The Economics of International Wage Differentials and Migration

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Summary

The key question for the economics of international migration is whether observed real wage differentials across countries for workers with identical intrinsic productivity represent an economic inefficiency sustained by legal barriers to labor mobility between geographies. A simple comparison of the real wages of workers with the same level of formal schooling or performing similar occupations across countries shows massive gaps between rich and poorer countries. These gaps persist after adjusting for observed and unobserved human capital characteristics, suggesting a “place premium”—or space-specific wage differentials that are not due to intrinsic worker productivity but rather are due to a misallocation of labor. If wage gaps are not due to intrinsic worker productivity, then the incentive for workers to move to richer countries is high. The idea of a place premium is corroborated by macroeconomic evidence. National accounts data show large cross-country output per worker differences, driven by the divergence of total factor productivity. The lack of convergence in total factor productivity and corresponding spatial productivity differentials create differences in the marginal product of factors, and hence persistent gaps in the wages of equal productivity workers. These differentials can equalize with factor flows; however their persistence and large magnitude in the case of labor, suggest legal barriers to migration restricting labor flows are in fact constraining significant return on human capital, and leaving billions in unrealized gains to the world’s workers and global economy. A relaxation of these barriers would generate worker welfare gains that dwarf gold-standard poverty reduction programs.

Keywords: international migration, mobility, labor, wages, productivity

Subjects: Economic Development, International Economics, Labor and Demographic Economics

Cross-National Wage Differentials

Perhaps the single most obvious and striking fact about the global economy is the cross-national difference in wages of equivalent workers. The key question for the economics of international migration is to what extent these observed differences in wages represent potential gains from the movement of labor. Are there really “trillion-dollar” bills on the sidewalk (Clemens, 2011)? Differences in average wages may not reflect any incentives for migration/mobility if lower wages are due to the lower intrinsic productivity (“human capital”) of workers. However, five independent strands of evidence suggest massive consumption
wage differences across countries for workers of identical intrinsic individual productivity due to a “place premium” in high productivity places, and hence large pressures for international labor mobility which are prevented by border-based barriers to labor mobility in high wage countries.

First, the consumption wages in purchasing power parity (PPP)\(^1\) of workers with the same level of formal schooling, for example, secondary schooling complete, differ by a factor of 10 between the average for low wage (bottom 30%) and high wage [Organisation for Economic Co-operation and Development (OECD)] countries. There are even larger gaps across individual countries: wages of workers with secondary schooling in Netherlands are 25 times higher than those in Ethiopia. Even adjusting schooling for differences in learning leaves massive wage gaps between equal “schooling capital” workers across countries (see subsection “Wages by Level of Schooling”). Second, wages of workers in the same narrow, low- to medium-skill occupations, like waiters or truck drivers or construction workers, differ by a factor of 5 to 10 between low wage countries (bottom 30%) and the OECD (see subsection “Occupational Wage Gaps”). Third, even adjusting econometrically for both observable and unobservable worker characteristics, the ratio of wages in the United States to the wages of low-/medium-skill workers from the other 42 countries, “adjusted to equal individual productivity,” ranges from a low of 2 to over 10 (see subsection “Wage Gap of Observationally Equivalent Workers to Lower Bound on Equal Productivity”). Particularly for low- to medium-skill workers (not perhaps for mobility of “global superstars”), most studies show adjusting for selectivity produces a modest-sized adjustment in moving from wage differences of “observational equivalent” to “equal productivity” workers (Ambrosini & Peri, 2012; Bertoli, Fernández-Huertas Moraga, & Ortega, 2011; Collins & Wanamaker, 2014; McKenzie, Stillman, & Gibson, 2010). Triangulation from numerous methods suggests selectivity accounts for between 0 and 25% of the observed earnings difference between immigrants and their observed “counterfactual” at home. This implies accounting for migrant selection would, at most, reduce an “observational equivalent”-adjusted wage ratio of 5 to 1 to 4 to 1 for “equal productivity” workers (see subsection “Selection Bias and the LATE of Place”). Fourth, both survey data and behavior reveal massive excess demand for mobility, consistent with much higher available incomes for movers (see subsection “Are There Unrealized Gains from International Mobility? Migration Desires and Action”). Importantly, these four sources of microeconomic evidence about wage gaps are consistent with the fifth element: national accounts data also show large cross-national differences in wages, adjusted for schooling capital (see subsection “Aggregate Theories of the Level and Growth of Output and Labor Mobility”).

There is a relatively straightforward reconciliation of the existing economy-wide models of aggregate income/growth and the microeconomic evidence about observed wage differentials and labor mobility. Across countries there are both: (a) a long-term persistence in aggregate productivity levels with, at best, weak pressures for absolute cross-national convergence; and (b) spatial shocks to labor productivity over time (e.g., changes in world demand for resources). These create spatial differences in the marginal product of factors, even across exactly equal intrinsic productivity workers, and hence, without mobility, lead to persistent
large differences in the wages of workers with exactly the same intrinsic productivity. Legally enforced barriers to the mobility of labor enforced by rich countries prevent these large differences in productivity from creating labor flows. If this is correct then there are “trillion-dollar bills” due to this enforced economic inefficiency and, at the margin, the relaxation of the binding constraints on labor mobility are the highest return to human well-being actions available in the world—with gains orders of magnitude larger than other types of policy reform or “anti-poverty” projects that attempt to raise productivity in situ (Pritchett, 2018) (see subsection “Gains from Relaxing Barriers to Labor Mobility”).

This review of the economics of migration pays relatively little attention to the voluminous literature on the impact on wages of receiving countries as: (i) there are massive 21st-century reviews of this literature (National Academies of Sciences, 2017); (ii) this literature shows convincingly that the gains to average incomes of native-born workers in the United States (the most widely studied country) are small and positive and the only uncertainty is about wage impacts among small populations of workers (National Academies of Sciences, 2017); (iii) there are good reasons to believe restrictions in rich countries are mainly political, particularly about control of the ethnic composition of the population, and are not primarily based on narrow economic criteria; and (iv) general equilibrium estimates of modest-sized incremental movements of labor from poor to rich countries show that the gains/losses for non-movers in receiving countries are orders of magnitude smaller than the gains to movers (e.g., Walmsley & Winters, 2005).

**International Wage Differences by Schooling and Skill**

Comparing the raw distribution of wages across countries is largely irrelevant to international migration as the structure of the labor force and the distribution of human capital is widely different. That the daily wages paid to a person with no schooling to transplant rice in rural Vietnam and the daily wage of a professor of economics in Geneva, Switzerland, are widely different is obvious, and, at the same time, not relevant to questions of labor mobility. This section presents data from two independent sources showing the differences across wages of individuals with the same levels of formal schooling and in the same occupations. Since gross domestic product (GDP) is value added and labor is a major source of value added it is (very near) an accounting identity that earnings per worker are higher in high GDP per capita than low GDP per capita countries. But it is possible that these differences are all, or mostly, compositional and that workers with the same “human capital” have the same earnings and average differences across countries that are (proximally) accounted for by differences in “human capital.” While the analysis gets more sophisticated, it is worth starting by documenting the magnitude of the gaps in wages of workers across countries with the same levels of schooling or in the same occupations. This provides a set of facts that both microeconomic theories and macroeconomic theories have to be capable of encompassing.
Wages by Level of Schooling

The World Bank has collected the raw data from a large number of labor force surveys from around the world that provide broadly comparable data on earnings and level of formal schooling, and some data on occupation, sector, and location. These data have been used to estimate Mincer-like regressions of wages on schooling (and other characteristics) for a large number of countries (King, Montenegro, & Orazem, 2012; Montenegro & Patrinos, 2014). The empirical results show incremental wage gains of around 10% per year of schooling, with only a modest degree of variability around that average.

As the raw data are not publicly available, the following analysis uses the median wages by level of schooling for those countries for which this is available, which was provided directly to the authors in local currencies, converted into PPP units in 2011 using the exchange rates in Penn World Tables (PWT) 9.1 (Feenstra, Inklaar, & Timmer, 2015). Two points. One, this is a consumption wage that is potentially relevant for labor migration and potential gains to migrants, not a product wage or unit labor cost, that would be more relevant to, say, investment location decisions. Two, this adjustment in PPP assumes that all of consumption from wages is in the country in which the labor income is earned. This can substantially understate the gains from labor migration from a poor country to a rich country if: (i) wages in rich receiving countries generate remittances spent in the sending country, and the World Bank estimates total magnitude of remittances to developing countries was $529 billion in 2018; or (ii) migrants had high savings and returned to consume in the sending country. Since prices in sending countries are, on average, substantially lower than in the receiving country, would-be migrant wages adjusted for the location of consumption could be higher than the PPP wage differences by a factor of 2 or more (so a factor of 5 difference in cross-national fully adjusted PPP wages could be a factor of 10 in consumption-location-adjusted wages).

Figure 1 shows the wages by level of schooling between the high-income countries and those countries with GDP per capita less than P$15,000. Figure 2 shows wages by level of schooling for three selected countries: the Netherlands (high wage), Dominican Republic (medium wage), and Ethiopia (low wage). These figures illustrate three key facts.

First, the wage gaps or ratios of wages for workers with the same level of schooling are massive at each level of schooling. For workers with secondary schooling, the gap is around P$14,813 between the rich industrial world and the low-income countries. The ratio of wages is 10 to 1. Between workers in the Netherlands and workers in Ethiopia the gap is P$23,000, a ratio of 22 to 1.

Second, the wage gap in absolute terms is larger at higher levels of schooling, even when the ratio is smaller. A basic Mincer regression assumes the natural log of wages of the $i$th worker in the $j$th country is linear in the level of schooling (equation 1):

$$\ln(w^{i,j}) = \alpha^j + r^j \cdot S^{i,j}$$

(1)
If high- and low-income countries had same Mincer wage increment ($r^{\text{High}} = r^{\text{Low}}$) then the ratio of wages would stay constant and the absolute gap increase. Even if the wage increment in the rich country were much lower ($r^{\text{High}} << r^{\text{Low}}$), consistent with a lower return to schooling where the level of schooling is higher (Bils & Klenow, 2000; Pritchett, 2006), the ratio of wages could fall but the absolute gap still increase. Figure 1 shows that wages increase proportionately much more by level of schooling in the rich countries (wages are higher by 30% in the OECD for those with post-secondary schooling, but a factor of 2.8 in the poorer countries) but the absolute gap grows to $\text{P}\$16,371 for workers with post-secondary schooling.

**Figure 1.** Annual Earnings (in PPP$) of Workers with Secondary Schooling in Rich Industrial Countries and Low-Income Countries.

*Source:* Author’s calculations using data provided by Claudio Montenegro from World Bank Labor Force Survey data.

*Note:* The annual earnings (in PPP$) of workers with secondary schooling is $\text{P}16,456$ in the rich industrial countries and 10 times lower ($\text{P}1,643$) in low-income (GDP per capita below $\text{P}15,000$) countries. Old (prior to new members) OECD countries include Australia, Austria, Belgium, Denmark, Finland, France, Germany, Luxembourg, Netherlands, Norway, Spain, Sweden, Switzerland, the United Kingdom, and the United States. As a number of these countries lacked data for "no schooling" results were predicted from the level of wages of those with secondary schooling. Countries at less than $\text{P}15,000$ are Bangladesh, Bulgaria, Colombia, the Dominican Republic, Ecuador, Ethiopia, Gambia, Honduras, India, Indonesia, Jordan, Mexico, Mongolia, Nepal, Nigeria, Pakistan, Paraguay, Peru, Philippines, Rwanda, South Africa, Uganda, Vietnam, and Zambia.
Figure 2. Wages by Level of Schooling in the Netherlands, the Dominican Republic, and Ethiopia.

Source: Author’s calculations using data provided by Claudio Montenegro from World Bank Labor Force Survey data.

The third point that emerges from these figures is that the “place” effect ($\alpha^j$ in the Mincer equation) dominates the individual schooling effects ($\beta^j \cdot s^j$) in determining wages. It is much more where you are (place) than who you are (personal characteristics) that determine wages. The wages of workers with no schooling at all in the rich countries are much, much higher than those of workers with post-secondary schooling in the low-income countries. In calculations of the wage gain from migration, adjusting wages in a rich receiving country for the human capital acquired from schooling in a poorer sending country is almost certainly second order (especially for the poorest countries). That is, all standard cross-national assessments of learning (e.g., Programme for International Student Assessment (PISA), Trends in International Mathematics and Science Study (TIMSS), Demographic and Health Survey (DHS)) reveal that a year of schooling conveys very different amounts of academic capabilities—like reading (Pritchett & Sandefur, 2017) and mathematics skills—across countries, as documented more recently in 2019 and comprehensively in the World Bank’s learning measures for their Human Capital Index (Angrist, Djankov, Goldberg, & Patrinos, 2019). Moreover, it is plausible that even beyond general measures of academic capabilities schooling can provide attributes (norms, dispositions, beliefs) that have country-specific labor market benefits that are not portable—the most obvious example being learning one’s native tongue versus the language of a potential destination country.

However, between countries like Ethiopia and Netherlands the gaps between the place ($\alpha^{\text{Ethiopia}} \ll \alpha^{\text{Netherlands}}$), proxied as the wage of individuals with no schooling (though obviously there are few such people in the Netherlands and many in Ethiopia), are very large
compared to the increment to ln(w) from a year of schooling in the Netherlands, which, crudely, if everything were linear (which it isn’t) is a modest per year of schooling gain:

$$(\ln(38861) - \ln(23945))/16 = 0.485/16 = 0.0303.$$ 

Suppose that the wage increment to the wages from a year of schooling in Ethiopia in the labor market in the Netherlands is some fraction of the gain to a year of schooling in the Netherlands in the labor market in the Netherlands. Imagine a worker with post-secondary schooling (assume ~16 years of schooling) gets a predicted wage of:

$$\ln(w_{\text{Ethiopian in Netherlands}}) = \alpha_{\text{Netherlands}} + (1 - Melt_{\text{Eth.Nld}}) \times r_{\text{Nld}} \times S_{\text{Ethiopia}}$$

Where “melt” is the fraction of a year of schooling received in Ethiopia producing wage gains in the Netherlands compared to a year of schooling in the Netherlands. If “melt” is 1 then an Ethiopian with post-secondary schooling just makes the same in the Netherlands as someone with no schooling, Ethiopian schooling has zero return in the Netherlands. If “melt” is 0 then an Ethiopian would make the same as someone who got their schooling in the Netherlands. As Table 1 shows, since the earnings of someone even with a post-secondary schooling in Ethiopia are so low, the absolute magnitude of the gap is very high even if “melt” is complete. Table 1 also shows this same calculation between “rich” and “poor” countries on average. It is still the case that, even with complete “melt”— that is “melt” is 1 and schooling in a source country counts for nothing in a receiving country, the wage gaps for post-secondary workers in poor countries and workers with no schooling in rich countries are P$14,456.
### Table 1. Wage Gains from Schooling

<table>
<thead>
<tr>
<th>Degree of “Melt” of Returns to Schooling</th>
<th>Difference in Wages of Post-Secondary Workers</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Ethiopia to Netherlands</td>
</tr>
<tr>
<td>Schooling in source has same impact on wages in receiving country as schooling in that country:</td>
<td>0</td>
</tr>
<tr>
<td>Schooling in source has no impact on wages in receiving country:</td>
<td>0.25</td>
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<td></td>
<td>0.5</td>
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<td></td>
<td>0.75</td>
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<td>1</td>
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</table>

Note: Even if the wage gains from schooling are not portable to a different country the wage gaps are considerable.

Source: Author’s calculations using wage data from World Bank Labor Market Survey data.
Occupational Wage Gaps

The International Labour Organization (ILO) Occupational Wages around the World (OWW) data (Oostendorp, 2012) provides wages of workers in the same occupation but different countries in local currency, again converted with PPP exchange rates to produce consumption wages. This data is sparse, as different countries have reported wages for different occupations so the composition of the categories “OECD” and “low income” differ from category to category. Figure 3 shows the annualized wages for waiters, construction workers, nurses, and technicians. These data on wages by occupation, which are from a completely different source from the data on wages by level of schooling, reproduce the same three key points as in the subsection “Wages by Level of Schooling.”

![Wage gaps by occupation in 2011 PPP USD](image)

**Figure 3.** Low-Income and OECD Countries: Wage gaps across Workers in the Same Occupations at All Skill Levels.

*Source: Own calculation using data from PWT9.0 and ILO OWW data.*

*Note. U.S. annual average wage includes wages in years matching countries in sample. * Technician includes chemistry technician, petroleum and gas extraction technician, medical x-ray technician, and electronics engineering technician.*

First, the wage gaps are massive for each occupation, even for a low-skill occupation like “waiter” where the actual work performed is nearly identical. It is striking (given that the data are from completely different sources and the composition of countries in the “high” and “low” income are different) that the data for “waiters” reproduces the wages and wage gaps for “secondary educated” almost exactly, P$1,737 in low-income countries (versus P$1,643) and P$16,346 in OECD (versus P$16,456), for a wage gap of P$14,609 (versus P$14,813).
Ashenfelter (2012, Table 3) takes this comparison further, comparing product wages for workers in the same company (McDonald’s), doing exactly the same work and shows “Big Macs per hour of work” is 6.9 times higher in the United States than in India or Latin America, 6.2 times higher than in the Middle East, and 3 times higher than in South Africa.

Second, as Figure 4 shows, the absolute wage gaps increase with skill level. Oostendorp (2005) estimates the skill level of each occupation, categorized from skill level I to IV. Even though the gains to skill are larger proportionately in poorer countries (as one might expect in countries with low average skill levels) the absolute gap increases.

![Median wages by skill level at PPP USD (year = 2008)](image)

**Figure 4.** Wage Gap Increases in Absolute Terms according to Skill Level of Occupations.  
*Source:* Author’s calculations using OWW data (Oostendorp, 2012).

Third, again the “place” effect dominates the “skill” effect and a worker in the lowest skill occupations (Level I) in a rich country makes almost twice as much as a worker in the highest skill category (Level IV) in a poor country (P$21,451 versus P$11,547).

Wage Gaps in “Observed Equivalent” and “Equal Productivity” Labor

This section moves from observational facts to estimate the local average treatment effect (LATE) of place on wages—or the wage premium from working in a specific place. Chiang’s (2019) science fiction story *Anxiety is the Dizziness of Freedom* takes economist’s concern with causal identification of treatment effects to the next level. Going beyond even a twin’s
thought experiment, he takes Hugh Everett’s “many-worlds” interpretation of quantum mechanics literally and imagines a machine allows a person to track their alternative lives that result from quantum events. Imagine two identical selves split by a quantum event into parallel realities and that one self is instantaneously transplanted into a different labor market (say, from Ethiopia to the Netherlands). What would be the LATE—evaluated over various time horizons (e.g., one month, six months, three years, etc.) of the “treatment” of movement across place of exactly the same individual? There is a reason it is science fiction; we cannot observe the factual and counterfactual for the same person. Moreover, people are not (typically) “exogenously” anywhere, people are where they are for reasons (this is true of movement within countries and across borders). Recovering the LATE of place, the wage difference of equal productivity workers, is going to be a challenge.

The first subsection, “Wage Gap of Observationally Equivalent Workers to Lower Bound on Equal Productivity,” reports the results of using observational data and econometric techniques to estimate lower bounds on effects with selection on observables to estimate a lower bound on the wage differences across places (each of 42 countries versus the United States) of equal productivity workers. The second, “Selection Bias and the LATE of Place,” discusses the literature that uses methods of identification, such as random selection from a pool of eligible or regression discontinuity, to estimate the LATE of place. Both conclude that while the wage gaps reported by level of schooling and skills tend to overstate the LATE of place due to positive selectivity of migrants on unobservable characteristics, the wage gaps of equal productivity workers across different places (labor markets) are massive.

Wage Gap of Observationally Equivalent Workers to Lower Bound on Equal Productivity

Clemens, Montenegro, and Pritchett (2019) combine the World Bank’s collection of labor market surveys and the U.S. Census data to estimate the LATE of place or “place premium.” One can distinguish two conceptual steps: (1) estimating the wages of observationally equivalent workers in two different places (their home country and the United States) for 42 different countries; and (2) using the Altonji (2005)/Oster (2015) methods to adjust for the potential selectivity on unobserved variables that affect productivity, hence adjusting the "observational equivalent" wage gap to a lower bound on the “equal productivity” wage gap.

The first conceptual step is the estimation of two wage surfaces. With Nigeria as an example, Clemens et al. (2019) use the Nigerian labor force survey data to estimate the wage surface of workers in Nigeria with respect to observed characteristics (e.g., age, sex, residence, sector, and schooling) associated with wages. They then use the U.S. Census data of people born and educated in Nigeria but working in the United States to estimate a wage surface for these people.\(^4\) Clemens et al. (2019) estimate the United States versus sending country “place premium” for observationally equivalent workers as the difference between the predicted wages (point on the respective wage surfaces) of two workers sharing the same “observables” but one working in Nigeria and one working in the United States. The “place premium” is
specific to the “reference” worker characteristics: a Nigerian-born, Nigerian-educated, 35-year-old, urban resident, male, with 9–12 years of schooling, working in the formal sector. The estimated wage U.S./Nigeria wage ratio for this reference category worker is 16.3.

Clemens et al. (2019) estimate this wage ratio for 42 different countries (all of those with both labor market data to estimate the “home-country” wage surface and sufficient observations in the U.S. Census to estimate the U.S. wage surface for people born in that country). For India, Indonesia, Bangladesh, and Pakistan this wage ratio stands at 7.8, 7, 5.5, and 7.4 respectively.5

This is a big advance on the wage gap estimates by level of schooling (see subsection “Wages by Level of Schooling”), which compares all workers in Nigeria to all workers in the United States with secondary schooling, because this new estimate only compares nationals in two labor markets and with more controls. However, the interpretation of the wage gaps of “observed equivalent, reference category” workers as the LATE of place for a typical or marginal mover, even with the same characteristics, is limited as there is no correction for selectivity on unobserved characteristics that affect productivity/wages in the sending and receiving markets.

Before reporting Clemens et al.’s (2019) results using the Altonji/Oster econometric techniques developed to adjust for selectivity, it is worth honing one’s intuition for how large these adjustments might be expected to be. First, the reference category workers in Clemens et al. (2019) are making on average $10 an hour in the United States—the low end of the wage distribution for natives—and are working in mainly low-skilled occupations: these are not global superstars. If one were trying to estimate the wages of Argentine-born professional soccer players who are playing in Argentina versus playing in Spain, based on height and measured 40 yard dash speed, one would expect both a fantastically long tail of earnings based on realized performance, only weakly related to observables (that is, Lionel Messi) and hence the place premium might appear fantastically large and be entirely selection. Second, given: (i) the massive differences in predicted wages (a factor of say, 10 to 1); and (ii) the modest variance of the distribution of the regression residual (which is the distribution of wages, reduced by the component explained by observed characteristics), there is almost no common support of the distributions of predicted wages for the reference category worker between home and United States. That is, the predicted wages in Nigeria are around P$1,200/month and in the United States are P$18,000, even if those working in the United States would have otherwise been at three times the predicted wage due to observables correlated with migration propensity, this is still just P$3,600 a month. Third, there is not actually very much either on the choice of the mover or on the process used by the United States for legal migration possibilities for low- to medium-skill workers, which suggests the process is driven by selection on unobserved productivity—unlike “talent” visas or those for education or specialty occupations (such as the H1-B visa in the United States)—and that why some people are in Nigeria and others are in the United States has to do with having a relative in the latter, not one’s counterfactual wage being high in Nigeria. Fourth, selectivity might be very different for different countries, particularly, say, countries close to/sharing a border with the
United States (e.g., Mexico, Central America, and the Caribbean) versus countries far from the United States. For this we have the advantage of 42 countries, so we are not estimating only one type or mode of selectivity.

Altonji, Elder, and Taber (2005) proposed a method for adjusting estimates for selectivity on unobserved variables, which has been honed by Oster (2015). The basic intuition is to estimate a bound on LATE-like estimates by assuming that selectivity on all unobservable characteristics is the same magnitude as selectivity on the observed characteristics, and adjusting coefficients for this bias. Table 2 presents the Clemens et al. (2019) results for the Oster (2015) adjusted estimates of the differences in wages of the reference category, which is a lower bound of the estimate for “equal productivity” workers (adjusting for both observed and unobserved productivity) or the LATE of place because the selectivity on unobservable characteristics may well be non-existent or weaker than on observables. The population-weighted average gain for the reference category worker (9–12 years of schooling, urban, male, 35 years old, formal sector) across the 10 largest developing countries is P$17,816, and the population-weighted average across all 42 countries is P$17,115. The unweighted median wage gap is P$15,512 (lower than the population-weighted average since India is very big and has a high estimated place premium).
Table 2. Estimates of the (Selectivity-Adjusted) Wage Gains for Low-/Medium-Skill Workers from the 10 Largest Countries to the United States

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<tbody>
<tr>
<td>India</td>
<td>493.0%</td>
<td>5.9</td>
<td>$23,846</td>
<td>$4,021</td>
<td>$19,825</td>
<td>545</td>
</tr>
<tr>
<td>Indonesia</td>
<td>519.1%</td>
<td>6.2</td>
<td>$21,194</td>
<td>$3,423</td>
<td>$17,771</td>
<td>117</td>
</tr>
<tr>
<td>Brazil</td>
<td>240.0%</td>
<td>3.4</td>
<td>$23,818</td>
<td>$7,005</td>
<td>$16,813</td>
<td>97</td>
</tr>
<tr>
<td>Bangladesh</td>
<td>407.7%</td>
<td>5.1</td>
<td>$19,315</td>
<td>$3,804</td>
<td>$15,510</td>
<td>67</td>
</tr>
<tr>
<td>Pakistan</td>
<td>484.7%</td>
<td>5.8</td>
<td>$21,662</td>
<td>$3,705</td>
<td>$17,957</td>
<td>65</td>
</tr>
<tr>
<td>Nigeria</td>
<td>1476.4%</td>
<td>15.8</td>
<td>$18,689</td>
<td>$1,186</td>
<td>$17,503</td>
<td>57</td>
</tr>
<tr>
<td>Mexico</td>
<td>155.7%</td>
<td>2.6</td>
<td>$17,511</td>
<td>$6,849</td>
<td>$10,662</td>
<td>54</td>
</tr>
<tr>
<td>Vietnam</td>
<td>655.4%</td>
<td>7.6</td>
<td>$19,820</td>
<td>$2,624</td>
<td>$17,196</td>
<td>44</td>
</tr>
<tr>
<td>Philippines</td>
<td>247.5%</td>
<td>3.5</td>
<td>$18,133</td>
<td>$5,218</td>
<td>$12,915</td>
<td>40</td>
</tr>
<tr>
<td>Thailand</td>
<td>139.6%</td>
<td>2.4</td>
<td>$18,205</td>
<td>$7,598</td>
<td>$10,607</td>
<td>36</td>
</tr>
<tr>
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<td>-------------------------------------------------------------------------------------------------</td>
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<td>-------------------------------------------------------------------------------------------------</td>
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<td>-------------------------------------------------------------------------------------------------</td>
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</tr>
<tr>
<td>Egypt</td>
<td>1111.6%</td>
<td>12.1</td>
<td>$20,739</td>
<td>$1,712</td>
<td>$19,028</td>
<td>34</td>
</tr>
<tr>
<td>11 largest population countries average (total population)</td>
<td>423.6%</td>
<td>5.2</td>
<td>$22,022</td>
<td>$4,206</td>
<td>$17,816</td>
<td>1,156</td>
</tr>
<tr>
<td>Population-weighted average, 40 countries (total)</td>
<td>361.1%</td>
<td>4.6</td>
<td>$21,855</td>
<td>$4,740</td>
<td>$17,115</td>
<td>1,435</td>
</tr>
<tr>
<td>Assuming 2,080 hours, per hour</td>
<td></td>
<td></td>
<td>$10.51</td>
<td>$2.28</td>
<td>$8.23</td>
<td></td>
</tr>
</tbody>
</table>

Note: The table shows a gain of $15,981 per year. 
Source: Adapted from Clemens et al. (2019, table 3).
Selection Bias and the LATE of Place

This subsection turns to experiments measuring the causal impact of labor migration on income, thus helping predict the extent to which migrant self-selection on unobserved traits biases observed wage difference outcomes. Migrants self-select into a work location based on a mix of observed and unobserved traits. While observed traits can be controlled for, the observed wage gaps between observationally equivalent workers in destination and sending countries are likely to be biased (upward or downward) by the extent that migrants’ self-selection on unobserved traits impacts their earnings in destination countries (Clemens, 2019). This means that estimating the LATE of a place requires predicting the degree of migrant selection on unobservable characteristics.

It is assumed, primarily on the basis of “intuition” or the casual observation of (or introspection by) “superstar” migrants, that the selection of migrants is nearly always positive (e.g., higher wages in the receiving country are due to unobserved characteristics) and potentially large (e.g., selection can account for a large fraction of the higher wages of migrants than non-migrants). Relative to this, Clemens (2019) built a simple self-selection model and used it to test the causal impact of India–United Arab Emirates (UAE) guest work, demonstrating two important points on this issue. One, when selection is modeled in a sophisticated way, even the direction of the effect of selection on estimating the LATE of place depends on details of the type of migration sector, and can be positive, negative, or zero. Two, the empirics nearly always reveal that, for low- to medium-skill workers, selectivity can account for a relatively small portion of the wage ratios of observed equivalent workers.

Clemens (2019) builds on the standard self-selection model (Borjas, 1991; Chiquiar & Hanson, 2005; Hanson, 2006; Roy, 1951) to predict selection on observable traits for temporary migrants and extends the model to predict selection on unobserved traits. Under the standard self-selection model, migrants choose between their work location (temporary (T) destination country, permanent (P) destination country, home (H)) by maximizing their real wage, subject to migration costs (equation 3): the natural log of real wage \( (w^j) \) in a given location \( j \) is determined by the place premium \( (\mu^j) \), the pure country nominal wage for unskilled worker adjusted by the price levels to give real wage) and the return \( (\delta^j) \) on observable skills such as schooling \( (s) \). Migration cost \( \theta^j \) is a function of wage units and is zero if the worker decides to remain at home \( (\theta^H = 0) \).

\[
\ln w^j = (\mu^j + \delta^j s) - \theta^j \quad j \in [H, T, P]
\]

(3)

The country effect \( (\mu^T) \) is the highest for temporary migrants, then for permanent ones. This is because temporary migrants earn high wages abroad but spend the majority of foreign income at home (at lower prices), compared permanent migrants, who spend their wage in
the destination country at high prices. At the same time, the return on observed skills is the lowest for temporary migrants—compared to permanent migrants, who enjoy a long time horizon to adapt their skills to the destination country, and to home-stayers who enjoy higher returns on skills utilized in the home market where skills are scarce. Figure 5 illustrates the real wage function in three locations (temporary destination, permanent destination, and home) and the predicted selection patterns. Workers at each skill level will choose to maximize their real wage. The model’s simple prediction is that temporary migrants exhibit intermediate self-selection that is less positive that permanent migrants. This implies that the overall self-selection effect on wages (negative, positive, or zero) depends on a set of parameters affecting and affected by observed and unobserved traits and can differ significantly depending on the type of migration, the context in the country origin, and cost of investing in migration. For example, in a country with high poverty, where workers’ reservation wage is low, one can expect less positive self-selection by temporary migrants.

Figure 5. The Standard Migrant Self-Selection Model.

Source: Reprinted from Clemens (2019).

Note. The standard migrant self-selection model predicts migrants in the middle skill range will select temporary migration, compared to low-skilled workers who choose to stay at home or high-skilled workers who choose permanent migration.

Clemens uses this model to provide a causal estimate of the impact on households from Indian workers performing temporary work in the UAE and, in doing so, an estimate of the direction and magnitude of selection. In late 2008/early 2009, thousands of Indian workers had been
hired to perform construction work in the UAE but the global financial crisis led to a collapse in oil prices and interrupted many such projects. Many workers who had been selected and hired in India (and other home countries) faced different probabilities of actually departing for their job depending on the date of their application. Clemens uses a survey of the full universe of the workers who had been hired by a major UAE construction company, and national data, to calculate wage gaps between migrant workers (who choose to move to the UAE) and: (1) workers who chose to stay in India, as well as (2) observationally equivalent workers in Indian national data, estimating the impact of temporary migration on household income, corrected for (or minimizing) selection bias. The fact that this is data on workers who expressed interest and received offers for a construction job in the UAE narrows the degree of selection on unobservable characteristics when the workers are compared to each other. Comparing the unconditional (observed) wage gap between an Indian worker in the UAE versus India, to the wage gap between observationally equivalent workers across the two countries, corrects for selection bias on observed traits. A drop in the wage gap after controlling for observed traits indicates a positive selection bias—that is, the observed wage gaps are not fully due to the place premium but also observed traits (and vice versa for an increase in the gap). Comparing the wage gaps between successful applicants who moved to the UAE versus successful applicants who did not move, exploiting the exogenous selection in this particular case, minimizes selection on unobserved traits—hence the causal estimate of the impact of temporary work on income. Table 3 shows the results: positive selection bias on observables for those applying for jobs abroad but negative selection into actually taking the job offer and working in the UAE. In this particular case the wage ratio controlling for observables understates the true gain.
### Table 3. Negative and Positive Selection Effects on Temporary Construction Workers from India Working (or Not) in the UAE

<table>
<thead>
<tr>
<th></th>
<th>I Unconditional Wage Difference between Indian Worker in UAE vs. India, Using Nationally Representative Data</th>
<th>II Wage Ratio, Controlling for Observables, Using Nationally Representative Data</th>
<th>III IV-2SLS (instrument: oil price index on day of job application)</th>
<th>IV IV-DEV (dummy endogenous variable IV model)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ln(wage)</td>
<td>2.76</td>
<td>1.3–1.5</td>
<td>1.381</td>
<td>1.337</td>
</tr>
<tr>
<td>Wage gap</td>
<td>15.8</td>
<td>3.6–4.4</td>
<td>3.98</td>
<td>3.81</td>
</tr>
<tr>
<td>Nature</td>
<td>Observed, unconditional, wage gaps</td>
<td>Observed, for observably equivalent workers (men, 30–34 years old, some secondary schooling) in UAE vs. India</td>
<td>Upper-bound causal effect of guest work on wage—adjusted for intermediate selection bias (compares wages of applicant in UAE vs. applicant who stayed in India)</td>
<td>Lower-bound causal effect of guest work on wage—adjusted for intermediate selection bias (compares wages of applicant in UAE vs. applicant who stayed in India)</td>
</tr>
</tbody>
</table>

Note: Clemens' (2019) study of temporary construction workers from India working (or not) in the UAE suggests positive selection on observables into applying and being hired for jobs in UAE but negative selection effects on unobserved characteristics among those taking up jobs.

Source: Adapted from Clemens (2019, tables 2 and 4).
McKenzie et al. (2010) provide an experimental measure of income gain from migration by leveraging the random migrant selection mechanism of New Zealand’s Pacific Access Category (PAC). PAC allocates visa quotas for Tongans to migrate to New Zealand, outside New Zealand’s migration policies for skilled workers or family reunification. Tongans file applications under PAC and if the number of applicants exceed the quota, a lottery is used to randomly select from among the applicants. McKenzie et al. (2010) compare the expected income of lottery winners and losers to estimate: (1) the intent-to-treat (ITT), or the effect of being selected in the lottery to migrate on expected income; and (2) the average treatment effect on the treated (LATE)—the effect of migrating on the expected income of those who actually migrate. The idea is that the lottery randomly allocates applicants as winners or losers, creating a control group of individuals who do not migrate (remain untreated). Comparing the income of those who were selected in the lottery to the income of those who lost generates an experimental estimate of the impact of the treatment (winning the lottery) on the treated (winners)—the ITT. However, this is not yet the causal impact of migration on migrant income because of the possibility that some of the treated (lottery winners) may drop out of the treatment (choose to not migrate)—creating a bias in the causal impact of migration. The authors use an instrumental variable (winning the lottery) to estimate the effect of the treatment (migration) on the treated who were randomly selected for the treatment and complied with it (winners who migrated), and they find this to be a significant gain of 263%. The authors also predict selection bias based on unobservable traits by comparing the income of lottery applicants vs. non-applicants (and winners who migrant vs. winners who stay in Tonga) prior to applying (migrating). They find evidence of positive selection for those who apply to migrate, but no evidence of selection among those who chose to migrate.

Clemens and Tiongson (2017) study the impact on households of workers from the Philippines performing temporary work in Korea and use a natural policy discontinuity generated by the fact the migrants had to pass a Korean language test to be eligible to migrate. Comparing the outcomes of applicants just above and just below the threshold shows a significant and large—hundreds of percent—income gain from migration. Additionally, the migration of one household member increases household spending on healthcare and education and quality-of-life expenditures.

Clemens et al. (2019) review the existing literature and the range of methods for estimating selectivity (e.g., some countries use rotating panels that allow the estimation of the wages, conditional on observable characteristics, of workers who subsequently attrite from the sample due to migration) and find: (i) about as many examples of negative as positive selectivity on unobserved characteristics; and (ii) there are very, very few estimates from any method that suggest a correction to wage ratios conditioned on observable characteristics of more than 25%. The most conservative adjustment supported by the literature would be to reduce estimates of wage premia that condition on observables by a factor of around 1.25, so that the median wage gap across 42 countries on “observed equivalent” workers of 5 would become a wage ratio of 4, though again, there is no theoretically or empirically supported
argument that selection is uniformly positive and one could equally make the case the “typical” adjustment should be about zero unless there are specific arguments or evidence to the contrary.

The one notable exception to studies that find large wage differentials is Hendricks and Schoellman (2019), who find wage gains of only 3.2 and 2.8 for migrants from very poor (<1/16 of U.S. GDP per capita) and poor countries (<1/8 but greater than 1/16) respectively. For these countries they use a data source, the New Immigrant Survey, that has quite small samples of movers from these countries (only 281 for very poor and 617 for poor); is based exclusively on those granted permanent legal status in 2003, which is obviously a massively selective group of movers; and relies on self-reported wages prior to the move, with a very large number of respondents who do not report pre-migration wages and hence induce an unknown degree of bias in self-reported wages. These are not plausible estimates for the typical gains for the marginal low- to medium-skill mover from a poor country as the adjustments for “human capital” are just implausibly large for these groups and imply a “human capital” of workers from the poorest countries that is just too low to justify any of them being employed in the United States, even at minimum wage, which is counterfactual. We suspect their results are biased toward a sample of people who show low gains because they had high human capital in their home country but were not able to use their skills in the United States (e.g., doctors from Ethiopia unable to certify to practice in the United States), and hence these estimates are heavily influenced by the wage experiences of relatively few people who, for a variety of reasons, choose to move from the high end of the wage distribution in a poor country to the low end in the United States.

**Are There Unrealized Gains from International Mobility? Migration Desires and Action**

It might be the case that observed wage differentials represent an equilibrium that is not constrained by border-based restrictions on mobility. For many, many reasons people often prefer to live in the country/region they were born/raised in. Hence, even with fully integrated, zero-policy-based restrictions to spatial mobility, one can expect real wage differentials to have long (if not infinite) persistence. It is possible, as some have suggested, that observed wage differentials are consistent with observed relatively low levels of cross-national mobility because people do not want to move. However, one should be doubly careful of arguments made by those who enjoy a liberty but want to deny it to another on the grounds that “they” are “not like us” and “don’t want” this liberty: careful first because of the obvious deviation from the simple golden rule/Kantian “do unto others” morality and second because such arguments, commonly made about various groups across history, have been proven empirically wrong again and again and again.

All the evidence is consistent with the idea that existing cross-national wage differentials produce a demand for mobility that is sharply restricted by the coercively enforced border-based restrictions enacted by nearly all countries. Survey evidence suggests that many more people would like to move than actually do (see the subsection “Expressed Intentions”).

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date: 28 March 2022
Moreover, both historical and contemporary evidence suggests that an equilibrium wage ratio for equal productivity workers across cultural distinct and geographically distant places is in the range of 1 to 1.5, not the factors of 3 to 5 to 10 we currently observe (see the subsection “Mobility Behavior and Wage Gaps”).

**Expressed Intentions**

The Gallup organization has, since 2010, included in its rolling global survey questions about migration. The key question is: “Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?” and then, of those who express a desire to move, “To which country would you love to move?” (only one response permitted). Using these numbers Gallup (2017) calculates a “Permanent Net Migration Index” (PNMI), which is the percentage change in the country population if everyone relocated to their preferred ideal location, which they calculate both overall and for levels of schooling and age groups. Figure 6 shows the plot of the Gallup youth (age 15 to 29) PNMI against the labor compensation hour per data from the PWT9.1. While clearly migration desires are based on many factors and not just wages (e.g., dissatisfaction with corruption, conflict, violence, anticipated growth, social networks in destination countries, political conditions), the results are striking that, for the median country in the bottom tercile of wages the youth population would fall by 29% if migration ideals were realized. In contrast, the median country in the top tercile of wages would see its youth population **double** (with the United States, the largest destination country in absolute terms, right at the median) while a number of smaller high-waged countries (e.g., Singapore, Canada, Switzerland) would see their populations quadruple. There are clearly unrealized ideas of migration that would, if realized, occasion massive movements of labor and these are correlated with wages. That said, wages are of course are associated with GDP per capita and many other phenomena (e.g., democracy, freedom, lack of corruption, more equal opportunity, better education, lower crime) that would attract migrants, and a variety of empirical exercises have examined the correlates of bilateral migration ideals (Dao, Docquier, Parsons, & Peri, 2018; Docquier, Peri, & Ruyssen, 2014; Migali & Scipioni, 2018).
For economists (and all social scientists) it is hard to know exactly what to make of responses to hypothetical questions like these. There are arguments both that these may overpredict mobility if barriers were reduced and that these underpredict mobility. Since Gallup only asks about permanent migration, one suspects this understates the desire to move permanently or temporally by a substantial margin. In surveys of youth 15 to 24 for *The World Bank World Development Report* (2006), youth were asked in seven countries: “If it were possible for you to legally move to another country to work would you?” with options “move permanently,” “move temporarily,” “try it out,” and “not move.” In Bangladesh, which has a 23% youth PNMI of males only 9% said “not move”—so 91% (!) expressed some preference for moving, but only about 5% said “move permanently,” about 60% said “move temporarily,” and the remainder wanted to “try it out.” In Ethiopia (with 38% youth PNMI) less than 10% said “move permanently” and over 60% said “move temporarily” or “try it out.” This is suggestive
evidence that the Gallup forcing of responses into “move permanently” and “not move” both overstates the desire for permanent movement and vastly understates the desire for temporary labor mobility.

The Gallup poll also asks respondents whether they have “plans” to move or have made “preparations” to move, and some have pointed out these are more in line with actual measured bilateral flows (Gallup, 2018). This question is hardly informative. Suppose one were to ask women in a country where they legally could not vote if they, in an ideal world, would like to vote and then also asked whether they had plans to vote in the next election. An “ideal-plan” gap in those questions would only reveal that women expected the law to be enforced and would reveal nothing about the intensity of their desire to vote, nor the likelihood they would vote, if they could do so.

**Mobility Behavior and Wage Gaps**

If border-based restrictions are creating wage differentials across equal productivity individuals who would like to move, then we should observe that episodes of lowered restrictions should produce movements at high wage ratios and that permanently lower restrictions should produce low wage differentials.

**Historical Episodes of Open Borders**

Up to the early 20th century there was near complete open mobility from Europe to the “areas of recent settlement” (United States, Canada, Brazil, Argentina, Australia, New Zealand, etc.) and there were substantial migration movements from many European countries. The work of Williamson (1995) constructs comparable (PPP) wages for urban unskilled occupations in a numbers of countries. Hatton and Williamson (1992) show very large emigration rates were consistent with what, by post-1990 standards, were modest wage differentials. Ireland had a gross emigration rate (to non-European destinations) of 1.6% per year from 1880 to 1889 and the real wage of the receiving areas to Ireland was only 2 to 1. In the aftermath of the Great Famine of 1845–1849 Ireland’s population fell by outmigration by half, from 1,850 to 1,900 (so a youth PNMI of 50% needn’t be unrealistic), while the real wage ratio of receiving countries to Ireland never exceeded 2.7 to 1 (Hatton & Williamson, 1992, Table 2). Italy, from 1900 to 1913, had a gross emigration rate of 1.8% per year and the unskilled wage ratio for receiving countries to Italy in that decade was 3.4 to 1. Even in periods in which travel was slower, communication more difficult and costly, but migration was legally possible, and PPP-adjusted wage ratios less than half, many of those observed since the late 1980s generated mass mobility and large population shifts.

**Culturally Distinct and Geographically Distant Labor Markets**

The existence of much smaller real wage gaps within countries, and evidence, for instance, that one would expect relatively small steady state differences in wages of equal productivity workers across U.S. states, even with substantial moving costs and home preference (Kennan
& Walker, 2011), isn’t compelling evidence that cross-national wage gaps are not maintained by binding constraints, as it is possible that the psychic disutility of moving is smaller within a country, for a variety of reasons. More interesting is the comparison of culturally distinct (and sometimes geographically distant) but legally integrated labor markets, some of which exist as the result of colonial history. The wage ratio for low-skill, private-sector workers between Reunion (a small island off the east coast of Africa that is an overseas department of France) and France is only 1.18, and between Guadeloupe (a Caribbean French overseas department) and France only 1.35. Puerto Rico is a Spanish-speaking Caribbean island which is a U.S. territory and Guam is a small Pacific island, but the residents of both are U.S. citizens, hence can freely travel to and work in the United States. Applying the Clemens et al. (2019) estimates to Puerto Rico and Guam produce estimates of 1.56 and 1.31. These are larger than within the United States but are very small compared to the lower-bound, selectivity-adjusted estimates for other Caribbean countries (even those with large U.S.-based social networks): Haiti at 4.87 or Jamaica at 3.78, or a distant Pacific country with strong historical U.S. ties like the Philippines at 3.47.

Existing Mobility Behavior

Three types of contemporary evidence suggest the legal constraints are binding in constraining the magnitude of mobility and maintain much larger wage gaps than possible with free mobility.

First, there have been instances of relaxations of barriers to mobility in the early 21st century that show immediate large flows. The United Kingdom allowed for immediate free mobility with the accession of Poland to the European Union (EU) and the Polish-born residents of the United Kingdom increased from less than 100,000 to over 500,000 in just four years (Barrell, Gottschalk, Kirby, & Orazgani, 2009), whereas the wage differences in the GDP data suggest a PPP wage gap of less than 2 to 1 (Budnik, 2009). In general, mobility is neither immediate, nor total, but can be cumulatively large: even with modest wage differentials, wages in East Germany were 70% of those in West Germany even by 1995 (Burda, 1995).

Second, some countries, such as the oil-rich Gulf states and Singapore, maintain high flows, relative to population, of workers who are allowed in under very strict conditions. Workers are often allowed in only on short-term contracts, lack pathways to citizenship, there is no expectation of equal wages with citizens, there is a risk of fraud (in both recruitment and in the host country), and even the risk of abuse. Yet even in those conditions there is evidence of excess demand in this expressed ideal location and in practice. In the Gallup youth PNMI the Gulf states would be massive gainers, even from their already high levels: Bahrain 72%, Saudi Arabia 114%, UAE 330%, and Kuwait 349%. The Gulf states can demand that applicants for work, even in unskilled trades in construction, meet many requirements. Moreover, in the relationship between actual and potential sending countries and receiving countries it is clear there is excess demand for the placement of workers, not an unmet demand for workers.
Third, the existing differentials in wages (and living conditions more broadly) do induce people to pay high costs in travel and to brokers and suffer physical risks (even death) to cross borders and gain entrance at rates that tragically belie the notion existing differentials are an coercion-free equilibrium. Obviously counting migrant deaths is difficult and has high uncertainty. The International Organization for Migration (IOM) (Reineke, Martínez, Brian, & Laczko, 2014) estimated a total of 22,400 deaths of migrants attempting to enter Europe between 2000 and 2014. The Missing Migrants Project (<https://missingmigrants.iom.int/>) (IOM) estimates deaths of potential migrants to Europe between 2014 and 2018 at over 38,000. The U.S. Border Patrol estimates migrant deaths crossing the U.S. border with Mexico between 1998 and 2019 were over 7,800 (Customs and Border Protection 2019). The total U.S. military fatalities from the Afghanistan War have been 2,216, and 4,497 in Iraq. The total deaths of those attempting to cross the Berlin Wall are estimated to be 245, which is roughly the deaths per year along the U.S.–Mexico border.

Bah and Batista (2018) report on a “lab in the field experiment” in rural Gambia of the empirical relationship between expressed willingness to migrate illegally to Europe and expected risk of death in doing so. Of the 406 interviewed, 46.5% (189) expressed a willingness to migrate illegally to Europe and of that group their average expectation was a 43% chance of dying en route. When provided with the researchers’ “correct” estimate that the probability of death was “only” 20%, this increased the willingness to migrate illegally by 2.3% age points. Literally the day this section was being drafted (October 28, 2019) in the United Kingdom there was a headline about a young Vietnamese woman who had died while being smuggled into the United Kingdom, and who texted her mother, “I’m sorry Mom, I am dying.”

Existing Enforcement Costs

Countries spend considerable sums enforcing their restrictions on mobility. The United States, in fiscal year 2019, budgeted US$24.7 billion dollars to the U.S. Immigration and Customs Enforcement (ICE) and U.S. Customs and Border Protection (CBP) and has spent 324 billion on border enforcement since the creation of the Department of Homeland Security in 2003 (American Immigration Council, 2019). As a reference point, this is more than the U.S. foreign assistance budget for health, humanitarian assistance, economic development, democracy and human rights, and multisector activities combined, at US$19.6 billion (ForeignAssistance.gov <https://www.foreignassistance.gov/>).

Aggregate Theories of the Level and Growth of Output and Labor Mobility

The previous sections have relied entirely on data from and about individuals: wages, expressed intentions, and observed mobility behavior. Economics also, of course, has generated theories and evidence about the sources of cross-national differences in income. Lucas (1990) famously argued it was “hard to think about anything but” questions in economics like: “why are some countries rich and others poor?” the related question “why is there rapid growth in some country/periods and why do other country/periods involve slow
growth/collapse?” and “What, if anything, can be done about it?” The 2019 version (MRW) of PPP-comparable national accounts shows that the standard macroeconomic models provide estimates of the gap in wages (adjusted for human capital) or marginal product of labor that are consistent in magnitude with the microeconomic evidence (see the section “Macroeconomic Estimates of Wage Differentials and their Sources”). Reconciling the theories of cross-national income differences with observed wage gaps of equal intrinsic productivity workers is important, as a dominant interpretation of “workhorse” growth models that emerged in the 1960s and 1970s suggested that incomes would converge across countries without labor mobility, but these predictions have been proven false (see the section “‘A’ (TFP) Did Not Converge (Much, for a Long Time)”). The “best” available theories since the 2000s about cross-national differences in labor productivity do not generally predict absolute income convergence in the absence of labor mobility (see the section “Models of Persistent Gaps in A”).

**Macroeconomic Estimates of Wage Differentials and their Sources**

The Penn World Tables are a collection of national accounts data and estimates of PPP that allow the comparison of levels of national income across countries and over time. They also provide data on capital stocks and estimates of human capital that allow the calculation of measures of the total factor productivity (TFP) of a country. The PWT9.1 (Feenstra et al., 2015) is the latest iteration of this data and makes possible three calculations that are informative about wage gaps across countries.

First, the national accounts estimate of annual labor compensation,\(^7\) adjusted to equal hours \((2080 = 52 \times 40)\), is:

\[
\text{Annual labor compensation} = \frac{\text{GDP}}{\text{Workers}} \times \text{(Labor Share)} \times \left( \frac{2080}{\text{hours}} \right)
\]

Regressing this measure of wages on the log of capital per worker and on the PWT9.1 Index of Human Capital (constructed from data on years of schooling and returns) and the 2017 World Bank data on learning and the interaction of these two “human capital” variables generates coefficients to predict the annual labor compensation per worker at 2,080 hours for all countries, if, instead of their own human capital measures, they were at the 33rd percentile.\(^8\) Hence this “nets out” the contribution of human capital (as measured) to country wages.

The results in column I of Table 4 show wage gaps, adjusted for human capital, modestly larger in absolute terms and in ratios than the microeconomic evidence (see the section “International Wage Differences by Schooling and Skill”) by level of schooling and occupation (part of this may be due to the difference between reported wages and labor compensation). The ratio of human capital-adjusted annual labor compensation of the United States and
OECD to the 33rd percentile is 6.5 to 1 and 4.9 to 1. A simple bivariate regression suggests that 60% of this variation in “human capital-adjusted wages per hour” is associated with the PWT9.1 reported measure of TFP.

The second calculation is just a simple calculation of the marginal product of labor assuming a Cobb–Douglas production function in physical capital, human capital, and labor, with shares of a third each. In this simple case:

$$MP_L = \frac{dQ}{dL} = \frac{Q}{L} \alpha_L$$

(5)

This generates the differences of the United States and the OECD to the 33rd percentile of 5.2 to 1 and 4.4 to 1. In this case, the R2 with the TFP measures is 0.62.
Table 4. Calculations of Wage (Labor Compensation) Gaps using GDP Data

<table>
<thead>
<tr>
<th></th>
<th>I</th>
<th>II</th>
<th>Total Factor Productivity (A) of Country Relative to United States (= 1)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Wages (annual labor compensation per worker, adjusted to same hours) if each Country were at Same Point (33rd percentile) of Human Capital (HC from PWT9.1) and Measured Learning (Harmonized learning outcomes from World Bank)</td>
<td>Cobb–Douglas Calculation of Marginal Product of Labor (labor coefficient = 1/3)</td>
<td></td>
</tr>
<tr>
<td>Average of bottom tercile</td>
<td>$3,224</td>
<td>$4,244</td>
<td>0.37</td>
</tr>
<tr>
<td>First tercile (33rd percentile)</td>
<td>$8,455</td>
<td>$8,682</td>
<td>0.52</td>
</tr>
<tr>
<td>United States</td>
<td>$54,879</td>
<td>$45,422</td>
<td>1.00</td>
</tr>
<tr>
<td>Ratio tercile I to United States</td>
<td>6.49</td>
<td>5.23</td>
<td>1.93</td>
</tr>
<tr>
<td>OECD</td>
<td>$41,566</td>
<td>$38,350</td>
<td>0.86</td>
</tr>
<tr>
<td>Ratio tercile I to OECD</td>
<td>4.92</td>
<td>4.42</td>
<td>1.65</td>
</tr>
<tr>
<td>90/10 ratio</td>
<td>47.60</td>
<td>16.06</td>
<td>3.14</td>
</tr>
<tr>
<td>I</td>
<td>Wages (annual labor compensation per worker, adjusted to same hours) if each Country were at Same Point (33rd percentile) of Human Capital (HC from PWT9.1) and Measured Learning (Harmonized learning outcomes from World Bank)</td>
<td></td>
<td></td>
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<tr>
<td>---</td>
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<td></td>
<td></td>
</tr>
<tr>
<td>II</td>
<td>Cobb–Douglas Calculation of Marginal Product of Labor (labor coefficient = 1/3)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Total Factor Productivity (A) of Country Relative to United States (= 1)</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

| Bivariate regression R2 on A | 0.60 | 0.62 | 1.00 |
| Number of countries | 107 | 107 | 107 |

Note: Adjusted for PPP shows similarly large gaps in wages, even adjusted for human capital, driven in large part by differences in measured A.

Source: Author’s calculations using PWT9.1 data and World Bank data on learning outcomes (Angrist et al., 2019).
Column III reports results on the PWT9.1 measure of A relative to the United States (= 1). The 33rd percentile country has A about half that of the United States and average OECD productivity is about 0.86 of the United States (so about 60% higher than the 33rd percentile, a little more than twice as high as the bottom third on average).

“A” (TFP) Did Not Converge (Much, for a Long Time)

Solow (1956) proposed a model to study economic growth, which, in its MRWs, assumes a production function in which output per worker differences (over time or across countries) can be decomposed into physical (K) and human (H) capital and total factor productivity (A). “A” (TFP) is empirically only a residual and hence a “measure of ignorance” (Abramovitz, 1956).

One particular interpretation of this model (and its extensions) guided the early generation of economic development research and practice. If A was interpreted as “technology” or “knowledge” or “codes and blueprints,” A is a potentially public good (non-rival and non-excludable), though, of course, patents and other types of intellectual property restrictions attempt to create excludability. In this interpretation of A as “technical” knowledge, it should diffuse easily and hence one could expect A to converge rapidly across countries. If A converges fast then countries with low K/L and low HK/L will have high productivity and hence high returns to factor accumulation. This will create potential for rapid factor accumulation through both domestic savings and, possibly, foreign savings, as capital will want to move to high A/low K/L, H/L places. Therefore, in this interpretation, labor did not need to migrate as the movement of labor was thought to be slower and more difficult than capital but the fast convergence of A plus accumulation-driven convergence in K/L and HK/L would equalize wages, reducing and then eliminating labor mobility pressure.

In this model one could believe that the pace of factor accumulation is limited by savings that could be mobilized (domestic and foreign) and hence, as Arthur Lewis (1954, p. 416) famously wrote,

> the central problem in the theory of economic development is to understand the process by which a community which was previously saving, and investing, 4 or 5 per cent of its national income or less converts itself into an economy where voluntary saving is running at about 12 to 15 per cent of national income or more. This is the central problem because the central fact of economic development is rapid capital accumulation.

The core working growth models of the International Monetary Fund (IMF) and World Bank assumed that growth was limited by investment, which was limited by domestic savings, and that filling the “financing gap” was key to growth (and these models were used in practice long after the economics profession have given them up; Easterly & Levine, 1997).
However, the plausible sequence of “A converges fast, then factor accumulation flows (limited by pace of domestic and foreign savings) cause convergence in factors per worker, the combination of which cause convergence in income per worker” did not happen. What is striking is that this is at least in part because A (TFP) did not converge. Table 5 shows Bosworth and Collins’ (2003) calculations from a standard “growth-accounting” exercise by region between the years 1960 and 2000. In three regions, Middle East, Latin America, and Africa, there was absolute income divergence and a large part of this was that A grew slower than in the OECD. Even in the two regions with income convergence (East Asia (not including China) and South Asia), A growth was only at exactly the same rate as in the OECD and convergence is driven by faster growth in physical capital accumulation. The PWT9.1 data provide new capital stock estimates and estimates of A growth based on national accounts data in real (constant, local currency) units. These show the same fundamental features. Over the entire period since 1960 the median annual A growth in every developing region was slower than in the OECD. Only East Asia (excluding China) and South Asia (which in this data is just India and Sri Lanka) outperformed even the slow A growth of the OECD post-1990 of 0.3% per annum.
Table 5. Measured A Growth in Developing Compared to OECD Countries

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<tr>
<td></td>
<td>N</td>
<td>Growth in output per worker 1960–2000 (%)</td>
<td>PK per worker (%)</td>
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<td></td>
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<td>(%)</td>
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<tr>
<td>Industrial</td>
<td>22</td>
<td>2.2</td>
<td>0.9</td>
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<tr>
<td>East Asia (except China)</td>
<td>7</td>
<td>3.9</td>
<td>2.3</td>
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<tr>
<td>South Asia b</td>
<td>4</td>
<td>2.3</td>
<td>1.0</td>
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<tr>
<td>Middle East c</td>
<td>9</td>
<td>2.1</td>
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<td>Latin America</td>
<td>22</td>
<td>1.1</td>
<td>0.6</td>
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<tr>
<td>Sub-Saharan Africa</td>
<td>19</td>
<td>0.6</td>
<td>0.5</td>
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Note: Standard growth-accounting exercises show that measured A growth in developing countries was about the same or lower than the OECD: no convergence of A on average.

a This calculation uses all available data since 1960 for each country, which is at most to 2017;
b the only South Asian countries with data;
c this calculation uses only the non-oil Middle East countries from the PWT9.1 as the measured productivity growth of the high-oil countries is very low.
Source: Bosworth and Collins (2003) for first five columns. The last four columns are the author’s calculations using the PWT9.1 data, which included adjustments for the composition of capital, using the TFP at constant national prices ($rtfpna$) variable, and are least squares growth rates over the periods.
“Growth decomposition” exercises consistently find that differences in A account for most of the differences in GDP per capita. Hall and Jones (1999) argue that A differentials explain the majority of the output gap between the countries: A gaps between the five richest countries and five poorest countries in 1988 contributed a factor of 8.7 to output gaps, compared to much smaller factors for physical and human capital (1.8 and 2.2 respectively). Caselli (2005) assesses cross-country output gaps and shows that physical and human capital only accounted for 35% of the 90–10 percentile gap in per capita income. Inklaar, Wolter, and Gallardo-Albarrán (2019) use more sophisticated data to measure capital stocks to be sensitive to the composition of the capital stock. While these do increase some of the measured impact of capital, they still conclude that A differences account for about two thirds (0.648) of the observed differences in GDP per capita.

If A does not converge, then returns to capital in low-income countries with low capital stock per worker (K/L) are not necessarily high. The latest PWT9.1 data includes estimates of the “internal rate of return (IRR)” to capital. Simple regressions of those returns on capital stock per worker, A relative to the United States and their measure of human capital, finds a modest-sized partial correlation of IRR and K/L, such that moving from the 25th percentile of K/L (P$31,220 per worker) to the 75th percentile of K/L (P$220,504)—a sevenfold increase—only reduces the IRR by about 3 to 5 percentage points. Caselli and Feyrer (2007) suggest that marginal products of capital have converged (even if K/L hasn’t), a finding confirmed in 2019 (Lowe, Papageorgiou, & Perez-Sebastian, 2019), only with the caveat that this appears to be truer of private than public capital.

**Models of Persistent Gaps in A**

Empirically, A is residually measured and hence is both model dependent (how one “accounts” for “factors” determines the measure of A) and ultimately a “measure of ignorance.” The interpretation of A as “technical progress” was always only a conjecture about what accounts for the observed differences in measured productivity and its components. The “technical” interpretation of A in the Solow model was influential in suggesting the possibility of (rapidish) convergence in absolute income (and wages) across countries without labor mobility, which helps explain the relatively little sparse attention to labor mobility in development and international economics. However, there are at least four other interpretations of the sources of productivity gaps, each of which has its own implications for pressures for labor mobility and for its consequences on movers, receiving countries, and sending countries.

**A and “Institutions”**

The literature on “institutions” suggests that cross-national TFP and its dynamics is not the diffusion of technical knowledge but rather whether countries can create “rules of the game” that support high productivity of factors (Acemoglu, Johnson, & Robinson, 2001; Hall & Jones, 1999; North, Wallis, & Weingast, 2009; Rodrik, Subramanian, & Trebbi, 2004). In these
models there are not necessarily predictions of convergence of “institutions” toward high TFP. Nunn (2007) and Dell (2010) demonstrate very long-lived effects of history (via slavery in Africa or the *mita*10 in Peru and Bolivia) on levels of income across places (even within countries). In many of these “institution” models of growth and A there were no “policy recommendations” as there was no theoretically and empirically grounded models of the dynamics of “institutions” that had “policy levers” and “institutions” might only change at “critical junctures” (Acemoglu et al., 2001)—hence, long-term persistence of low levels of income was a possible, even common, outcome.

If TFP is determined by “institutions” and “institutions” have large degrees of persistence then this is a powerful case for labor mobility, as in “productivity world” factors flow to places where productivity is high, not necessarily where K/L is low (Easterly & Levine, 2001). Movers are better off as the LATE of place is large due to large A differences, receiving countries are roughly neutral (a bit better off) as A is impervious to most levels of mobility, and sending countries are roughly neutral (a bit better off) as since movers do not predictably change the dynamics of A or growth there is no long-term loss (and K/L goes up). There is an emerging literature that suggests there might be non-linear impacts of migration on A as “institutions” are supported by norms that migrants may not share, but the empirical evidence suggests that most countries are far from this level of migration and that, even with a possible level of “A deteriorating migration,” migration levels, as of today, are far lower than optimal (Clemens & Pritchett, 2019).

**A and “Capabilities” and “Structural Transformation”**

Hausmann et al. (2014) propose a model that does away with “A” altogether. In their model country (and regional) productivity depends on product-specific, Leontief-like production functions in “capabilities” where more complex products require more (and rarer) capabilities. These “capabilities” take a variety of forms, some are physical infrastructure, some are standard tradable inputs, some are legal/policy-facilitating/enabling regulations/laws, and some are practical tacit knowledge of how to combine all of these and produce goods. They often explain their model by analogy with a game of Scrabble—the player with more letters is more productive because he can produce more and more complex words, compared to players with few letters, who only make shorter and fewer words. At least some capabilities are place specific and non-tradable.

This model also suggests high and persistent pressures for labor mobility. First, this model has the feature that the productivity of a given unit of human capital is dependent on the place-specific availability of other factors with which to combine—so cities are far more productive than rural areas. Second, this model can generate situations in which the returns to acquiring new capabilities are high when there are already more capabilities, and hence agglomeration economies, so that it is hard to draw, say, new capital investment, to low productivity places and hence there is a lack of pressure for convergence in productivity. This implies that: (a) there are, potentially large, differences in the productivity of labor (or various “types”) and hence gains to movers; (b) high capability economies have high productivity from diversity, not just “magnitude” of capabilities, and hence productivity is not deteriorated, and could be
enhanced, by labor mobility (and this is even more true for developing countries that need to bring people to bring tacit knowledge); (c) senders do not lose from outward migration of low-/medium-skill labor as they have these capabilities (often in abundance) and really need diversity (Easterly, 2004; Hausmann, 2015); and (d) capability growth in many developing countries is expected to be very slow. Andrews, Pritchett, and Woolcock (2017) construct a “Capability Index” to measure a country’s policy implementation capability level and growth. They reveal a “capability trap” problem for countries with weak and very weak capability—that is, most countries are witnessing deteriorating capability or very slow growth, such that the time horizon needed for these countries to reach high capability is infinite.

**Intrinsically Spatial Productivity and Optimal Population**

A third explanation of differences in A are intrinsically spatial models of productivity, often building off of resources. That is, even with equal “institutions” and policies people often locate in a specific place because of its specifically spatial features—for example, the soil/climate/water availability are good at producing wheat (or rice or rubber or coffee or etc.), there are valuable minerals on or under the ground, or it is near a port or crossroads (or not). If there are spatially specific productivities that are sources of product-specific comparative advantage, then this, particularly when combined with any non-linear agglomeration economies of the types that produce cities (Black & Henderson, 1999; Ellison & Glaeser, 1999), leads to the possibility of large, persistent shocks to place-specific productivity and hence “optimal” population.

Pritchett (2004) shows that even within in a large, integrated, mostly “institutionally” homogenous country like the United States there has been massive labor mobility. The variance of population growth rate of spatially contiguous regions (made up of U.S. counties) is orders of magnitude larger than across similarly sized developing countries. This suggests a combination of enduring spatial shocks to optimal population. On the other hand, the variance in the growth and level of income per capita is much smaller within integrated regions than across all developing countries (where the dispersion of wage growth is massive). Pritchett constructs the population of declining U.S. counties, had outmigration been restricted, and finds that the their actual population in 2004 is a third what it would have been in the no-migration case. This means that even in spaces with perfectly free trade, perfectly mobile capital, and more or less equal “institutions,” large amounts of labor mobility are consistent with large, persistent shocks to intrinsically spatial productivity. While capital, both physical and human, could have migrated to these shrinking counties, that is not the dominant feature of what happened, mostly labor migrated to other counties within states (e.g., urbanized) and to other states.

Zambia, for example, whose GDP per capita peaked in 1964, had a population of 3.5 million then, which would have fallen to 2.52 million in 2004 had outmigration been as free as it is among U.S. counties. Instead, Zambia’s GDP per capita in 2004 stood at 59% of its peak and its population had increased to 10 million as outmigration to high productivity places was limited. This is in sharp contrast to the historical experience with Ireland, in which, in response to the positive shock of the potato (as a cheap source of calories amenable to
Ireland’s conditions), population grew and then, following a blight to the potato in 1847 (and onwards), fell to a third of its previous level—and real wages relative to the United Kingdom never fell—because this was in a period of free labor mobility (for the Irish), including to areas of later (Western) settlement like the United States.

Gains from Relaxing Barriers to Labor Mobility

The fields of international economics and “welfare economics” have developed tools and models for measuring the magnitude of efficiency losses from policy interventions in markets and of border-based obstacles. In 2019, the Nobel Prize in Economics was awarded to researchers using randomized control trials (RCTs) to investigate the gains to poverty-focused interventions. Using either of these approaches reveals that the gains at the margin from relaxation of border-based restrictions on labor mobility are currently orders of magnitude larger than gains from further liberalization or from in situ interventions (see subsection “Gains at the Margin”). The general equilibrium extensions suggest similar gains (see subsection “Gains from ‘Open Borders’”).

Gains at the Margin

While not a good stopping point, simple arithmetic is a good starting point. The Gallup (2017) data suggest that 750 million people say they would move permanently if they could. If all of these were workers and real consumption wage gains to of the typical mover were P$15,000, the gains would be 11.25 trillion dollars. That is roughly three times as large as the entire German economy.

Gains from International Liberalization

Simple partial equilibrium calculations of the welfare losses from price distortions of the “Harberger Triangle” type start from the basic area of the triangle as \( \frac{1}{2} \times \text{base} \times \text{height} \) where the “height” is the price equivalent of the distortion and the base is the magnitude of the market. Nearly all analysis of welfare losses from price distortions—in trade, from subsidies, from market regulations—start with estimates of the price equivalent of the distortion and these are nearly always measured in percent. Most of our discussion of wage price distortions have been in terms of factor multiples, like 4, which one needs to multiple by 100 (two orders of magnitude) to get the percentage. Given that nearly all border-based price distortions in goods trade in OECD countries are less than 10% and the wage distortions for low-/medium-skill labor are on the order of 400–1000%, one should expect small relaxations in labor distortions to be similar in magnitude to the complete elimination of the small restrictions on trade. With the demise of the Doha Round negotiations—and the, not unrelated, general pushback against “globalization”—there have been fewer calculations of the gains from further liberalization of the flow of goods and services using standard economic models. Walmsley and Winters (2005), using a standard computable general equilibrium model (GTAP), estimate that the gains from a 3% increment to the OECD labor force by relaxing restrictions on temporary mobility on developing countries would produce net gains (adding
gains to movers, receiving countries, and sending countries) of US$156 billion. This is 50% more than estimated gains from a complete liberalization of trade in goods and services of $104 billion.

### Gains from in Situ Development Interventions

The 2019 Nobel Prize in Economics was given to a trio who extended the use of RCT methods to the evaluation of anti-poverty programs. A high-profile article in *Science* magazine (Banerjee et al., 2015) reported on the evaluation across six countries of a multipronged “Graduation” livelihoods program that had been developed and implemented by the non-government organization (NGO) BRAC (Building Resources Across Communities). A rough summary is that, averaged across the five countries in which the program worked, the program spent $4,545 per household in the first two years of program implementation and generated $344 household gain in year three. If one assumes a 5% discount rate and that the $344 gain persists for 40 years the net present value (NPV) of the program to the household is $1,128. The authors claim this is the new “gold standard” of the evidence-based anti-poverty program.

![Figure 7](image-url)

**Figure 7.** NPV Gains to Low-Skilled Workers from Access to Rich Country Labor Markets.

*Source:* Author’s calculations from Banerjee et al. (2015) and Clemens et al. (2019).

*Note.* The NPV gains are orders of magnitude larger than of the best rigorously demonstrated poverty programs.

Figure 7 shows a comparison of these gains from the anti-poverty program versus gains from working in a rich country like the United States. If one uses the lower bound for wage differentials equal productivity reference category workers for those same five countries, the annual gain is $13,119 (again, this is a serious *under*statement of the gains from mobility as it assumes all the wage gain is spent on goods/services at the higher prices in the United States than on goods/services in the home country). If one assumes a $2,000 each-way mobility cost, then the gains from one-year access are roughly 8 times the lifetime gain and the “lifetime to...
The gains do not end here. Migrants’ extended families are made better off due to the channeling of income gains from migrants to their households in sending countries, allowing migrants to invest in the human capital accumulation of family members (Yang, 2008, 2011). Additionally, moving from private gains to movers to gains to sending countries (Nunn, 2019) discusses outmigration from sending countries as the basis for country development strategies, given the role remittances can play, or the role of the diaspora in generating new international business links between sending and receiving countries. Yang (2011) shows that remittances, since the late 1990s, have been a stable source of inflows to developing countries, exceeding official foreign aid or development assistance flows, and nearing total foreign direct investments. Hausmann and Nedelkoska (2018) analyze the impact of Albanian return migration from Greece, following the debt crisis, on the employment and wages of Albanians who never migrated. They show that return migrants engaged mostly in commercial agricultural activities. In effect, this return migration had a positive impact on wages of low-skilled Albanians who never migrated and has “pulled non-migrants out of non-participation [in the labor market], unemployment and subsistence agriculture towards commercial farming” (p. 32). The authors argue Albanians in Greece have accumulated agricultural know-how and transferred this know-how, along with greenhouse technology, back to Albania—thus creating productive activities.

**Gains from “Open Borders”**

The steady-state gains from “open borders” have been a bit capricious since 2010’s (and foreseeable) political circumstances and in terms of the strain they put on assumptions about being able to correctly model GDP far outside existing conditions. The outcome is reasonably predictable: If lots of factors, especially labor, is working at very low levels of productivity then allowing all factors to move (with time for physical capital, human capital, etc. to adjust) is going to produce very large gains (Kennan, 2013). Hamilton and Whalley (1984) estimated a rough doubling of world GDP. There have since been a number of alternative estimates, many of which demonstrate a wide range of possible outcomes depending on the assumptions made (Bradford, 2012; Delogu, Docquier, & Machado, 2014; Docquier, Machado, & Sekkat, 2015; Iregui, 2005; Klein & Ventura, 2007; Moses & Letnes, 2004), with a plausible low end range of 10% and upper range of 100%; one could split the difference at 55%. As world GDP is US$133 trillion, the plausible pessimistic estimates suggest gains of 13 trillion and the “split the difference” would be a gain of 73 trillion. As one comparison, a computable general equilibrium (CGE)-modeled estimate of GDP losses in 2100 from 4°C warming versus 2°C warming are US$17.5 trillion (Kompas, Pham, & Che, 2018).

An objection to these large gains general equilibrium calculations is that they assume that TFP (”A”) in the high A countries is not affected by migration and that there might be non-linearities in the relationship between A and migration, such that “too much” migration deteriorates A. Clemens and Pritchett (2019) take this possibility seriously, modeling this possibility and attempting a calibration of the parameters of such a model. Their paper has three findings. First, there is no evidence in OECD countries of an association between the TFP-weighted migrant stocks and growth in TFP, in spite of this measure of migration already
differing across OECD countries by an order of magnitude. Second, the conceptually relevant measure is not the “stock of the foreign born” but the “stock of the foreign born that cause deterioration in A,” and this requires very specific assumptions about the dynamics of institutions and the dynamics of “assimilation”—in the narrow sense of the extent of pressure on reducing A—and how this is affected by the mix of migrants; we do not have particularly good data or theory on any of this. Third, even in models where a sudden, rapid change in migrant stocks could reduce A, there are, in a calibrated model, “optimal control” paths of migrant dynamics that reach very high stocks of the foreign born without any impact on A; these “optimal” paths suggest flows of migration much higher, not much lower, than the flows of at least the last two decades.

Conclusion

It is easier to spot elephants than mice and to spot mice than the fleas that live on the mice. The elephant in the room of discussions of “globalization” and international economics is that, while markets for goods and capital have been increasingly liberalized by policy and integrated by technological changes (Baldwin, 2016), and hence “true” price differentials are small and hard to measure, the international economics of labor mobility is pretty simple. There are massive differences in the real consumption wages of equal intrinsic productivity workers, which depend on their place. These wage differences are maintained by border-based restrictive policies, erected and enforced by countries which prevent workers from moving to places which offer opportunity. The economic loss from these restrictions, mainly to thwarted movers but also to the receiving countries themselves, is the largest policy-induced welfare loss in the world, at least since the 2000s, quite possible in the history of mankind. The gains to human well-being from more and better flows of workers between countries are an order of magnitude larger than feasible “interventions” to people in situ.

Further Reading


References


Kompas, T., Pham, V. H., & Che, T. N. (2018). The effects of climate change on GDP by country and the global economic gains from complying with the Paris Climate Accord. *Earth’s Future, 6*(8), 1153-1173.


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**Notes**

1. Unless otherwise specified all comparisons in this paper of wages across countries are deflated by PPP and hence reflect consumption, not product wages.


3. Milanovic (2013) uses income/consumption (not wage) data to show that more than two thirds of total household/individual global inequality in 2000 is due to differences across countries, and only a third to the differences in the income distribution within countries.

4. Since the United States includes data on the timing of one’s arrival in the country, separate coefficients for schooling in the United States versus schooling in Nigeria can be produced, so the quality of schooling in Nigeria for producing wage gains in the United States (including both academic quality and labor market-specific factors—the “melt” of human capital) is already taken into account.
5. All of these wage ratios are estimated in PPP. As noted in the subsection “Wages by Level of Schooling,” this can substantially understate the gains to moving if some significant fraction of earnings is spent in the home country (e.g., remittances, savings) where prices are (much) cheaper.


7 Labor compensation isn’t “(net) money wages,” as labor compensation includes costs to firms like benefits and taxes and other costs, but for simplicity we will refer to this as “wages.”

8. These calculations use data from the countries that have A data in the PWT91 (which is 117) and then loses some countries as they do not have World Bank learning data (four countries), as well as high-income oil countries (e.g., Saudi Arabia, Kuwait) as they are economically quite distinct (six countries) (and it mostly doesn’t matter, as the authors report results for the bottom third and the OECD). Hence the sample includes 107 countries.


10. Mita is a forced-labor system imposed by the Spanish government in Peru and Bolivia between 1573 and 1812

11. Andrews et al. (2017) constructed the Capability Index using cross-national measures of quality of government and selected indicators from: (1) the failed state index, and (2) world governance indicators. Hence, capability is characterized broadly to include: law and order, bureaucratic quality, corruption control, public services/government effectiveness (rating infrastructure, education and health systems, policing). The Capability Index allows categorizing countries by: (a) capability level: very weak, weak, middle, strong; and (b) capability growth rate over time: rapid negative, negative, positive, rapid positive.

12. And the outward mobility is often from “good institution” places like the Midwest, not just from “bad institution” places like the rural Deep South.

13. The five countries are: Ethiopia, India, Pakistan, Ghana, and Peru. This excludes Honduras, where the livestock asset transferred to the poor was chickens and most of them died of a disease. Conceptually, this country should not be excluded in estimating the ex ante distribution of program impact as this is a real risk. It is also worth mentioning that an impact evaluation of the same type of livestock asset transfer program in a different state of India had no impact as the local economy was growing and the returns to moving to jobs were higher than accepting and tending additional livestock (Murdoch South Asia program India). Hence, all the calculations are generous (upward-biased) estimates of “average” program impact.

14. It is important to note that the very low estimates of Docquier et al. (2015) of the effect on world GDP of liberalization of labor flows stems not from differences in the magnitudes of wage differentials nor are based on productivity differentials. Their semi-elasticity of world GDP with respect to movement is very close to those of other estimates. Their estimates are low because their estimation, using the Gallup data, of the “incompressible” costs of migration imply that relatively few people would move even if wage differentials were large.
Macroeconomics of the Euro