The Place Premium:
Wage Differences for Identical Workers across the US Border

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Abstract: We compare the wages of workers inside the United States to the wages of observably identical workers outside the United States—controlling for country of birth, country of education, years of education, work experience, sex, and rural-urban residence. This is made possible by new and uniquely rich microdata on the wages of over two million individual formal-sector wage-earners in 43 countries. We then use five independent methods to correct these estimates for unobserved differences between the productivity of migrants and non-migrants, as well as for the wage effects of natural barriers to international movement in the absence of policy barriers. We also introduce a selection model to estimate how migrants' wage gains depend on their position in the distribution of unobserved wage determinants both at the origin and at the destination, as well as the relationship between these positions. For example, in the median wage gap country, a typical Bolivian-born, Bolivian-educated, prime-age urban male formal-sector wage worker with moderate schooling makes 4 times as much in the US as in Bolivia. Following all adjustments for selectivity and compensating differentials we estimate that the wages of a Bolivian worker of equal intrinsic productivity, willing to move, would be higher by a factor of 2.7 solely by working in the United States. While this is the median, this ratio is as high as 8.4 (for Nigeria). We document that (1) for many countries, the wage gaps caused by barriers to movement across international borders are among the largest known forms of wage discrimination; (2) these gaps represent one of the largest remaining price distortions in any global market; and (3) these gaps imply that simply allowing labor mobility can reduce a given household's poverty to a much greater degree than most known *in situ* antipoverty interventions.

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1 Introduction

Three questions have each launched a thousand papers. First, how large are the gaps in compensation caused by various types of labor market discrimination? Second, how large are the relative price differentials in global markets caused by international borders? Third, how can public policies raise the incomes of poor households?

We bring these literatures together with an estimation of the differences between the wages of workers in 42 low- and middle-income countries and the wages those same people would earn in the United States. This calculation at once documents an enormous form of wage discrimination, measures a massive cross-border price wedge, and suggests a policy that could dramatically raise the earnings of many low-income families.

The first section of the paper creates baseline estimates of wage gaps controlling for individual observable traits. It does so with a unique harmonized database on the purchasing power price-adjusted wages and other traits of over two million workers in 43 countries, including the United States. This allows us to predict wages of observably identical workers on either side of the US border for each of these countries. Crucially, the US data identify the individuals' country of birth and, for the foreign-born, year of arrival in the US. This allows our definition of "observably identical" to go beyond standard covariates such as years of schooling, age, sex, and rural/urban residence. We can also compare workers of the same country of birth—implicitly controlling for culture, language, and social networks—and same country of schooling—which adjusts for the quality and relevance of schooling.

The wage gaps that emerge from these initial estimates are large. For instance, in our preferred econometric specification,[1] a Bolivian-born, Bolivian-educated, 35 year-old urban male formal sector wage worker with 9-12 years of schooling earns an average of US$1,831 per month working in the United States but US$460 (at purchasing power parity) working in Bolivia. Hence the earnings ratio between these observably identical people is 3.98. We produce estimates of the wage ratios of observably identical workers for each of 42 countries—we call these ratios R_o, where the subscript signifies "observably identical".[2] Bolivia's ratio R_o of 3.98 is near the median, while the lowest such ratios we observe are from the Dominican Republic, at 1.37–1.43 (depending on the regression functional form) and the highest are for Nigeria, at 11.3–13.6.

But wage gaps for observably equivalent workers do not necessarily reflect wage discrimination. The second part of the paper grapples with the fact that, no matter how many individual traits are controlled for, wage differentials for observably equivalent workers do not necessarily constitute evidence of wage differentials across workers of

[1] We test the sensitivity of the results to econometric specifications, in particular relaxing key assumptions imposed in the standard Mincer functional form of wage regression specifications that have recently received important criticism (e.g. Heckman, Lochner, and Todd (2006) in the US context and Rosenzweig (2007) for international comparisons).
[2] Rosenzweig (2007) estimates country specific skill price differentials comparing the same individuals on both sides of the United States border using the observations of wage changes of individuals from the US New Immigrant Survey. Those calculations heavily influence ours, but the results reported here are the first we know of that pool individual-level survey data across countries to estimate the impact of borders.

equal intrinsic productivity, as foreign-born workers in the US can obviously differ in unobservable ways from their observably identical counterparts back home. This issue is common to all attempts to measure discrimination. In other words, wage ratios for observably-equivalent workers—R_o—are not the same as wage ratios for workers of equal intrinsic productivity who would be willing to move from one country to another; we call this latter ratio R_e.

One factor that leads to wage gaps between foreign-born workers in the US and observably identical workers abroad is selection on unobservable determinants of productivity—selection both by the migrants themselves and by migration policy. The effect of selection on the wage gaps we measure is complex and we explore it below with a new theoretical model. The true wage gain to a typical migrant depends on two separate but related aspects of selection: where migrants come from within the source-country distribution of unobservable productivity determinants (selection), and where they end up within the destination-country distribution of unobservable productivity determinants (sorting). The higher is migrants' typical position in the origin-country distribution of unobserved productivity determinants—all else equal—the lower is the wage gain. But the higher is migrants' typical position in the destination-country distribution of unobserved productivity determinants—all else equal—the higher is the gain.

The model shows that positive selection on unobservable traits from the origin country is neither necessary nor sufficient for overestimation of the wage gain to migration. If migrants come from the upper part of the distribution of unobserved productivity determinants in the origin and were randomly sorted into the wage distribution in the destination country, then comparing "average" workers would indeed overstate the wage gain. But if selected migrants sort into the upper end of the distribution in the destination country, the comparison of wages for average workers with given observed traits can accurately reflect or even *understate* the gain. That is, if the people who are uncommonly intelligent, energetic or ambitious in the source country selectively migrate and are people who are uncommonly intelligent, energetic and ambitious in the destination country, their wage gain could be the same as—or even larger than—the wage gain to less intelligent and energetic people. Furthermore, among those positively selected on unobservables from the origin, those bound for upper end of the distribution in the destination are more likely to be seen in the data than those bound for the middle or the bottom—the former have more to gain from migration than the latter and are thus more likely to move. We match a theory of selection (from the source country) and sorting (in the destination country) with data to estimate the bias attributable to selection on unobservables.

Another, separate factor that can cause the wages of observably identical workers to differ across countries in the absence of discrimination is what we call "natural" barriers. Workers might require a compensating differential to bear the costs—broadly considered—of moving to a new land. These include the difficulty of learning a new language, being away from one's family, and entering new social networks, as well as the direct cost of travel. Workers might also be credit-constrained and have difficulty financing the move.

Only a completely exogenous movement of workers across borders would allow estimation of wage gains without selection and without "natural" forces determining who is willing and able to move. We do not present (or desire!) such an experiment. Instead, we triangulate using five distinct methods to place estimated bounds on selection and natural barriers—drawing on theory and various empirical literatures. These independent calculations yield the remarkably consistent result that selection of migrants on unobservable wage determinants results in an observed US-to-foreign wage ratio for observably equivalent workers (R_o) of around 1.25 times the true ratio for equal-productivity workers on average across countries, and that the combined effect of selection and natural barriers produces observed ratios about 1.5 times the true ratio for equal-productivity workers willing and able to move (R_e).

Even after this correction, wage gaps across borders remain extremely large. Given our median observed ratio R_o of about 3.9, the median ratio purged of selection on unobservable wage determinants and the effects of "natural" barriers—wage ratios of equally productive workers willing to move (R_e)—is roughly 2.6 (=3.9/1.5). Even this conservative estimate of R_e is above 3 for many countries—including India, Vietnam, Indonesia, Ghana, Yemen, Egypt, Haiti, and Nigeria. In other words, a worker from one of these countries can expect at the margin his or her wages to triple or more, solely due to stepping across the US border.

This wage gap is a "marginal" effect in two distinct senses: It is the effect on the wage of the next person who would arrive after a small relaxation of the migration barrier—*not* the effect on the typical person in the sending country—and it is the marginal effect given a small change in current levels of migration—*not* the general equilibrium wage under fully open borders.

The final section relates our results to the three separate literatures on wage discrimination, border-induced price wedges, and the marginal impacts of antipoverty policies. Researchers measuring each of these would do well to pay much more attention to restrictions on migration; the wage gaps we measure constitute one of the largest known forms of all three. Empirical estimates in these other literatures are comparable to ours because they, too, are measured at the margin.

2 Baseline estimates

It is obvious that there are large wage differences across countries; the question is the source of those wage differences. Some of the difference might be due to sector of employment or occupational composition across countries (e.g. more lawyers in the US and more small farmers in India). But previous researchers have documented that wage gaps across countries are enormous even for workers in the same sector, such as manufacturing, or in the same narrowly defined low-skill occupations, such as carpenters, laborers, or bus drivers (World Bank 1995). Table 1 gives a sampling of these estimates from other sources for the countries also in our sample. The ratio of real wages in the US to those in India for the same low-skill occupation is somewhere between 5 and 14.

Table 1: Previous estimates of the ratio of wages in the US to those in other countries (PPP adjusted), without controlling for individual traits

	Freeman & Oostendorp[3]		Rama & Artecona[4]	UBS[5]
Occupation	Carpenter	Laborer	Industry	Laborer
Year	1995	1995	1990-94	2006
Median	6.36	7.67	4.26	4.65
N	12	11	28	13
Selected countries				
Bolivia	6.15	6.37	5.32	
India	9.15	7.67	5.32	14.16
Mexico	6.57		2.78	7.49
Nigeria		10.60		
Turkey			1.99	2.97

N gives the number of countries in the source that 1) have data for both the country in question and the US, and 2) are one of the 42 countries studied in this paper. Blank cells indicate no data for that country.

But even in the same sector or occupation, workers in the US can differ from those in India with characteristics (e.g. education) that affect their intrinsic productivity. The unanswered question is how much of these observed gaps reflect differences in compensation between otherwise equally productive workers who would wish to migrate. Such gaps are (a) the result of wage discrimination (b) induced by border restrictions (c) whose relaxation would create large income gains for very poor people.

Wage ratios adjusted for worker productivity (and compensating differentials) are our ultimate goal. We begin by laying out the assumptions that underlie the baseline

[3] Freeman and Oostendorp (2005) calculate average monthly wage rates for male workers, in US dollars at Purchasing Power Parity, in 1995. "Carpenter" refers to ILO occupation code 88 ("construction carpenter"), and "laborer" refers to ILO code 90.
[4] Rama and Artecona (2002) calculate "industry" wages as: "Labor cost per worker in manufacturing in current US dollars per year. Includes male and female workers. Calculated as the ratio between total compensation and the number of workers in the manufacturing sector as a whole. Compensation includes direct wages, salaries and other remuneration paid directly by the employer; plus all employers' contributions to social security programs on behalf of their employees. Data on labor costs per worker are from plant-level surveys covering relatively large firms, mostly in the formal sector of the economy. Figures are converted into US dollars using the average exchange rate for each year. In countries of the former Soviet Union, the exchange rate of 1989 is used for previous years." "Government" wage is "Average wage of employees in the central or general government, in current US dollars per year. Includes male and female employees. Calculated dividing the government payroll by the total number of employees. Data are from government records. Figures are converted into US dollars using the average exchange rate for each year." Both of these are converted to PPP dollars using the PPP-to-official-exchange-rate ratio from World Bank (2007).
[5] The UBS estimates (Hoefort and Hofer (2007)) are for urban areas (respectively: Buenos Aires, New Delhi, Seoul, Mexico City, Manila, Bangkok, and Istanbul, with the US represented by New York City), and show the hourly wage (assuming 50 working weeks per year) of a "building laborer", 25 years old, single, unskilled or semi-skilled (p. 41) adjusted for cost of living in each city by the prices of 95 goods and 27 services (p. 8).

estimates of wage ratios for *observably equivalent* workers R_o, and calculating these ratios for 42 countries.

2.1 *The estimation problem*

Our analysis takes the vantage point of the migrant destination country, thus *h* ("home") is the country of destination (US) and *f* ("foreign") is the country of migrant origin. People move until wages in foreign equal wages in the US but for a factor $\delta \geq 0$, so that $(1+\delta)w_f = w_h$.

The wedge $(1+\delta) \equiv (1+\delta_n)(1+\delta_p)$ is the result of two forces. The wedge δ_n represents the effect of "natural" barriers such as credit constraints, transportation costs, language differences, psychic costs of leaving home, and job-search in an unfamiliar setting, which requires a compensating differential to make a mover indifferent between moving and staying in the absence of any policy-based impediments. The other element, δ_p, represents Becker's (1971) "discrimination coefficient"—the cumulative result of all policy barriers such as price and quantity restrictions on international movement enacted by governments.[6]

The wage of an individual *i* born in home and residing in home is denoted w^i_{hh}, where the first subscript denotes country of birth and the second subscript denotes country of residence. That wage is the product of a function θ_{hh} of a vector of individually specific observable traits x^i (schooling, age, sex, and rural/urban residence) and a function ϕ_{hh} of a vector of unobservable wage determinants x'^i. Thus $w^i_{hh} \equiv \theta_{hh}(x^i)\phi_{hh}(x'^i)$. Similarly, the wage of a person born in foreign and residing in foreign is $w_{ff} \equiv \theta_{ff}(x^i)\phi_{ff}(x'^i)$. We wish to compare these wages to $w^i_{fh} \equiv \theta_{fh}(x^i)\phi_{fh}(x'^i)$, the wage of a person with traits x and x' born in foreign who has migrated to home.

This is captured simply and intuitively by the ratio of home wages to foreign wages for a person born in foreign, $R^i \equiv w^i_{fh} / w^i_{ff}$ so that:

$$E[R^i] = E\left[\frac{w^i_{fh}}{w^i_{ff}}\right] = (1+\delta_n)(1+\delta_p). \tag{1}$$

The estimation problem of (1) is that, at any given moment, the counterfactual w^i_{ff} is unobservable for those who have migrated from foreign (e.g. Bolivia) to home (the US) as they are working in the US.

[6] It is certainly possible for governments to affect the psychic costs of leaving home, language usage, and so forth. We abstract away from minor effects of this kind and consider the main effect of government action on wage gaps to arise through restrictions on entry and domestic job-search placed upon people who reside in other countries.

2.2 *The selected migrant*

Our starting point to estimate quantity (1) is a set of identifying assumptions that we will later relax. This set of assumptions A_1, which permit unbiased measurement of R_o for the marginal selected migrant, are:

- that the wage returns to migrants' attributes, $\theta_{fh}(\cdot)$ and $\phi_{fh}(\cdot)$, can be approximated by the observed wage returns to the observable and unobservable traits of those who have already migrated from foreign to the US, denoted by $\hat{\theta}_{fh}(\cdot)$ and $\hat{\phi}_{fh}(\cdot)$;
- that the unobserved traits x'^i of the typical migrant do not differ from the unobserved traits of the non-migrant;
- that the partial association of wages and unobservable traits in foreign is independent of the same association in home $\left(E\left[\phi_{fh}\left(x'^i\right)/\phi_{hh}\left(x'^i\right)\right] = E\left[\phi_{fh}\left(x'^i\right)\right]/E\left[\phi_{hh}\left(x'^i\right)\right]\right)$;
- and that migration is costless to the migrant.

In other words, if we restrict ourselves to consideration of the marginal migrant, if we assume that the marginal migrant's unobserved traits are identical to those of non-migrants, and if we assume that the translation of unobserved traits into wages happens in independent fashion on each side of the border, then we can estimate $R_e \equiv 1+\delta_p$. In this case, equation (1) reduces to:

$$R_e \mid A_1 \equiv E[R^i \mid A_1] = \frac{\hat{\theta}_{fh}(x)E\left[\hat{\phi}_{fh}\left(x'^i\right)\right]}{\theta_{ff}(x)E\left[\phi_{ff}\left(x'^i\right)\right]}. \tag{2}$$

The right-hand side of (2) is observable. The result is an estimate of the ratio of wages after and before migration from foreign to home of *the typical selected migrant* with observed traits x^i and unobserved traits x'^i. Notably, it does not assume that *observable* traits have the same return in foreign and home. We can test whether or not being born in Ghana or educated in Ghana, for example, have different wage returns in the US labor market relative to Ghana's.

On the other hand, it has important disadvantages: It makes the strong assumption that the *unobserved* traits of emigrants from foreign are the same as the *unobserved* traits of non-migrants. In particular, if there is positive selection on unobservables—if the unobserved traits of migrants contribute positively to their earnings relative to non-migrants—then the estimate (2) will be biased upwards by a factor $1+\delta_s$ ("selection"). Furthermore, nonzero costs of migration will bias estimates of (2) by the wedge $1+\delta_n$. To the extent that there is selection and that migration is costly, estimates of (2) will be wage ratios for observably identical workers (R_o) rather than for equal-productivity workers willing to move (R_e).:

$$R_o = (1+\delta_n)(1+\delta_s)R_e = (1+\delta_n)(1+\delta_s)(1+\delta_p), \qquad (3)$$

that is, a combination of natural barriers, selection, and policy-induced wage discrimination. We will return to the subject of selection in detail below. It turns out that the effect of this assumption on the estimates is closely related to the assumption of the independence of $\phi_{fh}(x'^i)$ and $\phi_{ff}(x'^i)$.

2.3 *Data*

We estimate equation (2) using unusually rich and standardized collection of individual level data sets on wage-earners compiled by the World Bank[7] plus the US Census Public Use Microdata Sample (PUMS) five percent file.

A series of steps brings us from the raw collection of data sets to the estimation sample. First, we remove all self-employed people and unpaid family workers from the data, leaving only wage-earners. This has the advantage of increasing the comparability and accuracy of the earnings measures, but has the disadvantage of eliminating a large portion (though not all) of the informal sector from the sample—especially many agriculturists in the poorest countries. Second, we remove all people aged 14 or less and all people aged 66 or greater, as well as all people reporting zero wage earnings. Third, we remove data from twelve transition countries because many of these countries were undergoing extraordinary instability of prices, wages, and currencies at the time the survey was administered. Rather than picking and choosing among them we just eliminated the transition countries as a block.[8] Fourth, we randomly delete US-born US-residents from the PUMS to reduce the size of that group from about 6.13 million to about half a million, due to binding memory constraints in the microcomputer conducting the statistical analysis (the resulting group was 8.15% of its original size, so the sampling weight of each remaining individual was multiplied by 12.257). Fifth, we drop Chad from the sample because the sample of US residents in the public-use data does not happen to contain any working-age wage-earners born in Chad. Finally, we drop Honduras from the sample for reasons described below.

The result is a data set with 2,015,411 wage-earners residing in 43 countries. 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. Each individual record contains the person's wage in 1999 US dollars at Purchasing Power Parity, country of residence, years of schooling, age, sex, an indicator of urban or rural residence, and indicator variables for the periodicity of the reported wage (weekly, monthly, etc., with monthly as the base group). For those residing in the US, there is additional information on country of birth and year of arrival for the foreign-born. A sampling weight is assigned to each

[7] The sources for all data are given in the appendix. The basic database is also described in Montenegro and Hirn (2008).
[8] The twelve we remove are: Armenia, Azerbaijan, Bulgaria, Belarus, Croatia, Hungary, Latvia, Moldova, Republic of Macedonia, Russia, Romania, and Slovakia.

observation indicating the number of individuals in the national population represented and is used in all regressions.

The US census data were collected for the year 1999 while the surveys were in the 1990s and early 2000s (only India's survey was carried out in 1999). We convert each wage estimate in current year local currency to current year US dollars at Purchasing Power Parity using factors from the World Bank (2007) and then deflate these dollar amounts to 1999 PPP US dollars using the PPP factor deflator.[9] To the extent that real wages rose (or fell) relative to the US between 1999 and the year of a country's survey, the wage ratios for those countries will be slightly under (or over) estimated. Converting to PPP also naturally introduces the possibility that errors in any given country's PPP calculation could affect the results; note, however, that each of the 42 wage ratios we calculate is independent of any data from the 41 other countries. Thus any error in any one country's PPP rate does not propagate to the other estimates.

By using PPP exchange rates we assume that all consumption of the wage gain occurs in the US, which substantially understates the gains to overall earnings for migrant families, in two ways. First, this ignores remittances. If a worker is in one country with nuclear family members in another, and if we assume a unitary household utility function, then *household* consumption should be deflated in the location where consumption occurs. This suggests at the least that all remittances should enter the analysis at sending country prices (official exchange rates), not PPP. Second, migrants, and especially temporary workers, should optimally have very high savings rates. A simple model of intertemporal consumption smoothing would suggest that if a worker had access to a much higher wage rate for an explicitly temporary period they should optimally smooth these windfall gains over his or her lifetime. Alternatively, temporary migration is often modeled as driven by "target savers" who accumulate savings for a specific purpose (e.g. a house, business, car, wedding/marriage), consumption that again would occur in their country of origin. Much, perhaps most consumption of the US earnings of temporary migrants would be in their own country, not the US.

For instance, take our median country, Bolivia. The ratio of official exchange rate (5.81 LCU/$) to PPP (2.09 LCU/PPP$) in 1999 was 2.78. Assuming all consumption occurs in the US produces a wage ratio for our base case observably identical worker of 4 at PPP. If one assumes that half of consumption is in Bolivia the ratio would be 7.5 and if only 20 percent of consumption were in the US (a combination of remittances and very high propensity to save) the ratio would be 9.7. The mean ratio of official to PPP exchange rates for countries with GDP per capita under PPP$10,000 in 1999 was 3.7 (larger than Bolivia's). At this ratio of official to PPP exchange rates even if only 20 percent of migrant worker earnings in the US were consumed (through remittances of savings) in their home country this would mean our estimates were *understated* by a factor of 1.5 and if as much as 50 percent were consumed in their home country the PPP comparisons

[9] After we carried out our analysis the World Bank announced intentions to retroactively adjust the PPP factors we use, but these were unavailable at the time of writing. We note, however, that the most important adjustments foreseen are those to India's and China's PPP factors, both of which will tend to lower the PPP dollar-value of non-migrants' earnings and therefore make the wage ratios reported here tend to underestimate the true ratios.

understate welfare gains by more than a factor of 2. In the spirit of keeping our results as conservative as possible the paper will devote a great deal of attention to adjustments, such as for migrant selectivity or compensating differentials that scale down the raw estimates of wage differentials. But we highlight the fact that our assumption that all consumption occurs in the destination country goes a long way toward ensuring that our estimates *understate* the gaps in real wages we document.

A key question is the reliability and comparability of reported earnings. Research comparing multiple sources of income data at the individual level suggest that self-reported income is an unbiased estimator of true income, both in rich countries (Bound and Krueger (1991)) and in poor countries (Akee (2007a)). There is less certainty about comparability. Wage data for the US reflect total earnings from all jobs, whereas wage data for the 42 developing countries in our sample reflect wages from the respondent's principal occupation. For the vast majority of formal-sector wage earners in the sample we nevertheless expect wage earnings from the principal occupation to closely reflect total wage earnings. Furthermore, wage data for the United States reflect gross earnings before taxes, and we expect that most people responding to a general question about their wages or earnings would have provided gross wages on most of the country surveys, but for a handful of countries it may be that the responses reflect after-tax wages.[10] If respondents provided net-of-tax instead of gross wages this would result in some upward bias to our estimated R_o. This bias will be small, however, if it is present at all. Formal-sector income taxes are on the order of 5% in most developing countries (Easterly and Rebelo (1993)). For the median ratio of 3.92, for example, a 5% underestimation of the denominator means that the corrected ratio is 3.73.

2.4 *Method*

We seek estimates of international wage ratios for equal-productivity workers (R_e); we begin by estimating wage ratios for observably-identical workers (R_o). First we compare workers residing in one pair of countries at a time—the US and another country $j \in J$—estimating a separate regression for each country j. We do this with three different regression specifications. We discuss the standard Mincer specification first since it is the easiest to describe, though we rely on it the least:

$$\ln w_{ij} = X_{ij}\zeta + \begin{pmatrix} \delta_0 + \beta_0 s_{ij} \\ \delta_j^r + \beta_j^r s_{ij} \\ \delta_j^l + \beta_j^l s_{ij} \\ \delta_j^e + \beta_j^e s_{ij} \end{pmatrix}' \begin{pmatrix} 1 \\ I_{ij}^r \\ I_{ij}^l \\ I_{ij}^e \end{pmatrix} + \varepsilon_{ij}. \tag{4}$$

[10] In a small number of the countries (such as Yemen) the survey explicitly requests after-tax earnings, and in a few of the others (such as Chile) custom may dictate that formal sector "wages" refer to after-tax earnings unless otherwise specified. The text of the wage question from each survey is in the Appendix.

Where w_{ij} is the wage of person i in country j. In the Mincer specification ζ is a vector of coefficients to be estimated and X_{ij} is age, age squared, an indicator variable for sex, and an indicator variable for residence in a rural area.

The number of years of schooling is denoted s_{ij}, and each δ and β is a coefficient to be estimated. The intercept and slope term on schooling are allowed to differ across four values of the indicator variables that combine country of birth and country of residence. I_{ij}^{r} (r for "resident" of country j) takes the value 1 if individual i resides in country j, or 0 otherwise; these are people born in foreign, residing in foreign. I_{ij}^{l} (l for a "late" arriver) is 1 if individual i was born in country j, now resides in the US, and arrived in the US at or above age 20. I_{ij}^{e} (e for early arriver) takes the value 1 if individual i was born in country j, now resides in the US, and arrived in the US below age 20. As the base group is the United States, scalar β_0 is the coefficient on years of schooling for US-born US-residents, and $\beta_0 + \beta_j^r$ is the coefficient on schooling for residents of country j.

We distinguish between early and late arrivers because we do not wish to assume that a year of schooling acquired abroad has the same value in the US labor market as a year of schooling acquired in the US. We assume that most late arrivers received most of their education in their countries of birth. Since we focus on workers with education of 12 years or less, virtually all of those with 9–12 years of education who arrived at or after age 20 were indeed educated in their country of birth.

Heckman, Lochner, and Todd (2006) question the validity of assumptions underlying the traditional Mincer functional form, so we also estimate two alternative specifications. The 'extended' Mincer specification includes quadratic and cubic terms in years of schooling, sex, and age (and their interactions).[11] We do not present the results from the 'extended' specification, as the estimates of wage ratios based on this less restrictive functional form do not differ substantially from those of the standard Mincer specification. In our second alternative, the 'category' specification—on which our analysis focuses—the columns of X_{ij} contain indicator variables for nine quinquennial age groups, an indicator variable for sex, and an indicator variable for residence in a rural area. In that specification, s_{ij} is a vector of five indicator variables for different levels of education based on years completed (1-4, 5-8, 9-12, 13-16, 17-28). Hence the β_0, β_j^r, β_j^l, and β_j^e are also 5×1 vectors of coefficients to be estimated.[12] Aside from defining age and education groups, this last makes no assumptions about the functional form of the relationship between wages and schooling or age. This is particularly important as it

[11] This addresses the ambiguity of functional form by Stone-Weierstrass approximation of the unknown function with higher-order polynomials. Letting a = age and g = sex, the "extended" Mincer specification replaces the constant, sex, age, and schooling terms above with $\sum_{\lambda=0}^{1}\sum_{\mu=0}^{3}\sum_{\nu=0}^{3}\beta_{\lambda\mu\nu}\, g_{ij}^{\lambda} a_{ij}^{\mu} s_{ij}^{\nu}$.

[12] The six schooling categories are: 1) zero (base group), 2) 1-4 years, 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 (base group), 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).

reduces the influence of returns to tertiary/higher education on the estimates of returns to earlier education.

The raw regression results from estimating equation (4) for the Mincer specification for each of the 42 countries are given in Appendix Table A1. The results from the category specification are given in Appendix Table A2.[13]

From these bilateral regression results we estimate (2), the ratio of the expected wage of an individual—with *s* years of education, born and educated in country *j* ("late" arrival) but now working in the US—to the expected wage of the observably equivalent person working in the foreign country of birth *j*:

$$\hat{R}_{o,j} \equiv e^{\left(\hat{\delta}_j^l - \hat{\delta}_j^r\right) + \left(\hat{\beta}_j^l - \hat{\beta}_j^r\right)s} \qquad (5)$$

where $\hat{\delta}_j^l, \hat{\delta}_j^r, \hat{\beta}_j^l$, and $\hat{\beta}_j^r$ are empirical estimates of the corresponding parameters. Regression (4) allows estimation of other versions of (5) as well, such as comparison of the wage of an "early" arrival (many of which received education in the US) to the wage of a foreign resident.

2.5 *Baseline estimates of wage ratios from bilateral regressions*

Table 2 presents a variety of the wage ratio estimates. Columns I-III report the results of the regressions using education and age categories, as these are our preferred estimate as they impose fewer assumptions about the structure of wage profiles than the standard Mincer specifications. They also compare individuals who are the most similar: foreign-born, foreign-educated (late-arrival) people on either side of the border, allowing education acquired abroad to have different returns than education acquired in the US. With this specification the median $\hat{R}_o$ for 9-12 years of schooling is 3.9. The highest is Nigeria at 13.6 (earning \$1,625/month in the US versus \$120/month in Nigeria) while the lowest is Dominican Republic at 1.4 (\$1,553 versus \$1,137).

The median difference in PPP-adjusted annual wage earnings by observably equivalent workers (35-39 year-old formal-sector urban male wage-earners with 9-12 years of schooling, born and educated in each country of origin) is \$15,339, from a high of \$21,000 in Ghana to a low of roughly \$5,000 in Dominican Republic.

These ratios are robust to changes in the specification of the underlying regressions. Comparing the results using a category of "9-12 years of schooling" to the ratios using the Mincer specification in column IV, computing a ratio for each of the years 9 to 12 and taking the geometric average produces results with a cross-country correlation of 0.99—but which are lower on average by about 0.3.

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[13] We also estimate versions of each specification that assume no country-specific coefficients on schooling, which is to say that they assume that all coefficients β except β_0 are equal to zero. The raw results of these, and those using the 'extended' Mincer specification, are available upon request.

The wage premia tend to be modestly lower at higher levels of education (although this is in ratios; in absolute terms the gap grows). This can be attributed mechanically to the fact that the partial association of wages in the US labor market and schooling acquired abroad (median 6.1% increase in wages per year of schooling) is typically substantially lower than the association of US wages and US schooling (median 12.3%) or the association of foreign wages and foreign schooling (median 8.2%).

Finally, column V shows estimates of wage ratios that do not control for any observable traits at all. Note that these are comparable in order of magnitude to the low-skill occupation-specific ratios from Table 1 that do not control for observable worker traits besides occupation or industry. Comparing column V with the rest of Table 2 reveals that controlling for the observable characteristics of workers accounts for, on average, about 30-40% percent[14] of the observed cross-national differences in wage ratios. It is not just cross-national differences in the quantity and quality of schooling, but also differences in average age and in rural-urban residence that account for the explained portion of the variance.

Other authors have pooled microdata sets on PPP adjusted income (or consumption expenditures) to compute measures of international personal inequality. Milanovic (2008) shows that country fixed effects explain roughly 60 percent of all income inequality across individuals in the world, but this includes inequality due to differential access to capital and different levels of human capital. In contrast, our results are specific to labor income for workers with the same characteristics.

These estimates of R_o use regressions simply as a tool for calculating conditional means for data description—we are only comparing the distributions of wages of workers in given categories (born outside the US, male, urban, aged 35-39, with 9-12 years of schooling acquired in the country of origin) on both sides of the border. On this level the ratios R_o are just factual summary statistics (ratios of conditional means) that happen to be robust to a variety of ways of making adjustments across categories implied in various functional forms. As with most empirical work in economics all the theory comes in deciding how to interpret these facts, to which we now turn.

[14] Median 1 – 3.93/6.20 = 0.366; average, 1 – 4.36/7.27 = 0.400.

Table 2: Estimates of the wage ratios of observably equivalent workers (male, urban, 35 years old) comparing late arrivers working in the US versus their country of birth at various levels of schooling

Column	I	II	III	IV	V	VI
Specification	Category	Category	Category	Mincer	Raw wage ratios, no controls	Annual dollar gain in column I
Schooling	9-12	5-8	13-16	Geom. Avg. 9-12		
Average	**4.36**	**4.86**	**4.15**	**4.06**	**7.27**	**$14,999**
Median	**3.93**	**3.87**	**3.05**	**3.44**	**6.20**	**$15,339**
Correlation with col. I	**1.00**	**0.92**	**0.92**	**0.99**	**0.82**	**—**
Nigeria	13.59	11.97	12.63	11.72	13.45	$18,068
Haiti	11.11	13.76	8.38	10.81	23.50	$18,459
Egypt	9.98	12.60	13.29	8.77	11.98	$19,788
Yemen	8.89	9.03	10.77	8.71	14.07	$18,344
Ghana	8.37	9.21	7.24	7.45	9.37	$21,053
Sierra Leone	7.61	8.80	4.98	7.10	8.35	$19,436
Indonesia	5.82	7.52	5.35	6.09	9.52	$17,971
Cameroon	6.28	6.88	5.78	5.91	10.06	$18,883
Vietnam	5.54	4.66	6.13	5.84	10.29	$16,994
Venezuela	4.86	5.51	5.07	4.91	8.89	$16,337
Pakistan	5.18	4.40	5.61	4.69	12.58	$15,381
India	5.21	5.12	5.96	4.46	10.88	$16,827
Bangladesh	4.92	4.12	3.69	4.45	6.89	$15,031
Ethiopia	4.08	8.65	2.73	4.30	13.08	$17,308
Ecuador (median)	3.87	4.63	3.56	4.15	7.29	$14,520
Jordan	4.82	5.89	4.15	4.10	6.21	$17,643
Cambodia	4.72	3.04	6.92	4.05	9.16	$18,547
Sri Lanka	4.98	3.99	4.21	4.04	9.90	$17,446
Bolivia (median)	3.98	4.52	4.07	3.97	5.78	$16,458
Uganda	4.67	8.89	3.02	3.96	7.71	$17,925
Philippines	3.99	5.74	3.09	3.80	6.18	$16,882
Nepal	4.59	4.52	8.13	3.08	13.48	$14,846
Guyana	2.94	2.58	3.19	2.92	4.81	$16,537
Brazil	2.99	3.75	2.21	2.91	5.03	$15,193
Chile	2.85	3.31	2.36	2.79	3.09	$15,297
Panama	2.94	2.71	2.87	2.77	3.87	$15,084
Jamaica	2.97	3.64	2.48	2.76	3.20	$16,881
Peru	3.11	3.44	2.38	2.73	4.43	$14,248
Thailand	2.37	3.22	1.84	2.50	4.79	$12,992
Turkey	2.16	2.60	2.17	2.46	3.15	$11,814
Uruguay	2.22	2.78	2.05	2.23	3.22	$14,307
Colombia	2.13	2.66	1.93	2.16	4.42	$10,505
Guatemala	2.09	2.47	1.87	2.14	6.25	$10,782
Nicaragua	2.42	2.25	1.88	2.13	4.96	$11,447
Morocco	2.00	1.25	2.01	2.00	3.48	$10,009
Mexico	1.99	2.45	1.60	1.94	3.82	$9,180
South Africa	2.16	2.80	1.40	1.79	2.83	$13,939
Argentina	2.13	2.38	1.85	1.74	2.40	$12,420
Belize	1.53	1.79	1.38	1.69	3.58	$8,597
Paraguay	2.00	1.52	1.31	1.64	3.20	$14,024
Costa Rica	1.56	1.74	1.55	1.61	2.85	$7,548
Dominican Rep.	1.37	1.48	1.38	1.36	3.32	$4,991

Sorted in descending order by column IV, for comparability with Table 3. “No controls” means that the regression includes only country and wage-unit dummies. “Mincer” is standard Mincer specification. “Category” uses dummies for five education levels and nine age levels. Both specifications include interaction terms allowing different returns to schooling acquired abroad. The numerator is the predicted wage for “US-born” (US-born US-residents), and the denominator is predicted wage for “Foreign-born, late arrival” (foreign-born US-residents who arrived at age 20+).

3 Bounding the biases of selection and natural barriers

We wish to know what portion of the ratios R_o is the result of policies that prevent the movement of labor. That is, we wish to know R_e, the wage ratio for equally productive workers who wish to move. R_e is the relevant measure because (a) it represents wage discrimination as workers would be willing to work at those wages but are prevented from doing so by government policy, (b) it is analogous to price gaps across identical goods and services induced by borders, and (c) it represents the potential welfare gain to the marginal mover if the policy induced costs were removed.

Going back to equation (3), we want to know what portion of R_o represents (i) unobservable differences in productivity between selected migrants and non-migrants $(1+\delta_s)$, (ii) a compensating differential for "natural" barriers such as credit constraints, search costs, distance, language, and unfamiliarity $(1+\delta_n)$ to make movers willing to move, and (iii) what portion represents the pure wedge introduced by borders $(1+\delta_p)$.

No one would claim that $\delta_p = 0$, which would imply that the entire apparatus of control of the US border—from visas at airports and border crossings to fences and agents—was ineffectual with respect to all countries and workers from all countries are just indifferent between moving and staying. This is obviously counter-factual as both high-skill and low-skill visas to work in the United States are vastly oversubscribed: the entire stock of high-skill temporary "H-1" work visas for all of fiscal year 2008 was famously exhausted in a few hours on the first day in 2007 that they became available. The same year, the US "diversity visa" lottery drew 6.4 million applications (representing 10 million individuals) for 50,000 visas—oversubscribed by a factor of 200 to 1.

Drawing upon economic theory and a range of empirical results in the literature, we discuss five separate ways to place useful bounds on the relative contributions of selection, natural barriers, and policy:

- Simulation exercises with a calibrated structural model of selection
- A version of our regressions that eliminates selection on unobservables, but must be adjusted for international differences in wage-returns to observables
- Discussion of the sole extant experimental analysis allowing precise evaluation of the wage effect of an exogenous change to δ_p in isolation
- Several migration flows in which policy barriers δ_p are roughly zero, allowing measurement of $(1+\delta_s)(1+\delta_n)$
- Comparison with predictions based macroeconomic data, where $\delta_s \approx 0$

Surprisingly, all of these methods give very similar results. They suggest that the wage ratios of observably equivalent workers overestimate wage discrimination and by at most a factor of $(1+\delta_s)(1+\delta_n) \approx 1.5$, resulting in roughly equal measure from selection δ_s and natural barriers δ_n.

3.1 *A calibrated selection model*

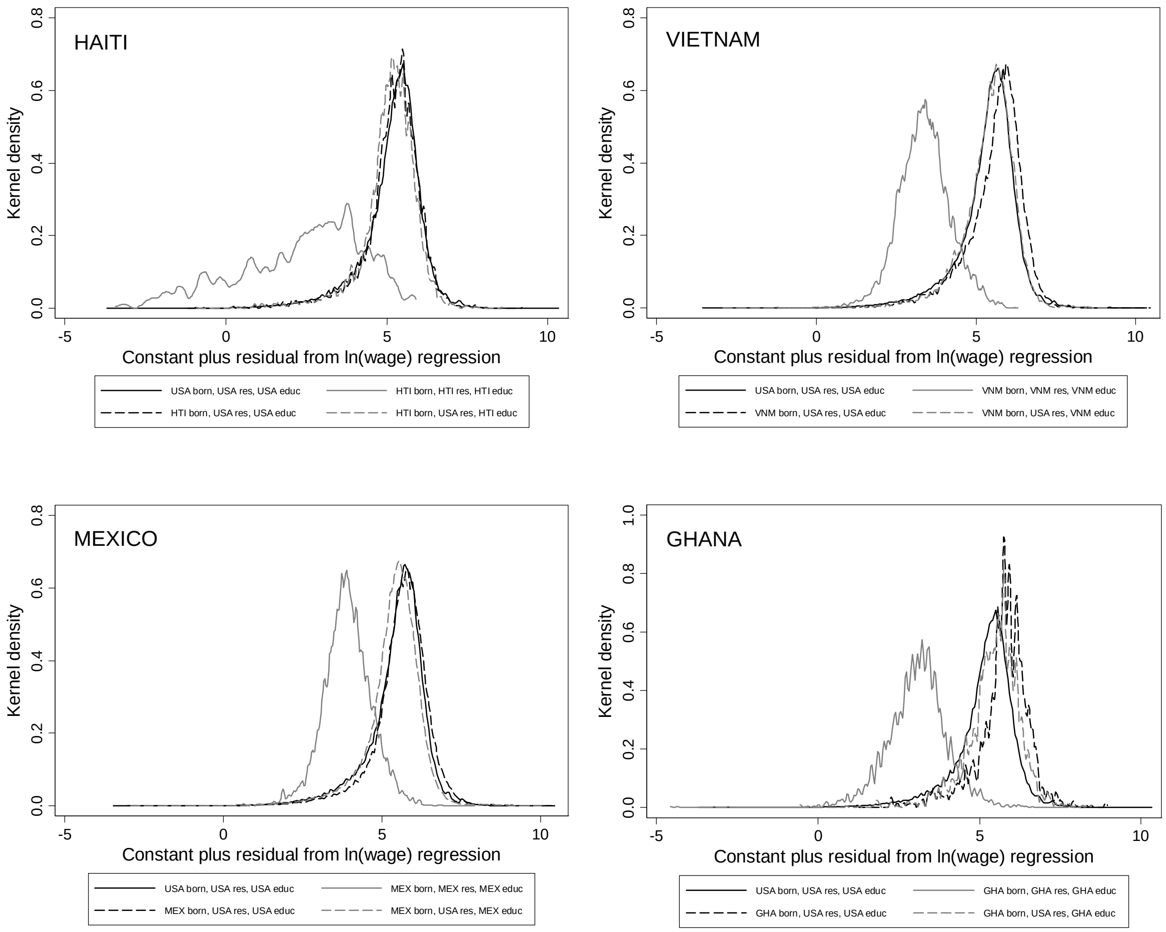
The key issue with selection is that, while we have controlled for observables, it may well be that Bolivians working in the US would have had higher wages, had they remained in Bolivia, than observably equivalent Bolivians who stayed. Thus while we observe that our base-case worker earns a monthly wage of $1,831 in the US and $460 in Bolivia, it is possible that had the typical Bolivian-born worker in the US been in Bolivia he or she would have made more than $460.

Keep in mind, however, the magnitudes at hand. Abstract away from compensating differentials for a moment and suppose that the migrant worker is highly selected on unobservable traits, and would have been 50 percent more productive than the average observably-identical non-migrant. This large difference would only reduce the true wage ratio from 3.9 to 2.6. Even if the migrant worker were *twice* as productive as an observably-identical non-migrant the true wage ratio would still be 2.0.

Figure 1 gives another way to get a feel for the limited degree to which selection can explain the observed ratios R_o. For four countries of origin—Haiti, Vietnam, Mexico, and Ghana—these kernel-density plots show the distribution of the unexplained component of wages in the bilateral regressions with the "category" specification. Four groups are shown: US-born US-residents ($\hat{\delta}_0 + \hat{\varepsilon}_{ij}$), early arrivers ($\hat{\delta}_0 + \hat{\delta}_j^e + \hat{\varepsilon}_{ij}$), late arrivers ($\hat{\delta}_0 + \hat{\delta}_j^l + \hat{\varepsilon}_{ij}$) and foreign residents ($\hat{\delta}_0 + \hat{\delta}_j^r + \hat{\varepsilon}_{ij}$).

In order for selection to fully explain the wage gap between Vietnam residents and Vietnam-born Vietnam-educated US-residents, two conditions must simultaneously be met: 1) the *typical* immigrant from Vietnam must be drawn from the top 1% of the distribution of unobserved determinants of earnings in Vietnam or higher, and 2) there must be zero or negative correlation between that person's unobserved component of wages in Vietnam and in the United States—that is, this observed characteristic must have raised their wages in Vietnam but not in the United States. For Haiti and Mexico, similar conditions would need to apply: enormously high positive selection on unobservables (top 3-4% of the foreign distribution) and zero or negative correlation of the foreign and home unobserved component of wages. These conditions are extreme.

Figure 1: Kernel densities of the unexplained component of wages



Economic theory can moreover serve to place bounds on the degree of bias introduced by selection. If people move in order to maximize wages subject to an individual-specific cost of movement, we can derive the precise degree to which any given relationship between the returns to unobservables in foreign and home creates a gap between R_o and R_e for the new mover in response to a marginal relaxation of migration barriers. We do this via a simple selection model rooted in the classic model introduced by Roy (1951).

Let $f_i \equiv \ln w^i_{ff}\big|_{x_i}$ represent the wages of a given foreign-born individual in foreign (the migrant-origin country), for a given vector of observables x, and let $h_i \equiv \ln w^i_{fh}\big|_{x_i}$ represent the wages of the same individual if he or she resides in home (the migrant-destination country). Individual i migrates from foreign to home if $h_i \geq f_i + c_i$, where $c_i \equiv \ln(1+\delta)$ where $\delta \geq 0$ is the cost of moving (broadly considered), expressed in fractions of the individual's foreign wage.

Suppose now that each person i with observable traits x_i has home and counterfactual foreign wages that are jointly normally distributed as $\pi(f_i, h_i) \sim N[(\bar{f}, \bar{h}), (\sigma_f, \sigma_h), \rho]$ where ρ is the correlation. Any level set of this distribution $(f, h)|_{\pi=\Pi}$ is an ellipse, as in Figure 2. Individuals to the upper left of the line $h = f + c$ move from foreign to home, and the rest do not. The modal new migrant due to an infinitesimal decrease in c is represented by the tangent point (f_m, h_m). Using the equations of the ellipse and movement threshold, we can calculate precisely the wage gain to this marginal migrant.

Suppose that the marginal migrant's US wage h_m is observed, but his foreign wage f_m is not observed and is instead approximated by $\bar{f}$, the average for workers in foreign with equal observable traits. In this case if the true wage ratio $R_e = e^{h_m - f_m}$ is estimated by $R_o = e^{h_m - \bar{f}}$, with a bias of $R_o / R_e = e^{f_m - \bar{f}}$. There are two cases:

No selection or negative selection: Where there is no selection of migrants or negative selection on unobserved traits in the foreign market $(f_m - \bar{f} \le 0)$, the estimated wage ratio underestimates the true ratio. The left-hand panel of Figure 2 shows one such case. Recent evidence from Fernández-Huertas Moraga (2008) indicates that Mexican migrants to the US are modestly negatively selected on unobserved traits.

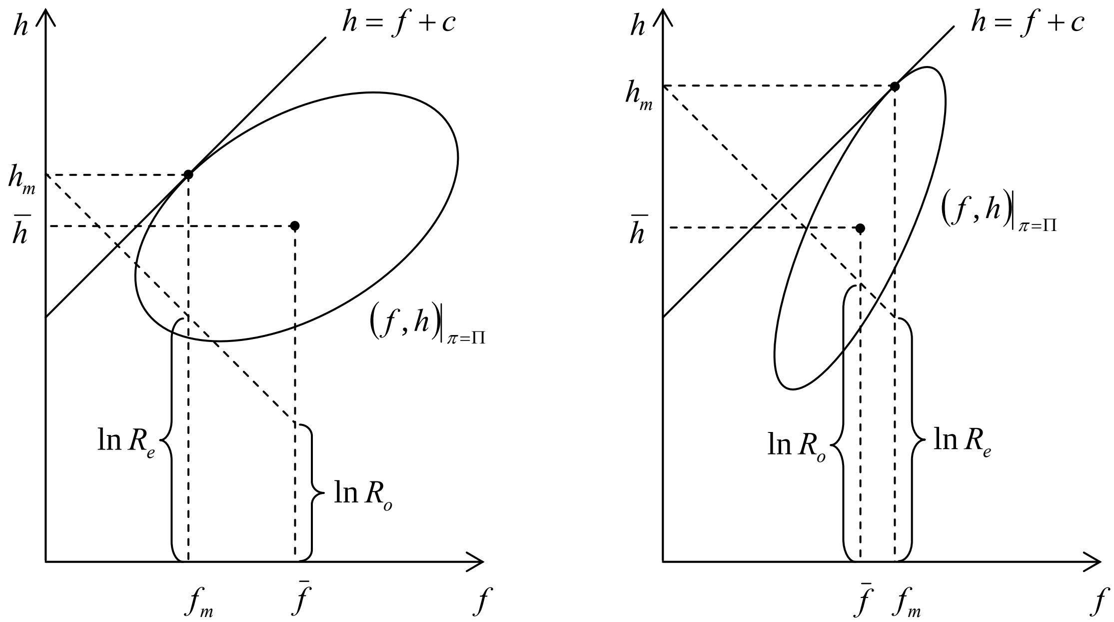
Positive selection: Where there is positive selection on unobserved traits $(f_m - \bar{f} > 0)$, as in the right-hand panel of Figure 2, the estimated wage ratio overstates the true ratio by

$$R_o / R_e = e^{(h_m - \bar{h})\Theta} \tag{5}$$

where $\Theta \equiv (\gamma(\rho - \gamma))/(1 - \rho\gamma)$ and $\gamma \equiv \sigma_f / \sigma_h$ and ρ is the correlation.[15] To begin to evaluate the magnitude of bias (5) note that if $\gamma \approx 1$ (as for three of the four countries in Figure 1) then $\Theta = -1$ (as long as $\rho < 1$) and $R_o / R_e = e^{(\bar{h} - h_m)}$. At $\gamma = 0.84$ (the average γ across the countries) and with positive selection overestimation occurs if and only if $h_m < \bar{h}$ and $\rho < 0.84$.

[15] Marks (1982) shows that the equation of this ellipse is $a\hat{f}^2 - b\hat{f}\hat{h} + a\hat{h}^2 = Z$, where $\hat{f} \equiv (f - \bar{f})/\sigma_f$, $\hat{h} \equiv (h - \bar{h})/\sigma_h$, $a \equiv 1/(1 - \rho^2)$, $b \equiv 2\rho/(1 - \rho^2)$, and Z is some constant. The implicit function theorem yields yields $d\hat{h}/d\hat{f} = (b\hat{h} - 2a\hat{f})/(2a\hat{h} - b\hat{f})$, which equals the slope of the tangent line $\gamma \equiv \sigma_f / \sigma_h$ at any point (f_m, h_m) such that that $\hat{h}_m / \hat{f}_m = (2a - b\gamma)/(b - 2a\gamma)$. This implies that for the marginal new migrant, $f_m - \bar{f} = (h_m - \bar{h})\Theta$, where $\Theta \equiv (b\gamma - 2a\gamma^2)/(2a - b\gamma) = (\gamma(\rho - \gamma))/(1 - \rho\gamma)$.

Figure 2: Graphical representation of the selection model



(a) Negative selection, underestimation of gain

(b) Positive selection, overestimation of gain

We can therefore assess the magnitude of the bias R_o/R_e if we can place bounds on $\left|h_m - \overline{h}\right|$, the gap between the destination-country (US) wage of the marginal migrant and the destination-country wage of the average observably identical person from foreign. This is easy under one assumption: that the average expected difference between the unobservable component of worker productivity is zero across countries. That is, suppose that there is no unobserved trait (such as "culture") that makes a randomly-selected worker from any one country less productive than a randomly-selected worker from any other country, when both work in the same market and both have equal training, schooling quality, experience, and so on. This allows us to approximate $\overline{h}$ —the (unobservable) wage that the average, randomly-selected foreign-born person would earn in home conditional on observed traits—as the average wage that a home-born person earns in home conditional on observed traits. If this is so, then the gap between the observed residual wage (after controlling for observables) of foreign-born in the destination country $\left(\hat{\delta}_0 + \hat{\delta}_j^l + \hat{\varepsilon}_{ij}\right)$ and that of the native-born $\left(\hat{\delta}_0 + \hat{\varepsilon}_{ij}\right)$ can proxy for the gap between the residual wage of the actual migrants $\left(h_m\right)$ and what the wage of a randomly-selected person from the migrant-sending country would be $\left(\overline{h}\right)$.

In our sample the simple average of $\hat{\delta}_j^l$ across all countries j is –0.17 in the standard Mincer specification and –0.21 in the 'category' specification. Suppose that $h_m - \bar{h} \approx -0.21$. Assuming $\rho = 0.5$ and $\gamma = 0.84$, then $\Theta = -0.49$ and the bias in (5) is $R_o / R_e \approx 1.11$.

Note that this implies $f_m - \bar{f} = 0.10$, and if $\sigma_f \approx 1$ then migrants are being drawn from the 54th percentile of the foreign distribution of unobserved traits, while the worth of those unobserved traits in home is only in the 42nd percentile of the home-value of the unobserved traits of all people in foreign. Alternative assumptions on ρ make little difference. The highest bias at any correlation ρ is 1.2 when $\rho = -1$, with the implausible assumption that the observed characteristics that raise wages in the foreign country are perfectly negatively correlated with those that raise wages in the US.[16]

3.2 *Pooled regressions and the randomly-selected migrant*

There is a second, completely different way of assessing the impact of selection. We can directly estimate wage ratios purged of selection effects under a different set of assumptions. Recall the estimation problem (1), which we solved with a set of assumptions A_1. If we accept for a moment the strong assumption that observable worker traits have similar returns across countries, we are not far from a different solution to the selection problem. Suppose that we return to equation (1) and adopt a different set of assumptions, A_2:

- that the schooling and other traits of people from foreign residing in home have the same association with wages as they do for people from home residing in home $\left(\theta_{fh}\left(x^i\right) = \theta_{hh}\left(x^i\right)\right)$;
- that the mean unobservable contribution to wages does not differ across countries due to "culture" or other factors $\left(E\left[\phi_{fh}\left(x'^i\right)\right] = E\left[\phi_{hh}\left(x'^i\right)\right]\right)$;
- that the partial association of wages and unobservable traits in foreign is independent of the same association in home $\left(E\left[\phi_{fh}\left(x'^i\right) / \phi_{hh}\left(x'^i\right)\right] = E\left[\phi_{fh}\left(x'^i\right)\right] / E\left[\phi_{hh}\left(x'^i\right)\right]\right)$;
- and that migration is costless to the migrant.

Now, equation (1) reduces to

$$R_e \mid A_2 \equiv E[R^i \mid A_2] = \frac{\theta_{hh}(x)}{\theta_{ff}(x)}. \tag{6}$$

The right-hand side of (6) is observable. The result is an estimate of the ratio of wages after and before migration of a person with observed traits x in foreign who is *randomly selected* and obliged to migrate. It has the advantage of being unaffected by selection (by definition). It has the disadvantage of making the strong assumption that returns to

[16] Given $h_m - \bar{h} = -0.21$, then if $\rho = 0.3$ the selection bias in the observed ratios is 1.14. If $\rho = 0.7$ the bias is 1.06; if $\rho = 0$ the bias is 1.16; if $\rho = -0.5$ the bias is 1.18.

observable traits x do not depend on the origin of those traits; for example, a year of additional schooling acquired in Ghana is rewarded in the US labor market by the same amount as a year of additional schooling acquired in the US. It has the further disadvantage of assuming costless migration. Below we will relax these counterfactually strong assumptions on the returns to schooling and, later, migration costs.

3.2.1 *No adjustment for schooling quality*

To estimate equation (6) we combine in a single sample all individuals residing in the US and in all countries $j \in J$, where J is the set of all countries besides the US, and estimate the regression equation

$$\ln w_{ij} = X_{ij}\zeta + \begin{pmatrix} \delta_0 + \beta_0 s_{ij} \\ \delta_j^r + \beta_j^r s_{ij} \end{pmatrix}' \begin{pmatrix} 1 \\ I_{ij}^r \end{pmatrix} + \varepsilon_{ij}. \tag{7}$$

Just as before, *mutatis mutandis*, we run three different specifications of (7): a basic Mincer version, and “extended” Mincer version with quadratic and cubic terms, and a “category” version with indicator variables for levels of schooling and age. But whereas regression (4) was run with only individuals from the US and one other country at a time, regression (7) is run with all individuals from the US and all other countries at once.

From these pooled estimates we calculate the ratio of the expected wage of a randomly-selected individual with s years of education residing in the US to the expected wage of the same person residing in country j as

$$\tilde{R}_{o,j} \equiv e^{-\left(\hat{\delta}_j^r + \hat{\beta}_j^r s\right)}, \tag{8}$$

where $\hat{\delta}_j^r, \hat{\beta}_j^r$ are empirical estimates of the corresponding parameters. Table 3 presents estimates of (8). The first column reproduces the bilateral estimates of $\hat{R}_{o,j}$ from column IV of Table 2 for comparison. The second column shows pooled estimates of $\tilde{R}_{o,j}$ based on the ‘category’ specification, and the third column shows pooled estimates of $\tilde{R}_{o,j}$ based on the basic Mincer specification. The agreement between the columns is close.

Table 3: Wage ratios from pooled regressions

Column	I	II	III
Sample	Bilateral	Pooled	Pooled
Specification	Mincer	Category	Mincer
Schooling	Avg. 9-12	9-12	Avg. 9-12
Average	**4.06**	**4.92**	**4.38**
Median	**3.44**	**4.20**	**3.74**
Correlation with col. I	**—**	**0.98**	**0.99**
Nigeria	11.72	14.41	12.39
Haiti	10.81	13.51	11.92
Egypt	8.77	10.12	8.69
Yemen	8.71	9.47	9.28
Ghana	7.45	10.40	8.70
Sierra Leone	7.10	7.24	7.18
Indonesia	6.09	6.69	6.12
Cameroon	5.91	7.04	5.93
Vietnam	5.84	5.94	5.72
Venezuela	4.91	6.28	5.69
Pakistan	4.69	6.31	6.07
India	4.46	5.49	4.51
Bangladesh	4.45	5.84	5.19
Ethiopia	4.30	6.50	4.62
Ecuador	4.15	5.28	4.75
Jordan	4.10	5.54	4.91
Cambodia	4.05	3.80	4.20
Sri Lanka	4.04	5.41	4.69
Bolivia	3.97	4.68	4.16
Uganda	3.96	4.86	3.77
Philippines	3.80	4.36	3.71
Nepal	3.08	4.56	4.18
Guyana	2.92	2.76	2.82
Brazil	2.91	3.36	2.96
Chile	2.79	2.92	2.78
Panama	2.77	3.28	3.01
Jamaica	2.76	3.21	2.88
Peru	2.73	4.03	3.38
Thailand	2.50	2.88	2.42
Turkey	2.46	2.42	2.45
Uruguay	2.23	2.42	2.19
Colombia	2.16	2.89	2.54
Guatemala	2.14	2.81	2.51
Nicaragua	2.13	3.01	2.63
Morocco	2.00	2.49	2.21
Mexico	1.94	2.79	2.32
South Africa	1.79	2.10	1.70
Argentina	1.74	1.78	1.71
Belize	1.69	1.84	1.70
Paraguay	1.64	1.95	1.75
Costa Rica	1.61	1.94	1.72
Dominican Rep.	1.36	1.94	1.82

Col. 1 reproduces Table 2, col. 4 for comparison. Results are for 35 year-old urban male formal-sector wage earners. “Mincer” is standard Mincer specification. “Category” uses dummies for five education levels and nine age levels. Columns I and III show, for each country, the geometric average of the ratios obtained for 9, 10, 11, and 12 years of education.

As a means of bounding the bias due to selection, these raw results are no help. The predicted wage ratios from pooled regressions in Table 3—which are unaffected by selection—are frequently higher than the earlier estimates of Table 2 (the median is higher by 0.3). We might expect the opposite if the bilateral regressions comparing actual migrants were biased upward by selection. A natural candidate for the cause of that anomaly is that the pooled regression wage ratios are premised on the assumption that *s* years of schooling acquired in a foreign country are worth exactly the same amount in the US labor market as *s* years acquired in the US, to which we now turn.

3.2.2 *With adjustment for schooling quality*

To calculate wage ratios that relax this strong assumption requires some measure of the difference between the US labor market value of schooling acquired in the US and schooling acquired abroad. We perform two calculations, one based on differences in international standardized test scores and one based on the estimates in the bilateral regressions.

A first cut is to assume that years of schooling that produced equal skills would produce equal returns (even if assessed in the native language). Hanushek and Woessmann (forthcoming) construct an internationally-comparable measure of learning achievement acquired by eighth-grade students in 50 countries, by transforming any available internationally-standardized math and science examination score for a country into a single measure and then averaging those measures into an omnibus achievement score with mean 500. We transform that score into a "years of equivalent schooling" to the US by taking the difference between the foreign score and the US score and dividing by an assumed US annual learning gain of 40 score points per year (based on the comparisons of equal aged students in eighth or ninth grades on the PISA examination). For instance, the quality score for Argentina is 392 and for the US is 490, so the achievement-adjusted years equivalent deficit is (490–392)/40 = 2.45, and an Argentine with 9 years of schooling working in the US would be predicted to have wages in the US equivalent to a US worker with 6.55 years.

For the seventeen countries that are both in our sample and have scores from Hanushek and Woessman (Forthcoming), the average (median) years-equivalent deficit is 2.39 (2.18).[17] We then use the pooled coefficient estimates to calculate the wage ratios for each country based on the assumption that in the US labor market a year of schooling acquired abroad is valued only as an achievement-adjusted year. The top panel of Table 4 shows the results. If the entire difference between $\hat{R}_o$ and $\tilde{R}_o$ were due to the selection of migrants then this bias would be 1.25 to 1.34.

[17] This accords well with the finding by Bratsberg and Ragan Jr. (2002) (Fig. 3) that immigrants to the US with 9 or more years of education acquired abroad have roughly the same earnings on average as US-educated immigrants who have two years less education but are otherwise observably identical (the gap disappears below 9 years of education). They find that this effect is not due to language differences or to the effect of arriving earlier and growing up in the US, but primarily to either superior skill acquisition or superior skill certification in US schools compared to foreign schools, for the US labor market.

The first method assumes that all of the difference in impact of wages on schooling is associated with measures of learning achievement. A second method uses our own empirical estimates of the wage increment from schooling acquired abroad in the US labor market versus that of a year acquired in the US, from the bilateral regressions. This includes not just "quality" in the sense of learning achievement but also relevance to the US labor market and US-specific signaling effects. Averaged over all bilateral regressions the coefficient on s of US residents is $\beta_0 \equiv 0.123$, for foreign-born US residents who arrived before age 20 (some of whom acquired schooling in the US) it is $\beta_j^e = 0.106$, and for foreign-born US residents who arrived at or after age 20 the coefficient is $\beta_j^l = 0.065$.[18] We calculate wage ratios using the pooled regression coefficients but compare a US worker with 9 years of schooling to a foreign worker with $s = 9 \times \left(\beta_j^l / \beta_0\right)$ to account for the "evaporation" of schooling in the move from foreign to the US. The middle panel of Table 4 shows this calculation. Again, if we interpret the excess of the wage ratios of observably equivalent actual migrants to the wage ratios of observably equivalent non-migrants to selection effects the effect is somewhere between 1.65 and 1.72.

Finally we ask the question of how many years of foreign-acquired schooling would have to "evaporate" when an immigrant arrives in the US to make the estimates for wage gains to the randomly-selected migrant (pooled regressions) accord with those for the marginal migrant (bilateral regressions) at a given degree of selection of the marginal migrant. Given the above estimates that an immigrant with nine years of education experiences the "evaporation" of roughly two years of schooling, an evaporation fraction of 0.2-0.3 is reasonable. This suggests that selection biases the bilateral estimates of the wage ratios relative to the quality adjusted pooled regressions (with no selection) upwards by a factor of between 1.2 and 1.3.

[18] We get similar results with the bilateral regressions using educational categories, the ratio of the coefficient for immigrant late arrivers to that of the US for those with 9 to 12 years of schooling is 0.49.

Table 4: Adjusting the estimates of wage ratios of observably equivalent workers from pooled regressions adjusting for the quality of schooling

(a) *Adjustment using Internationally Comparable Assessments of Learning Achievement*

Row		Average, N = 18	Median, N = 18
I	Wage ratios, bilateral regressions, basic Mincer, S=9	4.66	3.03
II	Unadjusted wage ratios, pooled regressions, basic Mincer, S=9	4.13	2.63
III	Wage ratios, pooled regressions, schooling in US in test score adjusted years equivalent	3.08	2.11
IV	Ratio row I / row III	**1.34**	**1.25**
V	Number of score adjusted years equivalent of schooling that foreign is less than that of the US	2.39	2.18

(b) *Adjustment using differences in returns in the US labor market of schooling acquired in the US and abroad*

Row		Average, N = 42	Median, N = 42
I	Wage ratios, bilateral regressions, basic Mincer, S=9	4.10	3.59
II	Unadjusted wage ratios, pooled regressions, basic Mincer, S=9	4.09	3.75
III	Wage ratios, pooled regressions, schooling of migrants in US scaled back by β^l_j/β_0	2.47	2.18
IV	Ratio row I / row III	**1.65**	**1.65**
V	Ratio of Mincer coefficient for years acquired in foreign country versus US, β^l_j/β_0	0.533	0.495

(c) *Adjustment using a hypothetical "evaporation" ratio*

Row		Median, N = 42					
I	Wage ratios, bilateral regressions, basic Mincer, s = 9	3.59	3.59	3.59	3.59	3.59	3.59
II	Unadjusted wage ratios, pooled regressions, basic Mincer, s = 9	–	–	–	–	–	–
III	Wage ratios, schooling in US labor market scaled by (1 – evaporation ratio)	3.75	3.37	3.02	2.71	2.44	2.19
IV	Ratio row I / row III	**0.96**	**1.07**	**1.19**	**1.32**	**1.47**	**1.64**
V	Assumed fraction of a year of schooling acquired abroad that "evaporates" in US labor market	0.0	0.1	0.2	0.3	0.4	0.5

3.3 *Natural experiments*

A third way to bound the bias is to rely on natural experiments that permit identification of the wage gain from migration *per se*. McKenzie, Gibson, and Stillman (2006) present a unique experimental estimate of the wage gain to migration from a poor to a rich country. They take advantage of New Zealand's Pacific Access Category residence visa, which is designed to allow a limited number of citizens of Tonga (and three other island states) to settle in New Zealand each year via a random lottery. Any person age 18-45 who is a citizen and natural of the four PAC countries may register for the lottery, and among those registered a certain number are randomly allocated the chance to apply for residence. Though not all lottery winners later acquire New Zealand residence, a survey comparing the wages of Tongan lottery losers with those of Tongan lottery winners allows calculation of an intent-to-treat effect, which in turn allows isolation of the pure effect of movement on wages.

In the sample of McKenzie, Gibson, and Stillman (2006), the mean weekly income of Tongan non-applicants to the lottery is NZ$70. The OLS estimate of the income gain to migration—which controls only for education, age, sex, height, and birth on Tonga's principal island—is NZ$360 per week. The experimental estimate is NZ$274. Controlling for observables, then, the wage ratio R_o is (70+360)/70 = 6.14, while the true wage ratio R_e controlling for both observables and unobservables is (70+274)/70 = 4.91. In other words, the predicted wage ratios of observational equivalent workers overstates the true ratio of selected movers by a factor of 6.14/4.91 = 1.25.

While currently there are no other estimates from policy-induced migration channels free of selection, there is evidence comparing destination-country incomes among migrant streams that plausibly differ in their basis and degree of selection. Cortes (2004) finds that refugees who arrived in the US between 1975 and 1980 had 6% lower earnings in 1980 than economic migrants who arrived in the same period—controlling for education, age, language ability, marital status, and region of residence. If indeed the unobserved traits x' of migrants differ systematically from the true unobserved traits x'_r of non-migrants, then estimates of R_e based on (3) are biased by $E\left[\hat{\phi}_{fh}(x')\right] / E\left[\hat{\phi}_{fh}(x'_r)\right]$. The more refugees' unobservable determinants of earnings resemble those of the average non-migrant, the more closely this bias term is approximated by the Cortes (2004) finding of 1/(1–0.06) = 1.064. This means that either (i) if refugees are much less positively selected on unobservables than other migrants, then the selection bias in our estimates of R_e is not large, and is not far above 1.1, or (ii) refugees are selected on unobservables as much as other migrants, in which case their earnings relative to other migrants are not informative about the degree of bias that selection produces in our estimates.[19]

3.4 *Bounding the effects of natural barriers*

The wage ratio R_e measures the wedge between an individual's wages after and before migration, and reflects the cost of moving. As in equation (1), part of this cost we term "natural" (δ_n)—which is to say largely independent of government policy—such as transportation expenses, psychic costs of an unfamiliar environment, costs of language acquisition, and so on. The rest is the policy-induced cost of moving (δ_p).

[19] Cortes (2004) also finds that by 1990, the same refugee cohort had 20% *higher* earnings than economic migrants, controlling for the same observable traits. Measured at this time-point, then, if refugees are much less positively selected on unobservables than economic migrants, our wage ratios are not only overestimated to a small degree—they are underestimated.

Table 5: PPP wage ratios for urban males between continental US and Puerto Rico, and between US states

Sample	Pooled	Pooled	Pooled	Bilateral	Bilateral
Specification	Mincer	Mincer	Category	Category	Category
Years of education	9	9	9-12	9-12	9-12
Age	35	35	35-39	35-39	35-39
State-specific educ.?	N	Y	N	Y	Y
Comparison				US-born	PR-born
Puerto Rico	**1.51**	**1.31**	**1.56**	**1.75**	**1.50**
48 US States, base group New York					
Average	0.97	0.87	0.97		
Median	0.95	0.86	0.96		
Standard deviation	0.08	0.08	0.07		
Maximum	1.15	1.02	1.15		
Minimum	0.84	0.74	0.85		
Max./Min.	**1.37**	**1.37**	**1.35**		

The maximum states are North Dakota (cols. 1 & 3) and Rhode Island (col. 2); the minimum states are Texas (cols. 1 & 3) and Mississippi (col. 2). "Basic" means standard Mincer specification with linear education term and linear + quadratic age terms. "Category" means dummies for five education levels and nine age levels. "State-spec ed" means the education terms are allowed to differ in each state. The Berry, Fording, and Hanson (2000) US state cost-of-living index does not include estimates for Alaska, Hawaii, or the District of Columbia, so no comparable PPP wages can be calculated for those. New York is the base group in the regressions. "US born" column is the ratio of wages for US-born, US residents to those Puerto Rico-born Puerto Rico residents. The "PR born" column shows the ratio of wages for Puerto Rico-born US residents to Puerto Rico-born Puerto Rico residents. Sample is roughly 1.6 million people, all regressions weighted with sampling weights.

What portion of the ratios in Table 2 can be explained by natural costs δ_n? We approach this question by estimating equation (1) in settings where there are few legal barriers to movement, so that $\delta_p \approx 0$: migration within countries, and movement from Puerto Rico and Micronesia to the United States.

Table 5 presents estimates of $\hat{R}_o$ and $\tilde{R}_o$ for workers in New York to those in 47 other continental US states and Puerto Rico, using regressions similarly structured to those underlying Table 2.[20] The earnings of observably identical workers across US states differ by a ratio of 1.35-1.37 at the extremes, which is not surprising given the spatially integrated labor market.

The earnings of workers born in Puerto Rico but residing in New York state are about 1.5 times the earnings of observably identical residents of Puerto Rico born in Puerto Rico. This ratio combines the effect of natural barriers δ_n and any bias to the ratio (2) from

[20] Dollar wages in Puerto Rico are converted to "mainland PPP" dollars by dividing by the PPP factor 0.86 from Heston, Summers, and Aten (2006). The census data do not contain year of arrival on the mainland for people born in Puerto Rico, so for these regressions we combine "early" and "late" arrivals in (6) into a single category. Dollar wages in each of the US states are standardized for purchasing power based on the state cost-of-living estimates by Berry, Fording, and Hanson (2000). Only 48 states are estimated because the cost-of-living estimates used to standardize wages by purchasing power are missing for Alaska and Hawaii (and the District of Columbia). The state of New York is used as the base group.

selection of emigrants. This suggests that a reasonable upper bound on $(1+\delta_n)(1+\delta_s)$ for a non-English-speaking country close to the United States is 1.5.

These results for US states accord well with domestic rural-urban wage gaps for observably identical workers within all 43 developing countries we study. The coefficient on the "rural" dummy in the pooled regressions of Section 3.2 is approximately –0.3. This suggests that the average ratio of an urban worker's wages to those of an otherwise observably identical rural worker is about 1.4 across all these countries on average. Such a wedge reflects the combined effect of natural barriers and selection $(1+\delta_n)(1+\delta_s)$. In spatially integrated labor markets, movement itself places bounds on how high this wedge can rise.

There is another, less-studied case of a developing country facing no migration barrier from the US. Since 1986, any citizen of the Federated States of Micronesia (FSM) can acquire on demand a US work visa of unlimited duration. Akee (2007b) presents linked microdata on 632 individuals who were in FSM at the time of the 1994 census and had migrated to the United States by the time of the Micronesian Immigrant Survey of Hawaii and Guam in 1997. Many people in the sample were self-employed or unemployed prior to migration, and only 92 report wage income before and after migration. Mean annual pre-migration earnings are roughly US$4,000 and mean annual post-migration earnings are roughly US$8,000.[21] Using the PPP deflator of 0.51 for FSM with respect to the US as a whole (Heston, Summers, and Aten (2006)), purchasing power-adjusted wages would be roughly the same before and after migration. But group housing arrangements, food choices, and other strategies allow recently-arrived FSM migrants to live at somewhat less than the typical cost of living for full US citizens, suggesting that the ratio representing their true real wage gain to migration is likely to be in the range of 1.1 to 1.4. Note that in this case the same individuals are being followed across the border, so selection bias $\delta_s = 0$.

In other words, the FSM and US labor markets are moderately well-integrated in wage terms, and δ_n is likely to be in the range of 0.1 to 0.4. The sample is quite small so this estimate cannot be treated as exact, but it has the advantage of being entirely free of selection effects and therefore a pure estimate of natural barriers aside from those related to language differences. It suggests that for a middle-income developing country whose official language is English, δ_n can be low despite substantial transportation costs.

Prior to 1917, Italians faced only minimal legal restrictions to enter and work in the US. Hatton and Williamson (2005) pp. 53-55 show that in the period 1880-1900, real unskilled urban wages were roughly 2.5 times larger in the United States than in Italy, a disequilibrium phenomenon that helped to spark an enormous emigration of Italians as the real cost of transportation plummeted. Roughly 25% of the population departed between 1870 and 1920. Before major legal barriers to Italian and other immigration

[21] Wage and price information in personal communication from the author. These data only capture a relatively short window of experience in the destination (less than two years) and wages may increase over a longer time period.

were raised during 1918-1924, this flood has already slowed dramatically and return migration had soared as the wage ratio descended to about 1.5 (Williamson (1997), Fig. 6). This suggests $(1+\delta_n)(1+\delta_s)\approx 1.5$ as a plausible estimate for a developing country facing moderate distance and language barriers.

3.5 *Corroboration with macroeconomic evidence*

A final way of assessing the bias induced by selection of migrants is to compare our individual based estimates with aggregate estimates. A basic question in the economic growth literature is how much of the observed income differentials across countries are due to differences in the accumulation of factors—physical and human capital versus country specific productivity. There are two important implications of this literature for our results.

First, if our wage ratios comparing observably equivalent worker estimates are biased upward by the positive selection of migrants, then one would expect our estimates to be typically much higher than the macroeconomic estimates of the relative productivities of labor with equal human capital, which are much less subject to this bias. Hall and Jones (1998) and Hall and Jones (1999) estimate a decomposition of countries' relative output per worker relative to the US into physical capital stocks, human capital, and country specific productivity. The agreement is striking between our wage ratio R_o and the Hall and Jones growth accounting estimates of the relative marginal product of human capital equivalent workers for the 37 countries which have both. For these countries our median wage ratio estimate is 3.11 ('category' specification) or 2.92 (Mincer specification, geometric average of 9 to 12 years of schooling). The estimates of the ratio of marginal products of human capital adjusted labor from Hall and Jones (1999) is 3.07.

This close general agreement across 37 countries, both in mean and correlation is particularly striking as they are calculated by completely unrelated methods from unrelated data.[22] This suggests, once again and via independent evidence, that there can only be a limited degree to which our estimates of R_o are biased upward relative to R_e by the positive selection of migrants. Our numbers also agree closely with those of Hendricks (2002), who finds that even after controlling for cross-country differences in physical capital and both observable and unobservable human capital, GDP per capita in the United States is three times higher than in the average low-income country; it is eight times higher than in the poorest countries.[23]

[22] The raw correlation between the two estimates is 0.5, but this is driven by a relatively small set of countries for which the estimates diverge substantially. If one drops Uganda, Yemen, Jordan, Egypt and Venezuela—all of which have very large informal sectors that our estimates omit—the correlation is 0.8.
[23] Hendricks (2002) compares earnings of observably identical workers from different countries in the United States to estimate the unobservable portion of human capital across countries, but his analysis is otherwise macroeconomic.

Table 6: Comparison of estimated wage ratios of observably equivalent workers to Hall and Jones growth accounting estimates of the relative marginal product of human capital adjusted labor for 37 countries

Row		Median	Average	Avg. without 5 outliers
I	Bilateral, regressions with education categories	3.11	4.39	4.04
II	Bilat. Mincer regressions geometric avg. of years, 9-12	2.92	4.10	3.79
III	Ratios of marginal product of human capital equivalent labor, US to country	3.07	3.69	3.66
IV	Ratio of United States *A* to country *A*	2.44	2.71	2.86
V	Ratio of row I (preferred R_o estimate) to row III (growth accounting estimate)	1.01	1.19	1.11
VI	Proportion of cross-national difference in human capital adjusted labor due to differences in *A* (ratio IV to III)	0.80	0.74	0.78

Sources: Author's calculations and Hall and Jones (1998) (Table 7), Hall and Jones (1999).

A second important implication of these results is that the large majority of the cross-national gap in marginal products of workers with equivalent human capital is due to productivity differences, not physical capital. This has consequences for the expected welfare gains of migration for people in the destination country: If the differences are mostly productivity ("*A*") and country-specific "*A*" is a pure public good, then there is little "factor shallowing" effect of reducing wages for all existing workers by lowering the capital-labor ratio with labor inflows.

In fact, row IV of table 5 shows that the typical ratio of factor productivities (A) is 2.44, which is roughly 80 percent of the observed wage (or marginal product) ratio. Caselli (2005) reviews the literature on growth decompositions and shows that, in the standard models, it is typical for physical and human capital differences to account for less than 50 percent of differences in per-worker output. As Easterly (2004) points out, in "productivity world" factors move to higher productivity locations, as opposed to "factor world" in which places with scarce factors attract more factors. In "productivity world" the gains to movers are not offset by losses to existing residents.

4 Comparison to other literatures

The preceding section gives us the tools to plausibly estimate what fraction of the wage gaps we measure can be attributed to wage discrimination. We can then compare wage discrimination at the border to other forms of wage discrimination that have been observed in different labor markets. We go on to compare international price gaps for labor to those in goods and factor markets. Finally, we compare the potential gains from migration to other forms of poverty reduction policies or programs.

4.1 *Comparison to other forms of wage discrimination*

The diverse evidence reviewed in the previous section is notably consistent: Each piece suggests that the measured ratios (3) in Table 2 overstate the true ratio by a factor of roughly 1.5 or less, due to a combination of selection bias and natural barriers. The remaining portions of these ratios, that attributable to pure border effects $1+\delta_p$, are very large for many countries. This suggests, for example, that of the median wage ratio of 3.92 unexplained by observable characteristics, roughly a ratio of 2.6 constitutes a pure border effect. Here we compare these gaps, by definition a form of wage discrimination, to other forms of discrimination within countries.

By “wage discrimination” we mean any labor market force that causes workers of equal intrinsic productivity involuntarily to receive different wages because of socially constructed characteristics of the workers. Barriers to the movement of workers across international borders create wage discrimination of precisely this kind. Suppose a given worker has a higher realized productivity in the United States than in Bolivia—due to the institutions, technologies, and other complementary inputs available in the United States—and therefore earns higher wages in the United States. Any policy that limits the realized productivity of that worker, by limiting access to the United States and its complementary inputs, obliges that person to accept a lower wage than he or she could potentially realize. This is wage discrimination based on a socially-constructed trait, workers’ country of their birth. While in common usage “discrimination” and “prejudice” have become synonymous, no American need feel toward that worker any distaste or prejudice, racial or otherwise, in order to produce this result.[24]

We begin with a comparison of sex discrimination to country-of-birth discrimination, using only information internal to our dataset. We perform threefold Oaxaca-Blinder decompositions for each of the 42 developing countries under study.[25] These estimate what portion of the average difference of some variable between two groups is due to differences in observable characteristics, what portion is due to differences in the coefficients on those characteristics, and what portion is due to differences in the interaction between coefficients and characteristics. The bars in the top pane of Figure 3 show the mean difference between ln(wage) between residents of each country and observably identical US-residents who were born and educated in that country.[26] The dark area of each bar shows the portion that is due to differences in coefficients on observable traits, while the light area of the bar shows the portion that is due to either differences in observable traits or the interaction of observable traits and coefficients.[27] In general the large majority of wage gaps across the border are not explained by differences in observed characteristics.

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[24] Some might question the use of the term “discrimination” for international differences. The foundational model of labor market discrimination by Becker (1971) (p. 35) actually begins—for conceptual clarity—as a model of labor migration from a black ‘country’ to a white ‘country’, and only later comes to represent racial discrimination in a single spatially integrated market by analogy. Becker defines wage discrimination as cross-group differences in wages unrelated to worker productivity, which is precisely what we seek here. He notes that “treating discrimination as a problem in trade and migration is far from artificial, since they are closely and profoundly related”.

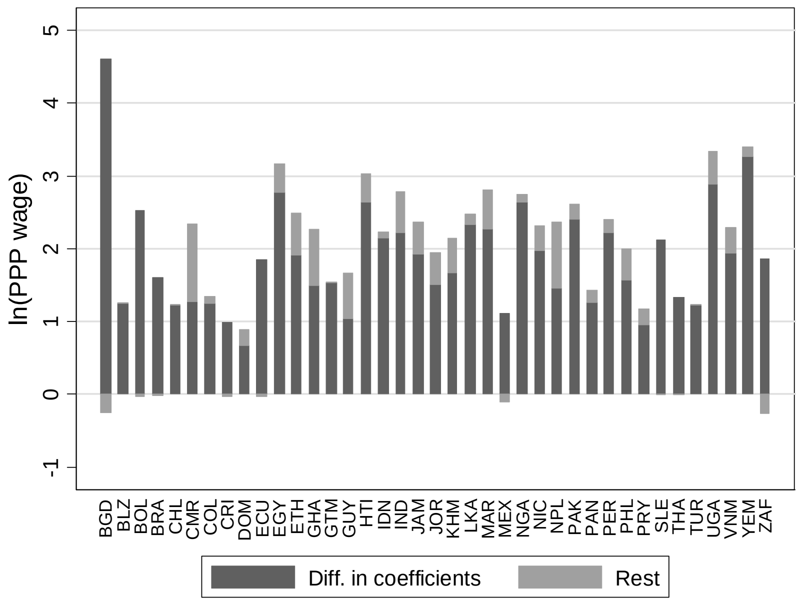
[25] We use the threefold decomposition of Winsborough and Dickenson (1971), closely related to the decomposition of Oaxaca (1973) and Blinder (1973).

[26] These decompositions use the “category” regression specification with country-specific coefficients on schooling. The results are similar for the Mincer specification.

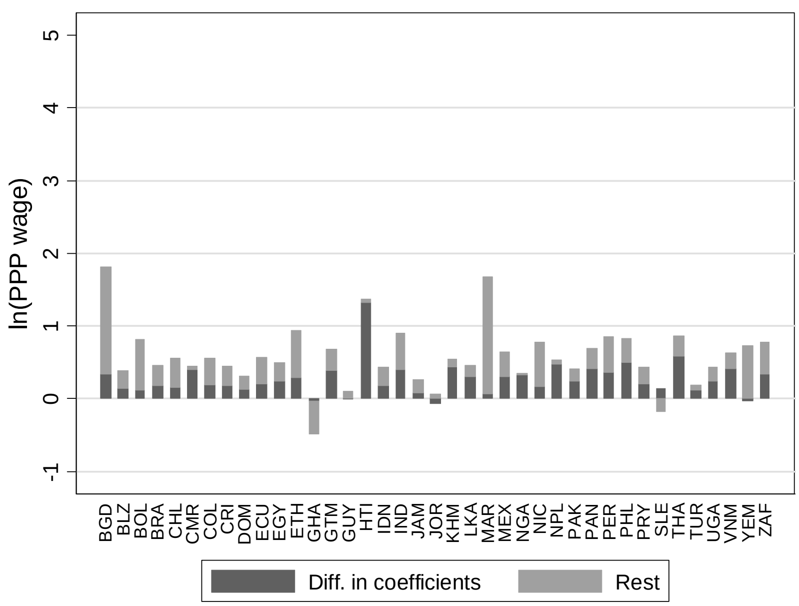
[27] For the present purpose this rendering is conservative, as only the portion completely attributable to differences in coefficients is represented by the dark area. The light area includes the portion attributable to the interaction between differences in coefficients and differences in observable traits, which includes some portion of the gap attributable to differences in coefficients.

Figure 3: Comparing unexplained international wage gaps with unexplained sex wage gaps domestically, with the Oaxaca-Blinder decomposition

(a) Oaxaca-Blinder decomposition of foreign-born, foreign-educated, by residence in US versus residence in country of birth



(b) Oaxaca-Blinder decomposition of residents in their country of birth, by sex



Threefold decompositions where “Rest” contains both differences in observed traits and the interaction term.

The lower pane of Figure 3 shows a strictly domestic decomposition of the gap between male and female wages for residents of each country, but is otherwise identical to the cross-border decomposition above. Even corrected for the greatest plausible degree of selection and natural wedges in the cross-border wage gaps, the degree of unexplained difference in wages across international borders is—for every country in the sample—dramatically greater than male-female wage gaps within countries unexplained by observables. Wage discrimination across the border generally exceeds sex discrimination within borders by an order of magnitude.

In the United States, the wages of males to those of observably identical females who are in the workforce is about 1.3.[28] In our sample of 42 developing countries the median domestic ratio of wages of males to observably equivalent females is 1.4 (Madagascar), a large magnitude of which explains the justifiable widespread concern about sex and gender discrimination. But for 35 of the 42 countries, estimated border discrimination estimates, R_e, exceeds the estimate of domestic gender discrimination. In our sample the *highest* the ratio of wages of men to observably equivalent identical women is, perhaps not surprisingly, in Pakistan: a ratio of 3.1. For 17 of the countries we study, the effect of the US border $R_e \approx R_o / 1.5$ exceeds the most egregious estimated gender discrimination.

The literature contains several regressions exploring wage gaps between workers that are observably identical except for their ethnicity, within spatially integrated labor markets. These have included measures of the gaps for black workers in the United States, scheduled castes/scheduled tribes in India, indigenous Malaysians, Indigenous Tanzanians, and indigenous Bolivians.[29] All of these reveal that wages in the base group exceed wages in the group discriminated against by a factor of 1.1–1.9 unexplained by characteristics. For example, the ratio of a white man's wages to those of an observably identical black man in the United States was about 1.6 in 1939 (by 1995 it had fallen to about 1.1).[30]

Table 7 summarizes all of this evidence on past and present domestic wage discrimination, and contrasts it with our estimates of wage discrimination across borders. The first column compiles various estimates of wage discrimination within countries. The

[28] Altonji and Blank (1999) (Table 1, column 2) show that ln mean hourly wage for a full-year, full-time employed white male in 1995 is ln(17.97) = 2.89. The coefficient on female in Table 4, column 6 is –0.241, suggesting the ln mean hourly wage for an otherwise observably identical woman is 14.12, and 17.97/14.12 = 1.27

[29] Banerjee and Knight (1991) report that in survey data collected in Delhi from October 1975 to April 1976, observably identical Indians who were not members of Scheduled Castes/Scheduled Tribes (SC/ST) earned 10.9% more than members of SC/STs (p. 185). Knight and Sabot (1991) find that in a survey of 1,000 urban Tanzanian firms in 1971, people who were not indigenous Africans (almost all of Asian ancestry) earned 87% more than observably identical indigenous Africans (p. 65). The Mincer wage regressions of Patrinos and Psacharopoulos (1993) (p. 305) indicate that a 28% wage deficit for indigenous workers relative to other workers is unexplained by differences in observable characteristics, thus 1/(1–0.28) = 1.39. Schafgans (1998) (p. 483) finds a maximum differential in wages between Chinese and Malay males unexplained by observables of 27%, thus 1/(1–0.27) = 1.37.

[30] Altonji and Blank (1999) (Table 1, column 2) show that ln mean hourly wage for a full-year, full-time employed white male in 1995 is ln(17.97) = 2.89. The coefficient on black is –0.067, suggesting that ln mean hourly wage for an otherwise observably identical black male is 16.81, and 17.97/16.81 = 1.07. Chandra (2000) documents the substantial narrowing of the black-white wage gap between 1940 and 1990, and Heckman, Lyons, and Todd (2000) suggest that some substantial portion of the narrowing during this period remains unexplained by changes in observable characteristics other than race. Sundstrom (2007) (p. 412) conducts Mincer regressions using the 1940 US census and finds a black-white wage gap of 36% unexplained by observable characteristics in 1939 wages.

second column summarizes the measured effects of natural barriers and selection on wage gaps in the absence of policy barriers to movement, discussed in Section 3.4. The third column gives our estimate of R_e, the pure border effect on the wage ratio. The fourth column gives the estimates of R_o—wage ratios for observably equivalent workers—from Table 2. The estimates in column 3, with the exception of the McKenzie, Gibson, and Stillman (2006) experimental estimate, are calculated by dividing the values in column 4 by 1.5. Figure 4 summarizes the same information graphically.

The key lesson of Table 7 is that the wage discrimination created by international borders is, for large numbers of developing countries, at least as large as any current form of wage discrimination against socially disfavored groups within spatially integrated labor markets. For many of the 42 developing countries we investigate, it is much larger. For several countries—including Nigeria, Haiti, Egypt, and Ghana—the US border effect on the wages of equal intrinsic productivity workers is greater than *any* form of wage discrimination (gender, race, or ethnicity) that has *ever* been measured.[31]

4.2 *Comparison to other border-induced price wedges*

Empirical economics has frequently commented on the fact that borders introduce large wedges in the price of labor between countries. O'Rourke and Williamson (2000) (p. 155) document that lower migration barriers (due to decreased transportation costs) led directly to mass movements of people and an erosion of international wage gaps prior to 1914. A similar, massive convergence in wages has been observed following more recent reductions in migration barriers, such as German reunification in 1990 (Burda (2006)) and many others. But there has been much less discussion of the fact that international wage price gaps exceed any other form of border-induced price gap by an order of magnitude or more.

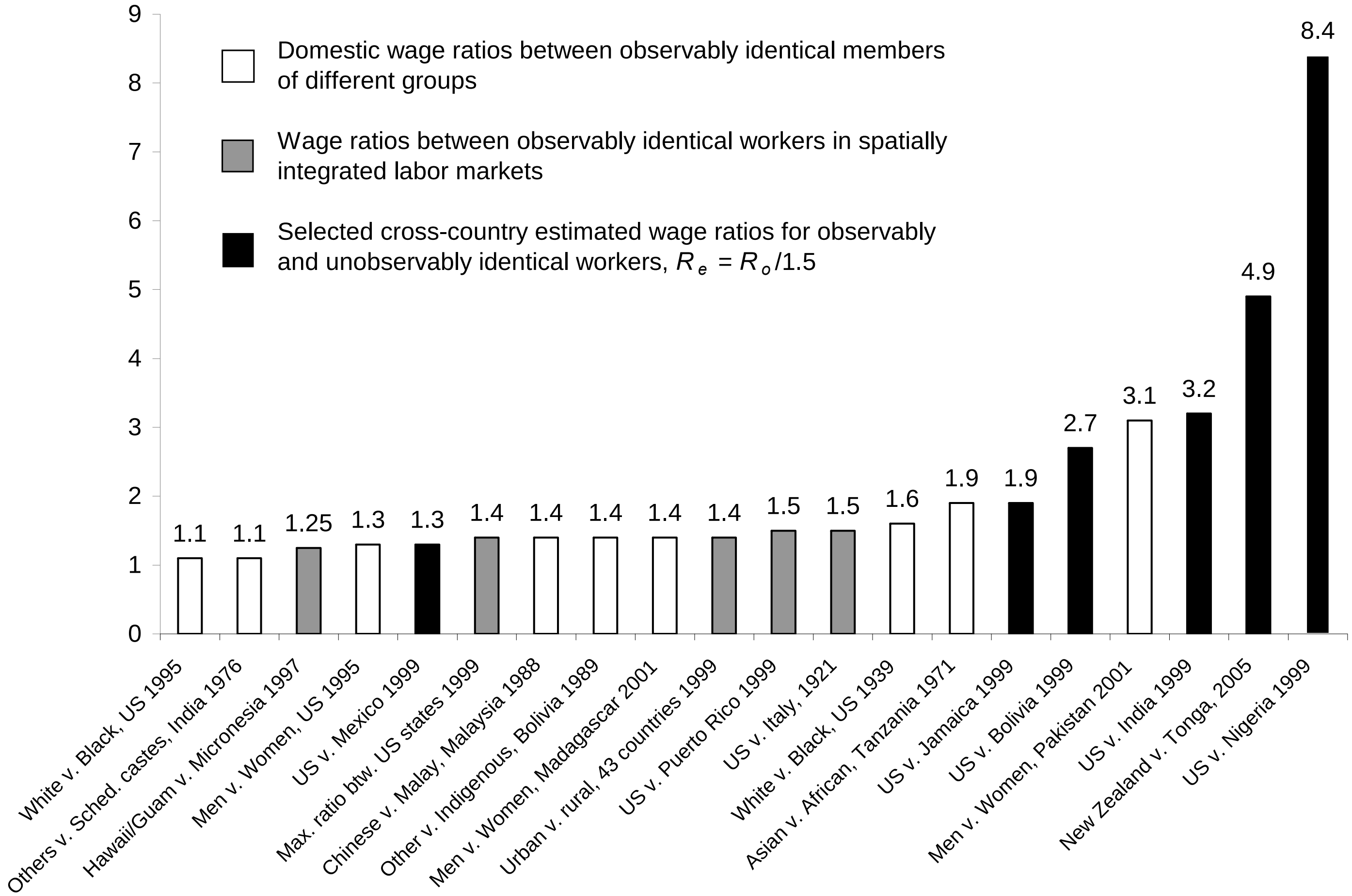
Typical cross-border price gaps for equivalent goods, factors, and financial instruments are much lower than the price gaps we document for the labor of equal-productivity workers. Detailed price comparisons for 120 industries in 1999 executed by Bradford and Lawrence (2004) (p. 7) reveal that consumer prices in many European countries are 30-50% higher than the lowest available prices for essentially the same goods in any country, while prices in Japan are roughly 100% higher.

[31] It is not clear that historical wage discrimination against African Americans, even at its most egregious—which is to say, when African Americans were routinely and forcibly held as property by other Americans—was ever as large as the wage discrimination caused by today's limitation of movement between the US and the poorest countries. Wage discrimination against slaves (not the full effect of slavery, but exclusively the earnings effect) can be plausibly measured by comparing slave rental rates—reflective of slaves' productivity indicator in a competitive rental market—to the cash value of owners' maintenance costs. Though surviving data are controversial and inexact, some estimates suggest that US slaves' productivity was roughly four times the cash value of food, shelter, clothing, and medical care that they received. This is well below today's degree of wage discrimination produced by international borders against naturals of many development countries. Fogel and Engerman (1974) (II.159), for example, estimate $60.62 as the cash value of average maintenance and compensation for adult male enslaved field hands on large plantations, and the same authors (II.73) present rental rates for the Lower South of $143 in 1841-45, $168 in 1846-50, $167 in 1851-55, and $196.5 in 1856-60; the simple average of these four figures is $168.6. Note that these rental rates are net of maintenance costs (II.75), so the average rate inclusive of maintenance is $229.2. In a competitive rental market, this would suggest that enslaved workers were producing 3.8 times what was spent to sustain them.

Table 7: A comparison of domestic and international wage gaps unexplained by intrinsic worker productivity

Comparison	Domestic wage discrimination	Wage ratios w/o borders	Wage ratios with borders		Based on source
	$1+\delta$	$(1+\delta_s)(1+\delta_n)$	***R_e* (*equal productivity*)**	***R_o* (*observably equivalent*)**	
White v. Black, US 1995	1.1				Altonji and Blank (1999)
Others v. Sched. castes, India 1976	1.1				Banerjee and Knight (1991)
Men v. Women, US 1995	1.3				Altonji and Blank (1999)
Hawaii/Guam v. Micronesia 1997		~1.1–1.4			Akee (2007b)
Max. ratio btw. US states 1999		1.4			Table 5
Chinese v. Malay, Malaysia 1988	1.4				Schafgans (1998)
Other v. Indigenous, Bolivia 1989	1.4				Patrinos and Psacharopoulos (1993)
Men v. Women, Madagascar 2001	1.4				Median |coeff.| on female, –0.33
Urban v. rural, 43 countries 1999		1.4			Pooled coeff. on rural, –0.3
US v. Puerto Rico 1999		1.5			Table 5
US v. Mexico 1999			~1.3	1.9–2.0	Table 2
US v. Italy, circa 1920		~1.5			Williamson (1997)
White v. Black, US 1939	1.6				Sundstrom (2007)
Asian v. African, Tanzania 1971	1.9				Knight and Sabot (1991)
US v. Jamaica 1999			~1.9	2.8–3.0	Table 2
US v. Bolivia 1999			~2.7	4.0–4.1	Table 2
Men v. Women, Pakistan 2001	3.1				Max. |coeff.| on female, –1.14
US v. India 1999			~3.2	4.5–5.2	Table 2
New Zealand v. Tonga, 2005			4.9	6.1	McKenzie, Gibson, and Stillman (2006)
US v. Nigeria 1999			~8.4	11.7–13.4	Table 2

Figure 4: Comparing wage gaps across borders to wage gaps within spatially-integrated labor markets



Though Bradford and Lawrence point to these deviations from goods price equalization as evidence that globalization has not "gone far enough", we note that international price gaps for the labor of people from poor countries make all remaining global price gaps in trade look small. Many gaps in goods prices have been small for a century: O'Rourke and Williamson (2000) (pp. 29-55) report that the transportation revolution in the late nineteenth century brought prices of the most basic commodities, such as wheat, to near parity between the US and Great Britain. Over the same period, wage gaps for equal-productivity workers between the richest and poorest countries have almost certainly risen dramatically.

This holds to an even greater degree for price gaps in international finance. Lamont and Thaler (2003) survey the literature on cross-border price gaps for equivalent financial instruments, and point out that gaps of 15% are considered "inexplicably large". Alongside gaps on the order of 1,000% in the price of equal-productivity labor, all remaining price gaps in financial markets amount to rounding error.

The fact that price gaps for labor frequently exceed price gaps for goods by an order of magnitude has strong implications for the social welfare effects of different forms of economic liberalization. Welfare effects move as the square of price wedges. The World Bank (2005) (p. 128) finds that following an elimination of *all* remaining policy barriers to trade worldwide, developing countries would gain $109 billion in annual income by 2015. But the simulations of Walmsley and Winters (2005) (Table 4, col. V) suggest that just a tiny relaxation of barriers to migration—allowing a movement amounting to just 3% of the OECD countries' labor force—would raise the annual incomes of people from developing countries by $156 billion. These aggregate figures suggest that liberalizing labor movements might be a more effective antipoverty measure than others—like trade liberalization—that have dominated policy discussions. We now take up this question in greater detail.

4.3 *Comparison to other antipoverty interventions for the marginal worker*

The increment in wages for the marginal worker moving to the United States is, for workers from many countries, enormously larger than the marginal effect on wages caused by any available *in situ* antipoverty measures.

For example, the pioneering (if controversial) estimate by Pitt and Khandker (1998) of the net return on microloans to Bangladeshi women is 18%. Taking this substantial return at face value, this translates into an increase in annual household income of US$65 at purchasing power parity, so that a lifetime of continuous access to lending with these returns would return US$683 in net present value.[32] From Table 2, the annual PPP dollar gain to a male Bangladeshi solely due to working in the US is just over $10,000 when

[32] Their estimate of the return to males is 11%, but we use the higher figure for females to be conservative. Average annual female borrowing is Tk3415, or US$361 at PPP using the average PPP conversation factor from World Bank (2007) over the relevant period (1986-1992) of 9.47. The resulting increase in household income is thus Tk615 or US$65 at PPP. Average life expectancy in Bangladesh during 1986-1992 was 55 years and average borrower age in the sample is 23, and a 33-year stream of US$65 payments (including one at time 0) discounted at 10% has a net present value of US$683. At 5% the value is US$1091 and at 15% it is US$493.

scaled to purge selection bias.[33] In other words, simply allowing one member of a Bangladeshi household to work in the US for *one month* (for a gain of US$835 in present value) brings a larger increase in earnings to that household than a *lifetime* of microcredit (for a gain of US$683 in present value).

Harrison and Scorse (2004) find that international anti-sweatshop campaigns against textile, footwear, and apparel plants in Indonesia caused a 20-25% increase in real wages for workers at foreign-owned and export-focused plants between 1990 and 1996. This translates to an annual wage gain of US$647 at purchasing power parity, which at a 10% discount rate has a present value of about US$6960. This is 58% of the *annual* wage gain from working in the US.[34] In other words, cumulative the lifetime effect of the anti-sweatshop movement on an Indonesian worker's earnings could be earned if that person had the chance to work in the US once for a period of about 30 weeks.

In the country in our sample whose wage ratio is closest to the median, Bolivia, the coefficient on years of education in a simple Mincer regression for Bolivian residents is 0.061. Since the average annual wage for Bolivian residents in our sample is US$3371 at purchasing power parity, this suggests that an additional year of schooling is associated with an annual wage gain of US$205. The net present value of a lifetime of such additional payments is about US$2250,[35] which is about 21% of the annual wage gain to a Bolivian working in the US.[36]

But the ability of many interventions to achieve even one year of increased education is circumscribed. Kremer and Miguel (2004) (p. 206), find that medicating children to eliminate hookworm, roundworm, and other helminthic parasites is "by far the most cost-effective method of improving school participation among a series of educational interventions" attempted in Busia, Kenya that underwent randomized evaluations. They find that deworming raised the present value of lifetime wages for a treated child by US$30, or US$71 at purchasing power parity.[37] This lifetime quantity is equal to about 0.6% of the annual gain to a Kenyan's wages simply from working in the US.[38]

[33] R_o from Table 2, column 1 is 4.92, thus R_e is roughly 3.28 and the annual dollar gain for an equal productivity worker willing to move is $10,021.
[34] They identify the effect by comparing textile, footwear, and apparel firms (the targets of the campaign) to other firms that were not targeted. The 20-25% real wage gain (p. 21 and Table 1B) is from a base mean 1990 wage of Rp1775000 for foreign firms and Rp1462000 for exporting firms. We use the former of the two to be conservative, so that a 25% gain constitutes Rp444000 per year, or US$647 at PPP, using the 1991 PPP conversation factor from World Bank (2007) of 685.78. The annual gain to an Indonesian male from working in the US is roughly US$11981 ($R_o$ = 5.82 in gives the dollar gain of $17971 in Table 2, and scaling by $R_e = R_o /1.5 = 3.88$ gives US$11981). The present value of $6960 includes a payment in year zero.
[35] This is not the "return" to schooling, as Heckman, Lochner, and Todd (2006) stress; it does not account for costs of the education investment, most notably the opportunity cost of time. Strictly speaking, by the assumptions underlying the Mincer model the marginal return to schooling is zero, since individuals acquire education until the benefit equals the opportunity cost.
[36] The present value is calculated at a discount rate of 10%, including a payment at time zero. The gain to a Bolivian migrant in Table 2 is US$16458 at PPP. Dividing this by 1.5 to adjust for selection bias gives approximately US$11000.
[37] World Bank (2007) gives the PPP exchange rate to official exchange rate ratio as 0.42 for Kenya in 1999.
[38] Kenya is not in our sample, but six low-income sub-Saharan African countries are, and here we use the lowest wage gain from among those: Ethiopia, whose wage gain is slightly lower than that of Uganda, a country that might be considered more comparable to Kenya. The wage gain to an Ethiopian from Table 2 is US$17,308 which, divided by

Table 8 summarizes these calculations. For the marginal worker in a developing country, the wage gain to a one-off period of working in the US for several weeks overwhelms the present-value lifetime wage gain to the marginal worker from some of the most effective antipoverty policy interventions rigorously documented in the development economics literature.

Table 8: A comparison of annual wage gains from international movement of the marginal worker, to present-value lifetime wage gains to the marginal worker from different *in situ* antipoverty interventions

Intervention	Country	Present-value lifetime wage increment due to intervention (US$ at PPP)	Annual wage increment due to working in US (US$ at PPP)	Weeks of US work equivalent to *lifetime* NPV of intervention
Microcredit	Bangladesh	700	~10,000	4
Anti-sweatshop activism	Indonesia	2,700	~12,000	30
Additional year of schooling (at zero cost)	Bolivia	2,250	~11,000	11
Deworming	Kenya	71	~11,500	0.3

The figures for annual wage increments to US work are from the rightmost column of Table 2 divided by 1.5 to adjust for selection bias. The figures for present-value lifetime wage gain from development interventions are calculated based on Pitt and Khandker (1998) for microcredit, Harrison and Scorse (2004) for anti-sweatshop activism, a standard Mincer regression for the additional year of schooling, and Kremer and Miguel (2004) for deworming; see text.

This of course does not suggest *in situ* interventions are not worth carrying out. Any intervention with a positive net present value is worth carrying out, and there is no reason not to do several in parallel. Furthermore, the general equilibrium effects of very large movements of people could differ from the marginal effects we measure, just as the general equilibrium effects of *in situ* antipoverty programs can differ enormously from the marginal effects measured by a field trial. That said, the marginal wage effects of movement can greatly exceed the marginal wage effects of *in situ* policies and almost certainly deserve a larger role in the discussion of development and antipoverty initiatives.

5 Conclusion

The combination of wage surveys around the world with the US Census allows us to estimate the wage differences across observably equivalent workers for 42 countries. The median wage gap for a male, unskilled (9 years of schooling), 35 year old, urban formal

1.5 to adjust for selection bias, yields a wage gain for equal productivity workers willing to move of about US$11,500 at purchasing power parity.

sector worker born and educated in a developing country is US$15,000 per year at purchasing power parity.

This estimate tends to understate the gain in household welfare caused by crossing the border, to the degree that any of the wage increment is spent in the country of origin—as remittances or repatriated savings—where prices are lower and a dollar is worth more than a PPP dollar. We show that for a worker from the typical country in our sample, if one fifth of the wage increment from migration is spent in the country of origin, the real wage increment exceeds our estimates by roughly 50%. To be conservative, we do not make any corresponding adjustment in our estimates.

On the other hand, the same estimate of the wage gain tends to overstate the true gains to a potential migrant both because of selection of migrants and because of welfare losses from moving. We use a variety of techniques to bound these two impacts are and conclude that, on average, it would be conservative to scale back the ratio of observational equivalent workers by 1.5 to produce an estimate of the wage gains to a worker willing and able to move. This is a gain of $10,000 per worker, per year. It is a marginal gain in two senses: It is the gain to the next person who moves following a marginal opening (not the gain to a randomly-chosen person), and it is the gain given a small increase in current movement (not the general equilibrium gain under open borders).

The same figure therefore represents the effect of movement restrictions on individuals' wages. We contrast the magnitude of these wage differentials with closely related estimates from three separate literatures.

First, these wage differences meet any descriptively adequate definition of wage discrimination. They arise only from socially constructed characteristics of the worker (like country of birth) that are not related to worker productivity (labor demand) or to the preferences of potential migrants (labor supply). That this discrimination is legally supported—in fact, mandated—and widely regarded as normatively legitimate does not make it any less discrimination.[39] The magnitude of wage differences induced by the US border exceed by roughly an order of magnitude existing discrimination in the US and at the upper ranges of our estimates exceed *any* documented form of wage discrimination.

Second, the differentials we record are generally larger than price gaps for goods or financial instruments between developing countries and the US. While we do not produce estimates of the welfare gains in a general equilibrium model, our empirical results strongly support earlier estimates such of those by Walmsley and Winters (2005) that the gains from a marginal relaxation on barrier to labor mobility produced welfare gains would greatly exceed the total gains to developing countries from elimination of all remaining global trade barriers.

[39] We are not unique in this assertion. Arguments that migration restrictions constitute employment discrimination have been advanced in legal theory by Chang (2003) and in moral philosophy by Carens (1987).

Third, there is a constant search for policies to raise the well-being of the world's poorest people at the margin. Based on our estimates of the annual wage gains from labor mobility one can scale the gains from these efforts—from microcredit to additional education to fair trade—in weeks-equivalent of access to the US labor market. These estimates strongly suggest that no existing policy carried out *in situ* can benefit the marginal poor household as much as than one year of access to the US labor market. This is particularly striking as, while development interventions have positive costs, the estimated welfare cost on existing residents of relaxing the barriers at the border are negative. By no means do we argue for the elimination or replacement of *in situ* measures, but we do call on researchers to pay much greater attention to the antipoverty effects of movement.

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Appendix Table A1: Raw bilateral regression results, Mincer specification (ARG to JOR)

Dependent variable: ln(wages in US$ at PPP

Country	ARG	BGD	BLZ	BOL	BRA	CHL	CMR	COL	CRI	DOM	ECU	EGY	ETH	GHA	GTM	GUY	HTI	IDN	IND	JAM	JOR
Years educ	0.123***	0.124***	0.122***	0.122***	0.130***	0.123***	0.122***	0.124***	0.122***	0.122***	0.123***	0.124***	0.122***	0.122***	0.123***	0.122***	0.122***	0.129***	0.138***	0.122***	0.122***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age	0.199***	0.194***	0.203***	0.202***	0.168***	0.199***	0.203***	0.195***	0.202***	0.201***	0.199***	0.192***	0.200***	0.203***	0.199***	0.203***	0.203***	0.171***	0.124***	0.203***	0.203***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age^2	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.001***	-0.002***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Female	-0.489***	-0.499***	-0.493***	-0.492***	-0.490***	-0.485***	-0.492***	-0.483***	-0.493***	-0.492***	-0.490***	-0.481***	-0.489***	-0.493***	-0.489***	-0.493***	-0.492***	-0.452***	-0.467***	-0.493***	-0.493***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)
Rural	-0.186***	-0.197***	-0.186***	-0.186***	-0.178***	-0.185***	-0.188***	-0.188***	-0.186***	-0.186***	-0.185***	-0.194***	-0.187***	-0.186***	-0.192***	-0.186***	-0.189***	-0.188***	-0.303***	-0.186***	-0.185***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)
Early arrival	0.161*	-0.559	0.402	0.462	0.701***	0.456*	-0.686	0.369***	0.843***	0.613***	0.983***	-0.232	0.545**	0.144*	1.375***	0.257**	0.041*	-1.268	-0.800	-0.027	-0.567
	(0.483)	(0.761)	(0.839)	(0.760)	(0.377)	(0.555)	(2.039)	(0.240)	(0.529)	(0.153)	(0.244)	(0.631)	(0.635)	(0.843)	(0.109)	(0.391)	(0.263)	(0.673)	(0.259)	(0.223)	(0.788)
Early × educ.	-0.006*	0.037*	-0.031	-0.031	-0.051***	-0.031*	0.015	-0.026***	-0.054***	-0.051***	-0.069***	0.013	-0.039**	-0.011*	-0.101***	-0.008*	-0.002**	0.074	0.047	0.008	0.035
	(0.034)	(0.056)	(0.063)	(0.055)	(0.028)	(0.040)	(0.139)	(0.018)	(0.040)	(0.013)	(0.019)	(0.042)	(0.045)	(0.061)	(0.010)	(0.029)	(0.020)	(0.046)	(0.018)	(0.016)	(0.056)
Late arrival	0.544	0.461	0.729	0.923	0.720*	0.580	0.654	0.711***	0.781	0.718***	0.912***	-0.021	0.786	0.725	0.881***	0.540**	0.491***	0.578	0.210**	0.493***	0.321
	(0.278)	(0.313)	(0.497)	(0.457)	(0.191)	(0.334)	(1.077)	(0.109)	(0.284)	(0.084)	(0.126)	(0.388)	(0.357)	(0.346)	(0.069)	(0.184)	(0.111)	(0.484)	(0.124)	(0.133)	(0.572)
Late × educ.	-0.047	-0.066**	-0.067	-0.088**	-0.062***	-0.056	-0.066	-0.079***	-0.078*	-0.088***	-0.096***	-0.017	-0.075	-0.066*	-0.092***	-0.045**	-0.059***	-0.053	-0.017***	-0.039***	-0.045
	(0.019)	(0.021)	(0.040)	(0.034)	(0.014)	(0.024)	(0.069)	(0.008)	(0.023)	(0.007)	(0.011)	(0.025)	(0.026)	(0.024)	(0.007)	(0.014)	(0.009)	(0.033)	(0.008)	(0.010)	(0.040)
Foreign res.	0.459***	-0.886***	-0.281	-0.699***	-0.803***	-0.909***	-1.555***	-0.552***	-0.114***	0.025	-1.033***	-1.351***	-1.514***	-1.200**	-0.297***	-0.217	-2.030***	-1.425***	-0.851***	-0.658	-0.974***
	(0.013)	(0.014)	(1.414)	(0.035)	(0.009)	(0.022)	(0.047)	(0.013)	(0.036)	(0.025)	(0.021)	(0.014)	(0.017)	(0.571)	(0.017)	(0.303)	(0.059)	(0.010)	(0.009)	(0.999)	(0.055)
Foreign. res. × educ.	-0.092***	-0.080***	-0.021	-0.065***	-0.019***	-0.012***	-0.024***	-0.032***	-0.038***	-0.051***	-0.047***	-0.098***	0.005**	-0.074	-0.052***	-0.075**	-0.045***	-0.034***	-0.058***	-0.026	-0.056***
	(0.001)	(0.001)	(0.151)	(0.003)	(0.001)	(0.002)	(0.005)	(0.001)	(0.003)	(0.003)	(0.002)	(0.001)	(0.002)	(0.047)	(0.002)	(0.032)	(0.008)	(0.001)	(0.001)	(0.074)	(0.005)
Constant	1.938***	2.038***	1.870***	1.893***	2.445***	1.931***	1.875***	1.998***	1.894***	1.905***	1.940***	2.042***	1.924***	1.870***	1.936***	1.871***	1.873***	2.387***	3.222***	1.871***	1.878***
	(0.013)	(0.013)	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)	(0.013)
Adjusted R^2	0.385	0.744	0.376	0.409	0.576	0.393	0.386	0.426	0.379	0.381	0.410	0.572	0.429	0.376	0.396	0.376	0.380	0.618	0.730	0.376	0.385
N (total)	477869	462096	458302	461198	567995	516669	459060	493729	467842	475832	479941	463347	480361	462035	472732	463357	468665	567914	567096	474222	468632
N US-born	456557	456557	456557	456557	456557	456557	456557	456557	456557	456557	456557	456557	456557	456557	456557	456557	456557	456557	456557	456557	456557
N early arrival	1088	412	476	503	1425	675	55	4175	767	6528	2722	710	515	309	5115	1988	3523	717	4900	6101	465
N late arrival	1996	1613	628	597	3752	1324	282	8870	1190	9723	5052	2101	1460	1599	8480	3731	8066	1059	22688	9468	609
N foreign resident	18228	3514	641	3181	106261	58113	2166	24127	9328	3024	15610	3979	21829	3570	2580	1081	519	109581	82951	2096	11001

Appendix Table A1 (continued): Raw bilateral regression results, Mincer specification (KHM to ZAF)

Dependent variable: ln(wages in US$ at PPP

Country	KHM	LKA	MAR	MEX	NGA	NIC	NPL	PAK	PAN	PER	PHL	PRY	SLE	THA	TUR	UGA	URY	VEN	VNM	YEM	ZAF
Years educ	0.123***	0.123***	0.123***	0.128***	0.122***	0.122***	0.122***	0.126***	0.122***	0.123***	0.124***	0.122***	0.122***	0.123***	0.122***	0.122***	0.122***	0.123***	0.126***	0.122***	0.122***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age	0.199***	0.199***	0.199***	0.175***	0.201***	0.201***	0.200***	0.185***	0.202***	0.198***	0.188***	0.201***	0.203***	0.194***	0.203***	0.202***	0.202***	0.198***	0.183***	0.201***	0.199***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age^2	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Female	-0.485***	-0.485***	-0.488***	-0.460***	-0.478***	-0.493***	-0.491***	-0.530***	-0.493***	-0.488***	-0.474***	-0.491***	-0.493***	-0.468***	-0.493***	-0.491***	-0.494***	-0.486***	-0.460***	-0.493***	-0.493***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Rural	-0.187***	-0.188***	-0.182***	-0.196***	-0.195***	-0.185***	-0.188***	-0.194***	-0.188***	-0.195***	-0.230***	-0.187***	-0.186***	-0.241***	-0.186***	-0.188***	-0.186***	-0.186***	-0.202***	-0.180***	-0.200***
	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)
Early arrival	0.893***	0.081	0.159	1.171***	-0.078	0.743***	-0.530	-0.062	0.016	0.619***	-0.054	0.420	0.719	0.050**	0.417	-0.744	0.593	0.169	0.409***	1.468**	-0.613
	(0.302)	(1.179)	(0.980)	(0.022)	(0.589)	(0.280)	(1.825)	(0.414)	(0.396)	(0.353)	(0.163)	(1.603)	(1.275)	(0.379)	(0.513)	(2.135)	(1.141)	(0.536)	(0.138)	(0.834)	(0.784)
Early × educ.	-0.059***	-0.005	-0.007	-0.093***	-0.003*	-0.052***	0.029	0.000	0.001	-0.041***	0.010	-0.023	-0.060	-0.005***	-0.034**	0.064	-0.031	-0.012	-0.024***	-0.121**	0.039
	(0.024)	(0.080)	(0.069)	(0.002)	(0.041)	(0.022)	(0.130)	(0.030)	(0.029)	(0.027)	(0.012)	(0.118)	(0.096)	(0.029)	(0.036)	(0.144)	(0.085)	(0.037)	(0.010)	(0.071)	(0.054)
Late arrival	0.936***	-0.213	0.372	0.867***	0.273	0.667***	0.500	0.037	0.141	0.618**	0.370***	0.663	0.605	0.775**	0.506	0.450	0.893	0.462	0.780***	0.538	-0.123
	(0.150)	(0.595)	(0.507)	(0.017)	(0.317)	(0.154)	(0.767)	(0.217)	(0.370)	(0.178)	(0.089)	(0.881)	(0.678)	(0.222)	(0.314)	(0.889)	(0.557)	(0.331)	(0.067)	(0.527)	(0.527)
Late × educ.	-0.088***	-0.002	-0.045	-0.094***	-0.042	-0.081***	-0.062	-0.027*	-0.022	-0.068***	-0.031***	-0.060	-0.064	-0.072***	-0.048*	-0.043	-0.078	-0.053*	-0.072***	-0.068	0.014
	(0.014)	(0.039)	(0.035)	(0.002)	(0.020)	(0.013)	(0.052)	(0.014)	(0.027)	(0.013)	(0.006)	(0.065)	(0.048)	(0.016)	(0.021)	(0.059)	(0.042)	(0.022)	(0.006)	(0.044)	(0.034)
Foreign res.	-0.464***	-1.456***	-0.180***	-0.553***	-1.949***	-0.576***	-0.948***	-1.082***	-0.913***	-0.658***	-1.595***	-0.069**	-0.954	-1.017***	-0.429**	-1.352***	-0.359***	-1.314***	-0.871***	-1.415***	-0.918***
	(0.023)	(0.018)	(0.018)	(0.009)	(0.022)	(0.040)	(0.017)	(0.010)	(0.048)	(0.022)	(0.013)	(0.033)	(1.487)	(0.012)	(0.172)	(0.036)	(0.044)	(0.019)	(0.012)	(0.026)	(0.016)
Foreign. res. × educ.	-0.087***	-0.017***	-0.058***	-0.021***	-0.065***	-0.035***	-0.031***	-0.068***	-0.019***	-0.043***	0.029***	-0.038***	-0.102	0.012***	-0.045***	-0.003	-0.036***	-0.036***	-0.083***	-0.088***	0.035***
	(0.003)	(0.002)	(0.002)	(0.001)	(0.002)	(0.004)	(0.004)	(0.001)	(0.004)	(0.002)	(0.001)	(0.003)	(0.145)	(0.001)	(0.015)	(0.003)	(0.004)	(0.002)	(0.001)	(0.002)	(0.001)
Constant	1.933***	1.945***	1.940***	2.318***	1.894***	1.898***	1.921***	2.186***	1.882***	1.958***	2.123***	1.898***	1.870***	2.038***	1.871***	1.889***	1.885***	1.957***	2.200***	1.909***	1.943***
	(0.013)	(0.013)	(0.013)	(0.011)	(0.014)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.012)	(0.013)	(0.013)	(0.013)	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Adjusted R^2	0.394	0.442	0.485	0.476	0.459	0.402	0.407	0.577	0.381	0.482	0.497	0.380	0.376	0.465	0.376	0.420	0.380	0.448	0.528	0.465	0.435
N (total)	466367	473901	461527	721542	463008	466491	457949	474006	472114	477360	535153	463021	457639	488980	530470	459559	475661	489750	504097	467427	476991
N US-born	456557	456557	456557	456557	456557	456557	456557	456557	456557	456557	456557	456557	456557	456557	456557	456557	456557	456557	456557	456557	456557
N early arrival	1687	134	349	116807	697	2712	77	1303	2101	2207	13705	77	109	1984	772	71	238	1041	10856	143	531
N late arrival	1748	605	713	130236	3097	3519	251	3646	1772	5514	30869	175	503	2265	1342	290	429	1627	16781	194	1197
N foreign resident	6375	16605	3908	17942	2657	3703	1064	12500	11684	13082	34022	6212	470	28174	71799	2641	18437	30525	19903	10533	18706

Table A1 notes: Standard errors in parentheses. All regressions include dummy variables for the periodicity of wages in the dependent variable (weekly, monthly, etc.) with monthly as the base group. Each regression sample includes only US residents who are US born, US residents born in the sending country of each column, and residents of that sending country. “Early arrival” signifies a person born in the sending country residing in the United States who arrived before age 20. “Late arrival” signifies a person born in the sending country residing in the United States who arrived at or after age 20. “Foreign res.” signifies a person residing in the sending country. US-born US-residents are the base group. Asterisks show statistical significance at * 10%, ** 5%, and *** 1% levels.

Table A2 notes: Standard errors in parentheses. All regressions include dummy variables for the periodicity of wages in the dependent variable (weekly, monthly, etc.) with monthly as the base group. Each regression sample includes only US residents who are US born, US residents born in the sending country of each column, and residents of that sending country. “Early arrival” signifies a person born in the sending country residing in the United States who arrived before age 20. “Late arrival” signifies a person born in the sending country residing in the United States who arrived at or after age 20. “Foreign res.” signifies a person residing in the sending country. US-born US-residents are the base group. Asterisks show statistical significance at * 10%, ** 5%, and *** 1% levels. Education categories are 1) zero (base group), 2) 1-4 years, 3) 5-8 years, 4) 9-12 years, 5) 13-16 years, and 6) 17-28 years. Age categories are 1) 15-19 (base group), 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.

Appendix Table A2: Raw bilateral results, ‘category’ specification (ARG to GHA)

Dependent variable: ln(wages in US$ at PPP

	ARG	BGD	BLZ	BOL	BRA	CHL	CMR	COL	CRI	DOM	ECU	EGY	ETH	GHA
Educ1	0.117**	0.118**	0.118**	0.118**	0.100*	0.117**	0.118**	0.115**	0.117**	0.117**	0.116**	0.113**	0.117**	0.118**
	(0.055)	(0.056)	(0.055)	(0.055)	(0.055)	(0.052)	(0.055)	(0.054)	(0.055)	(0.054)	(0.054)	(0.056)	(0.054)	(0.055)
Educ2	0.122***	0.123***	0.123***	0.122***	0.105***	0.123***	0.123***	0.120***	0.122***	0.122***	0.121***	0.119***	0.122***	0.123***
	(0.029)	(0.030)	(0.030)	(0.030)	(0.030)	(0.028)	(0.030)	(0.029)	(0.029)	(0.029)	(0.029)	(0.030)	(0.029)	(0.030)
Educ3	0.546***	0.544***	0.548***	0.546***	0.527***	0.546***	0.547***	0.542***	0.547***	0.546***	0.545***	0.543***	0.545***	0.548***
	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.025)	(0.027)	(0.026)	(0.027)	(0.026)	(0.026)	(0.027)	(0.026)	(0.027)
Educ4	0.869***	0.870***	0.867***	0.868***	0.890***	0.868***	0.868***	0.871***	0.868***	0.868***	0.869***	0.871***	0.869***	0.867***
	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.025)	(0.027)	(0.026)	(0.027)	(0.026)	(0.026)	(0.027)	(0.026)	(0.027)
Educ5	1.286***	1.290***	1.281***	1.283***	1.322***	1.284***	1.281***	1.289***	1.283***	1.284***	1.286***	1.289***	1.284***	1.281***
	(0.027)	(0.028)	(0.027)	(0.027)	(0.027)	(0.026)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)
Age1	1.057***	1.033***	1.062***	1.052***	0.832***	1.060***	1.060***	1.014***	1.053***	1.052***	1.041***	1.023***	1.039***	1.062***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.005)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)
Age2	1.710***	1.694***	1.740***	1.723***	1.344***	1.720***	1.735***	1.654***	1.725***	1.718***	1.701***	1.656***	1.699***	1.740***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.005)	(0.006)	(0.007)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)
Age3	1.884***	1.859***	1.911***	1.894***	1.518***	1.888***	1.906***	1.822***	1.895***	1.888***	1.868***	1.818***	1.870***	1.911***
	(0.006)	(0.006)	(0.007)	(0.007)	(0.005)	(0.006)	(0.007)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)
Age4	1.975***	1.945***	2.006***	1.990***	1.621***	1.983***	2.002***	1.919***	1.991***	1.984***	1.965***	1.915***	1.968***	2.006***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Age5	2.039***	2.005***	2.068***	2.052***	1.693***	2.045***	2.065***	1.985***	2.052***	2.046***	2.027***	1.986***	2.031***	2.068***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Age6	2.085***	2.042***	2.116***	2.100***	1.752***	2.093***	2.113***	2.035***	2.101***	2.094***	2.075***	2.039***	2.080***	2.116***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)	(0.006)	(0.007)	(0.006)	(0.006)	(0.007)	(0.006)	(0.007)
Age7	2.115***	2.074***	2.145***	2.128***	1.787***	2.124***	2.142***	2.065***	2.130***	2.124***	2.107***	2.074***	2.109***	2.145***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Age8	2.045***	2.006***	2.071***	2.055***	1.736***	2.052***	2.068***	1.994***	2.056***	2.050***	2.032***	2.009***	2.035***	2.071***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Age9	1.789***	1.757***	1.809***	1.794***	1.505***	1.797***	1.806***	1.734***	1.796***	1.790***	1.775***	1.750***	1.775***	1.810***
	(0.008)	(0.008)	(0.009)	(0.009)	(0.008)	(0.008)	(0.009)	(0.008)	(0.009)	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)
Female	-0.486***	-0.493***	-0.487***	-0.485***	-0.487***	-0.479***	-0.486***	-0.477***	-0.486***	-0.486***	-0.483***	-0.475***	-0.483***	-0.487***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Rural	-0.200***	-0.212***	-0.201***	-0.200***	-0.193***	-0.200***	-0.202***	-0.201***	-0.200***	-0.200***	-0.199***	-0.207***	-0.202***	-0.201***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)
Early arrival	0.619	0.482	0.673	0.343	0.510	0.472	-0.580	0.133	0.662	0.223	0.143	-0.759	0.269	0.166
	(1.649)	(2.206)	(3.763)	(2.105)	(0.823)	(1.485)	(1.031)	(0.634)	(1.392)	(0.273)	(0.551)	(2.494)	(1.370)	(2.164)
Early × Educ1	-0.540	0.335	(dropped)	(dropped)	0.977	(dropped)	(dropped)	0.291	-0.590	0.013	0.481	0.454	(dropped)	-0.314
	(3.237)	(4.049)			(1.759)			(1.036)	(2.355)	(0.513)	(0.982)	(3.780)		(3.670)
Early × Educ2	-0.660	-0.166	0.541	0.834	-0.132	-0.323	(dropped)	0.155	-0.034	-0.184	0.338	0.933	-0.448	0.180
	(1.831)	(2.487)	(3.910)	(2.429)	(0.962)	(1.684)		(0.716)	(1.524)	(0.310)	(0.594)	(2.754)	(1.660)	(3.096)
Early × Educ3	-0.562	-0.647	-0.739	-0.284	-0.435	-0.407	0.271	-0.120	-0.456	-0.316	-0.052	0.694	-0.142	-0.142
	(1.657)	(2.219)	(3.769)	(2.117)	(0.831)	(1.497)	(1.240)	(0.638)	(1.402)	(0.277)	(0.557)	(2.505)	(1.388)	(2.180)
Early × Educ4	-0.538	-0.543	-0.708	-0.383	-0.531	-0.476	0.178	-0.143	-0.599	-0.344	-0.155	0.795	-0.375	-0.273
	(1.655)	(2.217)	(3.769)	(2.115)	(0.833)	(1.494)	(1.150)	(0.638)	(1.403)	(0.281)	(0.559)	(2.499)	(1.382)	(2.178)
Early × Educ5	-0.484	-0.477	-0.692	-0.177	-0.618	-0.657	(dropped)	-0.171	-0.661	-0.429	-0.201	0.770	-0.486	-0.375
	(1.668)	(2.266)	(3.808)	(2.157)	(0.871)	(1.523)		(0.660)	(1.449)	(0.353)	(0.626)	(2.509)	(1.431)	(2.224)
Late arrival	0.613	0.184	0.549	0.433	-0.036	0.257	-0.189	0.051	0.208	-0.015	0.041	-0.271	0.142	0.576
	(0.883)	(0.737)	(0.933)	(1.153)	(0.416)	(0.821)	(0.446)	(0.195)	(0.580)	(0.142)	(0.240)	(1.556)	(0.654)	(0.683)
Late × Educ1	-0.609	-0.526	-0.839	-0.394	0.089	-0.148	(dropped)	-0.130	-0.274	-0.278	-0.069	0.750	0.223	-1.124
	(2.922)	(1.201)	(1.405)	(3.825)	(0.654)	(1.420)		(0.392)	(0.957)	(0.247)	(0.456)	(2.392)	(1.874)	(2.696)
Late × Educ2	-0.464	-0.399	-0.588	-0.161	0.180	-0.242	0.184	-0.108	-0.198	-0.086	-0.013	0.574	0.198	-0.372
	(0.931)	(0.839)	(1.064)	(1.249)	(0.469)	(0.917)	(2.792)	(0.229)	(0.632)	(0.159)	(0.262)	(1.730)	(0.854)	(0.976)
Late × Educ3	-0.751	-0.541	-0.641	-0.640	-0.056	-0.389	(dropped)	-0.344*	-0.456	-0.356**	-0.357	0.091	-0.303	-0.703
	(0.890)	(0.751)	(0.948)	(1.164)	(0.423)	(0.831)		(0.200)	(0.594)	(0.148)	(0.248)	(1.564)	(0.664)	(0.694)
Late × Educ4	-0.823	-0.676	-0.803	-0.740	-0.142	-0.507	-0.122	-0.431**	-0.520	-0.496***	-0.480*	0.016	-0.502	-0.803
	(0.890)	(0.746)	(0.954)	(1.165)	(0.423)	(0.831)	(0.503)	(0.203)	(0.602)	(0.157)	(0.254)	(1.558)	(0.664)	(0.692)
Late × Educ5	-0.758	-0.804	-0.629	-1.018	-0.209	-0.568	-0.189	-0.660***	-0.720	-0.750***	-0.774**	0.160	-0.537	-0.879
	(0.894)	(0.751)	(1.212)	(1.198)	(0.437)	(0.845)	(0.550)	(0.219)	(0.675)	(0.202)	(0.305)	(1.561)	(0.701)	(0.708)
Foreign res.	-0.148***	-1.790***	-0.939	-1.847***	-1.600***	-1.433***	-2.381***	-1.440***	-1.025***	-0.904***	-2.113***	-2.377***	-2.358***	-2.086***
	(0.029)	(0.029)	(2.276)	(0.091)	(0.028)	(0.086)	(0.086)	(0.036)	(0.095)	(0.048)	(0.060)	(0.033)	(0.031)	(0.593)
For. Res. × Educ1	-0.785***	0.093	0.072	0.323***	0.104*	0.057	0.008	0.116*	0.238**	0.149**	0.199**	0.322***	0.267***	0.015
	(0.064)	(0.061)	(3.438)	(0.105)	(0.056)	(0.102)	(0.126)	(0.061)	(0.115)	(0.074)	(0.082)	(0.061)	(0.065)	(1.154)
For. Res. × Educ2	-0.569***	0.160***	0.321	0.611***	0.424***	0.253***	0.448***	0.404***	0.480***	0.414***	0.608***	0.146***	0.541***	0.069
	(0.034)	(0.033)	(2.411)	(0.097)	(0.031)	(0.088)	(0.095)	(0.040)	(0.100)	(0.055)	(0.063)	(0.037)	(0.042)	(0.851)
For. Res. × Educ3	-0.743***	-0.160***	0.421	0.258***	0.413***	0.252***	0.354***	0.389***	0.334***	0.222***	0.442***	-0.103***	0.791***	-0.166
	(0.034)	(0.031)	(2.608)	(0.092)	(0.029)	(0.086)	(0.093)	(0.038)	(0.099)	(0.053)	(0.062)	(0.033)	(0.036)	(0.606)
For. Res. × Educ4	-0.679***	-0.007	0.366	0.136	0.628***	0.323***	0.315***	0.403***	0.276***	0.070	0.404***	-0.464***	0.994***	-0.120
	(0.033)	(0.042)	(2.724)	(0.098)	(0.029)	(0.087)	(0.100)	(0.042)	(0.106)	(0.062)	(0.064)	(0.037)	(0.043)	(1.130)
For. Res. × Educ5	-0.699***	-0.164**	0.047	0.150	0.826***	0.480***	0.735***	0.363***	0.226**	0.063	0.182***	-0.680***	1.109***	-0.208
	(0.035)	(0.072)	(8.717)	(0.100)	(0.035)	(0.088)	(0.148)	(0.040)	(0.104)	(0.072)	(0.066)	(0.034)	(0.157)	(1.219)
Constant	5.194***	5.228***	5.171***	5.184***	5.495***	5.184***	5.174***	5.240***	5.184***	5.189***	5.204***	5.236***	5.203***	5.171***
	(0.027)	(0.028)	(0.027)	(0.027)	(0.027)	(0.026)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.028)	(0.027)	(0.027)
Adjusted R^2	0.389	0.744	0.376	0.410	0.571	0.393	0.387	0.424	0.379	0.382	0.410	0.573	0.429	0.377
N (total)	475731	459990	456241	459128	565725	514339	457004	491547	465756	473726	477768	461275	478235	459889
N US-born	454503	454503	454503	454503	454503	454503	454503	454503	454503	454503	454503	454503	454503	454503
N early arrival	1088	412	476	502	1422	675	55	4173	766	6528	2722	708	515	309
N late arrival	1969	1611	624	953	3740	1308	282	8808	1184	9679	5028	2088	1457	1593
N foreign resident	18171	3464	638	3170	106060	57853	2164	24063	9303	3016	15515	3976	21760	3484

Appendix Table A2 (continued): Raw bilateral results, ‘category’ (GTM to NPL)

Dependent variable: ln(wages in US$ at PPP

	GTM	GUY	HTI	IDN	IND	JAM	JOR	KHM	LKA	MAR	MEX	NGA	NIC	NPL
Educ1	0.117**	0.118**	0.118**	0.108**	0.098	0.118**	0.118**	0.117**	0.118**	0.116**	0.108**	0.118**	0.117**	0.117**
	(0.055)	(0.055)	(0.055)	(0.053)	(0.063)	(0.054)	(0.054)	(0.055)	(0.055)	(0.055)	(0.048)	(0.058)	(0.055)	(0.055)
Educ2	0.122***	0.123***	0.123***	0.114***	0.101***	0.123***	0.123***	0.122***	0.123***	0.121***	0.113***	0.124***	0.122***	0.122***
	(0.029)	(0.030)	(0.029)	(0.029)	(0.034)	(0.029)	(0.029)	(0.030)	(0.029)	(0.030)	(0.026)	(0.031)	(0.030)	(0.030)
Educ3	0.545***	0.548***	0.548***	0.527***	0.496***	0.548***	0.547***	0.545***	0.545***	0.544***	0.527***	0.547***	0.546***	0.545***
	(0.027)	(0.027)	(0.027)	(0.026)	(0.031)	(0.026)	(0.027)	(0.027)	(0.027)	(0.027)	(0.023)	(0.028)	(0.027)	(0.027)
Educ4	0.869***	0.867***	0.867***	0.879***	0.900***	0.867***	0.868***	0.868***	0.868***	0.870***	0.878***	0.866***	0.869***	0.869***
	(0.027)	(0.027)	(0.027)	(0.026)	(0.031)	(0.026)	(0.027)	(0.027)	(0.027)	(0.027)	(0.023)	(0.028)	(0.027)	(0.027)
Educ5	1.285***	1.281***	1.281***	1.313***	1.358***	1.281***	1.282***	1.285***	1.286***	1.286***	1.310***	1.279***	1.283***	1.284***
	(0.027)	(0.027)	(0.027)	(0.026)	(0.031)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.024)	(0.028)	(0.027)	(0.027)
Age1	1.037***	1.062***	1.063***	0.888***	0.515***	1.062***	1.062***	1.043***	1.053***	1.026***	0.873***	1.063***	1.048***	1.041***
	(0.006)	(0.007)	(0.007)	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.007)	(0.006)	(0.007)
Age2	1.699***	1.739***	1.740***	1.420***	0.911***	1.740***	1.734***	1.703***	1.711***	1.684***	1.437***	1.737***	1.717***	1.708***
	(0.006)	(0.007)	(0.007)	(0.006)	(0.005)	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.005)	(0.007)	(0.007)	(0.007)
Age3	1.865***	1.910***	1.911***	1.577***	1.045***	1.911***	1.905***	1.871***	1.878***	1.855***	1.604***	1.902***	1.889***	1.879***
	(0.006)	(0.007)	(0.007)	(0.006)	(0.005)	(0.006)	(0.007)	(0.007)	(0.006)	(0.006)	(0.005)	(0.007)	(0.007)	(0.007)
Age4	1.961***	2.006***	2.006***	1.689***	1.148***	2.006***	2.002***	1.966***	1.972***	1.953***	1.695***	2.001***	1.985***	1.974***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.007)	(0.006)	(0.006)
Age5	2.027***	2.068***	2.069***	1.766***	1.229***	2.068***	2.064***	2.028***	2.031***	2.018***	1.767***	2.068***	2.047***	2.036***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.007)	(0.006)	(0.006)
Age6	2.072***	2.115***	2.116***	1.814***	1.272***	2.116***	2.112***	2.076***	2.079***	2.065***	1.812***	2.119***	2.096***	2.083***
	(0.007)	(0.007)	(0.007)	(0.006)	(0.005)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.005)	(0.007)	(0.007)	(0.007)
Age7	2.102***	2.145***	2.145***	1.842***	1.318***	2.145***	2.141***	2.106***	2.105***	2.094***	1.850***	2.140***	2.125***	2.113***
	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.005)	(0.007)	(0.007)	(0.007)
Age8	2.029***	2.070***	2.071***	1.777***	1.286***	2.071***	2.067***	2.032***	2.036***	2.021***	1.782***	2.069***	2.051***	2.039***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.008)	(0.007)	(0.007)
Age9	1.768***	1.809***	1.812***	1.524***	1.002***	1.811***	1.806***	1.773***	1.777***	1.761***	1.536***	1.812***	1.790***	1.779***
	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.007)	(0.009)	(0.009)	(0.009)
Female	-0.483***	-0.487***	-0.486***	-0.447***	-0.473***	-0.486***	-0.487***	-0.479***	-0.479***	-0.482***	-0.455***	-0.473***	-0.486***	-0.485***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)
Rural	-0.206***	-0.201***	-0.203***	-0.203***	-0.316***	-0.201***	-0.199***	-0.202***	-0.203***	-0.196***	-0.209***	-0.210***	-0.200***	-0.202***
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)
Early arrival	0.599***	0.667	0.089	0.167	0.846	0.079	0.341	0.148	0.663	0.587	0.292***	-0.086	0.116	0.323
	(0.167)	(1.205)	(0.540)	(0.270)	(0.797)	(0.589)	(2.440)	(0.411)	(2.070)	(3.128)	(0.041)	(1.166)	(0.547)	(8.455)
Early × Educ1	-0.056	-0.844	-1.264	(dropped)	-1.065	-0.551	(dropped)	0.312	(dropped)	-0.711	-0.127*	(dropped)	-0.604	(dropped)
	(0.271)	(1.947)	(1.130)		(1.852)	(1.691)		(1.037)		(4.064)	(0.073)		(1.274)	
Early × Educ2	-0.178	-0.595	-0.060	1.145	-0.630	0.087	-1.419	0.438	0.322	(dropped)	-0.007	0.330	0.243	0.968
	(0.196)	(1.333)	(0.657)	(3.106)	(1.021)	(0.783)	(2.747)	(0.731)	(2.885)		(0.046)	(2.040)	(0.619)	(9.132)
Early × Educ3	-0.495***	-0.601	-0.164	-0.617*	-1.202	-0.086	-0.483	-0.072	-0.924	-0.470	-0.292***	0.091	-0.096	-0.556
	(0.178)	(1.209)	(0.546)	(0.348)	(0.802)	(0.592)	(2.452)	(0.425)	(2.135)	(3.144)	(0.042)	(1.185)	(0.552)	(8.474)
Early × Educ4	-0.667***	-0.572	-0.103	-0.327	-0.902	0.007	-0.340	-0.222	-0.688	-0.546	-0.438***	-0.058	-0.204	-0.501
	(0.196)	(1.209)	(0.545)	(0.306)	(0.800)	(0.591)	(2.448)	(0.429)	(2.103)	(3.136)	(0.047)	(1.177)	(0.556)	(8.469)
Early × Educ5	-0.740*	-0.560	-0.050	(dropped)	-0.752	-0.049	-0.372	-0.266	-0.505	-0.398	-0.554***	-0.034	-0.193	-0.240
	(0.390)	(1.237)	(0.584)		(0.805)	(0.606)	(2.477)	(0.574)	(2.148)	(3.166)	(0.092)	(1.209)	(0.635)	(8.517)
Late arrival	0.004	0.101	-0.064	0.400	0.208	0.097	0.443	0.146	-1.670	0.153	-0.040	0.550	-0.027	0.455
	(0.105)	(0.328)	(0.169)	(1.384)	(0.292)	(0.291)	(1.912)	(0.172)	(1.545)	(0.867)	(0.033)	(0.953)	(0.264)	(2.183)
Late × Educ1	-0.097	-0.059	-0.155	0.602	-0.362	-0.209	-0.768	-0.057	(dropped)	-0.163	-0.133**	-0.863	-0.212	-1.202
	(0.171)	(0.552)	(0.337)	(3.390)	(0.640)	(0.541)	(2.484)	(0.487)		(3.118)	(0.061)	(2.046)	(0.451)	(2.660)
Late × Educ2	-0.010	-0.087	-0.076	-0.318	-0.228	0.041	-0.199	-0.095	1.179	-0.757	-0.013	-0.632	-0.132	-0.193
	(0.124)	(0.386)	(0.206)	(1.568)	(0.367)	(0.323)	(2.044)	(0.296)	(2.041)	(1.230)	(0.038)	(1.298)	(0.304)	(2.507)
Late × Educ3	-0.267**	-0.181	-0.228	-0.541	-0.325	-0.161	-0.640	-0.277	1.462	-0.445	-0.264***	-0.873	-0.300	-0.807
	(0.118)	(0.335)	(0.175)	(1.397)	(0.299)	(0.294)	(1.924)	(0.207)	(1.562)	(0.891)	(0.035)	(0.963)	(0.275)	(2.210)
Late × Educ4	-0.435***	-0.067	-0.185	-0.591	-0.201	-0.080	-0.838	-0.359	1.537	-0.420	-0.417***	-0.792	-0.391	-0.964
	(0.142)	(0.340)	(0.182)	(1.390)	(0.294)	(0.296)	(1.920)	(0.235)	(1.558)	(0.881)	(0.042)	(0.956)	(0.282)	(2.203)
Late × Educ5	-0.665**	-0.150	-0.220	-0.706	-0.170	-0.133	-0.641	-0.429	1.476	-0.410	-0.712***	-0.909	-0.710**	-0.933
	(0.266)	(0.383)	(0.241)	(1.399)	(0.294)	(0.318)	(1.938)	(0.530)	(1.562)	(0.920)	(0.065)	(0.959)	(0.336)	(2.216)
Foreign res.	-1.115***	-0.323	-2.804***	-2.479***	-1.940***	-1.199	-1.725***	-1.546***	-2.122***	-1.037***	-1.515***	-2.792***	-1.453***	-1.901***
	(0.034)	(1.746)	(0.072)	(0.031)	(0.031)	(2.128)	(0.126)	(0.042)	(0.040)	(0.032)	(0.026)	(0.041)	(0.055)	(0.031)
For. Res. × Educ1	-0.062	-0.617	-0.257*	0.190***	0.134**	0.420	-0.092	0.268***	-0.043	0.114*	0.138***	0.514***	0.180**	0.509***
	(0.062)	(1.787)	(0.147)	(0.057)	(0.063)	(2.466)	(0.177)	(0.069)	(0.065)	(0.061)	(0.050)	(0.094)	(0.075)	(0.067)
For. Res. × Educ2	0.202***	-0.612	0.043	0.542***	0.287***	0.044	0.197	0.487***	0.249***	0.207***	0.567***	0.228***	0.484***	0.653***
	(0.042)	(1.753)	(0.110)	(0.034)	(0.034)	(2.223)	(0.134)	(0.049)	(0.045)	(0.035)	(0.029)	(0.047)	(0.057)	(0.046)
For. Res. × Educ3	0.113***	-0.836	0.106	0.576***	0.172***	0.046	-0.045	-0.136***	0.310***	0.053	0.525***	-0.140***	0.243***	0.024
	(0.043)	(1.750)	(0.097)	(0.031)	(0.031)	(2.121)	(0.128)	(0.050)	(0.042)	(0.039)	(0.026)	(0.043)	(0.059)	(0.065)
For. Res. × Educ4	0.059	-0.804	0.429*	0.610***	0.162***	0.307	-0.093	-0.602***	0.552***	0.071	0.590***	0.015	0.403***	-0.704**
	(0.075)	(1.799)	(0.231)	(0.032)	(0.032)	(2.239)	(0.130)	(0.116)	(0.058)	(0.044)	(0.028)	(0.042)	(0.070)	(0.348)
For. Res. × Educ5	-0.152**	(dropped)	-0.583	0.422***	0.109***	0.291	-0.140	-0.627***	0.161*	0.152	0.431***	-0.314***	1.255***	(dropped)
	(0.059)		(0.423)	(0.045)	(0.032)	(2.355)	(0.149)	(0.088)	(0.092)	(0.098)	(0.027)	(0.052)	(0.241)	
Constant	5.208***	5.171***	5.171***	5.426***	5.932***	5.170***	5.174***	5.202***	5.197***	5.215***	5.426***	5.169***	5.189***	5.199***
	(0.027)	(0.027)	(0.027)	(0.026)	(0.031)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.024)	(0.028)	(0.027)	(0.027)
Adjusted R^2	0.396	0.377	0.381	0.614	0.725	0.376	0.385	0.394	0.442	0.485	0.470	0.460	0.402	0.407
N (total)	470657	461272	466569	565564	564435	472060	466559	464296	471740	459465	718994	460931	464411	455887
N US-born	454503	454503	454503	454503	454503	454503	454503	454503	454503	454503	454503	454503	454503	454503
N early arrival	5115	1988	3522	717	4897	6099	465	1687	134	349	116752	697	2712	77
N late arrival	8465	3701	8026	1048	22593	9375	604	1743	603	709	129862	3091	3506	251
N foreign resident	2574	1080	518	109296	82442	2083	10987	6363	16500	3904	17877	2640	3690	1056

Appendix Table A2 (continued): Raw bilateral results, ‘category’ (PAK to ZAF)

Dependent variable: ln(wages in US$ at PPP

	PAK	PAN	PER	PHL	PRY	SLE	THA	TUR	UGA	URY	VEN	VNM	YEM	ZAF
Educ1	0.111**	0.118**	0.116**	0.117**	0.117**	0.118**	0.118**	0.118**	0.118**	0.118**	0.116**	0.112**	0.116**	0.118**
	(0.056)	(0.054)	(0.055)	(0.054)	(0.055)	(0.055)	(0.056)	(0.051)	(0.055)	(0.054)	(0.054)	(0.055)	(0.055)	(0.055)
Educ2	0.114***	0.123***	0.121***	0.123***	0.122***	0.123***	0.124***	0.123***	0.123***	0.123***	0.122***	0.117***	0.121***	0.123***
	(0.030)	(0.029)	(0.030)	(0.029)	(0.030)	(0.030)	(0.030)	(0.028)	(0.030)	(0.029)	(0.029)	(0.030)	(0.030)	(0.030)
Educ3	0.537***	0.547***	0.544***	0.538***	0.546***	0.548***	0.542***	0.548***	0.547***	0.547***	0.544***	0.532***	0.546***	0.547***
	(0.027)	(0.026)	(0.027)	(0.026)	(0.027)	(0.027)	(0.027)	(0.025)	(0.027)	(0.026)	(0.026)	(0.027)	(0.027)	(0.027)
Educ4	0.882***	0.868***	0.870***	0.870***	0.868***	0.867***	0.866***	0.867***	0.868***	0.868***	0.869***	0.872***	0.869***	0.869***
	(0.027)	(0.026)	(0.027)	(0.026)	(0.027)	(0.027)	(0.027)	(0.025)	(0.027)	(0.026)	(0.026)	(0.027)	(0.027)	(0.027)
Educ5	1.305***	1.281***	1.286***	1.294***	1.283***	1.281***	1.281***	1.281***	1.282***	1.282***	1.287***	1.300***	1.284***	1.283***
	(0.028)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.025)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)
Age1	0.903***	1.060***	1.030***	0.995***	1.049***	1.062***	1.020***	1.062***	1.053***	1.058***	1.037***	0.935***	1.043***	1.062***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)
Age2	1.520***	1.734***	1.681***	1.605***	1.720***	1.740***	1.652***	1.739***	1.723***	1.732***	1.689***	1.545***	1.709***	1.709***
	(0.006)	(0.007)	(0.006)	(0.006)	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)
Age3	1.688***	1.905***	1.855***	1.771***	1.890***	1.911***	1.820***	1.910***	1.895***	1.903***	1.858***	1.702***	1.882***	1.884***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)
Age4	1.786***	2.000***	1.949***	1.863***	1.986***	2.006***	1.926***	2.005***	1.993***	1.998***	1.955***	1.793***	1.978***	1.981***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Age5	1.851***	2.062***	2.013***	1.927***	2.048***	2.068***	2.000***	2.067***	2.054***	2.060***	2.017***	1.855***	2.041***	2.050***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Age6	1.899***	2.111***	2.061***	1.971***	2.096***	2.116***	2.049***	2.115***	2.102***	2.108***	2.065***	1.901***	2.089***	2.097***
	(0.006)	(0.007)	(0.006)	(0.006)	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)
Age7	1.924***	2.140***	2.092***	2.000***	2.124***	2.145***	2.076***	2.145***	2.132***	2.138***	2.095***	1.940***	2.119***	2.127***
	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.007)
Age8	1.861***	2.066***	2.019***	1.933***	2.051***	2.071***	2.009***	2.070***	2.058***	2.064***	2.023***	1.872***	2.045***	2.054***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Age9	1.609***	1.805***	1.764***	1.690***	1.791***	1.809***	1.740***	1.809***	1.796***	1.804***	1.766***	1.615***	1.785***	1.800***
	(0.008)	(0.009)	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)	(0.008)	(0.009)	(0.009)	(0.008)	(0.008)	(0.009)	(0.009)
Female	-0.522***	-0.486***	-0.483***	-0.467***	-0.485***	-0.487***	-0.462***	-0.487***	-0.485***	-0.488***	-0.479***	-0.453***	-0.487***	-0.487***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Rural	-0.209***	-0.202***	-0.210***	-0.242***	-0.201***	-0.201***	-0.253***	-0.201***	-0.203***	-0.201***	-0.200***	-0.215***	-0.195***	-0.219***
	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)
Early arrival	0.986	1.564	0.363	0.142	0.145	1.390	0.255	0.844	-0.273	0.796	1.703	0.363	0.760	1.381
	(0.900)	(3.491)	(0.843)	(0.554)	(1.129)	(2.084)	(1.033)	(1.280)	(0.668)	(3.977)	(2.227)	(0.240)	(1.127)	(2.559)
Early × Educ1	-1.313	-1.773	-0.037	-0.214	(dropped)	(dropped)	0.055	0.587	(dropped)	(dropped)	-2.295	-0.026	-2.021	(dropped)
	(3.956)	(4.938)	(1.990)	(1.077)			(1.725)	(2.168)			(5.250)	(0.623)	(3.061)	
Early × Educ2	-0.681	-1.471	-0.084	-0.060	(dropped)	-2.395	0.171	-0.281	(dropped)	-1.241	-0.811	-0.199	0.037	(dropped)
	(1.173)	(3.571)	(0.988)	(0.624)		(2.994)	(1.196)	(1.653)		(4.230)	(2.585)	(0.345)	(1.410)	
Early × Educ3	-1.133	-1.502	-0.233	-0.098	0.030	-1.481	-0.200	-0.830	(dropped)	-0.479	-1.614	-0.370	-0.713	-1.355
	(0.911)	(3.493)	(0.849)	(0.556)	(1.325)	(2.137)	(1.037)	(1.292)		(3.987)	(2.234)	(0.246)	(1.182)	(2.572)
Early × Educ4	-1.122	-1.576	-0.421	-0.105	-0.074	-1.498	-0.354	-0.972	0.806	-0.792	-1.737	-0.365	-1.275	-1.495
	(0.908)	(3.493)	(0.849)	(0.555)	(1.231)	(2.122)	(1.039)	(1.289)	(0.815)	(3.988)	(2.231)	(0.243)	(1.227)	(2.566)
Early × Educ5	-0.829	-1.482	-0.385	-0.097	-0.361	-1.897	-0.321	-0.884	0.268	-0.705	-1.627	-0.296	-0.770	-1.359
	(0.934)	(3.498)	(0.885)	(0.567)	(1.574)	(2.363)	(1.073)	(1.313)	(1.134)	(4.075)	(2.242)	(0.264)	(2.029)	(2.582)
Late arrival	-0.240	0.336	-0.183	0.201	-0.004	-0.481	0.181	-0.047	0.718	0.255	0.357	0.063	-0.556	-0.815
	(0.440)	(0.989)	(0.346)	(0.232)	(0.757)	(1.621)	(0.437)	(0.918)	(2.405)	(1.479)	(0.780)	(0.093)	(0.632)	(5.557)
Late × Educ1	0.060	-0.053	0.095	-0.187	0.455	1.184	-0.177	0.446	-0.961	-0.225	-0.259	-0.170	(dropped)	(dropped)
	(1.062)	(1.819)	(0.748)	(0.350)	(2.479)	(3.507)	(0.586)	(1.345)	(3.401)	(3.279)	(1.886)	(0.217)		
Late × Educ2	-0.032	-0.574	0.175	-0.120	(dropped)	0.763	-0.125	0.135	-0.230	-0.036	-0.457	-0.048	0.680	0.791
	(0.546)	(1.133)	(0.418)	(0.255)		(2.059)	(0.521)	(0.984)	(2.704)	(1.580)	(0.913)	(0.125)	(0.991)	(5.644)
Late × Educ3	-0.093	-0.506	-0.061	-0.356	0.040	0.288	-0.350	-0.165	-0.891	-0.298	-0.620	-0.277***	0.290	0.775
	(0.451)	(0.995)	(0.352)	(0.235)	(0.843)	(1.635)	(0.454)	(0.932)	(2.437)	(1.493)	(0.793)	(0.099)	(0.702)	(5.561)
Late × Educ4	-0.123	-0.547	-0.162	-0.187	-0.423	0.253	-0.396	-0.143	-0.891	-0.589	-0.691	-0.228**	0.106	1.004
	(0.447)	(0.995)	(0.353)	(0.233)	(0.845)	(1.633)	(0.448)	(0.927)	(2.417)	(1.503)	(0.787)	(0.102)	(0.748)	(5.558)
Late × Educ5	-0.002	-0.435	-0.324	-0.274	-0.168	-0.069	-0.680	-0.272	-0.842	-0.587	-0.758	-0.272*	-0.382	1.008
	(0.451)	(1.015)	(0.371)	(0.240)	(0.961)	(1.659)	(0.464)	(0.928)	(2.437)	(1.536)	(0.796)	(0.158)	(1.094)	(5.559)
Foreign res.	-2.035***	-1.167***	-1.261***	-2.291***	-1.309***	-1.835	-1.799***	-0.709**	-2.035***	-1.209***	-2.151***	-1.962***	-2.384***	-1.397***
	(0.028)	(0.045)	(0.035)	(0.050)	(0.096)	(1.778)	(0.036)	(0.311)	(0.055)	(0.207)	(0.045)	(0.034)	(0.039)	(0.034)
For. Res. × Educ1	0.236***	-0.427***	-0.547***	0.266***	0.473***	0.457	0.132**	(dropped)	0.055	-0.020	0.061	0.147**	0.230***	0.085
	(0.059)	(0.106)	(0.068)	(0.070)	(0.114)	(4.747)	(0.061)		(0.082)	(0.221)	(0.069)	(0.060)	(0.075)	(0.062)
For. Res. × Educ2	0.281***	-0.068	0.016	0.626***	0.890***	-0.059	0.685***	-0.160	0.338***	0.405*	0.345***	0.439***	0.307***	0.343***
	(0.032)	(0.058)	(0.039)	(0.052)	(0.100)	(2.421)	(0.039)	(0.322)	(0.060)	(0.209)	(0.048)	(0.037)	(0.045)	(0.037)
For. Res. × Educ3	0.057*	-0.082	-0.116***	0.753***	0.653***	-0.389	0.767***	-0.274	0.321***	0.367*	0.306***	0.036	-0.068	0.587***
	(0.029)	(0.053)	(0.035)	(0.051)	(0.099)	(1.960)	(0.037)	(0.322)	(0.060)	(0.208)	(0.046)	(0.035)	(0.042)	(0.034)
For. Res. × Educ4	-0.053	-0.099	0.047	1.179***	0.616***	(dropped)	0.976***	-0.255	0.758***	0.159	0.193***	-0.016	-0.444***	1.248***
	(0.033)	(0.062)	(0.038)	(0.051)	(0.103)		(0.037)	(0.334)	(0.064)	(0.212)	(0.068)	(0.042)	(0.045)	(0.037)
For. Res. × Educ5	-0.209***	(dropped)	0.642***	1.251***	0.588***	-0.906	1.193***	-0.387	(dropped)	0.119	-0.015	-0.450***	-0.731***	1.235***
	(0.035)		(0.087)	(0.074)	(0.116)	(3.039)	(0.048)	(0.524)		(0.222)	(0.058)	(0.037)	(0.066)	(0.051)
Constant	5.382***	5.175***	5.219***	5.292***	5.188***	5.171***	5.237***	5.171***	5.183***	5.178***	5.211***	5.343***	5.194***	5.191***
	(0.028)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.028)	(0.025)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)
Adjusted R^2	0.574	0.381	0.483	0.495	0.380	0.376	0.464	0.377	0.420	0.380	0.447	0.526	0.465	0.436
N (total)	471846	470010	475229	532727	460959	455581	486832	528367	457497	473507	487625	501954	465329	474877
N US-born	454503	454503	454503	454503	454503	454503	454503	454503	454503	454503	454503	454503	454503	454503
N early arrival	1303	2097	2207	13696	77	109	1984	772	71	238	1041	10856	143	531
N late arrival	3628	1755	5472	30630	175	503	2256	1332	290	424	1623	16712	194	1188
N foreign resident	12412	11655	13047	33898	6204	466	28089	71760	2633	18342	30458	19883	10489	18655

APPENDIX: DATA

A1 Sources

Survey data on wages and other worker characteristics from 42 developing countries were compiled by Indermit S. Gill and his team at the World Bank. Appendix Table A3 lists the original sources and size of each sample, as well as reproducing the exact text of the wage question from each survey. A detailed description of the database can be found in Montenegro and Hirn (2008).

In three surveys (India, Turkey and the US), the respondent's education level is listed as achievement categories rather than as years of schooling. We translate these categories into years of schooling according to the information available in the surveys. In the particular case of the US we use the following concordance: 0 years if "less than 1st grade"; 3 years if "1st through 4th grade"; 5.6666 years if "5th or 6th"; 7.6666 years if "7th or 8th"; 9, 10, 11, or 12 have separate categories; 12 years if "high school equivalent"; 13.5 years if "some college but no degree"; 14 years if "associate degree" or equivalent; 16 years if "bachelor's degree"; 18 years if "master's degree"; 19 years if "professional degree"; 20 years if "doctoral degree". This is a compromise blend of the methods used in Bratsberg and Terrell (2002) and in Jaeger (1997).

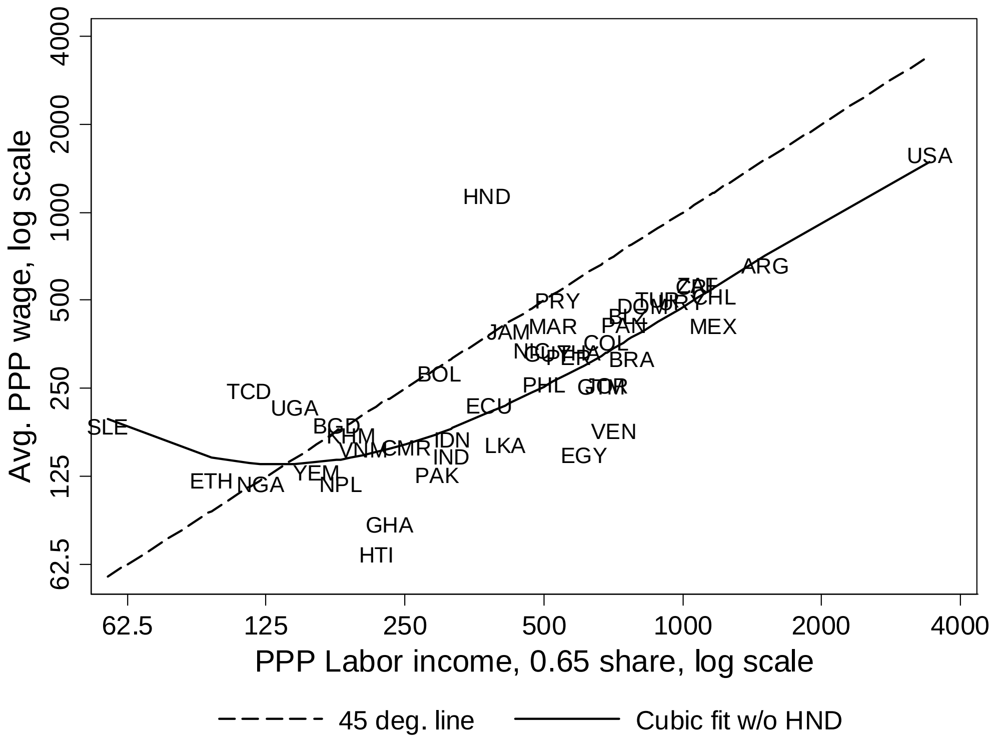
All data except the cost of living index used in the wage regressions for US states and Puerto Rico come from the United States Public Use Microdata Sample (5%) of the 2000 census. US state and Puerto Rico cost of living index comes from the revised 2004 version of the Berry-Fording-Hanson (BFH) state cost of living index (described in Berry, Fording, and Hanson (2000)), which excludes Alaska, Hawaii, and the District of Columbia. In the BFH index for 1999, 1 is the purchasing power of $1 in the median US state. For Puerto Rico we use 0.86, which is the PPP conversion factor for 1999 from the Penn World Table 6.2 (Heston, Summers, and Aten (2006)).

A2 Quality

An important question we do not take up in the text is the degree to which the surveys are representative of the wage sector and of the country—though all were designed to be. One way to check the representativeness of the wage surveys is to compare national accounts estimates of labor income per worker at PPP in each country to the average wage from the surveys we use. There is no reason to expect these to be equal—most importantly because the wage data we use do not include self-employed people and therefore do not include large portions of the informal sector, and even informal-sector wage workers can be harder to sample than formal-sector workers. But enormous differences between the two could signal problems in the representativeness of the survey data.

Appendix Figure 1 plots this comparison. Labor income per worker is calculated by the method of Gollin (2002), under the assumption that a 0.65 share of GDP accrues to labor. The dotted line shows a 45-degree line and the solid line shows a cubic least-squares fit to the data including a dummy for Honduras (R^2 = 0.756; or R^2 = 0.607 without the Honduras dummy). Large amounts of self-employment would tend to push countries down and to the right; large amounts of low-wage informal sector work would tend to push countries up and to the left.

Appendix Figure A1: Comparison of labor income per worker and survey-based wages



Line shows cubic regression fit of ln(wage) on ln(labor income), its square, and its cube, with a dummy for Honduras.

We draw three lessons from Figure 1. First, the agreement is in general very good. Average wage is typically some reasonable fraction of average labor income, varying across countries as would be expected given different relative sizes of the informal sector and the self-employed sector. Second, formal-sector wages are clearly not representative of typical worker earnings in the poorest countries with very large informal sectors (e.g. Sierra Leone, Chad, Nigeria, Bangladesh, Ethiopia, Uganda). This is to be expected given that earnings gaps between the formal and informal sector are highest in the poorest countries (Vollrath (Forthcoming)). Third, the Honduras survey appears anomalous and we drop Honduras from subsequent reported analysis (since our preferred results are from bi-lateral regressions this has no consequence for other countries' results). Overall, this analysis highlights the fact that all of the estimates to follow can only be interpreted as applying to a worker moving across the formal wage-labor sector. If indeed the formal-informal gap is much larger in poor countries, this *underestimates* the wage gains for a typical worker.

Appendix Table A3: Household survey data sources

Country	Year	Survey	Survey agency	Sample	Wage question
Argentina	2001	Encuesta Permanente de Hogares	Instituto Nacional de Estadísticas y Censos (INDEC)	19,706	Cuanto cobró por ese mes por esos conceptos? (Monto total de sueldos/jornales, salario familiar, horas extras, otras bonificaciones habituales y tickets vales o similares)
Bangladesh	2000	Household Income Expenditure Survey	Bureau of Statistics	3,517	What is your total net take-home monthly cash remuneration after all deductions at source?
Belize	1995	Survey of Living Conditions	Central Statistical Office	783	What is your gross monthly income?
Bolivia	2002	Encuesta de Hogares	Instituto Nacional de Estadísticas	3,244	Cuál es el salario líquido de su trabajo en horario normal?
Brazil	2005	Pesquisa Nacional per Amostra de Domicílios	Instituto Brasileiro de Geografia e Estatística	107,955	Qual era o rendimento mensual que você ganhava normalmente em setembro de 2003, nesse trabalho?
Cambodia	2004	Household Socio-Economic Survey	National Institute of Statistics	8,578	How much did you earn in salary/wages from this activity last month?
Cameroon	2001	Enquête Camerounaise Auprès de Ménages	Direction de la Statistique et de la Comptabilité Nationale	5,098	A quel montant estimez vous la totalité des revenues issus de votre emploi principal le mois dernier?
Chile	2003	Encuesta de Caracterización Socio-económica Nacional	Ministerio de Planificación	59,532	En el mes pasado, cuál fue su ingreso o remuneración líquida en su ocupación principal?
Colombia	2000	Encuesta Continua de Hogares	Departamento Administrativo Nacional de Estadística	27,996	Cuanto ganó el mes pasado en este empleo? (incluya propinas y comisiones y excluya viáticos y pagos en especie)
Costa Rica	2001	Encuesta de Hogares de Propósitos Multiples	Instituto Nacional de Estadísticas y Censos	12,503	En su ocupación principal, cuál fue el ingreso efectivamente percibido por concepto de sueldo, salario, jornal, propinas, horas extras, en el último periodo de pago (semana, quincena o mes)?
Dominican Republic	1997	Encuesta Nacional de Fuerza de Trabajo	Departamento de Cuentas Nacionales y Estadísticas Económicas del Banco Central	3,056	Cuánto le pagan o gana usted y cada qué tiempo en ese trabajo?
Ecuador	2004	Encuesta de Empleo, Desempleo y Subempleo	Instituto Nacional de Estadísticas y Censos	17,576	En su ocupación cuánto dinero líquido recibió por concepto de sueldo o salario u otros ingresos en el mes de marzo?
Egypt	1998	Labor Market Survey	Central Agency for Public Mobilization and Statistics	4,776	What is the net amount received in basic net wage?
Ethiopia	2005	National Labour Force	Central Statistical Authority	22,568	What was the amount paid in your main occupation during the last period?
Ghana	1991	Living Standards Surveys Round Three	Statistical Office	5,749	What is the amount of money you will receive for this work?
Guatemala	2002	Encuesta Nacional Sobre Condiciones de Vida	Instituto Nacional de Estadísticas	2,584	Cuál es el último ingreso neto o ganancia que recibió?
Guyana	1992	Living Standards Measurement Survey	Bureau of Statistics	1,266	What is your cash income from paid employment (BASIC wage or salary)?
Haiti	2001	Les Conditions de Vie en Haïti	Institut Haïtien de Statistique et d'Informatique	1,220	What is your wage, salary, commission payments, bonuses or other cash income (includying overtime) from employer?
India	1999	Socio-economic Survey	National Sample Survey Organization	94,306	What are the wage and salary earnings (received or receivable) for the work done during the week?
Indonesia	2002	Survei Sosial Ekonomi Nasional	Badan Pusat Statistik	129,279	How much is the wage/net salary received in a month of main work?
Jamaica	2002	Jamaica Survey of Living Conditions	Statistical Institute of Jamaica	3,723	What is the value of all income received in cash or in kind during the past 12 months?
Jordan	2002	Household Income Expenditure Survey	Household Surveys Directorate	12,824	What is the total income from employment?
Mexico	2002	Encuesta Nacional de Ingresos y Gastos de los Hogares	Instituto Nacional de Estadística, Geografía e Informática	18,064	Cuánto recibió el mes pasado por sueldos, salarios y jornales en el mes pasado? (declare su ingreso bruto)
Morocco	1998	Enquête Nationale sur les Niveaux de Vie des Ménages	Secretariat d'État à la Population, Direction de la Statistique	4,043	Quel a été votre salaire en espèce dans votre travail ?
Nepal	2003	Living Standards Survey II	Central Bureau of Statistics	2,216	How much did you get in cash per day for this job?

Appendix Table A3, continued: Household survey data sources

Nicaragua	2001	Encuesta Nacional de Hogares Sobre Medición de Nivel de Vida	Instituto Nacional de Estadísticas y Censos	3,757	Cuál es el último ingreso neto que tuvo usted?
Nigeria	2003	Living Standards Surveys	Federal Office of Statistics	3,084	What is the amount of money you received or you will receive for this work?
Pakistan	2001	Integrated Household Survey	Federal Bureau of Statistics	13,186	How much is your take-home pay, including bonuses or cash allowances?
Panama	2003	Encuesta de Hogares	Dirección de Estadística y Censo	14,392	Cuál es salario o ingreso mensual en su trabajo? (si es empleado investigue sueldos y salarios brutos—sin deducir impuestos ni contribuciones al seguro social)
Paraguay	2001	Encuesta Permanente de Hogares	Dirección General de Estadísticas, Encuestas y Censos	6,254	Cuál fue el monto del último pago neto o líquido que recibió (incluyen descuentos por préstamos, asociaciones, etc.)? Si no le han pagado todavía, cuánto espera que le paguen y que periodo de tiempo incluye este pago?
Peru	2002	Encuesta Nacional de Hogares	Instituto Nacional de Estadísticas e Informática	13,367	Cuál fue ingreso total en el pago anterior incluyendo horas extras, bonificaciones, pago por concepto de refrigerio, mobilidad, comisiones, etc.?
Philippines	2002	Annual Poverty Indicators Survey	National Statistics Office	34,626	Total Income, salary/wages from employment
Sierra Leone	2003	Integrated Household Survey	Statistics Sierra Leone	565	What is the amount of money you received or you will receive?
South Africa	2000	Labour Force Survey	Statistics South Africa	21,707	What is your total salary/pay in your main job?
Sri Lanka	2002	Household Income and Expenditure Survey	Department of Census and Statistics	16,772	What is the wage/salary you received last calendar month?
Thailand	2002	Socio-economic Survey	National Statistical Office	28,258	Wage and salaries
Turkey	2005	Household Labour Force Survey	State Institute of Statistics	75,610	How much did you earn from main job activity during the last month?
Uganda	2002	Socio-economic Survey	Uganda Bureau of Statistics	3,204	How much do you earn per period? (effort should be taken to get the net salary after the deduction of taxes)
United States	1999	2000 Census Population and Housing (Public Use Microdata Sample)	US Census Bureau	1,124,253	Wages, salary, commissions, bonuses, or tips from all jobs. Report amount before deductions for taxes, bonds, dues or other items
Uruguay	1995	Encuesta Continua de Hogares	Instituto Nacional de Estadísticas	19,142	Cuánto ganó el mes pasado como empleado u obrero del sector público o privado?
Venezuela	2004	Encuesta de Hogares por Muestreo Nacional	Instituto Nacional de Estadísticas	34,569	En un mes normal cuánto es su ganancia neta?
Vietnam	2002	Household Living Standard Survey	General Statistical Office	19,920	In the past 12 months, how much did you receive from this work in money and in kind?
Yemen	2005	Household Budget Survey	Central Statistical Organization	10,583	How much was your last pay? (net of taxes and any other deduction)

Reported sample size corresponds the number of people in each survey who are wage earners and of age 15-65. Of the US sample, 500,319 were born in the US.