SCHOOL QUALITY AND RETURNS TO EDUCATION OF U.S. IMMIGRANTS

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Using the U.S. labor market as a common point of reference, this article investigates the influence of source country school quality on the returns to education of immigrants. Based on 1980 and 1990 census data, we first estimate country-of-origin specific returns to education. Results reveal that immigrants from Japan and northern Europe receive high returns and immigrants from Central America receive low returns. Next we examine the relationship between school quality measures and these returns. Holding per capita GDP and other factors constant, immigrants from countries with lower pupil-teacher ratios and greater expenditures per pupil earn higher returns to education. (*JEL* J61, I21)

I. INTRODUCTION

Economists agree that human capital is an important factor of production, but considerable controversy exists over how public investments create human capital (Heckman, 2000). In particular, the relation between expenditures per student in schools and the performance of students educated in those schools remains an open question. Using the U.S. labor market as a common point of reference, this study investigates the linkages between cross-country differences in school resources and postschooling labor market outcomes. We first estimate the rates of return to education for U.S. immigrants from 67 countries using 1980 and 1990 census data. We next analyze the relationship between attributes of the source country's educational system and the U.S. return to education for individuals educated under those systems. The primary focus of the article is to examine the relation between school resources in the source country and the rate of return to education earned by U.S. immigrants, but the article also contributes to the immigration and growth literatures.

Hanushek (1986) summarizes a literature that appeared to be heading toward a consensus that school attributes, such as expenditures per pupil and pupil-teacher ratios, had little to do with the performance of students. However, Card and Krueger's (1992a) study of U.S. males educated between 1920 and 1949 showed a strong relationship between pupil-teacher ratios of states and the wages of workers educated in those states. The Card and Krueger study has led to renewed debate on the relationship between school resources and the performance of students. Some studies (Card and Krueger, 1992b, 1996b; Altonji and Dunn, 1996; Angrist and Lavy, 1999; Kreuger, 1999) support Card and Krueger's (1992a) findings, whereas others (Betts, 1995, 1996a; Grogger, 1996a) find little or no relation between commonly used quality attributes of the educational system

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ABBREVIATIONS

GDP: Gross Domestic Product
GNP: Gross National Product
OLS: Ordinary Least Squares
SMSA: Standard Metropolitan Statistical Area

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and the performance of students measured by test scores or wages.[1]

Most previous studies focus on U.S. education, but this work investigates the relation between attributes of educational systems in foreign countries and the return to that education in the U.S. labor market. The approach closely follows Card and Krueger's two-step estimation procedure. In the first step, we estimate the rates of return to education for immigrants from 67 countries using microdata from the 1980 and 1990 censuses. In the second step, we regress returns to education on attributes of the source country's educational system such as expenditures per pupil and the teacher-pupil ratio. The results generally support Card and Krueger's (1992a) finding that pupil-teacher ratios and expenditures per pupil have important impacts on the wages of students educated in those school systems. For example, our results predict that decreasing the number of pupils per teacher by 10% increases the wage of a high school educated immigrant by 1.7% to 3.1%. Similarly, a 10% increase in expenditures per pupil leads to a 0.9% to 1.0% increase in the U.S. wage of a high school–educated immigrant.

An important advantage of this study is the substantial variation in the attributes of the educational systems across nations compared to that observed across states or school districts in the United States. Another important feature is that the first-step results supply the rates of return to education from 67 nations measured in a single labor market. These results may be useful for both the study of immigration and empirical tests of growth models.

Economists studying immigration have long noted the links between education and the labor market outcomes of immigrants in the United States. For example, Chiswick (1978) reports that the effect of an additional year of education on earnings is lower for foreign-born men than for native-born men. Similarly, Butcher (1994) finds that black immigrant groups receive lower rates of return to education than native blacks. Although Chiswick and Butcher propose a number of explanations for these stylized empirical findings (among them that the quality of schooling may be lower in foreign countries), no prior study offers a comparative analysis of the variation in rates of return to education across a large number of immigrant groups, and no study addresses the linkages between the returns to education received by immigrants in the United States and the characteristics of the educational system in the source country.

The article is organized as follows. Section II contains a description of the methodology. Section III presents estimates of the rates of return to education for 67 countries and describes the data. Section IV examines the relationship between the rates of return to education and attributes of educational systems. Section V examines the robustness of results to several methodological issues, and section VI concludes.

II. METHODOLOGY

The major objective of this article consists of assessing the relations between attributes of educational systems and the rates of return to education received by workers. We accomplish this goal by examining the relation between the quality of the educational system in foreign countries and the wages of immigrants in the United States. Implicitly, our empirical specification assumes that immigrants receive the same rate of return to human capital acquired through education but allows the quality of education to vary by country. In particular, the specification uses cross-country differences in attributes of the educational system to identify differences in quality-adjusted education among U.S. immigrants.

Our empirical methodology broadly follows the two-step framework of Card and Krueger (1992a). In the first step, we use microlevel data on U.S. immigrants from the 1980 and 1990 censuses and estimate the rate of return to education by country of birth in the wage regression:

$$(1) \quad \ln w_{ijt} = \beta_t' x_{it} + \sum_j \gamma_{jt} D_{ij} s_{it} + u_{jt} + \varepsilon_{it},$$

where w_{ijt} denotes the weekly wage of immigrant i born in country j and observed in

1. See Card and Krueger (1996a, 1996b), Hanushek (1996), or Betts (1996b) for recent reviews of this literature and also the assessments of the current state of the empirical evidence in Blau (1996), Burtless (1996), and Moffitt (1996).

census t; x is a vector of socioeconomic characteristics (specifically, age and its square, English fluency, marital status, residence in a standard metropolitan statistical area (SMSA), health status, year of immigration, and census division); D_j is an indicator variable set to unity if the immigrant is born in country j; and s is the years of schooling the immigrant obtained in the source country.[2] The error term of the wage regression consists of a country-specific component (u) and an individual-specific component (ε). To address the sensitivity of results to accounting for unobserved differences between source countries that may be correlated with educational attainment, we include results with and without country-specific fixed effects in this first step, imposing the restriction $u_{jt} = 0$ in the models without country-specific fixed effects. The parameter γ_{jt} measures the value the U.S. labor market places on a year of schooling from country j. It is instructive to think of γ_{jt} as the multiplicative of two components (Welch, 1966; Behrman and Birdsall, 1983):

$$\gamma_{jt} = \gamma_t^* Q_{jt}, \tag{2}$$

where γ_t^* denotes a common return to quality-adjusted education return earned by all immigrants in census t, and Q_{jt} is an index reflecting the quality of the educational system in country j at the time when immigrants in census t undertook their schooling. The second step of the two-step methodology addresses the relationships between the quality index, Q_{jt}, and characteristics of the educational system in the source country. Specifically, we model these relationships as

$$Q_{jt} = \alpha_t + \pi' z_{jt} + v_j, \tag{3}$$

where z_{jt} is a vector of characteristics influencing the quality of education and v_j is a country-specific component of Q. Thus, in the second step, we estimate π (identified up to a constant, γ_t^*) by regressing estimates of γ_{jt} obtained in the first step on a set of characteristics describing the educational system in the source country at the time immigrants in census t attended school. As in the first step, we estimate the second-step equation with and without a country fixed effect (v_j).

An advantage of the two-step procedure is that estimates of γ_{jt} from the first step provide an index of the quality of schooling for countries in our sample. Because the index is constructed on the basis of returns to education in a single market economy, it supplies a productivity-based estimate of the quality of educational institutions in foreign countries.

III. COUNTRY-OF-ORIGIN–SPECIFIC RETURNS TO EDUCATION

In this section, we present average rates of return to education in the United States for immigrants from 67 countries. The average rates of return are estimated based on samples of immigrant males drawn from the 5/100 public use samples of the 1980 and 1990 U.S. Censuses of Population.[3] To avoid including immigrants who undertook some of their schooling in the United States, the samples exclude individuals whose birth year plus six plus years of schooling exceeds the year of immigration.[4] The regression samples also exclude persons younger than 25 or older than 64 and those currently enrolled in school. The sample from the 1980 census includes 86,728 immigrants; the 1990 sample consists of 125,503 immigrants.

Table 1 reports results from estimation of equation (1) in the samples of immigrant males drawn from the 1980 and 1990 censuses. The results reveal substantial differences in rates of return to education obtained in different nations. For example, the largest rate of return in 1990 was 8.2% to one year of education from Japan, and the smallest rate of return was 2.0% to one year of education obtained in Haiti. To understand the magnitude of this difference compare the impact of education on the wages of

2. To avoid confusing schooling obtained in the United States and in the source country, in the empirical analysis we exclude immigrants who received some of their education after arriving in the United States.

3. The data appendix contains detailed descriptions of sample restrictions and variable definitions. Also, Table A1 gives descriptive statistics of the regression samples and lists control variables included in the first-step regression model.

4. The 1980 census reports year of immigration only in five-year intervals, and the 1990 census reports two-year intervals for recent immigrants and five-year intervals for older immigrants. We apply the most restrictive interpretation of the data and assume that all immigrants in each immigration interval arrived in the United States the earliest year of the interval. For example, we impose sample restrictions as if all immigrants who report arriving between 1975 and 1979 immigrated in 1975.

TABLE 1
U.S. Rate of Return to Education by Country of Birth

	1980 Census			1990 Census		
Country	**Rate of Return**	**Standard Error**	**Observations**	**Rate of Return**	**Standard Error**	**Observations**
Europe						
Austria	.0533	.0023	360	.0699	.0032	194
Belgium	.0584	.0032	170	.0690	.0033	160
Czechoslovakia	.0442	.0018	637	.0534	.0020	430
Denmark	.0590	.0031	213	.0692	.0032	181
Finland	.0490	.0044	114	.0671	.0046	89
France	.0531	.0017	632	.0645	.0017	623
Germany	.0509	.0009	3,314	.0635	.0011	2,149
Greece	.0300	.0014	1,963	.0429	.0016	1,454
Hungary	.0400	.0017	753	.0482	.0021	541
Ireland	.0429	.0017	955	.0587	.0016	1,030
Italy	.0442	.0010	5,270	.0542	.0012	3,182
Netherlands	.0511	.0018	600	.0654	.0021	440
Norway	.0632	.0029	264	.0789	.0032	168
Poland	.0398	.0012	2,544	.0431	.0010	2,461
Portugal	.0433	.0019	1,892	.0446	.0018	1,967
Romania	.0414	.0021	485	.0501	.0017	733
Spain	.0424	.0021	587	.0518	.0021	603
Pakistan	.0317	.0022	304	.0379	.0014	951
Sweden	.0543	.0029	220	.0739	.0027	237
Switzerland	.0630	.0025	273	.0716	.0023	301
UK	.0560	.0008	3,860	.0703	.0008	4,025
USSR	.0339	.0011	1,916	.0450	.0012	1,457
Yugoslavia	.0432	.0015	1,520	.0522	.0017	1,078
Asia						
China	.0247	.0010	2,732	.0274	.0009	4,213
Hong Kong	.0316	.0030	209	.0407	.0019	634
India	.0382	.0009	2,082	.0476	.0007	4,500
Indonesia	.0402	.0025	288	.0508	.0025	297
Iran	.0477	.0018	500	.0491	.0012	1,337
Iraq	.0303	.0030	241	.0431	.0025	377
Israel	.0386	.0021	457	.0562	.0017	654
Japan	.0522	.0011	1,548	.0822	.0010	2,037
Korea	.0333	.0010	1,774	.0449	.0008	3,448
Lebanon	.0398	.0026	338	.0479	.0019	613
Malaysia	.0317	.0056	48	.0439	.0032	185
Pakistan	.0317	.0022	304	.0379	.0014	951
Philippines	.0269	.0008	4,356	.0344	.0006	7,404
Singapore	.0456	.0078	24	.0622	.0057	54
Sri Lanka	.0497	.0048	56	.0556	.0033	141
Taiwan	.0336	.0020	358	.0463	.0010	1,605
Thailand	.0252	.0027	235	.0341	.0021	456
Turkey	.0434	.0025	325	.0544	.0025	342
Africa						
Egypt	.0408	.0017	495	.0469	.0014	853
Kenya	.0440	.0055	43	.0560	.0039	103
Morocco	.0394	.0046	90	.0402	.0035	169
Sierra Leone	.0293	.0129	9	.0314	.0056	54
Tanzania	.0281	.0071	29	.0439	.0056	58
Uganda	.0382	.0071	30	.0472	.0053	60
Oceania						
Australia	.0566	.0026	236	.0703	.0024	297
New Zealand	.0440	.0038	106	.0729	.0033	160

continued

TABLE 1 continued

	1980 Census			1990 Census		
Country	Rate of Return	Standard Error	Observations	Rate of Return	Standard Error	Observations
North America						
Canada	.0555	.0008	4,754	.0685	.0009	3,100
Costa Rica	.0296	.0036	207	.0377	.0032	295
Cuba	.0302	.0009	5,262	.0330	.0009	5,480
Dominican Republic	.0122	.0019	1,324	.0210	.0014	2,102
El Salvador	.0182	.0023	749	.0221	.0012	3,951
Guatemala	.0200	.0026	566	.0214	.0016	1,922
Haiti	.0119	.0017	862	.0202	.0014	1,832
Honduras	.0254	.0034	283	.0234	.0024	701
Jamaica	.0246	.0014	1,611	.0350	.0013	2,108
Mexico	.0248	.0009	20,455	.0203	.0006	41,412
Panama	.0372	.0020	495	.0364	.0023	418
Trinidad and Tobago	.0270	.0021	592	.0375	.0019	722
South America						
Argentina	.0436	.0018	704	.0506	.0016	875
Brazil	.0496	.0028	246	.0417	.0019	659
Chile	.0406	.0023	352	.0438	.0021	514
Colombia	.0283	.0015	1,287	.0332	.0012	2,269
Ecuador	.0220	.0020	783	.0277	.0017	1,120
Peru	.0301	.0019	581	.0320	.0014	1,275
Uruguay	.0322	.0040	160	.0461	.0034	243
Mean (67 countries)	.0389			.0482		
Standard deviation	.0119			.0156		

Notes: Rates of return to education are estimated using the 5/100 public use samples of the 1980 and 1990 censuses of population. Samples are limited to immigrant males age 25–64 who completed their schooling before migrating to the United States; see text for other sample restrictions. Sample sizes are 86,728 for the 1980 sample and 125,503 for the 1990 sample. Additional regressors include age and its square and indicator variables for English fluency, married with spouse present, residence in SMSA, health limiting work, eight census divisions, and five (nine in 1990 sample) immigrant cohorts.

two high school graduates, identical in every respect except that one individual was educated in Haiti and the other in Japan. The estimated returns to education indicate that the worker from Japan doubles his earnings ($\exp[12(.0822 - .0202)] = 2.10$) by obtaining education in Japan rather than Haiti.

The estimated rate of return to education from each country supplies a market-based measure of the productivity of education from a cross-section of countries measured in a common market. Thus, the numbers complement recently assembled international databases on human capital stock (Barro and Lee, 1993, 1996; Nehru et al., 1995; Psacharopoulus and Arriagada, 1986) and may be used to adjust for quality of education in studies examining differences in growth rates across nations. For example, the differences in productivity of education help better quantify the differences in human capital that may explain the high growth rates of Asian nations (such as Japan and Singapore) and much lower growth of poorer nations (such as Haiti and Sierra Leone).

The results in Table 1 indicate similar general patterns across countries for the two census years; the simple correlation coefficient between the two series equals .920. In both 1980 and 1990, the table shows high rates of return to education obtained in northern Europe, Australia, and Canada and low rates of return to education obtained in Central America. The largest improvements between 1980 and 1990 were for education obtained in Japan or New Zealand. We also constructed similar regression samples of native-born workers and estimated the return to education for the United States. Results show that the average rates of return to one year of schooling obtained in the United States were .0565 in the 1980 census and .0776 in the 1990 census. In a ranking with the 67 nations listed in Table 1, these returns place the United States sixth (between Australia and

United Kingdom) in 1980 and third (between Norway and Sweden) in 1990.

The mean of the average returns to education across the 67 countries rose from .0389 in 1980 to .0482 in 1990. The rise in the mean reflects in part an economy-wide increase in the returns to education in the U.S. economy over that period found in previous studies (Katz and Murphy, 1992; Juhn et al., 1993; Buchinsky, 1994) but may also indicate an improvement in the global quality of education.[5]

The model presented in section II hypothesizes that differences in average returns to education across countries in this study reflect differences in school quality across source countries. To examine this possibility, we compiled a data set that links the estimated rates of return with quality measures of the educational system in the source country. We lag the educational quality data 20 years to better capture differences in school quality at the time immigrants undertook their schooling; that is, we match 1980 and 1990 census data with school characteristics from 1960 and 1970, respectively. The measures of school quality include the pupil-teacher ratio in primary schools, relative expenditures per pupil,[6] and years of compulsory education. The first two measures reflect the resources devoted to education, and the last is intended to capture the commitment to education. The data appendix contains a detailed description of data sources and the construction of variables.

Unfortunately, reliable measures of educational quality were unavailable for at least one of the sample years for Switzerland and China, leaving a sample of 65 countries for the empirical analysis. Matching the 1980 census returns with 1960 school quality attributes and the 1990 census returns with 1970 school attributes supplies 2 observations per nation and a total of 130 observations for returns to education and attributes of the educational system generating those returns.

Are the school resource variables correlated with estimated rates of return to education? Figure 1 contains graphs of the average return to education versus the pupil-teacher ratio and relative education expenditure. Panel A plots the average return to education in 1980 against the pupil-teacher ratio in 1960. This panel reveals a strong negative correlation between the pupil-teacher ratio and the estimated rate of return to education. Panel B contains a similar plot of the 1980 return to education versus the 1960 relative education expenditure and shows a positive relationship between the two variables. Panels C and D contain plots for 1990 returns to education versus 1970 school attributes and reveal similar patterns. Although the figures suggest relations between school quality and the rates of return to education across educational systems, a more detailed analysis is necessary to verify the robustness of the results.

IV. SOURCES OF VARIATION IN RATES OF RETURN TO EDUCATION

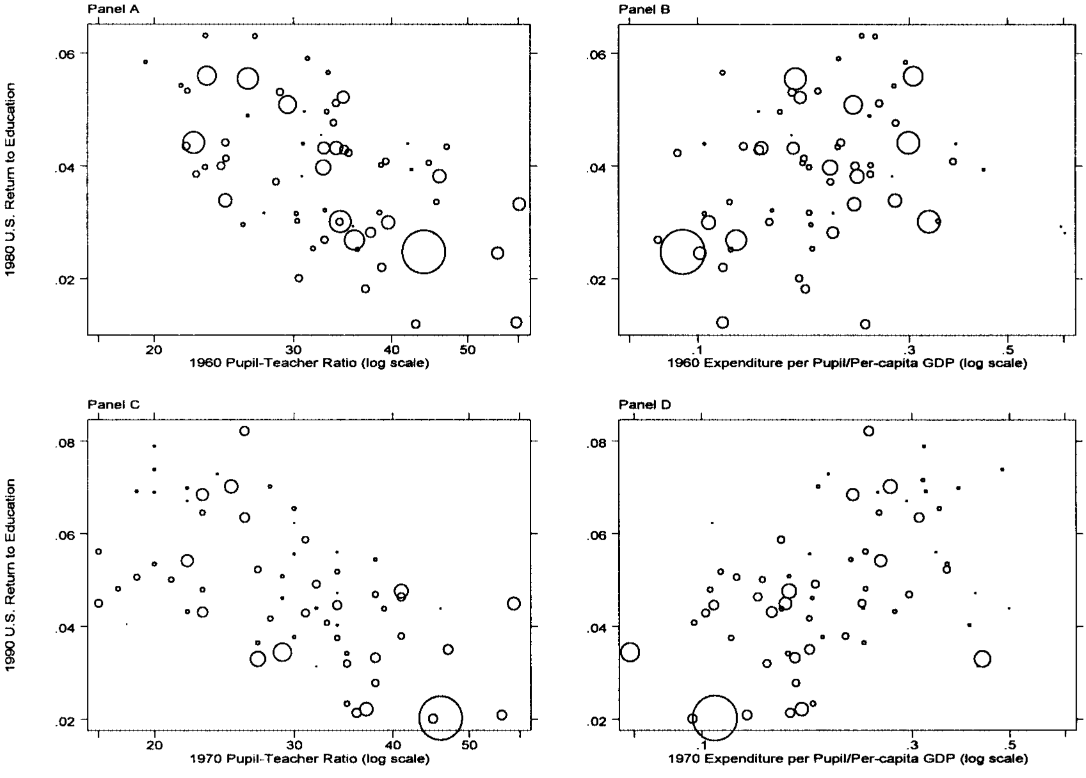
We now turn to the results from second-step regressions in which we regress the average returns to education on the attributes of the educational system in the source country. Table 2 contains summary statistics for the school quality measures and other variables used in the analysis. An important feature of this study is that the variation in school quality across countries is much larger than the variation in these measures across U.S. states. For example, in the international data the standard deviation of the pupil-teacher ratio is 8.6 in 1960 and 8.9 in 1970 (see appendix Table A3) compared to standard deviations of 4.8 (the 1920s), 3.9 (the 1930s), and 3.1 (the 1940s) in Card and Krueger's (1992a) data set of U.S. states. Such greater variation should allow more precise estimation of the impact of the quality measures on returns to education.

The bottom of Table 2 displays the correlation coefficients between the rate of return to education and the measures of school resources. These results reveal the same relationships depicted in Figure 1 and also indicate a positive correlation between years of compulsory education and the U.S. return

5. Another possible explanation for the rise in returns is Jaeger's (1997) assertion that the change in wording of the census question about level of education in the 1990 census leads to higher estimates of returns to education in studies that linearize education in the 1990 census as we do.

6. Relative expenditures per pupil are expenditures per student divided by per capita gross domestic product (GDP). We use this variable rather than nominal expenditures because it better measures the proportion of resources devoted to education and is not sensitive to exchange rates or differences in prices of non-traded goods across countries.

FIGURE 1
School Quality and U.S. Returns to Education



Note: Size of symbol is proportional to cell count of first step.

to education. Not surprisingly, the results also show a negative correlation between the pupil-teacher ratio and relative education expenditure. Countries that spend more on education also tend to have lower pupil-teacher ratios. The correlations also suggest that educational systems of wealthier nations tend to be better with regard to all three attributes and that education from these nations earns a higher return in the U.S. labor market. The next step is to separate the impact of GDP from that of the educational attributes.

Table 3 contains results for regressions of the rate of return to education regressed on attributes of the educational systems. The table lists results of the second-step regression with and without country fixed effects in both the first and second steps. Across all specifications, the regressions reveal a very robust negative relationship between the pupil-teacher ratio and the rate of return to education. Overall these results also indicate a positive relationship between relative education expenditures and the rate of return to education, though this result is not robust across specifications. Consider first the coefficient on the log pupil-teacher ratio, which gives the change in the rate of return to education for a proportionate change in the pupil-teacher ratio. The coefficient of the log pupil-teacher ratio ranges from −.0392 to −.0144 across all specifications and is significant at the 1% level in all models. Column (5), which includes the largest set of country-specific variables and country fixed effects in the first step, yields a coefficient of −.0261, implying that for an immigrant with ten years of schooling (the sample mean; see Table A1) the elasticity of the U.S. wage with respect to the pupil-teacher ratio in the source country is −.261. Thus, a 10% reduction in the pupil-teacher ratio raises the expected wage

TABLE 2
Sample Statistics—Second Step Regression Samples

Variable	Mean	SD
A. Descriptive statistics		
U.S. Rate of Return to Education	.0359	.0151
ln(Pupil-teacher Ratio)	3.5609	.2864
ln(Expenditure per Pupil/Per-capita GDP)	−1.8737	.4645
Years of Compulsory Schooling	6.8757	1.8222
ln(Per-capita GDP)	7.9566	.6623
Income Inequality (Top 10 to Bottom 20 Percentiles Wealth)	9.5169	6.3960
English Official Language (Indicator)	.1155	.3208
Communist Regime (Indicator)	.1113	.3157
Coup or Revolution During Decade (Indicator)	.2961	.4583
Assassinations During Decade per Million Population	.1941	.6436
Americas (Indicator)	.5484	.4996
Asia (Indicator)	.1865	.3910
Africa (Indicator)	.0097	.0986

	U.S. Return to Education	ln(Pupil-teacher Ratio)	ln(Expenditure per Pupil)	Compulsory Schooling
B. Correlation matrix				
ln(Pupil-teacher Ratio)	−.7073			
ln(Expenditure per Pupil)	.5571	−.5615		
Compulsory Schooling	.5846	−.5424	.4028	
ln(Per-capita GDP)	.4005	−.3465	.1021	.5318

Notes: Sample size is 130. Country-of-birth characteristics are lagged 20 years from the census estimate of return to education; i.e., rates of return based on the 1980 census are matched with country data from 1960 and rates of return based on the 1990 census with country data from 1970. Observations are weighted by cell count of first step.

of a high-school educated immigrant from the country by 3.1%.

The results also predict a positive relationship between the relative education expenditures and the rate of return to education. Focusing again on the specification in column (5), a 10% increase in the relative expenditure on education leads to a predicted .75% increase in the wages of an immigrant with ten years of schooling.

The remaining variables in the extended regression models also likely pick up variation in school quality across source countries or may reflect differences in transferability of schooling to the U.S. labor market and therefore serve as important control variables for isolating the impact of the school quality measures. The signs of the coefficients on these variables are mostly as expected. Compulsory schooling generally has a positive (although statistically insignificant) impact on the rate of return to education, and immigrants from English-speaking countries earn a higher rate of return to their education than immigrants from non–English-speaking countries, other things equal. Because the first-step regression controls for English-speaking ability of the immigrant, the latter result likely reflects greater transferability of schooling from these countries. Greater income inequality, communist regimes, and political turmoil are all associated with lower returns to education—reflecting either lower school quality or less transferability of schooling under such conditions. The coefficient of log per capita GDP is positive and significant in one of four specifications and negative in two specifications. Thus, our results are probably best interpreted as inconclusive on the impact of source country development on the U.S. return to education holding educational attributes constant.

How do our results compare to previous studies? As a general summary, we note that our predicted effects of quality of education attributes are similar to estimates from a number of studies based on U.S. school quality data. For example, our estimates of the change in the rate of return to education from a proportionate change in the pupil-teacher ratio range between −1.44 and −3.92, whereas the summary of results from

TABLE 3
Determinants of U.S. Returns to Education

	(1)	(2)	(3)	(4)	(5)	(6)
ln(Pupil-teacher Ratio)	−.0239** (.0040)	−.0144** (.0032)	−.0271** (.0070)	−.0234** (.0070)	−.0261** (.0060)	−.0392** (.0112)
ln(Expediture per Pupil)	.0067** (.0023)	.0082** (.0017)	−.0012 (.0032)	.0068 (.0040)	.0075* (.0031)	−.0048 (.0052)
Compulsory Schooling	.0022** (.0006)	.0008 (.0005)	.0000 (.0008)	.0009 (.0010)	.0010 (.0009)	−.0009 (.0013)
ln(Per-capita GDP)		.0047** (.0018)	.0079 (.0063)		−.0048 (.0032)	−.0151 (.0101)
Income Inequality		−.0003* (.0001)	−.0013 (.0014)		−.0004 (.0003)	−.0022 (.0022)
English Official Language		.0105** (.0021)			.0236** (.0039)	
Communist		−.0063 (.0032)			−.0127* (.0058)	
Coup or Revolution		.0033 (.0018)	.0022 (.0029)		.0028 (.0033)	.0040 (.0046)
Assassinations		−.0027* (.0011)	.0008 (.0016)		−.0054** (.0019)	.0009 (.0025)
Americas		−.0069** (.0021)			.0057 (.0038)	
Asia		.0058* (.0024)			.0217** (.0044)	
Africa		.0042 (.0065)			.0291* (.0119)	
1980 Observation	−.0024 (.0018)	−.0020 (.0014)	.0002 (.0023)	−.0033 (.0031)	−.0047 (.0025)	−.0067 (.0037)
Constant	.1194** (.0154)	.0656** (.0221)		.1284** (.0269)	.1726** (.0406)	
Country fixed effect in first step?	No	No	No	Yes	Yes	Yes
Country fixed effect in second step?	No	No	Yes	No	No	Yes
R^2	.5894	.8360	.9577	.2520	.6148	.9353

Notes: Sample size is 130. Standard errors are reported in parentheses. Observations are weighted by cell count of first step.

*Statistically significant at the 5% level (two-tailed test).

**Statistically significant at the 1% level (two-tailed test).

previous studies (that control for state-of-birth effects) in table 5.3 of Card and Krueger (1996a) range from −1.07 to −1.81. Further, Betts (1996b) computes the elasticity of earnings with respect to school spending per pupil from 23 studies, and although he emphasizes the range of these elasticities, most estimates are near the mean elasticity across studies, which is .1041. These studies do not control for the effect of the pupil-teacher ratio and are therefore not directly comparable to ours. When we exclude the pupil-teacher ratio from the specifications in Table 3, the coefficient of log expenditures per pupil becomes .0099 in column 2 and .0106 in column 6.[7] Evaluated at sample mean educational attainment (ten years), these estimates generate elasticities that are remarkably close to those summarized by Betts.

Of course, our estimates generally exceed those of Betts (1995), Grogger (1996a), and others who find small or zero impact of these quality measures on earnings. This discrepancy has been explained in many ways. First, a difference in samples may explain the

7. Results are not reported in Table 3 but are available on request.

results. Card and Krueger's (1992a, 1992b) studies find a strong relationship between quality attributes and wages in samples of workers educated before 1960, whereas studies focusing on U.S. workers educated after 1960 more often find a weaker relation or no relation at all. Burtless (1996) hypothesizes that the difference may be due to nonlinearities in the relation between school inputs and the rate of return to schooling. The variation in school attributes across states and school districts in the U.S. has dropped markedly over time (Heckman et al., 1996a). Both Card and Krueger's samples and those used in the present study have much more variation in quality measures, which may allow detection of nonlinear relationships.

Hoxby (1996) argues that teachers' unions may explain the discrepancy. Her results show that strong teachers' unions increase resources devoted to education but may reduce student achievement. Thus studies focusing on students educated after the onset of collective bargaining in the public sector (early 1960s) will find no substantial relation between school inputs and student achievement, but studies based on those educated before 1960 find an important relation. Because very little of the variation in school attributes in our sample would be attributable to unionization, Hoxby's argument suggests that this study should find estimates similar to Card and Krueger (1992a), as we do.[8]

Grogger (1996b) and Hanushek et al. (1996) suggest another explanation of the discrepancy in results that focuses on the Card and Krueger's use of aggregate data. In particular, Hanushek et al. (1996) argue that the omission of regulations affecting the operations of schools, primarily state-level regulations, leads to more severe misspecification bias in aggregate studies and thus an upward bias in the estimated impacts of school resources on achievement in these studies.[9] Because the organization of school systems differs greatly across nations, the bias suggested by Hanushek et al. (1996) may apply to the present study. For example, highly developed countries might have lower pupil teacher ratios and better organized school systems than poorer nations. However, although the aggregation bias could plausibly generate correlation in a cross-section of nations, the organization of school systems should vary much less within nations over time. The results in columns (3) and (6) of Table 3 address this issue by including fixed effects in the second-step regressions. The coefficient of the pupil-teacher ratio is slightly larger in these formulations, suggesting that this form of aggregation bias does not affect our results.

Given the international data used in our study, several other issues emerge. In section V, we examine the sensitivity of results to selective immigration, birth-cohort restrictions, and convexity of the education-earnings profile.

V. SENSITIVITY ANALYSIS

Selective Immigration

Though the immigrant data offer the advantage of large variation in educational characteristics, they have the drawback that the selection mechanism guiding the immigration decision could introduce selectivity bias into the estimation of the parameters of equation (1). Indeed, one of the chief criticisms of the Card and Krueger methodology focuses on selective migration (Heckman et al., 1996a, 1996b).[10] As pointed out by

8. Although these studies focus on school resource effects on earnings, Loeb and Bound (1996) also find larger effects of school inputs on student achievement in older birth cohorts than studies based on more recent birth cohorts, suggesting "that both earnings and achievement effects may simply have diminished over time" (Moffit, 1996) in U.S. data.

9. More specifically, suppose the omitted quality of the educational regulations is positively correlated with the expenditures per pupil and also positively correlated wages. Regressions omitting the quality of the educational regulations would find a positive relation between expenditure per pupil and wages, even if no such relation exists. Under more restrictive assumptions, Hanushek et al. (1996) show that if key regulations are state-specific, the bias in the estimates of the impact of pupil-teacher ratios and other resources on wages will be largest in studies using state-level attributes of the education system. In our case, the criticism applies if omitted institutional features are country-specific.

10. This criticism centers on the lack of a pattern and frequent sign reversals in correlations between earnings and school quality when the earnings of workers residing in a given census division are compared to the school quality in the state where they grew up. When we follow Heckman et al.'s approach, we find that the immigrant data reveal a consistent pattern in rankings of school quality and earnings across census divisions, with signs according to the schooling quality hypothesis (results are available on request). The implication is that regional variation in demand for skill is less important for the

Burtless (1996), it is not clear whether or how nonrandom migration biases the estimates of school resource effects in the two-step procedure. To shed some light on this issue, we consider a simplified version of the Roy model (Borjas, 1987, 1991) focusing on the role of schooling in wage determination. Suppose the wages a potential immigrant could earn in the source country (w_0) and in the United States (w_1) are determined by

$$\text{(4)} \qquad \ln w_0 = \mu_0 + \gamma_0 s + v_0, \quad \text{and}$$

$$\text{(5)} \qquad \ln w_1 = \mu_1 + \gamma_1 s + v_1,$$

where s denotes the years of schooling of the individual; and v_0 and v_1 measure the contributions to wages of unobservable skills—known to the individual but unknown to the researcher. Assume that the population distribution of v_0 and v_1 is bivariate normal with zero means, standard deviations σ_0 and σ_1, and correlation coefficient ρ. Also, v_0 and v_1 are uncorrelated with s.

If migration costs are given by c, income-maximizing behavior generates the migration condition $I = \ln w_1 - \ln w_0 - c > 0$. Thus, the emigration rate from the source country to the United States is given by

$$\text{(6)} \quad p = \Pr\{I > 0\} = \Pr\{(v_1 - v_0) > \mu_0 - \mu_1 + c - (\gamma_1 - \gamma_0)s\},$$

and, in a random sample of immigrants, the expectation of the log wage is

$$\text{(7)} \quad E\{\ln w_1 | s, I > 0\} = \mu_1 + \gamma_1 s + E\{v_1 | (v_1 - v_0) > \mu_0 - \mu_1 + c - (\gamma_1 - \gamma_0)s\}.$$

Thus the Roy model predicts that the error term in the regression of log wages on years of schooling in a random sample of immigrants is truncated and correlated with the regressor, s, causing biased and inconsistent ordinary least squares (OLS) estimates of the parameters in the wage regression.

settlement pattern of immigrants across regions than it is for native-born migrants. Moreover, the consistent sign patterns across census divisions confirm our approach in which we view the U.S. labor market as a common point of reference for assessing educational quality in international data.

To continue, we make the simplifying assumption that v_0 and v_1 are perfectly correlated in the population.[11] The conditional expectation of the log wage then becomes

$$\text{(8)} \quad E\{\ln w_1 | s, I > 0\} = \mu_1 + \gamma_1 s + E\{v_1 | (\sigma_1 - \sigma_0)v_1/\sigma_1 > \mu_0 - \mu_1 + c - (\gamma_1 - \gamma_0)s\}.$$

With the additional assumption of normality of v_1, the last term simplifies to

$$\text{(9)} \quad E\{v_1 | s, I > 0\} = \begin{cases} \phi(z)/p & \text{if} \quad \sigma_0 < \sigma_1 \\ -\phi(z)/p & \text{if} \quad \sigma_0 > \sigma_1 \end{cases},$$

where ϕ denotes the standard normal density function and $z = [\mu_0 - \mu_1 + c - (\gamma_1 - \gamma_0)s]/(\sigma_1 - \sigma_0)$.[12] Equations (8) and (9) show that the truncation of v_1 is strictly from below when σ_0 exceeds σ_1 (the immigrant pool is characterized by "positive sorting" in unobservables), and strictly from above when σ_0 exceeds σ_1 ("negative sorting").

The OLS bias in equations (7) and (8) takes the sign of the correlation between s and the truncated error term. If U.S. immigration is characterized by positive sorting (in education and unobservable skills), this correlation is negative as selectivity in unobservables intensifies with lower levels of schooling. Under such conditions, OLS estimates of the rate of return to education are downward biased. This is exactly the bias discussed in Chiswick (1978) and Butcher (1994). Unfortunately, to assess the bias in estimates of school resource effects additional assumptions on the linkages between school resources and the parameters of the Roy model are needed.

Perhaps more important for the present study, however, is that equations (8) and (9) suggest that parameters of the wage regression can be estimated consistently if we account for the truncation of the error term. To accomplish this, we adapt a variant of Heckman's (1979) method of controlling for sample selectivity, treating the bias in the

11. This assumption rules out the refugee sorting scenario in Borjas (1987).

12. Note that p is itself a function of z. In particular, $p = \Phi(z)$ when $\sigma_0 > \sigma_1$, and $p = 1 - \Phi(z)$ when $\sigma_0 < \sigma_1$.

OLS estimator as omitted variable bias stemming from omission of the expectation of the truncated error term, which is conditional on the level of schooling of the immigrant.

The first step in the sample selectivity procedure requires estimating the probability of migration to the United States conditional on education. In particular, migration rates were computed for male immigrants from each country in our sample at three levels of schooling (corresponding to primary, secondary, and higher education levels in international data): fewer than 7 years, 7 to 12 years, and more than 12 years of education. We use census data to estimate the number of individuals at each level of schooling living in the United States. For each country in our sample, a combination of the population and the proportion of the population of each country with each level of education supplies the number of individuals in that nation in each education category. The resulting migration rates are reported in Table A2, and the data appendix provides further detail on the construction and on data sources.

Based on the estimated migration rates, we compute proxies for the conditional expectation of v according to equation (9), which we then add to the first-step regression model in equation (1) to control for sample selectivity.[13] Results from the first-step model incorporating sample selectivity controls largely parallel results based on OLS. The correlation coefficients between the selectivity adjusted series and those reported in Table 1 are very high—.988 in the 1980 data and .976 in the 1990 data—and the mean rates of return are only slightly higher than those in Table 2—3.9812 in 1980 and 5.1159 in 1990. The first three columns of Table 4 contain a replication of earlier second-step regressions using rates of return to education estimated with selectivity corrections. Comparing these results to Table 3 reveals that the selectivity controls do not substantially alter the results.[14]

13. Because this procedure is sensitive to the assumption of normality, we also used a procedure that adds a cubic polynomial of the migration rate to the first-step wage regression. Results from this alternative procedure were very close to OLS outcomes.

14. All results were estimated using rates of return estimated with selectivity controls as the dependent variable in the second step. There were no cases where selectivity control altered results in a substantial manner.

Age-Restricted First-Step Samples

Another potential problem with the earlier results lies in the assumption that attributes of the 1960 educational system apply to all individuals in the 1980 census and that 1970 attributes apply to those from the 1990 census. An obvious solution to this problem is to restrict the first-step regression samples according to age at the time of the census. To focus on this issue, equation (1) was reestimated for narrowly defined birth cohorts. The cohorts were defined by associating the 1960 school attributes with immigrants born between 1945 and 1955 and 1970 attributes with immigrants born between 1955 and 1965. An important drawback of this approach is that sample sizes became quite small for a number of source countries, triggering large sampling variances for some first-step parameter estimates. Nevertheless, the rates of return to education estimated from the restricted first-step samples exhibit high correlations with the returns in Table 2 (simple correlation coefficients are .923 for 1980 and .943 for 1990).

The last three columns of Table 4 report second-step regression results based on the restricted birth cohort data. A comparison of these results to comparable results based on the full sample of male immigrants reveals very similar parameter estimates for the pupil-teacher ratio but slightly smaller effects of expenditures per pupil than in previous tables. The finding of smaller resource effects in samples that are restricted to young workers is consistent with Card and Krueger's (1996a) observation that school quality effects are likely understated in samples of young workers.[15] Finally, a closer look also reveals larger standard errors in columns (4)–(6) of Table 4 than in Table 3—a result caused by the smaller sample sizes in the first-step regression.

Nonlinear Returns to Education

Results thus far indicate large effects of school resources in the source country on the returns to education earned by immigrants in the United States. But the evidence is based on the restrictive assumption that the

15. Betts (1996b), however, finds no significant age dependence in school quality effects.

TABLE 4
Sensitivity Analyses

	Selectivity Adjusted Returns			Restricted Birth Cohorts		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
ln(Pupil-teacher Ratio)	−.0114**	−.0347**	−.0266**	−.0152**	−.0392**	−.0291**
	(.0039)	(.0080)	(.0055)	(.0034)	(.0081)	(.0080)
ln(Expediture per Pupil)	.0100**	−.0009	.0050	.0061**	−.0042	.0029
	(.0020)	(.0037)	(.0029)	(.0019)	(.0036)	(.0045)
Compulsory Schooling	.0011	−.0001	.0009	.0006	.0000	.0000
	(.0006)	(.0010)	(.0008)	(.0004)	(.0008)	(.0010)
ln(Per-capita GDP)	.0040	.0095	−.0034	.0049**	.0137*	−.0033
	(.0021)	(.0072)	(.0030)	(.0018)	(.0067)	(.0043)
Income Inequality	−.0005**	−.0012	.0000	−.0002	−.0020	−.0002
	(.0002)	(.0016)	(.0002)	(.0001)	(.0018)	(.0003)
English Official Language	.0133**		.0219**	.0114**		.0208**
	(.0026)		(.0036)	(.0024)		(.0055)
Communist	−.0041		−.0144**	−.0055		−.0039
	(.0038)		(.0054)	(.0039)		(.0092)
Coup or Revolution	.0027	−.0001	.0028	.0030	.0014	.0025
	(.0022)	(.0033)	(.0031)	(.0019)	(.0032)	(.0044)
Assassinations	−.0030*	.0006	−.0030	−.0023*	.0010	−.0031
	(.0013)	(.0018)	(.0018)	(.0009)	(.0013)	(.0021)
Americas	−.0082**		.0060	−.0103**		.0037
	(.0025)		(.0035)	(.0025)		(.0058)
Asia	.0064*		.0203**	.0026		.0151*
	(.0029)		(.0041)	(.0025)		(.0059)
Africa	.0041		.0302**	−.0003		.0208
	(.0078)		(.0110)	(.0065)		(.0153)
1980 Observation	−.0042*	−.0008	−.0046	−.0001	.0038	−.0023
	(.0016)	(.0026)	(.0023)	(.0013)	(.0024)	(.0031)
Constant	.0659*		.1577**	.0570*		.1586**
	(.0265)		(.0375)	(.0228)		(.0535)
Country fixed effect in first step?	No	No	Yes	No	No	Yes
Country fixed effect in second step?	No	Yes	No	No	Yes	No
R^2	.8229	.9583	.6397	.8275	.9526	.4429

Notes: Sample size is 130. Standard errors are reported in parentheses. Observations are weighted by cell count of first step.

*Statistically significant at the 5% level (two-tailed test).

**Statistically significant at the 1% level (two-tailed test).

schooling-log wage profile is linear, that is, that returns to education are not related to levels of education. In some human capital investment models, the relationship between schooling and log wages is convex—returns increase with educational attainment. If this relationship is convex for U.S. immigrants, it is possible that our findings reflect that immigrants from countries with higher school quality have more educational attainment and earn higher returns because they are farther out a common schooling-log wage function. Although the higher attainment in this scenario may be the consequence of school quality, the higher returns are not, which affects the interpretation of the relationship between school quality and wages.

Heckman et al. (1996) offer evidence from the United States that the impact of school quality differs by level of education. When they estimate nonlinear schooling–log wage models they find that the effects of school resources on returns to education are concentrated at high levels of education and that such effects are strongest for those with at least a college education. In this section, therefore, we relax the assumption of linear returns and allow marginal returns to education to differ after 9 and 12 years of schooling. In the two-step approach this implies a very highly parameterized first-step model—indeed, there are at least 390 separate returns to be estimated—so we instead substitute equations (3) and (2) into equation (1) and estimate school-quality effects directly in the microsamples based on the equation

$$\ln w_{ijt} = \beta_t' x_{it} + \alpha_t s_{it} + \pi' z_{jt} s_{it} + \lambda' z_{jt} + u_j + \varepsilon_{it}. \tag{10}$$

We further augment the regression model with a three-segment spline function in education with splines at 9 and 12 years of schooling. School quality impacts on the education slope are captured by interaction terms between the school quality characteristics and schooling ($z_{jt}s_{it}$). To facilitate interpretation of main schooling effects, interactions use sample mean deviates of continuous variables (such as the log pupil-teacher ratio). Results appear in Table 5.

Consider first the results in columns (1) and (2), in which we maintain the linear assumption of prior sections and estimate the log wage regression first without, and then with, source-country fixed effects. These columns offer a robustness check of the two-step estimator, as, in the absence of specification errors, the coefficients of the interaction terms in column (2) should be equivalent to those in Table 3, column (5). As a comparison of the two tables reveals, coefficient estimates are very close.

In columns (3) and (4), we introduce the spline specification of educational attainment. Results support the notion of convexity of the schooling–log wage profile for U.S. immigrants. According to the estimates in column (3), each year of schooling raises wages of the baseline group by .0078 log points for the first nine years of schooling. Returns then increase by a significant .0186 log points after 9 years and an additional .0516 log points after 12 years of schooling. In other words, in the immigrant sample the return to each year of schooling beyond high school is 8.1% (exp[.0078 + .0186 + .0516] − 1). Allowing for a nonlinear education-wage profile reduces the magnitudes of school quality effects, but estimates remain statistically significant and within the range of estimates obtained from the two-step approach.

The specification in columns (5) and (6) allows for differential school quality effects in each of the three segments of the spline function. Although estimates of column (5) suggest that the pupil-teacher ratio has the largest impact on returns to the first nine years of education, we do not uncover significant differences in the pupil-teacher ratio effect across segments of the spline. Expenditures per pupil, on the other hand, have a significantly larger impact on returns at midrange levels of education than at lower or higher levels of educational attainment. In summary, results in Table 5 show that the finding that school quality affects the returns to education is not the consequence of failure to account for convexity of the schooling-wage relationship.[16]

Educational Attainment and Returns to Education

We conclude this section with some observations on the relation between educational attainment and returns to education across groups. First, there appears to be a discrepancy between the international and the U.S. evidence on this relationship. Psacharopoulos (1994) finds a negative correlation between returns to education and attainment across countries and attributes this to diminishing marginal returns to investments in education. In contrast, across states of birth and birth cohorts in the United States the association is positive. In the models of Card and Kreuger (1996a) and Heckman et al. (1996), for example, the positive relation arises because higher market returns provide an incentive for students to attend school longer. The U.S. empirical evidence also points to a positive effect of school quality on educational attainment (Johnson and Stafford, 1973; Card and Krueger, 1992a; Heckman et al., 1996a).

16. We reach similar conclusions—the effect of the pupil-teacher ratio is greatest at low levels of attainment and the effect of expenditures per pupil increases with attainment—when we introduce nonlinearity in the schooling-wage profile through discrete intercept shifts rather than rotation of the slope as in the spline function.

TABLE 5
Log Wage Regressions with Education-School Quality Interactions

	Linear		Spline			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Education	.0291** (.0011)	.0292** (.0012)	.0078** (.0013)	.0092** (.0013)	.0064** (.0013)	.0073** (.0014)
Education > 9			.0186** (.0021)	.0128** (.0021)	.0210** (.0022)	.0163** (.0022)
Education > 12			.0516** (.0020)	.0555** (.0020)	.0496** (.0021)	.0522** (.0021)
Education* ln(Pupil-teacher Ratio)	−.0207** (.0019)	−.0246** (.0020)	−.0111** (.0019)	−.0143** (.0020)	−.0166** (.0049)	−.0107* (.0049)
Education > 9* ln(Pupil-teacher Ratio)					.0166 (.0109)	−.0069 (.0110)
Education > 12* ln(Pupil-teacher Ratio)					−.0150 (.0088)	−.0019 (.0088)
Education* ln(Expenditure per Pupil)	.0135** (.0010)	.0091** (.0010)	.0080** (.0010)	.0033** (.0010)	−.0040 (.0028)	−.0059* (.0028)
Education > 9* ln(Expenditure per Pupil)					.0303** (.0066)	.0206** (.0067)
Education > 12* ln(Expenditure per Pupil)					−.0178** (.0055)	−.0081 (.0055)
Education* Compulsory Schooling	.0015** (.0002)	.0006* (.0003)	.0001 (.0002)	−.0009** (.0003)	.0001 (.0002)	−.0009** (.0003)
Education* ln(Per-capita GDP)	.0002 (.0009)	−.0028** (.0010)	.0014 (.0009)	−.0015 (.0010)	.0010 (.0009)	−.0023* (.0010)
Education* Income Inequality	−.0005** (.0001)	−.0004** (.0001)	−.0005** (.0001)	−.0004** (.0001)	−.0005** (.0001)	−.0005** (.0001)
Education* English Official Lang.	.0157** (.0013)	.0258** (.0014)	−.0006 (.0013)	.0090** (.0014)	−.0003 (.0013)	.0094** (.0015)
Education* Communist	−.0148** (.0017)	−.0115** (.0019)	−.0206** (.0017)	−.0166** (.0018)	−.0209** (.0017)	−.0172** (.0018)
Education* Coup or Revolution	−.0001 (.0010)	.0024* (.0010)	−.0040** (.0010)	−.0011 (.0010)	−.0037** (.0010)	−.0004 (.0011)
Education* Assassinations	−.0053** (.0006)	−.0055** (.0006)	−.0031** (.0006)	−.0032** (.0006)	−.0029** (.0006)	−.0031** (.0006)
Education* Americas	.0120** (.0012)	.0068** (.0012)	.0160** (.0012)	.0114** (.0012)	.0159** (.0012)	.0111** (.0013)
Education* Asia	.0305** (.0013)	.0243** (.0015)	.0105** (.0014)	.0046** (.0015)	.0109** (.0014)	.0050** (.0015)
Education* Africa	.0313** (.0043)	.0273** (.0045)	.0080 (.0043)	.0043 (.0045)	.0078 (.0043)	.0026 (.0045)
Education* 1980 Observation	−.0077** (.0008)	−.0084** (.0008)	−.0099** (.0008)	−.0101** (.0008)	−.0100** (.0008)	−.0102** (.0008)
ln(Pupil-teacher Ratio)	.1178** (.0253)	.1712** (.0345)	.0347 (.0251)	.0442 (.0344)	.0579 (.0363)	.0323 (.0429)
ln(Expenditure per Pupil)	−.0520** (.0129)	−.0926** (.0168)	.0004 (.0128)	−.0281 (.0168)	.0598** (.0198)	.0167 (.0222)
Compulsory Schooling	−.0069* (.0032)	−.0097* (.0044)	.0103** (.0032)	.0103* (.0043)	.0102** (.0032)	.0099* (.0043)
ln(Per-capita GDP)	.0701** (.0125)	.1733** (.0242)	.0617** (.0124)	.1546** (.0240)	.0649** (.0125)	.1644** (.0241)
Income Inequality	.0025** (.0010)	−.0082 (.0048)	.0029** (.0010)	−.0056 (.0047)	.0030** (.0010)	−.0047 (.0047)
English Official Lang.	−.1002** (.0172)		.1049** (.0175)		.1023** (.0175)	

continued

TABLE 5 continued

	Linear		Spline			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Communist	.0969** (.0219)		.1736** (.0218)		.1786** (.0218)	
Coup or Revolution	.0140 (.0123)	−.0199 (.0159)	.0521** (.0122)	.0366* (.0159)	.0486** (.0123)	.0283 (.0160)
Assassinations	.0350** (.0058)	.0514** (.0074)	.0125* (.0058)	.0270** (.0074)	.0111 (.0058)	.0255** (.0074)
Americas	−.2306** (.0146)		−.2695** (.0145)		−.2683** (.0146)	
Asia	−.3413** (.0181)		−.1097** (.0184)		−.1132** (.0185)	
Africa	−.4099** (.0657)		−.1049 (.0654)		−.1067 (.0659)	
1980 Observation	−.3606** (.0502)	−.4086** (.0506)	−.3140** (.0498)	−.3693** (.0502)	−.3165** (.0499)	−.3692** (.0503)
Constant	4.3053** (.0350)		4.526** (.0351)		4.5311** (.0351)	
Regression includes country fixed effects?	No	Yes	No	Yes	No	Yes
R^2	.3394	.3463	.3498	.3561	.3499	.3563

Notes: Sample size is 204,712. Standard errors are reported in parentheses. Regressions also include age and its square, marital status, English fluency, SMSA, health, eight census divisions, nine immigrant cohorts, and interactions between each of these variables and the 1980 indicator.

*Statistically significant at the 5% level (two-tailed test).

**Statistically significant at the 1% level (two-tailed test).

Interestingly, our estimates of rates of return to education are negatively correlated with returns to investment in education calculated within each nation such as those compiled by Psacharopoulos (1985, 1994). For example, table A2 of Psacharopoulos (1994) lists coefficients of schooling from log wage regressions for 62 separate countries, most based on microsamples drawn between 1980 and 1990. For the countries that overlap, the correlation coefficients between source-country estimates of the rate of return to schooling and the U.S. estimates listed in Table 1 are −.54 for the 1980 data and −.57 for the 1990 data. Furthermore, average educational attainment in our samples, average schooling in the source population, and enrollment in postsecondary education are all positively related to our school quality measures and to U.S. returns to education but are negatively related to returns to education in the source country.

On further consideration, the contrast between our results and those of Psacharopoulos was quite predictable. As Schultz (1988) observes, returns to education within any one nation are primarily driven by the aggregate quantity of educated workers and other factors of production. However, the supply of educated workers in the U.S. labor market is mainly determined by U.S. natives—educational attainment in other nations has little impact on the quantity of education available in the U.S. labor market. Thus our measures of returns to education for each nation are influenced by very different factors (such as quality of education) than those reported by Psacharopoulos. The positive correlation between our returns and attainment is consistent with the argument that better-quality education leads to increases in attainment, though further research is needed to provide any conclusive evidence on this issue.[17]

17. An alternative explanation is that both attainment and quality of education are positively correlated with real GDP. Increases in income may lead individuals to choose more education and to improve the quality of education as well.

VI. CONCLUSION

This article examines the relationship between attributes of a country's educational system and the rate of return to education received by U.S. immigrants from that country. Results reveal that differences in the attributes of educational systems account for most of the variation in rates of return to education earned by immigrants applying their source-country education in the U.S. labor market. We find a particularly robust inverse relationship between the rate of return to education and the pupil-teacher ratio in primary schools in the source country, and similarly robust direct relationships between the rate of return and relative teacher wages and expenditures per pupil in the source country. The methodology applied in the study also yields several other interesting results.

The results from the first-step regressions estimating rates of return to education for immigrants also supply an index of the quality of a nations education system. As such, Table 1 shows that Japan, Australia, Canada, and northern European nations provide the highest-quality education, with the lowest-quality education coming from educational systems of Caribbean nations. A potentially important application of such rankings is that they complement educational attainment in cross-country studies of the relation between human capital and economic growth.

The study also makes important contributions to the immigration literature. Because the valuation of an immigrant's education in the U.S. labor market depends on the investments made in the educational system in the source country, differences in educational investments create disparities in U.S. earnings across immigrant groups. Indeed, the immigration literature has long recognized source-country effects in labor market outcomes of U.S. immigrants (Chiswick, 1978, 1986; Jasso and Rosenzweig, 1986, 1990; Borjas, 1987, 1993; Borjas and Bratsberg, 1996); the link between school quality and the rate of return to education provides another explanation of the existence of source-country effects.

Cross-country growth regressions, development economists, and World Bank policies continue to stress quality education as a key to economic development. The results of this study affirm the linkage between the attributes of a nation's educational system and the productivity of workers educated in that system. These results provide evidence of potential productivity gains from increases in expenditures per pupil and improvements in pupil-teacher ratios and also provide estimates of the return to such investments in educational systems. As most economists have long maintained, improving the quality of the educational system enhances the productivity of workers receiving that education, even when the education is applied in a very different environment from where it was obtained.

APPENDIX A: DATA

This appendix details data sources and the construction of variables used in the empirical analyses.

Rates of Return to Education by Country of Origin

We estimate rates of return to education using wage regressions in microdata samples drawn from the 5/100 public use samples of the 1980 and 1990 censuses of population. In the two-step analysis, we run separate regressions for each census, thereby allowing every parameter of the wage model to change between census years. The dependent variable of the wage regression is the natural log of the weekly wage, constructed as 1979 or 1989 wages or salary income divided by the number of weeks worked that year. The wage regressions include a standard set of control variables: age and its square and dummy variables for English fluency (speak English well or very well), married with spouse present, residence in an SMSA, health limiting work, eight census divisions, and five (nine in 1990 sample) immigrant cohorts. We obtain the estimate of the country-of-birth specific rate of return to education as the coefficient on the interaction term between a country-specific dummy variable and years of schooling of the individual.

Samples are restricted to immigrant males who arrived in the United States after completing their schooling. During the initial phase of the project, we focused on immigrants from 67 countries chosen on the basis of cell sizes in census data and availability of school quality characteristics. We later dropped two countries—China and Switzerland—from the second-step analyses because we expanded the set of school quality characteristics to include variables unavailable for these countries. Because the census questionnaire does not ask the year of graduation of the individual, we infer year of graduation as year of birth plus six plus years of schooling. Also, the census data only gives the year of immigration in five-year intervals (with the exception of immigrants who arrived during the 1980s for whom year of immigration is known in two- or three-year intervals).

We exclude persons from the regression sample if the inferred year of graduation falls within or after the five-year immigration interval. We also exclude persons who report being enrolled in school during the census year or earned less than $1,000 during the year preceding the census. Finally, we exclude persons less than 25 years of age and alternately impose two upper age restrictions: 64 and 35. The latter age group is designed to match up (i.e., they would have been 5–15 years of age) with the years for which we collect school quality characteristics, 1960 for immigrants in the 1980 census and 1970 for those in the 1990 census.

The sample restrictions leave sample sizes of 86,728 (1980) and 125,503 (1990) for the full sample and 26,414 (1980) and 42,459 (1990) for the restricted age group sample. Descriptive statistics for the full samples are presented in Table A1.

In the 1980 census data, we base years of schooling on the "highest year of schooling attended" question, and subtract one year if the respondent did not finish the highest grade attended. In the 1990 data, we convert educational attainment to years of schooling using the following rule: years of schooling equals zero if educational attainment is less than first grade; 2.5 if first through fourth; 6.5 if fifth through eighth, educational attainment if ninth, tenth, eleventh, or twelth; 12 if GED earned; 13 if some college, but no degree; 14 if associate degree; 16 if bachelor's degree; 18 if master's degree; 19 if professional degree; and 20 if doctorate degree. See Jaeger (1997) for a discussion of alternative conversion rules.

Immigration Rates by Educational Level

To form variables that allow us to control for immigration selectivity in the first-step regression models, we compute immigration rates for three levels of schooling (corresponding to the primary, secondary, and post-secondary levels). The computation uses the number of male immigrants with the level of schooling in the 5/100 public use sample of the census (I_{jlt}, where j subscripts country of birth, l level of schooling, and t census year), the percentage in the male source country population having attained the level of schooling (p_{jlt}), and the source country population (pop_{jt}). We compute

TABLE A1
Descriptive Statistics—First-Step Regression Samples

	1980 Census (Sample Size = 86,728)		1990 Census (Sample Size = 125,503)	
	Mean	**SD**	**Mean**	**SD**
ln(Weekly Wage)	5.616	.678	6.019	.756
Years of Schooling	10.399	4.998	10.138	5.349
Age	43.204	11.067	41.755	10.586
Age Squared	1,989.090	980.293	1,855.530	925.507
Speaks English Well or Very Well	.705	.456	.660	.474
Married Spouse Present	.841	.365	.777	.416
SMSA	.904	.294	.947	.225
Health Limiting Work	.035	.184	.032	.176
Region				
New England	.063	.244	.048	.213
Mid-Atlantic	.254	.435	.201	.401
East North Central	.127	.333	.081	.273
West North Central	.015	.123	.010	.101
South Atlantic	.109	.312	.134	.340
East South Atlantic	.006	.077	.005	.069
West South Atlantic	.076	.265	.100	.301
Mountain	.031	.174	.038	.190
Year of Immigration				
1985–86			.108	.310
1982–84			.117	.322
1980–81			.137	.344
1975–79			.175	.380
1970–74	.227	.419	.140	.347
1965–69	.176	.381	.091	.288
1960–64	.117	.322	.057	.231
1950–59	.128	.334	.038	.192
Pre-1950	.068	.251	.002	.047

the migration rate (m_{jlt}) as

(A1) $$m_{jlt} = 20 * I_{jlt}/(20 * I_{jlt} + p_{jlt} * .5 * pop_{jt}).$$

We collect data on p_{jlt} from Barro and Lee (1996). For seven countries not included in the Barro and Lee data set, we compute p_{jlt} from enrollment ratios lagged 20 years. The enrollment data are drawn from UNESCO (various years). Finally, we collect population figures from Summers and Heston (1991), Banks (various years), and U.S. Bureau of the Census (1996). The computed migration rates are listed in Table A2. The table also contains summary statistics.

Source-Country School Quality Measures

We collect data on school quality characteristics from 1960 and 1970 (to be linked with estimated returns to education from 1980 and 1990, respectively). Descriptive statistics are presented in Table A3.

The pupil-teacher ratios in primary schools are collected from UNESCO (various years). For 1970, the data

TABLE A2
Estimated Migration Rate for U.S. Immigrant Males by Schooling and Country of Birth

	1980 Census				1990 Census			
Country/Schooling	**0–6**	**7–12**	**13–20**	**Weighted Average**	**0–6**	**7–12**	**13–20**	**Weighted Average**
Europe								
Austria	.0050	.0141	.0989	.0149	.0014	.0069	.0450	.0094
Belgium	.0011	.0037	.0110	.0033	.0007	.0033	.0111	.0034
Czechoslovakia	.0015	.0158	.0206	.0064	.0005	.0087	.0204	.0049
Denmark	.0018	.0117	.0146	.0081	.0007	.0063	.0182	.0068
Finland	.0019	.0061	.0102	.0046	.0004	.0048	.0088	.0035
France	.0003	.0046	.0094	.0023	.0003	.0024	.0098	.0022
Germany	.0037	.0433	.0437	.0148	.0037	.0249	.0459	.0153
Greece	.0098	.0498	.0482	.0232	.0027	.0289	.0609	.0180
Hungary	.0156	.0075	.0484	.0123	.0007	.0152	.0411	.0100
Ireland	.0089	.0725	.1015	.0420	.0040	.0541	.0999	.0385
Italy	.0087	.0216	.0370	.0147	.0025	.0184	.0229	.0104
Netherlands	.0021	.0074	.0206	.0075	.0008	.0059	.0184	.0068
Norway	.0590	.0116	.0289	.0153	.0006	.0153	.0274	.0107
Poland	.0040	.0153	.0335	.0105	.0018	.0099	.0361	.0092
Portugal	.0103	.0639	.0252	.0178	.0067	.0800	.0432	.0190
Romania	.0008	.0038	.0099	.0028	.0008	.0035	.0179	.0038
Spain	.0010	.0076	.0058	.0025	.0005	.0054	.0088	.0026
Sweden	.0033	.0125	.0121	.0088	.0009	.0067	.0159	.0064
Switzerland	.0023	.0062	.0166	.0069	.0009	.0040	.0213	.0064
UK	.0023	.0165	.0248	.0104	.0016	.0117	.0347	.0111
USSR	.0011	.0019	.0021	.0017	.0008	.0008	.0034	.0012
Yugoslavia	.0028	.0116	.0172	.0069	.0010	.0082	.0154	.0055
Asia								
China	.0003	.0001	.0189	.0003	.0003	.0002	.0065	.0005
Hong Kong	.0048	.0160	.0803	.0161	.0040	.0151	.1072	.0236
India	.0001	.0002	.0065	.0003	.0000	.0006	.0060	.0006
Indonesia	.0000	.0005	.0113	.0002	.0000	.0004	.0092	.0003
Iran	.0005	.0057	.0741	.0040	.0004	.0039	.0630	.0042
Iraq	.0008	.0075	.0172	.0027	.0002	.0056	.0134	.0024
Israel	.0093	.0180	.0370	.0199	.0066	.0233	.0346	.0202
Japan	.0013	.0021	.0048	.0023	.0012	.0018	.0060	.0027
Korea	.0046	.0044	.0197	.0066	.0080	.0075	.0299	.0121
Lebanon	.0071	.0544	.0382	.0226	.0033	.0691	.0653	.0284
Malaysia	.0002	.0008	.0221	.0008	.0002	.0011	.0344	.0018
Pakistan	.0001	.0006	.0108	.0005	.0001	.0012	.0139	.0009
Philippines	.0042	.0172	.0306	.0111	.0022	.0295	.0358	.0139
Singapore	.0008	.0030	.0250	.0024	.0012	.0037	.0471	.0048
Sri Lanka	.0001	.0002	.0208	.0004	.0000	.0005	.0265	.0008
Taiwan	.0014	.0031	.0205	.0043	.0017	.0061	.0440	.0111
Thailand	.0004	.0027	.0144	.0011	.0006	.0129	.0074	.0018
Turkey	.0004	.0043	.0085	.0013	.0002	.0027	.0077	.0011

continued

TABLE A2 continued

	1980 Census				1990 Census			
Country/Schooling	0–6	7–12	13–20	Weighted Average	0–6	7–12	13–20	Weighted Average
Africa								
Egypt	.0002	.0010	.0102	.0012	.0001	.0014	.0101	.0015
Kenya	.0001	.0006	.0488	.0005	.0000	.0012	.0383	.0007
Morocco	.0001	.0024	.0207	.0007	.0000	.0029	.0190	.0009
Sierra Leone	.0001	.0017	.0690	.0008	.0001	.0045	.0977	.0016
Tanzania	.0000	.0023	.0054	.0002	.0000	.0024	.0620	.0003
Uganda	.0001	.0022	.0382	.0003	.0000	.0019	.0483	.0005
Oceania								
Australia	.0013	.0019	.0045	.0023	.0010	.0021	.0051	.0027
New Zealand	.0049	.0025	.0057	.0038	.0011	.0082	.0067	.0051
North America								
Canada	.0156	.0494	.0270	.0318	.0106	.0219	.0550	.0280
Costa Rica	.0032	.0504	.0355	.0114	.0021	.0662	.0347	.0134
Cuba	.0144	.1478	.3450	.0575	.0095	.1177	.1892	.0635
Dominican Republ	.0089	.1172	.0634	.0252	.0079	.1708	.0701	.0360
El Salvador	.0066	.1099	.1120	.0180	.0218	.5871	.1204	.0802
Guatemala	.0033	.0513	.0478	.0086	.0059	.2206	.0602	.0231
Haiti	.0030	.0885	.3505	.0164	.0052	.1133	.4875	.0303
Honduras	.0027	.0565	.0638	.0087	.0039	.1029	.0603	.0176
Jamaica	.0182	.2539	.4614	.0763	.0125	.1883	.5142	.0954
Mexico	.0234	.1160	.0332	.0339	.0257	.1335	.0488	.0562
Panama	.0103	.0627	.1410	.0358	.0084	.0534	.1072	.0384
Trinidad and Tobago	.0093	.1314	.2842	.0540	.0072	.1105	.3967	.0645
South America								
Argentina	.0005	.0053	.0156	.0025	.0003	.0045	.0123	.0029
Brazil	.0001	.0019	.0023	.0003	.0001	.0060	.0033	.0006
Chile	.0008	.0052	.0166	.0033	.0006	.0076	.0178	.0042
Colombia	.0013	.0148	.0272	.0053	.0012	.0355	.0336	.0081
Ecuador	.0026	.0377	.0295	.0107	.0017	.1194	.0178	.0117
Peru	.0008	.0060	.0116	.0034	.0008	.0214	.0155	.0063
Uruguay	.0014	.0100	.0242	.0048	.0009	.0151	.0211	.0068
Mean (unweighted)	.0047	.0281	.0507	.0112	.0029	.0393	.0558	.0140
SD	.0084	.0451	.0848	.0148	.0047	.0839	.0961	.0196

Notes: The migration rate is computed for each education level as U.S. male immigrants/(country-of-birth male population + U.S. male immigrants). Data sources are Barro and Lee (1996), Summers and Heston (1991), UNESCO (various years), U.S. Bureau of the Census (1996), and tabulations from 5/100 public use samples of the 1980 and 1990 censuses of population.

TABLE A3

Descriptive Statistics—School Quality Characteristics

	1960 Data		1970 Data		Correlation between 1960 and 1970 Data
Variable	Mean	SD	Mean	SD	
Pupil-teacher Ratio	33.7	8.6	30.8	8.9	.895
Expenditure per Pupil/ per-capita GDP	.212	.112	.221	.104	.716
Compulsory Schooling	6.1	3.0	6.8	2.9	.822
Per-capita GDP	2,798.6	2,613.5	4,072.0	3,067.5	.973

Note: Sample size is 65.

source lists the pupil-teacher ratio, and for 1960 we compute the ratio from enrollment in primary schools and the number of primary-school teachers. These data cover both private and public schools.

We base the measure of expenditures per pupil on government educational expenditures as percentage of GDP. The educational expenditure data refer to recurring expenditures over the five-year period following 1960 or 1970 and are collected from Barro and Lee (1993). For countries not included in the Barro and Lee data set, we apply their method and compute recurring educational expenditure percentages based on data drawn from UNESCO (various years). We calculate nominal expenditures per pupil as educational expenditures as percentage of GDP multiplied by GDP divided by total student enrollment. GDP is computed from per capita GDP (in constant $ chain indexed 1985 international prices) and population size. The GDP and population data for 1960 and 1970 are collected from Summers and Heston (1991), except for two countries not included in the Summers and Heston data (Cuba and Lebanon) and three observations from 1960 missing in these data. For these data points, we collect population and per capita gross national product (GNP) figures from U.S. Arms Control and Disarmament Agency (1984), and impute per capita GDP from per capita GNP figures and sample means of per capita GDP and per capita GNP for countries with nonmissing GDP figures in the Summers and Heston data set. Empirical results presented herein are not sensitive to the exclusion of data points, for which we were forced to impute GDP figures.

Finally, we collect the duration (in years) of compulsory education from UNESCO (various years).

Other Source-Country Characteristics

Other source country characteristics used in the empirical analyses include a measure of income inequality, indicator variables for English being the official language, communist regime, and coup or revolution, the number of assassinations per million population, and indicator variables for continent. Summary statistics are presented in Table 2.

We construct the measure of income inequality as the ratio of income accruing to the top 10% of households to income accruing to the bottom 20% of households. These data are drawn from Jain (1975), Taylor and Jodice (1983) and the World Bank (various years). Because these data are unavailable for the early 1960s for a large number of countries in our sample, we use data from around 1970 and 1980.

Data on official language and political status are collected from Banks (various years), data on coups and revolutions from Taylor and Jodice (1983) and Banks (various years), and data on assassinations from Barro and Lee (1993) and Banks (various years). The assassinations variable reflects the number of politically motivated murders or attempted murders of high government officials or politicians during the 1960s or 1970s, respectively. We construct the variable by adding up the number of assassinations per million population for each year during the decade.

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