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, which implies that "country" accounts for 40 percent of income differences, while human capital accounts for 60 percent. Our approach handles selection by comparing the wage of the same individual in two different countries. We also provide evidence on and a correction for skill transfer.

JEL Classification: O11, J31

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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 (U.S.). 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. 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]

[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. (forthcoming).

[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 (NIS), a sample of adult immigrants granted lawful permanent residence in the U.S. in 2003 (colloquially, green card recipients) (Jasso et al., n.d.). The unique advantage of this dataset is that it asked immigrants detailed questions about both their pre- and post-migration labor market experiences.[4] We use this 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. 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.[5] 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 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.

[4] We are not the first to use the pre-migration labor market information in the NIS. 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.

[5] A related literature have used models of the wage gain at migration to quantify the welfare gains from freer migration across countries (Klein and Ventura, 2009; Kennan, 2013).

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 40 percent of the total gap in GDP per worker, implying that 40 percent of cross-country income differences are accounted for by physical capital and TFP, with the remaining 60 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 immigrants' skills. To investigate this issue, we compare the pre-migration and post-migration occupations of immigrants. We find most immigrants switch occupations upon migration. 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 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.

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 allowing for 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. We conclude that a model with perfect substitution across education types fits our data well, although we have relatively few very uneducated immigrants.

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. 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':

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

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, 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 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 c and $U.S.$, we find a direct measure of the importance of countries:

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

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, because it uses wage observations from the same worker in two countries. 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 D.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.[6] 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

[6] Similar logic shows that a binding minimum wage would also imply that we are calculating a lower bound on the share of human capital in development accounting. However, less than five percent of our sample is paid at or below the minimum wage.

across skill groups that is common in most of the literature, but we revisit this point in Section 7. We now turn to the data.

3 New Immigrant Survey

The New Immigrant Survey (NIS) is a nationally representative sample of adult immigrants granted lawful permanent residence (colloquially, green card recipients) between May and November of 2003, drawn from government administrative records (Jasso et al., 2005, n.d.). Our sample is roughly equally split between newly-arrived immigrants granted lawful permanent residency from abroad and immigrants who adjusted to lawful permanent residency after previously entering the U.S. through other means. The focus on legal permanent residents leads to some differences between NIS respondents and those in other samples. Most notably, there are few Mexicans in this sample, as compared to the Census. More generally, immigrants in the NIS are a little younger, better educated, and lower paid than in the Census or the American Community Survey. Nonetheless, the key stylized facts of the literature obtain in the NIS sample as well. See Appendix C for details.

The NIS 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 U.S. Third, it surveys them about their labor market experiences in the U.S. It contains information on their current job at the time of the survey and their first post-migration job (if different). Fourth, it surveys them about their 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). Throughout, we focus on the most recent pre-migration job. For all jobs we know standard information such as occupation, industry, earnings, and hours and weeks worked.

Given our focus on 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, 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 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 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-euro currencies or euros.

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. Second, we translate the currency to U.S. dollars by using the market exchange rate between the reported currency and the U.S. dollar prevailing at the time, taken from the Penn World Tables.[7] Third, we adjust wages for the purchasing power parity prevailing in the country at the time, again taken from the Penn World Tables.[8] 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.

There are two potential complications to these adjustments that we discuss here and explore further in our robustness section. First, some immigrants report being paid in currencies that have experienced large changes in value or revaluations. Second, some immigrants report unusual currency-country pairs, for example being paid in lira in Brazil. In each case, we are concerned about measurement error: that immigrants may be misremembering the currency or the year in which they were paid, and that doing so could substantially alter the implied wage. Following the advice of the NIS manuals, we exclude all immigrants who were paid in currencies with subsequent revaluations. We also flag all immigrants who report being paid in currencies that ever had a devaluation or experienced high inflation but not a devaluation; or immigrants who report unusual currency-country pairs.[9] We explore robustness to excluding these groups.

At this point we have an estimate of pre-migration wages from year t converted into U.S. dollars and adjusted for cost of living, as well as up to two observations on post-migration wages. Conceptually, our goal is simple: we want to compute the wage gain at migration. This calculation is complicated 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). There are three interpre-

[7]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 U.S.SR, Czechoslovakia, Yugoslavia, and Myanmar come from PWT 5.6 (Heston et al., 2012, 2006, 2002, n.d.).

[8]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 ($PPP/XRAT$).

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

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

In the face of this ambiguity we consider a wide range of possibilities. Our baseline results use immigrants' later job, from 2003–04. We convert this into a year t wage by netting off the wage growth of observably similar natives between year t and 2003, where we use age, gender, and education as our observable characteristics.[10] This adjustment corrects for inflation and life-cycle wage growth. Any wage growth in excess of that of observably similar natives (assimilation) is included in our post-migration wages and the wage gains at migration. We do this because it will tend to increase reported post-migration wages and wage gains at migration, which makes our calculations more conservative. We include in the baseline sample anyone whose last pre-migration wage is from the years 1983–2003. Below, we consider robustness to using instead the first rather than current job in the U.S., and to focusing on subsets of immigrants whose last pre-migration wage was from earlier or later years.

After these checks, 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 U.S. 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. 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 1,383 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.

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

[10] Data from the Current Population Survey. See Appendix B for details.

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, Albania
$1/4 - 1/2$	Poland, Mexico, 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.

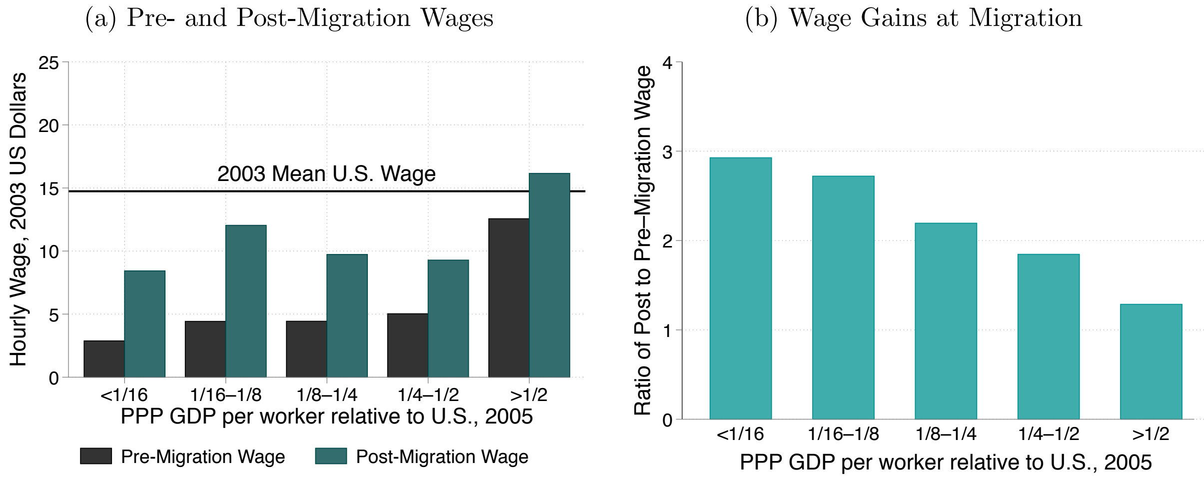
in GDP per worker. Our measure of the latter is the log-difference in GDP per worker between the U.S. and country b in 2005 from PWT 7.1, although all of our results hold if we use year-of-migration gaps in GDP per worker instead. Confidentiality restrictions prevent us from reporting statistics by country 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, which we report in year 2003 U.S. dollars. We compute the mean pre- and post-migration log wage by PPP GDP per worker category. We plot the exponentiated results in Figure 1a, with the exact figures given in Table 2. 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 \$2.88 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 for each individual as the log of the ratio of post-migration to pre-migration wages. We average this figure by PPP GDP per worker category and plot the exponentiated results in Figure 1b, with the exact figures given in Table 2. The average 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 by a factor of 2.9, while immigrants

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



from the richest gain factor of 1.3. The gains for immigrants from poor countries are 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 human capital is one minus the log-wage change at migration relative to the log-GDP per worker gap. We implement this idea by constructing the implied share for every immigrant in our sample. We then compute the mean of the share 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.[11]

Our primary focus is 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 0.55–0.69 with fairly tight confidence intervals. For most of our results we pool these three income groups; when combined, the implied share of human capital in development accounting is 60 percent against a share of country-specific factors

[11]We 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

GDP p.w.		Hourly Wage		Gain	Human Capital Share		N
Category	Mean Gap	Pre-Mig	Post-Mig		Estimate	95% C.I.	
< 1/16	33.4	\$2.88	\$8.43	2.9	0.69	(0.62, 0.76)	181
1/16 – 1/8	12.0	\$4.43	\$12.04	2.7	0.60	(0.55, 0.64)	424
1/8 – 1/4	5.6	\$4.43	\$9.73	2.2	0.55	(0.46, 0,65)	302
1/4 – 1/2	3.0	\$5.03	\$9.28	1.8	0.46	(0.29, 0.64)	175
> 1/2	1.3	\$12.57	\$16.15	1.3	0.83	(-0.06, 1.71)	301

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

of only 40 percent. The 95 percent confidence interval is narrow, ranging from 55 to 64 percent, implying that we can rule out that human capital accounts for as little as even half of cross-country income differences.

These figures also align with the results from a small literature that has investigated the wage gains to migration in select cases. 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. For this case, they find a relatively small wage gain an an implied human capital share of 0.52. Clemens (2013) studies the wage gains for computer programmers who are randomly granted H1B visas in 2007–08. In addition to providing experimental evidence on the gains to migration, his study also offers the advantage that workers do very similar tasks before and after migrating, limiting concern about skill transfer. The implied human capital share in this case is 66 percent. We conclude that existing studies with pre- and post-migration wages for select cases support our general finding of small wage gains. We now turn to decomposition and robustness exercises.

4.2 Decomposition: Select Countries

Six countries in our sample have enough migrants that we can report results separately without violating confidentiality restrictions: Ethiopia, India, Philippines, China, the UK, and Canada. These countries span the PPP 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 difficulty with correctly converting the pre-migration wage to U.S. dollars.

Figure 2: Wages for Select Countries

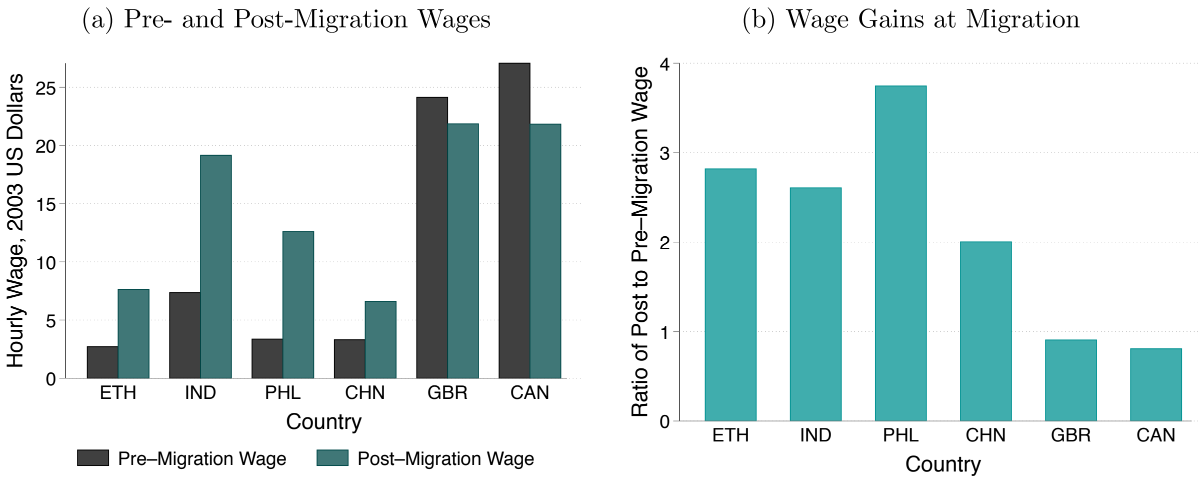


Figure 2 shows the results for wages and wage gains for these countries, ordered by PPP GDP per worker. The wage gains at migration are very similar to those reported above: a factor of 2 to 4 for immigrants from poor countries, with little or no wage gain for immigrants from the two rich countries. There are interesting differences in the patterns of pre- and post-migration wages within the set of poor countries, driven mostly by cross-country heterogeneity in visa class, which we turn to in a moment. First, we construct the implied human capital share in development accounting for each of the four poor countries, shown in Panel B of Table 3. The implied share ranges from 0.47 to 0.77, in line with the baseline result but somewhat more variable.

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. It is worth

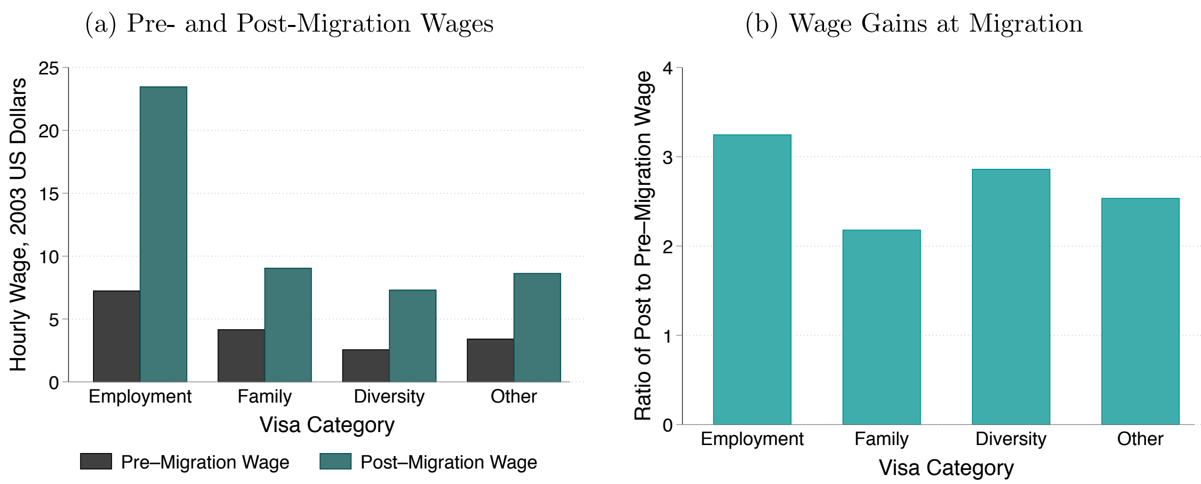
Table 3: Human Capital Share in Development Accounting by Subgroups

Robustness Check	Human Capital Share	95% Confidence Interval	N
Panel A: Baseline			
Baseline	0.60	(0.55, 0.64)	907
Panel B: Decomposition by Country			
Ethiopia	0.77	(0.67, 0.86)	41
India	0.63	(0.58, 0.69)	167
Philippines	0.47	(0.39, 0.55)	111
China	0.70	(0.57, 0.83)	63
Panel C: Decomposition by Visa Status			
Employment visa	0.52	(0.46, 0.59)	196
Family visa	0.64	(0.53, 0.74)	148
Diversity visa	0.58	(0.49, 0.67)	186
Other visa	0.58	(0.47, 0.68)	121

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.

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. 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.

Figure 3: Wages and Visa Status



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.

4.4 Robustness: Assimilation

In this section we show that our results are robust to alternative ways of thinking about assimilation. Recall that our baseline figures focus on the most recent (year 2003–04) job and included any assimilation by immigrants in their wage gains at migration. The results for this case are repeated in Panel A of Table 4. We perform four different robustness checks in Panel B. First, we restrict our attention to immigrants who have been in the

Table 4: Robustness: Human Capital Share in Development Accounting and Assimilation

Robustness Check	Human Capital Share	95% Confidence Interval	N
Panel A: Baseline			
Baseline	0.60	(0.55, 0.64)	907
Panel B: Robustness to Assimilation			
First Job in U.S.	0.62	(0.56, 0.68)	417
1988–1997 arrivals	0.55	(0.45, 0.65)	167
1998–2002 arrivals	0.60	(0.52, 0.69)	219
2003 arrivals	0.60	(0.55, 0.66)	510

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.

U.S. for longer and who have worked at least two jobs. For this subsample, we use the immigrant's first post-migration job to construct wages and wage gains at migration. The results are very similar to the baseline. Second, we focus on subgroups of immigrants who arrived to the U.S. in 2003 (new arrivals), between 1998 and 2002, and 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 4 that our results are very similar across the groups, suggesting that assimilation is not a first-order concern for our estimates of the wage gain at migration.

4.5 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-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 5.

Panel A reports again the baseline results discussed above, for comparison. Panel B reports

Table 5: Robustness: Human Capital Share in Development Accounting

Robustness Check	Human Capital Share	95% Confidence Interval	N
Panel A: Baseline			
Baseline	0.60	(0.55, 0.64)	907
Panel B: Robustness to Migration Details			
Sampled interviewees only	0.59	(0.54, 0.64)	632
Direct migration to U.S.	0.63	(0.59, 0.68)	805
Simple migration cases	0.60	(0.55, 0.64)	743
Speaks and understands English	0.62	(0.57, 0.68)	373
Panel C: Robustness to Wage Construction and Job Type			
Wage workers	0.56	(0.52, 0.61)	797
Trim outliers	0.58	(0.54, 0.62)	854
Total compensation adjustment	0.50	(0.46, 0.54)	907
Only men	0.63	(0.58, 0.68)	579
Panel D: Robustness to Currency Conversion Complications			
Currency-country match	0.59	(0.55, 0.63)	869
No revaluations ever	0.61	(0.56, 0.66)	683
No high inflation	0.60	(0.55, 0.64)	891
No high inflation ever	0.64	(0.60, 0.69)	565

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.

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 including only those whose first and only migration was to the U.S. 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 U.S., and that they worked their last job in their birth country within one year of their first job in the U.S. The last row shows results for immigrants who report both speaking 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 result 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, less than \$5.00 per hour in the U.S., 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 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. 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. 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 0.50–0.64, 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. 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

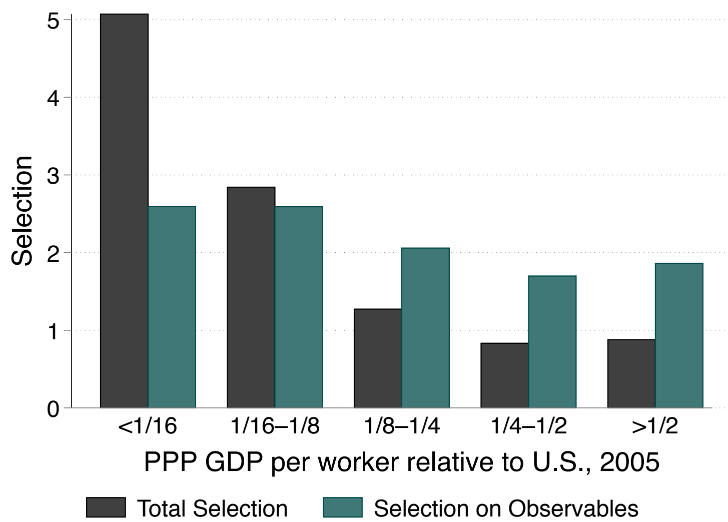
We start by measuring the implied extent of selection on wages. In principle, one would like to compare the pre-migration hourly wage of immigrant i to the mean wage of non-migrants in the same country, $w_{i,c}/\overline{w_c}$. Unfortunately, we lack widespread data on pre-migration wages for many countries; 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 $\overline{w_c} = (1-\alpha_c)y_c/n_c$, where n_c is the 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 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:

$$\sigma_i = \frac{w_{i,c}/y_c}{\overline{w_{U.S.}}/y_{U.S.}}. \tag{5}$$

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 nearly a factor of five, whereas immigrants from the richest countries are nearly unselected by this measure.

Figure 4: Selection of Immigrants by GDP per worker



The degree and pattern of selection is interesting in its own right, but it also helps explain why our results differ so much from the previous literature, particularly Hendricks (2002). That paper constructs each nation's human capital stock using a two-step procedure. The first step measures the human capital stock associated with observable aggregate characteristics such as years of schooling, following the standard development accounting methodology as in Hall and Jones (1999) and Bils and Klenow (2000). The second step measures the remaining components of the human capital stock (sometimes called unobserved human capital or human capital quality) using evidence from immigrants. The main idea is to compare residual post-migration wages of immigrants from countries with different development levels, such as Germany and Ethiopia. Residual wages here means wages purged of the effects of observed factors such as education and experience. Using residual wages has two benefits. First, it measures the implied cross-country variation in unobserved human capital, or human capital that cannot be measured using aggregate data as in Step 1. In other words, the estimates from Step 1 and Step 2 can be added without duplication to arrive at a nation's total human capital stock (Hendricks, 2002). Empirically, Hendricks (2002) found small differences in residual wages between immigrants from poor and rich countries, which led him to conclude that cross-country differences in unobserved human capital were likely to be small.

The second advantage of focusing on residual wages is that they help account for the fact that immigrants are selected on observable proxies for human capital. To understand the underlying assumption on selection, consider two workers i and i' who were born in c and c' and now working in the U.S. The main idea is to compare their residual wages $\tilde{w}_{i,c,U.S.}$ and $\tilde{w}_{i',c',U.S}$ to the gap in GDP per worker:

$$\begin{aligned}\frac{\log(\tilde{w}_{i,c,U.S.})-\log(\tilde{w}_{i',c',U.S.})}{\log(y_c)-\log(y_{c'})} &= \frac{\log(\tilde{h}_i)-\log(\tilde{h}_{i'})}{\log(y_c)-\log(y_{c'})} \\ &\equiv \frac{\log(\tilde{h}_c)-\log(\tilde{h}_{c'})+\log(\tilde{\sigma}_i)-\log(\tilde{\sigma}_{i'})}{\log(y_c)-\log(y_{c'})}\end{aligned}$$

The first equality follows from the same assumptions made in this paper, which allow one to translate differences in wages to differences in human capital stocks. In the second line we define $\tilde{\sigma}_i \equiv \tilde{h}_i/\tilde{h}_c$ as selection on unobservables, which is the ratio of residual (non-observable) human capital of individual i to the residual (non-observable) human capital of the average non-migrant in c. The object of interest is the variation in average residual (non-observable) human capital with respect to GDP per worker. This can be measured using only immigrants' post-migration wages if immigrants from countries at different development levels are equally selected on unobserved characteristics ($\log(\tilde{\sigma}_i)$ independent of $\log(y)$). Given our data, we can test this assumption.

To do so, we construct a measure of residual wages and selection along the lines of Hendricks (2002). The details are in Appendix B, 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 2003–04 ACS, 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.

The results of this exercise, averaged by PPP GDP per worker group, are labeled as selection on observables in Figure 4. This measure does capture a fair amount of selection, around 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 0.8 and 4.6, selection on observables varies between only a factor of 1.7 and 2.6. It follows that there is a strong relationship between selection on unobserved characteristics and GDP per worker. This fact is key to the difference between our paper and Hendricks (2002). Our interpretation is that the small gap in residual wages between poor and rich country immigrants is due in part to stronger selection of poor country immigrants on unobserved characteristics.[12]

5.2 Selection and Wages: A Microdata Approach

Given the preceding discussion, it is clear that the key reason why our results differ from the previous literature is that we are finding substantial selection of immigrants, particularly those from poor countries. A key question is whether this substantial degree of selection is plausible, or whether the results may be driven by, say, measurement error or recall bias. We undertake two additional exercises to speak to these concerns.

In the first, we construct an alternative measure of selection by comparing immigrants' pre-migration outcomes to those of non-migrants using microdata. We focus on India because there are many Indian immigrants in the NIS; because those immigrants are measured to be highly selected; and because we have access to nationally representative microdata with wages, the 1999 Indian Socio-Economic Survey.[13] We limit our attention to immigrants whose last pre-migration job was in India and who were paid in rupees between 1995 and 2003. For this sample, the degree of selection implied by equation (5) is substantial: a factor of 5.25, much larger than the typical poor country (see Figure 4). We adjust the reported pre-migration wage to a year-1999 equivalent by deflating at the rate of nominal GDP per worker growth, taken from the Penn World Tables. After doing so, we can directly measure selection by comparing the mean pre-migration wage of immigrants to the mean wage of non-migrants, which yields a very similar estimate of selection, a factor of 5.97. This fact offers support for the approximations used in equation (5).

Our goal is to think about whether this degree of selection is plausible. To do so, we ask how much of it can be accounted for by the observable characteristics of the worker, as in the previous subsection. Since we have access to Indian microdata, we can account for a broader set of characteristics and use the prevailing returns to those characteristics in India, not the U.S. We focus on four sets of characteristics: sex, education, experience, and occupation. We regress log hourly wages in India on dummies for sex, education (in seven

[12] The quantitative importance of unobserved characteristics is also a difference between our work and that of Clemens et al. (2008), who use estimates of wages by observed characteristics to estimate the gap in marginal products across countries. The key assumption behind this methodology is that the mean unobserved human capital of non-migrants is the same across countries.

[13] For details see Appendix B.2.

categories, as in Barro and Lee (2013)), and occupation, as well as a quartic in potential experience. We then match these coefficients to workers in the NIS. For the most part the mapping is straightforward, but we do have to construct our own crosswalk between India's detailed occupational coding scheme and the NIS coding scheme. After doing so, we can predict for each worker the wage we would expect them to earn in the Indian Census based on their characteristics.

We find that the mean expected wage (based on observed characteristics) is 29 rupees per hour, whereas the actual reported pre-migration wage is 64 rupees. This implies that the factor 5.97 selection can be partitioned into a factor of 2.40 selection on observable characteristics and a factor 2.18 selection on unobservable characteristics. Thus, the majority of selection can be understood especially as selection on occupation and education.[14] We can go even further in understanding selection if we utilize the work of Commander et al. (2008), who surveyed a number of large Indian software firms about their business. Among other information, they note that mean annual earnings paid for software developers by these firms was 223,000 rupees in 1999. Assuming developers work 40–50 hours per week, 50 weeks per year, this corresponds to an hourly wage of 89–112 rupees per hour, which is more than double the overall mean wage for programmers from the Indian Census. This information is useful because a large majority of Indian immigrants in our sample report some variant of computer programming as their pre-migration occupation. It suggests that much of the remaining selection can likely be attributed to the fact that Indian migrants were likely employed in the large, skill-intensive firms with the connections and knowledge to secure visas for their employees.

We acknowledge that the specifics of this finding are unlikely to generalize to immigrants from other countries. Nonetheless, we find it instructive that for one of the most selected countries in our sample, we can plausibly explain the pre-migration wage gap using information on education, occupation, and employer, which in turn suggests that we do not need to appeal to measurement error or recall bias. We now turn to a more general investigation.

5.3 Selection on Other Characteristics

Our second approach to thinking about the plausibility of strong selection for poor country immigrants is to study selection on non-wage dimensions. For example, immigrants from the poorest group have on average 13.0 years of schooling. 32 percent have a college degree

[14] To put the selection on unobservables into context, a factor of 2.18 above the mean puts immigrants at the 91st percentile of the distribution of residual wages.

while only 17 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, 77 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 three most commonly reported are office and administrative support; sales and related; and management. They account for 45 percent 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 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 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 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. In Appendix D.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

Table 6: Occupational Changes at Migration

GDP category	Occupational Switch			Mean Change
	Lower-Paying	Same Occupation	Higher-Paying	
<1/16	68%	7%	25%	-14%
1/16–1/8	60%	18%	22%	-15%
1/8–1/4	65%	8%	28%	-15%
1/4–1/2	65%	9%	26%	-17%
>1/2	47%	30%	23%	0%

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

estimate of the human capital share down towards one-half.

6.1 Evidence on Skill Transferability

We measure skill transfer by comparing immigrants' pre- and post-migration occupations.[15] 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.[16]

We begin by examining the frequency of occupational switches. We focus on the detailed occupational coding scheme, which includes roughly 450 categories. We find that most immigrants switch jobs after migrating. The fraction staying in the same occupation is given in column 3 of Table 6 and ranges from 7–30% depending on the level of development. This

[15] The 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) employs an alternative strategy of comparing immigrants' education to that of natives in the same occupation, using "overeducation" as a proxy for imperfect skill transfer.

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

figure is driven mostly by changes to entirely new occupations; only 12–46% of immigrants work even in the same broad occupational category after migrating (not shown).

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 for that occupation from the 2003–04 American Community Survey (ACS).[17] 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 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. is $9.52 while the mean wage of physicians is $38.71, 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.

The remaining columns of Table 6 give a sense of the distribution and average change in occupation at arrival. Roughly two-thirds of immigrants move to lower-paying jobs after migrating, while only one-quarter move to higher-paying jobs, except for the highest GDP per worker group. The mean change in occupation quality (again, judged by mean native wage) is a loss of 14–17 percent upon migration. Only immigrants from the richest countries report no occupational downgrading at migration. One interpretation of this finding is that most immigrants cannot perfectly transfer their skills to the U.S.

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 two ways, focusing throughout on immigrants from countries with less than one-fourth of US GDP p.w. 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 7. While human capital accounts

[17] See Appendix B for details.

Table 7: Development Accounting and Skill Transfer

Robustness Check	Human Capital Share	95% C.I.
Baseline	0.60	(0.55, 0.64)
Employment visa	0.52	(0.46, 0.59)
Same narrow occupation	0.56	(0.49, 0.63)
Skill transfer: mean wage	0.46	(0.42, 0.50)

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.

for 60 percent of cross-country income differences in the baseline, it accounts for a modestly lower 52–56 percent when focusing in 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. 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 their pre- and post-migration occupations. For example, take an immigrant who reports having been a doctor before arriving to the U.S., 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 \$29.19, resulting in a total wage of \$37.17. The resulting adjustment is substantial, increasing the mean post-migration wage of immigrants by 39 percent. We then re-compute 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 account for just under half of cross-country income differences.

There are two main take-aways from this section. First, most immigrants switch to lower-paying occupations when they immigrate to the U.S. 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 this could lower the human capital share to 46–56 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 60 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 the elasticity of substitution. We formalize this idea in Appendix D.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). In the context of development it would be interesting to further subdivide the less than high school group: perhaps the important margin of substitution is between those with no schooling versus some, or primary versus secondary. The small sample size of the less than high school group prevents us from exploring this further.

Figure 5 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 5: Wages and Education Level

The corresponding development accounting results are given in Table 8. 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.[18]

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

[18] 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 8: Robustness: Human Capital Share in Development Accounting by Education

Robustness Check	Human Capital Share	95% Confidence Interval	N
Less than High School Graduate	0.50	(0.39, 0.61)	138
High School Graduate	0.58	(0.48, 0.68)	183
Some College, No Degree	0.50	(0.35, 0.64)	82
College Degree or More	0.66	(0.61, 0.71)	504

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.

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 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 imperfect skill transfer lower the human capital share in development accounting to roughly one-half.

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A New Immigrant Survey Details

The NIS attempted to sample 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 cases. Surveyors 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 in the text 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 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 throughout are summarized in Table A1. The first row shows the initial sample size of workers for whom we can construct pre-migration hourly wage, post-migration hourly wage, or both (necessary for computing the wage gains at migration). While we have 4,700–5,700 workers with wages before or after migration, we only have both for around 2,300 workers. The main reason for the disparity is that many of our immigrants did not work before immigrating or are not working at the time of the survey. Roughly 60 percent of immigrants with post-migration wages but no pre-migration report never working before immigrating; roughly 75 percent of immigrants with pre-migration wages but no post-migration wage report that they are not working. The remaining cases are typically those who work but do not report the information needed to construct hourly wage, such as income, hours worked, currency in which they were paid, and so on.

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 identify the immigrant's previous country of work or birth; we need to be 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. The most important restrictions in terms of lost sample size are being able to adjust wages, excluding immigrants who were paid in currencies that were subsequently devalued, and excluding immigrants with some U.S. schooling; each reduces the sample by 200–300 immigrants.

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	3,875	5,612	2,006
Matched GDP	3,873	5,602	2,005
No Devaluation	3,486	5,224	1,808
Trim Wage Outliers	3,357	5,156	1,745
Arrived After 1983	3,018	4,621	1,612
No U.S. Schooling	2,640	3,364	1,383

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.

Our primary focus is on the sample of workers with both pre- and post-migration wages, for whom we can 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 look for systematic differences between three mutually exclusive groups of immigrants: those with both pre- and post-migration wages; those with only pre-migration wages; and those with only post-migration wages. We focus on the standard characteristics used throughout the paper: age, education, and hourly wage. We construct the means for each of the three mutually exclusive groups by GDP per worker category and display the results in Figure A1. 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).

Likewise, panel (d) shows small differences in age. The main differences of interest are in panel (c): immigrants with only post-migration wages are much less educated than the other groups. In part this gap is attributable to our sample restriction to workers who have no U.S. education; the set of workers who have not worked before coming to the U.S. but also have no U.S. school are less educated. The remainder of the gap can be

Figure A1: Comparison of NIS Balanced and Unbalanced Samples

attributed to the fact that the set of workers who have not worked before entering the U.S. are disproportionately likely to be immigrants from Mexico or legalization cases. Mean years of schooling after accounting for these two facts are essentially the same for the three groups.

B Details on Other Data Sources

This appendix provides detailed information on sample selection and exercises for the non-NIS data used in the paper.

B.1 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 2003–04 ACS, which combine to form a nationally representative sample of slightly less than one percent of the U.S. population surveyed in the same years as the NIS.[19] 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 U.S. or its territories. The second is immigrants, defined as individuals born outside the U.S. 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 U.S., which 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 their log-wage net of the predicted wage from this regression.

B.2 Indian Socio-Economic Survey

Micro data for India are taken from the 1999 Socio-Economic Survey (retrieved from IPUMS-I (Minnesota Population Center, 2014)). The sample contains full time (FULLTIME = 2) wage workers (CLASSWRK = 2) between the ages of 18 and 65. We require information on schooling (EDATTAIND), wage and salary income (INCWAGE), and occupation (OCC), as well as a positive sample weight (PERWT). The wage is defined as weekly labor earnings (INCWAGE) divided by 40 hours per week. We drop observations with wages below 2% of the median wage or above 200 times the median wage. The sample contains 58,503 men and 16,865 women.

Our objective is to calculate mean log wages for workers with given education, experience,

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

sex, and occupation. To do so, we regress log wages on a quartic in experience, dummies for seven school levels (no school, some primary, primary, some secondary, seconday, some tertiary, tertiary; based on EDATTAIND), and occupation dummies, using weighted least squares. The regressions are estimated separately for men and women.

B.3 Current Population Survey

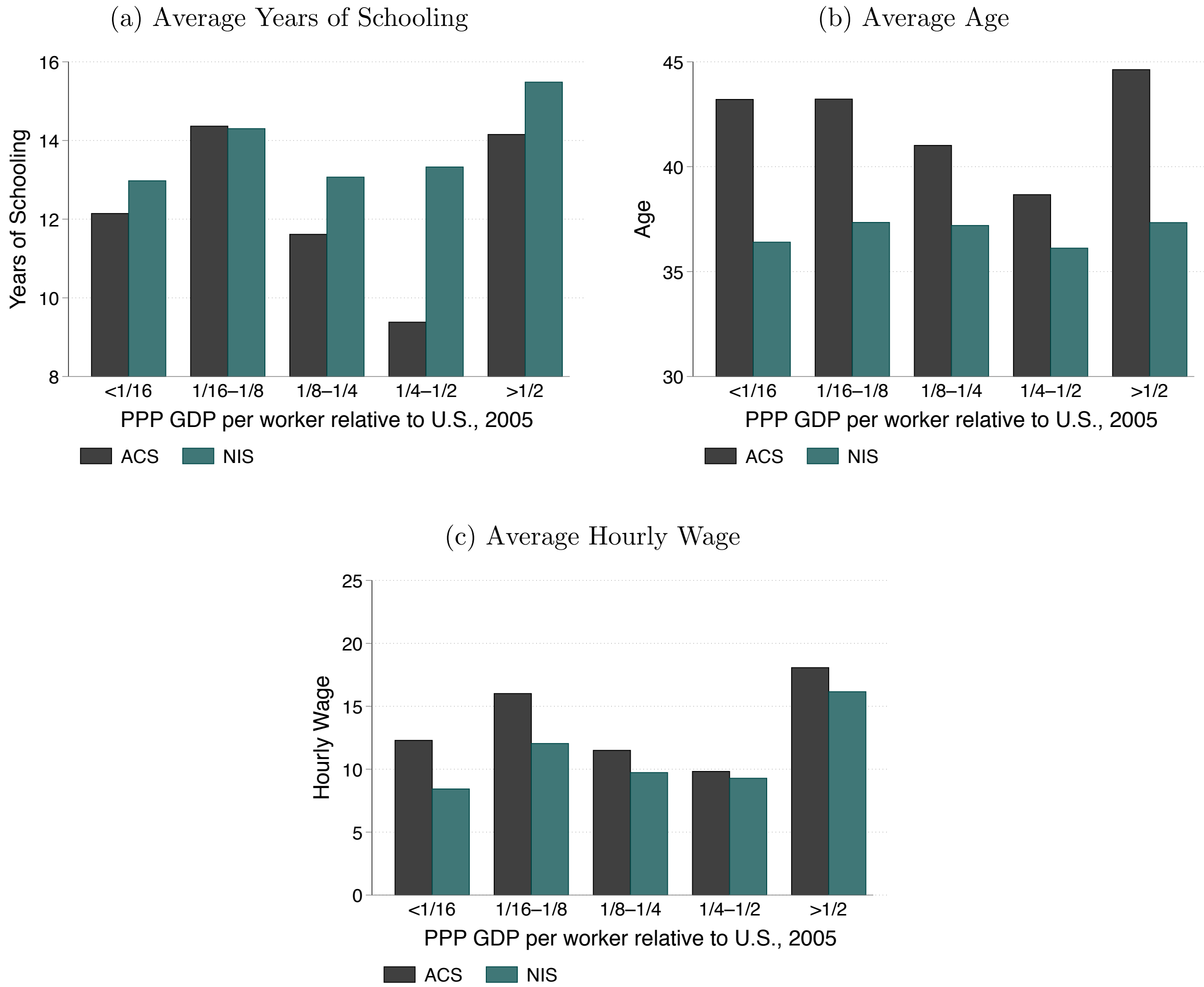
We obtain data from the March Current Population Surveys from IPUMS (King et al., 2010). The sample contains workers between the ages of 16 and 66 with valid information on education (EDUC). We also require positive earnings weights (EARNWT) or person weight (PERWT, when EARNWT is not available), positive 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 (CLASSWLY ¡ 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% 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).

C Comparison of ACS and NIS

The NIS 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 do so by comparing the NIS and the ACS samples; the ACS is a useful choice because it is the successor to the long-form Census that is commonly used in the literature to study immigrant wage patterns.

We start by comparing education, age, and hourly wage in the NIS and the ACS samples, aggregated in both cases to the same PPP GDP per worker categories used throughout the paper. These results are displayed in Figure C1. We can see that the NIS sample of new lawful permanent residents is much younger, a little better educated, and receives a

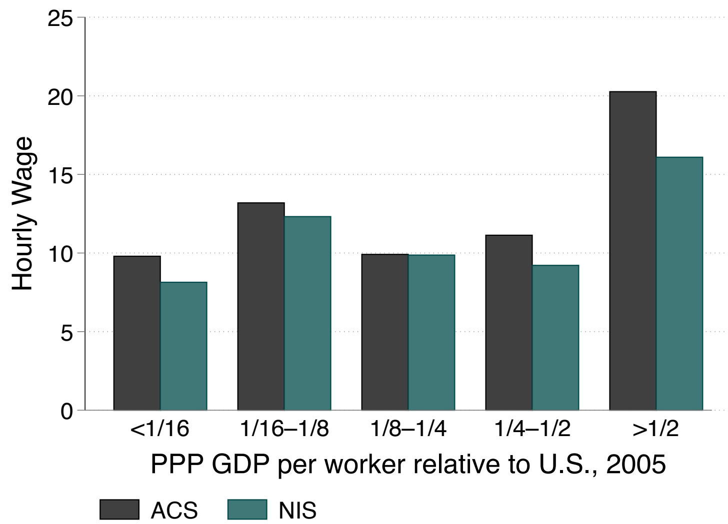
Figure C1: Comparison of NIS and ACS Samples



somewhat lower wage than the overall sample of immigrants from the ACS.

The most important difference for our purposes is the gap in hourly wages for the three lowest GDP per worker categories. Much of this gap can be attributed to composition effects: immigrants in the NIS come from somewhat poorer countries and tend to have immigrated more recently. To demonstrate this, we estimate log-wages in the ACS as a function of a full set of country of birth-year of immigration interactions. We then use this regression to predict the "ACS wage" of each immigrant in the NIS. We aggregate both the reported wage and the "ACS wage" up to the GDP per worker category and show the results in Figure C2. The wage figures agree much more closely now: the average gap for the three lowest GDP per worker categories is just five percent. This suggests that much of the wage gap in Figure C1c can be attributed to a change in the composition of countries (which our method handles naturally) and assimilation, which is discussed in detail in Section 4.4.

Figure C2: Comparison of Matched NIS and ACS Samples



C.1 Replication of Literature Results

A second useful way to compare the NIS and the ACS is to ask whether they give the same answer to the questions typically asked in the literature. As discussed in Section 5, the most common previous approach involved comparing the residualized wages of immigrants from rich and poor countries. Here, we replicate the simplest approach in both data sets. To do so, we first construct residualized wages in both, as discussed in Appendix B. We then find the mean residualized wage by country; we aggregate to the country level to avoid over-weighting countries such as Mexico that happen to have large samples of immigrants. Our main result comes from a regression of mean residualized log wage on log GDP per worker (year 2005). This closely parallels the main approach in Hendricks (2002).

The results are given in Table C1. In both datasets the elasticity of residualized wage with respect to GDP per worker is roughly 0.1, which is also in line with Hendricks (2002) and Borjas (1987). One possible concern is that in the NIS there are some countries with almost no migrants because of the small sample size. Nonetheless we find very similar elasticities if we restrict the sample to countries with at least ten immigrants. This shows that the

Table C1: Replication of Literature Results

Sample	Intercept	Log(GDP per worker)
ACS	0.784	0.101
	(0.027)	(0.014)
NIS (all countries)	0.523	0.095
	(0.071)	(0.035)
NIS (at least ten migrants)	0.534	0.135
	(0.080)	(0.040)

Note: Dependent variable is mean log residualized wage, averaged by country of birth. Residualized wages computed as discussed in Appendix B. Standard errors are in parentheses.

main patterns of interest to the previous literature also hold in the NIS. Our results differ because of our approach, not the data.

D Model Extensions

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

D.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 $\log(z_{U.S.}) - \log(z_c)$, 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:

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

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 gaps in z and y are 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.

D.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 c. 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 c are not valued in the U.S.; 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:

$$\begin{aligned}\log(w_{i,c}) &= \log[(1-\alpha)z_c] + \log(h_i) \\ \log(w_{i,U.S.}) &= \log[(1-\alpha)z_{U.S.}] + \log(h_i) + \log(\phi).\end{aligned}$$

The wage gains at migration are given by:

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

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.

D.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:

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

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 unskilled immigrants. This is given by:

$$\left[\log(w_{i,s,U.S.}) - \log(w_{i,s,b})\right] - \left[\log(w_{i,u,U.S.}) - \log(w_{i,u,b})\right] = -\frac{1}{\sigma}\log\left(\frac{H_{s,U.S.}}{H_{u,U.S.}}\frac{H_{u,c}}{H_{s,c}}\right) \tag{7}$$

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 c. 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.