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IMMIGRANT EARNINGS, RELATIVE TO WHAT? THE IMPORTANCE OF EARNINGS FUNCTION SPECIFICATION AND COMPARISON POINTS

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SUMMARY

Immigrant relative earnings estimates are sensitive to the choice of comparison point and the specification of earnings. Non-sample mean comparisons (Borjas, 1985) understate relative earnings. Simple earnings specifications (linear education, quadratic experience) overstate relative earnings for both poorly and well-educated immigrants. Specifications which ignore omitted variables understate the relative earnings of poorly educated immigrants and overstate those of well-educated ones. Although measures of assimilation and changes in immigrant quality are insensitive to earnings specification, they indicate strong earnings growth for post-1964 immigrants, an overall decrease in immigrant quality, and an increase in Mexican immigrant quality.

1. INTRODUCTION

A large volume of recent research concerns itself with the measurement of earnings differentials in the US population, usually focusing on immigrant/native-born, male/female and black/white earnings disparities. Typically, these studies control for human capital (education and experience) in a simple way, and count any residual differential as evidence of discrimination, assimilation or differences in unmeasurable skills. In this large literature there is surprisingly little discussion of earnings comparison methodology: at what point should earnings comparisons be made? How sensitive are they to the misspecification of the human capital function? In this paper we discuss several important econometric considerations in the measurement of earnings differentials, and demonstrate their empirical significance in a sample of US immigrants.

Earnings differentials are defined as the difference in the estimated earnings of two groups, calculated at a point chosen by the researcher. How should the point of comparison be chosen? This paper begins by providing some practical justifications for the conventional calculation of relative earnings at the mean of the immigrant/minority/female sample. Although the sample-mean comparison point does not have a rigorous theoretical basis, as a practical matter it captures aggregate group relative earnings in a way that alternative choices do not. The practical case for comparisons at the sample-mean is much stronger when the well-documented omitted variables problem ('ability bias') in earnings function estimates (Griliches, 1977) is taken into account. The use of immigrant/minority/female means eliminates the need to correct the bias in those samples, and enables us to concentrate on the more feasible task of correcting the bias in the control group/comparison sample.

Misspecification in the earnings function has implications for the choice of comparison

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point; it also has a more direct impact on estimated earnings differentials. In this paper we will address two types of misspecification: inadequately flexible functional forms and omitted variables. As Murphy and Welch (1990) show, the usual quadratic term in experience and linear term in education inadequately describe the human capital–earnings relationship. As we have already mentioned, left-out variables are an enduring problem in human capital studies (Griliches, 1977). Both types of misspecification matter for the measurement of relative earnings, particularly for well- and poorly educated groups.

To demonstrate the empirical consequences, both of the choice of comparison point and the correct specification of the human capital function, we will focus on the measurement of immigrant relative earnings. The literature on immigration is an ideal choice. At least one important paper in this field (Borjas, 1985) makes use of non-sample mean comparison points; all work to date (except Lalonde and Topel, 1990) inadequately controls for human capital, and none attempts to correct for omitted variables bias. Moreover, large differences in education and age across immigrant groups allow us to check the importance of specification across both well- and poorly educated samples.

The work of Borjas (in particular, 1985 and 1987) suggests that more recent immigrants to the USA (post-1964) have lower relative wages, and slower relative wage growth, than earlier immigrants. He attributes this slowdown to the 1964 changes in the immigration law, which de-emphasized skills in favour of family reunification, and redistributed visas towards Third World immigrants.

Several researchers have questioned this result. Jasso and Rosenzweig (1990) emphasize the sensitivity of Borjas's results to the exclusion of certain immigrant groups and remigrants (Borjas, 1990, is a response); Lalonde and Topel (1990) control for changes in the income distribution from 1970 to 1980, and Yuengert (1991) incorporates self-employed workers into the estimates.

The sensitivity analysis presented here suggests that the choice of comparison point and misspecification in the human capital earnings function have important effects on relative earnings estimates. The choice by Borjas (1985) of non-sample mean comparison points tends to understate immigrant relative earnings. Linear education specifications tend to overstate relative earnings for both poorly and well-educated immigrants. Specifications which do not control for omitted variables tend to understate the relative earnings of poorly educated immigrant groups, and overstate those of well-educated ones. The net effect of these biases, which sometimes work in different directions, is a decrease in estimated relative earnings for poorly educated immigrant groups and a larger decrease for the most highly educated ones.

While estimates of relative earnings are sensitive to the misspecification addressed in this paper, estimates of relative earnings growth and changes in immigrant *quality* are relatively insensitive. Nevertheless, the estimates provide only weak evidence that the 1964 changes in the law on immigration had any impact on immigrant relative economic performance. Assimilation rates are large for post-1964 immigrants, and Mexican immigrant quality increased after 1964. However, overall immigrant quality appears to have declined.

2. MEASURING RELATIVE EARNINGS

Assume that the log earnings of native individual i, Y_{iN}, and the log earnings of individual i of immigrant group I, Y_{iI}, are given by

$$Y_{iN} = X_{iN}\beta_N + \varepsilon_{iN} \tag{1}$$

$$Y_{iI} = X_{iI}\beta_I + \varepsilon_{iI} \tag{2}$$

where β_j $(j = N, I)$ are parameters and ε_{ij} $(j = N, I)$ are white-noise errors. X_{ij} $(j = N, I)$ includes a vector of education and experience variables. For now, assume that ε_{ij} and X_{ij} are uncorrelated.

The first step in the calculation of relative earnings is the estimation of the parameters in equations (1) and (2). Figure 1 represents the two estimated lines. The second step is to choose a point of comparison, and calculate the vertical distance between the two lines. Not surprisingly, different comparison points will yield different estimates. In some cases the estimates may be of different signs if the estimated lines cross. How do we choose the comparison point?

The researcher need not choose a single point of comparison, but may instead choose an array of points, and express relative earnings as a weighted average. A general expression for relative earnings is given by

$$RE_I = \int_X [\hat{Y}_I(X) - \hat{Y}_N(X)]\, w(X)\, dX \tag{3}$$

This equation defines immigrant relative earnings as a weighted average of the earnings differentials between immigrants of different education and experience (X) and similar native workers. $w(X)$ is the weight attached to each value of X. To choose a single point of comparison, set $w = 1$ at that point and $w = 0$ elsewhere.

The most popular comparison point is the sample mean of the immigrant group $(\bar{X}_I)$; this corresponds to the use of weights $w(X) = f_I(X)$, where $f_I(X)$ is the joint density of the vector X_I. Substitution into equation (3) yields

$$\begin{aligned} RE_I &= \int_X [\hat{Y}_I(X) - \hat{Y}_N(X)]\, f_I(X)\, dX \\ &= \bar{X}_I\hat{\beta}_I - \bar{X}_I\hat{\beta}_N \\ &= \bar{Y}_I - \bar{X}_I\hat{\beta}_N \end{aligned} \tag{4}$$

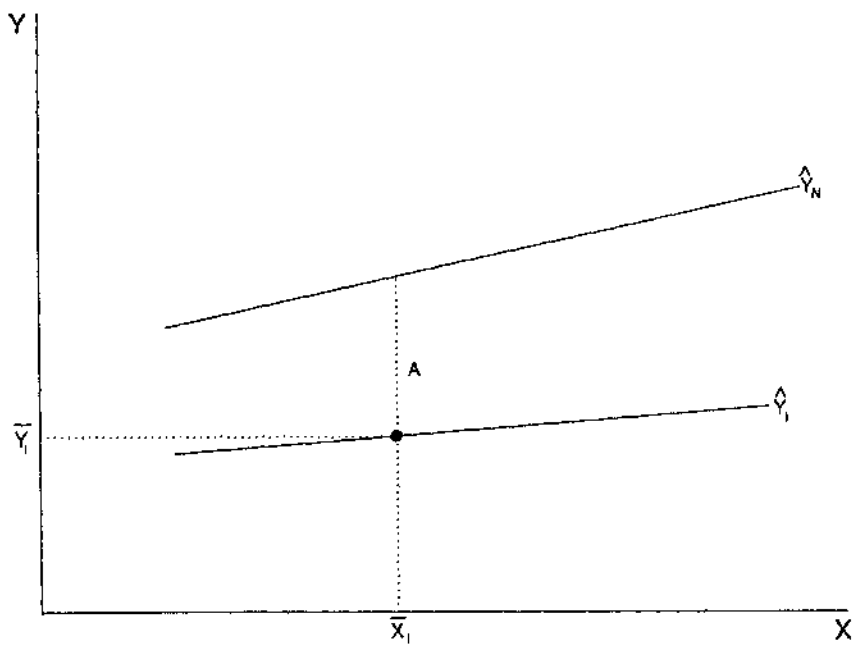

Figure 1

In Figure 1 relative earnings are given by line segment A. The immigrant sample mean has two desirable qualities as a comparison point, neither of which are shared by alternative choices. First, it is descriptive of the entire cohort: it most exactly signifies what we mean when we say 'immigrants do relatively well' or 'immigrants do relatively poorly'. For this reason, X_I has been an automatic choice in most cases. Second, and more importantly, the use of the immigrant sample mean summarizes aggregate relative performance. Because it is equivalent to a weighted average, its use gives us a sense of how the total earnings of the group compare to native earnings. Poor relative earnings of one segment of the immigrant group (for example, the less educated) are balanced against better relative performance by other segments (the well educated). This avoids misleading statements about an entire group's relative earnings, based on an unbalanced focus on one segment of the immigrant sample.

The practical advantages of earnings comparisons at the immigrant sample mean become much stronger when ε_{ij} $(j = I, N)$ is correlated with the regressors. To confirm this, we consider the last line of equation (4). When the sample-mean comparison point is used, all that is needed for the relative earnings calculation are mean earnings and the means of the right-hand side variables for the immigrant group ($\bar{Y}_I$ and $\bar{X}_I$) and estimates of the coefficients of the native earnings function (β_N). It is not necessary to estimate an immigrant education–earnings profile; we can instead concentrate on the consistent estimation of the native-born profile. If any other comparison point is used, consistent estimates of β_I will be necessary. The relevant controls for omitted ability (measures of ability, education relative to some reference group) are unlikely to be available for the often small immigrant samples. As we will see, omitted variables are a cause for concern in the analysis of immigrant relative earnings;[1] their presence makes the practical case for comparisons at sample means stronger.[2]

The comparison point is rarely an issue in cross-section studies of relative earnings: sample mean comparison points are conventional. On the other hand, when calculating immigrant earnings *growth*, other points are often chosen in an attempt to control for human capital across time. For example, consider Figure 2. The lines marked NAT70, NAT80, IMM70 and IMM80 represent the 1970 and 1980 native and immigrant education–earnings profiles, respectively. Relative earnings growth might be calculated in one of two ways. Borjas (1985) measures it as the growth of immigrant predicted earnings, calculated at $\bar{X}_{I,80}$ (B + C in Figure 2), minus the predicted earnings growth of a native worker at $\bar{X}_{I,80}$ (A + B in Figure 2). Relative earnings growth is C − A.

This measure of relative earnings growth does not capture the aggregate experience of the entire group. It compares the earnings of a representative immigrant in 1980 with those of relatively well-educated immigrants in 1970, whose earnings may be unrepresentative of the group. A measure of relative earnings growth that more accurately captures the aggregate experience of the immigrant group is D − A. This measure compares relative earnings of a representative immigrant in 1970 to those of one in 1980. Omitted variables bias in this sample also makes the use of the 1980 mean in the 1970 calculations problematic, because it depends on consistent estimates of $\beta_{I,70}$. Although the two measures seem only slightly different

[1] As Section 4 will demonstrate, the omitted variables bias in the native sample is on the order 20–30%. Because it is unclear how to correct for any potentially omitted variables in the immigrant sample it is not possible to test for them. In light of the presence of omitted variables bias in the native sample it is prudent to assume that the same problem exists in the immigrant samples.

[2] One could use the native mean as a comparison point, which entails the use of the weights $w(X) = f_N(X)$. This point has the same desirable properties as the immigrant mean; it does not, however, avoid the necessity of generating consistent estimates of the immigrant earnings profile (β_I). We know of no research that calculates relative earnings at this point.

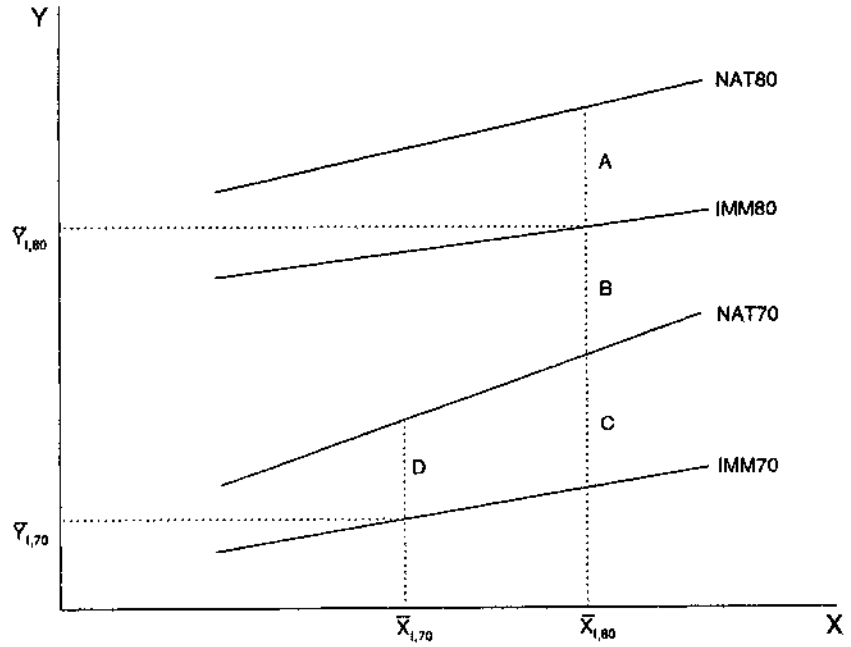

Figure 2

(comparing earnings growth rates of immigrants and natives versus comparing relative earnings at two points in time), the two measures can yield significantly different results when $X_{I,70}$ and $X_{I,80}$ are not equal, as we will see in Section 4.[3]

It is also clear from the last line of equation (4) that relative earnings estimates are sensitive to the specification of the native earnings profile and its consistent estimation. Conventional estimates of earnings profiles are deficient in two respects. They ignore both important non-linearities in the return to education and experience and omitted variables bias on the return to education. In the next section we address these issues.

3. ESTIMATES OF NATIVE EDUCATION–EARNINGS PROFILES

3.1. Methodology

In this section two modifications are made to the simple human capital specification: the new specification takes into account certain important non-linearities in education–earnings and experience–earnings profiles, as well as the effects of omitted variables, or 'ability'. Both modifications change the shape of native earnings profiles in important ways.

In the log-earnings regressions typical in the literature on immigrant assimilation, education enters linearly and experience quadratically. As Murphy and Welch (1990) and Lalonde and Topel (1990) stress, this simple specification is an inadequate description of

[3] After calculating the growth rate of relative earnings (D – A) it seems natural to decompose it into growth due to changes in immigrant characteristics X_I and growth due to changes in the parameters of the earnings functions. In order to calculate this decomposition, we will need to address the omitted variables bias in the immigrant samples. This sort of project is undoubtedly worth trying, but it is beyond the scope of this paper, which is the consistent estimation of relative earnings and relative earnings growth.

education–earnings and experience–earnings profiles. There are significant differences in the return to education across grammar school, high school and college. Moreover, there are important degree effects. A more appropriate specification of the return to education is a series of linear splines with degree dummies. A more appropriate specification of the return to experience is a quartic. In addition, there are important interactions between education and the experience–earnings profile.

Their failure to account for non-linearities is not the only shortcoming of standard specifications. A large literature suggests that unobserved variables ('ability') may bias OLS estimates of returns to schooling (e.g. Griliches, 1977); biases in the estimates of these coefficients may, in turn, bias OLS estimates of relative earnings.

Consider the following two-equation model:

$$Y_{iC} = X_{iC}\beta + A_{iC} + \varepsilon_{iC} \tag{4}$$

$$E_{iC} = E_C + A_{iC}\gamma + \nu_{iC} \tag{5}$$

where Y_{iC} is log earnings of individual i of cohort C (the cohorts are defined below). X_{iC} is a vector of explanatory variables, including a vector of education variables, A_{iC} is the omitted variable, 'ability', E_{iC}, is education, and E_C is mean education for cohort C. β and γ are parameters (assumed constant across cohorts) and ε_{iC} and ν_{iC} are white-noise errors. Because education and ability are correlated, OLS estimates of β will be inconsistent. Substituting from equation (5) into equation (4), we get

$$Y_{iC} = X_{iC}\beta + (1/\gamma)(E_{iC} - E_C) + \varepsilon_{iC} - \frac{\nu_{iC}}{\gamma} \tag{6}$$

The term $(E_{iC} - E_C)$ in equation (6) is an index of relative education, which is taken as a proxy for ability.[4] The index controls for patterns in earnings among the more and less educated members of each cohort. To identify the system, the cohorts for the analysis must be defined on at least one variable not included in X. We define the cohorts by age and state (or state group) of birth. Table AI in the Appendix lists the state groups. There are a total of 665 cohorts (19 state groups times 35 age groups). We assume that persons born in the same state in the same year have the same expected ability ($A_C = 0$, $\forall C$).[5]

The exclusion of controls for place of birth raises one complication: if education quality varies across states, then both cohort ability and the index of relative education will be functions of education quality.[6] We can control separately for education quality by including interactions of education and experience with a vector of education quality variables. As indexes of education quality, we chose one expenditure and two input variables: log teachers' average wage, log student–teacher ratios, and log average term length. The use of interactions allows the effects of quality to vary across different levels of education.[7] All quality variables are demeaned.

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[4] This is similar to the use of aptitude scores or other independent measures of ability. However, it implies neither a positive nor a negative relationship between ability and education.

[5] An alternative approach is instrumental variables, which would replace the education variables in X in equation (4) by their cohort mean values. Such an approach would avoid the assumption that the correlation between ability and education (γ) is equal across cohorts, but at the cost of less efficient estimates. Since most cohort means are clustered between 10 and 13 years it would be impossible to estimate the entire education earnings profile.

[6] There is another possible complication: if current state of residence is correlated with state of birth, the cohorts may pick up residual price variation across region. To test this, we deflated earnings by regional price indices; the estimates were unaffected.

[7] Card and Krueger (1992) model the effect of quality on earnings somewhat differently. They allow the rate of return to education to vary across state-age cohorts, and model education quality as a fixed effect.

3.2. Estimates of Education–Experience–Earnings Profiles

The sample consists of native male full-time workers, age 26–60, in the continental United States, drawn from the 1970 and 1980 US Census Public Use Samples. Data on education quality are taken from Card and Krueger (1992). The quality variables are age- and state-specific.[8] Three specifications are estimated: linear (linear in education and quadratic in experience), non-linear (linear splines in education and a quartic in experience), and ability-adjusted (the non-linear model plus an index of relative education to capture differences in ability). Earnings profiles are estimated separately for 1970 and 1980. Table I presents the estimates for 1970.

The first column of the table presents the simple linear model. The return to education is 7·6% and the experience–earnings profile has a familiar concave shape. The second column shows the estimates of a linear spline in education and a quartic in experience. It also includes education–experience interactions. Note that the return to education is negligible (though significant) in grammar schools and largest in college. There are also important degree effects (sometimes large and negative because of the interactions with experience). The simple specification of column 1 is rejected ($F(17; 44004) = 92{\cdot}37$).

Table I. Coefficient estimates, log earnings equations, 1970, native workers

Variable	Linear model	Non-linear model	Controls for ability
Constant	0·65 (492)	9·26 (46·5)	8·95 (42·9)
Education	0·0760 (83·1)	—	—
Years 1–8	—	0·0138 (2·30)	0·0449 (6·10)
Grammar school graduate	—	0·0590 (3·28)	0·0315 (1·70)
Years 9–12	—	0·279 (6·87)	0·0528 (9·10)
High school graduate	—	−0·692 (3·14)	−0·747 (3·36)
Years 13–16	—	0·101 (25·3)	0·133 (22·6)
College graduate	—	0·292 (2·60)	0·326 (2·69)
Years of graduate school	—	0·0263 (1·56)	0·0561 (3·20)
18 years of education	—	0·134 (4·14)	0·126 (3·86)
Experience	0·0170 (16·2)	−0·0004 (0·01)	−0·0092 (0·28)
Experience^2/100	−0·0278 (13·5)	0·0901 (0·49)	0·150 (0·81)
Experience^3/1000	—	−0·0334 (0·76)	−0·0464 (1·06)
Experience^4/10000	—	0·0032 (0·86)	0·0042 (1·13)
Interactions:			
HS × EXP	—	0·153 (4·00)	0·160 (4·14)
HS × EXP^2/100	—	−1·04 (4·41)	−1·067 (4·49)
HS × EXP^3/1000	—	0·285 (4·64)	0·288 (4·65)
HS × EXP^4/10000	—	−0·0274 (4·74)	−0·0273 (4·68)
COLL × EXP	—	−0·0717 (2·62)	−0·0857 (2·92)
COLL × EXP^2/100	—	0·679 (2·96)	0·804 (3·32)
COLL × EXP^3/1000	—	−0·226 (2·88)	−0·269 (3·28)
COLL × EXP^4/10000	—	0·0244 (2·59)	0·0294 (3·02)
Education-ED$_C$	—	—	−0·285 (6·69)
R^2	0·155	0·184	0·196
N	44025	44025	44025

t-statistics in parentheses.

[8] See Card and Krueger (1923) for details.

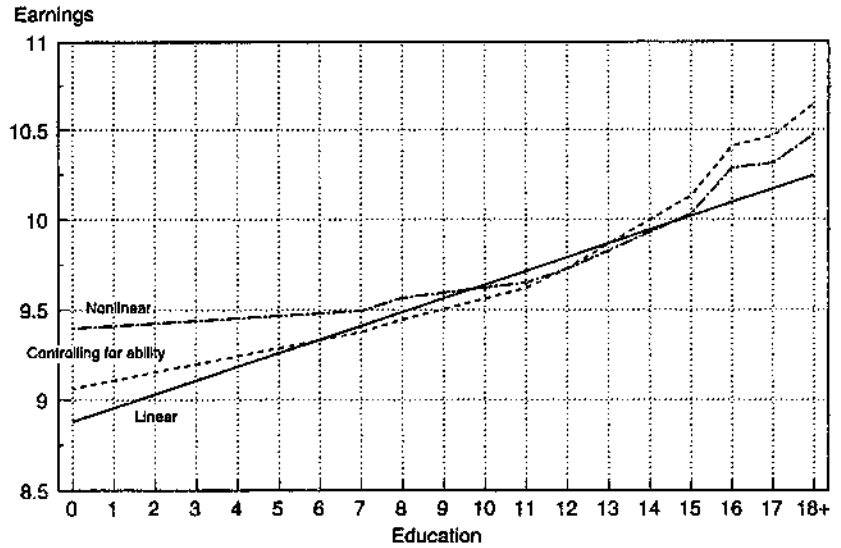

Figure 3. Estimated log earnings profiles, 1970 (experience = 20)

Table II. Coefficient estimates, log earnings equations, 1980, native workers

Variable	Linear model	Non-linear model	Controls for ability
Constant	8·45 (612)	8·93 (32·0)	8·23 (28·7)
Education	0·0781 (108)	—	—
Years 1–8	—	−0·159 (0·84)	0·0575 (2·84)
Grammar school graduate	—	0·0746 (3·62)	−0·220 (0·09)
Years 9–12	—	0·0716 (5·07)	0·0118 (7·57)
High school graduate	—	−0·140 (0·64)	−0·0769 (0·35)
Years 13–16	—	0·0835 (13·1)	0·126 (15·6)
College graduate	—	−0·0250 (0·35)	−0·0141 (1·79)
Years of graduate school	—	0·105 (16·8)	0·144 (17·2)
20 years of education	—	0·0851 (4·79)	0·0910 (4·94)
Experience	0·0314 (39·8)	0·0747 (2·04)	0·0702 (1·90)
Experience^2/100	−0·0480 (29·3)	−0·354 (1·73)	−0·302 (1·47)
Experience^3/1000	—	0·0840 (1·70)	0·0764 (1·53)
Experience^4/10000	—	−0·0078 (1·82)	−0·0075 (1·74)
Interactions:			
HS × EXP	—	0·0294 (0·78)	0·0095 (0·25)
HS × EXP^2/100	—	−0·142 (0·62)	0·0226 (0·10)
HS × EXP^3/1000	—	0·0287 (0·49)	−0·0231 (0·39)
HS × EXP^4/10000	—	−0·0022 (0·41)	0·0033 (0·60)
COLL × EXP	—	0·0019 (0·11)	0·0260 (1·38)
COLL × EXP^2/100	—	0·105 (0·72)	−0·0476 (0·30)
COLL × EXP^3/1000	—	−0·0507 (1·01)	0·0115 (0·21)
COLL × EXP^4/10000	—	0·0063 (1·04)	0·0029 (0·45)
ED × EXP/100	—	0·0348 (0·70)	0·0271 (0·54)
GM × ED × EXP/100	—	−0·152 (3·46)	−0·158 (3·22)
HS × ED × EXP/100	—	0·0192 (0·67)	0·0024 (0·08)
COLL × ED × EXP/100	—	−0·244 (8·03)	−0·239 (7·04)
Education-ED$_C$	—	—	−0·0410 (9·21)
R^2	0·134	0·152	0·157
N	82006	82006	82006

t-statistics in parentheses.

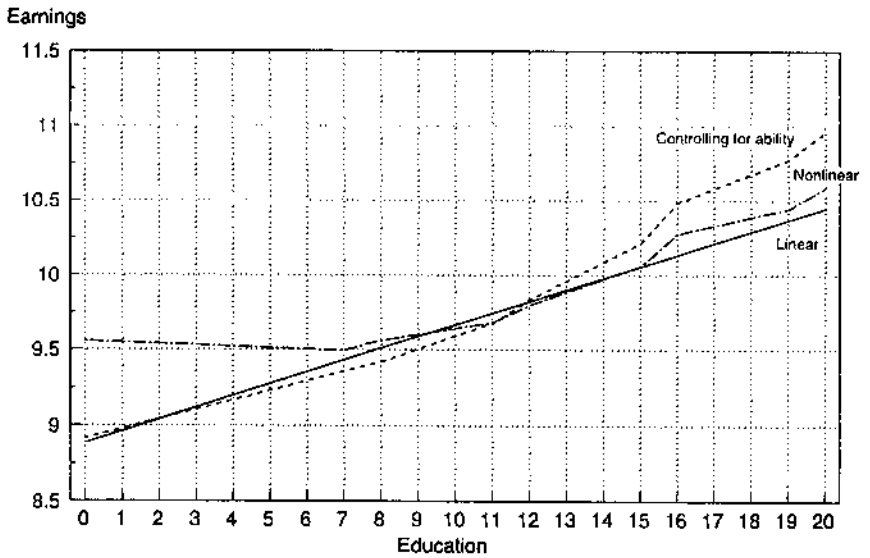

Figure 4. Estimated log earnings profiles, 1980 (experience = 20)

Column three controls for education quality (via interactions of the education variables with education quality variable, as mentioned above[9]) and adds the index of relative education. The estimated coefficient on relative education is significantly negative—ability is negatively correlated with relative education. This result is consistent with the analysis of Griliches (1977), and suggests that comparatively better job market opportunities induce early school exit. Controlling for ability, the return on education increases at every level by roughly 2·9%.[10]

Figure 3 shows the estimated education–earnings profile for 20 years experience. It is workers in the tails of the education distribution whose earnings are most severely mismeasured by conventional specifications. The linear specification underpredicts earnings for both well- and poorly educated workers. Taking the ability-adjusted line as the true profile, the non-linear model over-adjusts earnings for poorly educated workers and under-adjusts for well-educated ones.

Table II shows the estimates of the earnings profile for 1980. The results are similar to the 1970 estimates: the simple model is rejected ($F(21;\ 81\,981) = 85{\cdot}63$), and the inclusion of relative education increases the rate of return on education at all levels by about 4·1%. Figure 4 shows the estimated profiles. The results are similar to those in Figure 3: simple linear specifications underpredict earnings for both well- and poorly educated workers. Taking the ability-adjusted line as the true profile, the non-linear model over-adjusts earnings for poorly educated workers and under-adjusts for well-educated ones.

[9] The estimated earnings profiles were essentially unaffected by the controls for education quality. The coefficient estimates for the quality interactions are reported in Table AII in the Appendix. In each year, the estimated coefficients are jointly significant and of the expected sign.

[10] A richer specifications of the index of relative education (a vector of dummies for each of 18 one-year relative education cells) suggests that a simple linear term is adequate.

4. ESTIMATES OF IMMIGRANT RELATIVE EARNINGS

With consistent estimates of the education–experience–earnings profiles in hand we can construct consistent estimates of immigrant relative earnings. Table III lists the native and immigrant means, by year of immigration; it also includes several of the immigrant groups used in the analysis.[11] To qualify, each country of birth-year of immigration group had to be large enough (at least 100 observations in 1970 and 1980) to yield tight estimates of sample means. The sample selection rules are otherwise similar to those for the native sample. The immigrant groups detailed in Table III represent the extremes of the education distribution. The Portuguese and Mexicans have the least education and the Filipinos and Indians have the most. Relative earnings estimates should be most sensitive to the specification of the earnings profiles for both well- and poorly educated groups.

We begin the empirical analysis by assessing the importance of the comparison point in Table IV. This table presents relative earnings estimates for 1970, using the simple linear specification of Table III. The first three lines show relative earnings for all immigrants,

Table III. 1970 and 1980 immigrant group means (standard deviation)

Country	Year of immigration	1970 Means				1980 Means			
		N	ln *Y*	Exp	Educ	*N*	ln *Y*	Exp	Educ
Native-born		44025	9·8	24·9	11·9	82006	9·9	22·2	13·0
			(0·55)	(10·7)	(3·0)		(0·58)	(11·1)	(2·9)
All imigrants	1965–9	7033	9·7	20·4	11·2	22266	9·9	22·3	11·7
			(0·52)	(10·9)	(5·0)		(0·59)	(10·2)	(5·1)
	1960–64	6838	9·8	21·6	11·0	17170	10·0	23·8	12·2
			(0·51)	(10·1)	(4·6)		(0·59)	(10·3)	(4·7)
	1950–59	12954	9·9	25·4	10·8	27312	10·1	25·7	12·3
			(0·50)	(10·2)	(4·3)		(0·55)	(11·4)	(4·3)

Selected immigrant groups	Year of immigration	1970 Means				1980 Means			
		N	ln *Y*	Exp	Educ	*N*	ln *Y*	Exp	Educ
Portugal	1965–9	184	9·5	29·1	5·2	613	9·8	29·2	6·4
Mexico	1965–9	543	9·3	23·1	6·3	3892	9·6	22·7	7·1
	1960–64	729	9·4	24·5	6·2	2733	9·7	26·9	7·4
	1950–59	1315	9·5	27·3	6·8	3667	9·8	28·7	8·6
Italy	1965–9	505	9·6	26·0	7·1	1155	9·9	24·6	9·2
	1960–64	516	9·8	25·2	7·5	997	10·0	26·6	9·7
	1950–59	1234	9·8	27·3	8·0	2273	10·0	26·8	10·6
France	1950–59	140	10·0	21·7	12·4	330	10·1	22·9	13·4
UK	1965–9	518	10·1	16·5	13·8	857	10·4	22·0	14·9
	1960–64	420	10·1	20·9	13·2	819	10·3	24·0	14·3
	1950–59	765	10·1	24·7	12·8	1506	10·2	24·3	13·9
Philippines	1965–9	282	9·7	14·9	14·9	1329	10·0	19·1	14·5
India	1965–9	176	9·9	10·9	16·6	830	10·5	14·7	18·5

[11] Table AIII in the Appendix lists all of the immigrant groups, along with their means.

grouped only by year of immigration. The rest of the table shows the estimates for various immigrant groups, ordered by average education. The least-educated immigrant groups are at the top and the most highly educated are at the bottom. The first column in Table IV shows relative earnings estimates calculated at immigrant mean education and experience in 1980 (as in Borjas, 1985).[12] This corresponds to the distance C in Figure 2. The second column shows relative earnings calculated at the 1970 immigrant mean (distance D in Figure 2). Bold numbers indicate a large change (0·05 or more) from the previous column.

Let us begin with the first three lines. The estimates of column two are 2–4 percentage points larger than those of column one. The aggregate estimates mask the sensitivity of the immigrant group estimates. Relative earnings estimates change by more than 5 percentage points for 21 of the 38 immigrant groups. In general, relative earnings estimates increase from column one to column two.

Table IV shows that the choice of comparison point has important empirical consequences. By using the 1980 means in column one we essentially calculate relative earnings for the better-educated portions of the immigrant groups in 1970 (mean education usually increased over the 1970s). Relative earnings, calculated at the more representative 1970 sample means, are consistently higher. For the reasons given in Section 2 (sample means are more representative of the immigrant group, they have a useful aggregate interpretation, and they are insensitive to bias in the immigrant earnings function), we will calculate relative earnings at sample means for the rest of the paper.

Table V presents relative earnings estimates for 1970, for different specifications of the earnings function. Column one is the simplest specification (linear in education, quadratic in experience). It is identical to column two in the previous table. Column two incorporates the non-linear estimates of the earnings profiles (linear splines in education, quartic in experience). Column three incorporates the ability-adjusted profile.[13]

The first three lines show relative earnings for all immigrants by year of immigration. The estimates of the nonlinear model (column two) are 6–8 percentage points lower than the linear model estimates of column one. The estimates increase somewhat as we go to the ability-adjusted estimates of column three. The net effect (from column one to column three) is negative.

The relative earnings estimates for the various immigrant groups shed more light on the nature of the specification biases. Note first that, as expected, it is the poorly and well-educated immigrant groups, who appear at the top and bottom of the table, whose relative earnings are most sensitive to specification (as indicated by the bold type). For the poorly educated immigrant groups in the top half of the table the non-linear specification decreases relative earnings sharply, while the ability-adjusted estimates increase somewhat. Mexico 1965–9 (second line) is typical; the estimate decreases from −8% to −27% from column one to column two; the substantial increase in the estimate of column three (to −12%) does not fully counter the decrease to column two. The relative earnings estimates of the two highly educated immigrant groups at the bottom of the table, Philippines 1965–9 and India 1965–9, are also sensitive to the specification of the earnings profile. As predicted, the estimates fall sharply when non-linearities are taken into account, and fall further when the model controls for ability.

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[12] Note that, for the estimates in the first column, we must estimate the parameters of the various immigrant earnings functions. The expression for the estimates, analogous to the last line of equation (4), is

$$RE_J = \bar{X}_{I,70}\hat{\beta}_{I,70} - \bar{X}_{I,80}\hat{\beta}_{N,70}$$

[13] In calculating relative earnings, ability ($E_{ic} - E_c$) and education quality are set to zero.

Table VI shows that relative earnings estimates in 1980 are also sensitive to the specification of the native earnings profile, particularly for immigrant groups with low and high levels of education. For the poorly educated immigrant groups, accounting for non-linearities sharply lowers relative earnings estimates and accounting for ability increases them (for example, see Portugal 1965–9 on the first line). For the highly educated groups, both non-linearities in the

Table IV. Relative earnings estimates, linear model, 1970

Immigrant group		Comparison point: 1980 Immigrant Mean (Borjas)	Comparison point: 1970 Immigrant Mean
1965–9		−0·042 (6·11)	−0·024 (3·66)
1960–64		0·038 (11·9)	0·120 (18·8)
1950–59		0·183 (33·7)	0·225 (46·7)
Portugal	1965–69	0·142 (4·39)	**0·224 (7·37)**
Mexico	1965–9	−0·110 (5·63)	−0·079 (4·10)
	1960–64	0·040 (2·21)	**0·092 (5·65)**
	1950–59	0·069 (4·63)	**0·144 (11·0)**
Italy	1965–9	0·081 (3·98)	**0·198 (10·6)**
	1960–64	0·180 (8·52)	**0·320 (16·0)**
	1950–59	0·186 (12·6)	**0·336 (25·7)**
Dom. Rep.	1960–64	−0·129 (2·10)	**−0·068 (1·72)**
Greece	1965–9	−0·054 (1·62)	−0·016 (0·60)
	1950–59	0·176 (5·76)	0·224 (9·35)
Yugoslavia	1965–9	0·001 (0·02)	**0·052 (1·33)**
	1950–59	0·229 (9·07)	**0·338 (17·0)**
Poland	1965–9	−0·033 (0·85)	**0·052 (1·34)**
	1960–64	0·139 (4·44)	**0·232 (8·24)**
	1950–59	0·149 (7·64)	0·249 (17·0)
Cuba	1965–9	−0·232 (13·0)	**−0·174 (12·1)**
	1960–64	−0·118 (7·66)	**−0·065 (4·61)**
	1950–59	−0·075 (3·20)	**0·007 (0·31)**
Jamaica	1965–69	−0·176 (3·97)	−0·144 (4·12)
Ireland	1960–64	0·043 (1·00)	**0·173 (4·86)**
	1950–59	0·081 (2·59)	**0·219 (8·84)**
Colombia	1965–9	−0·163 (3·75)	**−0·089 (2·70)**
	1960–64	−0·087 (1·72)	**−0·028 (0·62)**
Canada	1965–9	0·247 (9·30)	0·251 (11·5)
	1960–64	0·264 (11·5)	0·298 (14·1)
	1950–59	0·266 (15·9)	0·309 (22·4)
Argentina	1965–9	−0·030 (0·49)	−0·005 (0·11)
Germany	1965–9	0·139 (4·41)	0·151 (4·83)
	1960–64	0·229 (9·51)	**0·278 (13·1)**
	1950–59	0·209 (15·2)	**0·282 (23·5)**
Hungary	1950–59	0·173 (7·38)	0·215 (11·8)
Netherlands	1950–59	0·170 (5·84)	**0·226 (9·32)**
France	1950–59	0·151 (3·20)	0·196 (4·98)
UK	1965–9	0·230 (9·43)	0·221 (11·4)
	1960–64	0·258 (10·4)	0·266 (12·8)
	1950–59	0·231 (12·6)	0·262 (16·8)
Philippines	1965–9	−0·275 (8·05)	−0·273 (10·4)
India	1965–9	−0·145 (2·43)	−0·115 (3·14)

t-statistics in parentheses.

earnings function and the adjustment for ability work in the same direction, decreasing relative earnings estimates.

Tables IV–VI suggest strongly that the specification of the education–experience–earnings profile matters for the measurement of relative earnings. The choice of the comparison point, non-linearities in the earnings profile, and omitted variables all affect relative earnings estimates, especially for immigrant groups with high and low levels of education.

However, researchers into immigrant earnings are interested in more than relative earnings.

Table V. Relative earnings estimates, 1970

Immigrant group		Linear model	Non-linear model	Controls for ability
1965–9		−0·024 (3·66)	**−0·103 (14·6)**	−0·077 (9·04)
1960–64		0·120 (18·8)	**0·048 (7·17)**	0·077 (9·19)
1950–59		0·225 (46·7)	**0·161 (31·0)**	0·188 (16·5)
Portugal	1965–9	0·224 (7·37)	**−0·004 (0·15)**	**0·165 (3·68)**
Mexico	1965–9	−0·079 (4·10)	**−0·270 (13·5)**	**−0·119 (4·53)**
	1960–64	0·092 (5·65)	**−0·094 (5·34)**	**0·054 (1·18)**
	1950–59	0·144 (11·0)	**−0·021 (1·46)**	**0·107 (3·69)**
Italy	1965–9	0·198 (10·6)	**0·041 (2·26)**	**0·164 (5·50)**
	1960–64	0·320 (16·0)	**0·184 (9·71)**	**0·297 (10·5)**
	1950–59	0·336 (25·7)	**0·225 (17·8)**	**0·321 (15·4)**
Dom. Rep.	1960–64	−0·068 (1·72)	**−0·170 (4·71)**	**−0·064 (1·99)**
Grece	1965–69	−0·016 (0·60)	**−0·101 (3·95)**	**−0·019 (1·08)**
	1950–59	0·224 (9·35)	**0·160 (6·85)**	0·196 (7·60)
Yugoslavia	1965–9	0·052 (1·33)	**−0·034 (0·94)**	**0·016 (0·13)**
	1950–59	0·338 (17·0)	**0·264 (13·9)**	**0·320 (14·0)**
Poland	1965–9	0·052 (1·34)	−0·020 (0·51)	0·020 (0·31)
	1960–64	0·232 (8·24)	**0·178 (6·45)**	**0·230 (7·34)**
	1950–59	0·249 (17·0)	**0·196 (13·8)**	0·233 (13·9)
Cuba	1965–9	−0·174 (12·1)	**−0·249 (17·7)**	**−0·196 (11·6)**
	1960–64	−0·065 (4·61)	−0·115 (8·20)	−0·114 (7·90)
	1950–59	0·007 (0·31)	−0·035 (1·66)	0·005 (0·04)
Jamaica	1965–69	−0·144 (4·12)	−0·177 (5·17)	−0·136 (4·10)
Ireland	1960–64	0·173 (4·86)	0·147 (4·09)	0·193 (4·60)
	1950–59	0·219 (8·84)	0·192 (7·69)	0·222 (8·04)
Colombia	1965–9	−0·089 (2·70)	**−0·133 (4·28)**	−0·094 (3·15)
	1960–64	−0·028 (0·62)	−0·064 (1·55)	−0·053 (1·39)
Canada	1965–9	0·251 (11·5)	0·231 (10·6)	0·227 (9·65)
	1960–64	0·298 (14·1)	0·259 (12·5)	0·283 (12·7)
	1950–59	0·309 (22·4)	0·286 (20·5)	0·292 (19·9)
Argentina	1965–9	−0·005 (0·11)	−0·039 (0·92)	−0·031 (0·81)
Germany	1965–9	0·151 (4·83)	0·125 (3·98)	0·104 (2·90)
	1960–64	0·278 (13·1)	0·258 (12·0)	0·270 (11·3)
	1950–59	0·282 (23·5)	0·264 (22·1)	0·269 (21·1)
Hungary	1950–59	0·215 (11·8)	0·175 (9·77)	0·174 (9·02)
Netherlands	1950–59	0·226 (9·32)	0·200 (8·41)	0·193 (7·61)
France	1950–59	0·196 (4·98)	**0·144 (3·91)**	0·138 (3·38)
UK	1965–69	0·221 (11·4)	0·202 (10·3)	0·165 (7·57)
	1960–64	0·266 (12·8)	0·244 (11·8)	0·218 (9·87)
	1950–59	0·262 (16·8)	0·253 (16·3)	0·229 (14·0)
Philippines	1965–9	−0·273 (10·4)	**−0·347 (13·3)**	**−0·412 (14·1)**
India	1965–9	−0·115 (3·14)	**−0·187 (4·94)**	**−0·291 (6·76)**

t-statistics in parentheses.

Relative earnings growth (a measure of assimilation) and changes in immigrant quality (a measure of the effect of immigration policy on immigrant group earnings) are also of interest. Because the modifications of Tables V and VI tend to move relative earnings estimates in 1970 and 1980 in the same direction, estimates of relative earnings growth and changes in quality are relatively unaffected. These estimates are of interest, however, and are displayed in Tables VII and VIII for the ability-adjusted model only.[14]

Table VI. Relative earnings estimates, 1980

Immigrant group		Linear model	Non-linear model	Controls for ability
1965–9		0·143 (28·1)	**0·052 (9·78)**	**0·102 (12·5)**
1960–64		0·188 (34·3)	**0·121 (24·0)**	0·147 (23·2)
1950–59		0·233 (52·5)	0·185 (46·3)	0·203 (39·6)
Portugal	1965–9	0·385 (20·1)	**0·140 (6·93)**	**0·380 (10·7)**
Mexico	1965–9	0·183 (20·8)	**−0·004 (0·31)**	**0·234 (7·76)**
	1960–64	0·199 (20·0)	**0·011 (0·88)**	**0·215 (7·84)**
	1950–59	0·223 (25·5)	**0·071 (6·79)**	**0·220 (10·1)**
Italy	1965–9	0·290 (20·7)	**0·182 (13·4)**	**0·323 (14·5)**
	1960–64	0·306 (19·6)	**0·212 (14·3)**	**0·326 (15·2)**
	1950–59	0·317 (30·2)	**0·241 (23·6)**	**0·318 (21·3)**
Dom. Rep.	1960–64	−0·064 (1·48)	**−0·160 (4·97)**	**−0·042 (1·21)**
Grece	1965–69	0·163 (7·54)	**0·080 (3·73)**	**0·195 (7·33)**
	1950–59	0·283 (12·9)	**0·227 (10·5)**	0·262 (11·0)
Yugoslavia	1965–9	0·327 (14·5)	**0·260 (21·1)**	**0·340 (13·2)**
	1950–59	0·385 (20·9)	**0·324 (18·6)**	0·372 (18·1)
Poland	1965–9	0·258 (9·46)	0·218 (8·02)	0·258 (8·26)
	1960–64	0·247 (12·2)	0·209 (10·7)	0·250 (11·2)
	1950–59	0·321 (17·8)	**0·263 (19·2)**	0·297 (18·3)
Cuba	1965–9	0·001 (0·12)	**−0·071 (6·18)**	**−0·004 (0·26)**
	1960–64	0·103 (9·44)	0·069 (6·37)	0·056 (4·89)
	1950–59	0·071 (4·14)	0·042 (2·48)	0·068 (3·87)
Jamaica	1965–69	0·012 (0·54)	−0·009 (0·43)	0·040 (1·66)
Ireland	1960–64	0·255 (8·06)	0·233 (7·24)	0·264 (7·53)
	1950–59	0·258 (14·2)	0·253 (14·1)	0·275 (14·1)
Colombia	1965–9	0·004 (0·20)	−0·030 (1·41)	0·001 (0·06)
	1960–64	0·103 (3·40)	0·080 (2·71)	0·080 (2·65)
Canada	1965–9	0·329 (17·8)	0·290 (15·4)	0·295 (14·6)
	1960–64	0·295 (19·4)	0·274 (17·9)	0·278 (17·0)
	1950–59	0·296 (27·6)	0·280 (26·2)	0·275 (24·3)
Argentina	1965–9	0·130 (3·53)	0·088 (2·46)	0·092 (2·42)
Germany	1965–9	0·318 (14·2)	0·290 (12·9)	0·255 (10·2)
	1960–64	0·326 (18·1)	0·311 (17·3)	0·307 (15·9)
	1950–59	0·252 (29·7)	0·234 (28·8)	0·227 (25·0)
Hungary	1950–59	0·238 (14·4)	0·214 (13·1)	0·201 (11·3)
Netherlands	1950–59	0·217 (11·3)	0·207 (10·9)	0·180 (8·84)
France	1950–59	0·233 (8·07)	0·211 (7·51)	0·192 (6·50)
UK	1965–69	0·333 (20·4)	0·294 (18·1)	0·218 (11·2)
	1960–64	0·277 (16·6)	0·263 (15·7)	0·209 (11·1)
	1950–59	0·262 (20·2)	0·262 (20·2)	0·221 (15·5)
Philippines	1965–9	−0·006 (0·45)	**−0·071 (5·39)**	−0·122 (8·33)
India	1965–9	0·235 (14·4)	**0·115 (6·70)**	**−0·094 (3·20)**

t-statistics in parentheses.

[14] The full set of estimates are available from the author.

Table VII presents estimates of immigrant relative earnings growth. As is evident from the top three lines, immigrant who arrived during 1965–9 had the most rapid relative earnings growth in the 1970s. This pattern is borne out among the groups below; all the 1965–9 immigrants experienced significantly positive relative earnings growth; immigrant groups with negative or insignificant growth are all pre-1965. This contrasts with the results of Borjas (1985, 1987), who found, also in Census data, low earnings and stagnant earnings growth for immigrants who arrived during 1965–9, after the 1964 immigration law. These results are not

Table VII. Estimate of relative earnings growth, 1970–80, ability-adjusted

Immigrant group		Growth rate of relative earnings
1965–9		0·179 (15·2)
1960–64		0·070 (6·72)
1950–59		0·014 (1·59)
Portugal	1965–9	0·215 (2·79)
Mexico	1965–9	0·353 (6·64)
	1960–64	0·162 (2·69)
	1950–59	0·113 (2·28)
Italy	1965–9	0·158 (3·50)
	1960–64	0·029 (0·02)
	1950–59	−0·003 (0·85)
Dom. Rep.	1960–64	0·022 (0·13)
Greece	1965–9	0·214 (4·66)
	1950–59	0·066 (1·72)
Yugoslavia	1965–9	0·324 (6·21)
	1950–59	0·052 (1·33)
Poland	1965–9	0·238 (4·36)
	1960–64	0·020 (0·24)
Cuba	1965–9	0·192 (7·66)
	1960–64	0·170 (9·51)
	1950–59	0·063 (2·02)
Jamaica	1965–69	0·176 (3·90)
Ireland	1960–64	0·071 (1·33)
	1950–59	0·053 (1·48)
Colombia	1965–9	0·095 (2·31)
	1960–64	0·134 (2·62)
Canada	1965–69	0·068 (2·28)
	1960–64	−0·005 (0·03)
	1950–59	−0·017 (0·87)
Argentina	1965–9	0·122 (2·12)
Germany	1965–9	0·151 (4·04)
	1960–64	0·037 (1·47)
	1950–59	−0·046 (2·49)
Hungary	1950–59	0·027 (1·28)
Netherlands	1950–59	−0·013 (0·15)
France	1950–59	0·054 (1·24)
UK	1965–9	0·053 (2·87)
	1960–64	−0·009 (0·36)
	1950–59	−0·007 (0·20)
Philippines	1965–9	0·289 (9·60)
India	1965–9	0·197 (5·45)

t-statistics in parentheses.

Table VIII. Estimated changes in immigrant quality, 1950–59 to 1965–9

Country	Change in quality	*t*-statistic
All immigrants	−0·086	7·87
Mexico	0·130	3·17
Italy	0·002	0·06
Greece	−0·001	0·02
Yugoslavia	0·020	0·58
Poland	0·024	0·69
Cuba	−0·009	0·32
Canada	0·002	0·10
Germany	−0·014	0·49
United Kingdom	−0·011	0·43

sensitive to the specification of the earnings profile, so the difference in results is a puzzle. Lalonde and Topel (1990) and Yuengert (1991) report a similar failure to replicate; both emphasize the differences in sample selection rules.[15]

Table VIII presents estimates of changes in immigrant quality. This is essentially a comparison of the relative earnings of 1965–9 immigrants in 1980 with those of 1950–59 immigrants in 1970. At these points in time, both immigrant groups have roughly 15 years of US experience. Borjas (1985, 1987) found declines in immigrant quality across a wide range of groups, and attributed the decline to the 1964 changes in the immigration law.

The first line of Table VIII shows that, overall, immigrant quality has decreased by 8·6%. However, none of the immigrants groups below show significant declines in quality. Particularly notable are the Mexicans and Cubans, both of whom had declines in quality in Borjas (1985, 1987). The quality of Mexican immigrants increased by 13% after the 1964 law, and Cuban immigrant quality was unchanged. As with relative earnings growth in Table VII, the difference in estimates is a puzzle. As was the case for relative earnings, it is not a result of the specification of earnings.

5. CONCLUSIONS

This study makes three points about the measurement of immigrant relative earnings. First, relative earnings are sensitive to the choice of the point at which they are calculated. Given this sensitivity, relative earnings estimates should be made at the mean values of the control variables in the earnings function; the sample mean is more representative of the aggregate performance of the immigrant group, and avoids the need to confront any ability bias in the immigrant samples.

The second and third points are similar. Measurements of immigrant relative earnings are sensitive to the specification of the native earnings profile against which immigrant earnings are compared. Both non-linearities in the return to human capital and omitted variables bias have important consequences for relative earnings estimates.

Estimates of earnings which do not take into account certain non-linearities in the returns

[15] Borjas excludes the self-employed, considers log wages instead of log earnings, and includes immigrants who arrived in the USA before 1950. These results are not sensitive to the first two of these differences; we have not checked for the third.

to education and experience tend to understate the earnings of workers with low and high levels of education. Consequently, they tend to overstate the relative earnings of both poorly and highly educated immigrant groups. Correcting for a negative relationship between omitted ability and education tends to decrease relative earnings estimates for both poorly and well-educated immigrants.

Measures of relative earnings growth and changes in immigrant quality were insensitive to the modifications of the estimated earnings function, although they indicate strong earnings growth for post-1964 immigrants and an increase in Mexican immigrant quality, contrary to the results of Borjas (1985, 1987) but in line with those of Lalonde and Topel (1990).

These results have implications beyond the literature on immigration. Studies of discrimination depend crucially on the accurate measurement of the relative earnings of Blacks, Hispanics, and females. Inappropriate specifications of control group earnings profiles may translate into incorrect estimates of relative earnings, particularly for Blacks and Hispanics, whose education levels are relatively low.

APPENDIX

Table AI. State groups for index of relative education

1. IL	11. FL, GA
2. NY	12. AL, MS, TN
3. OH	13. LA, AR, OK
4. PA	14. IN
5. TX	15. MI
6. ME, NH, VT, MA, RI	16. MO, KS, NE, IA
7. CT, NJ	17. MN, WI, ND, SD
8. VA, DC, MD, DE	18. MT, ID, WY, CO, UT, NM, AZ, NV
9. NC, SC	19. CA, WA, OR, AK, HI
10. KY, WV	

Table AII. Coefficient estimates of education quality interactions

	1970 estimates	1980 estimates
Interaction of log student–teacher ratio with:		
Years 1–8	−0·0161 (0·89)	−0·141 (3·78)
Grammar school graduate	0·237 (1·50)	0·789 (4·31)
Years 9–12	−0·0507 (1·36)	−0·158 (1·33)
High school graduate	0·0383 (0·05)	2·389 (4·40)
Years 13–16	0·2037 (5·69)	0·0484 (0·76)
College graduate	0·0543 (0·05)	−1·348 (2·34)
Years of graduate school	−0·0102 (0·06)	−0·0483 (0·73)
18(20) years of education	−0·2078 (0·67)	−0·0500 (0·29)
HS × EXP	−0·0439 (0·26)	−0·404 (4·31)
HS × EXP^2/100	0·412 (0·24)	2·763 (4·11)
HS × EXP^3/1000	−0·129 (0·36)	−0·764 (3·81)
HS × EXP^4/10000	0·0137 (0·36)	0·0719 (3·39)
COLL × EXP	−0·0181 (0·74)	0·327 (2·38)
COLL × EXP^2/100	1·644 (0·79)	−2·290 (1·97)

(*Continued overleaf*)

Table AII. *Continued*

	1970 estimates	1980 estimates
COLL × EXP^3/1000	−0·607 (0·85)	0·585 (1·43)
COLL × EXP^4/10000	0·0760 (0·89)	−0·0508 (1·01)
ED × EXP/100	—	0·112 (1·27)
GM × ED × EXP/100	—	0·309 (0·85)
HS × ED × EXP/100	—	0·225 (0·85)
COLL × ED × EXP/100	—	0·259 (0·87)
Interaction of school term with:		
Years 1–8	0·149 (3·04)	0·0896 (0·59)
Grammar school graduate	0·0457 (0·10)	0·164 (0·29)
Years 9–12	−0·118 (0·96)	−0·811 (1·54)
High school graduate	−0·194 (0·06)	4·010 (2·25)
Years 13–16	0·349 (2·69)	0·156 (0·48)
College graduate	−3·732 (0·92)	−0·0922 (0·05)
Years of graduate school	0·697 (1·24)	−0·136 (0·40)
18(20) years of education	−1·489 (1·37)	−0·816 (1·03)
HS × EXP	0·0680 (0·10)	−0·479 (2·10)
HS × EXP^2/100	−1·033 (0·22)	3·573 (2·15)
HS × EXP^3/1000	0·375 (0·28)	−1·178 (2·22)
HS × EXP^4/10000	−0·0396 (0·28)	0·129 (2·15)
COLL × EXP	0·747 (0·78)	−0·0198 (0·05)
COLL × EXP^2/100	−7·432 (0·94)	1·474 (0·41)
COLL × EXP^3/1000	2·944 (1·10)	−0·877 (0·67)
COLL × EXP^4/10000	−0·395 (1·25)	0·133 (0·80)
ED × EXP/100	—	−0·0180 (0·05)
GM × ED × EXP/100	—	2·050 (1·43)
HS × ED × EXP/100	—	−0·101 (0·09)
COLL × ED × EXP/100	—	−0·435 (0·33)
Interaction of log teacher's relative wage with:		
Years 1–8	−0·0106 (0·88)	−0·0692 (2·25)
Grammar school graduate	0·0115 (0·11)	0·130 (0·93)
Years 9–12	0·0299 (1·11)	0·285 (2·92)
High school graduate	−0·0371 (0·06)	−0·800 (2·36)
Years 13–16	−0·0009 (0·03)	0·0370 (0·68)
College graduate	0·172 (0·22)	−0·170 (0·44)
Years of graduate school	−0·0951 (0·85)	0·0265 (0·49)
18(20) years of education	0·197 (0·91)	−0·103 (0·73)
HS × EXP	0·0042 (0·03)	0·0358 (0·78)
HS × EXP^2/100	0·239 (0·25)	−0·0403 (0·12)
HS × EXP^3/1000	−0·148 (0·52)	−0·0008 (0·01)
HS × EXP^4/10000	0·0218 (0·74)	0·0007 (0·06)
COLL × EXP	−0·0212 (0·12)	0·0414 (0·48)
COLL × EXP^2/100	−0·917 (0·06)	−0·535 (0·73)
COLL × EXP^3/1000	0·113 (0·22)	0·218 (0·84)
COLL × EXP^4/10000	−0·205 (0·34)	−0·0267 (0·83)
ED × EXP/100	—	0·136 (1·88)
GM × ED × EXP/100	—	−0·801 (2·79)
HS × ED × EXP/100	—	0·0731 (0·34)
COLL × ED × EXP/100	—	0·0229 (0·10)

t-statistics in parentheses.

Table AIII. 1970 and 1980 cohort means

Country	Year of immigration	1970 cohort means				1980 cohort means			
		N	ln *Y*	Exp	Educ	*N*	ln *Y*	Exp	Educ
Portugal	1965–9	184	9·5	29·1	5·2	613	9·8	29·2	6·4
Mexico	1965–9	543	9·3	23·1	6·3	3892	9·6	22·7	7·1
	1960–64	729	9·4	24·5	6·2	2733	9·7	26·9	7·4
	1950–59	1315	9·5	27·3	6·8	3667	9·8	28·7	8·6
Italy	1965–9	505	9·6	26·0	7·1	1155	9·9	24·6	9·2
	1960–64	516	9·8	25·2	7·5	997	10·0	26·6	9·7
	1950–59	1234	9·8	27·3	8·0	2273	10·0	26·8	10·6
Dom. Rep.	1960–64	111	9·4	22·0	8·2	210	9·6	27·0	9·7
Greece	1965–69	251	9·5	23·5	8·8	625	9·8	24·6	9·8
	1950–59	417	9·9	24·2	10·5	677	10·1	28·2	11·6
Yugoslavia	1965–9	121	9·7	21·7	10·2	388	10·1	24·2	10·9
	1950–59	452	9·9	27·5	9·5	647	10·2	30·6	11·2
Poland	1965–69	140	9·7	24·5	10·2	350	10·1	24·7	11·7
	1960–64	230	9·8	29·1	9·5	461	10·0	27·1	11·5
	1950–59	897	9·9	31·4	9·9	1037	10·1	34·3	11·2
Cuba	1965–9	857	9·4	26·0	9·7	1956	9·7	29·1	10·7
	1960–64	1069	9·7	23·2	11·9	2454	10·0	23·6	13·2
	1950–59	460	9·7	25·3	10·3	972	9·9	27·4	11·9
Jamaica	1965–69	134	9·5	21·8	10·6	400	9·8	24·7	11·6
Ireland	1964–64	146	9·8	18·5	10·7	295	10·1	23·3	12·2
	1950–59	330	9·9	23·2	11·0	607	10·1	28·4	12·1
Colombia	1965–9	142	9·6	18·3	10·9	428	9·8	21·9	12·3
	1960–64	103	9·7	17·0	12·1	257	10·0	21·3	13·1
Canada	1965–9	423	10·0	18·3	12·5	766	10·2	22·2	12·8
	1960–64	540	10·0	21·2	11·3	1170	10·2	23·4	12·8
	1950–59	1051	10·1	24·7	11·6	2220	10·2	23·6	13·0
Argentina	1965–9	111	9·7	18·5	11·9	236	10·0	23·3	12·8
Germany	1965–9	237	10·0	15·3	13·4	455	10·3	20·8	14·0
	1960–64	393	10·0	18·4	12·0	885	10·2	22·6	13·1
	1950–59	1275	10·1	22·7	11·9	3510	10·1	13·4	21·2
Hungary	1950–59	559	10·0	23·4	12·1	951	10·2	26·6	13·1
Netherlands	1950–59	301	10·0	24·4	12·1	606	10·2	24·2	13·6
France	1950–59	140	10·0	21·7	12·4	330	10·1	22·9	13·4
UK	1965–69	518	10·1	16·5	13·8	857	10·4	22·0	14·9
	1960–64	420	10·1	20·9	13·2	819	10·3	24·0	14·3
	1950–59	765	10·1	24·7	12·8	1506	10·2	24·3	13·9
Philippines	1965–9	282	9·7	14·9	14·9	1329	10·0	19·1	14·5
India	1965–9	176	9·9	10·9	16·6	830	10·5	14·7	18·5

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[Footnotes]

[7] **Does School Quality Matter? Returns to Education and the Characteristics of Public Schools in the United States**

David Card; Alan B. Krueger

The Journal of Political Economy, Vol. 100, No. 1. (Feb., 1992), pp. 1-40.

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