

The Place Premium: Bounding the Price Equivalent of Migration Barriers

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Abstract: Large international differences in the price of labor can be sustained by differences between workers, or by natural and policy barriers to worker mobility. We use migrant selection theory and evidence to place lower bounds on the *ad valorem* equivalent of labor mobility barriers to the United States, with unique nationally-representative microdata on both U.S. immigrant workers and workers in their 42 home countries. The average price equivalent of migration barriers in this setting, for low-skill males, is greater than \$13,700 per worker per year. Natural and policy barriers may each create annual global losses of trillions of dollars.

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1 Introduction

Economists often study the costs of frictions in international commerce by estimating their *ad valorem* equivalent. Such estimates are made for frictions that include trade quotas (e.g. [Anderson and van Wincoop 2004](#)), transportation costs (e.g. [Hummels 2007](#)), and capital controls ([Edwards et al. 1999](#)). But there are no systematic estimates of the price equivalent of barriers to the international movement of labor. Both the simple “Harberger triangle” intuition that the welfare losses rise with the square of the price distortion and calibrated models of the world economy suggest that if the price equivalent of migration barriers is high, the annual global costs are trillions of dollars.¹

We use a unique collection of data sets on individuals’ wages from 42 developing countries and the United States to place lower bounds on the price equivalent of barriers to labor mobility into the U.S. market. We estimate the real (Purchasing Power Parity) wage gaps between immigrants in the United States and their observably-equivalent national counterparts in the 42 home labor markets. We then use theory and evidence on migrant self-selection to bound the real wage gap for fully equivalent workers—adjusted for both observable and unobservable characteristics. We call this wage gap a ‘place premium’ because it does not arise from portable individual traits. We then use these bounds on the place premium to discuss what fraction of this price wedge might plausibly be attributed to natural barriers and what fraction to policy barriers.

¹Surveyed by [Clemens \(2011\)](#), and recently investigated by [Benhabib and Jovanovic \(2012\)](#); [Kennan \(2013\)](#); [di Giovanni et al. \(2015\)](#).

Our focus is on prime-age, low-skill males educated abroad (35–39 years old, 9–12 years of education acquired in the home country), though we present estimates for other demographic categories as well. We calculate lower bounds on the ratio of real wages in the U.S. to real wages of an identical worker in each home country. This lower bound varies greatly across countries, from a high of 16.4 for Yemen to a low of 1.7 for Morocco. Weighted by the working-age (15–49) population of the home countries, the average lower bound on this wage ratio is 5.65. For the median country the lower bound is 3.95, and for the 80th percentile country the lower bound is 6.14. The working-age population weighted average of the lower bound on the absolute wage gain is PPP\$13,710/year across 1.5 billion working-age people from the 42 countries. The lower bound absolute gain for workers from the median country is PPP\$13,600, and for the 80th percentile country it is PPP\$15,600.

We cannot separately estimate for each country the relative contributions of natural and policy barriers. That said, we note that spatially integrated labor markets in the absence of policy barriers rarely sustain real wage ratios above 1.5—even in the presence of important cultural and geographic barriers. This suggests a plausible prior that policy barriers to labor mobility account for at least as much of the observed gap in wages of fully equivalent workers than do natural barriers to movement, such as psychic costs or transportation costs.

The empirical contribution of this work is the first country-specific bounds on the price equivalent of migration barriers using data on nationally-representative samples of individual workers from the same country working on both sides of the border.² The methodological contribution is to propose new measures of selection bias

²[Ashenfelter \(2012\)](#) measures large real wage gaps between several countries

in these estimates—derived from the theory of migrant self-selection, and predicting patterns in the estimates by country of origin, by skill group, and by labor-market outcomes in the destination country.

2 Wage ratios for observably equivalent workers

Our calculations analogous to *ad valorem* measures of trade barriers. We seek to place bounds on similar price ratios for labor—wage ratios—as *ad valorem* measures of natural and policy barriers to labor mobility.

2.1 Defining the wage ratios

R_u is the *unconditional* ratio of migrants’ wages in the United States to wages in the home country, without adjustment for observable or unobservable differences between average migrants and average non-migrants. R_c is the ratio *conditional* on observable inherent differences like age and education. Finally, R accounts for all inherent differences, both observable and unobservable. That is, ratio R measures the real wage gain that the same person could expect in the U.S. relative to the home country.

within one low-skill occupation: fast-food workers. Multiple studies use microdata on migrants to find that country of residence is at least as important a determinant of worker productivity as inherent characteristics, but do not estimate international labor-price wedges separately by country ([Hendricks 2002](#); [Milanovic 2015](#); [Hendricks and Schoellman 2018](#)).

Formally, suppose that a worker born and educated in a foreign country would earn w_0 in that home country and earn w_{US} in the United States, and that w_0 and w_{US} are determined by:

$$\ln w_0 = \left(\mu_0 + \gamma_0 s \right) + \tilde{\gamma}_0 \tilde{s} \quad \equiv \mu'_0(s) + \tilde{\gamma}_0 \tilde{s} \quad (1)$$

$$\ln w_{\text{US}} = \left(\mu_{\text{US}} + \gamma_{\text{US}} s \right) + \tilde{\gamma}_{\text{US}} \tilde{s} \quad \equiv \mu'_{\text{US}}(s) + \tilde{\gamma}_{\text{US}} \tilde{s}, \quad (2)$$

where $s \geq 0$ is observed skill, which has return γ_0 abroad and γ_{US} in the United States; $\tilde{s} \sim N(0, \sigma)$ is unobserved skill, which has return $\tilde{\gamma}_0$ abroad and $\tilde{\gamma}_{\text{US}}$ in the United States. Fundamental differences in worker productivity between the two countries are captured by μ_{US} and μ_0 .

The three wage ratios of interest can then be defined as

$$\ln R_u \equiv \mu_{\text{US}} - \mu_0 + \left(\gamma_{\text{US}} E_{\text{US}}[s] - \gamma_0 E_0[s] \right) + \left(\tilde{\gamma}_{\text{US}} E_{\text{US}}[\tilde{s}] - \tilde{\gamma}_0 E_0[\tilde{s}] \right) \quad (3)$$

$$\ln R_c(s) \equiv \mu_{\text{US}} - \mu_0 + \left(\gamma_{\text{US}} - \gamma_0 \right) E_{\text{US}}[s] + \left(\tilde{\gamma}_{\text{US}} E_{\text{US}}[\tilde{s}] - \tilde{\gamma}_0 E_0[\tilde{s}] \right) \quad (4)$$

$$\ln R(s, \tilde{s}) \equiv \mu_{\text{US}} - \mu_0 + \left(\gamma_{\text{US}} - \gamma_0 \right) E_{\text{US}}[s] + \left(\tilde{\gamma}_{\text{US}} - \tilde{\gamma}_0 \right) E_{\text{US}}[\tilde{s}]. \quad (5)$$

where E_0 and E_{US} denote expectations—across residents of the home country and residents of the United States, respectively—for people born in the home country. The ratio R is the ‘place premium’, the real wage premium that a worker earns by working in the United States rather than their home country.

The ratios R_u , R_c , and R compactly summarize migrant selection on observed and unobserved wage determinants. $R_u/R_c > 1$ if and only if there is positive selection of migrants on observables, since $\ln(R/R_c) = \gamma_0(E_{\text{US}}[s] - E_0[s]) > 0 \Leftrightarrow E_{\text{US}}[s] >$

$E_0[s]$. Likewise, $R_c/R > 1$ if and only if there is positive selection of migrants on unobservables, since $\ln(R_c/R) = \tilde{\gamma}_0(E_{\text{US}}[\tilde{s}] - E_0[\tilde{s}]) > 0 \Leftrightarrow E_{\text{US}}[\tilde{s}] > E_0[\tilde{s}]$.

To begin to estimate these ratios, for each country of birth we run a separate regression for each country (other than the U.S.) where the sample includes all workers born that country, whether they reside in the home country or the U.S.:

$$\ln w = \alpha + \beta I_{\text{US}} + \mathbf{I}'_{\text{edu}} \left(\boldsymbol{\eta}_{\text{edu}} + \boldsymbol{\zeta}_{\text{edu}} I_{\text{US}} \right) + \mathbf{I}'_{\text{age}} \left(\boldsymbol{\eta}_{\text{age}} + \boldsymbol{\zeta}_{\text{age}} I_{\text{US}} \right) + I_{\text{fem}} \left(\eta_{\text{fem}} + \zeta_{\text{fem}} I_{\text{US}} \right) + \varepsilon, \quad (6)$$

where w is the monthly wage in U.S. dollars, and I_{US} is equal to one if the person lives in the United States, zero otherwise. \mathbf{I}_{edu} and \mathbf{I}_{age} are vectors of indicator variables for different groupings of years-of-education and quinquennial age, and I_{fem} is an indicator for female.³ To be estimated are the parameters α , β , η_{fem} , and ζ_{fem} , and the parameter vectors $\boldsymbol{\eta}_{\text{edu}}$, $\boldsymbol{\zeta}_{\text{edu}}$, $\boldsymbol{\eta}_{\text{age}}$, $\boldsymbol{\zeta}_{\text{age}}$, while ε is an error term. This specification allows all observable traits to have different returns in the two countries, and assumes less about functional form than a model linear in traits. The key parameters are β and the vectors $\boldsymbol{\zeta}$.

³The six education categories are 1) no schooling, 2) 1–4 years of schooling, 3) 5–8 years, 4) 9–12 years, 5) 13–16 years, and 6) 17–28 years. The ten age categories are 1) 15–19, 2) 20–24, 3) 25–29, 4) 30–34, 5) 35–39, 6) 40–44, 7) 45–49, 8) 50–54, 9) 55–59, 10) 60–65 (intentionally includes 65). The regressions also include dummy variables for the periodicity of wage reported (daily, weekly, etc.), suppressed here for clarity, with monthly as the base group.

2.2 Results

We use a unique standardized collection of individual level data sets on wage-earners compiled by the World Bank, combined with the US Census Public Use Microdata Sample (PUMS) five percent file.⁴ The unified database describes 2,015,411 individual wage-earners, age 15 to 65, residing in 43 countries close to the year 2000. This comprises 891,158 individuals residing in 42 developing countries, 623,934 individuals born in those same 42 developing countries but residing in the US, and 500,319 individuals born in the US and residing in the US. Wages are measured in 1999 US dollars at Purchasing Power Parity (PPP).

[Table 1](#) presents estimates of R_u and R_c where wages are measured in Purchasing Power Parity (PPP) U.S. dollars. The first column shows $\hat{\beta}$ without any controls for education, age, and sex; the second column shows $\hat{\beta} + \hat{\zeta}_{\text{edu}}^{9-12} + \hat{\zeta}_{\text{age}}^{35-39}$ with controls included.⁵ The third column repeats the regressions with controls, but drops all

⁴Details of the database and all sources are given in the Appendix.

⁵The difference between the first and second column of results matches several results in the literature. The fact that the wage ratio falls for most countries when basic observable controls are added implies positive selection of migrants on observable determinants of earnings, in agreement with e.g. [Brücker and Defoort \(2009\)](#). The small change in the coefficient for Mexico when controls are added is compatible with previous findings of approximately neutral selection on observables for Mexico-U.S. migrants ([Chiquiar and Hanson 2005](#)), and the fact that the coefficient does fall slightly is compatible with findings of modest negative selection on basic observables (e.g. [Fernández-Huertas 2011](#); [Ambrosini et al. 2015](#)). The ratio likewise

U.S.-resident workers who were less than 20 years old when they arrived in the country. This eliminates most workers who received U.S. education, since domestic education and foreign education can have markedly different returns (e.g. [Friedberg 2000](#)). These last results are converted to the wage ratio \hat{R}_c for the final column, and countries are sorted in decreasing values of this ratio.

The estimated wage ratios are very large. For the working-age population weighted average country of birth, $R_c = 6.84$ while for the median country of birth, $R_c = 4.5$ (five of the six largest countries in our sample (India, Indonesia, Bangladesh, Pakistan, Nigeria) have estimates above the median). These ratios represent the difference in purchasing power-adjusted wages between immigrants to the U.S. who received their education in the home country and observably equivalent workers in their country of origin—35–39 year-old male workers with 9–12 years of education who were born and educated in that home country.

The ratios are quite precisely estimated. For the ratios R_c , the t -statistic is above 10 in 38 out of 42 countries. Standard errors on \hat{R}_c are bootstrapped with 500 draws to avoid the retransformation problem. The Appendix presents robustness checks for other ages, recently-arrived workers, and males only, and discusses the potential for reporting bias. All of these PPP-dollar wage ratios presume that wages are spent at U.S. prices, and are thus a conservative estimate of the gain to the extent that migrants remit a portion of wages to their (lower-price) home country.⁶

rises slightly between columns 1 and 2 for Nicaragua, as in [Barham and Boucher's \(1998\)](#) finding of negative selection.

⁶The Appendix presents a reestimate of Table 1 using official exchange rate

3 Bounding selection bias

The principal objection to the use of R_c to estimate the wage of equivalent labor in two different labor markets is that migrants are self-selected. For U.S. migrants negatively selected on unobserved determinants of earnings, such as Mexicans (e.g. [Fernández-Huertas 2011](#)), the estimates R_c form a lower bound on the wage ratio for fully equivalent workers R . Under positive selection, R_c can overstate R .

The rich microdata we use allow informative bounds on the bias from such self-selection, in three ways. The first uses coefficient stability tests to bound the bias, comparing the results to existing empirical estimates of selection on unobservables. The second is to derive tests for selection bias under [Roy \(1951\)](#) selection. The third tests predictions about positive selection arising from capital constraints.

3.1 Lower bounds from coefficient stability

The first approach is to estimate the degree of bias that would arise from different degrees of selection on unobservables, and compares this to selection estimates from the literature. [Altonji et al. \(2005\)](#) propose a method for bounding treatment effects under unobserved self-selection into treatment. They suggest that in many empirical settings the degree of selection on unobservables can be bounded from above by the degree of selection on observables. In rough terms, this is because if the included (observed) covariates were chosen at random from the set of possible (observed or

dollars. By that measure, R_c takes the value 18.9 in the average country of birth, and 13.9 in the median country of birth.

unobserved) covariates, then the degree of selection on observables would equal the degree of selection on unobservables. Researchers typically do not choose included covariates at random but specifically to reduce bias guided by theory, thus degree of selection explained by deliberately-chosen covariates must exceed the degree explained by omitted covariates. This suggests an avenue for bounding the degree of migrant selection on unobserved determinants of earnings, given that variables like education, age, and gender are chosen not at random but specifically to reduce selection bias: all are known to be first-order determinants of both earnings and migration.

Recently, [Oster \(2018\)](#) observes that this method may not be sufficiently conservative, and extends it. She shows that plausible bounds on selection must take account of the fraction of covariance in outcomes and treatment that is explained by observables. In other words, researchers must not only assert that they chose observables to reduce selection bias, but show that those observables do have the explanatory power to reduce selection bias. [Oster](#) derives a simple approximation of the consistent estimator for a treatment effect β ,

$$\hat{\hat{\beta}} = \hat{\beta} - \delta \left(\hat{\beta} - \hat{\beta} \right) \frac{\bar{R} - \hat{R}}{\hat{R} - \bar{R}}, \quad (7)$$

where $\hat{\beta}$ and \hat{R} are the estimated treatment effect and the coefficient of determination (R^2) from the regression including observed controls; $\hat{\beta}$ and \bar{R} are the estimate and the coefficient of determination without any controls; δ is the ratio of the degree of selection on unobservables to the degree of selection on observables; and $\bar{R} \equiv \Pi \hat{R}$ is the coefficient of determination from a hypothetical regression that includes all important observed and unobserved controls ($\Pi > 1$). With conservative choices

for δ and Π , (7) can bound the true treatment effect.⁷ Alternatively, setting $\hat{\beta} = 0$ in (7) and solving for δ allows estimation of how large selection on unobservables must be, relative to selection on observables, for the true treatment effect to be zero. Oster proposes a stringent standard for reporting results of $\delta = 1$ and $\Pi = 1.3$, the level of stability typically demonstrated by studies in the literature where treatment is randomized.

We can apply these standards to compute a lower bound on R for each country. Table 2 carries out this bounding exercise for the wage ratios at purchasing power parity, for a 35–39 year-old male with 9–12 years of education. The first column reproduces \hat{R}_c from Table 1. The second column estimates lower bounds on R using (7), under the robustness standard for quasi-random treatment assignment: $\Pi = 1.3$ and $\delta = 1$. All of these bounds remain above a treatment effect of zero ($R = 1$) and most remain very large. The lower bound on R exceeds 5.0 in 17 countries and exceeds 3.0 in 29 out of 42 countries. The third column adopts the even more conservative standard of $\Pi = 2$. The lower bound on R is still above 1.0 for 40 out of 42 countries, and above 3.0 for 22 countries.⁸

In column 4 we report the relative degree of selection on unobservables to observables

⁷Intuitively, if the explanatory power of the observables is much less than the amount of variance left to explain ($\hat{\mathbb{R}} - \mathring{\mathbb{R}} \ll \bar{\mathbb{R}} - \hat{\mathbb{R}}$), changes in the treatment effect estimate upon inclusion of observables ($-\delta(\mathring{\beta} - \hat{\beta})$) become uninformative about the degree of selection on unobservables.

⁸For two of the countries—Yemen and Cambodia—the procedure provides *upper* bounds on R and suggests that the original R_c is a lower bound on R .

(δ) that would be necessary in order for the estimated ratio R_c to be consistent with $R = 1$) using $\Pi = 1.3$. The selection on unobservables would typically need to be an order of magnitude larger than selection on observables (median $\delta|_{R=1} = 12.2$, 80th percentile 23.5) for R to be unity given the observed R_c . Column 5 reports the ratio R_u/R_c , showing generally positive selection on observables, with a median of 1.17. The median ratio of the estimates of the coefficient-stability lower bound on R is 1.12. The median ratio of the lower bounds on R in the third column ($\Pi = 2$) to R_c is 1.44.

Is it plausible that selection on unobservables is an order of magnitude greater than selection on observables? Several studies of migrant self-selection have recently been done, in a variety of settings, that allow calculation of the relevant parameters. [Table 3](#) presents all estimates of which we are aware. 11 of these use panel data to compare non-migrants with subsequent migrants prior to migration. These 11 results come from a variety of settings: origin areas both rich (Finland) and poor (Tonga); policy barriers both absent (Poland) and present (Mexico); distance both short (Lithuania) and long (Micronesia); time both contemporary (Israel) and historical (Norway). None of these settings records positive selection on unobservables with δ exceeding 0.89. In six cases there is positive selection on unobserved determinants of earnings, but the highest R_c/R ever recorded is 1.36. In three cases there is no appreciable selection on unobservables despite selection on observables, thus $\delta \approx 0$. In two of the cases there is negative selection on unobservables ($R_c/R < 1$), so that R_c serves as a lower bound on R . Both are studies of Mexico-US migration; in one of these $\delta < 1$ and in the other, δ reaches +2.25. One study has used retrospectively-reported pre-migration wages for recent U.S. immigrants to estimate $\delta < \frac{1}{3}$ for a group of home countries comprising 36 of the 42 we study, and $\delta \approx 1$

in the rest.⁹ In all of these cases of positive selection on unobservables where it is possible to estimate δ given the published results, δ is approximately equal to or much less than 1.

These studies support the interpretation of column 2 of [Table 2](#) as conservative lower bounds on R (not as unbiased or consistent estimates of R). The working-age population-weighted average of the lower bounds on R is 5.65. The lower bound for the median country (the Philippines) is 3.48, and for the 80th percentile country (India) is 5.93. The final column of [Table 2](#) shows the dollar-value difference in PPP annual wages implied by $R|_{\delta=1, \Pi=1.3}$. These are best interpreted as lower bounds on the price equivalent for observably and unobservably equivalent low skill, male, prime-age workers between the home country and the United States.

⁹[Hendricks and Schoellman \(2018\)](#) report relative self-selection on observable and overall wage determinants, using retrospectively recalled pre-migration earnings, for recently-arrived U.S. immigrants from five broad groups of countries. They report a graphical decomposition of selection that allows bounds on δ for recent U.S. immigrants. The large majority of self-selection on earnings determinants arises from selection on observables ($\delta \ll 1$). For workers from the group of countries with greater than 1/16 of U.S. PPP GDP per capita (including 36 of the 42 countries studied here), they find $\delta < \frac{1}{3}$. For the very poorest countries (the other eight of the 42 countries studied here), they find $\delta \approx 1$.

3.2 Testing predictions of Roy-model self-selection

We can gain more insight into the plausibility of large positive selection on unobservables by testing necessary conditions implied by theory. Here we follow [Hanson's \(2006\)](#) nonstochastic extension of the [Roy \(1951\)](#)-[Borjas \(1991\)](#) model of migrant self-selection, and consider selection on unobservables within observed skill groups as in [Ambrosini and Peri \(2012, p. 131\)](#). Suppose a worker with observed skill s will migrate if U.S. wages exceed the forgone foreign wage plus migration costs: $\ln w_{\text{US}} - \ln(w_0 + C) > 0$. Expressing migration cost in time-equivalent form ($\pi \equiv C/w_0$), then by (1) and (2) workers migrate if unobserved skill satisfies

$$\tilde{s} > \frac{\pi - (\mu'_{\text{US}}(s) - \mu'_0(s))}{\tilde{\gamma}_{\text{US}} - \tilde{\gamma}_0} \equiv \underline{s}(s). \quad (8)$$

This standard result implies that migrants will exhibit positive selection on unobservables if the return to unobservables at the destination exceeds the return at the origin ($\tilde{\gamma}_{\text{US}} > \tilde{\gamma}_0$). But because we have data from numerous countries, we can derive a necessary condition for bias in R_c due to Roy selection on unobservables. From (4), (5), and (8),

$$\left. \frac{\partial \ln(R_c(s)/R)}{\partial \tilde{\gamma}_{\text{US}}} \right|_{\tilde{\gamma}_0} = \tilde{\gamma}_0 \cdot \left. \frac{\partial E[\hat{s} | \hat{s} > \underline{s}(s)]}{\partial \tilde{\gamma}_{\text{US}}} \right|_{\tilde{\gamma}_0} > 0. \quad (9)$$

That is, if R_c is biased upward by positive selection on unobservables, Roy selection predicts that this bias will be greatest when the relative return to unobserved skill is higher in the destination country relative to the origin country.

We can test condition (9) by following the literature since [Juhn et al. \(1993\)](#) and

considering the dispersion of \tilde{s} for workers of a given country of birth, in each country of residence (σ_0 and σ_{US}), to proxy for the corresponding returns to unobserved skill. Let $\sigma_{\text{US}}(s)$ be the standard deviation of \ln wage conditional on observables, from regression (6), for workers born in each country and resident in the United States. Let $\sigma_0(s)$ be the same conditional standard deviation for workers resident in the country of birth. Thus $\sigma_{\text{US}}(s) - \sigma_0(s)$ proxies for $\tilde{\gamma}_{\text{US}}(s) - \tilde{\gamma}_0(s)$, the returns to unobserved skill in the United States relative to the country of birth, specific to each observed skill group.

Figure 1 tests for the relationship (9) by graphing R_c against $\sigma_{\text{US}}(s) - \sigma_0(s)$, by country, separately for each of three observed skill groups.¹⁰ For example, Figure 1a plots R_c against $\hat{\sigma}_{\text{US}}(s) - \hat{\sigma}_0(s)$ for workers with 5–8 years of education only, across all countries. There is no positive correlation between the estimates of R_c and the relative returns to unobserved skill, contrary to what theory predicts if positive selection is an important source of bias. If anything, the relationship is negative. This suggests that positive selection on unobservables predicted by the Roy model could not be a first-order determinant of the magnitude of the estimates R_c . Egypt and Yemen are slight outliers.

3.3 Testing for self-selection due to borrowing constraints

Theory predicts another reason why migrants might exhibit positive self-selection on unobserved determinants of wages. While migrant selection theory has traditionally

¹⁰The full estimates of R_c and $\sigma_{\text{US}}(s) - \sigma_0(s)$ separately by education group are in the Appendix.

focused on Roy selection, a recent literature has stressed borrowing constraints as an important determinant of selection.¹¹ Workers with low earnings for unobservable reasons may simply be unable to afford the costs of migration, broadly considered, so that migrants have levels of unobserved skill that exceed the average in the origin country.

Again extending [Hanson \(2006\)](#) to the case of selection on unobservables within observed skill groups, suppose that income y_0 of a worker in the origin country is a function of unobserved skill. For workers of observed skill s , $y_0(s) = \tilde{\xi}_0(s) + \tilde{\nu}_0(s)\hat{s}$, where $\tilde{\xi}_0, \tilde{\nu}_0 > 0$. Some workers cannot pay the migration cost $C(s)$, which is a function of observed skill, but can borrow it if they hold collateral $\psi C(s)$, $\psi > 0$. The condition for migration becomes

$$\hat{s} > \frac{\psi C(s) - \tilde{\xi}_0(s)}{\tilde{\nu}_0(s)} \equiv \underline{s}(s). \quad (10)$$

That is, positive selection on unobservables arises within observed skill groups because those with the highest unobserved determinants of earnings are the ones most likely to be able to acquire the necessary assets. This force for positive selection can act independently of Roy selection (8).

We can use condition (10), as we used (8), to make predictions about patterns the

¹¹A wave of studies have stressed the effect of poverty and credit constraints on selection in contemporary migration ([McKenzie and Rapoport 2010](#); [Hanson 2010](#); [Gould and Moav 2016](#)). This mechanism has also been important in the economic history literature on earlier migration flows ([Hatton and Williamson 2006](#); [Abramitzky et al. 2012](#)).

data should contain if selection of this kind is driving the results. Suppose that migration costs are lower for high-observed-skill workers ($\frac{\partial C}{\partial s} < 0$) and that wealth and the wealth-returns to unobserved skill are greater for workers with higher observed skill ($\frac{\partial \tilde{\xi}_0}{\partial s} > 0, \frac{\partial \tilde{\nu}_0}{\partial s} > 0$). Both of these are plausible: many countries actively encourage high (observed) skill migration while obstructing low (observed) skill migration. And workers in developing countries with higher observed skill typically have greater wealth and work in complex occupations with higher returns to unobserved skill than menial occupations. Suppose furthermore that credit constraints bind for workers without any observed skill ($\psi > \frac{\tilde{\xi}_0(s)}{C(s)}$). Together, these imply

$$\frac{\partial \ln R_c(s)/R}{\partial s} = \frac{\partial E[\hat{s} | \hat{s} > \underline{s}(s)]}{\partial s} < 0. \quad (11)$$

That is, if the estimates of R_c are systematically biased upward from R because of self-selection on unobservables arising from poverty constraints, then we should see estimates of R_c decline when higher and higher levels of observed skill are considered separately.

This test is possible with the information already discussed in the previous subsection: separate estimates of R_c for each education group: 5–8 years, 9–12 years, and 13+ years.¹² In 8 countries, R_c is *higher* for workers with 13+ years of education than for workers with 5–8 years of education, which is incompatible with (11). In the other 34 countries R_c falls somewhat at higher levels of observed skill, which is compatible with (11). The median ratio $R_c(5\text{--}8 \text{ years})/R_c(13+ \text{ years})$ is 1.38. Collectively, this evidence is compatible with modest positive selection on unobservables

¹²The full results are presented in the Appendix.

that induces upward bias in R_c as an estimate of R to a degree comparable to the independent estimates of this bias from [Table 2](#). In other words, to the extent that marginal workers who can afford university education can also afford migration, R_c for workers with 13+ years of education can serve as a lower bound on R for that category of worker.

A second test uses the fact that in the credit-constraint theory of positive selection, unlike in Roy selection, selection on observables and unobservables must go in the same direction. In this theory the poor do not migrate because they do not have the money, and from the standpoint of theory it does not matter whether the reason they do not have money is due to observable or unobservable traits. Take [Hanson's \(2006\)](#) observable counterpart to the wealth equation above and suppose that wealth is also positively correlated with *observed* skill: $y_0(s) = \xi_0 + \nu_0 s$, where $\xi_0, \nu_0 > 0$. Migrants are positively selected on observed skill analogously to [\(10\)](#), and just as above we can derive an observable counterpart to condition [\(11\)](#):

$$\frac{\partial \ln R/R_c(s)}{\partial \ln w_0} < 0. \tag{12}$$

with the innocuous assumption that income correlates positively with wealth. That is, if positive self-selection on observables arises due to poverty constraints, the degree of positive self-selection should fall as average wages rise.

[Figure 2](#) carries out this test, plotting the degree of selection on observables ($\ln R_u/R_c$) against $E[w_0]$ for all countries of birth, and each observed skill group. The pattern predicted by [\(12\)](#) is not present across all the countries at any level of observed skill. For workers of 5–8 years of education this is perhaps no surprise, since there

is less scope for positive selection on education. For higher levels of observed skill, the pattern is more informative. For workers with 9–12 years of education, the degree of positive selection on observables is roughly the same in Costa Rica and Argentina as it is in Vietnam and Sierra Leone, despite a fourfold difference in average wages. The conditional mean does fall slightly, from about 1.4 to 1.2, as the average wage ranges over an order of magnitude. This is consistent with a modest upward bias on R_u as an estimate of R_c due to selection on observables arising from credit constraints. For the most educated workers (13+ years of education), the conditional mean changes little between the average wage of PPP\$300/month and PPP\$1,200/month. It does fall by roughly 0.3 log points over the range PPP\$600–1,200/month. This too is compatible with modest upward bias arising from positive selection on observables due to credit constraints.¹³ The simple theory presented here does not suggest a reason why income that reflects observables should affect credit constraints differently from income that reflects unobservables.¹⁴

¹³At extremely low wages, PPP\$100–250/month, the conditional mean of $\ln R/R_c(s)$ rises with the wage for 9–12 years of education and 13+ years of education. This pattern could arise if, in these extremely poor countries, even the university educated face binding credit constraints. This evidence is compatible with binding credit constraints for potential migrants in Cambodia, Egypt, Haiti, Nigeria, Yemen, and perhaps Sierra Leone. In other words, we should consider with caution the estimates R_c that greatly exceed 10.

¹⁴Note that the global sample of workers here is restricted to employed wage-workers. The poorest of the poor—self-employed farmers or small-time informal retailers—are not included and these conclusions regarding credit constraints do not apply to them.

A third and separate test for bias due to positive selection of this kind takes advantage of information contained in the relative performance of migrants and natives in the U.S. labor market. Suppose that U.S. natives' wages, analogously to (1) and (2), are determined by $w^*(s) = (\mu_0^* + \gamma_0^* s) + \tilde{\gamma}_0^* \tilde{s}$ and natives' unobserved skill has mean zero. Migrants' skill is only partially transferable, as in the model advanced by [Gould and Moav \(2016\)](#). Observed skill is transferable from the migrant-origin country to the U.S. in the proportion $\gamma_{\text{US}}/\gamma_0$, and unobserved skill is transferable in the proportion $\tilde{\gamma}_{\text{US}}/\tilde{\gamma}_0$. We can express the wages of a migrant in the U.S. as

$$E_{\text{US}}[\ln w_{\text{US}}] = E[\ln w^*] - \left(1 - \frac{\gamma_{\text{US}}}{\gamma_0}\right) E[\ln w^* - \ln \underline{w}] + \tilde{\gamma}_{\text{US}} E_{\text{US}}[\hat{s}], \quad (13)$$

where \underline{w}^* is the wage of a U.S. worker with no observable skill (no education, no experience), and E_{US} denotes expectations for migrant workers in the U.S. The identity (13) states that the average wage of a migrant worker in the U.S. equals the average wage of an observably equivalent U.S. worker, minus the portion of migrant workers' observable wage determinants that do not transfer from the origin country to the U.S., plus the U.S. returns to migrants' unobservable skill. In the limiting case where none of migrants' observable skills are valued in the U.S. market ($\gamma_{\text{US}}/\gamma_0 = 0$) and migrants are neutrally selected on unobservables ($E_{\text{US}}[\hat{s}] = 0$), all migrants regardless of observed or unobserved skill have the earnings of a U.S. teenager with no schooling. From (4) and (5) we have $\ln \frac{R_c}{R} = \tilde{\gamma}_0 E_{\text{US}}[\hat{s}]$, into which we substitute (13) to get

$$E_{\text{US}}[\ln w_{\text{US}}] - E[\ln w^*] = \frac{\tilde{\gamma}_{\text{US}}}{\tilde{\gamma}_0} \left(\ln R_c - \ln R \right) - \left(1 - \frac{\gamma_{\text{US}}}{\gamma_0}\right) E[\ln w^* - \ln \underline{w}^*]. \quad (14)$$

Intuitively, migrants who are more positively selected on unobserved skill ($R_c > R$)

should earn more relative to natives of the same observable skill, to the extent that their unobserved skill is transferable ($\tilde{\gamma}_{\text{US}}/\tilde{\gamma}_0$). If there are zero returns to migration ($R = 1$), a regression of R_c on the native-immigrant wage gap within an observed skill group should have slope representing the transferability of unobserved skill. If that slope is zero, then either unobserved skill is completely untransferable—it does not represent IQ, energy, risk tolerance, or anything else that comes with migrants and has returns in the U.S.—or $R_c \approx R$.

We calculate $E_{\text{US}}[w_{\text{US}}] - E[w^*]$ for each country of birth and three observed education groups, always for 35–39 year-old males.¹⁵ This allows us to run the regression (14) nonparametrically in [Figure 3](#).

The slope is generally indistinguishable from zero across most of the support of R_c , for all three observed skill groups. Two exceptions, in workers with 5–8 years of education, are Cameroon and Morocco. This suggests that either unobserved skill exhibits near-zero transferability to the U.S. labor market, or that estimates of R_c do not greatly exceed R . Research that compares U.S. immigrants’ earnings to their pre-migration earnings estimates that the transferability of foreign unobserved skill is 0.34 shortly after arrival ([Jasso et al. 2002](#)), a lower bound on $\tilde{\gamma}_{\text{US}}/\tilde{\gamma}_0$ since the returns to migrants’ unobserved skill rise in the years following arrival ([Chiswick and Miller 2012](#)). This suggests that the gap between R and R_c is not large.

We can use this information to estimate a rough bound on the selection bias R_c/R . For the observed skill group with the most positive slope in [Figure 3](#) (5–8 years of schooling), a linear regression of $E_{\text{US}}[w_{\text{US}}] - E[w^*]$ on $\ln R_c$ gives the slope 0.144

¹⁵The full estimates are in the Appendix.

(standard error 0.061). If a lower bound on the transferability of unobserved skill for those who have chosen to migrate is 0.34, this puts an upper bound on R_c/R of $e^{(0.144/0.34)} = 1.53$ for workers with 5–8 years of education. For the group with 13+ years of schooling, the linear regression slope is 0.031 (standard error 0.043), and the corresponding upper bound on R_c/R is $e^{(0.031/0.34)} = 1.10$. These estimates independently corroborate the approximate magnitude of bias estimated above. The declining bias at higher observed skill also agrees with the prediction of (11).

These results are consistent with modest systematic bias in R_c as an estimator of R due to positive selection on unobservables arising from credit constraints. Incidentally, these results also have implications for the discussion of Roy selection in [subsection 3.2](#). The slopes in [Figure 3](#) further suggest that Roy selection is unlikely to create a large upward bias on R_c as an estimate of R . A well-known prediction of Roy selection is that positive selection on unobservables cannot occur without positive returns to unobservables in the destination country.¹⁶ The flat slopes in the figure imply either that almost none of migrants’ unobserved skill is transferable to the destination country, or that $R_c \approx R$. But if migrants’ unobserved skill is not

¹⁶Formally, if $\tilde{\gamma}_{US} = 0$ then condition (8) flips to $\tilde{s} < \frac{(\mu'_{US} - \mu'_0) - \pi}{\tilde{\gamma}_0}$, and those with below-average unobserved skill choose to migrate. In terms of the stochastic Roy model in [Borjas \(1991\)](#), when earnings at the destination are uncorrelated with earnings at the origin ($\rho = 0$), then as long as $\sigma_0 > 0$, selection must be negative. Intuitively, if all migrants within an observed skill group had exactly the same wage at the destination (zero return to unobserved skill), then those with the most to gain from migration must be those with the lowest levels of unobserved skill—provided that unobserved skill has any positive return in the origin country.

transferable, the Roy model predicts negative selection on unobservables. In that case the estimates of R_c would generally serve as a lower bound on R .

A final and intuitive robustness check, presented in the Appendix, is to simply truncate the very poorest workers from the analysis. The findings are robust to this change. After dropping workers below PPP\$4/day, the median ratio of R_c to the original result in Table 1 is 1.07.

3.4 Summary of findings

The various methods used here to place bounds on R broadly agree. Coefficient stability and diverse existing evidence about selection on unobservables imply that for the median country, $R > 3.95$ (Table 2) and for the 80th percentile country, $R > 6.14$. These correspond to lower bounds on the absolute gain per worker per year of PPP\$13,600 and PPP\$15,600, respectively. The corresponding upper bounds on the degree of selection on unobservables, R/R_c , are 1.12 at the median and 1.26 at the 80th percentile. Various robustness checks corroborate these bounds: The predictions of Roy self-selection are incompatible with R/R_c outside this range (Figure 1). The predictions of borrowing-constraint self-selection (Figure 3) are compatible with R/R_c in the middle of the range 1.1–1.5 (for the 9–12 years of schooling group). Dropping all workers in poverty leads to R/R_c of 1.1 for the median country. In 11 studies allowing point estimates of R/R_c for a real migration flow, most values are close to unity and the highest ever recorded is 1.36. In all studies of real migration flows, when there is positive selection on unobservables it is of a degree roughly equal to the degree of selection on observables in extreme cases, and much less in typical cases.

4 Discussion: Policy barriers and natural barriers

The place premium R measures an aggregate of two different kinds of costs. In a labor market at full spatial equilibrium workers move until the marginal benefit equals the marginal cost, thus $R = w_{\text{US}}/w_0 = 1 + \pi$. Part of the cost π could arise from barriers induced by policy such as visa fees, smuggler fees, or the price equivalent of visa rationing or professional licensing restrictions. Another part could arise from barriers largely independent of policy such as transportation costs or nonwage disamenities, such as a compensating differential for being far from home. Since the *Elements* of [Marshall \(1892, p. 282\)](#) it has been recognized that “the unwillingness to quit home, and to leave old associations, including perhaps some loved cottage and burial-ground, will often turn the scale against a proposal to seek better wages in a new place.”

These two types of migration barriers cannot be cleanly distinguished in the data used here. Beyond that, it is difficult to distinguish ‘natural’ and ‘policy’ barriers to migration even in theory. For example, migrant networks are known to be an important determinant of migration costs by reducing search frictions ([Munshi 2003](#)) and credit constraints ([McKenzie and Rapoport 2010](#)). Costs arising from a small network could be modeled as ‘natural’ barriers. But networks reflect prior migration flows, and those flows were themselves a consequence of policy. Generations of U.S. ‘national origin’ quotas were designed expressly to prevent migration from much of Southern Europe, Africa, the Middle East, and Asia, and did so ([Higham 2002, p. 312–324](#)). Language barriers and other disamenities of migration, too, can be modeled as ‘natural’ barriers. But such costs are shaped by policy: for example,

while in recent years the state of New Hampshire required its driving knowledge exam to be taken exclusively in English, neighboring Vermont allowed the same test to be taken in three foreign languages.

Here we discuss reasonable priors for the fraction of the place premium that arises from barriers that are unambiguously related to policy. While a quantitative decomposition is impossible, information is available to form reasonable qualitative priors about the fraction of the place premium that arises from policy barriers. To begin with, most people outside the United States are prohibited by default from entering the country and working there unless they acquire a special license from the federal government, a visa. This includes citizens of all 42 countries we study. Such policy barriers have large effects on migration flows. [Bertoli and Fernández-Huertas \(2015\)](#) find that visa requirements cut bilateral migration flows by half at equilibrium, while any new law tightening immigration policy typically reduces inflows by 6% in the same year [Ortega and Peri \(2013\)](#). Many U.S. visas are tightly rationed, with waiting periods measured in decades.¹⁷ The United States government spends more on enforcing its immigration restrictions than it spends on all other principal federal law-enforcement agencies combined—including the Federal Bureau of Investigation, the Drug Enforcement Administration, and the Bureau of Alcohol, Tobacco, and Firearms ([Meissner et al. 2013](#), p. 22). It would be strange if cross-border labor markets were unaffected by all of this, given that policies en-

¹⁷An example of a tightly binding quota is the U.S. Diversity Visa: for each person granted such a visa in 2015, there were 288 qualified applicants (14,397,781 qualified applicants for 50,000 visas). Waiting periods are over ten years for many categories of family-based visas for citizens of China, India, Mexico, and the Philippines.

forced at borders have large price effects on output prices and other factor prices (e.g. [Anderson and van Wincoop 2004](#)). These suggest a reasonable starting prior that the fraction of the wage gap R related to policy is substantial.

An ideal natural experiment to isolate policy costs would require countries that are highly similar to the 42 countries studied above, but do not face policy barriers on U.S. immigration. There are no areas so similar in all other respects as to allow precise decomposition of the ‘policy’ portion and ‘natural’ portion of the place premium. There do exist territories free of policy barriers that are nevertheless similar in some respects to foreign countries. People from Puerto Rico and Guam hold U.S. citizenship and can live and work at will to any part of the United States. The estimates of R_c for these areas without policy barriers—Puerto Rico and Guam—lie in the range 1.3–1.5, substantially above unity.¹⁸ This is compatible with sizeable natural barriers to migration even for workers who face no policy barriers. But these estimates are much smaller than those in [Table 1](#). The ratio for Haiti is several times the size of the ratio for Puerto Rico, even though both countries are close to the United States and have large migrant networks there. The ratio for the Philippines is several times the size of the ratio for Guam, even though both countries are very far from the United States and both likewise have large migrant networks. There are other ways that Puerto Rico and Guam differ from foreign countries—Puerto Rico receives sizeable inflows of Social Security payments, Guam hosts three U.S. military bases—thus these figures are only suggestive.

But Puerto Rico and Guam are not exceptional. It is difficult to find labor markets

¹⁸These regressions are presented in the Appendix.

anywhere on earth that sustain real wage differentials R_c much above 1.5 across geographic areas in the absence of policy restrictions on migration. [Kennan and Walker \(2011, p. 246\)](#) find that 34 year-old men in the United States have typically forgone a multiple of 1.21 in wage-equivalent utility gains that they could have reaped from an interstate move at age 20, reflecting the fact that moving incurs disutility from the loss of a “home premium” and climate amenities. [Burda \(1995, p. 3\)](#) finds that R_c between West Germany and East Germany collapsed to 1.3 in the years after policy barriers to migration were eliminated and migration flows spiked. Real wage differentials between metropolitan France and French overseas departments/territories, which exhibit no policy barriers to migration, fall in the range 1.2–1.4.¹⁹ This broad pattern holds in historical episodes of international migration without policy barriers. [Abramitzky et al. \(2012\)](#) find $R_c < 1.7$ for late 19th century migration from Norway to the United States.²⁰ [Williamson \(1999, p. 124\)](#) shows that R_c collapsed from as high as 4 to around 1.5 as migration soared from the Mediterranean to the New World 1880–1914, with falling transportation costs and absent policy restrictions.

¹⁹Between metropolitan France and faraway Réunion, $R_c = 1.18$ [the euro wage gap for typical private-sector low-skill workers (*ouvriers*) is 18,820/17,970, and prices are 12.4% higher in Réunion ([INSEE 2014](#), pp. 69, 121)]. For Guadeloupe it is 1.35 [the euro wage gap for moderately low-skill males (*ouvriers qualifiés*) is 15,937/13,556 ([INSEE 2010](#), p. 105), and prices are 14.8% higher in Guadeloupe ([INSEE 2014](#), p. 121)].

²⁰[Abramitzky et al. \(2012\)](#) estimate $R = 1.7$, an upper bound on R_c due to negative selection on observables for urban workers.

These estimates suggest limited scope for explaining the very large estimates of R_c and R in the preceding sections with natural barriers like pure transportation costs, or Marshall’s fondness-for-home. Wage gaps are an order of magnitude smaller in many settings that exhibit transportation costs and fondness for home, but do not exhibit policy restrictions. A reasonable prior is that a substantial portion of the large gaps measured in this paper arise from policy barriers, though precisely what portion we cannot estimate here. That portion is likely to vary considerably across different countries.

Under different assumptions about the relative effects of policy and non-policy barriers on R , we can construct scenarios for the supply price of labor from different countries. [Figure 4](#) carries out this exercise. The thick black line shows \bar{w}_0 , an upper bound on the unobserved home-country earnings of workers fully equivalent to those observed living in the United States, for 35–39 year old males with 9–12 years of foreign education. These are calculated using the lower bounds on R from [Table 2](#), column 2 ($\delta = 1, \Pi = 1.3$). The vertical axis shows annual \$PPP wages, and the horizontal axis shows cumulative working-age population of the source countries with \bar{w}_0 at or below each value. The black line, then, can be interpreted as the upper envelope for the curve of forgone home-country wages for existing migrants. Directly above each country’s flat step in that curve is a dash indicating the wages of fully equivalent migrants born and educated in that country who work in the U.S., and a dashed line shows the simple average of that wage across all immigrants. A further dashed line at the top of the graph shows the corresponding U.S. wage for the U.S.-born.

What would the supply curve of foreign labor in the U.S. market look like with a

different mix of policy and natural barriers? We cannot estimate that curve because we cannot precisely decompose R into policy and non-policy elements. But [Figure 4](#) also shows what the upper envelope of that supply curve would look like if wage ratios in the absence of policy barriers were 1.5, as discussed above, or the more extreme case of 2.0. Even in the more extreme case, the distortion arising from policy barriers would, at the margin, exceed PPP\$10,000 per worker per year for over a billion working-age people in the countries studied here. That would place the magnitude of the implied Harberger triangle plausibly in the trillions of dollars per year. This is not an estimate of the distortion from policy barriers, but is a lower bound on the magnitude of the distortion *if* real wage ratios above 2.0 cannot be sustained without policy barriers.

In one sense the wages-forgone curve in [Figure 4](#) is conservatively high, and the implied loss conservatively low. [Subsection 3.3](#) considered borrowing constraints as a theoretical reason for positive selection on unobservables ($R > R_c$). [McKenzie and Rapoport \(2010\)](#) find that such borrowing constraints induce positive selection among Mexico-U.S. migrants, but when those borrowing constraints are alleviated by migrant networks, selection is negative. Lesser policy barriers to migration would naturally tend to increase the size of migrant networks. This would allow poorer people to migrate, raising R and reducing \bar{w}_0 . In other words, if positive selection arises from borrowing constraints then policy barriers also shape selection. The estimates \bar{w}_0 are conservatively high to account for extensive positive selection, but the borrowing-constraint theory predicts that such positive selection arises in part from policy barriers themselves.

5 Conclusion

We have estimated real wage gaps between migrants from 42 countries in the United States and observably equivalent workers in the origin country. Focusing on male workers in their late thirties with 9–12 years of education, we estimate that for workers from the median country this ratio (R_c) is 4.54, for the 80th percentile country it is 7.58, and the working-age population weighted average is 6.83. We use a variety of independent methods to bound the plausible bias in these ratios as estimates of the real wage gap for fully equivalent workers (R) that could arise from positive selection of migrants on unobservable determinants of wages.

These bounds imply that workers migrating from the median country to the United States raise their real earnings by a factor greater than 3.95 (an absolute gain exceeding PPP\$13,600/year), while workers from the 80th percentile country raise their real wages by a factor greater than 6.14 (an absolute gain exceeding \$15,600 per year). Real wage gaps in the hundreds of percent for workers of equal inherent productivity appears to be a striking feature of the current global economy. This independently corroborates macroeconomic findings of large productivity gaps between countries that arise from places rather than people ([Caselli 2005](#); [Acemoglu and Dell 2010](#); [Jones 2016](#)). It likewise suggests that each type of migration barriers, both natural and policy barriers, creates Harberger triangles in the global economy that measure in the trillions of dollars per year. Further research should more precisely estimate rather than simply bound the real wage gaps R , and a priority should be to empirically isolate the portion of this place premium that arises from migration policy.

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Table 1: Wage differences: observably equivalent workers, purchasing power parity

	No controls		Controls		Controls, foreign-trained only			
	$\ln \hat{R}_u$	s.e.	$\ln \hat{R}_c$	s.e.	$\ln \hat{R}_c$	s.e.	\hat{R}_c	s.e.
Nigeria	2.742	(0.038)	2.878	(0.117)	2.792	(0.119)	16.308	(1.209)
Yemen	2.414	(0.081)	2.783	(0.185)	2.716	(0.224)	15.114	(0.249)
Haiti	3.019	(0.107)	2.683	(0.236)	2.656	(0.236)	14.245	(1.762)
Egypt	2.772	(0.027)	2.607	(0.078)	2.605	(0.088)	13.526	(0.642)
Cambodia	2.138	(0.022)	2.295	(0.063)	2.213	(0.089)	9.139	(0.353)
Vietnam	2.300	(0.010)	2.257	(0.026)	2.128	(0.030)	8.395	(0.150)
Ghana	2.343	(0.035)	2.121	(0.070)	2.099	(0.074)	8.160	(0.304)
India	2.793	(0.009)	2.130	(0.025)	2.062	(0.027)	7.859	(0.099)
Sierra Leone	2.087	(0.058)	2.054	(0.154)	2.029	(0.161)	7.608	(0.708)
Cameroon	2.338	(0.072)	1.895	(0.196)	2.012	(0.194)	7.477	(0.381)
Pakistan	2.613	(0.021)	2.050	(0.048)	2.006	(0.053)	7.433	(0.217)
Indonesia	2.238	(0.033)	1.948	(0.095)	1.956	(0.115)	7.069	(0.098)
Nepal	2.362	(0.072)	1.989	(0.206)	1.901	(0.220)	6.692	(0.275)
Sri Lanka	2.481	(0.047)	1.939	(0.117)	1.896	(0.130)	6.657	(0.265)
Venezuela	2.191	(0.025)	2.086	(0.063)	1.877	(0.083)	6.532	(0.147)
Jordan	1.949	(0.039)	1.818	(0.092)	1.721	(0.115)	5.593	(0.292)
Bangladesh	1.829	(0.034)	1.706	(0.081)	1.702	(0.086)	5.487	(0.268)
Ecuador	1.820	(0.015)	1.787	(0.040)	1.680	(0.049)	5.368	(0.122)
Uganda	2.303	(0.071)	1.477	(0.180)	1.665	(0.195)	5.286	(0.292)
Bolivia	1.706	(0.038)	1.734	(0.095)	1.630	(0.108)	5.106	(0.225)

Ethiopia	2.492	(0.028)	1.553	(0.068)	1.523	(0.076)	4.585	(0.084)
Philippines	1.998	(0.009)	1.656	(0.021)	1.505	(0.024)	4.504	(0.078)
Peru	1.413	(0.022)	1.497	(0.044)	1.424	(0.047)	4.153	(0.113)
Guyana	1.666	(0.025)	1.451	(0.060)	1.403	(0.064)	4.067	(0.145)
Jamaica	1.238	(0.033)	1.398	(0.056)	1.332	(0.060)	3.790	(0.110)
Brazil	1.579	(0.017)	1.362	(0.037)	1.327	(0.042)	3.769	(0.059)
Nicaragua	1.372	(0.030)	1.397	(0.059)	1.293	(0.062)	3.643	(0.152)
Panama	1.429	(0.021)	1.446	(0.056)	1.291	(0.086)	3.635	(0.123)
Chile	1.221	(0.027)	1.324	(0.067)	1.276	(0.084)	3.582	(0.064)
Guatemala	1.536	(0.025)	1.213	(0.078)	1.171	(0.080)	3.226	(0.107)
Uruguay	1.297	(0.041)	1.191	(0.104)	1.157	(0.130)	3.181	(0.126)
Colombia	1.353	(0.013)	1.195	(0.030)	1.121	(0.034)	3.068	(0.056)
South Africa	1.389	(0.037)	1.193	(0.090)	1.094	(0.107)	2.985	(0.121)
Paraguay	1.168	(0.074)	1.016	(0.156)	1.067	(0.179)	2.907	(0.082)
Thailand	1.335	(0.022)	1.242	(0.062)	1.040	(0.081)	2.828	(0.129)
Turkey	1.246	(0.028)	1.122	(0.071)	1.006	(0.087)	2.735	(0.017)
Belize	1.250	(0.048)	0.945	(0.129)	0.968	(0.158)	2.633	(0.247)
Mexico	1.001	(0.014)	1.045	(0.034)	0.951	(0.035)	2.589	(0.025)
Argentina	1.057	(0.024)	1.053	(0.067)	0.911	(0.089)	2.486	(0.160)
Costa Rica	0.963	(0.028)	0.870	(0.074)	0.786	(0.087)	2.194	(0.061)
Dominican Rep	0.890	(0.016)	0.758	(0.049)	0.734	(0.051)	2.084	(0.066)
Morocco	1.402	(0.041)	0.881	(0.087)	0.706	(0.105)	2.026	(0.107)

Estimates with controls: males age 35–39, 9–12 years education. Standard errors in parentheses (robust for $\ln \hat{R}_c$, bootstrapped for \hat{R}_c).

Table 2: Lower bounds on R from coefficient stability test

	$R_c _{\delta=0}$	Bound on $R _{\delta=1}$		$\delta _{R=1}$	R_u/R_c	\$ gain
		$\Pi = 1.3$	$\Pi = 2.0$			
Nigeria	16.308	> 15.764	> 14.565	82.319	1.022	>16,611
Yemen	15.114	> 16.368	> 19.713	-34.074	0.921	>23,475
Haiti	14.245	> 4.874	> 0.861	2.477	1.153	>4,742
Egypt	13.526	> 12.116	> 9.372	23.661	1.096	>16,766
Cambodia	9.139	> 9.151	> 9.179	-1669.983	0.999	>21,352
Vietnam	8.395	> 7.554	> 5.904	20.152	1.079	>15,432
Ghana	8.160	> 6.232	> 3.323	7.789	1.165	>12,810
India	7.859	> 5.930	> 3.074	7.322	1.415	>14,317
Sierra Leone	7.608	> 6.269	> 3.991	10.484	1.098	>12,789
Cameroon	7.477	> 6.287	> 4.196	11.608	1.240	>14,860
Pakistan	7.433	> 5.847	> 3.615	8.358	1.361	>13,845
Indonesia	7.069	> 6.191	> 4.545	14.759	1.222	>14,903
Nepal	6.692	> 5.286	> 3.048	8.058	1.314	>9,244
Sri Lanka	6.657	> 5.328	> 3.169	8.514	1.343	>12,218
Venezuela	6.532	> 5.778	> 4.339	15.287	1.169	>14,995
Jordan	5.593	> 5.012	> 3.882	15.715	1.150	>14,406
Bangladesh	5.487	> 5.077	> 4.236	21.919	1.134	>14,170
Ecuador	5.368	> 5.092	> 4.504	31.920	1.067	>13,537
Uganda	5.286	> 4.242	> 2.540	7.572	1.413	>12,140
Bolivia	5.106	> 4.890	> 4.421	37.767	1.073	>14,697
Ethiopia	4.585	> 3.240	> 2.091	4.388	1.685	>9,247

Philippines	4.504	> 3.475	> 1.897	5.802	1.404	>9,980
Peru	4.153	> 4.106	> 3.996	122.911	1.024	>15,375
Guyana	4.067	> 1.902	> 0.495	1.846	1.249	>5,042
Jamaica	3.790	> 3.788	> 3.784	2681.692	1.001	>15,605
Brazil	3.769	> 3.400	> 2.674	12.887	1.255	>15,019
Nicaragua	3.643	> 3.430	> 2.980	21.439	1.095	>12,488
Panama	3.635	> 3.451	> 3.058	24.861	1.101	>13,668
Chile	3.582	> 3.564	> 3.523	258.013	1.012	>15,971
Guatemala	3.226	> 2.617	> 1.607	5.603	1.336	>9,347
Uruguay	3.181	> 3.023	> 2.685	22.757	1.134	>20,241
Colombia	3.068	> 2.835	> 2.356	14.151	1.207	>11,282
South Africa	2.985	> 2.523	> 1.703	6.495	1.504	>16,207
Paraguay	2.907	> 2.752	> 2.421	19.464	1.167	>16,561
Thailand	2.828	> 2.396	> 1.628	6.275	1.521	>8,920
Turkey	2.735	> 1.949	> 1.043	2.972	1.344	>7,128
Belize	2.633	> 2.248	> 1.554	6.120	1.337	>12,006
Mexico	2.589	> 2.557	> 2.484	76.853	1.034	>10,523
Argentina	2.486	> 2.364	> 2.101	18.042	1.177	>12,135
Costa Rica	2.194	> 2.096	> 1.885	17.234	1.178	>9,563
Dominican Rep.	2.084	> 1.899	> 1.530	7.916	1.258	>7,728
Morocco	2.026	> 1.665	> 1.054	3.600	1.894	>5,876

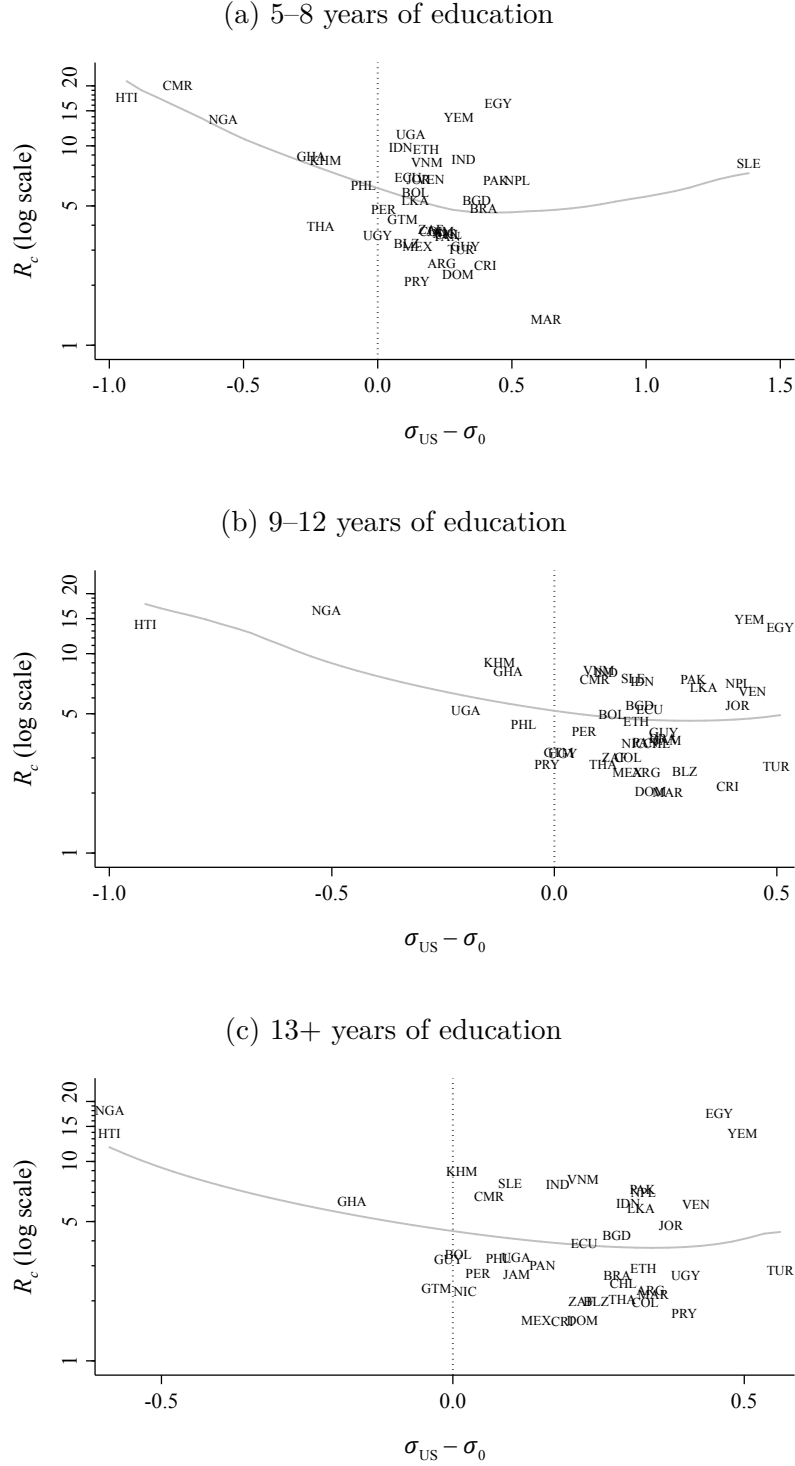
Lower bounds on dollar gain (col. 6) are PPP\$/year using $R|_{\delta=1, \Pi=1.3}$ from col. 2.

Table 3: Selection in the literature

Migrant origin	R_u/R_c	R_c/R	δ	Source
Micronesia \rightarrow US	0.71	1.36	-0.90	Akee et al. (2010)
Tonga \rightarrow NZ	1.38	1.33	+0.89	McKenzie et al. (2010)
Poland \rightarrow UK	—	1.14	—	Budnik (2009)
US Blacks 1920s \rightarrow North	1.11	1.05	0.48	Collins and Wanamaker (2014)
Finland \rightarrow Sweden	0.86	1.04	-0.24	Rooth and Saarela (2007)
Norway 1900 \rightarrow US	—	1.04		Abramitzky et al. (2012)
Lithuania \rightarrow UK/Ireland	< 1	~ 1	~ 0	Elsner (2013, p. 545)
Poland \rightarrow UK	—	~ 1	~ 0	Dustmann et al. (2015, p. 535)
Israel \rightarrow US	> 1	~ 1	~ 0	Gould and Moav (2016)
Mexico \rightarrow US	0.85	0.90	+0.65	Fernández-Huertas (2011)
Mexico \rightarrow US	0.89	0.73	+2.25	Ambrosini and Peri (2012)
Romania \rightarrow US	1.20	—	—	Ambrosini et al. (2015)
Nicaragua \rightarrow US	0.89	—	—	Barham and Boucher (1998)
Puerto Rico \rightarrow US	0.87	—	—	Ramos (1992)
Romania \rightarrow Spain	0.87	—	—	Ambrosini et al. (2015)
Poor countries \rightarrow US	> 1	>1	< 0.33	Hendricks and Schoellman (2018)
Poorest countries \rightarrow US	> 1	>1	≈ 1	Hendricks and Schoellman (2018)

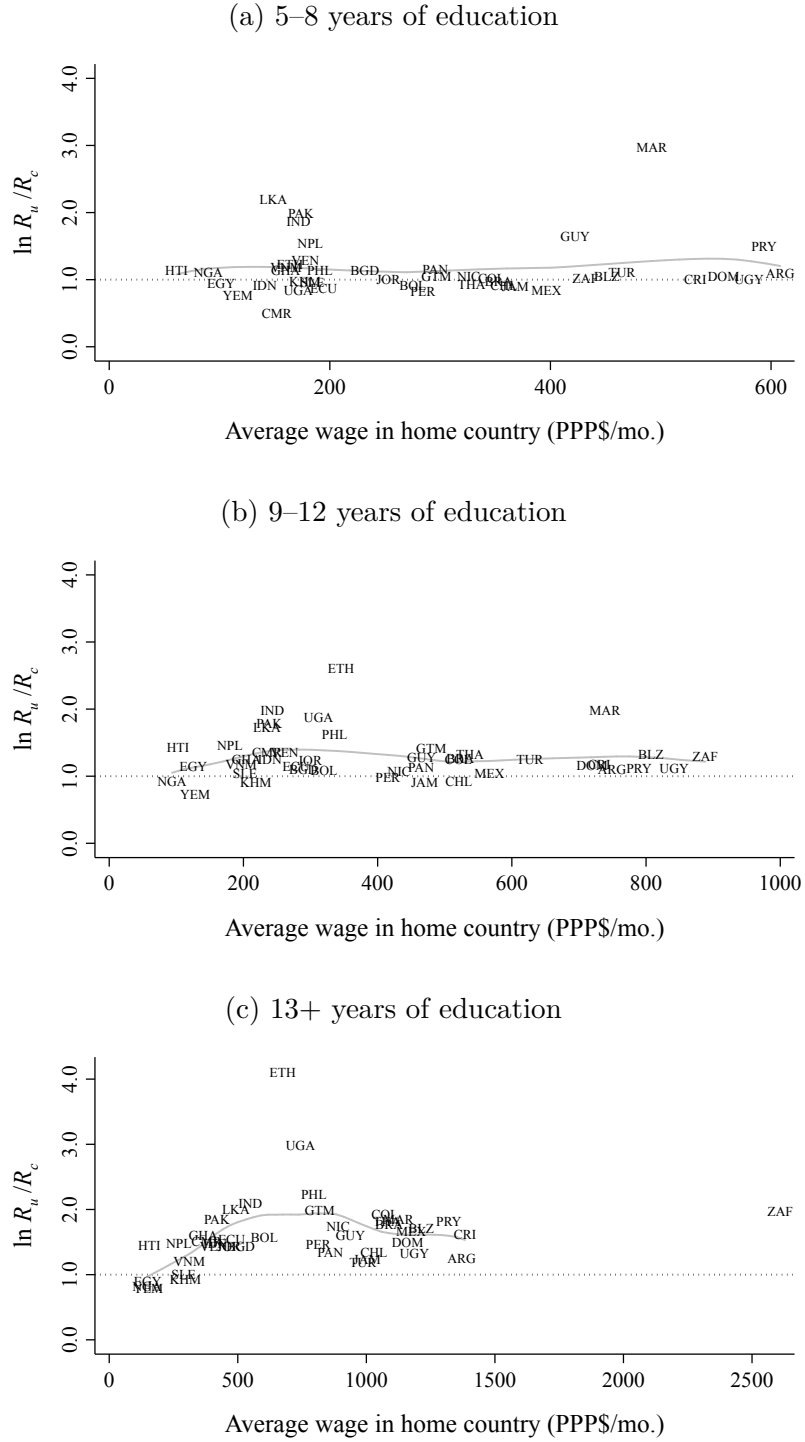
The calculations based on each source are explained in the Online Appendix. ‘Poor countries’ in the source have 1/2 to 1/16 of U.S. GDP per capita, including 36 of the 42 countries studied in this paper. ‘Poorest countries’ have < 1/16 of U.S. GDP per capita, including the other eight countries studied here.

Figure 1: Relative returns to unobserved skill, U.S. versus foreign, against R_c



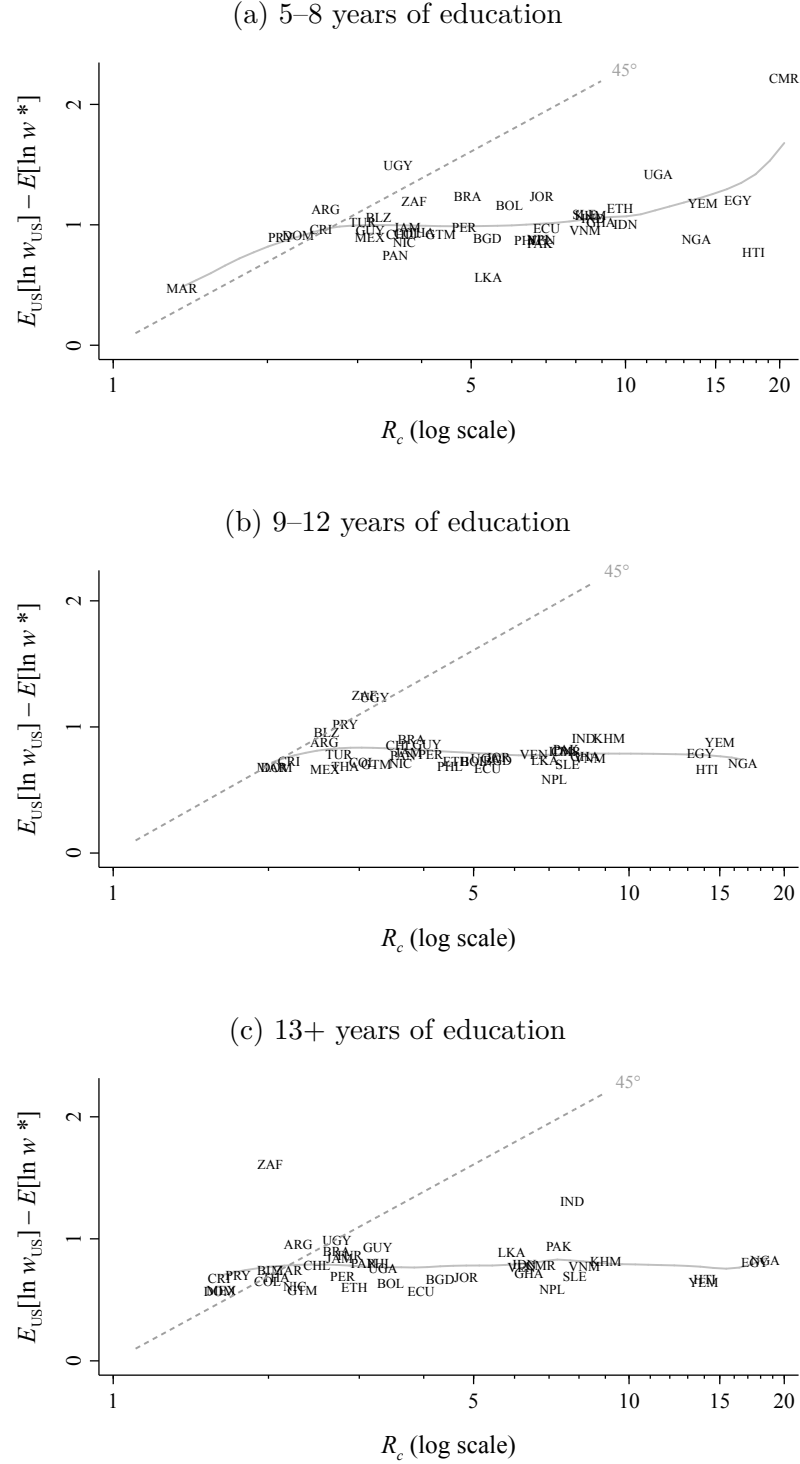
Gray line shows Fan local linear regression, Epanechnikov kernel, bandwidth 0.5.

Figure 2: The degree of selection on observables against average wage at the origin, by observed skill



Gray line shows Fan local linear regression, Epanechnikov kernel, bandwidth 100 (panels a,b) or 175 (c).

Figure 3: Relative wage of observably equivalent U.S. & immigrant labor, vs. R_c



Gray line shows Fan local linear regression, Epanechnikov kernel, bandwidth 0.3 log points.

Figure 4: Upper envelope of wages-forgone curve (\bar{w}_0) by working-age population



Note: For 35–39 year-old male workers with 9–12 years of schooling acquired in the home country. Upper envelope of wages forgone (\bar{w}_0) estimated using lower bounds on R from Table 2 col. 2: $\bar{w}_0 = w_{US}/R|_{\delta=1, \Pi=1.3}$. Single dash is wage if immigrant in U.S., born and educated in each country specified directly below that dash. “Immigrant avg.” is unweighted mean across country-of-birth for immigrants in U.S. “U.S. workers” is mean for U.S. born.