

Economics and Emigration: Trillion-Dollar Bills on the Sidewalk?

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What is the greatest single class of distortions in the global economy? One contender for this title is the tightly binding constraints on emigration from poor countries. Yet the effects of these distortions are little studied in economics. Migration economics has focused elsewhere—on *immigration*, how the movement of people affects the economies that receive migrants—while the effects of *emigration* go relatively neglected.

Vast numbers of people in low-income countries want to emigrate from those countries but cannot. The Gallup World Poll finds that more than 40 percent of adults in the poorest quartile of countries “would like to move permanently to another country” if they had the opportunity, including 60 percent or more of adults in Guyana and Sierra Leone (Pelham and Torres, 2008; Torres and Pelham, 2008). Emigration is constrained by many forces, including credit constraints and limited information at the origin (Hatton and Williamson, 2006). However, policy barriers in the destination countries surely play a major role in constraining emigration. The size of these constraints is apparent in the annual U.S. Diversity Visa Lottery, which allocates permanent emigration slots mainly to developing countries. In fiscal year 2010, this lottery had 13.6 million applications for 50,000 visas (U.S. Department of State, 2011)—272 applicants per slot. Many other potential destinations, such as Japan, restrict migration more than the United States.

How large are the economic losses caused by barriers to emigration? Research on this question has been distinguished by its rarity and obscurity, but the few estimates we have should make economists’ jaws hit their desks. When it comes

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to policies that restrict emigration, there appear to be trillion-dollar bills on the sidewalk. The first section of this paper reviews existing estimates of the global gains from the reduction of migration barriers. The gains to eliminating those barriers amount to large fractions of world GDP—one or two orders of magnitude larger than the gains from dropping all remaining restrictions on international flows of goods and capital. These estimates are sensitive to assumptions, and in the following sections I discuss the (limited) available research on four kinds of assumptions that underlie these estimates: how migrants affect nonmigrants, the shape of labor demand, the effect of location on productivity, and the feasibility of greater migration flows. These kinds of questions are not the primary focus of the traditional research agenda in migration economics; at the end of the paper, I speculate about why, and propose a new research agenda.

Estimates of the Gains From Reducing Migration Barriers

Researchers have built models of the world economy to estimate the gains from eliminating various barriers to trade, capital flows, and migration. Table 1 summarizes several recent estimates for policy barriers to trade, and (to my knowledge) all existing estimates for barriers to capital flows and migration. Even without delving into the details of these studies, the overall pattern is unmistakable and remarkable: The gains from eliminating migration barriers dwarf—by an order of a magnitude or two—the gains from eliminating other types of barriers. For the elimination of trade policy barriers and capital flow barriers, the estimated gains amount to less than a few percent of world GDP. For labor mobility barriers, the estimated gains are often in the range of 50–150 percent of world GDP.

In fact, existing estimates suggest that even small reductions in the barriers to labor mobility bring enormous gains. In the studies of Table 1, the gains from complete elimination of migration barriers are only realized with epic movements of people—at least half the population of poor countries would need to move to rich countries. But migration need not be that large in order to bring vast gains. A conservative reading of the evidence in Table 2, which provides an overview of efficiency gains from partial elimination of barriers to labor mobility, suggests that the emigration of less than 5 percent of the population of poor regions would bring global gains exceeding the gains from total elimination of all policy barriers to merchandise trade and all barriers to capital flows. For comparison, currently about 200 million people—3 percent of the world—live outside their countries of birth (United Nations, 2009).

Should these large estimated gains from an expansion of international migration outrage our economic intuition, or after some consideration, are they at least plausible? We can check these calculations on the back of the metaphorical envelope. Divide the world into a “rich” region, where one billion people earn \$30,000 per year, and a “poor” region, where six billion earn \$5,000 per year. Suppose emigrants from the poor region have lower productivity, so each gains just 60 percent of the simple

Table 1
Efficiency Gain from Elimination of International Barriers
(percent of world GDP)

<i>All policy barriers to merchandise trade</i>	
1.8	Goldin, Knudsen, and van der Mensbrugge (1993)
4.1	Dessus, Fukasaku, and Safadi (1999) ^a
0.9	Anderson, Francois, Hertel, Hoekman, and Martin (2000)
1.2	World Bank (2001)
2.8	World Bank (2001) ^a
0.7	Anderson and Martin (2005)
0.3	Hertel and Keeney (2006, table 2.9)
<i>All barriers to capital flows</i>	
1.7	Gourinchas and Jeanne (2006) ^b
0.1	Caselli and Feyrer (2007)
<i>All barriers to labor mobility</i>	
147.3	Hamilton and Whalley (1984, table 4, row 2) ^c
96.5	Moses and Letnes (2004, table 5, row 4) ^c
67	Iregui (2005, table 10.3) ^{c,d}
122	Klein and Ventura (2007, table 3) ^e

^a These studies assume a positive effect of trade on productivity; the other trade studies assume no effect.

^b Change in consumption rather than GDP.

^c Assumes two factors of production, immobile capital, and no differences in total factor productivity. Estimates from Hamilton and Whalley and from Moses and Letnes cited here assume no differences in inherent productivity of migrants and nonmigrants. Some much smaller estimates in Moses and Letnes assume that poor-country emigrants at the destination are $\frac{1}{5}$ as productive as nonmigrants at the destination, which (as the authors note in their footnote 12) is certainly extremely conservative.

^d Computable general equilibrium (CGE) model.

^e Assumes three factors of production and international differences in total factor productivity in a dynamic growth model.

earnings gap upon emigrating—that is, \$15,000 per year. This marginal gain shrinks as emigration proceeds, so suppose that the *average* gain is just \$7,500 per year. If half the population of the poor region emigrates, migrants would gain \$23 trillion—which is 38 percent of global GDP. For nonmigrants, the outcome of such a wave of migration would have complicated effects: presumably, average wages would rise in the poor region and fall in the rich region, while returns to capital rise in the rich region and fall in the poor region. The net effect of these other changes could theoretically be negative, zero, or positive. But when combining these factors with the gains to migrants, we might plausibly imagine overall gains of 20–60 percent of global GDP. This accords with the gasp-inducing numbers in Tables 1 and 2.

This calculation suggests a different kind of sanity check on the global estimates: comparing the price wedges caused by different types of international barriers. If the gains from eliminating barriers to labor mobility are greater than all remaining

Table 2

Efficiency Gain from Partial Elimination of Barriers to Labor Mobility

	<i>Removal of barriers</i>	<i>Net emigration rate (% origin-region population)</i>	<i>Efficiency gain (% world GDP)</i>
Moses and Letnes (2004, 2005)	Complete	73.6	96.5
	Partial	29.3	54.8
	Partial	10.3	22.0
Iregui (2005)	Complete	53	67
	Partial	24	31
Klein and Ventura (2007)	Complete	>99	122
	Partial	14.8	20
	Partial	7.3	10
Walmsley and Winters (2005)	Partial	0.8	0.6
	Partial	1.6	1.2
van der Mensbrugge and Roland-Holst (2009)	Partial	0.8	0.9
	Partial	2.0	2.3

Notes: The Moses and Letnes figures on emigration rates from are from Moses and Letnes (2005) table 9.3; figures on efficiency gains are from Moses and Letnes (2004) table 9, scaled to assume equal inherent labor productivity across countries (for example, 10 percent elimination of wage gap gives \$774 billion gain in table 9, multiplied by the ratio 96.5/9.6 in table 5 to equalize inherent labor productivity, and divided by world GDP gives 22 percent). Iregui (2005) figures are from tables 10.3, 10.6, 10.8, and 10.9. Klein and Ventura (2007) figures are from tables 2 and 7 (emigration rates calculated from population allocations given 80 percent initial population allocation to poor region). Walmsley and Winters (2005) figures from tables 4 and 11, assuming 80 percent of world population starts out in (net) migrant-sending countries. Van der Mensbrugge and Roland-Holst (2009) figures come from tables 6 and 7, and likewise assume 80 percent of world population starts out in (net) migrant-sending countries. World GDP in 2001 is taken to be \$32 trillion, doubling (in 2001 dollars) to \$64 trillion by 2025.

gains from eliminating barriers to trade and capital flows, we should expect to see proportionately greater international price wedges between different labor markets than between different goods and capital markets. In fact, this pattern is exactly what we see. Typical international trade costs, up to and including the border—not just policy barriers but *all* barriers, including distance, language, currency, and information—are the rough equivalent of a 74 percent ad valorem tariff, according to Anderson and van Wincoop (2004, p. 692)¹; price wedges between the same goods in different national markets are also of this magnitude (for example, Bradford and Lawrence, 2004). For identical financial instruments, Lamont and Thaler (2003) find that the price rarely differs across the globe by more than 15 percent. Both these wedges look small next to the global price wedges for equivalent labor. In Clemens, Montenegro, and Pritchett (2008), we document gaps in real earnings for

¹ This includes only international trade costs and excludes domestic distribution and retailing costs behind the border.

observably identical, low-skill workers exceeding 1,000 percent between the United States and countries like Haiti, Nigeria, and Egypt.² Our analysis suggests that no plausible degree of unobservable differences between those who migrate and those who do not migrate comes close to explaining wage gaps that large.

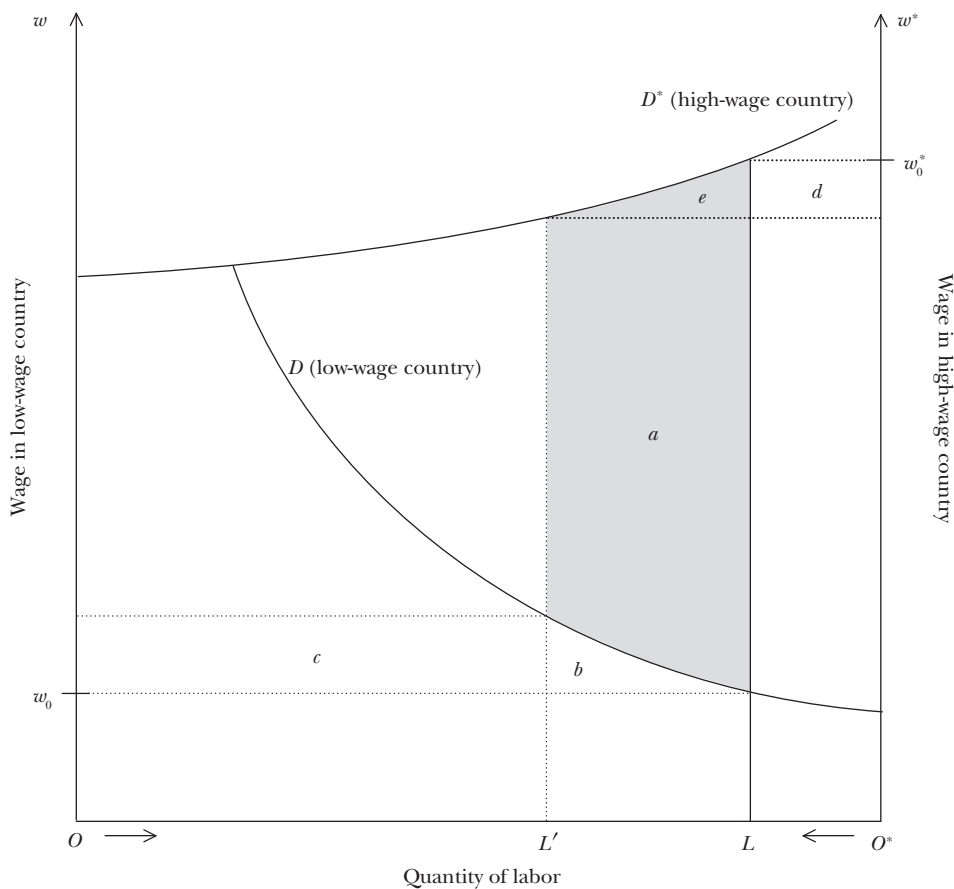
All of this suggests that the gains from reducing emigration barriers are likely to be enormous, measured in tens of trillions of dollars. But of course, the exact magnitudes of the estimates in Tables 1 and 2 are highly sensitive to modeling assumptions. For convenience, I will refer to the studies by their initials: Hamilton and Whalley (1984) [*HW*], Moses and Letnes (2004, 2005) [*ML*], Iregui (2005) [*I*], Klein and Ventura (2007) [*KV*], Walmsley and Winters (2005) [*WW*], and van der Mensbrugge and Roland-Holst (2009) [*VR*]. The backbones of these studies vary from a static partial equilibrium model (*HW* and *ML*), to a static computable general equilibrium model (*I*, *WW*, *VR*), to a dynamic growth model (*KV*). Some have two factors, labor and immobile capital (*HW*, *ML*, *I*), and some allow mobile capital plus third factors and international differences in total factor productivity (*KV*, *WW*, *VR*). Some include extensions that differentiate between skilled and unskilled labor (*KV*, *I*, *WW*, *VR*). Differences among the models' conclusions hinge critically on how the effects of skilled emigration are accounted for; the specification and parameters of the production function (and thus the elasticities of supply and demand for labor); assumptions on international differences in the inherent productivity of labor and in total factor productivity; and the feasible magnitude of labor mobility.³ Assumptions on the mobility of other factors matter a great deal as well; in *KV* the majority of global efficiency gains from labor mobility require mobile capital to "chase" labor—as described by Hatton and Williamson (1994).

To understand what underlies these various estimates of the gains from greater labor mobility, we need better information about at least four features of these models: 1) What are the external effects of (especially skilled) emigrants' departure on the productivity of non-emigrants? Many of the above estimates rest on the assumption that this effect is small or nil. 2) What is the elasticity of labor demand, in the origin and destination countries? Are these studies getting it about right? 3) How much of international differences in productivity depend on workers' inherent traits—accompanying them when they move—and how much depends on their surroundings? Is productivity mostly about who you are, or where you are? 4) Finally, given the many barriers that prevent emigration today, what future level of emigration is feasible?

² Here, "observably identical" means 35 year-old urban males with 9–12 years of education, born and educated in the country of origin.

³ Some of the estimates in the original papers assume that workers from the poorest countries working in rich countries are inherently one-third as productive (Hamilton and Whalley, 1984) or one-fifth as productive (Moses and Letnes, 2004) as workers born in rich countries. This assumption seems extraordinarily conservative (as Moses and Letnes point out in their footnote 12). The model of Klein and Ventura (2007) is extended to describe the transition to steady state in Klein and Ventura (2009).

Figure 1
Determinants of the Gains from Emigration



Note: The migrants gain welfare corresponding to area $a + b$. In the low-income country, labor gains area c ; owners of other factors (say, capital and land) lose area $b + c$. In the high-income country, labor loses area d ; owners of other factors gain area $d + e$. In sum, the global welfare gain is $a + e$, the shaded area.

Figure 1 illustrates the importance of the answers to these questions. Following Bhagwati (1984), consider a world with two countries—one with low wages and one with high wages. Wages for the low-wage country are on the left-hand vertical axis, and the quantity of labor in that country is measured from left to right, from O to L . The labor demand curve there is D . Wages for the high-wage country are measured on the right-hand vertical axis, and quantity of labor in the high-wage country is measured right to left from O^* to L . The labor demand curve there is D^* . Thus, world labor supply is the entire length of the horizontal axis OO^* . Initial wages in each country are w_0 and w_0^* . If completely free migration were allowed, the wage rate between the two countries would equalize at the point where the labor demand

curves intersect. An increase in migration—say by having the workers between L and L' migrate from the low-income to the high-income country—leads to a relatively small decrease in the wage rate for the high-income country, a relatively small rise in the wage rate for the low-income country, and a large rise in income for the migrants themselves. Migrant workers gain; nonmigrant workers gain in the low-income country and lose in the high-income country; owners of other factors (such as capital and land) lose in the low-income country and gain in the high-income country. Adding up these welfare effects, global welfare rises by an amount corresponding to the shaded area of the figure.

The figure makes it clear how the answers to the four questions above shape the size of the global welfare gain. If emigration exerts negative externalities on nonmigrants, the gain could be offset by consequent downward shifts in both demand curves. If labor demand becomes highly elastic at the origin or at the destination, the gain shrinks. To the extent that emigrants are *inherently* less productive than nonmigrant workers at the destination, the true demand curve for their labor lies further below D^* , and the gain also shrinks. Finally, as the size of feasible migration shrinks, L' gets closer to L , and once again, the gain shrinks.

In the following sections, I will consider each question in turn. I argue that, for most of them, economists need much more evidence than we have, but that the existing evidence gives us little reason to believe that the numbers in Tables 1 and 2 greatly overstate the gains to lowering migration barriers.

Question 1: What are the External Effects of Migrants on Nonmigrants?

Begin with the country of origin. The departure of some people—such as the skilled or talented—from a poor country might reduce the productivity of others in that country. Such an effect would tend to offset the gains from emigration. Externalities like these are often assumed to be so pervasive that the literature refers to skilled migration with a pejorative catchphrase—“brain drain”—embodying the assumption. (To see why economists should avoid this term, picture reading a journal article on female labor force participation that calls it the “family abandonment rate.”) In this issue, Gibson and McKenzie review and critique this literature.

But it is not well-established under what conditions the emigration of skilled workers results in a net depletion, in equilibrium, of the stock of skilled workers in the origin country. Mountford (1997), Stark, Helmenstein, and Prskawetz (1997), and a subsequent literature theorize that when emigration to high-wage countries becomes possible, even when it is costly and uncertain, the expected value of human capital rises for all potential migrants. Because not all of those who were thus encouraged to invest will leave, the existence of an emigration option for some people can tend to raise the human capital stock at home. Macro and micro studies suggest that this effect is real and large enough to substantially offset the departures

in some settings (Beine, Docquier, and Rapoport, 2008; Chand and Clemens, 2008; Batista, Lacuesta, and Vicente, 2011; Docquier and Rapoport, forthcoming).

But for the sake of argument, consider cases where skilled emigration unambiguously lowers the stock of human capital at the origin. A broad theoretical literature posits that human capital externalities shape the development of poor countries (for example, Romer, 1990; Kremer, 1993; Lucas, 1988). If positive human capital externalities are real and large, it is possible that the depletion of human capital stock via emigration inflicts negative externalities on nonmigrants. However, these externalities have proven difficult to observe, their theoretical basis remains unclear, and their use to justify policy remains shaky. I will discuss each of these in turn.

Human capital externalities are, it turns out, hard to locate and measure in the wild. The most commonly cited example of externalities that emigrants might impose on those remaining in the origin country involves healthcare workers. But if human capital externalities from health workers were a first-order determinant of basic health conditions, African countries experiencing the largest outflows of doctors and nurses would have systematically worse health conditions than other parts of Africa. In fact, those countries have systematically better health conditions (Clemens, 2007). More broadly, if the external effects of schooling were major and straightforward determinants of economic development, the vast increases in schooling levels across the world since 1960 would have been accompanied by a substantial rise in total factor productivity. As Pritchett (2001) points out, nothing like that happened in poor countries.

These facts do not negate the existence of human capital externalities. But they do suggest that externalities from national stocks of human capital per se—all else equal—might be small enough for their effects to be swamped by other forces.

Furthermore, the theoretical mechanisms of human capital externalities are poorly established in the literature, which has few conclusions in general and even fewer for developing countries (Ciccone and Peri, 2006). Potential mechanisms for human capital externalities include knowledge spillovers, research and development, physical health, political leadership, fertility, and capital accumulation (for example, Meng and Ye, 2009; Canton, 2009; Spilimbergo, 2009). Docquier, Özden, and Peri (2010) find that the effects of emigration on nonmigrants depend critically on the assumed mechanisms of human capital externalities. If economists are to understand, measure, or predict the external effects of emigration, they require greater clarity about these mechanisms.

For example, the external effects of emigration on nonmigrants depend crucially on the spatial extent of human capital externalities, which is poorly understood. If the external effects of human capital act over short distances—such as a doctor's care—the effects on nonmigrants from the emigration of human capital per se might be small. Most doctors in many African countries already work in cities (and in nicer neighborhoods), so the marginal effect of their international emigration on people in rural areas and slums could be limited (Clemens, 2009). On the other hand, if the external effects of human capital act over large distances—such as by the generation of public goods like new ideas that can flow back home—this too diminishes the external effects of emigration per se (Kerr, 2008).

Here is another example of the need for greater clarity: Let us assume that greater inequality has negative external effects, perhaps by fostering crime and mistrust. It is conceivable that emigration could exert external effects on nonmigrants by raising inequality—if for instance a few families with relatives abroad get big remittance flows. But if this is an argument for restricting migration, it proves a little too much. Inequality of remittance income would be lower if no families had this opportunity *or* if all families had this opportunity. So it is not clear whether a little emigration is “too much” or “too little” by this criterion. Furthermore, if a man from Morocco triples his income by moving to France, the effects on inequality of outcomes within Morocco and within France are both contingent: they depend on where in Morocco’s income distribution he came from, and where in France’s income distribution he goes to. But the effects on inequality of outcomes for France and Morocco *collectively* almost certainly declines. Does international inequality exert worse external effects than domestic inequality? Both theory and evidence are lacking here.

Even if the mechanism and magnitude of these external effects were crystal-clear, there are important concerns about policy instruments that would address the externalities by limiting or taxing migration flows: whether it is practical to set correct Pigovian taxes, statically or dynamically, and the extent to which the assumptions justifying Pigovian taxes hold here.

An economic case for emigration taxes or restrictions based on human capital externalities would require fabulous amounts of information. A social planner selectively restricting skilled emigration from each country would need a vast database of domestic labor market conditions for hundreds of skilled occupations, as well as reliable estimates of the socioeconomic externalities conveyed by those of different professions in different locations at different times: entrepreneurs, nurses, engineers, and others.⁴ In practice such restrictions end up indiscriminate—such as the policy of the United Kingdom’s National Health Service to ban recruitment from most developing countries, regardless of conditions there. The optimal future timepath of such restrictions is even less clear. Should the emigration of skilled workers from the Democratic Republic of the Congo, one of the poorest countries on Earth, be “temporarily” restricted so that those workers’ positive externalities somehow spark growth

⁴ Ng (2004) makes this argument formally: In the presence of an externality that cannot be directly removed, achieving a second-best welfare outcome via intervention (such as a Pigovian emigration tax) can be impossible if: 1) there are multiple substitute/complement relationships of unknown direction and sign among different goods in the market; and 2) the costs of administering the intervention are large. Ng (p. 202) does argue that a probabilistic “third-best” outcome may be achievable if there is at least good information on the size and sign of the original externality, but poor information on the size and sign of other relationships in the economy. Thus, if we held diffuse priors about the harm to emigrants from restricting emigration, but narrow priors about the positive effects on non-emigrants from restricting emigration, a probabilistic third-best outcome might be achievable by emigration restrictions. But regulating emigration based on human capital externalities faces the opposite situation: the magnitude of the harm to potential emigrants from emigration restrictions is clearly negative and can be large, whereas the size and sign of the human capital externalities that the intervention seeks to correct are theoretically and empirically uncertain.

there? Even if that happened—for which there is no clear economic evidence—such “temporary” restrictions might need to last for centuries before Congo caught up to the destination countries and the restrictions became unnecessary.⁵

What is more, policy measures to tax or limit emigration often rest on assumptions that bear closer examination. In one of the best-known proposals, Bhagwati and Dellalgar (1973) argue for a Pigovian tax on skilled emigration, to compensate their countries of origin (for additional discussion, see Bhagwati and Hamada, 1974; Wilson, 2008). Their argument is that skilled workers convey a positive externality on other workers in the same country, such as facilitating the adoption of foreign technologies. Their argument does not rest on fiscal effects, such as the loss of public subsidies for tuition spent on those who later emigrate, but rather on the pure external effects of having smart and talented people nearby.

There are at least two fundamental problems with this idea. First, it assumes that skilled labor emigration is not already taxed. But many skilled workers face binding migration restrictions that are the economic equivalent of large taxes. The United States strictly rations its visas for temporary and permanent employment-based skilled migration, especially from large countries like India, and most physicians from the developing world face large nonvisa migration barriers such as the requirement to repeat medical residency for U.S. licensing. Just as nontariff trade barriers have a tariff equivalent, quotas and licensing restrictions on the movement of skilled workers have a migration tax equivalent. International gaps in real earnings for high-skill workers are very high: 500–1,000 percent for some professors, computer programmers, and health workers (Clemens, 2009). Even if only a small fraction of these gaps is due to policy restrictions, the economic equivalent of a large emigration tax is already broadly applied.

Second, Coase (1960) taught us that the mere existence of an externality does not imply that a Pigovian tax maximizes welfare. In a world of transaction costs and externalities, welfare is maximized if the property right is assigned to the party with a higher cost of reducing the externality. Requiring skilled emigrants to pay a tax, rather than requiring non-emigrants to pay potential emigrants to stay, assigns ownership of emigrants’ positive externalities to non-emigrants. But in settings of skilled emigration, it is not obvious who bears the higher cost. Taxing an emigrant Filipino registered nurse at 10 percent of foreign income per year for 10 years might raise \$40,000.⁶ But the all-inclusive cost of eliminating the external effect by a different

⁵ Real per capita income in the United States is now 150 times real per capita income in the Democratic Republic of the Congo (as measured by the World Bank at purchasing power parity). Assume (heroically!) that human capital externalities are enormous, so that restricting emigration from Congo could raise its real per capita growth rate from roughly zero to about 4 percent per year. Given that real per capita growth in the United States is historically about 2 percent per year, it would take about two and a half centuries before emigration restrictions became unnecessary.

⁶ Bhagwati and Dellalgar (1973) suggest a tax of 10 percent on the after-U.S.-tax income of skilled emigrants from low-income countries working in the United States. Assuming an annual income in the United States of \$60,000 and U.S. tax rate of 30 percent, such an emigration tax would yield \$42,000 over 10 years.

route—training a new registered nurse in the Philippines—can be less than \$12,000. The former policy might be globally impoverishing relative to the latter.

Further complications arise from the implicit assumption that non-emigrants hold property rights in the positive externalities of skilled migrants. If non-emigrants own these rights, do they also own any negative externalities the emigrants would have provided by staying—like contributions to urban congestion or to pollution? And who decides whose positive externalities are owned by whom? Presumably, an American doctor's decision not to provide care in Haiti causes the same loss of positive externality to Haitians as a Haitian doctor's decision to leave Haiti, but few would consider taxing the American doctor's decision.

So far I have discussed externalities at the origin, as this piece seeks to shine light on emigration rather than immigration. But I now turn briefly to the issue of negative externalities imposed on people who already live at the destination when migrants arrive there. The arrival of migrants could, for example, decrease the availability of unpriced public goods at the destination like open space, clean air, publicly-funded amenities, and a degree of cultural homogeneity that may be valued by nonmigrants. These too would tend to reduce the global welfare gains of greater mobility.

Economics knows little about the mechanisms and magnitudes of such externalities at the destination, particularly under large-scale emigration. These deserve study. But there is little reason at present to think that they would greatly alter the message of Tables 1 and 2. First, the literature contains no documented case of large declines in GDP or massive declines in public-service provision at the destination caused by immigration. Second, century-old issues of the *American Economic Review* and the *Journal of Political Economy* extensively discuss concerns that any further emigration might degrade the American economy and society (for example, Hall, 1913; Kohler, 1914). Since then the American population has quadrupled—with much of the rise coming from increasingly diverse immigration to already settled areas—and the United States remains the world's leading economy, with much greater availability of publicly-funded amenities than a century ago. Third, there are also many plausible positive externalities from increased immigration. These include spatial aggregation economies in high-skill labor (for example, Glaeser and Maré, 2001) and the effects of low-skill labor availability on the productivity of high-skill labor, particularly women's labor (for example, Kremer and Watt, 2009; Cortes and Tessada, forthcoming). Fourth, all serious economic studies of the aggregate fiscal effects of immigration have found them to be very small overall—small and positive at the federal level (Auerbach and Oreopoulos, 1999; Lee and Miller, 2000), small and negative at the state and local level (Congressional Budget Office, 2007).

Here again, even if we had solid evidence that immigration exerted clear and large net negative externalities to those at the destination, an economic justification for internalizing those externalities with quotas or taxes would face unanswered questions. If people's taste for cultural homogeneity justifies limits on immigration from abroad, could a taste for cultural homogeneity also justify blocking certain

kinds of internal migration to a neighborhood or city that has, to its current residents, a pleasing degree of cultural homogeneity? This raises further difficult issues of the extent to which preferences for homogeneity are endogenous to exposure, so that greater immigration might alter the pre-existing preference. And returning to the issue of property rights, any taxes or quotas to internalize the negative externalities of immigration at the destination require an assumption: that migrants are responsible for their *negative* externalities at the destination. But as I have discussed above, some economists assume that migrants' home countries—not the migrants themselves—own migrants' *positive* externalities at the origin. It is doubtless possible, with some effort, to lay out a theory in which societies own the positive externalities of migrants in their origin country but societies are not the owners of *negative* externalities of migrants in their destination country. The economics literature has barely begun to address these issues.

In short, there is little in the admittedly scanty literature so far to support the notion that externalities from labor mobility would greatly affect the global welfare estimates presented earlier in this paper.

Question 2: What is the Elasticity of Labor Demand at the Origin and Destination?

Economists studying international migration have given much of their energy to estimating how the movement of emigrants affects the wages of nonmigrants. For example, does the arrival of immigrants lower wages for incumbent workers? In a U.S. context, Borjas (2003) and Borjas and Katz (2007) argue that low-wage workers do experience a modest decline in nominal wages from immigration. On the other side, Card (2009) and Ottaviano and Peri (forthcoming) find that millions of recent immigrants to the United States have caused the average worker's nominal wages to decline a few percent—if at all—while Cortes (2008) finds that immigration lowered the price of a typical consumption basket about half of 1 percent. The mass migrations of the nineteenth century likely caused a cumulative decline of 1 or 2 percentage points each decade in wages at the destination (Hatton and Williamson, 1994).

Conversely, does the departure of emigrants raise the wages of non-emigrants in the origin country? Mishra (2007) finds that the vast emigration of Mexicans to the United States between 1970 and 2000 may have caused an 8 percent increase in Mexicans' nominal wages in Mexico. Economic historians have evidence that comparable increases in home wages were caused by mass emigration from Sweden (Karlström, 1985) and Ireland (Hatton and Williamson, 1993; O'Rourke, 1995).

These estimates are roughly in line with the elasticities used in the global welfare estimates of Tables 1 and 2. For example, in the model of Moses and Letnes (2004), a 10 percent removal of emigration barriers generates a 3–4 percent increase in wages for non-emigrants at the origin, and a 2.5 percent decline at the destination. Even substantial adjustment of these elasticities is unlikely to alter the estimated efficiency gains a great deal.

Of course, these elasticities could be different at much higher levels of emigration. The literature gives no clear support for such a pattern, however, even under greatly increased migration. In historical cases of large reductions in barriers to labor mobility between high-income and low-income populations or regions, those with high wages have not experienced a large decline. For example, wages of whites in South Africa have not shown important declines since the end of the apartheid regime (Leibbrandt and Levinsohn, 2011), despite the total removal of very large barriers to the physical movement and occupational choice of a poor population that outnumbered the rich population six to one. The recent advent of unlimited labor mobility between some Eastern European countries and Great Britain, though accompanied by large and sudden migration flows, has not caused important declines in British wages (Blanchflower and Shadforth, 2009).

Further, even if emigrants modestly depress wages when they arrive at the destination, this does not justify restricting movement by the standard welfare economics analysis. Such effects represent “pecuniary” externalities rather than “technical” externalities. The human capital externalities discussed in the previous section, along with common examples like belching smokestacks, are examples of technical externalities. Pecuniary externalities, in contrast, operate through the price mechanism: for example, my decision not to place a bid on the house you are selling may lower the price you can receive from an alternative buyer. Pecuniary externalities are a near-universal feature of economic decisions. In standard economic analysis, they offer no welfare justification for taxation or regulation of those decisions.⁷

For example, research on domestic labor movements has found—to the surprise of few—that movement of labor from one city to another tends to modestly lower wages at the destination (Boustan, Fishback, and Cantor, 2010), and that the entry of women into the labor force can modestly lower men’s wages (Acemoglu, Autor, and Lyle, 2004). However, no economist would argue that these facts alone signify negative externalities that reduce social welfare and should be adjusted with a Pigovian tax on those who move between cities or on women entering the workforce, because these externalities seem to be almost purely pecuniary. Similarly, economists would be virtually unanimous against imposing a tax on new domestic competitors on the grounds that they imposed costs on existing firms, because again such externalities are pecuniary. Of course, this argument need not imply that policies to help low-wage U.S. workers in some manner are socially undesirable, only that such policies should be based on concerns over equity or building human capital, rather than on standard efficiency justifications.

⁷ This classification can be subtle. For example, McKenzie and Rapoport (forthcoming) find that some children in Mexico drop out of high school as a result of the emigration of a household member. Should we treat this finding as an externality? If the behavior occurs because those children see that the returns to emigration are much higher than the returns to education, then their behavior need not impose an externality. However, a negative externality could arise if high school graduates in Mexico convey substantial positive technical externalities on other workers. The literature has not established the theoretical extent or empirical magnitude of such externalities.

Question 3: Is Labor Productivity Mostly about Who You Are, Or Where You Are?

Existing estimates of the efficiency gains from greater emigration hinge on a critical assumption: How productive will migrants be at the destination? Many have low productivity where they now are, in poor countries. How much of that low productivity moves with them? Klein and Ventura (2007) assume that migrants' productivity is about 68 percent of the productivity of nonmigrants at the destination; Moses and Letnes (2004) estimate scenarios where this number is anything from 20 to 100 percent. The assumption has first-order effects on their efficiency estimates.

We can observe the earnings of today's migrants in destination countries to learn something about their productivity. The question is how these marginal migrants would differ from average migrants under larger-scale migration. Future migrants could be more positively or more negatively selected than today's migrants, with regard to observable or unobservable determinants of productivity. Selection on observable traits is easier to measure; selection on unobservables is harder.

This question is closely linked to the question of whether international differences in productivity are explained by differences in people or differences in places. If an emigrant to a rich country is more productive than an observably identical worker who stayed in a poor country, there are two broad explanations. One is that the emigrant is different in unobservable ways from the non-emigrant; this argument is compatible with a high degree of positive selection in migration and little influence of location itself on productivity. The other is that the emigrant is not that different in unobservable ways from the non-emigrant; this argument is compatible with less positive selection in emigration, and a large effect of location on productivity.

Empirical work is beginning to attack this problem. One approach is to use more sophisticated structural models to account for selection in macroeconomic estimations (Ortega and Peri, 2009; Grogger and Hanson, 2011). A complementary approach is to find natural experiments that identify the extent of selection on unobservables in microeconomic settings (summarized by McKenzie and Yang, 2010). Examples of the latter include McKenzie, Gibson, and Stillman (2010), who use a naturally randomized visa lottery to show that the gains from emigrating from Tonga to New Zealand are only somewhat lower than the simple wage difference for observably identical workers inside and outside Tonga—in other words, there is little unobserved self-selection in those who emigrated. In Clemens (2010), I also use a naturally randomized visa lottery to show that large gains to overseas work experienced by Indian software workers cannot be primarily the result of unobserved positive self-selection in those workers.

While this literature is actively evolving, in no case has one of these recent and rigorous studies identified a country pair for which large differences in earnings across the border can be mostly accounted for by self-selection of workers (migrant or otherwise) who cross the border. Numerical simulations using U.S. census microdata on immigrants (Hendricks, 2002) and combined U.S. and foreign microdata (Clemens, Montenegro, and Pritchett, 2008) have shown that plausible degrees of

positive selection are insufficient to explain more than half the earnings gap between workers in the United States and observably identical potential migrants abroad. Sophisticated survey data do not find positive emigrant selection on unobservable determinants of earnings to much exceed 50 percent in any case that has been studied (Jasso and Rosenzweig, 2009; Kaestner and Malamud, 2010; Fernández-Huertas, 2011; Ambrosini, Mayr, Peri, and Radu, 2011).

These initial results accord well with an entirely separate macroeconomic literature (for example, Hall and Jones, 1999) which finds that most of the productivity gap between rich and poor countries is accounted for by place-specific total factor productivity, not by productivity differences inherent to workers. Large differences in location-specific total factor productivity mean that free movement of goods and capital cannot by themselves achieve the global equalization of wages, as they can in the most abstract trade models (O'Rourke and Sinott, 2004; Freeman, 2006, Kremer, 2006).

In other words, the existing evidence, preliminary and spotty though it is, gives no reason to believe that a better accounting for unobserved differences in the determinants of productivity between migrants and nonmigrants would greatly alter the preceding estimates of efficiency gains from greater labor mobility.⁸

Question 4: What Future Level of Emigration Is Feasible?

The extent of feasible emigration depends on the willingness of politicians at potential destinations, acting as agents for their electorates, to allow immigration. About 59 percent of people in the United Kingdom tell pollsters that there are currently “too many” immigrants; in Italy it is 53 percent, in the United States 37 percent, in France 33 percent, in Germany and the Netherlands 27 percent, and in Canada 17 percent (German Marshall Fund, 2010).

The global gains in aggregate economic welfare in Figure 1 mask the consequent redistribution between labor and other factors, and between labor at different levels of skill. If the median voter at the destination holds relatively little capital or skill, this could limit the willingness of citizens and politicians in that country to reduce impediments to emigration from poor countries. Noneconomic attitudes such as nationalism can also play an important role. Mayda (2006) finds that it is the wealthier, better-educated, and less-nationalist individuals in rich destination countries who have more favorable attitudes toward immigration.

⁸In fact, the emerging evidence on selection suggests that some of the estimates of gains from emigration are small because they are too conservative. Walmsley and Winters (2005), for example, assume in their base scenarios that migrants from low-productivity countries to high-productivity countries acquire only half of the difference in productivity between the two countries. That is, they assume that the difference in productivity between observed non-emigrants at the destination and observed non-emigrants at the origin overstates by 100 percent the true productivity effect of emigration on new emigrants. None of the existing estimates suggests that the magnitude of selection—in the limited cases that have been studied—is close to that large.

A substantial expansion of emigration may thus seem politically impractical, and within a time frame of a few years, this objection holds some force. But the global efficiency gains from even small relaxations of existing barriers to emigration are large relative to the gains from further relaxation of barriers to trade and capital flows (as shown earlier in Table 2).

Furthermore, economists should be open to the possibility that dramatic changes in what is practical can happen over several decades. After all, changes in geographic labor mobility that were unthinkable only a few decades ago have come to pass. Through the 1980s, a Polish national attempting to emigrate to West Germany could be shot by soldiers sealing the Inner German border from the east. Today, Polish jobseekers may move freely throughout Germany. The world has summarily discarded vast systems of restrictions on the labor mobility of medieval serfs, slaves, women, South African blacks, indigenous Australians, and a long list of others.

Even modest expansions of emigration have provided great benefits for large numbers of people. In Clemens and Pritchett (2008), my coauthor and I show that among the people born in Haiti, Mexico, or India who live above a (uniform, purchasing power parity–adjusted) international poverty line, large fractions live in the United States. For example, among Haitians who live either in the United States or Haiti and live on more than \$10/day measured at U.S. prices—about a third of the U.S. “poverty” line—four out of five live in the United States.⁹ Emigration from Haiti, as a force for Haitians’ poverty reduction, may be at least as important as any economic change that has occurred within Haiti.

Economic policy might help ease political constraints. Several economists have proposed policy mechanisms to compensate nonmigrants at the destination for declines in welfare, by charging immigrants a tax—the figure \$50,000 often comes up—or auctioning work permits (including Becker and Becker, 1997; Freeman, 2006; Orrenius and Zavodny, 2010; Fernández-Huertas and Rapoport, 2010). All of these, in one way or another, seek to minimize the number of “losers” from labor mobility by moving from the Kaldor–Hicks concept of optimality—which is that social gains are sufficient so that it would be potentially possible to compensate losers—toward Pareto optimality, in which parties that would otherwise be losers receive actual compensation. Mechanisms like this might alleviate some of the political constraints to greater labor mobility.

⁹That figure probably overestimates the effect of migration on Haitians’ poverty, since there is evidence of some positive selection of Haitian emigrants on observable and unobservable determinants of earnings (Clemens, Montenegro, and Pritchett, 2008). But even strong assumptions about highly positive selection would still leave emigration as the cause of a large fraction of the poverty reduction that has occurred for people born in Haiti. In addition, this estimate tends to understate the effect of emigration on poverty reduction—because it does not count Haitians who emigrated to countries other than the United States; because part of the poverty reduction for non-emigrants could have been caused by remittances, in turn caused by emigration; and because the estimate was made before a 2010 earthquake crushed Haiti’s economy.

A Research Agenda Whose Time Has Gone

The four questions above outline a research agenda on emigration with which I will conclude this paper. But first I want to contrast that new agenda with the old agenda in the literature we have now, and speculate about why that old agenda has dominated so far.

Start talking about the welfare benefits and costs of emigration at any research or policy meeting on migration and economic development, and the conversation turns quickly to two topics: “brain drain” and workers’ remittances. These have been the primary focus of research on the relationship between emigration and global economic development, and not coincidentally, they are the focus of the other two papers in this symposium. Both issues are important. Yet neither of these approaches shows much promise as a way of better understanding the global efficiency gains from greater emigration. I have already discussed the reasons why the literature on migration and human capital externalities has not yielded good reasons to alter existing estimates of the gains to greater emigration. I turn now to the literature on remittances.

Global flows of remittances are rising toward \$400 billion per year (Mohapatra, Ratha, and Silwal, 2011). This trend has helped to launch a large and valuable research literature, presented and discussed by Dean Yang in this issue. But remittances are typically a small fraction of emigrants’ foreign wage, especially for permanent emigrants (van der Mensbrugge and Roland-Holst, 2009). To a first approximation, remittances are intrahousehold transfers that cross borders, and the reasons that people send remittances (Rapoport and Docquier, 2006) are broadly the same as the reasons people make other intrahousehold transfers (Laferrère and Wolff, 2006). If a Mexican woman experiences an income gain from working in Mexico, the whole value of that gain adds to her household’s welfare—both the portion she consumes and the portion she shares with her husband. This social welfare calculation is unaffected if she experiences an income gain by stepping over the Mexican border into Texas.

In short, barriers to emigration have a first-order effect on welfare; any barriers to flows of remittances have only a second- or third-order effect on welfare. But the literature has gone into great detail about the smaller effects, frequently investigating whether the welfare effects of remittances are attenuated by any withdrawal of remittance recipients from the labor force (for example, Cox-Edwards and Rodríguez-Oreggia, 2008) or by any consequent appreciation of the sending-country exchange rate (for example, Amuedo-Dorantes and Pozo, 2004; Vargas-Silva, 2009).

Why has the literature focused so much more on the relatively small and uncertain effects of remittances and “brain drain” than on the relatively massive and likely global effects of migration—including the benefits for the migrants themselves? Perhaps many economists consider the estimates of efficiency gains in Table 1 to be self-evident and thus not worthy of much study. But the review above suggests we have much to learn about those estimates. I suspect the reason lies elsewhere.

Perhaps the literature focuses on remittances and “brain drain” because those effects more obviously pertain to national welfare than individual welfare.

Focusing on national welfare is a grand old tradition in economics—older, indeed, than the work of Adam Smith, who prominently inserted “the wealth of nations” into the title of his great book. Economists in the long-ago mercantilist tradition largely agreed that the goal of economic policy was to encourage national production and exports, to discourage imports, and thus to bring specie into the country. To this end, they advised encouraging manufactured exports and discouraging raw material exports, to generate domestic employment (Heckscher, 1935[1955]; Irwin, 1996, p. 34). The classic work of Furniss (1920, p. 8) draws out the implications of mercantilist assumptions for labor: Raising export revenue requires mass production at low cost. High manufacturing employment at low wages requires a moral duty for low-income people to work in manufacturing and a moral duty for high-income people to preferentially consume domestic manufactures. In this view, the emigration of labor only affects national welfare to the extent that it encourages or discourages home production and exports. The mercantilist writer Josiah Child (1668 [1751], pp. 146–7), for example, argued that any emigration to colonial plantations “is certainly a damage, except the employment of those people abroad, do cause the employment of so many more at home in their mother kingdoms, and that can never be, except the trade be restrained to their mother kingdom . . .”

Economic research carried out under this set of working assumptions would focus on estimating the extent to which emigration tends to raise origin-country production (such as through remittances, minus the consequent labor force withdrawal) or tends to lower origin-country production (such as through “brain drain”). Little attention would be reserved for the gains to migrants. This, in very broad strokes, describes the bulk of extant literature on the economic effects of emigration. I am far from the first to claim a link between the mercantilist tradition and economics’ preoccupation with suspected negative, within-country effects from emigration.¹⁰ After Furniss (1920, p. 54), this link has been explicitly made by Nobel laureate Ted Schultz (1978); Charles Kindleberger (1986) when he was president of the American Economic Association; and one of the great scholars of migration and economic growth, Brinley Thomas (1973, pp. 1–6); among many others.¹¹

¹⁰ Incidentally, the leading economic research on *immigration*—not the focus of this essay—looks quite different. Under mercantilist assumptions, any wage-depressing effects of immigration at the destination would raise that country’s welfare by increasing the competitiveness of its manufactured exports. But the centerpiece of the immigration literature is the anti-mercantilist suspicion of immigration’s deleterious effects on labor. I have no explanation for the difference other than to point out that the modern immigration literature was founded by microeconomic labor economists such as George Borjas and David Card. The modern literature on economic growth and development overseas, in contrast, was founded by researchers with a primarily macroeconomic focus such as Alexander Gerschenkron, Paul Rosenstein-Rodin, and Robert E. Lucas.

¹¹ Others include Hovde (1934), Hamilton (1940), Shepperson (1953), Middendorf (1960), O’Brien (1966), Letiche (1969), Jeremy (1977), Dowty (1986), Backhaus and Wagner (1987), Davis (1988), de Soto (1989, p. 201), and Mokyr and Nye (2007).

A New Research Agenda On Emigration

The available evidence suggests that the gains to lowering barriers to emigration appear much larger than gains from further reductions in barriers to goods trade or capital flows—and may be much larger than those available through any other shift in a single class of global economic policy. Indeed, “some big bills have not been picked up on the routes that lead from poor to rich countries” (Olson, 1996). Research economists, however, write relatively little about emigration. The term “international trade” is 13 times more frequent than “international migration” in all the published article abstracts contained in the Research Papers in Economics (RePEc) archive. Furthermore, economists focus on arrival, not departure: in RePEc, “immigration” is four times as frequent as “emigration.”

It should be a priority of economic research to seek a better characterization of the gains to global labor mobility and to investigate policy instruments to realize a portion of those gains. The four questions in this paper suggest one structure for that agenda. We clearly need a better theoretical and empirical understanding of human capital externalities; the dynamics of labor demand under large-scale migration flows; the magnitude and mechanisms of the effect of workers’ location on their productivity, relative to the effect of workers’ inherent traits on their productivity; and the policy instruments that might make greater labor mobility possible. Many of the outstanding questions are discussed by Pritchett (2006), Rosenzweig (2006), Hanson (2009), and Docquier and Rapoport (forthcoming).

Complicating the empirical portion of this agenda is the fact that even basic statistics on international migration are often unavailable to economists (Commission on International Migration Data for Development Research and Policy, 2009). Detailed statistics are either held confidential by governments or not collected at all, and publicly-released data can be a mess of incomparable time periods, modes of migration (temporary vs. permanent, entries vs. individuals, and other categorizations), and definitions of occupations. Just estimating bilateral stocks of migrants at a single point in time, even without any other information about those migrants, is a costly enterprise requiring heroic assumptions and massive imputation (Parsons, Skeldon, Walmsley, and Winters, 2007). Publicly available international migration statistics have roughly the quality of international trade statistics in the 1960s. As occurred in international trade, investing in migration data collection and compilation must be part of the research agenda.

If this additional research tends to confirm that barriers to emigration place one of the fattest of all wedges between humankind’s current welfare and its potential welfare—no doubt with a number of useful caveats—then understanding and realizing the gains from emigration deserve much more research priority. Emigration’s literature remains scattered; emigration’s *Wealth of Nations* unwritten; emigration’s Ricardo undiscovered.

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