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

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Abstract

We reconsider the role for human capital in accounting for cross-country income differences. Our contribution is to bring to bear new data on the pre- and post-migration labor market experiences of immigrants to the U.S. Immigrants from poor countries experience wage gains that are only 40 percent of the GDP per worker gap. This fact implies that "country" accounts for only 40 percent of cross-country income differences, while human capital accounts for the other 60 percent. Our work deals with two well-known problems in the literature. It controls for selection by using data on the wages of the same individual in two different countries. We provide evidence on the importance of skill transfer by comparing pre- and post-migration occupations. Occupational downgrading at migration is common; corrections for this imply that human capital may account for as little as 50 percent of cross-country income differences.

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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, the consensus in the literature is that physical capital accounts for a small fraction of income differences, which has suggested to 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 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 U.S. with the human capital they acquired in their birth country, but not the physical capital or TFP. Hence, their labor market performance in the U.S. can be used to learn about the relative importance of human capital versus 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]

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

[2] The former figure comes from Hall and Jones (1999); the latter comes from Manuelli and Seshadri (2014) or Jones (2014). The literature also includes a wide range of estimates in between. See, for example, Erosa et al. (2010), Hanushek and Woessmann (2012), Cordoba and Ripoll (2013), Weil (2007), or Cubas et al. (2015).

[3] Previous papers that have investigated immigrants and cross-country differences in human capital include Hendricks (2002), Schoellman (2012), Schoellman (forthcoming), and Lagakos et al. (2015).

We address these challenges by utilizing new data from the New Immigrant Survey, a sample of immigrants granted lawful permanent resident status in the United States in 2003 (Jasso et al., 2006). The unique advantage of this dataset is that it asked immigrants detailed questions about both their pre- and post-migration labor market experiences. We use this data in three ways. First, we construct a measure of the importance of human capital for development accounting based on the wage gains immigrants experience upon 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. We describe the assumptions that are necessary to draw aggregate implications from the labor market experiences of immigrants. We show that the most direct measure of the importance of physical capital and TFP is the log-wage gain at migration relative to the log difference in GDP per worker. Intuitively, the idea is that 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 New Immigrant Survey offers carefully constructed and detailed wage data. It surveyed immigrants about up to two pre-migration jobs and up to two 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, and differences in reporting year to arrive at estimates of their pre-migration and post-migration hourly wages both denominated in real PPP-adjusted U.S. dollars. We also provide detailed information on sensitivity and robustness checks to possible confounding issues such as episodes of inflation or currency revaluation, migrants who report working in their non-birth country, and so on.

We use these data to construct the log wage change at migration relative to the log gap in GDP per worker. We focus on immigrants from poor countries, with PPP GDP per worker less than one-fourth the U.S. level. We find that the average wage gain at migration is

39 percent of the total gap in GDP per worker, implying that 39 percent of cross-country income differences are accounted for by physical capital and TFP, with the remaining 61 percent accounted for by human capital. We show that this figure is robust to many of the details of sample selection and wage construction. For example, similar results hold for immigrants who entered the U.S. with very different education levels and on very different visas.

This finding attributes 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 speak directly to two other important issues. The first is the transferability of skills of immigrants. To investigate this issue, we compare the pre-migration and post-migration occupations of immigrants. We find most immigrants switch occupations, regardless of whether we define switching on the basis of detailed occupations or broad occupation groups. Further, we find that 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 a difficulty transferring skills, 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.

The second issue we can speak to is how to aggregate labor provided by workers with different education levels. Although the development accounting literature usually assumes that they are perfect substitutes, Jones (2014) has recently shown that even moderate degrees of imperfect substitution would dramatically raise the importance of human capital in development accounting. The experiences of immigrants are useful for thinking about this issue because immigrants from poor countries move from a country where educated labor is scarce to one where it is abundant. If workers with different education levels are imperfect substitutes, then this implies that more educated immigrants should gain less at migration *relative* to less educated immigrants. Empirically, we find that wage gains are very similar across education groups, with no systematic trend. We conclude that a model with perfect substitution across education types fits our data well.

In addition to the work mentioned above, a few papers in the literature have investigated the wage gains at migration. Klein and Ventura (2009) and Kennan (2013) have used models to help quantify the gains from freer migration across countries. Empirically, several papers have looked at the actual wage gains to migration, typically for a limited set of countries. Clemens et al. (2008) mostly estimate the cross-country wage gaps between workers matched on observed characteristics, but also summarize some evidence from select countries on the actual wage gains to migration. McKenzie et al. (2010) and Gibson et al. (2015) offer useful experimental evidence on the returns to migration from Tonga to New Zealand. The use of a lottery to limit immigration allows them to estimate the gains to migration and control for selection on the gains to migration, which they find to be important. Nonetheless, for this single country pair their results imply a human capital share in development accounting of 0.52, in line with our results. Finally, we are not the first to use the pre-migration labor market information in the New Immigrant Survey. Probably the most related work is Rosenzweig (2010). The goal of this paper is to use immigrants' experiences to estimate a rich and flexible set of prices for a variety of skills. While useful, this evidence is difficult to interpret from a development accounting perspective.

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 and their robustness. Section 5 quantifies the importance of selection and Section 6 the importance of skill transferability. Section 7 investigates the elasticity of substitution between workers with different skill levels. Section 8 concludes.

2 Development Accounting Framework

We begin by outlining our accounting framework, which follows the literature closely (see Caselli (2005) or Hsieh and Klenow (2010) for recent overviews). Our particular focus here is on clarifying the assumptions needed to draw aggregate inferences from evidence on the wage gains at immigration. We start with the standard aggregate production function,

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

where Y_c is country c's PPP-adjusted GDP, K_c is its physical capital stock, A_c is its total factory 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 Rodriguez-Clare (1997), we re-write the production function in per worker terms:

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

where y_c denotes PPP-adjusted GDP per worker.[4] It is well-known that there is large variation in this object across countries. The goal of development accounting is to decompose variation in y into variation in three components, 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 effect of country, 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':

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

[4]The literature has also considered an alternative accounting equation that features the capital-labor ratio rather than the capital-output ratio. Since we focus only on differentiating between human capital and the sum of the other two factors, our results would be the same if we used that alternative equation.

Our goal is to provide guidance on the decomposition between human capital and country for development accounting.

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. Second, labor markets are assumed to be perfectly competitive, so that workers are paid their marginal product. Given these assumptions, we can write the labor demand problem of the representative firm as hiring a total quantity H_c of human capital at the prevailing wage per unit of human capital ω_c so as to maximize profits:

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

The first-order condition of the firm implies that the wage per unit of human capital is $\omega_c = (1-\alpha) z_c$.

The observed hourly wage of 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 they possess:

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

Given that we have data on both pre- and post-migration wages of immigrants, we can construct the log-wage gain to migration. If we divide this by the log-GDP per worker difference between b and US, we find a direct measure of the importance of countries:

$$\frac{\log(w_{i,US}) - \log(w_{i,b})}{\log(y_{US}) - \log(y_b)} = \frac{\log(z_{US}) - \log(z_b)}{\log(y_{US}) - \log(y_b)} = \text{share}_{\text{country}} \tag{4}$$

We construct $\text{share}_{\text{human capital}} \equiv 1 - \text{share}_{\text{country}}$. Intuitively, the idea is that a worker who migrates keeps their same human capital but switches physical capital and TFP levels. We study how much this changes their wages relative to the total gap in GDP per worker.

If the change in wages is as large as the gap in GDP per worker, then we conclude that country explained all of cross-country income differences, with no role for human capital. If there is no change in wages, then we conclude that human capital explained all of cross-country income differences, with no role for country. Our goal is to calculate where we stand between these two polar cases.

A few remarks are in order at this point. First, note that this statistic controls for the usual selection concern, namely that immigrants may be more talented or harder-working than non-migrants. In Section 5 we actually quantify the extent of selection by comparing the pre-migration wages of immigrants to the wages of non-migrants. A more subtle concern is that immigrants may be selected on their gains to migration. We provide a simple model of this in Appendix C.1. The main intuition is that if immigrants are positively selected on gains to migration (as in McKenzie et al. (2010)), then we provide an upper bound on the gains to migration and a lower bound on the share of human capital in development accounting. Second, this simple equation assumes that skills transfer perfectly upon migration; we revisit this point in Section 6. Finally, we have maintained so far the assumption of perfect substitutes across skill groups that is common in most of the literature, but we revisit this point in section 7. Our goal at this point is to use this simple theory that maps micro-evidence on immigrants to the aggregate implications. We now turn to the data.

3 New Immigrant Survey

The New Immigrant Survey is a nationally representative sample of adult immigrants granted lawful permanent residence between May and November of 2013, drawn from government administrative records (Jasso et al., 2005, 2006). It includes both newly-arrived immigrants granted lawful permanent residency from abroad and immigrants who adjust to lawful permanent residency after previously entering the United States through other means. In Appendix B we compare the characteristics of this special sample to those of immigrants in the American Community Survey, which is widely used to study immigrants. Immigrants in the NIS earn lower wages than those in the ACS; roughly half of the gap can be attributed to differences in the composition of immigrants by country of birth and year of immigration, but some gap remains even after adjusting for these differences.

The sample consisted of 12,500 potential adult interviewees. The New Immigrant Survey sought to directly interview all of them between June 2003 and June 2004; they were able to do so in 68.6 percent of cases. The survey also collected detailed data on the spouse of the interviewee. In many cases the spouse was also an immigrant; in such cases, we include the spouse in our sample, although we show below 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.

The New Immigrant Survey includes four main sets of information that we exploit. First, it surveys respondents about the usual set of demographic characteristics, such as age and 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. It contains information on their first post-migration job and their current job at the time of the survey. For each, we know their occupation, industry, labor income (reported at different frequencies) and hours and weeks worked. We use this information to construct hourly wage in the U.S. for those who work. We generally focus on the current job rather than the first post-migration job. By doing so we hope to alleviate concerns about skill transfer and skill loss; the intervening period gives immigrants time to assimilate and find a job that matches their skills. We also consider using the first job as a robustness check below.

The fourth set of information we exploit covers immigrants' pre-migration experiences, particularly their labor market experiences. Immigrants were surveyed about up to two jobs before entry, their first (after age 16) and last (if different than the first). For each they were asked to report the same information as for their U.S. job, as well as when they held the job, the country where the job was held and the currency in which they were paid. Throughout, we focus on the most recent job.

Our goal is to study the pre- and post-migration wages of immigrants, but especially their wage gains at migration. It is important for our analysis that immigrants' reported wages be accurate. Fortunately, the New Immigrant Survey was careful to allow immigrants a great deal of flexibility in reporting their pre-migration wages. Immigrants reported both how much they earned and the frequency at which they were paid (hourly, daily, weekly, monthly, annual, 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 wages 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 widespread use of the U.S. dollar as a medium of payment even outside of the U.S., or the tendency for European migrants to remember their earnings denominated in both pre- and post-euro currencies.

Of course, this flexibility necessitates a great deal of adjustment on our part. First, we use the reported earnings and payment frequency to construct hourly wage for all immigrants. Denote this wage by $w_{d,b,t}$: hourly wage denominated in currency d from working in country b at time t. We then make three further adjustments. First, we translate the currency to U.S. dollars by using the market exchange rate between currency d and the dollar \$ prevailing at time t, taken from the Penn World Tables.[5] We use these exchange rates to convert the wage to the dollar equivalent, $w_{\$,b,t}$. We then adjust wages for the purchasing power parity prevailing in country b at time t, again taken from the Penn World Tables.[6] This yields an estimate of $w_{\$,US,t}$, the purchasing power parity-adjusted dollar wage at time t. Note that in cases where workers report the "natural" currency for their country (e.g., pesos in Mexico) these first two adjustments are equivalent to simply dividing by the PPP exchange rate.

Finally, we adjust for wage growth to translate all wage values into year 2003 equivalent, which is the main year of observation for post-migration wages. We perform this adjustment using U.S. wage growth for similar workers. We use Current Population Survey data to compute mean wage by age, gender, education, and year. We then inflate each worker's reported year t wage by using the observed wage growth for workers of the same age, gender, and education between year t and year 2003. This yields an estimate of $w_{\$,US,2003}$.[7] We trim a small number of outliers that report being paid less than \$0.01 or more than \$1,000 per hour; we find similar results if we implement stricter rules for trimming outliers. The final sample includes 1,292 immigrants with data on both pre- and post-migration wages that we use for our exercises. Table A1 in Appendix A shows the number of immigrants dropped by each of our sample restrictions.

Conceptually, our goal is to measure the wage gain from migration by comparing this adjusted pre-migration wage to the immigrant's post-migration US wage. There are three potential complications that we discuss here and explore further in our robustness section. First, we are concerned about immigrants who report being paid in currencies that have experienced large changes in value or revaluations. This raises the concern that immigrants

[5]We use PWT 7.1 for most countries. Our pre-euro European exchange rates come from PWT 6.2; our pre-dollarization Ecuadorian exchange rate from PWT 6.1; and our exchange rate for the USSR, Czechoslovakia, Yugoslavia, and Myanmar come from PWT 5.6 (Heston et al., 2012, 2006, 2002, n.d.).

[6]This object was provided directly and called price level (P) in some editions of the Penn World Table; in others it is constructed as the ratio of purchasing power parity to nominal exchange rates ($PPP/XRAT$).

[7]We previously adjusted only for aggregate changes in nominal U.S. GDP per worker between t and 2003 and found similar results.

may report wages in the wrong year, which would then substantially affect the implied value, or that they may report their wage in the wrong currency, such as Brazilian cruzeiros instead of reals. The New Immigrant Survey manuals document unusual patterns in the wage data for immigrants from countries with subsequent currency revaluations. For this reason, we exclude from the sample all workers who report wages in for a currency-year if there was a subsequent revaluation of the currency; the NIS manuals list the relevant currency-years. We also flag workers who report wages denominated in currencies that have ever had a revaluation or ever had high inflation but not a revaluation and consider robustness to excluding these workers.[8]

A second complication is that some workers report unusual country-currency pairs, for example being paid in lira in Brazil. As noted above, our adjustment is simpler for workers who report the "natural" country-currency pair. Further, one may suspect that at least some such unusual pairs may be the result of misreporting. We again flag these observations and consider the robustness of our results to their exclusion.[9]

A final complication arises from the gap between the last pre-migration job and the current job in 2003/2004. Conceptually, we would like to compare earnings just before and just after immigrating; in some cases, the delay between pre- and post-migration outcomes is long, introducing the possibility of changes in an immigrant's skills or circumstances. We take a number of steps to guard against these concerns. First, we exclude from all calculations immigrants whose last pre-migration job was before 1983 (twenty years gap). Our remaining sample is roughly evenly split between new arrivals and adjustments of previous arrivals. While there is a tail of those who worked their last pre-migration jobs in the 1980s, the median worker reports their last pre-migration job in 1999. We show below that our results are also robust to focusing on these more recent arrivals.

For the most part, the remaining immigrants from poor countries have straightforward immigration-job histories. For example, more than three-fourths of the resulting sample had never lived outside their birth country for more than six months before permanently immigrating to the U.S. Again, more than three-fourths report working their first US job within one year of their last pre-migration job; more than 70 percent of immigrants satisfy both restrictions. We show below that our results are robust to focusing on this group.

Recall that our goal is to compare the log-wage change at migration to the log difference

[8]Inflation data comes from the World Bank (2014).

[9]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.

Table 1: Most Sampled Countries by GDP per Worker Category

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

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

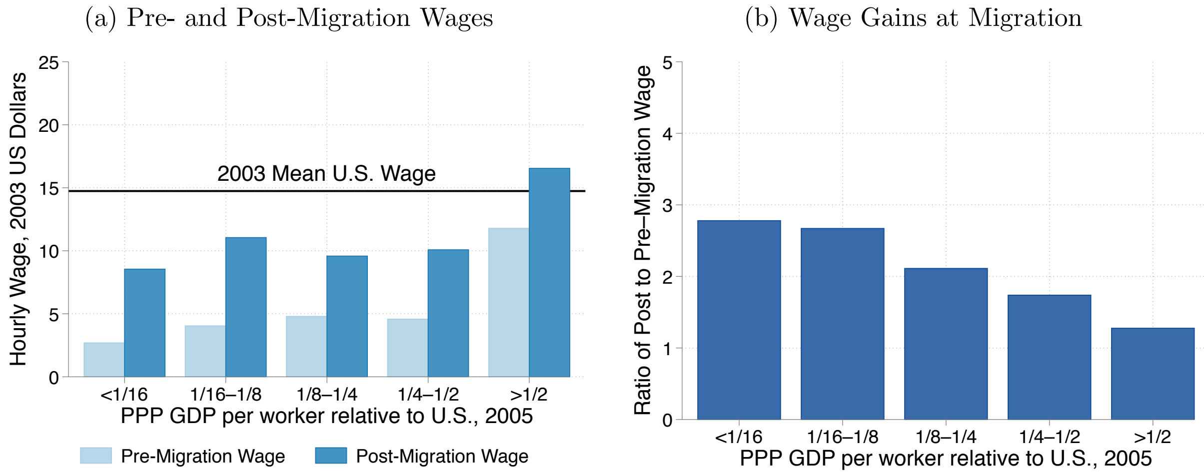
in GDP per worker. Our measure of the latter is the log-difference in GDP per worker between the U.S. and country b in 2005 from PWT 7.1, although all of our results hold if we use year-of-migration gaps in GDP per worker instead. Confidentiality restrictions prevent us from reporting statistics by country of origin in all but a few cases. For this reason our baseline approach is to report statistics for each of five PPP GDP per worker categories: less than 1/16th U.S. income; 1/16–1/8; 1/8–1/4; 1/4–1/2; and more than half. Table 1 lists the three countries with the most observations within each category.

4 Results

We now turn to our results. We begin by discussing the basic patterns of wages. Recall that our adjustments are designed to produce pre- and post-migration wages in 2003 US dollars, adjusted for purchasing power parity. We compute the mean of each wage by PPP GDP per worker category and plot the results in Figure 1a. 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 reported figures correspond to a PPP-adjusted hourly wage of $2.50 per hour even for immigrants from the very poorest countries.

A key statistic for our approach is the wage gain at migration, which we compute as the ratio of post- to pre-migration hourly wage. We show the results for the same groups of countries in Figure 1b. The typical immigrant has a substantial wage gain at migration. The extent of the gain is negatively correlated with development, as one would expect; immigrants from the poorest countries gain nearly a factor of 3, while immigrants from the richest gain less than a factor of 1.5. The gains for immigrants from poor countries are

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



quite small relative to the gap in GDP per worker, suggesting that "country" 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 country is the log-wage change at migration relative to the log-GDP per worker gap, with the importance of human capital constructed as one minus the importance of country. We implement this idea by constructing these statistics for every immigrant in our sample. We then compute the mean of this statistic within each PPP GDP per worker category. The resulting estimates and 95 percent confidence intervals for each GDP per worker category are given in Table 2.[10]

From this point on we focus on poor countries because they are of greater interest for development accounting. The estimates from the three poorest income groups agree closely on an estimate in the range of 0.57–0.70 with fairly tight confidence intervals. Overall, the implied share of human capital in development accounting is 61 percent against a share of country-specific factors of only 39 percent. The 95 percent confidence interval is narrow, ranging from 57 to 65 percent, implying that we can rule out that human capital accounts for as little as even half of cross-country income differences. We now decompose this result

[10]We find very similar results if we use instead the median of the ratios or if we use the ratio of the means. 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

GDP p.w. Category	Human Capital Share	95% Confidence Interval	N
$< 1/16$	0.70	(0.53, 0.78)	168
$1/16 - 1/8$	0.61	(0.56, 0.65)	386
$1/8 - 1/4$	0.57	(0.48, 0,67)	286
$1/4 - 1/2$	0.52	(0.32, 0.71)	160
$> 1/2$	0.88	(-0.07, 1.82)	292

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 gives 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.

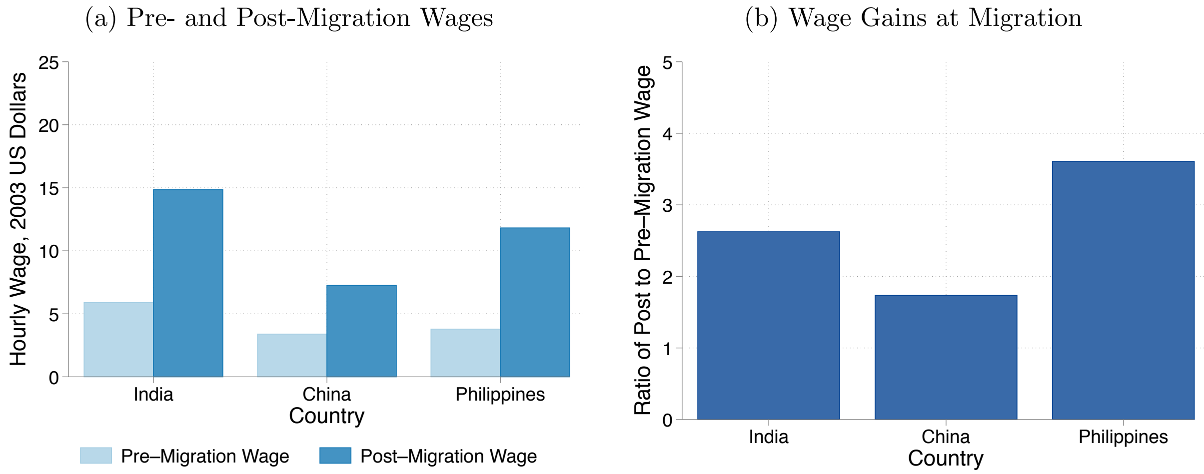
for different subgroups and consider its robustness.

4.2 Decomposition: Select Countries

Although confidentiality restrictions prevent us from reporting separate results for most countries, there are three poor countries above this threshold in our sample: China, India, and the Philippines. An additional advantage of these countries is that each has had a single, relatively stable currency, mitigating concerns about difficulty with correctly converting the pre-migration wage to U.S. dollars. At the same time there is interesting heterogeneity between them, in particular in how they arrived in the U.S.; while most Indian immigrants entered on employment visas, immigrants from China are fairly evenly mixed between employment, diversity, and family reunification visas.

Figure 2 shows the results for wages and wage gains for these three countries. Not surprisingly, the Indians are much more selected on wages than are the Chinese. Nonetheless, the wage gains at migration for each of these countries are very similar to the results found above, ranging from around two to a little less than four. We construct again the implied human capital share in development accounting for each country, shown in Panel B of Table 3. The implied share ranges from 0.49 to 0.76, in line with the baseline result but somewhat more variable.

Figure 2: Wages for Select Countries



4.3 Decomposition: Visa Status

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 refugees and asylees with "other" so that we have four categories: employment; family; diversity; and other. While we would ideally like to study refugees and asylees separately, there are unfortunately very few for whom we can calculate wage gains at migration. Our key question is whether the gain at migration is roughly the same for immigrants who enter for work, family reunification, and so on, or whether some immigrants have disproportionately large gains.

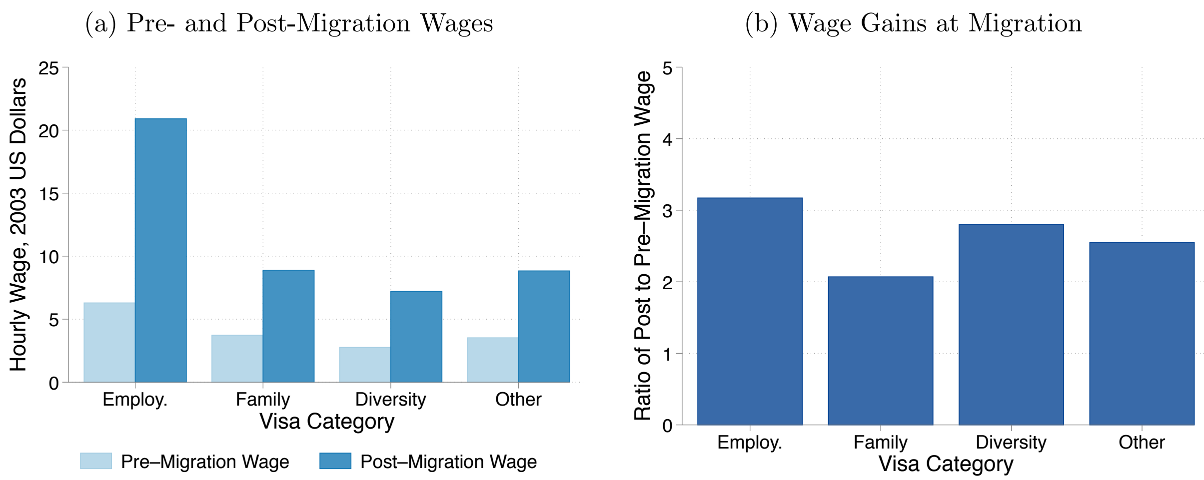
We pool all immigrants with GDP per worker less than one-fourth the U.S. level. We then break out the results by visa category. Figure 3 gives the raw data on wages and wage gains. Immigrants on employment visas are clearly selected on pre- and post-migration wages, while the other groups are fairly similar. There is even less variation in terms of wage gains, which range from a factor of two to a little more than a factor of three. Returning to Table 3 Panel C, we can see that the implied accounting shares are in line with the previous results.

Table 3: Human Capital Share in Development Accounting by Subgroups

Robustness Check	Human Capital Share	95% Confidence Interval	N
Panel A: Baseline			
Baseline	0.61	(0.57, 0.65)	840
Panel B: Decomposition by Country			
India	0.63	(0.57, 0.69)	146
China	0.76	(0.64,0.89)	56
Philippines	0.49	(0.41,0,57)	106
Panel C: Decomposition by Visa Status			
Employment visa	0.53	(0.46, 0.60)	180
Family visa	0.66	(0.55, 0.77)	134
Diversity visa	0.58	(0.49, 0.68)	176
Other visa	0.58	(0.46, 0.69)	113

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 gives 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.

Figure 3: Wages and Visa Status



4.4 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

Table 4: Robustness: Human Capital Share in Development Accounting

Robustness Check	Human Capital Share	95% Confidence Interval	N
Panel A: Baseline			
Baseline	0.61	(0.57, 0.65)	840
Panel B: Robustness to Migration Details			
Sampled interviewees only	0.61	(0.56, 0.66)	585
No secondary migration	0.65	(0.61, 0.70)	740
Recent arrivals	0.54	(0.49, 0.59)	514
Simple migration cases	0.62	(0.57, 0.68)	636
Panel C: Robustness to Wage Construction and Job Type			
First job in US	0.63	(0.58, 0.68)	656
Wage workers	0.58	(0.53, 0.62)	739
Trim outliers	0.58	(0.54, 0.62)	792
Total compensation adjustment	0.51	(0.47, 0.56)	840
Panel D: Robustness to Currency Conversion Complications			
Currency-country match	0.61	(0.57, 0.65)	802
No revaluations ever	0.63	(0.58, 0.67)	628
No high inflation	0.61	(0.56, 0.65)	828
No high inflation ever	0.66	(0.61, 0.71)	512

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 gives 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.

of interest. We focus throughout on immigrants from countries with GDP per worker less than one-fourth 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 4.

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 directly interviewed (excluding spouses); only immigrants who migrated directly from their birth country to the U.S.; or only immigrants who arrived to the U.S. recently, meaning during or after 1998. The final 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 U.S., and that they worked their last job in their birth country within one year of their first job in the U.S. All show results 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 result of using first job in the U.S. instead of the most recent as in the baseline; the second reports the results using only workers who worked for wages before and after migrating. 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 U.S. 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 NIPA. This lowers the results, but they still 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. 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.

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 0.51–0.66, 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.

5 Selection

In the previous section we measured 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 most common concerns about immigrant selection because it compares wages earned by a given worker in two different countries. Nonetheless, it is of interest to back out the implied degree of selection, which we measure here as the gap between immigrants' pre-migration characteristics and the characteristics of non-migrants in the same country. Although selection of immigrants is of interest in its own right, our primary motivation is to understand why our results for the human capital share in development accounting are so much larger than those in the literature: 61 percent versus 30 percent in Hendricks (2002) or 42 percent in Schoellman (2012). We argue that the main difference lies in the treatment of selection: because most previous work lacked data on pre-migration wages, they relied on assumptions that we test and reject.

5.1 Selection and Wages

Most of the existing literature on immigrants and human capital only has data on the post-migration wages of immigrants to a single country, usually the United States. The typical thought experiment is then to compare the wages of workers from poor versus rich countries within the United States. Intuitively, if immigrants from poor and rich countries earn similar wages in the U.S., then the inference is that average human capital varies little across countries. One way to formalize this measurement is to compute:

$$\frac{\log(w_{i,b,US}) - \log(w_{i',b',US})}{\log(y_b) - \log(y_{b'})} = \frac{\log(h_i) - \log(h_{i'})}{\log(y_b) - \log(y_{b'})}$$
$$\equiv \frac{\log(\overline{h_b}) - \log(\overline{h_{b'}}) + \log(\sigma_i) - \log(\sigma_{i'})}{\log(y_b) - \log(y_{b'})}$$

Here we use the notation $w_{i,b,US}$ to denote the wage of worker i who was born in b and now works in the U.S., $\overline{h_b}$ to denote the average human capital in b, and $\sigma_i = h_i/\overline{h_b}$ to denote the selectivity of immigrant i relative to the country b average. The object of interest is the variation of average human capital with respect to GDP per worker. This can be measured using immigrant earnings under one of two assumptions: if immigrants are unselected ($\log(\sigma_i) = 0$); or if immigrants from countries at different development levels are equally selected ($\log(\sigma_i)$ independent of $\log(y)$).

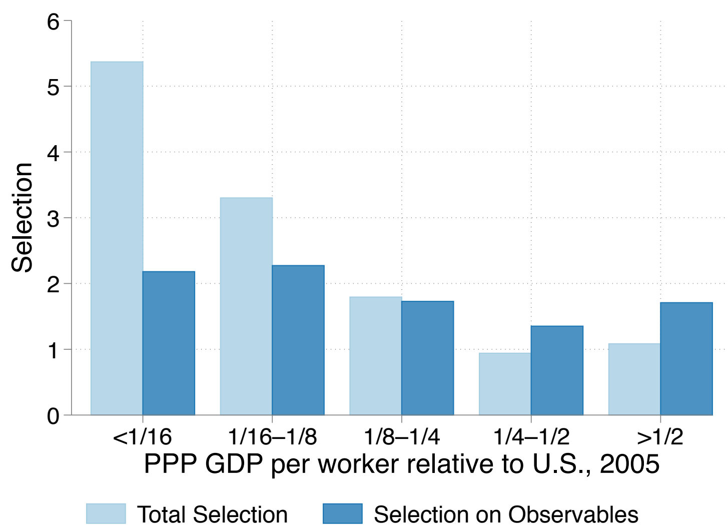
We can use our data on pre-migration wages of immigrants to test this assumption directly. In principle, one would like to compare the pre-migration hourly wage of immigrant i to the mean wage of non-migrants in the same country, $w_{i,b}/\overline{w_b}$. Unfortunately, we lack widespread data on pre-migration wages for many countries; given the high rates of self-employment in many poor countries, it is not clear whether such a database would even valuable. This leads us to substitute $\overline{w_b} = (1 - \alpha_b)y_b/n_b$, where n is the hours worked per worker per year. Gollin (2002) documents that α_b does not vary systematically with average income, while Bick et al. (2015) documents that hours worked per employed person do not differ much between the U.S. and poor countries. If we assume that these two factors are roughly constant, we arrive at a simple measure of selection for an individual:

$$\sigma_i = \frac{w_{i,b}/y_b}{w_{US}/y_{US}}. \tag{5}$$

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

We construct this measure of selection for all individuals in our sample. We then average it by PPP GDP per worker category and plot the result as "total selection" in Figure 4. There are two main takeaways. First, immigrants are substantially selected on pre-migration earnings, 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 more nearly a factor of six, whereas immigrants from the richest countries are hardly selected at all by this measure.

Figure 4: Selection of Immigrants by GDP per worker



Although previous studies lacked data on the pre-migration earnings of immigrants, they did have data on some other observable dimensions of selection. For example, it is well-known that immigrants are selected on education, particularly those from poorer countries. Thus, one approach to the selection problem is to construct and control for an index of selection on observable attributes to help mitigate the selection problem. Doing so allows one to rely on the weaker assumption that there is no selection on unobservable attributes or that selection on unobservable attributes is uncorrelated with development. We now ask whether controlling for differences in observable characteristics is sufficient to undo the selection gradient observed in Figure 4.

To do so, we construct a measure of selection following in the spirit of Hendricks (2002). There are two steps. First, we need to identify the set of attributes that we can measure among both the immigrant and non-migrant populations. Education and age are commonly used because there are widely available, comparable data on each for immigrants and non-migrants. Second, we need to value these characteristics. We follow the literature in using U.S. wages to do so. In particular, we take the 2003–04 ACS (matching roughly the time frame of the NIS) and construct a sample of employed natives with data on wage, age, and education. We then regress log hourly wages on years of schooling and dummies for five-year age bins (e.g., age 15–19, 20–24, ... 65+). We use these regression coefficients to value the observed attributes of immigrants in the NIS sample and the observed attributes of non-migrants from the same country. For the latter we use data on average years of schooling and the age structure of the population for the year 2000 from Barro and Lee (2013). We call the gap between the value of the immigrant's observed attributes and the observed attributes of the non-migrant population an index of selection on observables.

The results of this exercise, averaged by PPP GDP per worker group, are given in Figure 4. We can see that this measure does capture a fair amount of selection, a little less than a factor of two on average. However, it is much less variable across GDP groups than is our measure of total selection; whereas total selection varies between a factor of 1 and 5, selection on observables varies between only a factor of 1.5 and 2. This fact implies that controlling for differences in observed attributes reduces but does not entirely eliminate the effect of selection.

This finding has strong implications for previous work that compared immigrant earnings across countries. The key finding in that literature was that there were small earnings gaps between immigrants from poor and rich countries in the U.S. If immigrants from all countries were equally selected, then this would imply small gaps in average human capital between poor and rich countries. However, we find that immigrants from poor countries are systematically much more selected in a way that is not well-captured by observable characteristics. Hence, the small gap in immigrant earnings is in part explained by stronger selection of poor country immigrants; once this fact is taken into account, the implied gap in average human capital between countries is much larger.

5.2 Selection on Other Characteristics

Selection plays a central role in our development accounting results. We find small wage gains at migration in part because we find large pre-migration wages, which suggests strong selection for immigrants from poor countries. Given this, we also examine the other pre-migration attributes of immigrants from the poorest countries to see if they are consistent with the large degree of selection suggested by Figure 4. We find that they are. For example, immigrants from the poorest group have on average 13.2 years of schooling. 37 percent have a college degree while only 15 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.

We also study the characteristics of workers' pre-migration jobs. We again find that they are consistent with strong selection. First, 79 percent of immigrants from the poorest countries were employed for wages in their pre-migration job. This fact stands at odds with the general prevalence of self-employment in poor countries. Second, we study occupation as reported in 25 broad groupings. Of these 25, the four most commonly reported are office and administrative support; sales and related; management; and education, training, and literacy. They account for more than half of all the pre-migration occupations. On the other hand, not a single immigrant in the poorest group reports having previously worked in agriculture, despite the fact that this occupation accounts for the majority of employment in most poor countries (Restuccia et al., 2008). We conclude that there is ample evidence that immigrants from the poorest countries are extremely selected on their pre-migration labor market experiences.

6 Skill Transferability

Our baseline estimates accounted for selection by comparing the pre- and post-migration wages for 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 well to the U.S. This could happen either if skills are heterogeneous and they have acquired skills that are not highly valued in the U.S., or if skills are homogeneous but 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 comparing

Table 5: Occupational Changes at Migration

GDP category	Same Detailed Occ.	Same Broad Occ.	Mean Wage Change
<1/16	6%	12%	-14%
1/16–1/8	18%	33%	-13%
1/8–1/4	8%	19%	-14%
1/4–1/2	9%	22%	-15%
>1/2	30%	47%	-2%

Table note: Each column shows the fraction of immigrants who keep the same detailed occupation pre- and post-migration; the same broad occupation; and the average value of the pre- to post-migration occupation change, based on mean native wage. The rows give those results for different PPP GDP per worker groups.

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 difficulty transferring skills. We then consider the importance of this finding for our development accounting results. In Appendix C.2 we provide a simple model to formalize the following intuition: if immigrants have skills but cannot use them in the U.S., then this depresses their post-migration wage and our estimated wage gains at migration. It then follows that we understate the role of country and overstate the role of human capital in development accounting. We show that conservative corrections for skill transfer push our estimate of the human capital share down towards 0.50.

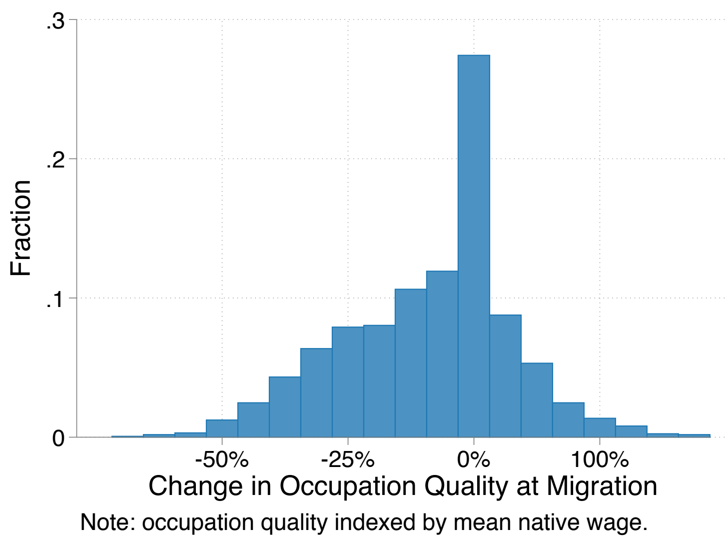
6.1 Evidence on Skill Transferability

We measure skill transfer by comparing immigrants' pre- and post-migration occupations. 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, e.g., that 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 includes over 450 possible occupational choices. With these two caveats in mind, we now turn to analyzing occupational switches.[11]

[11] We have also explored repeating all this analysis using industry data and find similar results throughout.

We begin by examining the frequency of occupational switches. Occupations are reported at both the detailed level (roughly 450 categories) and in broad groupings (25 categories). For example, there are 28 different detailed codes for managers, including human resource managers, financial managers, and farm managers, all grouped under the broad category of management occupations. We look at switches for both coding schemes to study the frequency of any type of switch and the frequency of large switches to totally new types of occupations. The results are given in Table 5 for each of the five GDP per worker categories. We find that maintaining the same detailed occupation is uncommon, with only 6–30% of immigrants reporting this. Occupational persistence is only modestly higher if we use broad categories, ranging from 12–47% of immigrants reporting the same occupation. There is perhaps some evidence of a trend: immigrants from the poorest countries are least likely to report maintaining the same occupation and immigrants from the richest countries most likely. On the other hand, the groups in between are mixed.

Figure 5: Evaluating Occupational Changes at Migration



A change in occupation does not indicate whether the new occupation is better, worse, or much worse than the old occupation. As a proxy for the "quality" of an occupation, we construct for each detailed occupation the mean wage of natives aged 16–65 employed in that occupation using the 2003–04 ACS.[12] 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

[12]Data from Ruggles et al. (2010).

hence a measure of the extent to which an immigrant's new job is better or worse than their 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 U.S. Based on the observation that the mean wage of taxi drivers in the U.S. was \$9.52 while the mean wage of physicians was \$38.70, we would infer that the immigrant's occupational switch involved a downgrade. The extent of the change in mean wages (75 percent) provides a metric to suggest that the occupational downgrading was significant.

Figure 5 displays the histogram of the change in job quality at migration. The bar at zero shows that more than one-quarter of immigrants had no change or a small change in job quality at migration. However, the distribution is heavily skewed; remaining at the same job and experiencing no wage change puts an immigrant at the 75th percentile of the distribution. Few immigrants move to higher-paying jobs, while the majority of immigrants move to lower-paying jobs. This evidence suggests that occupational downgrading at immigration is the typical experience for immigrants.

We aggregate these results on the extent of occupational downgrading by GDP per worker category in the last column of Table 5. The key take-away is that immigrants from all but the richest countries experience quantitatively important occupational downgrading, with the average immigrant moving to a job that is 13–15 percent worse at migration, as judged by mean native wages. Only for immigrants from the richest group of countries is the loss quantitatively small. One interpretation of this finding is that most immigrants have a hard time transferring their skills to the U.S.

6.2 Development Accounting with Imperfect Skill Transfer

If we interpret these findings as evidence of skill non-transferability, then these findings have important implications for our development accounting results. We explore this idea further in two ways. First, we check the robustness of our results to focusing on groups for whom skill transfer is likely less of a problem. There are two main groups in the NIS: immigrants who entered the U.S. on employment visas; and those who work the same detailed occupation before and after migrating. The implied development accounting results for these subsamples are shown along with the baseline in Table 6. While human capital accounts for 61 percent of cross-country income differences in the baseline, it accounts for a modestly lower 53–56 percent when focusing in these subsamples.

As a second check, we consider replacing the post-migration wages of "downgraded" im-

Table 6: Development Accounting and Skill Transfer

Robustness Check	Human Capital Share	95% C.I.
Baseline	0.61	(0.55,0.71)
Employment visa	0.53	(0.46,0.60)
Same narrow occupation	0.56	(0.48,0.64)
Skill transfer: mean wage	0.44	(0.40,0.48)

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 gives the result from constructing these statistics for a different sample or using different measures of post-migration wages.

migrants with higher counterfactual wages. 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. By raising 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. Our preferred counterfactual post-migration wage is the mean wage of natives who work in the immigrant's *pre-migration* occupation; we have in mind that in the absence of barriers, immigrants could have moved to the U.S., continued practicing their pre-migration occupation, and earned roughly the same wage as natives in that occupation. Using the same wage as natives is supported by the fact that the typical immigrant who does practice the same occupation before and after immigrating earns a post-migration wage just slightly lower than that of natives in the occupation. The resulting adjustment is substantial, increasing the mean post-migration wage of immigrants by 50 percent. We then compute the implied development accounting results for this sample with counterfactual post-migration wages and report these results in the last line of Table 6. We find that human capital in this case would account for as little as 44 percent of cross-country income differences, still much larger than in the previous literature.

There are two main take-aways from this section. First, most immigrants switch to lower-paying occupations when the immigrate to the U.S. If this fact is interpreted as the result of skill non-transferability, then our baseline results overstate the importance of human capital for development accounting. We conduct several checks that suggest that correcting for this could lower the human capital share to 44–56 percent, 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 61 percent is appropriate.

7 Elasticity of Substitution Across Skill Types

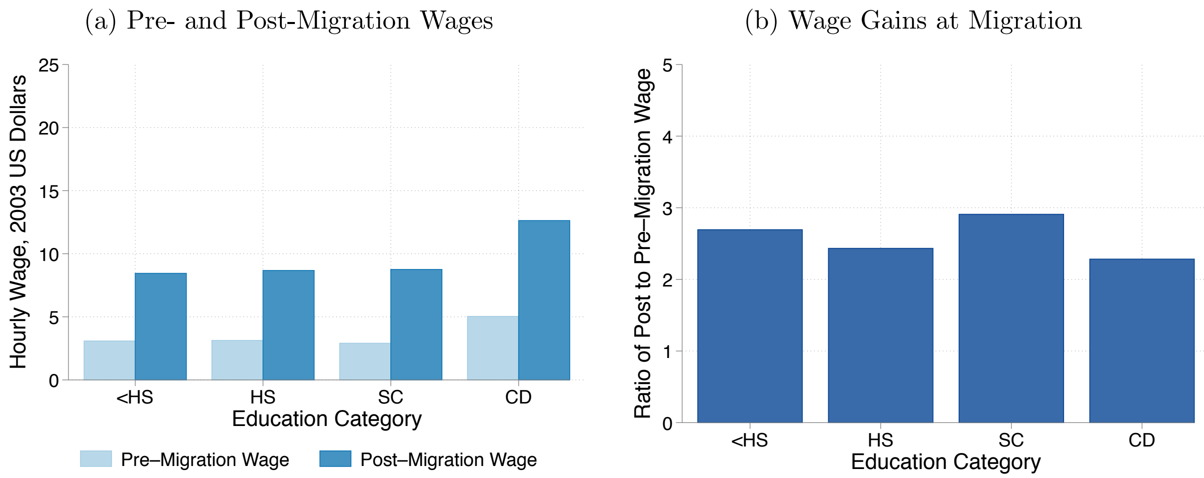
Our estimates so far have all followed the precedent of the accounting literature by assuming that workers of different skill levels are perfect substitutes in the aggregate production function. Some recent work has noted that this assumption is important for a number of development questions (Roys and Seshadri, 2014; Caselli and Coleman, 2006). The most directly related work is Jones (2014), who notes that development accounting results are very sensitive to it. Even modest reductions of the elasticity of substitution (from infinity) can substantially increase the role for human capital in accounting for cross-country income differences. At the same time, there is relatively little evidence on the long-run or cross-country elasticity of substitution. The best-known estimate spans the U.S. from 1950–1990, but there is no guarantee that a similar estimate applies to the much poorer countries in our sample (Ciccone and Peri, 2005).

Our insight is that the wage gains of immigrants to the U.S. can be informative about this parameter. We formalize this idea in Appendix C.3, but the intuition is as follows. In a model with imperfect substitution, the wage gains of immigrants depend on country-specific factors such as the capital-output ratio and TFP, but also on the difference in the relative supply of skilled and unskilled labor between the immigrant's birth country and the U.S. Educated immigrants from poor countries should gain less than uneducated immigrants because while educated immigrants move to a country where educated labor is relatively more common, uneducated immigrants move to a country where uneducated labor is relatively less common. Hence we can use the relative wage gains of immigrants with different education levels as evidence on the elasticity of substitution between education groups.

To implement this idea, we focus again on immigrants from countries with PPP GDP per worker less than one-quarter the U.S. level. We measure education by combining data on degree attainment and years of schooling, giving preference to the former where available. We then break workers into four groups: those with less than a high school degree (or less than twelve years of schooling); those with exactly a high school degree (or twelve years of schooling); those with some college but not a bachelor's degree (or 13–15 years of schooling); and those with a bachelor's degree or more (or 16 or more years of schooling). We have too few immigrants with less than a high school degree to further subdivide this group, although doing so would be of interest when thinking about poor countries.

Figure 6 shows the pre-migration wage, post-migration wage, and wage gain at migration by education group. We find little variation in pre- or post-migration wages among the first three groups, whereas college graduates earn more both before and after migration. In terms of wage gains, however, we find very similar results for each of the groups of immigrants.

Figure 6: Wages and Education Level



In principle this figure could be biased by composition effects: perhaps college graduates come from richer countries. To control for this, we compute the implied human capital share in development accounting for each education category. Recall that this statistic is simply (one minus) the log wage change at migration divided by the log GDP per worker gap. Hence, it effectively controls for the size of the gap in GDP per worker. The results are given in Table 7. We find no strong support for imperfect substitution: the proportional wage gains are roughly the same for all workers with at least a high school degree, and are lower for high school dropouts, whereas a theory with imperfect substitution would predict that it is higher. Indeed, it is apparent from our confidence intervals that we cannot reject that the wage change is the same across groups, implying that we cannot reject the case of perfect substitutes.[13]

[13]The framework Caselli and Coleman (2006) offers an alternative interpretation of these facts. There, educated and uneducated workers are imperfect substitutes, but countries operate technologies with different weights on educated and uneducated labor. In this case immigrants would be moving between countries with different relative supplies of and demand for educated labor; the lack of correlation between wage gains and education could simply reflect that those two forces roughly offset.

Table 7: Robustness: Human Capital Share in Development Accounting by Education

Robustness Check	Human Capital Share	95% Confidence Interval	N
Baseline	0.61	(0.57,0.65)	840
Less than High School Graduate	0.53	(0.42,0.65)	128
High School Graduate	0.61	(0.50,0.71)	165
Some College, No Degree	0.56	(0.41,0.71)	79
College Degree or More	0.65	(0.60,0.70)	468

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 gives the result from constructing these statistics for the baseline sample or for subsamples with the different levels of education.

8 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. We infer that human capital accounts for roughly 60 percent of cross-country income differences. We conduct a range of robustness checks and find this figure to be robust. Our result is much larger than those in the previous literature because it provides a direct way to measure and control for selection, which we find to be large and strongly correlated with development.

We also provide novel evidence on two issues frequently raised in the literature. First, we find that immigrants' experiences are consistent with the assumption of perfect substitution across labor types. The key finding here is that immigrants with different education levels have similar wage gains at migration, which is inconsistent with imperfect substitution. 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 skill loss at migration lower the human capital share in development accounting to perhaps 50 percent.

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A Sample Details

Table A1 shows the initial sample size for workers who have data on pre-migration hourly wages, post-migration hourly wages, and both (necessary for computing the wage gains at migration) in row 1. The subsequent rows show the effects on sample size of the various restrictions and adjustments we make. In order: we need to be able to compute hourly wage; we need to be able to identify the immigrant's birth country; we need to able to adjust the wage to PPP-adjusted U.S. dollars; we need to able to measure the PPP-adjusted GDP per worker in their birth country; we exclude immigrants who report wages with subsequent devaluations; we trim outliers in the wage distribution; we focus on immigrants who arrive during or after 1983; and we exclude anyone who reports having had some U.S. education.

Table A1: Sample Size by Adjustment

	Pre-Migration Wages	Post-Migration Wages	Both
Hourly Wage	4,721	5,710	2,328
Valid Country	4,615	5,612	2,284
Adjusted Wage	3,406	5,612	1,797
Matched GDP	3,404	5,602	1,796
No Devaluation	3,093	5,224	1,631
Trim Wage Outliers	2,981	5,164	1,576
Arrived After 1983	2,715	4,629	1,469
No US Schooling	2,418	3,368	1,292

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 two most important restrictions in terms of lost sample size are being able to adjust wages and excluding immigrants with U.S. schooling. We lose nearly 500 immigarnts in the matched sample because we cannot attach the appropriate exchange rate or purchasing power parity adjustment; we lose nearly 200 immigrants who immigrated who acquired some U.S. education after migrating.

B Comparison of New Immigrant Survey and American Community Survey Samples

The New Immigrant Survey is a sample of new recipients of lawful permanent residency in the U.S. in 2003. One natural question is how this sample frame compares to a broader sample of immigrants that one would observe in a standard representative cross section of the U.S. population, which will include a broader set of unauthorized immigrants, those not yet granted lawful permanent residency, and those who have been lawful permanent residents for some time. Here we draw on information from the 2003–04 American Community Survey (ACS) in order to compare the two. We use the ACS because it offers a large sample size (roughly 0.4 percent of the U.S. population in each year), detailed information on country of birth and year of immigration, and is available for the appropriate years.[14]

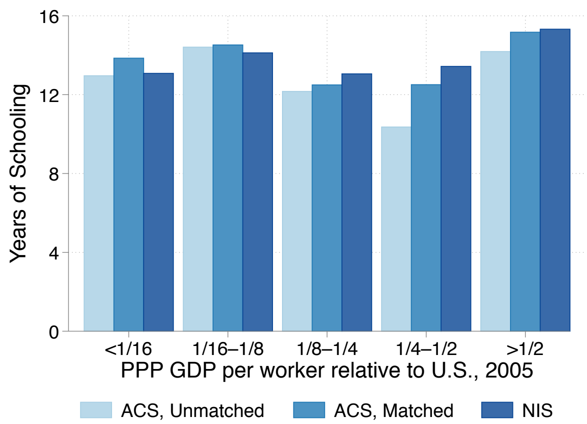
We conduct two comparison exercises. First, we construct mean age, years of schooling, and log-wage by PPP GDP per worker category in both the NIS and the ACS. These results are displayed in Figure B1 as NIS and ACS, Unmatched. The two data sources agree closely on the average years of schooling and age of immigrants from poor and rich countries. However, hourly wages are much lower in the NIS, particularly for immigrants from poorer countries; the NIS suggests that post-migration hourly wages are roughly one-half of what the ACS suggests.

To some extent, these differences reflect composition effects. Even within a PPP GDP per worker category the source of immigrants varies over time. Further, the NIS necessarily features more recent immigrants because roughly half of the NIS sample is newly arrived immigrants. To investigate the importance of composition effects, we perform a second comparison that controls for composition effects. We construct the mean age, years of schooling, and log-wage by birth country-year of immigration cell within the ACS. We then match each NIS immigrant to the appropriate cell mean from the ACS and average the resulting figures up to the GDP per worker category. These results are shown in Figure B1 as ACS, Matched. Controlling for composition (country of birth and year of immigration) produces estimates that are much closer to the NIS estimates. For the case of hourly wages, the gap is cut roughly in half. Nonetheless, we still see that NIS immigrants earn less than comparable immigrants in the ACS.

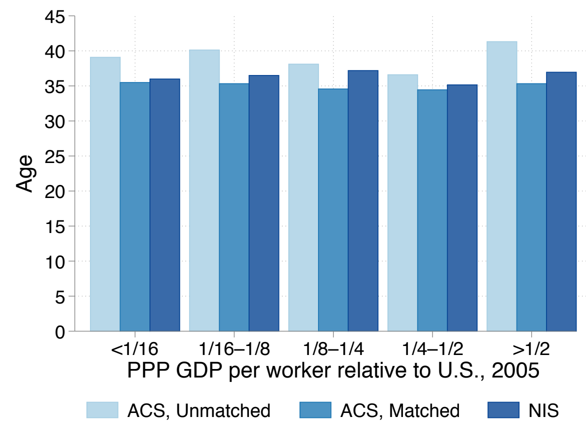
[14] Data downloaded from IPUMS Ruggles et al. (2010).

Figure B1: Comparison of NIS and Census Samples

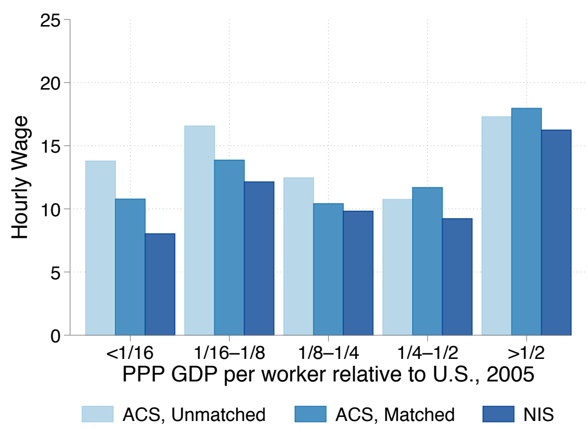
(a) Average Years of Schooling



(b) Average Age



(c) Average Hourly Wage



C Model Extensions

Here we formalize several of the extensions and complications to the basic model of immigrant wages and development accounting in Section 2.

C.1 Heterogeneity in Gains to Migration

In this appendix we study an alternative model of selection. In the baseline model of Section 2 we focus on selection on human capital. Each country is home to workers with heterogeneous levels of human capital; selection refers to the idea that the distribution of immigrants' human capital may differ from that of the overall population. Here, we consider a model of selection on the gains to migration. First, we need to introduce heterogeneity in the gains to migration. We assume that that the gains to migration depend on $\log(z_{US}) - \log(z_b)$, as in the baseline case, but that they also include some idiosyncratic component ε_i that is drawn from an unspecified distribution G.

A natural conjecture is that migrants are positively selected on ε_i. This would be the case if immigrants were choosing whether or not to immigrate subject to some cost as in Borjas (1987), creating a cutoff rule; or if American immigration officials were selecting which migrants to permit to enter the country. McKenzie et al. (2010) provide evidence that this is the case for migrants from Tonga to New Zealand. Under this case the gains to migration are given by:

$$\frac{\log(w_{i,US}) - \log(w_{i,b})}{\log(y_{US}) - \log(y_b)} = \frac{\log(z_{US}) - \log(z_b) + \varepsilon_i}{\log(y_{US}) - \log(y_b)} > \frac{\log(z_{US}) - \log(z_b)}{\log(y_{US}) - \log(y_b)} = \text{share}_{\text{country}}$$

The gains to migration relative to the gap in GDP per worker actually overstates the importance of country, implying that we are understating the importance of human capital. Hence, our calculations are conservative if immigrants are positively selected on gains to migration. A second implication of this framework is that the gains at migration are probably a better reflection of the share of country for cases with large gaps in country environment. In cases where the gap in z and y is small, the bias induced by selection on gains at migration is larger and inferences are less reliable. This point provides another motivation for focusing on immigrants from poorer countries.

C.2 Skill Transfer

Here we formalize a simple model of skill transfer. Suppose that immigrants with human capital level h_i can apply all of their human capital while working in their birth country b. However, when they move to the U.S. only a fraction $\phi \leq 1$ of their skills transfer. $\phi < 1$ could represent implicit skill heterogeneity, such that the type of skills acquired in b are not valued in the US; or it could represent barriers or discrimination that prevent the immigrant from using valued skills. The implied pre- and post-migration wages are then given by:

$$\log(w_{i,b}) = \log[(1-\alpha)z_b] + \log(h_i)$$
$$\log(w_{i,US}) = \log[(1-\alpha)z_{US}] + \log(h_i) + \log(\phi).$$

The wage gains at migration are given by:

$$\frac{\log(w_{i,US}) - \log(w_{i,b})}{\log(y_{US}) - \log(y_b)} = \frac{\log(z_{US}) - \log(z_b) + \log(\phi)}{\log(y_{US}) - \log(y_b)} < \frac{\log(z_{US}) - \log(z_b)}{\log(y_{US}) - \log(y_b)} = \text{share}_{\text{country}}.$$

If skills do not transfer upon migration then the wage gains at migration understate the share of country in development accounting, which in turn implies that we would overstate the share of human capital. Given that our results for human capital are larger than those in the literature, this is a point that we pursue at length in Section 6.

C.3 Imperfect Substitution Across Education Types

Immigrants present a natural laboratory to investigate the elasticity of substitution. To see why, it is helpful to extend the standard development accounting setup to allow for two types of labor, skilled and unskilled. In this case the production function is:

$$Y_c = K_c^{\alpha}\left[A_c\left(\theta_u H_{u,c}^{\frac{\sigma-1}{\sigma}} + \theta_s H_{s,c}^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}}\right]^{1-\alpha}$$

where $\theta_u + \theta_s = 1$. We continue to assume that there is heterogeneity and perfect substitution of human capital within each skill type. For example, unskilled workers could be anyone with less than a high school degree, which encompasses many different education levels and abilities.

We continue to maintain the assumption that labor markets are competitive and workers are paid their marginal product. In this case, the wage of worker i who provides skilled labor is given by:

$$\log(w_{i,s,c}) = \log\left[(1-\alpha)z_c\right] + \frac{1}{\sigma-1}\log\left[\theta_u\left(\frac{H_{u,c}}{H_{s,c}}\right)^{\frac{\sigma-1}{\sigma}} + \theta_s\right] + \log(h_i) \tag{6}$$

In this case, the marginal product depends on three terms. The first and third terms are the same as in the perfect substitutes case and capture the common effects of country z_c and the worker's human capital h_i. The second term is new and captures the relative supply of unskilled and skilled labor in country c.

Our approach is to construct a simple double-difference: we compare the wage gains at migration for skilled versus workers. Following the above, this is given by:

$$[\log(w_{i,s,US}) - \log(w_{i,s,b})] - [\log(w_{i,u,US}) - \log(w_{i,u,b})] = -\frac{1}{\sigma}\log\left(\frac{H_{s,US}}{H_{u,US}}\frac{H_{u,b}}{H_{s,b}}\right) \qquad (7)$$

By taking wage gains at migration we eliminate the effect of the worker's human capital at migration, h_i. By taking the second difference (between wage gains of skilled and unskilled workers) we eliminate country effects that are common to all workers such as z_c. Then we are left with relative supply effects, captured here as the relative supply of skilled labor in the U.S. as compared to the birth country b. When comparing the U.S. to poor countries there is a large gap in the relative supply of skilled labor, so a low value of the elasticity of substitution implies that the relative gains at migration should vary widely by education level.