

Human Capital and Development Accounting: New Evidence from Wage Gains at Migration

Lutz Hendricks
University of North Carolina

Todd Schoellman
Federal Reserve Bank of Minneapolis

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Human Capital and Development Accounting: New Evidence from Wage Gains at Migration*

Lutz Hendricks[†]

Todd Schoellman[‡]

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Abstract

We use new data on the pre- and post-migration wages of U.S. immigrants to measure the importance of human capital for development accounting. Wages increase at migration, but by less than half of the gap in GDP per worker. This finding implies that human capital accounts for a large share of cross-country income differences. Wage gains decline with education, consistent with imperfect substitution between skill types. We bound the human capital share in development accounting to between one-half and two-thirds; additional assumptions lead to an estimate of 60 percent. We also provide results on the importance of assimilation and skill transfer.

JEL Classification: O11, J31

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[†]University of North Carolina, Chapel Hill. E-mail: lutz@hendricks.org

[‡]Opportunity and Inclusive Growth Institute, Federal Reserve Bank of Minneapolis. E-mail: todd.schoellman@gmail.com

1 Introduction

One of the central challenges for economists is to explain the large differences in gross domestic product (GDP) per worker across countries. Development accounting provides a useful first step toward this goal. It measures the relative contribution of physical capital, human capital, and total factor productivity (TFP) in accounting for cross-country income differences. These accounting results can help highlight the types of theories or mechanisms most likely to explain cross-country income differences. For example, a consensus in the literature that physical capital accounts for a small fraction of income differences has led researchers to de-emphasize theories that assign a prominent role to variation in physical capital per worker.¹

The main unsettled question in this literature is the relative importance of TFP versus human capital in accounting for cross-country income differences. The literature has tried a number of approaches to measuring human capital and reached little consensus on the answer. Since TFP is measured as a residual explanatory factor, wide variation in measured human capital stocks implies wide variation in measured TFP and hence substantial disagreement about the relative contribution of the two. For example, the literature has found that human capital accounts for anywhere from one-fifth to four-fifths of cross-country income differences, with TFP in turn accounting for anywhere from three-fifths to none.²

Our contribution to this debate is to provide new evidence drawing on the experiences of immigrants to the United States. Intuitively, immigrants provide valuable information because they enter the United States with the human capital they acquired in their birth country, but not their birth country's physical capital or TFP. Hence, their labor market performance in the United States conveys information about their human capital separated from the other two country-specific factors. On the other hand, working with immigrants presents two well-known challenges. First, immigrants are selected: their human capital is not the same as the human capital of a randomly chosen person in their birth country. Second, their labor market performance may not accurately reflect their human capital if skills transfer imperfectly across countries.³

¹See, for example, [Klenow and Rodríguez-Clare \(1997\)](#), [Hall and Jones \(1999\)](#), [Caselli \(2005\)](#), or [Hsieh and Klenow \(2010\)](#) for classic references on development accounting and its interpretation.

²The former figure comes from [Hall and Jones \(1999\)](#); the latter comes from [Jones \(2014\)](#). The literature also includes a wide range of estimates in between. See, for example, [Erosa et al. \(2010\)](#), [Hanushek and Woessmann \(2012\)](#), [Córdoba and Ripoll \(2013\)](#), [Weil \(2007\)](#), or [Cubas et al. \(2016\)](#).

³Previous papers that have investigated immigrants and cross-country differences in human capital include [Hendricks \(2002\)](#), [Schoellman \(2012\)](#), [Schoellman \(2016\)](#), and [Lagakos et al. \(forthcoming-a\)](#).

We address these challenges by bringing to bear new data on the pre- and post-migration labor market experiences of immigrants. Our main data source is the New Immigrant Survey (NIS), a sample of adult immigrants granted lawful permanent residence in the United States in 2003 (colloquially, green card recipients) (Jasso *et al.*, 2007). We augment the NIS by using two additional data sources, the Mexican Migration Project and the Latin American Migration Project (jointly, the Migration Projects, or MPs), which collect similar information from immigrants who are not necessarily lawful permanent residents, who may have entered the United States illegally, and who are much less selected on observed characteristics.⁴ We use these data in three ways. First, we construct a measure of the importance of human capital for development accounting based on immigrants' wage gains at migration. Second, we address the challenge of selection by comparing the pre-migration characteristics of immigrants to non-migrants. Third, we address the challenge of skill transferability by comparing the pre- to post-migration occupations of immigrants.

We start by revisiting the standard development accounting framework, focusing on the assumptions necessary to draw aggregate implications from the labor market experiences of immigrants. The most direct measure of the importance of physical capital and TFP is the wage gain at migration relative to the difference in GDP per worker. Intuitively, an immigrant has the same human capital but different physical capital and TFP before and after migrating. The wage gain at migration is thus an index of the relative importance of these country-specific factors, while the residual can be attributed to gaps in human capital per worker. In addition to simplicity, this measure also has the useful feature that it controls for selection in a straightforward manner by studying the wages of the exact same worker in two different countries.

Our empirical work thus relies heavily on a comparison of pre- to post-migration wages. The NIS offers carefully constructed and detailed wage data. It surveyed immigrants about up to two pre-migration jobs and up to three post-migration jobs. It also allowed for a great deal of flexibility in how workers report their earnings. They could report their pre-migration earnings from working in any country, denominated in any currency, from any reference year, at whatever pay frequency they preferred. We discuss in detail how we adjust these data for exchange rate, purchasing power parity (PPP), and differences in reporting year to arrive at estimates of their pre-migration and post-migration hourly

⁴The Mexican Migration Project (MMP) is a collaborative research project based at Princeton University and the University of Guadalajara. We used version 161 of the database. It can be found online at <http://mmp.opr.princeton.edu>. The Latin American Migration Project is similar to the MMP but covers nine other countries. It can be found online at <http://lamp.opr.princeton.edu>.

wages, both denominated in real PPP-adjusted U.S. dollars. We also provide numerous robustness checks to address possible confounding issues such as episodes of inflation or currency revaluation.

We use these data to construct the wage change at migration relative to the gap in PPP GDP per worker. We focus on immigrants from poor countries, with GDP per worker less than one-quarter of the U.S. level. The average wage gain at migration is 38 percent of the total gap in GDP per worker, implying that 38 percent of cross-country income differences are accounted for by physical capital and TFP, with the remaining 62 percent accounted for by human capital.

[Jones \(2014\)](#) emphasizes that development accounting results are sensitive to allowing for imperfect substitution between unskilled and skilled labor. We show that the wage gain at migration is larger for less educated immigrants. This finding is evidence of imperfect substitution: unskilled immigrants find their skills to be relatively scarcer in the U.S. labor market and hence experience larger wage gains compared to skilled immigrants. Our benchmark results bound the role of human capital in development accounting with imperfect substitutes; the plausible range is one-half to two-thirds. Under additional assumptions, we can provide a point estimate of the human capital share under imperfect substitutes, which we find to be 60 percent.

Our findings attribute a much higher share to human capital than earlier papers in the literature that used immigrant earnings ([Hendricks, 2002](#); [Schoellman, 2012](#)). These earlier papers lacked data on pre-migration wages and so drew inferences based on a comparison of the post-migration wages of immigrants from poor and rich countries. The underlying assumption was that immigrants from poor countries and rich countries are similarly selected. Our data allow us to control for selection directly. We can also go a step further and back out the implied degree of selection by comparing the pre-migration characteristics of immigrants to those of non-migrants. We find that immigrants are highly selected on characteristics such as education or wages, and that immigrants from poor countries are much more selected on these characteristics than immigrants from rich countries. The correlation between selection and birth country development biased the inferences in the existing literature.

The data also allow us to measure the transferability of immigrants' skills. To investigate this issue, we compare the pre-migration and post-migration occupations of immigrants. Most immigrants switch occupations upon migration. Further, most immigrants experience occupational downgrading, meaning that their post-migration occupation is lower paying

than their pre-migration occupation, as judged by the mean wage of natives in those occupations. To the extent that this occupational downgrading represents imperfect skill transfer, it implies that we may be understating post-migration wages and the wage gains at migration, which would lead us to understate the role of country and overstate the role of human capital. We investigate several ways to adjust for occupational downgrading and find that doing so lowers the human capital share to roughly one-half.

In addition to the work mentioned above, our paper is also closely related to two literatures that use retrospective or panel data to investigate wage gains. The first literature specifically studies the wage gains for international immigrants, as we do, but typically for a very particular set of migrants (from just one country to another, or employed at a single firm). A particularly interesting set of these papers studies wage gains in special cases in which immigration slots are granted by lottery, allowing the authors to disentangle selection on gains to migration (see [Clemens \(2013\)](#), [Gibson and McKenzie \(2012\)](#), and especially the short-run and long-run gains in [McKenzie et al. \(2010\)](#) and [Gibson et al. \(2015\)](#)). It is comforting that these papers typically find wage gains that are in line with ours (when compared to the size of the GDP per worker gap).⁵ The main difference from our work is that we have a broader sample (immigrants from many countries to the United States), and that we are focused on the aggregate implications for development accounting. In this sense we are closer to [Jasso et al. \(2002\)](#) and [Rosenzweig \(2010\)](#), who also use the NIS data and the pre- and post-migration experiences of immigrants, but instead use this information to think about relative prices of different skills and factor price equalization. The second literature looks at the wage gains of workers who switch sectors or regions in a country and relates them to sectoral or regional productivity gaps ([Alvarez, 2015](#); [Hicks et al., 2017](#)).

The rest of the paper proceeds as follows. Section 2 introduces the development accounting framework and the mapping from our micro evidence on immigrants to aggregate cross-country income differences. Section 3 discusses the data and how we construct comparable pre- and post-migration hourly wages. Section 4 provides the main results. Section 5 quantifies the importance of selection and Section 6 the importance of skill transferability. Section 7 concludes.

⁵In addition to this work, our paper is also related to two other literatures that think about the gains to migration. First, [Klein and Ventura \(2009\)](#) and [Kennan \(2013\)](#) use models to quantify the gains from freer migration across countries. Second, a large literature estimates cross-country wage gaps between observably similar workers. The most similar work in this literature is [Clemens et al. \(2008, 2016\)](#), who compare the wages of immigrants to the United States with the wages of observably similar non-migrants born in the same country, then use various approaches to bound the possible extent of immigrant selection on unobserved characteristics. See also the extensive reference lists in the last paper for further work in this area.

2 Development Accounting Framework

We begin by outlining the simplest development accounting framework, following the literature closely (see [Caselli \(2005\)](#) or [Hsieh and Klenow \(2010\)](#) for recent overviews). Our focus is on clarifying the assumptions needed to draw aggregate inferences from evidence on the wage gains at immigration.

The aggregate production function is standard:

$$Y_c = K_c^\alpha (A_c H_c)^{1-\alpha},$$

where Y_c is country c 's PPP-adjusted GDP, K_c is its physical capital stock, A_c is its total factor productivity, and $H_c \equiv h_c L_c$ is the total labor input, which in turn can be decomposed into human capital per worker h_c and the number of workers L_c .

Following [Klenow and Rodríguez-Clare \(1997\)](#), we rewrite the production function in per worker terms:

$$y_c = \left(\frac{K_c}{Y_c} \right)^{\alpha/(1-\alpha)} A_c h_c, \quad (1)$$

where y_c denotes PPP GDP per worker. The goal of development accounting is to decompose the large cross-country differences in y into three proximate sources, given on the right-hand side: capital-output ratios, total factor productivity, and average human capital. In this paper we focus primarily on distinguishing the share of human capital versus the other two factors jointly, so we define $z_c \equiv (K_c/Y_c)^{\alpha/(1-\alpha)} A_c$. We call this term the country component, because it is what changes when immigrants move to a new country, while their human capital remains the same.

We conduct our accounting exercises in log-levels. Doing so produces results that are additive and order-invariant. Our focus is on separating the relative contribution of human capital from the other two terms in accounting for the difference in PPP GDP per worker between c and c' :

$$\begin{aligned} 1 &= \frac{\log(z_c) - \log(z_{c'})}{\log(y_c) - \log(y_{c'})} + \frac{\log(h_c) - \log(h_{c'})}{\log(y_c) - \log(y_{c'})} \\ &\equiv \text{share}_{\text{country}} + \text{share}_{\text{human capital}}. \end{aligned} \quad (2)$$

2.1 Wage Gains of Immigrants and Development Accounting Implications

We use the wages of immigrants to inform us about the role of country and human capital for development accounting. Our approach builds on the insights of [Bils and Klenow \(2000\)](#), who showed that wages are informative about human capital under two assumptions. First, workers of different types are assumed to be perfect substitutes. In this case, workers may provide varying quantities of human capital, but the total labor supply is simply the total human capital of all workers. We relax this assumption in Section 2.2. Second, labor markets are assumed to be perfectly competitive, so that workers are paid their marginal product. Given these assumptions, the representative firm hires a total quantity H_c of human capital at the prevailing wage per unit of human capital ω_c to maximize profits:

$$\max_{H_c} K_c^\alpha (A_c H_c)^{1-\alpha} - \omega_c H_c.$$

The first-order condition of the firm implies that the wage per unit of human capital is $\omega_c = (1 - \alpha)z_c$, where z_c is defined as in the previous subsection.

The observed hourly wage of worker i in country c $w_{i,c}$ is then the product of the wage per unit of human capital and the amount of human capital the worker possesses:

$$\log(w_{i,c}) = \log[(1 - \alpha)z_c] + \log(h_i). \quad (3)$$

Given that we have data on both pre- and post-migration wages of immigrants, we can construct the log-wage gain to migration. If labor markets are competitive in both countries, then we can divide the log-wage gain at migration by the log-GDP per worker difference between *U.S.* and c to measure the share of cross-country income differences accounted for by the country component:

$$\frac{\log(w_{i,U.S.}) - \log(w_{i,c})}{\log(y_{U.S.}) - \log(y_c)} = \frac{\log(z_{U.S.}) - \log(z_c)}{\log(y_{U.S.}) - \log(y_c)} = \text{share}_{\text{country}}. \quad (4)$$

We construct $\text{share}_{\text{human capital}} \equiv 1 - \text{share}_{\text{country}}$. Intuitively, a worker who migrates keeps the same human capital but switches physical capital and TFP levels. We study how much this changes the worker's wages relative to the total gap in GDP per worker. If the change in wages were as large as the gap in GDP per worker, then we would conclude that country accounts for all of the cross-country income differences, with no role for human capital. If

there were no change in wages, then we would conclude that human capital accounts for all of the cross-country income differences, with no role for country. Our goal is to calculate where we stand between these two polar cases.

Note that this statistic controls for the usual selection concern, namely that immigrants may be more talented or harder working than non-migrants, because it uses wage observations from the same worker in two countries. In Section 5 we quantify the extent of selection by comparing the pre-migration wages of immigrants to the wages of non-migrants. If immigrants were instead selected on the gains to migration, as in [McKenzie et al. \(2010\)](#), then our accounting metric would actually understate the share of human capital in development accounting.

2.2 Development Accounting with Imperfect Substitution

We also consider a framework with imperfect substitution between unskilled and skilled labor, motivated by the work of [Jones \(2014\)](#). In this case, the human capital aggregator is given by

$$H_c = \left(\theta_u H_{u,c}^{\frac{\sigma-1}{\sigma}} + \theta_s H_{s,c}^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}},$$

where $H_{u,c}$ and $H_{s,c}$ are the quantity of unskilled and skilled labor in country c , which are combined with weights θ_u and θ_s and an elasticity of substitution σ .

We continue to maintain the assumption that labor markets in both countries are competitive and workers are paid their marginal product. In this case, the wage gain at migration of any worker who provides type $j \in \{u, s\}$ labor is

$$\log(w_{j,U.S.}) - \log(w_{j,c}) = \log(z_{U.S.}) - \log(z_c) + \frac{1}{\sigma} \left[\log \left(\frac{H_{U.S.}}{H_{j,U.S.}} \right) - \log \left(\frac{H_c}{H_{j,c}} \right) \right]. \quad (5)$$

As with the perfect substitutes case, the wage gain depends on the change in z . In the imperfect substitutes case there is a second term that captures the change in the relative price of type j labor, which in turn depends on the change in the relative supply of type j labor. Skilled immigrants' wage gains will be less than $\log(z_{U.S.}) - \log(z_c)$ because the United States is abundant in skilled labor, which pushes down the relative price of skilled labor (given the usual restriction $\sigma > 1$). By similar logic, unskilled immigrants' wage gains will be larger than $\log(z_{U.S.}) - \log(z_c)$.

Equation (5) can be interpreted as a simple test for imperfect substitution between skill types: if skilled and unskilled workers are imperfect substitutes, then unskilled workers should have larger wage gains at migration. We implement this test below and find support for imperfect substitution. In this case it also suggests a method to bound the importance of country effects in development accounting. As long as skilled labor is relatively abundant in the United States and unskilled labor relatively scarce, then we can bound the contribution of human capital in development accounting as

$$1 - \frac{\log(w_{u,U.S.}) - \log(w_{u,c})}{\log(y_{U.S.}) - \log(y_c)} \leq \text{share}_{\text{human capital}} \leq 1 - \frac{\log(w_{s,U.S.}) - \log(w_{s,c})}{\log(y_{U.S.}) - \log(y_c)}. \quad (6)$$

This bounding approach sidesteps a number of challenges in specifying H_c . It relies only on exploiting the large differences in the relative supplies of unskilled and skilled labor documented in [Barro and Lee \(2013\)](#). Now that we have outlined our approach, it is time to turn to the data.

3 Data

Our main data source is the New Immigrant Survey (NIS), a representative sample of adult immigrants granted lawful permanent residence in the United States (colloquially, green card recipients) between May and November of 2003, drawn from government administrative records ([Jasso et al., 2005, 2007](#)). It includes both newly arrived immigrants granted lawful permanent residency from abroad and immigrants who adjusted to lawful permanent residency after previously entering the United States through other means. The survey consists of two rounds: round 1 was conducted in 2003–2004, shortly after the immigrants adjusted status. A follow-up round 2 was conducted in 2007–2009. See Appendix A.1 for details.

The NIS includes four main types of information that we exploit. First, it surveys respondents about the usual set of demographic characteristics such as age and education, including detailed questions on where immigrants acquired their education. Second, it contains administrative data on the type of visa they used to enter the United States. Third, it surveys them about their labor market experiences in the United States. In round 1 they were asked about their first job after migration and their current (year 2003–2004) job. In round 2 they were again asked about their current (year 2007–2009) job. Fourth, it surveys them about their experiences before entering the United States, particularly their

labor market experiences. Immigrants were asked in round 1 about up to two jobs before entry, their first (after age 16) and last (if different from the first). For all jobs, we know standard information such as earnings, hours and weeks worked, industry, and occupation. Given our focus on the pre-migration wages of immigrants and the wage gains at migration, it is important that immigrants' reported wages be accurate. Fortunately, the NIS was careful to allow immigrants a great deal of flexibility in reporting their pre-migration earnings. Immigrants reported both how much they earned and the frequency at which they were paid (hourly, daily, etc.). They also chose what year this report pertains to, what country they were working in, and what currency they were paid in. This flexibility is important because it allows immigrants to report earnings in the most natural way for them, rather than forcing them to do conversions. It also allows for unusual or non-obvious situations, such as the use of the U.S. dollar as a medium of payment even outside the United States, or the tendency for European migrants to remember their earnings denominated in both pre-euro currencies or euros.

The NIS provides a manual with the steps necessary to produce PPP-adjusted hourly wages in U.S. dollars. First, we use the reported earnings, payment frequency, and hours and weeks worked to construct the hourly wage for all immigrants. Second, we adjust the hourly wage to U.S. dollars by using the market exchange rate prevailing at the time. Third, we adjust wages for PPP. Note that in cases in which immigrants report the "natural" currency for their country (e.g., pesos in Mexico), these latter two adjustments are equivalent to simply dividing by the PPP exchange rate. Our exchange rates and PPP adjustments come from the Penn World Tables, mostly PWT 7.1, although we explored also using PWT 9.0. See Appendix A.1 for further details. We exclude immigrants who report being paid in currencies that were subsequently devalued and flag immigrants who report unusual country-currency pairs (e.g., liras in Brazil) or who report being paid in currencies with high inflation for possible exclusion to minimize concerns about measurement error.

At this point, we have several estimates of both pre- and post-migration wages at different dates reported in U.S. dollars and adjusted for cost of living. Conceptually, the last step is to adjust these wages to a common date and construct the wage gains at migration. This step is complicated somewhat by immigrant assimilation: immigrants' occupational status, wages, and earnings are generally found to grow more quickly than those of comparable natives in the years after migration (Akresh, 2008; Duleep, 2015). This fact has three possible interpretations. First, it could be that initial wages are temporarily depressed by the absence of "search capital," meaning that immigrants have not yet found a job

that suits them and values their talents. In this case, it would be preferable to focus on later post-migration jobs. Second, it could be that immigrants acquire human capital more rapidly than natives after migration, perhaps in response to the change in environment. Finally, it could be that immigrant wage patterns are driven by a composition effect through selective return migration based on wages (Lubotsky, 2007). In these latter cases, it would be preferable to focus on earlier post-migration jobs. There is no clear consensus in the literature about the relative importance of these three effects.⁶

For our baseline results, we combine wage estimates from different jobs to maximize our sample size. We use the most recent valid pre-migration wage. For post-migration wages, we give preference to the 2003–2004 estimate, but use the first post-migration estimate or the estimate in the 2007–2009 follow-up if a valid estimate of the 2003–2004 wage is not available. We consider an estimate of the wage to be valid if we can construct the adjusted hourly wage; if it falls within the range of \$0.01 to \$1,000 per hour; for pre-migration wages, if the wage was from the year 1983 or later; and for post-migration jobs, if the immigrant had no education in the United States before working in that job. We convert all wages into year 2003 wages by adjusting for the wage growth of observably similar natives between year t and 2003, where we use age, gender, and education as our observable characteristics.⁷ This adjustment corrects for inflation and life-cycle wage growth. Any excess wage growth due to assimilation is thus included in the measured wage gain at migration, which is the conservative choice in our approach. We further explore the importance of assimilation by studying how wages and the wage gain at migration evolve over time and across successive jobs in Appendix B. Our data support assimilation, but we find it plays a modest role in our calculations.

After these checks, the remaining immigrants from poor countries have straightforward immigration-job histories. For example, more than 80 percent of the resulting sample had never lived outside their birth country for more than six months before permanently immigrating to the United States. Again, more than 80 percent report working their first U.S. job within one year of their last pre-migration job; more than three-fourths of immigrants satisfy both restrictions. Our results are robust to focusing on this group. The final sample includes 2,006 immigrants with data on both pre- and post-migration wages that we use for our exercises. See Appendix A.1 for details on the number of immigrants dropped by each of our sample restrictions. There we also compare the baseline sample to

⁶Lessem and Sanders (2014) use data from the NIS to quantify the role of labor market frictions; they find that frictions can account for some but not all of assimilation.

⁷Data from the Current Population Survey. See Appendix A.5 for details.

Table 1: Most Sampled Countries by GDP per Worker Category

PPP GDP p.w. Category	Most Sampled Countries
$< 1/16$	Ethiopia, Nigeria, Vietnam
$1/16 - 1/8$	India, Philippines, China
$1/8 - 1/4$	Dominican Republic, Ukraine, El Salvador
$1/4 - 1/2$	Mexico, Poland, Russia
$1/2 - 1$	Canada, United Kingdom, Korea

Table note: Lists the three most common birth countries in each PPP GDP per worker category in the NIS sample.

samples of immigrants who have only pre- or post-migration wages and show that they do not differ greatly on observed characteristics. We also compare the NIS to the sample of immigrants in the American Community Survey, a standard data set used in the literature.

Recall that our goal is to compare the log-wage change at migration to the log difference in GDP per worker in year 2005 from PWT 7.1. Confidentiality restrictions prevent us from reporting statistics by country of origin in all but a few cases. For this reason, our baseline approach is to report statistics for five income categories, constructed on the basis of PPP GDP per worker relative to the United States: less than $1/16$, $1/16-1/8$, $1/8-1/4$, $1/4-1/2$, and $1/2-1$ (we exclude the few immigrants from countries richer than the United States). Table 1 lists the three countries with the most observations within each category.

3.1 Migration Projects

Although the NIS data are ideal for our purposes in most respects, they do have one limitation: they are confined to lawful permanent residents. The vast majority of these immigrants entered the country through legal channels. As we document below, this sample turns out to be highly selected on a wide variety of dimensions, including pre-migration education, occupation, and wage. It is useful to be able to study the wage gains of less selected immigrants. We accomplish this goal by adding data from the Mexican and Latin American Migration Projects (jointly, the Migration Projects, or MPs) when appropriate.

The MPs are collections of surveys that share a common basic design, implemented in Mexico and nine other Latin American countries. In each survey year, the survey team identifies several communities where the interviewers expect some international migration activity. The communities are chosen to represent diverse sizes, but they are not representative of

the country. In each community, a representative sample of households is interviewed. In addition, some surveys interview a small number of households that originate in the sampled communities but currently reside in the United States.

The surveys collect basic demographic information and individual job histories from household heads and their spouses. Job histories start with the first job ever held and record the start and end dates, location, and occupation. Wages are recorded for each person's first and last home country job and first and last job held abroad (including the United States). These wages allow us to calculate the wage change at migration, albeit typically in the opposite fashion (e.g., the wage loss upon returning to the foreign country). We limit our attention to native-born household heads and spouses aged 18 to 75 with valid responses to key variables and who report being paid in their country's local currency (as defined in PWT 7.1). We adjust wages and impose the same sample selection criteria as for the NIS. See Appendix A.2 for details and sample size by country.

4 Results

We now turn to our results, focusing for the moment on the NIS data. We begin by discussing the basic patterns of wages, reported in year 2003 U.S. dollars. We compute the mean pre- and post-migration log wage by PPP GDP per worker category. The exponentiated results are plotted in Figure 1(a), with the exact figures given in Table 2. Histograms of the underlying distributions of pre-migration wages, post-migration wages, and the wage gains at migration are available in Appendix A.1. Both pre- and post-migration wages are positively correlated with development, although the trend is surprisingly weak among the three middle income categories. More striking are the high levels of pre-migration wages for immigrants from poor countries: the PPP-adjusted hourly wage is \$2.82 even for immigrants from the very poorest countries.

A key statistic for our approach is the wage gain at migration, which we compute for each individual as the log of the ratio of post-migration to pre-migration wages. We average this statistic by GDP per worker category and plot the exponentiated results in Figure 1(b), with the exact figures given in Table 2. The average immigrant has a substantial wage gain at migration. The wage gain is negatively correlated with development, as one would expect; immigrants from the poorest countries gain by a factor of 3.2, while immigrants from the richest gain by a factor of 1.3. The gains for immigrants from poor countries are quite small relative to the gap in GDP per worker, suggesting that the country component

Figure 1: Wages, Wage Gains, and GDP per worker



plays a small role in development accounting. We formalize this idea in the next subsection.

4.1 Accounting Implications

Recall from equation (4) that our measure of the importance of human capital is one minus the log-wage change at migration relative to the log-GDP per worker gap. We construct the implied share for every immigrant in our sample. We then compute the mean of the share within each GDP per worker category. The resulting estimates and 95 percent confidence intervals for each GDP per worker category are given in Table 2.⁸

Our primary focus is on poor countries because they are of greater interest for development accounting. Thus, we focus for the remainder of the paper on immigrants from the three poorest income groups, or those with GDP per worker less than one-quarter of the United States. The estimates from these three groups cover a narrow range of 0.58–0.66 with fairly tight confidence intervals. When pooled, the implied share of human capital in development accounting is 62 percent against a share of country-specific factors of only 38 percent. The 95 percent confidence interval is narrow, ranging from 58 to 65 percent, implying that we can rule out that human capital accounts for as little as even half of cross-country income

⁸We find very similar results if we use instead the median of the implied human capital shares, or if we first compute mean log-wage changes at migration and mean log-GDP per worker gaps and then construct the implied human capital share. Our confidence intervals are constructed using a normal approximation, but bootstrapped confidence intervals are very similar.

Table 2: Implied Human Capital Share in Development Accounting

Country Group	Hourly Wage			Development Accounting			
	Pre-Mig.	Post-Mig.	N	Wage Gain	GDP Gap	h share	95% C.I.
< 1/16	\$2.82	\$8.91	281	3.2	31.8	0.66	(0.60, 0.73)
1/16 – 1/8	\$4.19	\$11.83	617	2.8	11.9	0.58	(0.54, 0.62)
1/8 – 1/4	\$4.95	\$9.48	436	1.9	5.6	0.63	(0.55, 0.71)
1/4 – 1/2	\$5.05	\$9.11	263	1.8	3.0	0.48	(0.34, 0.62)
1/2 – 1	\$12.64	\$15.18	409	1.2	1.3	0.48	(-0.23, 1.19)

Table note: Each row shows results for immigrants from one of five GDP per worker groups. Columns show the categories; the mean hourly pre- and post-migration wages, reported in 2003 U.S. dollars; the number of immigrants in the corresponding category; the wage gain at migration; the average gap in GDP per worker, relative to United States; the implied human capital share; and the corresponding 95 percent confidence interval.

differences.

We estimate the same statistic for the MPs. If we again focus on immigrants from countries with GDP per worker less than one-quarter of the United States, the implied human capital share in development accounting is 80 percent. For the rest of the paper, we expand our working MP sample slightly to also include Mexican immigrants because they are a large fraction of the MP sample and because they provide unique observations on less selected immigrants in terms of education and occupation that will be useful below. The implied human capital share for the MP sample including Mexico is 38 percent, driven by the fact that Mexican immigrants have larger wage gains (relative to the GDP per worker gap) than immigrants from most other countries. In the next section, we show that much of this difference can be explained by the composition of immigrants from Mexico.

Appendix B contains a lengthy exploration of these results and their robustness. We provide results by country (when possible) and visa status. We show how they are affected by the process of assimilation. And we show that they are robust to the details of how wages are constructed, currencies are converted, who is included in the sample, and so on. There is some variation across these robustness checks, but the implied human capital share is almost always greater than one-half. We now turn to extending the standard development accounting analysis to explore imperfect substitution between skill groups.

4.2 Development Accounting and Imperfect Substitution

We begin by exploring whether the wage gains at migration vary systematically with education. To do so, we pool the NIS and the MP data and focus on immigrants from countries with PPP GDP per worker less than one-quarter of the United States, plus immigrants from Mexico. Pooling the MP is critical because the NIS has very few immigrants from the lowest education groups. We measure education using data on degree attainment or years spent in school. Workers are divided into five groups: those with no exposure to high school (less than 9 years of schooling); those with some high school but no degree (9–11 years); those with a high school degree (12 years); those with some college but not a bachelor’s degree (13–15 years); and those with a bachelor’s degree or more (16 or more years of schooling).

We estimate wage gains at migration as a function of worker characteristics, controlling for country fixed effects. The results are given in column (1) of Table 3. The coefficients capture the log-wage gain relative to the omitted category, which is college graduates. The coefficients are all positive, indicating that less educated workers have larger wage gains. For those without any exposure to college, they are also large and statistically significant. For example, the coefficient on “no high school” indicates that immigrants who have never been to high school gain 82 percent more upon migration to the United States than immigrants with a college degree. These results help us begin to understand why Mexican immigrants in the MP sample have such large wage gains at migration: they are very poorly educated (62 percent have no exposure to high school). These results provide evidence that workers of different skill levels are imperfect substitutes.

We augment our development accounting results to allow for imperfect substitution between skill groups. We start with the bounding approach of equation (6). This approach requires us to partition our five education groups into two broad categories: unskilled and skilled. We explore three partitions, where the cutoff to be skilled ranges from some high school to some college. We focus on immigrants from countries with less than one-quarter of U.S. GDP per worker in the NIS sample so that figures will be comparable with the baseline figure reported above. For each possible partition, we construct the human capital share of development accounting separately for unskilled and skilled immigrants.

The results of this procedure are given in columns 2 and 3 of Table 4. The plausible range for human capital is in line with other robustness checks, extending from a little less than one-half to a little less than two-thirds. The point estimates we derived in the perfect substitutes case are closer to the upper bounds. This makes sense because most workers

Table 3: Wage Gains and Education

No high school	0.599*** (0.073)
Some high school	0.463*** (0.076)
High school graduate	0.264*** (0.073)
Some college	0.132 (0.101)
Country fixed effects	x
N	3,539

Table note: Estimated effects of education on wage gain at migration. Omitted category is college graduates. Standard errors are in parentheses. ***, **, and * denote estimates that are statistically different from zero at the 99, 95, and 90 percent level. N is the number of observations in the sample.

Table 4: Development Accounting with Imperfect Substitution

Cutoff for Skilled Group	Bounding Approach		Direct Calculation	
	Lower	Upper	Homog. Unskilled	Heterog. Unskilled
Any high school	49%	63%	59%	63%
High school graduate	54%	64%	46%	59%
Any college	60%	63%	36%	60%

Table note: Each row shows the implied results from a different division of the workforce into unskilled and skilled. The bounding approach produces lower and upper bounds as in equation (6). Direct calculation shows the result of constructing human capital stocks directly under the case in which unskilled labor is assumed to be homogeneous ($h_{u,c} \equiv 1$) or allowed to be heterogeneous.

in our data are highly skilled. Since the perfect substitutes case takes the simple average across workers and most workers are skilled, it naturally produces results closer to the upper bound.

As a complementary approach, we also construct human capital stocks directly, which requires us to impose further structure but allows us to produce point estimates of the human capital share. This exercise is in the spirit of Jones (2014), except that we can bring to bear our new data on the wage gains of unskilled and skilled workers. The total quantity of type j labor is given by $H_{j,c} \equiv h_{j,c}L_{j,c}$, where $L_{j,c}$ is the number of type- j equivalent workers in country c and $h_{j,c}$ is the human capital per type- j equivalent worker. We measure the number of unskilled and skilled workers using year 2005 data on the education level of the population in all countries from Barro and Lee (2013). We aggregate the bottom three categories in Barro and Lee (no schooling, some primary, primary complete) and map them into our bottom category (no high school). The remaining four categories in Barro and Lee map directly into our four remaining categories. We construct $L_{j,c}$ by assuming that workers within each broad skill group are perfect substitutes and that the subgroups can be aggregated using school duration and the observed Mincer return. We take this return to be 10 percent everywhere, motivated by Banerjee and Duflo (2005).

We start by constructing human capital stocks under the assumption that $h_{u,c} \equiv 1$ for all countries. This approach produces conservative results because it assumes that human capital per unskilled worker is the same everywhere; all aggregate human capital differences then have to come from the share of skilled workers and human capital per skilled worker. The main role of this calculation is to provide an important robustness check on our results. In particular, this calculation allows us to construct human capital stocks without using the wage gains of unskilled immigrants. This is important because our sample is highly selected, so we have few such immigrants from the poorest countries. After making this assumption, we need three parameters: $h_{s,c}$ for the United States and for the “poor country,” and the elasticity of substitution σ . “Poor country” here is an aggregate of all countries with less than one-quarter of U.S. GDP per worker, in line with the results so far.

We calibrate these parameters to fit the wage gains at migration of skilled workers while maintaining a skill premium in both countries that is equivalent to a 10 percent rate of return per year of schooling. Intuitively, wage gains and the skill premium are both informative about the relative supply of skilled labor, conditional on the elasticity of substitution. By considering them jointly, we can back out the implied elasticity of substitution and $h_{s,c}$ separately. The results of this exercise are shown in column 4 of Table 4. Human capital

accounts for 46–59 percent of cross-country income differences. The only way to push this figure lower would be to include workers with some college in the unskilled category; given the assumption $h_{u,c} \equiv 1$, this implies that the majority of the world’s population, including all high school graduates, provides identical unskilled labor services. Even this extremely strong assumption would only push the human capital share in development accounting to 36 percent.

Setting $h_{u,c} \equiv 1$ is a useful starting point, but it leads us to predict counterfactually large wage gains for unskilled workers, a factor of 7.6 instead of 3.0–3.7 in the data. This result supports a second idea of [Jones \(2014\)](#), which is that there may also be cross-country variation in the human capital of unskilled workers. We conduct a second analysis where we allow $h_{u,c}$ to vary. After normalizing $h_{u,U.S.} \equiv 1$, this introduces one additional parameter, which can be pinned down using the wage gain at migration of unskilled workers. The results of this exercise are shown in column 5 of Table 4. Human capital accounts for a larger share of cross-country income differences, as one would expect. The results vary much less across different possible partitions between unskilled and skilled, largely because $h_{u,c}$ is recalibrated to fit the wage gains of unskilled workers as we vary the partition. Overall, the evidence supports imperfect substitution between unskilled and skilled workers. The human capital share in development accounting can be bounded between one-half and two-thirds and is probably close to 60 percent. We now turn to quantifying selection, which is important for understanding why our results differ from those in the literature.

5 Selection

We measure the importance of human capital for development accounting by comparing the wage gains at migration to the total gap in GDP per worker. As discussed in Section 2.1, this deals with the simplest form of immigrant selection. However, it is of interest to back out the implied degree of selection, which is the gap between immigrants’ pre-migration characteristics and the characteristics of non-migrants born in the same country. The patterns and degree of selection are of interest in their own right. As we show below, they are also useful for understanding why our results differ so much from those in the literature.

5.1 Selection and Wages

We start by measuring the implied extent of selection on wages. In principle, one would like to compare the pre-migration hourly wage of immigrant i to the mean wage of non-migrants born in the same country, $w_{i,c}/\bar{w}_c$. Given the widespread prevalence of self-employment in poor countries, we substitute $\bar{w}_c = (1 - \alpha_c)y_c/n_c$ to approximate the mean labor income per hour of all workers (instead of using only wages of wage workers.) Here, n_c is hours worked per worker per year. [Gollin \(2002\)](#) documents that α_c does not vary systematically with average income, while [Bick et al. \(2015\)](#) document that hours worked per employed person do not differ much between the United States and poor countries. If we assume that these two factors are roughly constant, we arrive at a simple measure of selection for an individual:

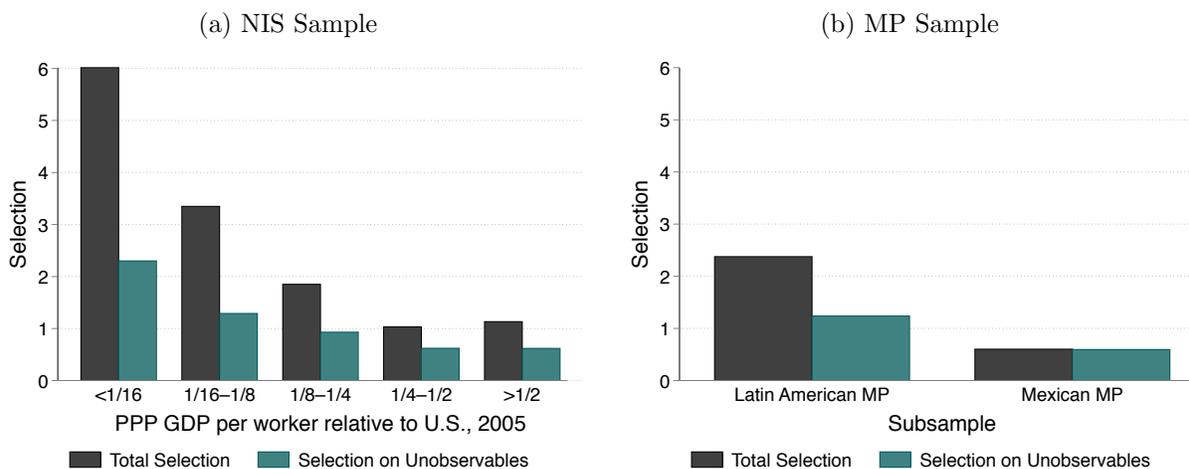
$$\sigma_i = \frac{w_{i,c}/y_c}{\bar{w}_{U.S.}/y_{U.S.}}. \quad (7)$$

In words, this equation says immigrants are highly selected if the ratio of their pre-migration wage to GDP per worker is high relative to the benchmark, which is the mean wage of Americans relative to U.S. GDP per worker.

We construct this measure of selection for all individuals in the NIS and MPs. For the NIS we average the results by GDP per worker category. For the MPs we differentiate between the large Mexican sample originating in the Mexican Migration Project and then pool the remaining poorer countries with much smaller samples from the Latin American Migration Project. The resulting measures of selection are plotted as “total selection” in Figure 2. This exercise has two main takeaways. First, immigrants are substantially selected on pre-migration wages, with a mean selection of more than two for the entire sample. Second, the degree of selection varies systematically with PPP GDP per worker. Immigrants from the poorest countries are selected by a factor of six, whereas immigrants from the richest countries are nearly unselected by this measure. We also find that immigrants from Mexico are roughly unselected, in line with an existing literature that debates whether the selection is positive or negative ([Morága, 2011](#)).

A second question is whether this selection corresponds with observed characteristics of workers. To test this, we construct a measure of residual wages and selection along the lines of [Hendricks \(2002\)](#). The details are in Appendix A.4, but the basic idea is to use a log-wage regression on a sample of natives to estimate the effect of observable characteristics, in this case age and education. We do so using the 2004 American Community Survey (ACS),

Figure 2: Selection of Immigrants



which is a large representative sample that closely matches the time frame of the NIS. We construct a measure of selection on observable characteristics by valuing the difference in age and education of immigrants and non-migrants with the estimated coefficients. Our data on the characteristics of non-migrants come from [Barro and Lee \(2013\)](#), who give the educational attainment and age composition of the population for most countries worldwide.

We then study selection on unobservables, which is the portion of selection on pre-migration wages that remains after netting out selection on observables as constructed above. The results are shown again in Figure 2.⁹ Immigrants from the poorest countries are much more selected on unobserved characteristics than those from the richest countries.

The degree and pattern of selection are interesting in their own right, but they also help to explain why our results differ so much from the previous literature, particularly the results in [Hendricks \(2002\)](#). [Hendricks \(2002\)](#) shows that residual wages are only modestly higher for immigrants from rich countries relative to those from poor countries. Under the assumption that these immigrants are equally selected on unobserved human capital, this finding implies relatively small differences in the human capital of non-migrants in rich and poor countries.¹⁰ This logic is central to his result that human capital accounts for little of

⁹Since the MP samples collect data on non-migrants in each country, we can also compute selection and residual selection directly using wages in these countries rather than using equation (7). The results are quite similar.

¹⁰He also argues that it is implausible that all of the wage gap between immigrants and non-migrants can be accounted for by selection. Our findings are consistent with this claim because we find that some but not all of the wage gap can be accounted for by selection. Focusing on the poorest GDP per worker

cross-country income differences.

Our data support the finding that residual (post-migration) wages are only modestly higher for immigrants from rich countries relative to those from poor countries. In Appendix C we show that the estimated relationship in the NIS is in line with the literature. However, Figure 2 suggests a very different interpretation of this finding. Immigrants from poorer countries are much more selected on unobserved characteristics than are immigrants from rich countries. The implied gap in human capital between non-migrants from rich and poor countries is thus found to be much larger than that in [Hendricks \(2002\)](#).

Our results are also larger than those in [Schoellman \(2012\)](#). That paper relies on a different identifying assumption, namely that selection on residual wages is uncorrelated with schooling for a given country. Although our sample size is too small to test this prediction, we show in Appendix C that selection on residual wages is not strongly correlated with schooling within a GDP per worker category. The more likely reason that our results are larger is that [Schoellman \(2012\)](#) has a more limited scope in trying to account for differences in quality-adjusted schooling, whereas our results include in principle all forms of human capital, including experience (as in [Lagakos et al. \(forthcoming-a\)](#)) or health.

5.2 Selection on Other Characteristics

Selection on residual characteristics plays an important role in explaining why our results differ from those in the previous literature. Given this role, we explore other non-wage attributes of immigrants from poor countries to see if they support strong selection. We start with the NIS sample, focusing particularly on the immigrants from the very poorest countries (less than 1/16 of U.S. GDP per worker). These immigrants are highly selected on education, with an average of 13 years of schooling. Thirty-two percent have a college degree, while only 18 percent have not graduated from high school. This finding is similar to what is reported in [Schoellman \(2012\)](#), namely that immigrants from poor countries are much more educated than non-migrants born in the same country.

Immigrants are also highly selected on non-wage occupational characteristics. For example, 79 percent of immigrants from the poorest countries were employed for wages in their pre-migration job, which stands at odds with the general prevalence of self-employment in poor countries. Selection on occupation is also strong. If we focus on 25 broad occupation

group in the NIS, the total wage gap can be decomposed as follows: the true wage gain (factor of 3.2); selection on observed components (factor of 2.6); and selection on unobserved components (factor of 2.3).

categories, the most commonly reported are sales and related, office and administrative support, management, and education and training. Combined, they account for more than half of all pre-migration occupations. Only a single immigrant in the poorest group of countries reported having previously worked in agriculture, forestry, and fishing, even though this occupation group accounts for the majority of employment in most poor countries (Restuccia et al., 2008).

Selection in the MPs is broadly similar except for Mexican immigrants, who are much less selected. They are much less educated, with only 7.1 years of schooling on average, and 60 percent have no exposure to high school. Further, they are actually over-representative of agriculture; 31 percent worked in agriculture, as compared to 15 percent of non-migrants.

5.3 Selection and Sectoral Wage Gaps

Selection on pre-migration employment implies that our results intersect with the growing literature that documents sectoral and regional labor productivity gaps in poor countries (Restuccia et al., 2008; Caselli, 2005; Herrendorf and Valentinyi, 2012). Two potential explanations account for these gaps. The first is selection by high human capital workers into non-agriculture and urban areas. In this case, the scarcity of former agricultural workers in our sample is simply another sign of strong selection on human capital. The second is gaps in the marginal value product of labor between sectors or regions combined with a “barrier” that prevents workers from reallocating to the sector or region with the higher marginal value product of labor. In this case, the NIS has oversampled immigrants from the most productive portions of poor countries. The extent to which selection or barriers account for sectoral productivity gaps is still an open question in the literature (Gollin et al., 2014; Hicks et al., 2017; Herrendorf and Schoellman, forthcoming).

We can test these two hypotheses using our data. The barriers view implies that immigrants who previously lived in unproductive areas or worked in unproductive sectors had lower wages, which in turn implies a larger wage gain at migration (assuming, as is conventional, that there are no such gaps in the United States). Thus, we look for heterogeneity in the wage gains at migration by sector and region of pre-migration employment. For sector we focus on whether the immigrant was employed in agriculture or non-agriculture. For region we know only whether an immigrant grew up in a rural or an urban area, and only for the NIS.

We estimate wage gains as a function of country fixed effects and sector or region using

Table 5: Wage Gains and Immigrant Characteristics

	(1)	(2)	(3)	(4)
Agriculture	0.536*** (0.053)		0.476*** (0.054)	
Grew up rural		0.285*** (0.079)		0.197** (0.081)
No high school			0.495*** (0.073)	0.664*** (0.157)
Some high school			0.431*** (0.076)	0.358*** (0.138)
High school graduate			0.259*** (0.072)	0.175* (0.104)
Some college			0.136 (0.100)	0.251* (0.144)
Country fixed effects	x	x	x	x
N	3,539	1,400	3,539	1,400

Table note: Estimated effects of immigrant characteristics (education, sector of employment, region of childhood) on wage gain at migration. Standard errors are in parentheses. ***, **, and * denote estimates that are statistically different from college graduates at the 99, 95, and 90 percent level. N is the number of observations in the sample.

the pooled NIS and MP samples of immigrants from countries with GDP per worker less than one-quarter of the United States, plus immigrants from Mexico. The results are given in columns (1) and (2) of Table 5. The effects of sector and region are large and statistically significant. Immigrants who previously worked in agriculture gain 71 percent more at migration than those who worked in non-agriculture; immigrants who grew up in rural areas gained 33 percent more than those who grew up in urban areas. One potential concern is that these results confound the effects of education. Columns (3) and (4) show the result of estimating the wage gain as a joint function of sector or region and education.¹¹ The results for sector are reduced only modestly, the results for region more so. These results are evidence in favor of gaps in the marginal value product of labor, especially between sectors. They also help us further to understand the high wage gains at migration for Mexican immigrants: Mexican immigrants are by far the most likely to have worked in agriculture before migrating.

These gaps imply that each country's aggregate z and our average wage gains at migration

¹¹It would also be interesting to test the effect of sector versus the effect of region. Unfortunately, we cannot do so because we only know rural status in the NIS, whereas almost all observations for agricultural workers are in the MP sample.

are affected by the sectoral composition of employment. For logical consistency, the sectoral employment shares used to calculate each object should match, but the selection of immigrants implies that they do not. To correct for this mismatch, we construct the human capital share in development accounting separately for workers who previously worked in non-agriculture and in agriculture. We then reweight the two using the employment share in agriculture of the corresponding country group, taken from [World Bank \(2014\)](#).

We make this correction for the baseline NIS sample. Agricultural workers are undersampled: they are 1.1 percent of the sample, versus 33 percent for non-migrants. Correcting for this undersampling puts more weight on the large wage gains of agricultural immigrants, but the effect is modest: the implied human capital share is 55 percent instead of 62 percent in the baseline. This calculation puts a lot of weight on a small number of agricultural workers in the NIS. However, we get similar results if we pool the NIS with the MP with or without Mexico, which helps to expand the sample of agricultural workers used in the calculation.

A second question of interest is whether these results are more important for the very poorest countries where agriculture is more common. For example, countries in our poorest GDP per worker group have a 60 percent agricultural employment share. However, we know little about the wage gains at migration for agricultural workers from the poorest countries because our sample includes only one such worker. We can provide some preliminary calculations if we assume that agricultural workers would gain 0.536 more than non-agricultural workers, as implied by column (1) of Table 5.¹² In this case adjusting for composition effects reduces the human capital share from 68 to 57 percent. An effect twice as large would reduce the human capital share to 47 percent.

6 Skill Transferability

Our baseline estimates measure the importance of country by comparing the pre- and post-migration wages of a fixed individual. If immigrants are able to use their human capital equally in the two countries, then the gap in wages is entirely determined by country-specific factors. However, a common concern with immigrants is that their skills may not transfer perfectly when they migrate. This could happen either if skills are heterogeneous and they

¹²We estimated wage gains as a function of sector and an interaction of sector and log GDP per worker. The coefficient on the interaction was positive and statistically insignificant. Thus, at least in our sample there is no systematic evidence that poorer countries have larger sectoral gaps in marginal value products.

have acquired skills that are not highly valued in the United States, or if barriers such as accreditation, licensure, or discrimination prevent them from fully utilizing their skills.

The first goal of this section is to provide evidence on skill transferability by studying proxies for skill transfer. Our main evidence comes from comparing immigrants' pre- and post-migration occupations. We document that occupational switching is widespread and that most immigrants move to lower-paying jobs, which is a possible sign of imperfect skill transfer. We then consider the importance of this finding for our development accounting results. If immigrants have skills but cannot use them in the United States, then this barrier depresses their post-migration wage and the wage gain at migration. It then follows that we understate the role of country and overstate the role of human capital in development accounting. Conservative corrections for skill transfer push our estimate of the human capital share down toward one-half.

6.1 Evidence on Skill Transferability

Our main measure for skill transfer comes from comparing immigrants' pre- and post-migration occupations.¹³ Measuring skill transferability through occupational changes is subject to two biases that push in opposite directions and are not easy to quantify. On the one hand, we are assuming that immigrants who do not practice their pre-migration occupation do so because of a lack of skill transferability, ruling out a lack of skill altogether (i.e., they may simply have been unqualified). On the other hand, our measure does not capture within-occupation skill loss. For example, we capture doctors who are forced to work as taxi drivers, but not specialized doctors forced to work as family doctors. However, we note that the NIS uses the 2000 U.S. Census occupation codes, which include over 450 possible occupational choices. With these two caveats in mind, we now turn to analyzing occupational switches.

We begin by examining the frequency of occupation switches. Most immigrants switch jobs after migrating. The fraction staying in the same occupation is shown in column 3 of Table 6 and ranges from 6 to 25 percent depending on the level of development. This figure is driven mostly by changes to entirely new occupations; if we aggregate to broad occupation groups, still only 15–41 percent of immigrants work in the same broad occupation group

¹³The literature on the economics of immigration has explored several ways to measure skill transfer. Our approach and findings parallel those of [Chiswick et al. \(2005\)](#) and especially [Akresh \(2008\)](#), who also uses the NIS. [Chiswick and Miller \(2009\)](#) employ an alternative strategy of comparing immigrants' education to that of natives in the same occupation, using "overeducation" as a proxy for imperfect skill transfer.

Table 6: Occupational Changes at Migration

GDP category	Occupational Switch (%)			Mean Change (%)
	Lower-Paying	Same Occupation	Higher-Paying	
<1/16	68	9	23	-17
1/16–1/8	61	16	22	-15
1/8–1/4	67	6	26	-16
1/4–1/2	60	10	30	-13
>1/2	49	25	26	0

Table note: Columns show the fraction of immigrants who switched to a lower-paying job, stayed at the same job, or switched to a higher-paying job at migration, as well as the average change in job pay at migration, where average pay is measured using the mean wage of natives. Rows show those results for different PPP GDP per worker groups.

after migrating.

A change in occupation does not indicate whether the new occupation is better or worse than the old occupation. As a proxy for the “quality” of an occupation, we construct the mean wage of natives employed in the occupation from the 2004 ACS.¹⁴ We merge this mean wage by occupation with both the pre- and post-migration occupations of immigrants in the NIS. This procedure provides us with a quantitative ranking of each immigrant’s pre- and post-migration occupation and hence a measure of the extent to which an immigrant’s new job is better or worse than his or her old one. For example, take an immigrant who worked as a physician in his or her birth country but works as a taxi driver in the United States. Based on the observation that the mean wage of taxi drivers in the United States is \$9.58 while the mean wage of physicians is \$37.11, we would infer that the immigrant’s occupational switch involved a downgrade. The extent of the change in mean wages (74 percent) provides a metric to suggest that the occupational downgrading was significant.

The remaining columns of Table 6 show a sense of the distribution and average change in occupation at arrival. Roughly two-thirds of immigrants move to a lower-paying job after migrating, while only one-quarter move to a higher-paying job, except for the highest GDP per worker group. The mean change in occupation quality (again, judged by mean native wage) is a loss of 13–17 percent upon migration. Only immigrants from the richest countries report little occupational downgrading at migration. One interpretation of this finding is that most immigrants cannot perfectly transfer their skills to the United States.

Since occupations are only an imperfect measure of skill transfer, we also explore other

¹⁴See Appendix A.5 for details.

proxies for ability to transfer skills, with a focus on two that we think are likely to be particularly relevant for immigrants: language and networks. For the former, we find that immigrants who speak English, and particularly those who use English at work, have larger wage gains and less occupational downgrading at migration. For the latter, we find that the extent of occupational downgrading declines over time, consistent with a theory in which immigrants need to build up networks: the first post-migration job is 18 percent worse, the round 1 job 14 percent worse, and the round 2 job 9 percent worse than the pre-migration job (wages also rise; see Appendix B). We also have two proxies for immigrants who may have had networks even before immigrating to the United States. Both immigrants who entered on employment visas and those who report having had a job offer in hand before immigrating to the United States, report larger wage gains and less occupational downgrading at migration. These proxies only add to the evidence that skill transfer is imperfect and the extent of skill transfer varies among immigrants. We now show how to correct our development accounting results for this imperfect skill transfer.

6.2 Development Accounting with Imperfect Skill Transfer

If we interpret these findings as evidence of imperfect skill transfer, then they have important implications for our development accounting results. We explore this idea further in three ways, focusing throughout on the NIS sample of immigrants from countries with less than one-quarter of U.S. GDP per worker. First, since our index of occupational downgrading is smaller for later jobs, it may be more appropriate to use the wage gains and human capital share of later jobs. Table B2 in Appendix B shows that using the wage gain from the last job leads to a human capital share of 0.53 instead of 0.62. Second, we check the robustness of our results to focusing on groups for whom skill transfer is likely to be less of a problem. The NIS includes four main groups: immigrants who entered the United States on employment visas; those who entered the United States with a job offer in hand; those who work the same detailed occupation before and after migrating; and those who speak English at work. The implied development accounting results for these subsamples are shown along with the baseline in Table 7. While human capital accounts for 62 percent of cross-country income differences in the baseline, it accounts for 45–59 percent when focusing on these subsamples.

As a second check, we consider imputing to immigrants a higher wage if they experienced occupational downgrading. This step is logical if the main reason for occupational downgrading is artificial barriers such as licensure rather than a lack of skills among immigrants.

Table 7: Development Accounting and Skill Transfer

Robustness Check	Human Capital Share	95% C.I.
Baseline	0.62	(0.58, 0.65)
Employment visa	0.56	(0.50, 0.62)
Job offer before migrating	0.45	(0.36, 0.55)
Same narrow occupation	0.56	(0.48, 0.64)
English at work	0.59	(0.54, 0.63)
Skill transfer: mean wage	0.55	(0.52, 0.59)

Table note: Each column shows the implied human capital share in development accounting (one minus the wage gain at migration relative to the GDP per worker gap) and the 95 percent confidence interval. Each row shows the result from constructing these statistics for a different sample or using different measures of post-migration wages.

By increasing the post-migration wage of immigrants, we also increase the implied wage gains at migration and lower the implied human capital share for development accounting. We implement this idea by adding to each downgraded immigrant’s wage the gap in mean native wages between his or her pre- and post-migration occupations. For example, take an immigrant who reports having been a doctor before arriving to the United States, but who is now a taxi driver earning \$8 an hour. We would add to this wage the difference between the mean native wage of doctors and taxi drivers, which is \$27.53, resulting in a total wage of \$35.53. The resulting adjustment is substantial, increasing the mean post-migration wage of immigrants by 14 percent. We then recompute wage gains at migration and the human capital share in development accounting. The results are reported in the last row of Table 7. We find that human capital in this case would still account for more than half of cross-country income differences.

This section offers two main takeaways. First, most immigrants switch to lower-paying occupations when they immigrate to the United States. If this fact is interpreted as the result of imperfect skill transfer, then our baseline results overstate the importance of human capital for development accounting. We conduct several checks that suggest that correcting for imperfect skill transfer could lower the human capital share to 45-59 percent, which is still much larger than the standard result in the literature. On the other hand, if occupational downgrading indicates a lack of skills, then the baseline result of 62 percent is appropriate.

7 Conclusion

In this paper we use data on pre- and post-migration outcomes of immigrants along with an extended development accounting framework to infer the importance of human capital versus country in accounting for cross-country income differences. Our key finding is that immigrants' wage gains at migration are small relative to gaps in PPP GDP per worker. Using the standard development accounting framework in which workers of different types are perfect substitutes, these figures imply that human capital accounts for 62 percent of cross-country income differences. Wage gains are larger for less educated immigrants and those who previously worked in agriculture, so we also consider extending the development accounting results to allow for imperfect substitution between workers of different types and sectoral productivity gaps within poor countries. A plausible range of variation for the human capital share is from just under one-half to two-thirds.

We also provide novel evidence on two issues frequently raised in the literature. First, we find that immigrants are highly selected on observable and unobservable characteristics. Both forms of selection are negatively correlated with development. Second, we study skill transfer through immigrants' changes in occupation. We find evidence that immigrants move to lower-paying occupations upon arrival. We provide calculations to show that reasonable corrections for this possible imperfect skill transfer lower the human capital share in development accounting to roughly one-half.

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Online Appendices: Not for Publication

A Data Details

A.1 New Immigrant Survey

The NIS attempted to interview 12,500 adult immigrants sampled from government records between June 2003 and June 2004. They were able to do so in 68.6 percent of the cases; these cases make up the main sample. A follow-up round 2 was conducted in 2007–2009. The follow-up had a lower response rate of 46.1 percent. [Massey et al. \(2017\)](#) provide details on the low response rate, which was explained in large part by the fact that the U.S. Citizenship and Immigration Services did not honor an agreement to provide updated addresses for sample respondents. They also provide weights so that the follow-up sample is balanced with respect to the initial sample in terms of observed characteristics. We use these weights when comparing wage gains across different time frames (as in Table B2). Of course, they do not necessarily guarantee that respondents and non-respondents are similar on non-observed characteristics, such as the rate of wage growth. Surveyors also collected the same detailed data from every immigrant’s spouse and for the parents of a separate sample of child refugees. In many cases, these spouses and parents were also immigrants; in such cases, we include them in our sample, although we show in Appendix B that this is not important for our main results. We utilize the restricted version of the data, which allows us to identify the exact country of birth and work, rather than broad geographic regions.

Table A1: Sample Size by Adjustment

	Pre-Migration	Post-Migration	Both
Ever in labor force	4,116	3,171	3,171
Any wage reported	3,554	2,864	2,560
Adjusted hourly wage	2,917	2,798	2,171
No U.S. schooling	2,536	2,284	1,841
Balanced sample	1,374	1,374	1,374

Table note: Each row shows the cumulative effect on the available sample size as we make the sequence of adjustments and restrictions used in the paper, starting from all immigrants who have hourly wages down to the final baseline sample in the last row. The columns indicate the number of observations with pre-migration wages, post-migration wages, and both; the last column is the sample size for computing gains at migration.

The goal of this appendix is to show how the various adjustments and sample restrictions affect the overall sample size for the results used in the paper. The figures are summarized in Table A1. We focus here on the 4,116 main sample respondents who worked before migrating to the United States. Of these, 3,171 have worked since coming to the United States, which represents our total potential sample.

The second and third rows show the effect of limiting the sample to workers who report their earnings (instead of saying that they do not know or do not want to answer) and whose earnings can be PPP-adjusted to a U.S. hourly wage (which requires us to know their pay period, how many hours they work per pay period, the appropriate exchange rate, and the appropriate cost of living adjustment). Although the NIS provides PPP-adjusted wages, we readjust all foreign wages ourselves in order to be able to identify potentially problematic cases. If we instead use the provided figures, we find a somewhat larger human capital share in development accounting: 66 percent instead of 62 percent in the baseline analysis.

The cost of living adjustment is either directly provided and called the price level (P) in older editions of the Penn World Table, or it is constructed as the ratio of PPP to nominal exchange rates ($PPP/XRAT$).¹⁵ We use PWT 7.1 for most countries because it uses year 2005 reference prices, which is the closest price benchmark year to our data period. We also collect some data from earlier versions of the Penn World Table to minimize sample loss. We get pre-euro European exchange rates from PWT 6.2; pre-dollarization Ecuadorian exchange rates from PWT 6.1; and exchange rates for the U.S.S.R., Czechoslovakia, Yugoslavia, and Myanmar from PWT 5.6 (Heston et al., 2012, 2006, 2002, n.d.).

We also explored using instead the newest edition of the Penn World Tables, version 9.0. The main potential advantage of using PWT version 9.0 is that it constructs GDP comparisons using chained PPPs, which allows us to compute wage gains at migration using a time-varying purchasing power adjustment. However, a trade-off of the methodology used to compare living standards with chained PPPs is that it does not explicitly define the reference prices (the PPP exchange rates), and they are not provided in the new Penn World Tables; see Feenstra et al. (2015). We explored naively backing out an implied PPP by taking the ratio of nominal to real chained GDP for every country and year. When we use these implied PPPs, we find similar and actually slightly smaller wage gains at migration and hence a larger implied human capital share for development accounting: 0.72 for a

¹⁵We use the standard PPP exchange rates so that our results are comparable to PPP GDP per worker gaps and the broader development accounting literature. If we were interested in the migrants' increase in purchasing power, then it would be preferable to use a PPP exchange rate more specific to their consumption bundle (Gibson and McKenzie, 2012).

sample of 1,276 immigrants as compared to 0.62 for a sample of 1,334 in the baseline case. This finding suggests to us that using fixed PPPs does not drive our results.

The fourth row of the table shows the effects of excluding workers who have received schooling in the United States since their human capital has obviously changed, which violates the spirit of our exercise. Finally, the balanced sample of 1,374 workers excludes some outliers in the wage distribution as well as immigrants who report being paid in currencies that were subsequently devalued or whose last pre-migration job was before 1983. The actual sample in the paper is larger than this because we include 632 immigrant spouses and parents as described above.

Figure A1: Distributions of Wages and Wage Gains

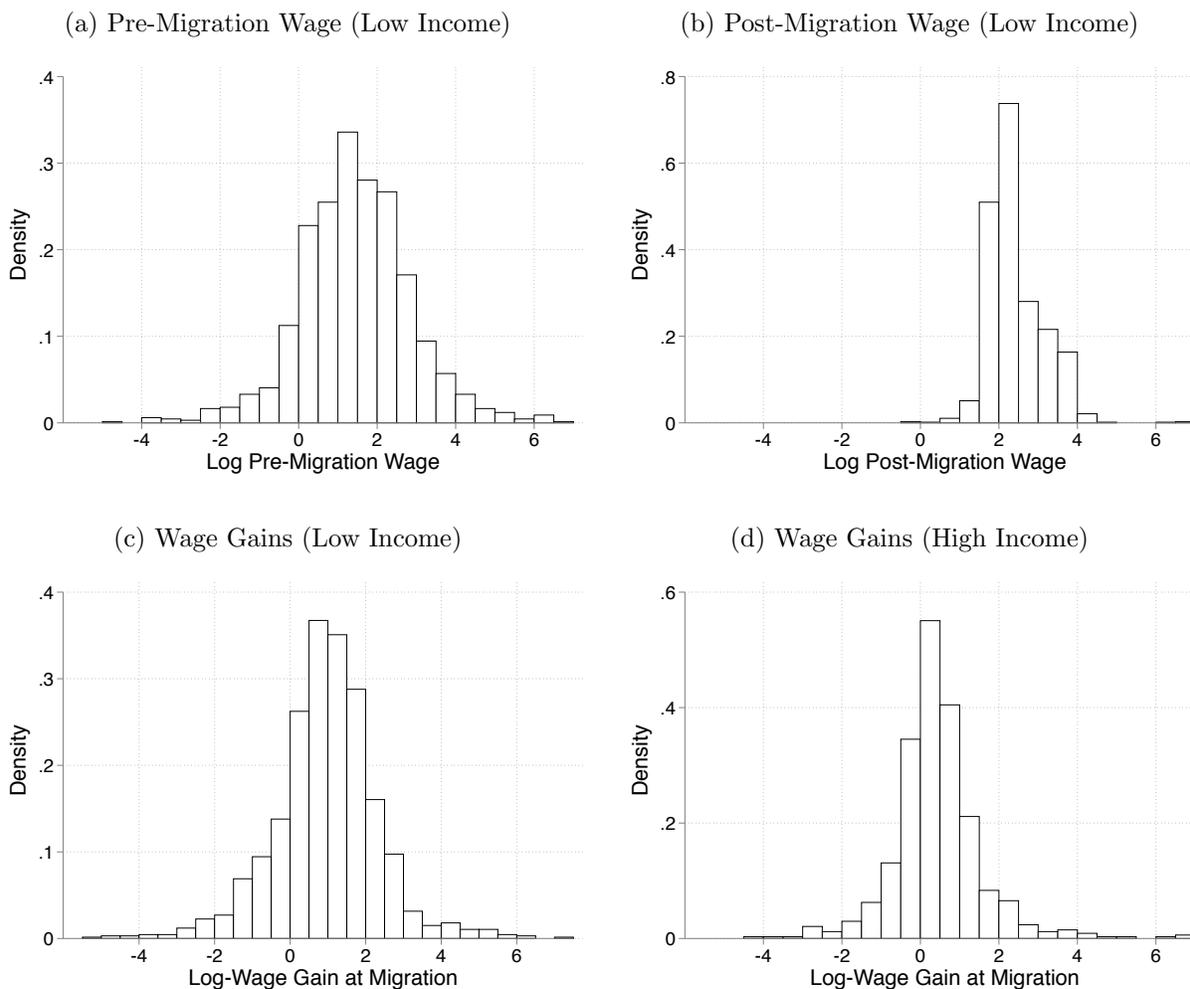


Figure A1 shows the distributions of wages and wage gains at migration. Figures A1(a)–

A1(c) show pre-migration wages, post-migration wages, and the wage gains at migration for countries with PPP GDP per worker less than one-quarter of the United States in 2005; this is the group of focus in the paper. Low pre-migration wages (less than one dollar per hour) are rare, and post-migration wages are generally between the minimum wage and ten dollars per hour. Combined, these figures imply Figure A1(c): most immigrants have small wage gains. For reference, Figure A1(d) shows the wage gains for immigrants coming from countries with PPP GDP per worker more than one-quarter of the United States in 2005; the distribution is concentrated closer to 0, indicating smaller wage gains for immigrants from richer countries.

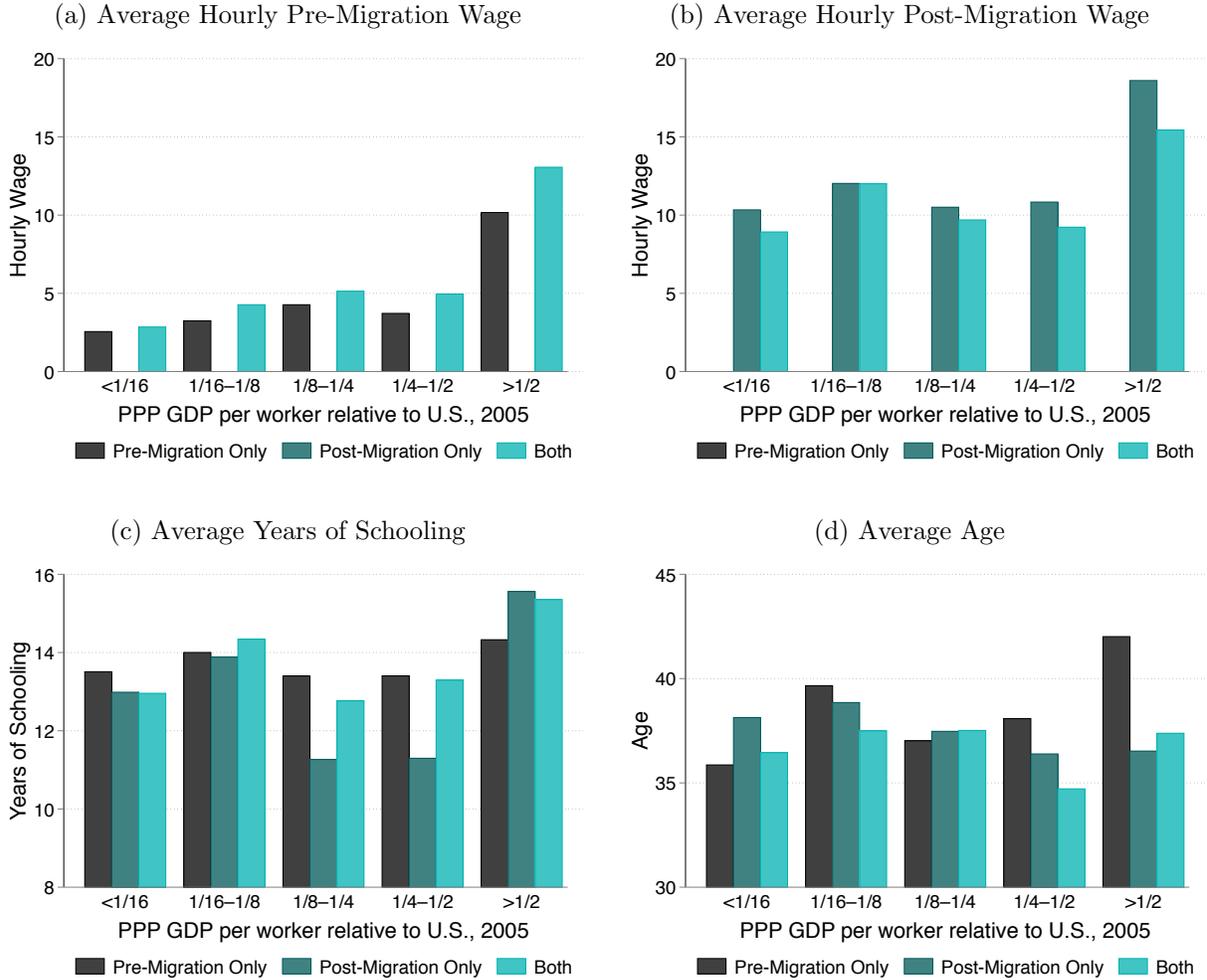
Our primary focus is on the sample of workers for whom we can reliably construct the wage gains at migration. A possible concern is that this sample is somehow selected to be different from typical migrants. To check this, we compare the balanced NIS sample to the unbalanced NIS sample. For each, we compare mean age, education, and hourly wage by GDP per worker category.

Within the NIS, we look for systematic differences between three mutually exclusive groups of immigrants: the baseline sample (with both pre- and post-migration wages); those with only pre-migration wages; and those with only post-migration wages. The results are displayed in Figure A2. Panels (a) and (b) show that the wages are fairly similar between the groups. This, of course, does not imply anything about the unobserved wages. However, our key finding is small wage gains at migration. In order to overturn this result, it would need to be the case that immigrants who do not work after migration would have earned exceptionally high post-migration wages (despite having had very typical pre-migration wages) or that immigrants who work after migrating but did not before would have earned exceptionally low pre-migration wages (despite having had very typical post-migration wages). Panels (c) and (d) give the same comparisons for age and schooling. The largest discrepancy is that immigrants for whom we have only a post-migration wage from the third and fourth GDP per worker category have 1.5–2 years less schooling than does the balanced sample. Otherwise, the characteristics line up well.

A.2 Migration Project Details

In this appendix we give the data construction details for the Mexican Migration Project (MMP) and the Latin American Migration Project (LAMP). MMP has collected information about employment and migration experiences for households across Mexico since 1982.

Figure A2: Comparison of NIS Balanced and Unbalanced Samples



LAMP is a collection of data sets that are structured similarly to MMP and cover nine other Latin American countries. The number of communities surveyed and the interview years are summarized in Table A2.

We focus our attention on native-born household heads and spouses aged 18 to 75 at the time of the interview with known educational attainment and valid wages earned on their last jobs at home and in the United States. For both home and U.S. jobs, we require that the individual be aged 18 to 65. Potential experience, defined as age minus years of schooling minus 6, must be non-negative. Wages must be paid in their natural currencies (those that match Penn World Table 7.1 currency units for each country).

Wages are converted into U.S. dollars using PPP exchange rates from PWT version 7.1

Table A2: Summary of Migration Project Data

	Year Range	Communities	Households	N
Colombia	2008–2015	15	3,023	26
Dominican Republic	1999–2001	7	978	76
Ecuador	2012–2012	4	803	5
Guatemala	2004–2004	3	513	28
Haiti	2000–2002	3	303	25
Nicaragua	2000–2003	9	1,789	42
Mexico	1982–2015	154	25,719	2,377
Peru	2001–2005	5	822	9
Puerto Rico	1998–1999	5	646	228
El Salvador	2007–2007	4	382	38

Note: Summary of data available after pooling the MMP and LAMP. For each country, the columns show s the year range of the data collection, the number of communities and households surveyed, and the number of individuals who report valid pre- and post-migration wages.

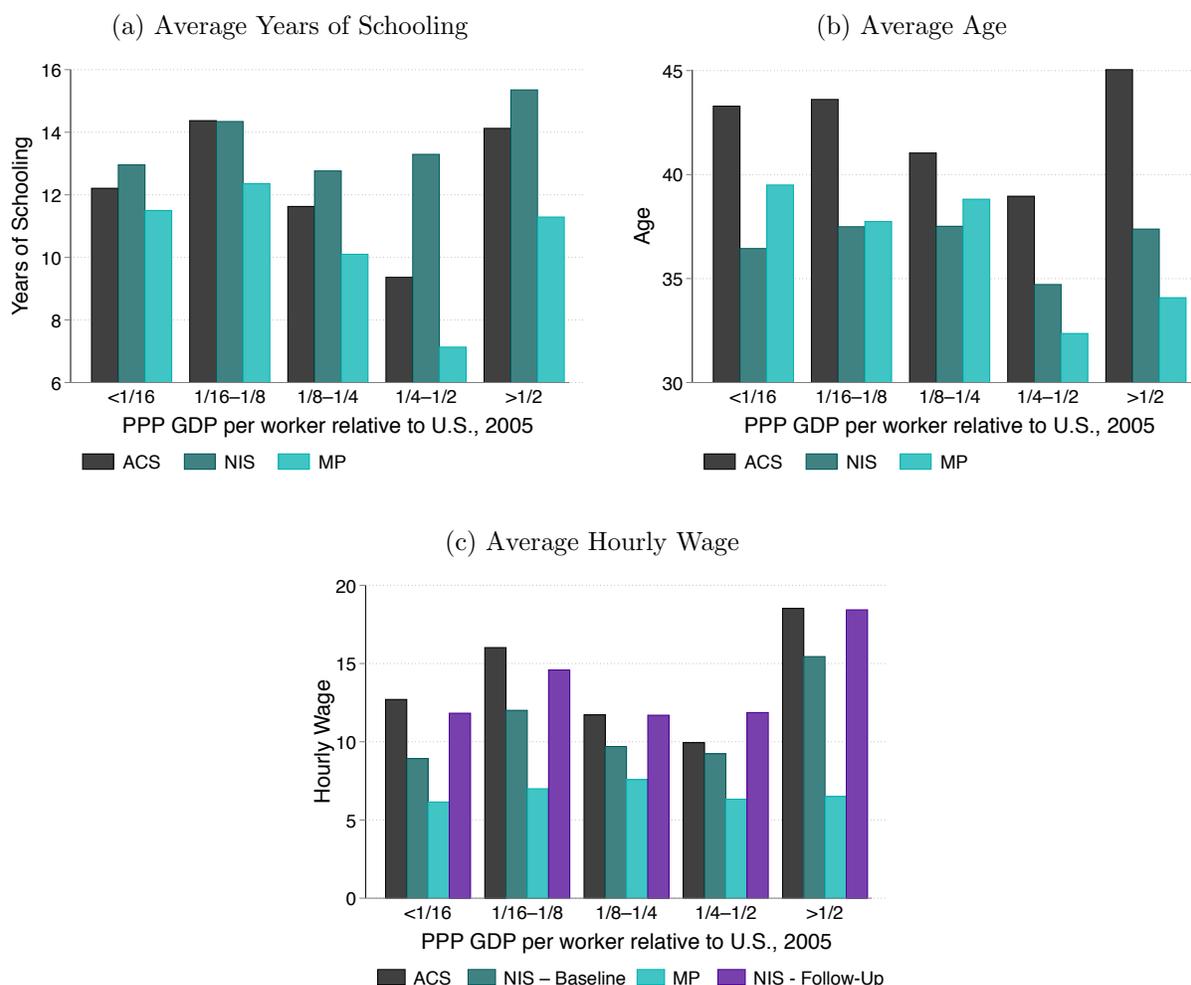
(since we restricted our attention to workers paid in the natural currency). Since PWT version 7.1 data end in 2011, we drop home country wages earned after that year. We adjust the earlier wage (generally the U.S. wage) to the date of the later wage (generally the foreign wage) using the wage growth of observably similar Americans. This procedure is conceptually similar to what we did with the NIS, except that it is not possible to adjust wages to year 2003 for some immigrants in the MP data. Because of this restriction, we focus on wage changes at migration and ignore wage levels.

A.3 Comparison of Data Sources to ACS

It is useful for context to compare the immigrants in the NIS and MP to the ACS. We expect there to be differences because the sampling frames are quite different: the NIS focuses on new lawful permanent residents; the MP focuses on immigrants from Mexico and Latin America who are quite likely to have entered the country illegally; and the ACS is a sample of all immigrants. Our goal is to show the importance of these differences in the sampling frames. In Figure A3 we plot average years of schooling, age, and hourly wage in the United States by GDP per worker category for each source.

Immigrants in the NIS have more education than those in the ACS, who in turn have more

Figure A3: Comparison of NIS and ACS Samples



than those in the MP, as one would expect. Immigrants in the NIS especially tend to be much younger, which is consistent with their being new immigrants. Finally, immigrants in both the NIS and the MP have lower wages in the United States than do immigrants in the ACS. Much of this gap can be attributed to the fact that about half of the immigrants in the NIS sample are newly arrived to the United States and so have low initial wages in their first job. To demonstrate this, in panel (c) we also show the results if we use only wages earned in the round 2 follow-up, taken 3–6 years after round 1. We find wages rise considerably over this time period and that the NIS wages match closely with the ACS wages for the broader sample of immigrants. We conclude that the balanced NIS sample is younger and somewhat better educated than the overall set of immigrants in the United

States and that their wages start out lower but converge rapidly to the U.S. immigrant average. On the other hand, the MP gives us a useful complement because its immigrants are much less educated and earn much lower wages.

A.4 American Community Survey

At several points in the paper, we use the American Community Survey (ACS) as a benchmark for our NIS results. We focus on the 2004 ACS, which is a large, nationally representative sample of the U.S. population. The 2004 ACS asked respondents retrospective questions about their 2003 labor market outcomes, which fits well with the time frame for the NIS.¹⁶ We focus on a standard sample of employed wage workers aged 16-70. We construct hourly wages using annual labor income, usual hours worked per week, and weeks worked in the previous year. We code the reported educational attainment codes into years of schooling in line with standard practice.

We study two groups in the ACS. The first is natives, defined as individuals born in the United States or its territories. The second is immigrants, defined as individuals born outside the United States or its territories. For immigrants, we further restrict the sample to exclude workers with imputed wages or who are likely to have completed some schooling inside the United States. The latter adjustment is based on reported schooling and the year of immigration.

Most of the results (such as mean native wages by occupation) are fairly straightforward, particularly since the NIS and the ACS use the occupation and industry coding schemes. The lone exception is the facts about residual wages of immigrants. To construct residual wages, we first run a standard wage regression using only natives, where log hourly wage is regressed on five-year age bins (15–19, 20–24, .. 65+), a gender dummy, and years of schooling. An immigrant’s residualized wage is then his or her log-wage net of the predicted wage from this regression.

A.5 Current Population Survey

We obtain data from the March Current Population Surveys from IPUMS ([King et al., 2010](#)). The sample contains workers between the ages of 16 and 66 with valid information on education (EDUC). We also require positive earnings weights (EARNWT) or person

¹⁶Data downloaded from IPUMS; see [Ruggles et al. \(2010\)](#).

weight (PERWT, when EARNWT is not available), positive wage and salary incomes (INCWAGE), at least 20 weeks worked (WKSWORK2), and between 20 and 80 hours worked per week (HRSWORK). We drop persons who do not work for wages or salaries (CLASSWKR < 20 or > 28) and members of the armed forces (CLASSWKR = 26). The hourly wage is defined as INCWAGE / WKSWORK2 / HRSWORK. Observations with wages below 5 percent of the median wage or above 200 times the median wage are dropped. We estimate mean log wages by (experience, schooling, sex, year) by regressing log wages on a quartic in experience (defined as age - schooling - 6) and dummies for four school categories (high school dropouts, high school graduates, some college, college graduates; based on EDUC).

B Robustness

The goal of this section is to explore the robustness of our main results. Throughout we focus on the subsample of immigrants from countries with GDP per worker less than one-quarter of the U.S. level in the NIS. We also focus on the human capital share implied by the perfect substitutes case. The range for the imperfect substitutes case moves closely in line with this value, so it is a useful guide to what sorts of factors would lead us to revise the estimated plausible role for human capital up or down.

Five poor countries have at least 50 migrants in our sample, allowing us to report meaningful country-specific results: Ethiopia, India, the Philippines, China, and the Dominican Republic. These countries span the GDP per worker range of interest and provide concrete cases to consider. An additional advantage of these countries is that each has had a single, relatively stable currency, mitigating concerns about potential difficulty in correctly converting the pre-migration wage to U.S. dollars.

Figure B1 shows the results for wages and wage gains for these countries, ordered by PPP GDP per worker. The wage levels vary in interesting ways related mostly to heterogeneity by country in visas. The wage gains are in line with the baseline result and generally decrease with development. The implied human capital share in development accounting is given in panel B of Table B1. The implied share ranges from 0.51 to 0.89, in line with the baseline result but somewhat more variable.

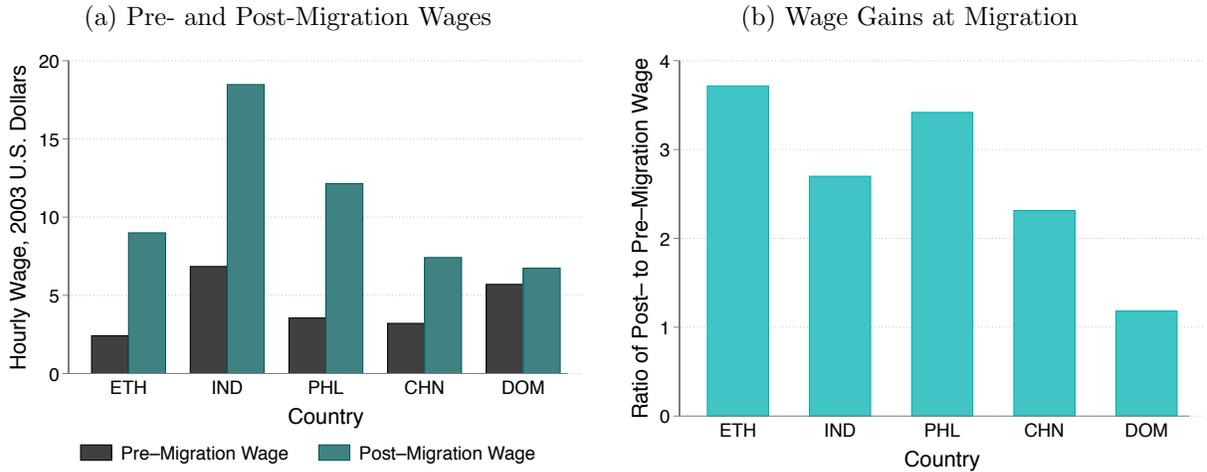
As a second decomposition, we exploit the available information on each immigrant's visa status. As noted above, the NIS includes each immigrant's visa type, coded from INS files. We aggregate categories slightly, grouping the family visas together and grouping

Table B1: Human Capital Share in Development Accounting by Subgroups

Robustness Check	Human Capital Share	95% Confidence Interval	N
<i>Panel A: Baseline</i>			
Baseline	0.62	(0.58, 0.65)	1,334
<i>Panel B: Results by Country</i>			
Ethiopia	0.70	(0.61, 0.80)	55
India	0.62	(0.57, 0.67)	232
Philippines	0.51	(0.44, 0.58)	166
China	0.64	(0.54, 0.74)	101
Dominican Republic	0.89	(0.71, 1.07)	51
<i>Panel C: Decomposition by Visa Status</i>			
Employment visa	0.56	(0.50, 0.62)	258
Family visa	0.65	(0.56, 0.75)	226
Diversity visa	0.59	(0.51, 0.67)	257
Refugee/Asylee visa	0.46	(0.28, 0.64)	47
Other visa	0.61	(0.51, 0.72)	149

Table note: Each column shows the implied human capital share in development accounting (one minus the wage gain at migration relative to the GDP per worker gap); the 95 percent confidence interval for that statistic; and the number of immigrants in the corresponding sample. Each row shows the result from constructing these statistics for a different subsample.

Figure B1: Wages for Select Countries



legalizations with “other” so that we have five categories: employment, family, diversity, refugee (and asylee), and other. It is worth noting that the U.S. government groups families and certain other cases together under the visa of the primary migrant for administrative purposes, so the spouse accompanying an immigrant who enters with an employment visa will also be recorded as having entered with an employment visa in this system. There are large differences in the level of pre- and post-migration wages by visa category. For example, immigrants on employment visas earn wages that are roughly twice the sample average before and after migrating, even if we control for country fixed effects. However, if we focus on the wage gain at migration and the implied human capital share, we find much less variation, shown in panel C of Table B1. Other than refugees (of whom we have relatively few), immigrants with different visas are clustered in a fairly narrow range between 56 and 65 percent.

B.1 Robustness: Assimilation

In this section we show that our results are robust to alternative ways of incorporating assimilation. We have two ways to think about the process of assimilation. The first is to study how our results vary if we focus on the first post-migration job, the round 1 job, or the round 2 job; assimilation generally implies that immigrants will have higher wages at later jobs. Indeed, we find that this is the case, and as a result, the implied human capital share for development accounting falls somewhat if we focus on the later rather than on

Table B2: Robustness: Human Capital Share in Development Accounting and Assimilation

Robustness Check	Human Capital Share	95% Confidence Interval	N
<i>Panel A: Baseline</i>			
Baseline	0.62	(0.58, 0.65)	1,334
<i>Panel B: Decomposition by Date of Post-Migration Job</i>			
First Post-Migration Job	0.65	(0.61, 0.69)	1,129
Round 1 Job (2003–2004)	0.60	(0.56, 0.64)	1,017
Round 2 Job (2007–2009)	0.53	(0.47, 0.60)	387
<i>Panel C: Decomposition by Year of Arrival</i>			
1988–1997 arrivals	0.58	(0.49, 0.67)	186
1998–2002 arrivals	0.58	(0.50, 0.66)	241
2003 arrivals	0.61	(0.55, 0.66)	576

Table note: Each column shows the implied human capital share in development accounting (one minus the wage gain at migration relative to the GDP per worker gap); the 95 percent confidence interval for that statistic; and the number of immigrants in the corresponding sample. Each row shows the result from constructing these statistics by comparing wages at different combinations of pre- and post-migration occupations (panel B) or by focusing on subsets of workers who immigrated during different periods (panel C).

the earlier jobs, as shown in panel B of Table B2.

The second way we can think about assimilation is to study workers who immigrated to the United States at different times and hence have been in the United States for shorter or longer periods. To do so, we focus our attention on immigrants who reported a valid wage for the job at round 1, and separate three subgroups: immigrants who arrived in the United States in 2003 (new arrivals), those who arrived between 1998 and 2002, and those who arrived before or during 1997. Because they arrived at different times, these groups have had varying periods over which to assimilate. Nonetheless, we see from Table B2 that our results are very similar across these groups.

As we discussed above, it is not clear whether assimilation is driven by human capital accumulation, the acquisition of search capital, or some other force, and hence it is not clear which of these estimates to focus on. However, the range of results is narrow, which suggests that how we treat assimilation is not central to our conclusion.

These results also provide suggestive evidence against a large role for recall bias. A possible concern with our approach is that we rely on retrospective measures of pre-migration wages to construct the wage gain at migration. If immigrants have a tendency to inflate their pre-

migration wages in their memory, this would bias downward the wage gain at migration and bias upward our implied human capital share for development accounting. Nonetheless, we can see from panel C of Table B2 that the implied human capital share for development accounting is similar for immigrants who arrived shortly before the survey and those who arrived much earlier. Likewise, the patterns are similar if we cut the data by the year of the last pre-migration job. For example, the implied share of human capital in development accounting is 58 percent if we focus solely on immigrants whose last pre-migration job and wage were for the year 2003. This suggests that recall bias is unlikely to drive our results, since we would expect very recent migrants to correctly remember the wages in jobs they worked abroad shortly before the survey.

B.2 Other Robustness

We now conduct a number of robustness checks in order to study the results in more detail. For each robustness check, we vary the data construction or focus on a particular subsample of interest. We focus throughout on immigrants from countries with GDP per worker less than one-quarter of the U.S. level. To compare the results using a common metric, we report the estimated share of human capital in development accounting for each exercise. We also report the corresponding 95 percent confidence interval and number of immigrants in the subsample. The results are reported in Table B3.

Panel A reports again the baseline results discussed above, for comparison. Panel B reports the results from a number of checks on the details of migration. We experiment with including only the immigrants who were sampled (excluding spouses and parents) or including only those whose first and only migration was to the United States. The second to last row of panel B constrains attention to immigrants with simple immigration histories, meaning that they had never left their birth country for more than six months before migrating to the United States, and that they worked their last job in their birth country within one year of their first job in the United States. The last row shows results for immigrants who report both speaking ENGLISH and understanding spoken English well or very well. The results throughout are very similar to the baseline.

Panel C reports the results from a number of robustness checks dealing with the construction of wages. The first row reports the results using only workers who worked for wages before and after migrating. The second row reports the results when we trim more potential outliers, now including anyone who reports less than \$0.10 per hour in their birth country,

Table B3: Robustness: Human Capital Share in Development Accounting

Robustness Check	Human Capital Share	95% Confidence Interval	N
<i>Panel A: Baseline</i>			
Baseline	0.62	(0.58, 0.65)	1,334
<i>Panel B: Robustness to Migration Details</i>			
Sampled interviewees only	0.60	(0.56, 0.65)	902
Direct migration to U.S.	0.65	(0.61, 0.68)	1,198
Simple migration cases	0.61	(0.57, 0.65)	1,087
Speaks and understands English	0.59	(0.54, 0.65)	543
<i>Panel C: Robustness to Wage Construction and Job Type</i>			
Wage workers	0.58	(0.55, 0.62)	1,140
Trim outliers	0.59	(0.56, 0.62)	1,238
Total compensation adjustment	0.52	(0.48, 0.55)	1,334
Non-competitive foreign labor market	0.53	(0.50, 0.57)	1,334
Only men	0.65	(0.61, 0.70)	810
<i>Panel D: Robustness to Currency Conversion Complications</i>			
Currency-country match	0.61	(0.57, 0.64)	1,276
No revaluations ever	0.63	(0.59, 0.67)	985
No high inflation	0.61	(0.57, 0.64)	1,310
No high inflation ever	0.63	(0.59, 0.67)	825

Table note: Each column shows the implied human capital share in development accounting (one minus the wage gain at migration relative to the GDP per worker gap); the 95 percent confidence interval for that statistic; and the number of immigrants in the corresponding sample. Each row shows the result from constructing these statistics for a different sample or using different measures of pre-migration wages, post-migration wages, or the GDP per worker gap.

less than \$5.00 per hour in the United States, or more than \$100 per hour in either country. The third row includes an adjustment to wages for total compensation. The idea is that the pre-migration wages in poor countries may reflect total payments to labor, whereas wages in the United States do not include benefits. To see whether this might matter, we multiply the reported U.S. wage by the national average ratio of total compensation to wages and salaries, which is 1.23, taken from national income and product accounts. Along the same lines, the fourth row studies the implications of a non-competitive foreign labor market. In this case, the foreign wage could exceed the marginal product of labor, which would cause us to understate the wage gains at migration. It is hard to know how large or widespread such deviations might be. To give a sense of magnitude, we explore lowering all foreign wages by 20 percent. This figure is at the upper end of the most widely studied wage distortion, the union wage premium ([Jakubson, 1991](#)). The last row includes only men. The results in all cases exceed one-half.

Panel D reports robustness to the details of currency conversion. We find similar results if we focus on cases where immigrants report being paid in a currency that “matches” their country of work, or if we exclude immigrants who report being paid in currencies that have ever been devalued. Data on currency-country pairs come mostly from the Penn World Tables and the CIA Factbook; we have also allowed some pairs where a currency is not the official currency of a country but has been in common use, such as the U.S. dollar in former Soviet economies in the 1990s. Recall that our baseline results already exclude immigrants who were paid in a currency that has been subsequently devalued. We also find similar results if we exclude immigrants who were paid in currencies that have subsequently or ever experienced high inflation. Inflation data come from the [World Bank \(2014\)](#).

Across all of these subgroups and robustness checks, we find that the human capital share in development accounting is remarkably consistent, in the range of 0.52–0.65, suggesting that it is not driven by complicated migration experiences, wage construction, or wage adjustment. Given that our results are robust, we turn to understanding the relationship between these results and the literature.

C Comparison with Literature

The goal of this appendix is to compare our approach and results with earlier work that uses immigrants to study human capital and cross-country income differences, particularly [Hendricks \(2002\)](#) and [Schoellman \(2012\)](#). We start with the former. As discussed in Section

Table C1: Replication of Literature Results

Sample	Intercept	Log(GDP per worker)
ACS	0.791*** (0.033)	0.113*** (0.017)
NIS (all countries)	0.461*** (0.070)	0.084** (0.035)
NIS (at least 10 migrants)	0.432*** (0.068)	0.095*** (0.034)
NIS + MP	0.461*** (0.070)	0.084** (0.035)

Note: Dependent variable is mean log residualized wage, averaged by country of birth. Residualized wages computed as discussed in Appendix C. Standard errors are in parentheses. *** and ** denote estimates that are statistically different from college graduates at the 99 and 95 percent level.

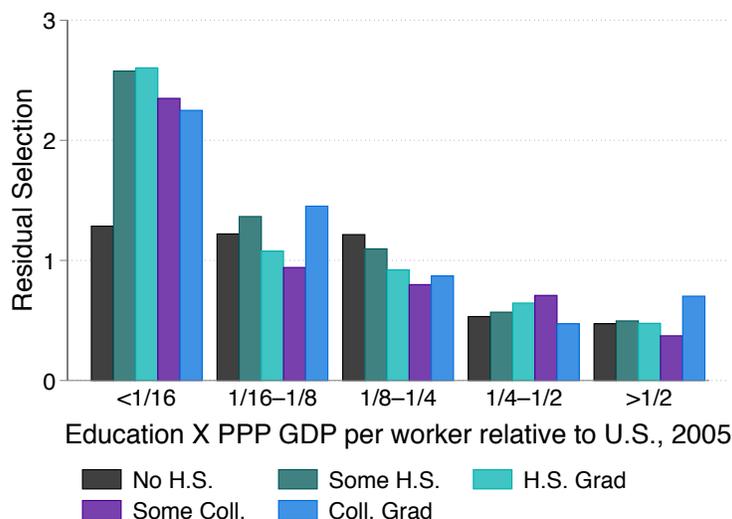
2, the key empirical object in [Hendricks \(2002\)](#) is the difference in residualized log-wages between immigrants from poor and rich countries. A useful way to summarize this statistic is to regress residualized log wages on birth country log GDP per worker (year 2005), a statistic first proposed in [Borjas \(1987\)](#). We estimate this relationship in the American Community Survey, which is the successor data set to the census and is widely used to study immigrants. Table C1 shows the results. In the ACS we find that the elasticity of residualized wages with respect to GDP per worker is 0.113, which is also similar to the coefficient of 0.123 reported by [Hendricks \(2002\)](#) using the 1990 census. In the following two rows of the table, we compute the same statistic in our main data set, the NIS. If we use all countries (including many with very few immigrants), we find the elasticity to be 0.084; if we use only countries with at least 10 immigrants in the sample, we get an elasticity of 0.095.

These findings are all quite close, which implies that the different data sources agree about the variation in residualized wages between poor and rich countries. This fact isolates the main difference between our work and that of [Hendricks \(2002\)](#), which is the assumption about the correlation between selection on unobserved characteristics and source country PPP GDP per worker. While [Hendricks \(2002\)](#) assumed that there was no correlation, we find a strong negative correlation (Figure 2). Hence, the gap in unobserved human capital between non-migrants from rich and poor countries is found to be much larger than in [Hendricks \(2002\)](#).

[Schoellman \(2012\)](#) relies on a different assumption: that selection on unobserved charac-

teristics not be correlated with schooling for immigrants from a given country. Given the small sample size by country, we cannot test this assumption in our data. However, we can conduct a weaker test, which is to look at the selection on unobserved characteristics by education and GDP per worker group. We pool the NIS and the MP samples to give the broadest possible coverage of the education spectrum. The patterns are shown in Figure C1. Although there are clear differences in selection on unobserved characteristics, we do not see a strong pattern of correlation between selection and education within a GDP per worker group.

Figure C1: Selection of Immigrants by GDP per worker and education level



We suspect the main difference between this paper and that of [Schoellman \(2012\)](#) is one of scope. [Schoellman \(2012\)](#) constructed estimates of only the human capital generated by quality-adjusted schooling, whereas this paper estimates all forms of human capital. It might be larger if there are cross-country differences in other forms of human capital, such as life-cycle human capital accumulation or health human capital ([Lagakos et al., forthcoming-a,b](#); [Weil, 2007](#)).