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## Country of Origin, Earnings Convergence, and Human Capital Investment: A New Method for the Analysis of U.S. Immigrant Economic Assimilation

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# **Country of Origin, Earnings Convergence, and Human Capital Investment: A New Method for the Analysis of U.S. Immigrant Economic Assimilation**

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September 2018

# **Country of Origin, Earnings Convergence, and Human Capital Investment: A New Method for the Analysis of U.S. Immigrant Economic Assimilation\***

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September 2018

## **ABSTRACT**

# **Country of Origin, Earnings Convergence, and Human Capital Investment: A New Method for the Analysis of U.S. Immigrant Economic Assimilation\***

The initial earnings of U.S. immigrants vary enormously by country of origin. Via three interrelated analyses, we show earnings convergence across source countries with time in the United States. Human-capital theory plausibly explains the inverse relationship between initial earnings and earnings growth rates: the good fit between data and theory suggests that variation in initial skill transferability—not variation in the “quality” of human capital—underlies variation in initial earnings. A new method of testing for emigration bias confirms that selective emigration does not cause the convergence. Functional form and sample selections embedded in most recent analyses of immigrant economic assimilation bias downwards the earnings growth of post-1965 U.S. immigrants. When both functional-form and sample-selection constraints are lifted, a dramatically different picture of the economic assimilation of U.S. immigrants emerges.

JEL Classification: J1, J2, J3, C1

Keywords: immigrant economic assimilation, human capital investment, country of origin, immigrant earnings convergence, earnings growth, unbiased estimation

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## **Country of Origin, Earnings Convergence, and Human Capital Investment: A New Method for the Analysis of U.S. Immigrant Economic Assimilation**

*Studies that have described immigrant cohorts, assessed the progress of immigrants in the United States, and examined the role of ethnicity in labor market behavior have assigned to country of origin a prominent part. Jasso and Rosenzweig (1986, p. 75)*

### **I. INTRODUCTION**

As the first economist to theoretically model how country of origin might affect immigrant economic assimilation, Chiswick (1978a, 1979) proposed several hypotheses relating country-specific factors (such as English speaking) to the degree to which source-country skills transfer to the host-country labor market. Jasso and Rosenzweig (1986, 1990a) identified source-country attractiveness, economic conditions, and costs of migration as key factors in the effect of country of origin on immigrant earnings. This paper explores how country-of-origin effects on earnings change as immigrant men live in the United States.<sup>1</sup>

Twentieth-century America experienced a sea change in the country-of-origin composition of her immigrants. The demise of a U.S. immigration quota system based on national origin and its replacement in 1965 with a policy emphasizing family reunification opened the gates to a large and increasing flow of immigrants differing in national-origin composition from prior immigrant flows. Once primarily European, U.S. immigration became predominantly Asian and Hispanic.

A precipitous decline in the entry earnings of working-age immigrant men relative to U.S. natives accompanied the source-country transformation and is most apparent since the

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<sup>1</sup> The role of country of origin has been explored in many contexts including its effect on English proficiency (Chiswick and Miller, 1992; Kossoudji, 1988; Rivera-Batiz, 1992), educational attainment (Chiswick and DebBurman, 2004), U.S. location (Jaeger, 2007), labor-market effects (Bohn and Sanders, 2007; Angrist and Kugler, 2003; Pischke and Velling, 1997; Rivera-Batiz and Sechzer, 1991; Gang and Rivera-Batiz, 1994a), women's labor-force behavior (Reimers, 1985), unemployment (DeFreitas, 1991, 1993), immigrant concentrations and businesses (Andersson et al., 2010), attitudes towards immigrants (Gang and Rivera-Batiz, 1994b), selectivity (Jasso and Rosenzweig, 1990b; Bauer et al., 2002), enclaves (Bauer, Epstein and Gang, 2005), employer training (Barrett et al., 2013), welfare use (Hansen and Lofstrom, 2009), and emigration (Jasso and Rosenzweig, 1990a).

1960's when U.S. immigration policy changed.<sup>2</sup> In 1960, working-age immigrant men who entered the United States in 1955-1959 earned on a par with U.S. natives. After 1965, the median initial earnings of immigrant men ages 25-54 as a percent of U.S.-natives' earnings plummeted: in 1970, those entering the United States in 1965-69 earned 65% of natives' earnings; in 1980, the 1975-79 entrants earned 50% of natives' earnings; and, in 1990, the 1985-89 entrants earned 41% of natives' earnings. These differences remain largely intact when immigrants and natives are categorized by age and years of schooling.

Current Population Survey data matched to Social Security longitudinal earnings records tell a similar story. The median earnings of men, ages 25-60, in the 1960-64 immigrant cohort were 100% of native-men's earnings in 1965, 71% in 1970 (for the 1965-69 immigrant cohort), 61% in 1980 (for the 1975-79 cohort), and 54% in 1984 (for the 1982-83 cohort). This decline persists weighting the foreign born and native born to the same age and years-of-schooling distribution (Duleep and Dowhan, 2002).

The coincidental change in immigrants' source-country composition and the decline in immigrants' initial earnings suggests that the former caused the latter. Weighting the 1980 initial earnings of immigrants who entered the United States in 1975-1979 by the pre-1965 country-of-origin immigrant mix nearly eliminates the foreign/native differential; had the pre-1965 source-country mix continued, recently arrived immigrants in 1980 may have earned on a par with their U.S.-born contemporaries, rather than earning half of U.S. natives' earnings.

The importance of the decline in immigrants' initial earnings and whether it hurt, helped, or had no effect on the U.S. economy depends on what those low earnings represent and whether

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<sup>2</sup> The dramatic decline in the initial earnings of U.S. immigrants can be measured with decennial census data. We used the 1980 5% "A" PUMS, the 1970 1% State PUMS based on the 5% questionnaire, and the 1960 1% PUMS (Bureau of the Census, 1983, 1977, and 1975). Although the 1960 census did not collect year-of-immigration information, place of residence in 1955 let us identify immigrants who entered the U.S. between 1955 and 1960.

they persisted, increased, or decreased. The social and economic consequences of low earnings associated with low ability differ profoundly from the social and economic consequences of low earnings associated with high human-capital investment.

We start by presenting the case that the post-1965 decline in immigrants' education- and age-adjusted entry earnings stemmed from decreases in the proportion of U.S. immigrants with skills that immediately transfer to the U.S. labor market (Part II). Building on Chiswick's Immigrant Assimilation Model,<sup>3</sup> we articulate a human-capital-investment perspective that predicts that country-of-origin earnings' effects caused by variations in skill transferability diminish as immigrants live and work in the United States (Part III). A human-capital-investment perspective illuminates two major problems with today's most commonly used econometric method for measuring immigrant earnings growth and suggests new methodological guidelines (Part IV). Following these guidelines, we test for earnings convergence by examining what happened to the earnings growth of U.S. immigrants during the great entry-earnings decline (Part V). Two complementary country-of-origin cohort analyses follow (Part VI). These analyses measure the importance of country of origin with immigrant time in the United States when changes in U.S. immigrant source-country composition, and the accompanying decline in immigrant entry earnings, were most intense. Part VII probes the sensitivity of these results to emigration. Part VIII examines whether the convergence results hold for immigrants who entered the United States after the 1960-1980 period of intense change in source-country composition. Part IX tests the validity of the way we measure skill transferability as well as the assumption that human capital investment underlies earnings growth.

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<sup>3</sup> See Chiswick (1978a, 1979).

## II. BEHIND THE GREAT ENTRY EARNINGS' DECLINE

A plausible explanation for the post-1965 drop in U.S. immigrants' education- and age-adjusted entry earnings is a decline in the proportion of immigrants with skills that immediately transfer to the U.S. labor market: the less source-country skills transfer, the lower the initial earnings of immigrants (Chiswick, 1978a, 1979).<sup>4</sup> Two developments associated with the 1965 immigration law likely spurred a skill-transferability decline in America's immigrants: one, immigration from less-economically developed countries increased and two, changes occurred in "type of admission" for immigrants from specific countries.

### **An Increase in Immigration from Less-Economically-Developed Countries**

The national-origin quota system, in place from the 1920's to 1965, favored immigration from economically-developed countries, while restricting immigration from less-economically-developed countries. When the national-origin system ended, immigration from less-economically-developed countries soared (D. Reimers, 1996).

Two theories link immigrants' skill transferability to the economic development level of the countries they come from. Holding constant levels of human capital (years of schooling and work experience), Chiswick (1978a, 1979, 1986) speculates that the skills of immigrants from economically-developed countries transfer easily to the United States because the educational,

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<sup>4</sup> Another perspective is that the earnings drop represents a decline in immigrant ability. For instance, Piracha et al. (2012) find that recently arrived Australian immigrants who are over-educated for the jobs they hold in Australia were also over-educated for the jobs they held *before* migrating. Rather than reflecting initial problems in transferring source-country skills, they argue that low labor-market ability underlies the education-occupation mismatch. High earnings growth by immigrants with low initial earnings counters this argument. Nevertheless, their model and empirical evidence inspire a broader view of what motivates persons lacking highly transferable skills to migrate. What economists call a skills mismatch in an immigrant's original country might be more comprehensively defined as a constraint. Among those who lack skills that would allow their human capital to transfer easily to another country, individuals who earn below their capabilities in their original countries would be more likely to migrate than would individuals who earn at or above their capabilities. More broadly, people who are constrained from pursuing dreams in their original country, for whatever reason, would be more likely to migrate, even if the migration entailed substantial investments in new human capital in the host country. This perspective could potentially encompass the key variables identified in Jasso and Rosenzweig (1986, 1990a).



industrial, and labor-market systems of these countries resemble those in the United States whereas the schooling and work experiences in less-economically-developed countries are dissimilar. Duleep and Regets suggest U.S. immigrants from less-developed countries have lower skill transferability because the limited opportunities in those countries make it worthwhile for individuals to migrate, even when immigration entails substantial post-migration investments in new skills and credentials;<sup>5</sup> sans U.S. positions that immediately value their source-country skills, their statistical twins in economically-developed countries are less likely to migrate.<sup>6</sup>

Regardless of the underlying etiology, initial earnings of U.S. immigrants do, on average, increase with the economic-development level of their source countries. Immigrants from regions of the world, such as Western Europe, with economic-development levels similar to the United States have initial earnings approaching or exceeding those of comparably educated and experienced U.S. natives; those hailing from the economically developing countries of Asia and Central America have low initial earnings relative to their U.S.-born counterparts (Table 1). Regressing the median 1989 entry earnings of U.S. immigrant men in the 1985-1990 cohort on the country-of-origin per capita gross domestic product reveals that initial U.S. earnings increase \$2,280 for each 10-percentage-point increase in the source-country's per capita GDP.<sup>7</sup> Given this

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<sup>5</sup> This hypothesis accommodates findings (e.g., Rivera-Batiz, 1996) that the quality of schooling in several less economically developed countries exceeds that of the United States. Evidently, both the Chiswick and the Duleep/Regets hypotheses may be at work, varying in their importance by both country and time period.

<sup>6</sup> In countries with opportunities resembling those in the U.S., a minority of individuals will face constraints, and choose to migrate, even when this entails substantial investment in new human capital. As the level of economic development decreases, the proportion of persons facing constraints would likely increase. See note 4.

<sup>7</sup> The  $R^2$  for this regression is .48. The 1987 per capita GDP of each source country is per adult GDP as a percent of the U.S. per adult GDP. The observations on U.S. median earnings for immigrant men and per capita GDP are for the following countries: Argentina, Australia, Bangladesh, Bolivia, Brazil, Canada, Chile, China, Colombia, Costa Rica, Czechoslovakia, Dominican Republic, Ecuador, Egypt, El Salvador, Fiji, France, West Germany, Greece, Guatemala, Guyana, Haiti, Honduras, Hong Kong, Hungary, India, Indonesia, Iran, Ireland, Israel, Italy, Jamaica, Japan, Jordan, The Republic of Korea, Laos, Malaysia, Mexico, Morocco, Myanmar, Netherlands, New Zealand, Nicaragua, Nigeria, Pakistan, Panama, Peru, Philippines, Poland, Portugal, Romania, South Africa, Spain, Sri Lanka, Sweden, Switzerland, Syria, Taiwan, Thailand, Trinidad and Tobago, Turkey, U.S.S.R., United Kingdom, Venezuela, and Yugoslavia. All countries for which we had information on the GDP per adult were included. Median earnings for immigrant men in the 1985-90 cohort from the aforementioned 65 countries were estimated

relationship, it is not surprising that as immigration from less-economically-developed countries increased, the initial earnings of U.S. immigrants decreased.

### **Post-1965 Changes in Type of Admission for Immigrants from Specific Countries**

In addition to an increase in immigration from less-economically developed countries, post-1965 changes occurred in “type of admission” for immigrants from specific countries. The 1965 Immigration and Nationality Act eliminated country-specific quotas that discriminated against particular groups and introduced a system of restricted and non-restricted admission categories greatly favoring immigrants with family members in the United States. It also authorized employment-based immigration: under this law, 20 percent of the numerically restricted visas were allocated to applicants based on their occupational skills.<sup>8</sup> The very nature of employment-based admissions (with employers hiring immigrants with specific occupational skills for specific jobs) insures that these immigrants will have higher skill transferability, hence higher initial U.S. earnings, than family-based immigrants—a prediction confirmed by several analyses.<sup>9</sup>

Because the national-origins legislation had been in place some 40 years when immigration policy changed in 1965, potential migrants from countries that previously faced severe restrictions lacked family members in the United States. Unable to immigrate via family ties, qualified migrants gained U.S. entry via the employment-based admission categories. Once established, their relatives—with less transferable skills—immigrated via the family-based

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using a 6 percent microdata sample created by combining and reweighting the 1990 Census of Population 5% and 1% Public Use samples. The per capita GDP statistics are from Heston and Summers (1991).

<sup>8</sup> The occupational skills (or 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.

<sup>9</sup> Using different data sets, Jasso and Rosenzweig (1995), Duleep and Regets (1996a, b) and DeSilva (1996) 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, the earnings of the two groups converge.

categories. In this way, the proportion of U.S. immigrants—*from the same country*—lacking skills that immediately transfer to the U.S. labor market, grew in the years following 1965. This scenario explains why the age- and education-adjusted entry earnings of immigrants from several countries declined in the post-1965 era. For instance, the 1970 adjusted earnings of working-age Korean men who entered the United States in 1965-1970 were 75% of the earnings of working-age U.S.-born men; the 1990 earnings of the 1985-90 Korean immigrant cohort were 44% the corresponding U.S.-born benchmark.

### III. SKILL TRANSFERABILITY, HUMAN-CAPITAL INVESTMENT, AND EARNINGS CONVERGENCE

Theoretically, immigrant skill transferability and the propensity to invest in human capital are inextricably linked. As Chiswick (1978a, 1979) hypothesized, immigrants entering the United States, or other host country, lack (in varying degrees) the skills that would enable their human capital to be fully valued in the labor market of their new home. To bring to life their source-country human capital, immigrants engage in various forms of human-capital investment such as learning English, pursuing informal and formal schooling and training, and becoming familiar with the host-country's institutions, production methods, and technical terms. As host-country-specific skills or credentials are gained, the labor-market value of immigrants' source-country human capital increases.

Building on Chiswick's Immigrant Assimilation Model, Duleep and Regets (1999, 2002) articulated an Immigrant Human Capital Investment (IHCI) model that formalizes Chiswick's concept of skill transferability to the host-country's labor market and introduces the human-capital-investment implications of two previously ignored aspects of skill transferability. One, by virtue of their lower wages, immigrants whose source-country skills do not fully transfer to the host country's labor market will have a lower opportunity cost of human-capital investment than

natives or immigrants with high skill transferability.<sup>10</sup> Two, source-country human capital that is not valued in the host country's labor market is useful for learning new skills: persons who have learned a set of skills have learned how to learn. Moreover, common elements between old and new skills aid learning.<sup>11</sup> Combined, these skill-transferability features suggest that low-skill-transferability immigrants will be more likely than high-skill-transferability immigrants (or natives) to invest in human capital, *in general*, versus (or in addition to) human-capital investment aimed at restoring the labor-market value of their source-country human capital.<sup>12</sup>

The above considerations—linking low skill transferability to high rates of human-capital investment, hence high rates of earnings growth—predict an inverse relationship between immigrants' (adjusted) entry earnings and earnings growth, hence earnings convergence:<sup>13</sup> across groups, the lower the entry earnings, the higher the earnings growth; over time, as entry earnings fall (rise), earnings growth should rise (fall). To the extent that source-country variation in immigrants' initial earnings reflect variation in initial U.S. skill transferability, a human-capital-investment perspective predicts that source-country effects on immigrant earnings will diminish with time in the United States. This will be true regardless of whether skill-transferability differences across immigrants by source country stem from variation in skills learned growing up and working in the original countries (as suggested by Chiswick), or from an opportunity-driven

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<sup>10</sup> The time they spend learning new skills, instead of applying their current skills to earning, is less costly than it is for natives or for high-skill transferability immigrants, who earn more with the same level of schooling and experience.

<sup>11</sup> For more discussion on this point, refer to Duleep and Regets (1999, 2002).

<sup>12</sup> This may explain why Green (1999) finds a greater propensity of immigrants to change occupations than natives beyond what can be explained by an assimilation effect; the timing of the changes across year-of-entry cohorts suggests that they are in response to an evolving demand for different types of labor-market skills. A topic for further research, along the lines pursued by Green, is what are the effects on a nation's economic productivity of the skill acquisition of immigrants that accompanies their high earnings growth? In addition to the ideas in the Chiswick and Duleep/Regets models, an alternative or contributing factor in the propensity to invest in human capital is an individual's propensity to undertake risk. Jaeger et al. (2010) explore direct evidence on risk attitudes and migration.

<sup>13</sup> The inverse relationship is predicted by Chiswick's Immigrant Assimilation model and gains further support via Duleep and Regets' IHCI model.

selection mechanism (as suggested by Duleep and Regets), or both.<sup>14</sup>

#### IV. METHODOLOGICAL IMPLICATIONS OF A HUMAN-CAPITAL INVESTMENT PERSPECTIVE

Beyond predicting convergence in immigrants' age- and education-adjusted earnings, a human-capital-investment perspective prescribes an empirical strategy to measure immigrant economic assimilation. This strategy differs in two major ways from the path currently pursued in most econometric analyses: It allows for earnings growth to vary with entry earnings, and it does not impose sample constraints such as excluding the self-employed or zero earners.

##### **The Inverse Relationship between Adjusted Entry Earnings and Earnings Growth**

Whether using cross-sections from multiple years to follow over time year-of-entry immigrant cohorts or longitudinal data on individuals, recent econometric analysts typically pool immigrants who have entered their host country at different points in time and estimate a variant of the following model:  $\log y_i = X'\beta + \gamma'C_j + \alpha'YSM + \varepsilon_i$  where  $y_i$  denotes the earnings of immigrant  $i$ ,  $X$  is a vector of variables measuring education and experience and  $\beta$  the corresponding coefficients,  $YSM$  measures years since migration, and  $C_j$  is a set of dummy variables representing each year-of-immigration category,  $j$ .<sup>15</sup>

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<sup>14</sup> Some scholars suggest that empirical evidence of an inverse relationship between immigrant entry earnings and earnings growth is due to regression to the mean. We have done several analyses that cast doubt on this explanation. 1. If the inverse relationship were created by random measurement error, we would not expect the relative size of the effect to stay the same for a country of origin group over time. We expected developed countries to have higher skill transferability, leading to higher initial earnings but lower earnings growth, and indeed find that for entry cohorts from different periods. 2. The results are consistent with even the minor predictions of our Immigrant Human Capital Investment Model. 3. We have performed sensitivity tests where we purposely increase sampling error by taking repeated samples of 50% or 75% of the original data. If the inverse relationship were an artifact of measurement error, a larger error would have made the correlation more negative. In fact, adding this noise to the estimates somewhat reduced the size of our negative correlations. 4. Another way to think of the regression-to-the-mean issue is that the error term in our first estimate of earnings appears in the denominator of the calculation of growth rates. We completely removed this issue by dividing our data in half: estimating first period earnings from the first half sample and using the second half-sample to estimate earnings growth. Again, we found results consistent with our other estimates. (Duleep and Regets, 2002)

<sup>15</sup> Exceptions to the cohort fixed-effect methodology, following cohorts, include Duleep and Regets (2002), Green and Worswick (2012), Lin (2013), and Borjas (2015). Following individuals, exceptions include Duleep and Dowhan (2002) and Hall and Farkas (2008) who use a multivariate analysis that models intercepts and slopes separately.

This fixed-cohort-effect methodology, pioneered by Borjas (1985), assumes that including a dummy (aka categorical) 0-1 variable for each year-of-entry immigrant cohort controls for cohort differences that are not captured by observed variables: it assumes that, once observable variables are controlled for, changes in earnings growth do not accompany changes in the entry earnings of immigrant cohorts. In stark contrast, a human-capital-investment perspective predicts that (controlling for initial levels of human capital) decreases in entry earnings *systematically* accompany increases in earnings growth, and vice versa.

With a systematic inverse relationship between entry earnings and earnings growth, the fixed-cohort-effect methodology produces misleading results when entry earnings (adjusted for observable variables) change. In analyses that pool year-of-entry cohorts and include a dummy variable for each year-of-entry cohort, the earnings-growth estimate (the estimated coefficient on years since migration) averages the earnings-growth rates of all of the year-of-entry cohorts. The actual earnings growth of a recent cohort with lower adjusted initial earnings than earlier cohorts will be higher than the preceding cohorts' earnings growth; an estimate that averages the earnings' growth rates of all the year-of-entry cohorts underestimates its earnings growth. The actual earnings growth of a recent cohort with higher initial earnings than earlier cohorts will be lower than the preceding cohorts' earnings growth; an estimate that averages the earnings' growth rates of all the year-of-entry cohorts overestimates its earnings growth.

This qualification holds whether the analyst's focus is all immigrants, where changes in initial (adjusted) earnings may arise from changes in the source-country composition of U.S. immigration, or U.S. immigrants from a single country where inter-cohort changes in entry earnings may reflect changes in that country's level of economic development or the admission program its emigrants pursue to become U.S. immigrants. By not allowing earnings growth to

change as entry earnings change, the fixed-cohort-effect method obfuscates important inter-cohort transformations in immigrant earnings paths while failing to control for unobserved variables that affect immigrant earnings and vary with year-of-entry cohort. Since cohorts that vary in their entry-level earnings systematically vary in their earnings growth, each year-of-entry cohort should (at least initially) be separately examined, thus freeing the estimation of its earnings trajectory from the paths of other cohorts.

Analysts also typically use all possible year-of-entry cohorts in their analysis, including those for which the available earnings information only begins years after the immigrants' initial year of entry. Yet, to avoid implicit assumptions about the relationship between entry earnings and earnings growth, the analyst should include only year-of-entry cohorts that can be followed from the immigrants' initial host-country years. Though reducing the information that is used, this approach insures that conclusions are not the result of an assumed relationship between immigrants' entry earnings and earnings growth.<sup>16</sup>

A human-capital-investment perspective further suggests caution when interpreting estimated returns to schooling and experience. Since low-skill-transferability immigrants will be more engaged in human-capital investment than high-skill-transferability immigrants, the estimated returns to *cumulative* levels of schooling and experience in an earnings regression will (at least in the initial years of immigration) be much lower for low-skill-transferability immigrants even though the return for schooling that low-skill-transferability immigrants undertake in the host country will, theoretically, exceed that of their high-skill-transferability statistical twins.

In fact, the very practice of controlling for current education level likely depresses

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<sup>16</sup> This approach also avoids confounding effects of age and assimilation and the choice of an appropriate reference group (see, for instance Kossoudji, 1989, Lalonde and Topel, 1991, and Friedberg 1992).

estimates of immigrant earnings growth two different ways. Those who got more education have their post-education earnings compared not to the previous earnings of a similar person holding their old education level, but to someone who already held the new education level in the previous period. The earnings growth estimate of someone 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.

This downward bias will be greatest for immigrants with low initial earnings due to initial problems transferring human capital.<sup>17</sup> According to both human-capital investment theory and American Community Survey data, they will invest in more schooling than either other immigrants or natives with the same age and education. Thus, their earnings growth will be underestimated in both absolute terms and relative to other immigrants or natives. Because of this problem, analysts who want to control for education in their earnings estimations should ideally only use education levels measured at immigrants' year of entry.<sup>18</sup>

## **Sample Constraints**

### ***What are we measuring?***

An important difference of this paper from others looking at earning growth across an immigrant entry cohort may come from who is excluded from the sample. In any statistical analysis, the functional form and data selection rules should reflect the question being asked.

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<sup>17</sup> In his 1986 paper, Chiswick (p. 188) notes the bias created 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.*

<sup>18</sup> Controlling for education using only entry-levels of schooling is possible with longitudinal data in analyses that follow the same individuals. With data following cohorts across cross-sectional datasets it is not possible to do this; the bias problem can be ameliorated some by using broad categories to measure the education levels of individuals.



When the goal is to estimate a rate of return of an extra year of school, it is common to make many exclusions to avoid biasing the regression coefficient. Our focus is on actual immigrant earnings, not potential earnings or an exact rate of return for activities increasing potential earnings. Thus, our cohort samples include everyone meeting the country-of-birth, age, and year-of-entry criteria. By including persons with zero earnings, we include out-of-the-labor force, unemployed, part-time, and in-school individuals. Most other immigrant cohort earnings growth estimates by economists exclude at least some of these categories.

We would argue that excluding zero earners hides dimensions of immigrant assimilation involving human capital investment: it omits immigrants who are pursuing job search and learning, instead of earning. Excluding such individuals likely understates immigrant earnings growth in general and in particular for groups with low initial skill transferability, while having less effect on earnings-growth estimates for immigrant groups characterized by high initial skill transferability. Further note that analyses that use as the dependent variable hourly wages or weekly earnings, instead of annual earnings, leave out an important component of economic assimilation—the ability to work longer hours and weeks.

Excluding the self-employed, as some analysts do, may also understate immigrant earnings growth, particularly for groups with low initial skill transferability.<sup>19</sup> Gallo and Bailey (1996), Bailey (1987), Portes and Bach (1985), Waldinger (1989), and Chunyu (2011) document an immigrant sector in various industries characterized by mutually beneficial arrangements in which recent immigrants working as unskilled laborers at low wages (or even no wages) in immigrant-run businesses receive training and other forms of support that eventually lead to more skilled positions or self-employment.

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<sup>19</sup> Lofstrom (2002) analyzes the effect on immigrant earnings of excluding the self-employed.

### ***Biases created by sample constraints***

In addition to hiding dimensions of immigrant assimilation involving human capital investment, sample constraints can seriously bias estimates of earnings growth in studies that follow immigrant cohorts across censuses or other cross-sectional datasets.

There is an obvious bias created in estimates of cohort earnings growth if you exclude from the first period those with zero earnings, but include individuals who previously had zero earnings in the second period. Even if the typical 10-years between measurement was enough for those with initial skill transferability problems to earn the same as immigrants with no initial difficulty, their exclusion from the first period downward biases earnings growth estimates. This bias also distorts inter-cohort comparisons of how earnings growth changes as adjusted entry earnings change. Specifically, Chiswick's Immigrant Assimilation model and Duleep and Regets IHCI model predict that, the greater the difficulty immigrants have transferring their human capital to the U.S. labor market, the greater the incentive for them to invest in human capital: controlling for initial levels of human capital, immigrant earnings growth should increase as adjusted entry earnings decrease. Yet, the greater the tendency for immigrants to invest in human capital, the greater the presence of zero earners in the first period, and the stronger the negative bias caused by excluding zero earners from the cross-sections over which earnings growth is being measured.

More generally, for studies that follow immigrant cohorts across two or more cross-sections, sample-selection rules such as excluding zero earners, students, and the self-employed compromise the comparability of the two census samples. Immigrants excluded from the initial census sample because they are unemployed or out of the labor force (perhaps because of job search or time spent in school) may be fully employed, hence included, in the second census

sample; immigrants also move from wage and salary jobs (thereby included in the first census sample) to self-employment (thereby excluded). These issues apply to any cohort followed between censuses (or other data sources), but are particularly important for the study of immigrant economic assimilation since immigrants have high occupational mobility, high in-school rates, and a high propensity to become self-employed.<sup>20,21</sup>

Because commonly used sample restrictions hide dimensions of immigrant economic assimilation and create bias for studies that follow cohorts across cross-sections, we advocate including the self-employed, imposing no labor-force-status restrictions, and meeting the concerns that prompted these sample restrictions in other ways.<sup>22</sup> For instance, excluding the self-employed comes from a desire to measure earnings from work per se versus returns from capital. Yet, if the main policy interest is to measure the economic contribution of immigrants, it matters little if a portion of their increased income comes from their investment of savings into a business whereas excluding those creating businesses greatly distorts the overall picture of immigrant economic assimilation.

## V. THE EARNINGS GROWTH OF IMMIGRANTS AS ENTRY EARNINGS DECLINED

Keeping in mind the guidelines inspired by a human-capital-investment perspective—allowing earnings growth to vary with initial earnings, separately examining each year-of-entry cohort, following only cohorts for which there is information on initial earnings,<sup>23</sup> not excluding

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<sup>20</sup> For occupational mobility, see Akresh (2006, 2008), Chiswick (1978b), Chiswick, Lee, and Miller (2005), Chiswick and Miller (2008, 2009), Green (1999), Jasso and Rosenzweig (1990a, 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 (1990a).

<sup>21</sup> Both analyses of Part VI include the self-employed and the second analysis includes all individuals irrespective of labor force status.

<sup>22</sup> For instance, rather than excluding the self-employed, analysts could reduce the effect of non-labor income flows on measured earnings by measuring earnings at the median instead of the mean.

<sup>23</sup> We also fully allow the effect on earnings of educational investment by not controlling for educational attainment. Please see note 23.

zero earners and the self-employed—we examine with decennial census data whether earnings convergence occurred for immigrant men who were in the entry cohorts of the great entry earnings’ decline. If the primary force behind the post-1960 decline in immigrants’ entry earnings was a decrease in the proportion of U.S. immigrants with skills that immediately transfer to the U.S. labor market then, according to a human-capital-investment perspective, increases in earnings growth should accompany declines in immigrant entry earnings. If, instead, the entry-earnings decline reflects a decline in immigrant quality then declines in immigrant entry earnings should be accompanied by either no change in earnings growth (a scenario consistent with the fixed-cohort-effect methodology) or lower earnings growth.

Removing the assumptions and sample constraints that accompany most estimations of immigrant earnings unveils an extraordinary picture (solid lines of Figure 1).<sup>24</sup> As immigrants’ entry earnings decreased over time, their earnings growth increased to such an extent that by the time of the subsequent census, ten years later, the earnings of the more recent cohort equals the relative earnings that earlier cohorts had achieved after 10 years. For instance, despite a 23.4 percent drop in initial earnings (relative to natives) between the 1965-1970 and the 1975-1980 immigrant entry cohorts, their relative earnings by the time of the subsequent decennial census are 85 percent (in 1980) for the 1965-1970 cohort and 84 percent (in 1990) for the 1975-1980 cohort. The convergence occurs because the more recent cohort, with lower relative entry earnings, had much higher earnings growth.<sup>25</sup>

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<sup>24</sup> The convergence in Figure 1, which does not control for education, is remarkable. However, it should not be construed to mean that the propensity for immigrants to invest in human capital is inversely related to education levels. The IHCI model predicts that *conditional on human capital levels (education and experience)* there is an inverse relationship between entry earnings and earnings growth, and that the inverse relationship increases the higher the level of education (Duleep and Regets, 2002). When we control for education, the inverse relationship shown in Figure 1 increases. See Chiswick (1986) for trends in education levels over this period.

<sup>25</sup> When no assumed relationship between entry earnings and earnings growth is imposed, a similar picture of earnings convergence emerges using longitudinal data on individuals (Duleep and Dowhan, 2002). Following the same individuals eliminates selective emigration as a cause for the earnings convergence.

A radically different picture emerges when we impose a trademark of the fixed-cohort-effect methodology—the assumption of inter-cohort constancy in earnings growth. Since the predicted earnings growth of the 1985-90 cohort is an average of the earnings growth rates of all three year-of-entry cohorts, its estimated earnings growth (shown with a broken line in Figure 1) lies below its true earnings growth.

To the extent that changes occur in the age- and education-adjusted earning trajectories of immigrants from specific countries,<sup>26</sup> the fixed-cohort-effect methodology is also problematic for measuring whether earnings convergence occurs across source-countries (the subject of Part VI). Convergence will be underestimated (overestimated) if recent country-specific immigrant cohorts with low initial earnings (relative to recent cohorts of other countries of origin) were preceded by cohorts from the same country with higher (lower) entry earnings and recent country-specific cohorts with high initial earnings (relative to recent cohorts of other countries of origin) were preceded by cohorts with lower (higher) entry earnings. The analyses of Part VI allow earnings growth to vary with initial earnings by separately analyzing each year-of-entry cohort and by following only cohorts for which we have information on initial earnings.

It is important to note that Figure 1 shows the effect of not allowing earnings growth to vary with entry earnings, *using estimates from analyses that do not impose any sample-selection constraints*. Some sense of how commonly used sample selection exclusions negatively bias earnings growth estimates in analyses that follow cohorts across cross-sectional surveys can be gleaned by comparing the solid lines for each cohort in Figure 1 with the corresponding estimates of earnings growth in Table 2 of Borjas (2015). The estimates in Borjas (2015) are

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<sup>26</sup> Such changes could stem from a variety of causes including the relaxation of immigration restrictions, the post-1965 emphasis on family admissions, changes in the macro-economies of the sending and receiving countries, and over time changes in the economic development of the source country relative to the host country.

based on the same census data that we follow and allow earnings growth to vary with entry earnings, but impose commonly used sample exclusions.<sup>27</sup>

## VI. TESTING FOR EARNINGS CONVERGENCE ACROSS SOURCE COUNTRIES

Whether country-of-origin influences increase, decrease, or stay constant with time in the United States depends upon their underlying causes. If they primarily reflect intergroup differences in immigrant skill transferability, then earnings differences should narrow: immigrants whose source-country skills initially transfer poorly to the U.S. labor market and who thus have lower initial earnings will have higher earnings growth than high-skill-transferability immigrants. If country-of-origin effects primarily reflect the selection of more or less able individuals then such influences will persist or even grow in importance with time in the United States since low (high) ability likely dampens (increases) earnings growth: individual ability affects both the workplace productivity associated with a given level of human capital and an individual's ability to gain new human capital. In addition, as immigrants spend time in the United States, their different individual success rates would increase within-country variance in earnings. This, in and of itself, would lead to an increase with U.S. residence in the importance of country of origin as a determinant of immigrant earnings.

Key evidence that country-of-origin effects diminish comes from Chiswick (1978a, 1979), who discovered that U.S. immigrants from non-English-speaking countries had lower initial earnings but higher earnings growth than immigrants from English-speaking countries. Chiswick's seminal research was based on 1970 census data. However, as introduced and

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<sup>27</sup> Each census sample in Borjas (2015) is limited to men with 1 to 40 years of labor market experience, who worked at some point during the survey year, and who are not in school. The dependent variable is weekly earnings defined as annual earnings divided by weeks worked.

empirically tested in Chiswick (1980), inter-cohort changes in unmeasured variables may produce biased estimates of immigrant earnings growth in analyses based on a single cross-section. Following cohorts of immigrants provides a potential solution to the cross-sectional bias problem, as long as both entry earnings and earnings growth are allowed to vary.<sup>28</sup> Accordingly, we pursue two complementary cohort analyses to illuminate whether country-of-origin influences diminish with time in the United States. The first examines the relationship between the importance of country of origin, as a determinant of immigrant earnings, and immigrant time in the United States. The second examines whether the earnings dispersion of demographically comparable immigrants, across different source countries, decreases over time.

These analyses test three empirical implications of the skill-transferability explanation for country-of-origin differences in immigrant earnings: (1) With time in the United States, earnings-related characteristics other than country of origin (such as years of schooling and experience) should become better predictors of immigrant earnings; (2) the importance of country of origin as a determinant of immigrant earnings should fade; and (3) the earnings of demographically comparable immigrants, regardless of origin, should converge.

### **Measuring the Explanatory Value of Country of Origin at Entry and Ten Years Later**

The skills-transferability explanation for intergroup differences in the initial education- and age-adjusted earnings of immigrants predicts that as immigrants with low skill transferability invest more heavily in U.S. human capital, than immigrants with high skill transferability, the importance of country of origin as a determinant of immigrant earnings will decrease while the importance of other earnings-related characteristics will increase. To determine how the

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<sup>28</sup>Because of the inverse relationship between entry earnings and earnings growth, when immigrant entry earnings have been falling over time, cross-sectional estimates overestimate the earnings growth of earlier year-of-entry cohorts while doing a better job than the fixed-cohort-effect methodology of estimating the earnings growth of more recent immigrant cohorts. Note the similarity in the cross-sectional and cohort-based estimates in Chiswick (1986).

importance of country of origin as a determinant of immigrant earnings changes with immigrant time in the United States, we estimate two log earnings regressions<sup>29</sup> for various immigrant entry cohorts using the Public Use Micro Samples (PUMS) from the 1960, 1970, and 1980 censuses.<sup>30</sup>

The first earnings regression—the basic human-capital model—includes years of schooling, years of work experience, and experience squared as regressors.<sup>31</sup> We purposely kept this earnings function sparse to allow other differences in productivity to be captured by country-of-origin variables in a second model, which adds to the base set of regressors a set of dummy variables denoting an immigrant’s country or region of origin, alone and interacted with the education and experience variables. Including interactions with education and experience allows country of origin to add explanatory power through country-of-origin differences in the value of education and experience, as well as through differences in the regression intercept.

$$\text{Human-capital model: } \log y_{it} = \alpha + \beta_1 \text{Ed}_{it} + \beta_2 \text{Exp}_{it} + \beta_3 \text{Exp}_{it}^2 + \varepsilon_{it}$$

*Human-capital model with country-of-origin variables:*

$$\log y_{it} = \alpha + \beta_1 \text{Ed}_{it} + \beta_2 \text{Exp}_{it} + \beta_3 \text{Exp}_{it}^2 + \gamma_1 G_{ji} + \gamma_2 G_{ji} \cdot \text{Ed}_{it} + \gamma_3 G_{ji} \cdot \text{Exp}_{it} + \gamma_4 G_{ji} \cdot \text{Exp}_{it}^2 + \varepsilon_{it}$$

Where  $y_{it}$  = the earnings of individual  $i$  in year  $t$ ;  $\text{Ed}$  = years of schooling;  $\text{Exp}$  = age minus years of schooling minus 6; and  $G_{ji}$  = a categorical variable denoting the source country  $j$  (or country-group  $j$ ) of immigrant  $i$ .

The above pair of earnings regressions is first estimated for a cohort of immigrant men, aged 25-54, who have only been in the United States 0-5 years. (The cohort that entered the United States during the 5 years prior to a decennial census.) Using the subsequent decennial

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<sup>29</sup> Consistent with standard professional practice in estimating Mincer earnings functions, our dependent variable is log of earnings which, since log of zero is undefined, eliminates zero earners. Analysts may want to reconsider the use of log earnings as their dependent variable. In our case, using it works to understate earnings convergence.

<sup>30</sup> We used the 1980 5% “A” PUMS, the 1970 1% State PUMS based on the 5% questionnaire, and the 1960 1% PUMS (Bureau of the Census, 1983, 1977, and 1975). The 1960 census did not collect year-of-immigration. Place of residence in 1955 allowed us to identify immigrants who entered the U.S. between 1955 and 1960.

<sup>31</sup> We limit our focus to annual earnings. There is no information in any census on wages per se. Hours and weeks worked information is in the 1980 census but only recorded in broad brackets in the 1960 and 1970 censuses.



census, we estimate the same pair of earnings equations, for the same cohort, 10 years later and aged 35-64. We can thus compare for each cohort the extent to which adding country of origin increases the explanatory value of the earnings regression at time of entry and 10 years later, as measured by R-squared.<sup>32</sup>

Table 2 shows the results from the analysis following the 1955-59 and 1965-69 cohorts.<sup>33</sup> For each cohort, the  $R^2$  for the base regression increases with the passage of ten years. Since the sample for each regression is limited to those who entered the United States during the same 5-year period, but whose ages span 30 years, most of the variation in experience in the sample occurs before migration. Since we start with 25-year-olds, much of the variation in education reflects education outside of the United States.<sup>34</sup> The same holds true for the ten-year-after regressions: although individuals now may have substantial U.S. experience, there is little

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<sup>32</sup> It is often suggested that we use  $\bar{R}^2$  versus  $R^2$  in this analysis. Theoretically,  $R^2$  is the correct measure as it measures the portion of the variance accounted for by a model's variables and coefficients.  $\bar{R}^2$  is useful for choosing between model specifications as it adjusts for the model's number of explanatory variables relative to the number of data points the model is estimated on, decreasing as explanatory variables are added that improve the model by less than expected by chance. This adjustment, however, means that we cannot interpret a change in  $\bar{R}^2$ , with the addition of the country variables, as the real increase in explanatory power. (We cannot interpret the difference in  $\bar{R}^2$  after adding the country variables as the increase in explanatory power of the source-country variables minus the increase in explanatory power from adding the same number of random variables.) Differences in  $R^2$  are more relevant than differences in  $\bar{R}^2$  for our analysis, even if some portion of the change in explanatory power is due to chance. Moreover, in each case we add the same number of country variables to the model, reducing degrees of freedom by the same number of points. To the extent that this results in a roughly similar increase in  $R^2$  by chance, the proportional change in the explanatory power of the country variables overtime will be understated. Empirically, it doesn't matter whether we use  $R^2$  or  $\bar{R}^2$ . For the cohorts in Table 7 (with the sample size for each cohort exceeding 30,000 observations)  $R^2$  or  $\bar{R}^2$  are nearly identical. Smaller sample sizes characterize the earlier cohorts of Table 2: There are 2,665 observations for the 1955-59 cohort in 1960 and 2,168 in 1970; 3,577 observations for the 1965-69 cohort in 1970 and 17,683 in 1980. To assuage any concerns, the results for Table 2 in terms of  $\bar{R}^2$  are shown below.

Entry Cohort, Census year	$\bar{R}^2$ for human-capital model	$\bar{R}^2$ for HC model with country variables	Change in $\bar{R}^2$ from adding country variables	Percentage change in $\bar{R}^2$ from adding country variables
1955-59, 1960	.1144	.2531	.1387	121.2
1955-59, 1970	.1594	.2202	.0608	38.1
1965-69, 1970	.0805	.1540	.0735	91.30
1965-69, 1980	.1662	.2029	.0367	22.08

<sup>33</sup> Full regression results are available from the authors.

<sup>34</sup> We use the term "much" but not "all" advisedly: among persons 25 years old and older, Duleep and Regets (1999, 2002) find higher rates of school attendance for recent immigrants than for natives.

additional variation in the amount of U.S. experience in the sample. Thus, the  $R^2$  for the base model primarily measures the explanatory power of country-of-origin human capital; its increase over time is consistent with an increase in the value of country-of-origin human capital over time, as it both complements and aids in the acquisition of new U.S. skills.

Country-of-origin effects over time can be seen more directly from the fourth and fifth columns where both the absolute and relative gain in  $R^2$  from adding country of origin is dramatically smaller ten years after our initial observations for each cohort. This suggests that the importance of country of origin as a determinant of immigrant earnings for a given cohort decreases with immigrant time in the United States.

***Do the changes in  $R^2$  reflect other processes?***

Quite apart from our hypothesis of immigrant earnings convergence, unexplained earnings variation will change over the life cycle, and in different economic environments. Since the early 1970's, earnings dispersion has increased due to differences in the return to observed (and, it is often theorized, unobserved) human capital. In addition, younger workers' observed earnings (as opposed to potential earnings) will vary in part due to greater variation in the proportion of earnings foregone due to human-capital investment, while older workers' earnings will vary in part due to greater variation in the stock of human capital. To what extent do the changes in  $R^2$  for each cohort reflect an immigrant phenomenon conforming to the skills-transferability hypothesis versus the change in age range from the first to the second period, or the over-time change in the earnings distribution? The sensitivity of our analyses to these life-cycle and period effects may be partially tested by examining the changes in  $R^2$  that occur following a cohort of U.S.-born men.<sup>35</sup>

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<sup>35</sup> It is not, of course, possible to use country-of-origin dummies with natives.

Table 2a shows little change in the explanatory power of the basic human-capital model for native male cohorts followed between 1960 and 1970, and between 1970 and 1980. Going from 1960 to 1970,  $R^2$  rose for natives, but by a much smaller amount than for the 1955-59 entry-cohort immigrants (Table 2). Going from 1970 to 1980, the base model  $R^2$  for natives fell slightly, while the base model  $R^2$  for the 1965-69 entry-cohort immigrants more than doubled. Consistent with initial differences in skill transferability, the initial base-model  $R^2$  for the entry-cohort immigrants is lower than for natives in both 1960 and 1970; ten years later, it is somewhat greater for immigrants than for natives in both periods.

### **Measuring Earnings Dispersion at Entry and Ten Years Later**

We also test whether earnings differences of demographically comparable immigrants by country of origin narrow with years since migration. To do this, we examine the degree of dispersion in the median earnings of immigrant men by country of origin within age/education cells for cohorts that had entered the United States during the five years prior to a decennial census, and again ten years later. Median earnings were measured within education and age subsets for 27 countries, cell sample sizes permitting.<sup>36</sup> We chose the coefficient of variation, defined in this case as  $\sigma/|\bar{x}_{med}|$  (the standard deviation of the distribution of median earnings divided by the mean of the distribution of median earnings) as our measure of dispersion since this measures dispersion in relation to mean earnings, which grew substantially in both real and

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<sup>36</sup> Sample size concerns led us to group many countries, and to group age and education each into two categories. Also, any estimate of median earnings based on a sample size of less than 5 individuals was dropped. The education categories are 1-12 years and 13 or more years. The age categories are 25-39 and 40-54 for entry cohorts on the 1960 and 1970 PUMS, and 35-49 and 50-64 for the same cohorts ten years later on the 1970 and 1980 PUMS. So that the results are not affected by changes in how the source countries/regions were defined, the source-country/region selection is kept constant across all of the analyses in this paper. We also conducted sensitivity tests and found very similar results regardless of how the source country/regions were defined. The source countries/regions used in the analyses are: Africa, Britain, Canada, China/Taiwan, Cuba, Czechoslovakia, Germany, Greece, Hungary, India, Ireland, Islamic Southwest Asia, Italy, Jamaica, Japan, Korea, Mexico, Oceania, Other Asia, Other Central America, Other Communist Europe, Other Non-Communist Europe, Philippines, Poland, Portugal, S. America, and Yugoslavia.

nominal terms over the 1960's and 1970's.<sup>37</sup>

All 8 cohort comparisons (Table 3) reveal important reductions in the coefficient of variation (CV) after ten years. Within age and education groups, the across-source-country earnings variation declines by 13 to 55 percent.

As with the previous analysis using  $R^2$ , we explore the sensitivity of this analysis to intra-cohort changes in age distribution and census-year earnings distributions by following an age cohort of U.S.-born men between the 1960 and 1970 censuses, and between the 1970 and 1980 censuses. The coefficient of variation in earnings for U.S. natives<sup>38</sup> is shown in Table 3a for the four age/education cells used in Table 3. While the CV of earnings for recent immigrants declined over time in each case, it increased in most cases for native males. The major exceptions are the group of younger, high-education natives, whose earnings CV shows large declines in both periods, and the group of older, high-education natives in the 1960-1970 period, who experienced a small decline. Yet, even in those cases, the percentage decline in the earnings CV for immigrants greatly exceeds that for natives.

## VII. EMIGRATION BIAS

Careful thought must be given to potential biases caused by immigrants leaving the United States since our findings are based on analyses that follow samples of individuals across decennial censuses, rather than the same individuals.<sup>39</sup> Does the country-of-origin earnings

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<sup>37</sup> Estimates of standard errors for our estimates of the coefficient of variation follow Kakwani (1990) in which the standard error of a coefficient of variation on a variable  $X$  is given by:  $\sigma_{cv} = (((M^2(M_4 - M_2^2) + 4 M_2(M_2^2 - (M M_3))) / (16 M^4 (M_2 - M^2)))^{0.5}$  where  $M$  is the mean of  $X$ ,  $M_2$  is the mean of  $X^2$ ,  $M_3$  is the mean of  $X^3$ , and  $M_4$  is the mean of  $X^4$ . In computing the coefficient of variation, each median earnings observation was weighted by the number of individuals in the age/education/country-of-origin category in the starting period.

<sup>38</sup> The coefficient of variation in individual earnings is a slightly different concept than that of the CV of median earnings of country-of-origin groups. This alternative measure was used since native males could not be grouped by source country. Although individual earnings show greater total variation, our focus is on how this variation changes over time as a cohort ages, and between time periods.

<sup>39</sup> From 1908 to 1950, the U.S. government collected annual statistics on those who left the United States. With this information, Warren and Kraly (1985) estimated that about 75 percent of the immigrants who emigrated did so in their first 5 years of U.S. residence. The analysis of more recent evidence confirms that most U.S. immigrants who

convergence reflect selective emigration systematically related to the U.S. earnings of immigrants?

The type of selection bias of earnings growth caused by emigration, and even the extent of U.S. emigration, are hotly contested issues in immigration research.<sup>40</sup> One continuing theme with early roots is that the less successful, relative to immigrants of the same age, education and country of origin, are most likely to emigrate.<sup>41</sup> In this case, lower earning individuals would not be in the sample for later censuses and earnings-growth estimates from following immigrant cohorts are biased upwards. Another continuing theme (also with early roots) suggests the reverse. Chiswick (1980) failed to find evidence that, controlling for education, the unsuccessful are more likely to emigrate: “If anything, re-emigration rates are higher the greater the transferability of skills and if the original migration is economic in nature,” a conclusion that resonates with the inter-decennial census analyses of Jasso and Rosenzweig (1990a).

Despite opposing predictions, these models (and their descendants) share a common trait: emigration occurs most frequently in the same part of the earnings distribution for each country-of-origin immigrant group, regardless of the group’s place in the overall earnings distribution. In other models, the distribution of emigration within each country-of-origin group is affected by its place in the overall earnings distribution. One model suggests that immigrants from countries with less (more) equal income distributions than the United States would (controlling for education and age) have relatively low (high) U.S. earnings; within those groups, the higher (lower) the earnings ability of immigrants, the less they would benefit from living in the U.S.

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emigrate do so within their first five years of residence (Duleep, 1994).

<sup>40</sup> Reviews of emigration studies include Lalonde and Topel (1997) and Dustmann and Görlach (2014)

<sup>41</sup> Yezer and Thurston (1976) proposed this theory in a study of U.S. migration. Other early studies with the prediction that it is the unsuccessful who migrate include Vanderkamp (1972) and DaVanzo (1983). Underscoring the complexity of emigration, DaVanzo(1983, p. 558) notes possible interactions between who emigrates and the timing of emigration: “Only those migrants who return promptly...conform to the ‘failure’ stereotype...” Also see Davanzo (1976) and DaVanzo and Morrison (1981).

relative to their country of origin, and the more likely they are to emigrate.

Given the diversity of perspectives with opposing conclusions, and the fact that there are other etiologies,<sup>42</sup> we test—theoretically and empirically—the sensitivity of Part V’s convergence results to emigration without embracing any assumption about the emigration-earnings relationship.

### **A Theoretical Approach for Assessing the Impact of Emigration**

Potential patterns of emigration as a function of the U.S. earnings of immigrants are shown in Figure 2. The series of distributions shown in panels A and B represent hypothetical distributions of U.S. immigrant earnings from particular countries of origin with  $\bar{Y}$  denoting the average earnings of immigrants across all countries of origin. The darkened areas indicate where in each country-of-origin earnings distribution emigration is most likely to occur.

In Panel A, emigration occurs most frequently in the same part of the earnings distribution for each country-of-origin immigrant group, regardless of the group’s place in the overall earnings distribution. In the specific example shown, low earners in each country-of-origin immigrant group are more likely to emigrate. This model might fit particularly well if the original decision to immigrate was affected by how fellow countrymen with similar levels of education and experience fared in the United States. Those in the lower end of the distribution, who did worse in the job market than they had originally expected, would be the most likely to emigrate. Such a pattern would reduce the dispersion within each country-of-origin group. Because of emigration, the relative contribution of country of origin to explaining earnings, as measured by the change in  $R^2$ , would increase. This would lead us to underestimate, in our first convergence analysis, the decline in the importance of country of origin with immigrant time in

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<sup>42</sup> See, for instance, Dustmann (2000) and Cortes (2004).

the United States.<sup>43</sup> However, since the propensity to emigrate is similarly distributed across all countries, this type of emigration does not pose a problem for our second convergence analysis, as it affects only the within-country earnings variance, not the dispersion of median earnings across all countries.

Another variant of Panel A is emigration of the high earners in the right tail of each group's distribution. This has exactly the same effect on our two convergence measures as emigration from the left tail: the reduction in the earnings dispersion within each country-of-origin group will cause an overestimate of the importance of country of origin with immigrant time in the United States, but no change in the dispersion of median earnings.

Finally, consider emigration in the center of each country-of-origin group's earning distribution. While still having no effect on the dispersion of median earnings, this will increase the dispersion of earnings within each country-of-origin group. This scenario is of particular concern to our first analysis of convergence since it would lead us to underestimate the importance of country of origin with immigrant time in the United States and thus *overstate* the decline in its importance.

Panel B shows a different emigration pattern wherein the distribution of emigration within each country-of-origin group is affected by its place in the overall earnings distribution: emigration is more likely among low-earning immigrants from country-of-origin groups with high median earnings and high-earning immigrants from country-of-origin groups with low median earnings. The reverse can also be considered: emigration is more likely among high

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<sup>43</sup> In the first convergence analysis we compute  $R^2_w - R^2_{wo}$  or  $[1 - \Sigma(y_i - \hat{y}_{iw})^2 / \Sigma(y_i - \bar{y})^2] - [1 - \Sigma(y_i - \hat{y}_{iwo})^2 / \Sigma(y_i - \bar{y})^2]$  for entering immigrants and for the same cohort 10 years later, where  $\hat{y}_{iw}$  is the predicted earnings of individual  $i$  from the equation with country of origin and  $\hat{y}_{iwo}$  is the predicted earnings of individual  $i$  from the equation without country of origin. Emigration at the tails of the country-of-origin earnings distributions will cause  $y_i$  to be more closely distributed around  $\hat{y}_{iw}$ , relative to  $\hat{y}_{iwo}$ , and  $R^2_w - R^2_{wo}$  to become larger; emigration at the center of the country-of-origin earnings distributions will have the reverse effect.

earners from high-earning countries and low earners from low-earning countries. Both scenarios would reduce the dispersion of earnings within the country-of-origin groups. In our first convergence analysis of the change in  $R^2$ , emigration would lead us to overestimate the importance of country of origin with immigrant time in the United States, and thereby underestimate the decline in its importance. In our second analysis, these two scenarios would have opposite effects: emigration increases the dispersion of median earnings leading to an underestimate of convergence. The second scenario is of greater concern as it would lead us to overestimate convergence.

The potential biases in our two convergence analyses caused by each stylized pattern of emigration are summarized in Table 4; a plus sign indicates emigration patterns that would cause us to overstate convergence. For each pattern of emigration, at least one of our two analyses measuring earnings convergence is either unaffected or understates earnings convergence. Thus while each of our analyses could produce a spurious convergence due to emigration, none of the theoretical emigration patterns would overstate convergence in both analyses.

### **An Empirical Approach for Assessing the Impact of Emigration**

To examine empirically the effect of emigration bias, we re-estimated our two convergence analyses dividing the sample into high- and low-emigration source-country groups.<sup>44</sup> If our findings of convergence reflect emigration, we should consistently find greater convergence in country-of-origin cohorts with high emigration. Re-estimating our analyses on samples divided by emigration level also allows us to determine whether our findings of convergence persist when emigration variation is reduced.

Our measure of 10-year emigration is calculated using the number of observations for

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<sup>44</sup>The emigration analyses in Jasso and Rosenzweig (1990a) inspired our empirical approach.



each cohort on the 1960-80 decennial censuses adjusted to the 5 percent sampling proportion of the 1980 census file. Since sample sizes and census-coverage changes over time make this a crude measure, we use it only to categorize the country-of-origin cohorts as having high or low emigration rates, with the median emigration rate serving as the dividing point.

Table 5 shows our findings when we re-estimate the explanatory power of country of origin over high- and low-emigration samples. For both high- and low-emigration country-of-origin groups, the addition of country of origin adds less to the explanatory power of the earnings regression after a cohort has been in the United States 10 years. Most importantly, the magnitude of the effects seem unrelated to high or low emigration.

Estimates of the dispersion of median earnings for high- and low-emigration samples are shown in Table 6. Not surprisingly, there is more variability in the estimates given the smaller cell sample sizes after dividing the sample. Nevertheless, in all eight age/education/entry-year cohorts, the coefficient of variation decreases in the ten years between decennial censuses, and no strong pattern emerges of larger (or smaller) reductions in earnings variation for the high-emigration cohorts.

## VIII. MORE RECENT EVIDENCE

Part V focused on the immigrant cohorts immediately before and after the Immigration and Nationality Act of 1965, when the earnings of entering immigrants fell from equaling to being vastly below the earnings of U.S. natives. To determine whether earnings convergence across source countries continued for subsequent cohorts of U.S. immigrants, we repeated the analyses of Tables 2 and 3 with more recent census data.<sup>45</sup>

Using 1980-2000 decennial census data, Table 7 shows for the 1975-80 and 1985-90

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<sup>45</sup> We used three datasets: a 1980 5% “A” PUMS, a 6% microdata sample created by combining and reweighting the 1990 5% and 1% PUMS, and a 2000 combined 5% and 1% PUMS (Bureau of the Census, 1983, 1992, 2008).

cohorts the extent to which adding country of origin increases the explanatory value of the earnings regression at time of entry and 10 years later, as measured by R-squared. Following our earlier framework, we estimate a pair of earnings regressions at entry, and ten years later. The first earnings regression in the pair includes as regressors level of schooling<sup>46</sup> and age and age squared (as proxies for years of work experience and experience squared).<sup>47</sup> The second earnings regression adds to the human-capital model a set of dummy variables denoting an individual immigrant's country or region of origin, both alone and interacted with the education and experience variables.

We find, as before, that the  $R^2$  for the human-capital regression increases with the passage of ten years, consistent with the hypothesis that schooling and experience become better predictors of immigrant earnings with time in the United States. Concomitantly, both the absolute and relative gain in  $R^2$  from adding country of origin are dramatically smaller ten years after our initial observations.

Table 8 examines whether earnings converge across immigrant source-country groups. Confirming our previous results, all cohort comparisons delineated by age and education show reductions in the coefficient of variation (CV) after ten years.<sup>48</sup>

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<sup>46</sup> Although the 1980 census data have years of schooling, information on schooling achievement in the 1990 data is in categories. To maintain conformity in the explanatory variables across censuses, we included five dummy variables for schooling categories in both the 1980 and 1990 earnings regressions: 9-11 years, high school degree, some college (including two-year degrees), Bachelor's degree, and graduate degree, for the 1990 census, and 9-11 years, 12 years, 13-15 years, 16-17 years, and 18 years or more, for the 1980 census. The excluded variable in both specifications is eighth grade or less. Another complication is that the 1990 census measures successful completion of various schooling levels; the 1980 definition measures years of completed schooling per se. Analysis of a sample from the Current Population Survey with the new and old census education questions revealed that 17 years of schooling was most consistent with completion of only a bachelor's degree (Jaeger, 1997, Jaeger and Page, 1996).

<sup>47</sup> For the 1980-1990 analysis, age rather than age minus years of schooling minus 6 was used for both periods for consistency. (See the previous note on the 1980-1990 changes in education measurement.)

<sup>48</sup> As in the previous analyses on earlier cohorts, we test the sensitivity of our results to intra-cohort changes in age distribution and census-year earnings distributions by following an age cohort of U.S.-born men between the 1980 and 1990 censuses and between the 1990 and 2000 censuses. The results echo the results of Tables 2A and 3A. For instance, for the 25-54 age cohort, the base model  $R^2$  in 1980 is .1185 and .1161 in 1990. Going from 1990 to 2000, the base model  $R^2$  for natives fell (.1563 in 1990 and .1222 in 2000).

## IX: MEASURING SKILL TRANSFERABILITY AND HUMAN CAPITAL INVESTMENT

Two implicit assumptions underlie our analyses. One is that the adjusted earnings gap (e.g., the difference between an immigrant's earnings and that of a native of the same age and education level) is a good measure of immigrant skill transferability. The other is that human capital investment underlies earnings growth.

Seeking a more direct measure of skill transferability than the adjusted earnings gap, scholars such as Akresh (2007) and Borjas (2015) have used English proficiency. Yet, English proficiency captures but one aspect of skill transferability whereas the adjusted earnings gap embodies all aspects of skill-transferability, measured and unmeasured. Moreover, the relationship between English proficiency and skill transferability does not occur in a vacuum. Immigrants' decisions as to what lines of work to pursue and where to live take into account their English proficiency. As such, the source-country skills of some immigrants who speak English poorly may transfer well to the U.S.: the exceedingly high prevalence of self-employment among Korean immigrants matches their exceedingly low English proficiency; the high entry earnings of Japanese immigrant men—despite their low English proficiency—reflects that many work in U.S.-based Japanese firms; Bauer, Epstein, and Gang (2005) find that Mexican migrants with poor English skills migrate to locations with a large enclave. Conversely, the skills of immigrants who speak English perfectly may not transfer well to the U.S. labor market: persons escaping problems in their countries will come to the U.S. even if it means starting a completely new line of work. For these reasons, English proficiency does not, by itself, provide a universal or comprehensive measure of immigrant skill transferability.

The program under which immigrants enter the U.S. may provide a better, all-inclusive, “direct” measure of skill transferability. By the very nature of their admission—based on an

employer's willingness to participate in a labor certification process—employment-based immigrants have skills that are immediately valued in the U.S., in contrast with persons who come in via family-based visas.

The likelihood of being a family- versus employment-based immigrant varies greatly by country of origin. Consistent with the findings of this paper, studies in both 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 employment-based admissions.<sup>49</sup> Updating previous results, we matched by age, year of immigration, and country of origin immigrant admissions data from the 1993-1998 INS public use files to the 2000 decennial census and 2008 American Community Survey. This permitted us to use multinomial logit parameter estimates to calculate the probability that individuals in the census data were family-based versus employment-based immigrants. We find, as in previous studies, that the more likely an immigrant is family-based, the lower the initial earnings and the higher the earnings growth. But does this higher earnings growth represent human capital investment, as we have assumed in this paper? And, if so, does this investment mostly represent becoming more proficient in English?

Human capital investment takes myriad forms and is difficult to measure. Nevertheless, the census asks individuals whether they are attending school. Since this question asks only about courses in degree or high school diploma programs, it is not measuring participation in English classes per se. Since it asks whether an individual is currently attending school, there is no need to follow a cohort over census samples (e.g., to measure changes in education level) with the attendant problems of emigration. Relating school attendance to the probability that an immigrant is family versus employment-based reveals a strong positive relationship (Figure 3).<sup>50</sup>

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<sup>49</sup> Refer to note 9.

<sup>50</sup> Figure 3 shows school attendance as a function of age and the probability of being a family-based immigrant

## X. CONCLUSION

We examine how the well-documented importance of country of origin to immigrant earnings changes with time in the United States. Scholars attribute the large decline in immigrant entry earnings following the 1965 Immigration and Naturalization Act to a dramatic change in the source-country composition of U.S. immigrants. Eliminating all assumptions and sample constraints that underlie most econometric analyses of immigrant earnings, we find that as immigrants' entry earnings decreased, earnings growth increased to such an extent that by the time of the subsequent census, ten years later, the earnings of the most recent cohort equal the earnings that earlier cohorts had achieved after 10 years.

We then pursue two country-of-origin cohort analyses. The first reveals a decrease in the explanatory power with time in the United States of country-of-origin variables in earnings regressions estimated across individuals in specific year-of-entry immigrant cohorts. This suggests that as immigrants stay in the United States, the importance of country of origin for explaining earnings decreases. The second analysis reveals a decrease with time in the United States in the dispersion of individual earnings across countries of origin within various age/education/year-of-entry cohorts. This suggests that the earnings of demographically comparable immigrants, regardless of country of origin, tend to converge over time.

As both analyses are based on following cohort samples, emigration is a serious concern. Nevertheless, the theoretical emigration patterns that would cause a spurious decrease in the importance of country of origin in one analysis are the complement of the emigration patterns

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

that would cause a spurious decrease in the other analysis. Moreover, using a technique that could be applied more broadly, both results persist in an empirical test for emigration bias.

The convergence results do not appear to be caused by other processes such as labor-market-wide changes in earnings distribution between censuses or by earnings-distribution changes that occur, in general, as individuals gain experience. Similar natives over the same periods do not show similar declines in earnings variation, and the importance of the basic human-capital model to explain that variation does not change importantly for U.S.-born cohorts over these periods.

Our three analyses suggest that although country of origin plays a prominent role in immigrants' entry-level earnings, its importance declines with time in the United States. This conclusion resonates with other research that predicts or finds an inverse relationship (conditional on initial immigrant experience/education levels) between immigrant entry earnings and earnings growth.<sup>51</sup> The inverse relationship between immigrants' initial earnings and earnings growth, which is at the bedrock of our findings, suggests that variation in initial skill transferability as opposed to immigrant quality underlies differences in immigrants' initial earnings, and that high human capital investment accompanies low initial earnings. A scenario in which low earnings are associated with high human-capital investment has profoundly different social and economic consequences than a scenario in which low earnings are associated with low ability.

A human capital perspective also has methodological implications for the measurement of immigrant economic assimilation. In the presence of an inverse relationship between

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<sup>51</sup> For instance, Chiswick (1978a, 1979), Lalonde and Topel (1991, 1997), Duleep and Regets (1994, 1996a, b; 1997; 1999; 2002), Jasso and Rosenzweig (1995), DeSilva (1997), Schoeni (1997), Demombynes (2002), Akresh (2007), Green and Worswick (2012), and Lin (2013).

immigrant entry earnings and earnings growth, a methodology that permits entry earnings but not earnings growth to vary misinforms as much, if not more, than a methodology that assumes stationarity in both entry earnings and earnings growth (the cross-sectional methodology). In the presence of an inverse relationship, it is as problematic to let entry earnings vary, but not earnings growth, as it would be to let earnings growth vary, but not entry earnings.

Scholars firmly understand that, in analyses following cohorts across multiple cross-sections, immigrant emigration can bias measures of immigrant earnings growth. Yet, no attention has been given to the fact that the sample exclusions economists commonly impose in such analyses impart a negative bias to immigrant earnings growth and may render meaningless inter-cohort comparisons over time and across countries.

Census data, because of their large sample sizes, numerous variables, and long historical span, will continue to illuminate the economic assimilation of U.S. immigrants. Adopting methodologies that permit both entry earnings and earnings growth to vary is critical in studies that follow cohorts across decennial censuses as well as in studies that follow individuals with longitudinal data. Of equal importance is to avoid defining the sample in ways that hide immigrant economic assimilation.

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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 Relative to the U.S. Born, by Immigrant Region of Origin:					
Ratio of 1989 Earnings of the 1985-1989 Immigrant Cohort to U.S. Natives					
	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	.406	.529	.485	.381	.500
<i><b>By region of origin</b></i>					
Asia	.443	.589	.434	.316	.439
Central/South America	.364	.506	.447	.376	.401
Western Europe	1.010	1.147	.931	.845	1.372
Estimates are based on a 6% micro-data sample created by combining and reweighting the 1990 Census of Population 5% and 1% Public Use samples.					

Table 2: Changes in the Explanatory Power of Country of Origin as Time in U.S. Increases, 1960-1980 Decennial Census Data (Bootstrap standard errors for $R^2$ in parentheses)				
Entry Cohort, Census year	$R^2$ for human-capital model	$R^2$ for human-capital model with country-of-origin variables	Change in $R^2$ from adding country-of-origin variables	Percentage change in $R^2$ from adding country-of-origin variables
<b>1955-59 cohort</b>				
At entry: 1960	.1154 (.0131)	.2758 (.0188)	.1604	139.0
Ten years later: 1970	.1606 (.0095)	.2493 (.0099)	.0887	55.2
<b>1965-69 cohort</b>				
At entry: 1970	.0812 (.0086)	.1731 (.0123)	.0919	113.2
Ten years later: 1980	.1663 (.0055)	.2065 (.0058)	.0402	24.2
The base model is the regression of individual log(earnings) on experience, experience squared, and education. In the second model, region/country dummies and region/country interactions with education and experience are included for Africa, Britain, Canada, China/Taiwan, Cuba, Czechoslovakia, Germany, Greece, Hungary, India, Ireland, Islamic Southwest Asia, Italy, Jamaica, Japan, Korea, Mexico, Other Asia, Oceania, Other Communist Europe, Other Non-Communist Europe, Other Central America, Philippines, Poland, Portugal, South America, and Yugoslavia. Estimates are based on the 1980 5% "A" PUMS, the 1970 1% State PUMS based on the 5% questionnaire, and the 1960 1% PUMS. Sample size information for all entry cohorts, at entry and 10 years later, including the more recent cohorts analyzed in Tables 7 and 8, is given in note 24.				

Table 2a: Changes in the Explanatory Power of the Base Model for U.S.-Born Men; 1960-1980 Decennial Census Data	
Age, Census year	$R^2$ for Base Model
25-54, 1960	.1317
35-64, 1970	.1475
<i>Change in <math>R^2</math></i>	.0158
25-54, 1970	.1312
35-64, 1980	.1143
<i>Change in <math>R^2</math></i>	-.0169
The base model is the regression of individual log(earnings) on experience, experience squared, and education.	

Table 3: Changes in the Dispersion of Median Earnings Across Country-of-Origin Groups; 1960-1980 Decennial Census (Coefficients of Variation in Percentages) (Weighted by Initial Cohort Size) (Standard Errors of CV in Parentheses)				
Entry Cohort, Census year	Young Low Education	Young High Education	Old Low Education	Old High Education
<b>1955-59 cohort</b>				
At entry: 1960	46.6 (2.641)	37.2 (2.590)	39.0 (3.130)	39.3 (3.210)
Ten years later: 1970	21.1 (0.894)	17.5 (2.047)	24.9 (1.778)	18.3 (2.731)
<i>Change in CV</i>	-25.5	-19.7	-14.1	-21.0
<i>Percentage Change in CV</i>	-54.7	-52.9	-36.1	-53.4
<b>1965-69 cohort</b>				
At entry: 1970	28.9 (1.459)	34.7 (1.993)	30.6 (2.233)	39.6 (1.695)
Ten years later: 1980	25.3 (1.280)	18.4 (1.128)	24.3 (2.018)	29.5 (1.258)
<i>Change in CV</i>	-3.6	-16.3	-6.3	-10.1
<i>Percentage Change in CV</i>	-12.5	-47.0	-20.6	-25.5
Young: Aged 25-39 in the year of the first Census used in the comparison. Old: Aged 40-54 in the year of the first Census used in the comparison. Low Education: 1-12 years of schooling. High Education: greater than 12 years				

Table 3a: Changes in the Dispersion of Individual Earnings; U.S. -Born Men: 1960-1980 Decennial Census (Coefficients of Variation in Percentages)				
Age, Census year	Young Low Education	Young High Education	Old Low Education	Old High Education
25-54, 1960	63.5	81.5	73.6	71.7
35-64, 1970	68.7	64.9	83.9	71.3
<i>Change in CV</i>	5.2	-16.6	10.3	-0.4
<i>Percentage Change in CV</i>	8.2	-20.4	14.0	-.6
25-54, 1970	60.7	72.3	72.2	66.2
35-64, 1980	73.2	63.7	96.5	73.8
<i>Change in CV</i>	12.5	-8.6	24.3	7.6
<i>Percentage Change in CV</i>	20.6	-11.9	33.7	11.5
Young: aged 25-39 in the year of the first Census used in the comparison. Old: aged 40-54 in the year of the first Census used in the comparison. Low Education: 1-12 years of schooling. High Education: greater than 12 years				

Table 4: Predicted Emigration Biases on Measures of Earnings Convergence Among Country-of-Origin Groups		
<i>Pattern of Emigration</i>	Type of Analysis	
	Addition to R <sup>2</sup> Adding Country-of-Origin Variables	Dispersion of Median Earnings
Left or right tail of each country-of-origin earnings distribution. (The unsuccessful, or the successful, of each country-of-origin group emigrate.)	-	NO BIAS
Center of each country-of-origin earnings distribution.	+	NO BIAS
Right tail of the earnings distribution for low-earning country-of-origin groups and left tail of the earnings distribution for high-earning country-of-origin groups.	-	-
Right tail of the earnings distribution for high-earning country-of-origin groups and left tail of the earnings distribution for low-earning country-of-origin groups.	-	+
+ indicates emigration bias works to overstate country-of-origin convergence. - indicates emigration bias works to understate country-of-origin convergence.		

Table 5: Change in the Explanatory Power of Country of Origin: High and Low Emigration Country-of-Origin Groups (Bootstrap standard errors of R <sup>2</sup> in parentheses)								
	LOW EMIGRATION				HIGH EMIGRATION			
Entry Cohort, Census Year	R <sup>2</sup> for human-capital model	R <sup>2</sup> for human-capital model with country-of-origin variables	Change in R <sup>2</sup> from adding country-of-origin variables	Percentage change in R <sup>2</sup> from adding country-of-origin variables	R <sup>2</sup> for human-capital model	R <sup>2</sup> for human-capital model with country-of-origin variables	Change in R <sup>2</sup> from adding country-of-origin variables	Percentage change in R <sup>2</sup> from adding country-of-origin variables
<b><i>1955-59 cohort</i></b>								
At entry: 1960	.1108 (.0176)	.2458 (.0231)	.1283	109.2	.0620 (.0141)	.2460 (.0351)	.1840	296.7
Ten years later: 1970	.1109 (.0184)	.2061 (.0261)	.0952	85.8	.1449 (.0247)	.2111 (.0270)	.0662	45.7
<b><i>1965-69 cohort</i></b>								
At entry: 1970	.0265 (.0078)	.0912 (.0126)	.0647	244.1	.1151 (.0149)	.1943 (.0195)	.0792	68.8
Ten years later: 1980	.0992 (.0064)	.1250 (.0070)	.0258	26.0	.1546 (.0107)	.1914 (.0121)	.0368	23.8



Table 6: Changes in the Dispersion of Median Earnings Across Country-of-Origin Groups Divided by High and Low Emigration (Coefficients of Variation in Percentages) (Standard Error of CV in Parentheses)								
	Low Emigration				High Emigration			
	Young Low Education	Young High Education	Old Low Education	Old High Education	Young Low Education	Young High Education	Old Low Education	Old High Education
<b><i>1955-59 cohort</i></b>								
At entry: 1960	53.5 (3.25)	39.1 (3.99)	21.6 (3.42)	40.7 (6.02)	22.0 (4.50)	33.6 (3.68)	45.7 (5.79)	28.6 (3.83)
Ten years later: 1970	17.9 (0.99)	15.4 (1.49)	16.8 (1.30)	23.5 (4.45)	12.7 (2.95)	14.1 (3.33)	29.2 (2.78)	12.4 (1.25)
Change in CV	-35.6	-23.7	-4.8	-17.2	-9.3	-19.5	-16.5	-16.2
<b><i>1965-69 cohort</i></b>								
At entry: 1970	22.2 (1.89)	30.5 (2.19)	21.3 (3.57)	36.1 (6.64)	29.6 (2.92)	25.1 (1.37)	41.3 (2.74)	22.4 (2.08)
Ten years later: 1980	17.8 (1.63)	19.0 (1.25)	21.2 (3.70)	19.5 (2.33)	22.5 (1.17)	13.7 (1.31)	28.1 (2.15)	17.0 (0.98)
Change in CV	-4.4	-11.5	-0.1	-16.6	-7.1	-11.4	-13.2	-5.4

Table 7: Changes in the Explanatory Power of Country of Origin as Time in U.S. Increases: 1980-2000 Decennial Census Data (Bootstrap standard errors for R <sup>2</sup> in parentheses)				
Entry Cohort, Census year	R <sup>2</sup> for human-capital model	R <sup>2</sup> for human-capital model with country-of-origin variables	Change in R <sup>2</sup> from adding country-of-origin variables	Percentage change in R <sup>2</sup> from adding country-of-origin variables
<b>1975-79 cohort</b>				
At entry: 1980	0.085 (0.003)	0.165 (0.004)	0.080	94.80
Ten years later: 1990	0.192 (0.005)	0.256 (0.005)	0.065	33.68
<b>1985-89 cohort</b>				
At entry: 1990	0.112 (0.003)	0.213 (0.004)	0.101	90.85
Ten years later: 2000	0.161 (0.004)	0.233 (0.004)	0.072	44.93
The base model is the regression of individual log(earnings) on experience, experience <sup>2</sup> , and education. The second model adds region/country dummies and region/country interactions with education and experience for Africa, Britain, Canada, China/Taiwan, Cuba, Czechoslovakia, Germany, Greece, Hungary, India, Ireland, Islamic SW Asia, Italy, Jamaica, Japan, Korea, Mexico, Other Asia, Oceania, Other Communist Europe, Other Non-Communist Europe, Other Central America, Philippines, Poland, Portugal, S. America, and Yugoslavia. Estimates are based on the 1980, 1990, and 2000 Census of Population Public Use Samples. A 6% micro-data sample is created by combining and reweighting the 5% and 1% Public Use files for each census.				

Table 8: Changes in the Dispersion of Median Earnings Across Country of Origin as Time in U.S. Increases: 1980-2000 Decennial Census Data (Coefficients of Variation in Percentages) (Weighted by Initial Cohort Size) (Standard Errors of CV in Parentheses)				
Entry Cohort, Census year	Young Low Education	Young High Education	Old Low Education	Old High Education
<b>1975-79 cohort</b> At entry: 1980	34.6 (.035)	55.1 (.0343)	52.1 (.0500)	57.6 (.0182)
Ten years later: 1990	26.4 (.019)	23.1 (.0127)	28.6 (.0190)	32.5 (.0300)
Change in CV	-8.2	-32.0	-23.5	-25.1
Percentage Change in CV	-23.7	-58.1	-45.1	-43.6
<b>1985-89 cohort</b> At entry: 1990	5.9 (0.152)	8.4 (0.128)	9.2 (0.392)	12 (0.130)
Ten years later: 2000	4.6 (0.105)	5.6 (0.07)	5.5 (0.118)	6.9 (0.141)
Change in CV	-1.3	-2.8	-3.7	-5.1
Percentage Change in CV	-21.9	-33	-40.6	-42.4
Young: Aged 25-39 in the year of the first Census used in the comparison. Old: Aged 40-54 in the year of the first Census used in the comparison. Low Education: 1-12 years of schooling. High Education: greater than 12 years				

Figure 1: The Earnings Trajectories of Immigrants

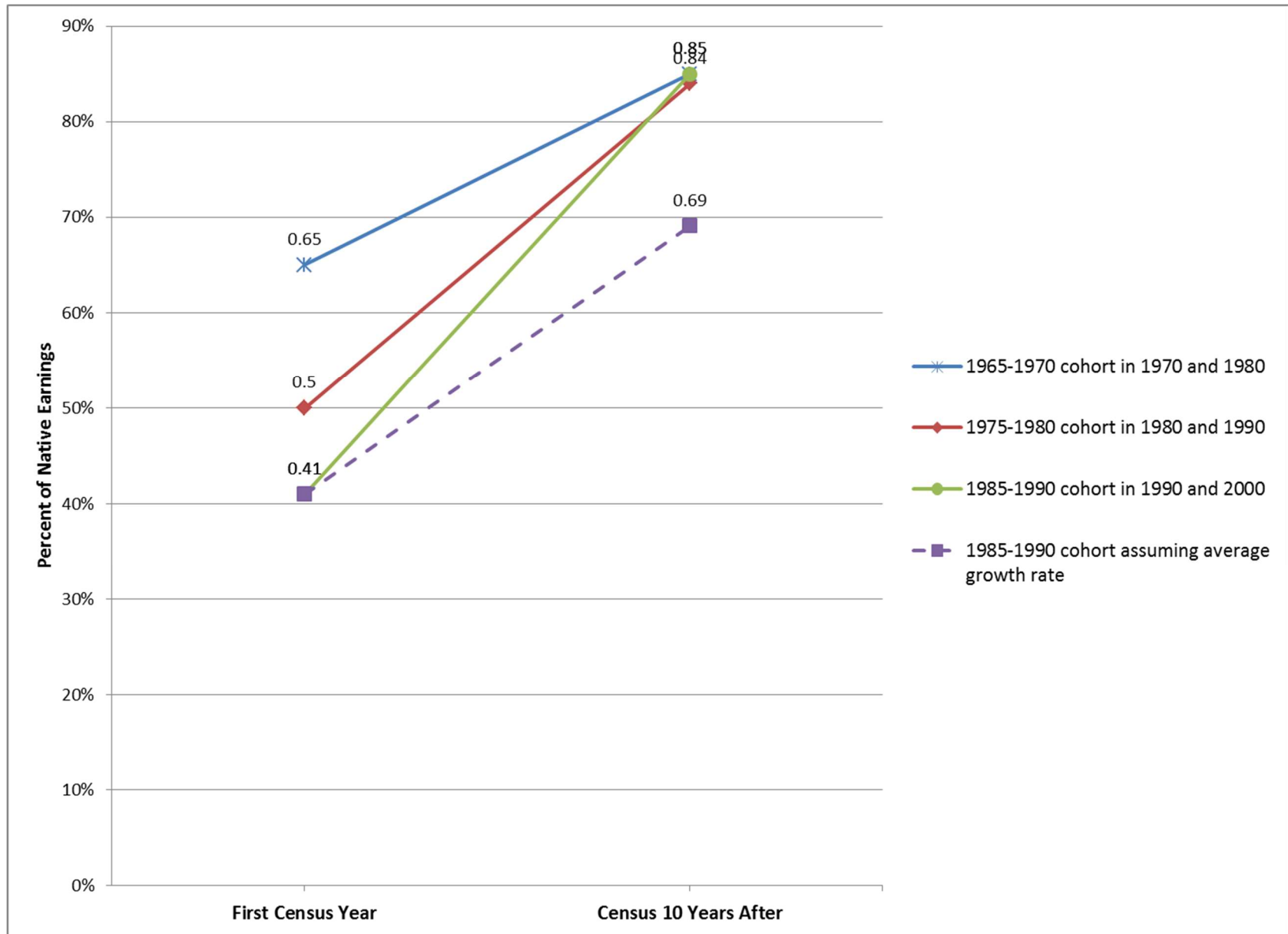


Figure 2: Hypothetical Emigration Patterns as a Function of the U.S. Earnings of Immigrants

Panel A



Panel B



Figure 3: Estimated In-School Rate in 2000 for Immigrants Entering 1993-1998 by Age and Probability of Family Visa

