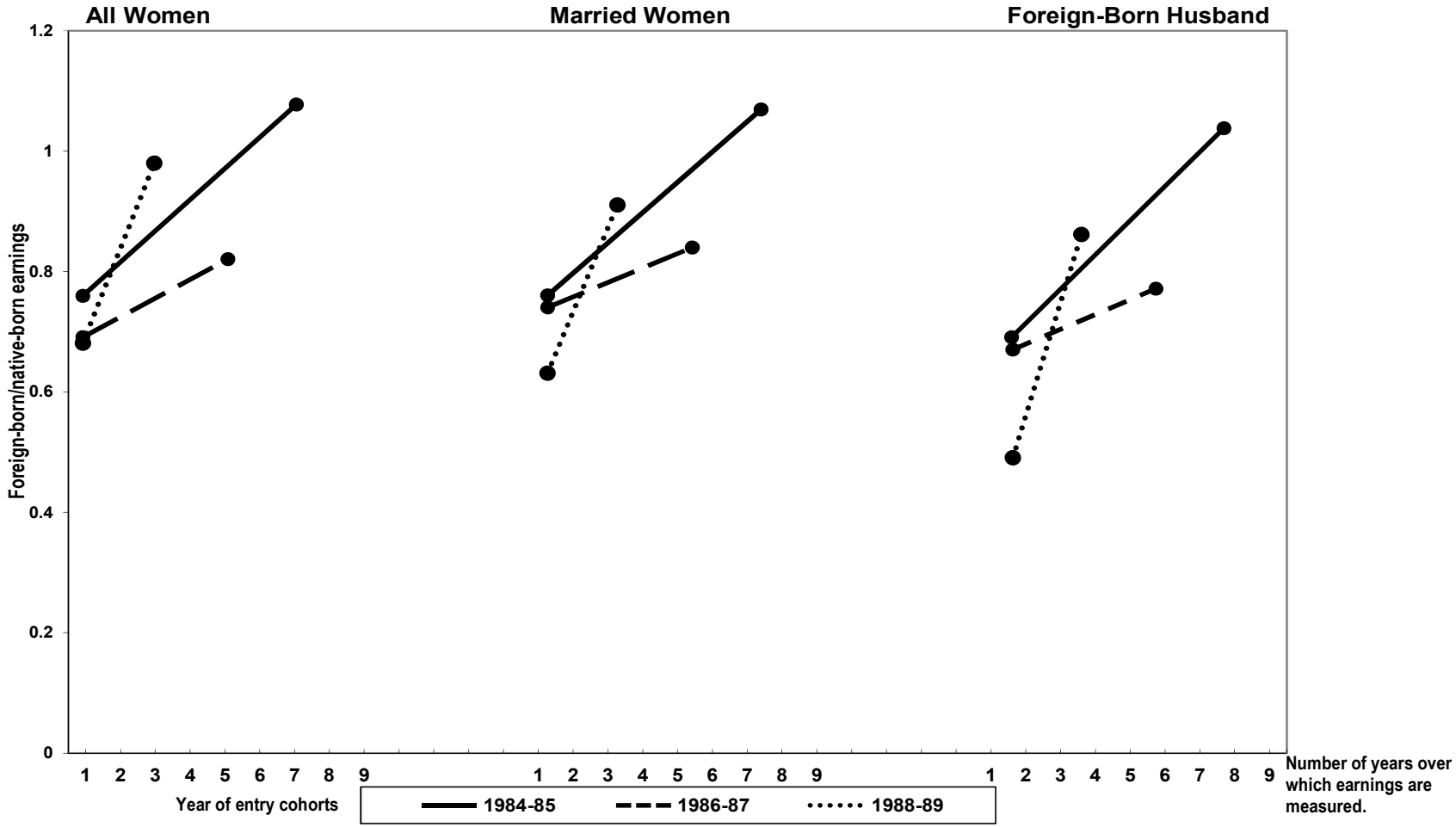
Source: Authors' calculations using the 1994 March Current Population Survey matched to the Social Security Administration's administrative record system. Each dot is the median annual earnings of foreign-born women, in a specific year, divided by the median annual earnings of U.S.-born women, wherein the individual observations in each U.S.-born sample are weighted to match the age and schooling distribution of the corresponding foreign-born cohort.

Figure 4: Cohort-specific Ratios of Foreign-Born to Native-Born Ten-Year Earnings Growth Rates: Women and Men



Source: Authors' calculations using the 1994 March Current Population Survey matched to the Social Security Administration's administrative record system. Each marker is a cohort-specific ratio of foreign-born to native-born ten-year relative growth rates defined as $[(Y_{END} - Y_1)/Y_1]_F / [(Y_{END} - Y_1)/Y_1]_N$ where Y_1 and Y_{END} denote the beginning- and end-year median annual earnings, and F and N denote foreign and native born. Median annual earnings for the U.S. born are estimated on samples weighted to match the age and schooling distribution of the corresponding foreign-born cohort.

Figure 5: The Earnings of Foreign-Born Women Relative to U.S.-Born Women, by Marital Status



Source: Authors' calculations using the 1994 March Current Population Survey matched to the Social Security Administration's administrative record system.
Notes: Each dot is the median annual earnings of foreign-born women, in a specific year, divided by the median annual earnings of U.S.-born women, wherein the individual observations in each U.S.-born sample are weighted to match the age and schooling distribution of the corresponding foreign-born cohort. The samples that underlie these graphs require earnings in the first and last years, only; the annual median earnings, measured at the first and last years, determine the lines. For the 1984-85 cohorts (solid lines), earnings are first measured in 1986 and last measured in 1992. For the 1986-87 cohorts (dashed lines), earnings are first measured in 1988 and last measured in 1992. For the 1988-89 cohorts (dotted lines), earnings are first measured in 1990 and last measured in 1992.