Experience Matters:

Human Capital and Development Accounting*

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Abstract

Using recently available large-sample micro data from 36 countries, we document that experience-earnings profiles are flatter in poor countries than in rich countries. Motivated by this fact, we conduct a development accounting exercise that allows the returns to experience to vary across countries but is otherwise standard. When the country-specific returns to experience are interpreted in such a development accounting framework – and are therefore accounted for as part of human capital – we find that human and physical capital differences can account for almost two thirds of the variation in cross-country income differences, as compared to less than half in previous studies.

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1 Introduction

Understanding the determinants of cross-country income differences is one of the central aims of development and growth economics. An important first step in addressing this difficult question is to assess what fraction of these income differences are due to observable factors of production, namely physical and human capital. One of the main challenges in such development accounting exercises is the measurement of human capital stocks. Klenow and Rodriguez-Clare (1997) and Hall and Jones (1999) first addressed this difficulty by constructing human capital stocks from country-level measures of educational attainment. This was further refined by studies that accounted for additional aspects of human capital, such as schooling quality (Barro and Lee, 2001; Hanushek and Kimko, 2000; Hendricks, 2002; Schoellman, 2012), experience (Klenow and Rodriguez-Clare, 1997), and health (Weil, 2007; Shastry and Weil, 2003).[1] However, even with the most expansive definitions of human capital, human and physical capital together account for less than half of the variation in cross-country income differences (see for example, Caselli, 2005; Hsieh and Klenow, 2010). In other words, more than half of the variation in cross-country income differences is accounted for by residual total factor productivity (TFP).

In this study, we document a new fact: experience-earnings profiles are flatter in poor countries than in rich countries. Then, motivated by this fact, we conduct a development accounting exercise that allows the return to experience to vary across countries but is otherwise standard. The accounting exercise implies that cross-country differences in human capital due to experience are much larger than previously thought, and that human and physical capital differences account for almost two thirds of the variation in income across countries, compared to less than half in earlier studies.

Our study proceeds in three steps. The first step is to collect recently available large-sample micro data for 36 countries. These data, which comprise over 200 household surveys, provide several important benefits. First, the large sample sizes – at least five thousand but often many more individuals per survey – allow us to estimate the returns to experience with minimal restrictions

[1] Barro and Lee (2001) construct quality of schooling measures by using student-teacher ratios and government spending on education. Hanushek and Kimko (2000) measure quality with test scores. Weil (2007) and Shastry and Weil (2003) account for health using data on adult mortality rates. Bils and Klenow (2000) also construct human capital stocks taking into account the levels of both schooling and experience, though with substantially less detailed data than in the current study. Hendricks (2002) and Schoellman (2012) both use returns to schooling for immigrants to the United States to estimate country differences in schooling quality.

on functional form and to decompose the data to examine the mechanisms that drive our main findings. Second, the comparable sampling frames across countries provide substantial scope for making international comparisons. Finally, the availability of multiple surveys for most countries allows us to partially control for cohort effects or time effects in our estimates and to check that the cross-sectional estimates of the experience-earnings profiles are not driven by spurious factors correlated with time such as aggregate growth or those correlated with cohorts such as improvements in health.

The second step is to document the relationship between wages and potential work experience, defined as the number of years an individual could have been working, which we will refer to simply as *experience*.[2] Absent theoretical predictions about the functional form of experience-earnings profiles, we begin our empirical analysis by allowing the returns to experience to vary fully flexibly for each additional year of experience. These fully flexible estimates show that experience-earnings profiles in poor countries typically lie below those of rich countries, i.e., the profiles are "flatter" in poor countries. We then demonstrate that this finding is robust to a number of alternative sample restrictions and definitions of experience and to controlling for either time or cohort effects to the extent that our data allow.

The final step is to illustrate the economic significance of our empirical findings by applying them to a simple development accounting exercise. While there are other frameworks that can potentially be used to interpret our results, this is a natural application because experience-earnings profiles translate directly into human capital stocks under the standard assumptions of development accounting.[3] We calculate the part of human capital due to experience and show that this is positively correlated with income, and furthermore that its cross-country dispersion is similar in magnitude to the dispersion of human capital due to schooling. Putting these together, we find that the contribution of physical and human capital in accounting for cross-country income differences increases from less than one-half to almost two-thirds.

While it is beyond the scope of this paper to conclusively explain why experience-earnings profiles

[2] We also refer to this relationship as experience-*earnings* profiles even though we really examine experience-*wage* profiles, simply because the former seems to be the more standard term in the literature. All our findings also hold for actual experience-earnings profiles, as well as age-earnings profiles and age-wage profiles.

[3] The standard assumptions of development accounting are that workers earn their marginal products, that they supply their entire human capital to the labor market, and that human capital is valued in efficiency units. This is discussed in detail in Section 5 where we also consider some alternative explanations in which factors other than human capital accumulation affect the shape of experience-earnings profiles.

are flatter in poor countries than rich countries, we explore some of the most obvious possibilities as far as the data allow. Specifically, we examine the extent to which our findings are driven by cross-country differences in the composition of workers (e.g., by schooling attainment or sector of work). We find that composition differences explain very little of the cross-country differences in the steepness of experience-earnings profiles. Thus, we explore the existing theoretical literature to shed light on the underlying mechanisms. Amongst other possible explanations, we note that our main finding that experience-earnings profiles are flatter in poorer countries is consistent with a class of theories in which TFP and experience human capital accumulation are complementary (i.e., low TFP in poor countries depresses the incentives to accumulate human capital). Two prominent examples from this literature are the work of Erosa et al. (2010) and Manuelli and Seshadri (2010).

One important limitation of our study is that our data cover only 36 countries and represent much but not all of the world's population. The countries in our sample represent 68% of the world's population and range between the first (United States) and 83rd percentile (Bangladesh) of the world income distribution. Notably, we exclude most of the very poorest countries in the world, such as those in Sub-Saharan Africa. Another limitation is that our results cover wage earners but not the self-employed, whom we exclude because of measurement issues as we discuss below.

Our study adds directly to the existing literature on development accounting discussed at the beginning of the introduction. Conceptually, the main difference between our study and the previous literature is the following: Existing studies eliminate all productivity differences across countries and estimate the fraction of cross-country income differences that can be accounted for with quantities of observable factors of production. In contrast, we only restrict aggregate production functions to be the same across countries but allow human capital production functions to vary. We then empirically identify what amounts to cross-country TFP differences in human capital production.

Empirically, the main improvement that our study makes over the existing literature is that we collect large-sample micro data to estimate country-specific experience-earnings profiles. Lacking such data, past studies have typically relied on secondary estimates from small and often non-representative samples. Better data also allows us to capture the non-linearity in experience-earnings profiles more accurately than these previous estimates, which have typically imposed a quadratic functional form on the profiles.[4] This is important because experience-earnings profiles are partic-

[4]For example, most of the estimated returns to experience used by Bils and Klenow (2000) come from the work

ularly non-linear in richer countries so that a quadratic specification would lead us to understate differences in steepness across countries.[5]

Our study is most closely related to Klenow and Rodriguez-Clare (1997) and Bils and Klenow (2000). These two studies allow the *levels* of – but not the *returns* to – experience to vary across countries and find that this has little effect on estimated human capital stocks of poor countries relative to rich ones.[6] Our results complement the work of Mankiw et al. (1992), Erosa et al. (2010), Manuelli and Seshadri (2010), Jones (2011) and Gennaioli et al. (2011), each of which emphasize the importance of human capital for explaining cross-country differences.[7]

Finally, we note that there are potentially many other interpretations of the cross-country patterns in experience-earnings profiles that we uncover beyond development accounting. In the context of this literature, the empirical patterns that we document should be interpreted as new facts that may be helpful for guiding future theories of cross-country differences in economic performance. In this sense, our study is similar in spirit to some recent studies that document productivity patterns across countries with survey data. For example, Hsieh and Klenow (2009) document that there is more dispersion in marginal revenue products across firms in China and India than the United States. Hsieh and Klenow (2011) refine the analysis by documenting that firms grow less with age in Mexico and India than the United States. Similarly, Bloom and Van Reenen (2007) and Bloom et al. (2010) document that managerial practices are worse in poor countries than in rich countries.

of Psacharopoulos (1994) who uses a quadratic functional form.

[5]We find that at least a quintic specification is needed to accurately capture the shape of experience-earnings profiles parametrically. The need of higher order polynomials is consistent with the findings of Murphy and Welch (1990) who argue that at least a quartic specification is needed to match age-earnings profiles in the United States.

[6]This is due to the fact that the positive effect of longer life expectancy and later retirement on the levels of experience in rich countries is offset by the negative effect of more years of schooling on the level of experience (e.g., higher schooling means that workers enter the labor force later in life).

[7]The specific mechanisms differ across studies. Mankiw et al. (1992) proxy for human capital with educational attainment and do not take into account human capital from experience. Schoellman (2012) estimates that schooling quality is lower in poor countries than rich countries using returns to schooling for immigrants to the United States and finds that after schooling quality is accounted for, human capital differences are twice as large as standard estimates constructed with country-specific returns to schooling. Jones (2011) points out that one critical assumption of traditional development accounting is that different skill types are perfect substitutes in production and argues that relaxing this assumption can substantially amplify the role of human capital in accounting for cross-country income differences (and even close the entire income gap between rich and poor countries if high and low skill types are sufficiently complementary). Manuelli and Seshadri (2010) and Erosa et al. (2010) use Ben-Porath style models to back out the implied quality differences in human capital across countries and argue that these differences are quantitatively substantial. Gennaioli et al. (2011) conduct a development accounting exercise for sub-national regions from countries around the world and argue that human capital is the most important determinant of regional development. Their main conceptual departure from existing studies is to distinguish between entrepreneurs and workers and to argue that the former may have substantially higher Mincerian returns to schooling than the latter, resulting in large cross-regional differences in aggregate human capital stocks

This paper is organized as follows. Section 2 describes the data. Section 3 documents experience-earnings profiles across countries. Section 4 applies the empirical estimates to a development accounting exercise, compares the results to existing accounting exercises and explores the importance of composition effects in driving our results. Section 5 briefly discusses possible mechanisms for our empirical findings, including some alternative explanations in which factors other than human capital accumulation affect the shape of experience-earnings profiles. Section 6 offers concluding remarks.

2 Data

Our analysis uses large-sample household survey data from 36 countries. The surveys we employ satisfy two basic criteria: (i) they are nationally representative or representative of urban areas; and (ii) they contain data on labor income for at least five thousand individuals. We make use of multiple surveys for each country whenever data are available. The final sample is comprised of 203 surveys spanning the years 1960 to 2011. The complete list of countries and data sources are listed in the Data Appendix.

The countries in our sample comprise a wide range of income levels, with the United States, Canada and Switzerland at the high end and Bangladesh, Vietnam and Indonesia at the low end. The combined population of the countries for which we have at least one survey amounts to 68% of the world population. Thus, while we lack data from many countries, our sample does represent a sizeable fraction of the world's total population. The biggest limitation of our sample in terms of coverage is that we have no data for the very poorest countries in the world, particularly those in Sub-Saharan Africa.

The main analysis uses individual-level data on age, years of schooling, labor income and the number of hours worked. We restrict attention to individuals with zero to 45 years of experience, who have positive labor income and non-missing age and schooling information. In all surveys, we impute the years of schooling using educational attainment data. In the majority of surveys, we measure labor income as monthly wages or salaries from both primary and secondary jobs. Similarly, in the majority of surveys, we measure hours as the actual hours worked at both primary and secondary jobs. The Data Appendix provides details for each country.

We also restrict attention to workers that are wage earners and exclude self-employed workers.

We do so for several reasons. First, self-employed individuals tend to under-report their income when asked directly and often report revenues instead of income (Deaton, 1997). Second, conceptually the income of the self-employed consists of payments to labor and to capital, and in our data (as in most other data) it is hard to distinguish between them (Gollin, 2002). Third, self-employed income often accrues in practice to the household, not the individual, making it hard to know how to treat self-employed income reported at the individual level.[8]

We follow the development accounting literature in defining experience as *potential experience* such that *experience = age – schooling – 6* for all individuals with eight or more years of schooling and *experience = age – 14* for individuals with fewer than eight years of schooling. This definition implies that individuals begin to work at age fourteen or after they finish school, whichever comes later. The cutoff at age fourteen is motivated by the fact that we observe very few individuals with positive wage income before the age of fourteen in our countries (see Figure A.4). Later, in Section 3.5 we show that our results are robust to several alternative definitions of potential experience and using age rather than experience.

We define an individual's wage to be her labor income divided by the reported number of hours that she worked. In 31 out of 36 countries, the surveys report the number of hours worked over the past week or some recent reference week. In the few countries without these data, we impute an individual's number of hours worked as the average number of hours across all other countries for that individual's experience level.

For most countries, we have surveys for two or more years. For these countries, we can control for cohort effects or year effects in our estimates. For Argentina and China, our data are representative of the urban population, and for all the remaining countries our data are nationally representative.

3 Estimating Experience-Earnings Profiles

3.1 Conceptual Framework

We motivate our empirical estimation using a simple model of human capital, similar to the one proposed by Bils and Klenow (1998). Human capital of individual i, who is a member of birth

[8]When we nonetheless include the self employed, taking their reported income at face value, we still find that profiles are flatter in poor countries. Results are available upon request.

cohort c at time t, h_{ict}, depends on schooling, s_{ict} and experience, x_{ict}:

$$h_{ict} = \exp(g(s_{ict}) + f(x_{ict})). \tag{1}$$

We further impose $f(0) = g(0) = 0$, meaning that we normalize the human capital of a worker with zero years of both schooling and experience to be one. Thus, we focus on the part of human capital due to schooling or experience.[9]

Following standard development accounting exercises, we assume that workers earn their marginal products, supply their entire human capital to the labor market and that human capital is valued in efficiency units up to a mean-zero error term. These assumptions will allow us to identify individual human capital stocks directly from individual wages.[10]

Hence, an individual's hourly wage is equal to the product of her human capital, a skill price ω_{ct}, and an error term ε_{ict}:

$$w_{ict} = \omega_{ct} h_{ict} \exp(\varepsilon_{ict}). \tag{2}$$

We allow the skill price, ω_{ct}, to differ across cohorts and time periods:

$$\omega_{ct} = \bar{\omega} \exp(\gamma_t + \psi_c). \tag{3}$$

Substituting (1) and (3) into (2) and taking logs, we obtain

$$\log w_{ict} = \log \bar{\omega} + g(s_{ict}) + f(x_{ict}) + \gamma_t + \psi_c + \varepsilon_{ict}. \tag{4}$$

In what follows we estimate the function $f(\cdot)$ and assess how it varies across countries. Our first empirical exercise is to estimate equation (4) under the assumption that there are no cohort or time effects, $\gamma_t = \psi_c = 0$. Then, we will show that the results are robust to including cohort and time effects to the extent possible.

[9] Note that the choice of an additively separable specification has the benefit that it will later allow us to separately identify human capital from schooling versus from experience. However, we will also show later in the paper that this assumption is not necessary for identifying total human capital.

[10] In Section 5 we will discuss the interpretation of experience-earnings profiles when some of these assumptions are relaxed. We will also provide a set of weaker assumptions under which we can identify aggregate human capital stocks.

3.2 Benchmark Results

We begin our empirical analysis by allowing the relationship between experience and wages to vary for each year of experience. This flexible functional form fully accounts for changes in the slope of the experience-earnings profile. We estimate

$$\log w_{ict} = \alpha + \theta s_{ict} + \sum_{x=1}^{45} \phi_x D_{ict}^x + \varepsilon_{ict}, \tag{5}$$

where D_{ict}^x is a dummy variable for a worker's number of years of experience. The coefficient ϕ_x estimates the average wage of workers with x years of experience relative to the average wage of workers with zero years of experience. In terms of our notation from the previous section, $\phi_x = f(x)$ such that the coefficient estimate corresponding to each experience level, x, identifies the experience-earnings profile evaluated at point x. We first estimate this equation without including either cohort or time effects, $\gamma_t = \psi_c = 0$.

Figure 1 presents the main finding of the paper: experience-earnings profiles are flatter in poor countries than rich countries. Panel (a) displays the experience-earnings profiles for six large and representative countries in our sample.[11] The steepest profile among these six countries is in the United States, which is also the richest country. Germany has the next steepest profile, followed by China, Brazil, Mexico and finally, India. Here "steepness" refers to the average slope of the profiles over all experience levels as opposed to the pointwise slope at a given level of experience. Figure 1a also shows that the cross-country differences in the profiles are mostly realized by twenty years of experience, which is also approximately the average experience level of most countries in our sample. Therefore, to illustrate the relationship between the steepness of the profiles and income for all of the countries in our sample, we plot the height of the estimated profiles evaluated at twenty years potential experience against the log of GDP per capita at PPP in Figure 1b. The figure clearly shows that the experience-earnings profiles in poor countries are systematically flatter than those in rich countries. This is our main empirical result. The correlation between the height of the profiles at 20 years and log GDP per capita is 0.60 and is significant at well below the 1% level.

[11] The estimated profiles for all of the countries in our sample are displayed in Appendix Figures A.2a-A.2d. The countries are grouped according to their position on the world income distribution.

3.3 Cohort and Time Effects

In this section, we investigate the effect of controlling for time or cohort effects. This could be important if there are many changes in aggregate conditions over time (e.g., periods of rapid economic growth) or if there are significant changes in the productivity of workers across cohorts (e.g., changes in the health or education quality of younger cohorts of workers). Given the fast economic growth of developing economies such as China or India during the time-period of our study, the omission of such controls could be important for our cross-country comparisons.

To address the potential influences of time effects, we control for calendar year fixed effects. Since there are often large intervals of time between the waves of data for the countries in our sample, we control for dummy variables that take the value of one if a worker belongs to a twenty-year non-overlapping birth-year category to address the potential influences of cohort effects. Henceforth, we will refer to these as cohort "fixed effects".[12] For this exercise, we only examine countries for which there is more than one cross-section of data.

In Figure 2 we plot the predicted profiles based on the estimates of equation (5) when year controls are included. There is little difference from the predicted profiles based on the cross-sectional estimates shown in Figure 5. The correlation between log GDP per capita and the profile heights at twenty years of experience is 0.61, and is significant at the 1% level. The fact that we find little change over time is not altogether surprising since our data cover a relatively short time period for most countries, often less than ten years. Figure 3 plots the predicted profiles for the returns to experience when birth-cohort fixed effects are included in equation (5). As the figure shows, adding cohort controls affects estimates for particular countries but does not change the

[12] Note that cohort fixed effects (defined as any interval) only allow for differences in the intercepts of experience-earnings profiles for different cohorts. It does not allow the slope of the profiles to differ across cohorts. For example, this could be a concern if one believes that younger workers in quickly growing economies have steeper experience-earnings profiles than older workers. In principle, the most direct way of addressing this problem is to estimate distinct experience-earnings profiles separately for each cohort. In practice, this is not possible unless one has repeated cross-sections over a very long period of time (because otherwise there will be no inexperienced older workers or experienced younger workers). The limited time coverage is also why our main estimates use twenty-year intervals to define birth cohort categories. This is a generic difficulty for all existing studies of development accounting, which have not attempted to allow for either the intercepts or slopes to differ across cohorts. We conduct an alternative indirect check of our cross-country results by simply omitting countries where different cohorts are most likely to have different experience-earnings profiles. In Appendix Table A.1 we alternatively omit the five countries with the largest changes in health or education or the largest change in income as measured by proxies we obtain from the World Development Indicators. Our estimated cross-country correlation between the steepness of the profiles and income is present and similar in magnitude in each case. Therefore, we conclude that our main cross-country results are unlikely to be confounded by differences across cohorts in either the intercept or the slope of experience-earnings profiles.

overall result. Figure 3b shows that the correlation between the height of the profiles at twenty years and income is still 0.53 and is statistically significant at the 1% level.

Finally, to attempt to control for both time and cohort effects, we estimated the experience profiles controlling for cohort effects and the logarithm of a country's per-capita GDP in a given year. These are shown in Figures 4a and 4b. They are similar to the other results, with a correlation between log GDP per capita and the profile heights at twenty years of experience of 0.62. We have also estimated profiles using the method suggested by Deaton (1997), which imposes the restriction that time effects sum to zero, and a variant of Deaton's method that restricts the average time effects to the average growth rates for the period for which we have data. These are again are similar to the other results and are available on request.

The estimates in this section show that our estimated profiles with cohort or year controls are similar to our benchmark cross-sectional estimates. Thus, in much of the remainder of the paper we will focus on the cross-sectional estimates of the experience-earnings profile.

3.4 Parsimonious Functional Form for Experience-Earnings Profiles

While the fully flexible estimates are necessary for revealing the true functional form of the wages-experience relationship, a parsimonious approximation of the relationship is more convenient for several exercises that we will conduct in this paper (e.g., examining compositional effects) and for comparing our results to the existing development accounting literature. Figures 1a to 4a show that experience-earnings profiles are highly non-linear, particularly in rich countries. A quadratic specification therefore provides a poor approximation of the true profiles, and we found that the lowest-order polynomial that well approximates the true profiles is a quintic. This is not surprising given the finding of Murphy and Welch (1990) that the U.S. experience-earnings profile cannot be captured with a quadratic specification, but instead requires at least a quartic.

The parsimonious quintic specification is

$$\log w_{ict} = \alpha + \theta s_{ict} + \sum_{k=1}^{5} \phi_k x_{ict}^k + \varepsilon_{ict}, \tag{6}$$

where the log wage of individual i of cohort c during year t is a function of her years of schooling, s_{ict}, and her years of experience, x_{ict}. This is the special case of the model presented in Section 3.1 with $g(s) = \theta s$ and $f(x) = \sum_{k=1}^{5} \phi_k x^k$. Figure 5a plots the predicted experience-earnings profiles based on our quintic estimates. It shows that the quintic estimates closely resemble the fully flexible estimates in Figure 1a. Thus, for parsimony we will use the quintic specification for the remainder of the paper.[13]

3.5 Robustness

This section demonstrates that our finding of flatter experience profiles in poor countries is robust to a variety of sample restrictions and alternative measures of potential experience, some of which can be important for interpreting the estimates. We also show that our main results are unlikely to be driven by measurement error in age or schooling in the data.

Sample Restrictions In the development accounting framework, we interpret the experience-earnings profile as the returns to experience (recall Section 3.1). One potential challenge to this interpretation comes from the concern that the experience-earnings profile for women do not accurately reflect human capital because of labor market exits and re-entry due to fertility. For example, if the labor market penalizes women for childbearing, then the experience-earnings profiles for women will be difficult to interpret. Another related concern is that a significant portion of wage earners (especially in very poor countries) work for the public sector and that the public sector does not pay wages to reflect worker productivity (e.g., the public sector pays workers with non-wage benefits as well as with wages). Similarly, one may worry that wages for agricultural workers in poor countries are mis-measured, which in turn causes our estimates of the average experience-earnings profiles to be mis-measured.

To address these concerns, we repeat our estimates of the experience-earnings profiles on samples that are restricted to male workers, private-sector workers, non-agricultural workers, and different combinations thereof. Table 1 shows that our main finding is robust to these alternative sample restrictions. For simplicity, we focus on the correlation between the height of the profile at twenty years of experience and the log of GDP per capita as the statistic for comparison. The correlation in the benchmark estimates from Section 3.2 is 0.60 and significant at the 1% level. The panel that follows presents the correlation coefficients for male workers, private-sector workers, full-time workers, male private-sector workers, male private full-time workers and non-agricultural workers.

[13] The correlation between the height of the profiles at 20 years of experience from the quintic specification and log GDP per capita equals 0.60, which is identical to the correlation when using the fully flexible specification. Also all of our results are robust to using the fully flexible specification in (5). They are not reported for brevity but are available upon request.

The correlations range from 0.53 to 0.68 and are all significant at the 1% level.

Experience Definition Our main exercise assumes that individuals start work when they finish schooling or reach fourteen years of age, whichever comes sooner. The middle panel of Table 1 reports the correlation for alternative definitions of potential experience.

The first of these makes the more standard assumption, as in Caselli (2005), that all workers begin work at age six or whenever they finish schooling and hence sets *experience = age - schooling – 6.* The next three take the same definition of potential experience but restrict the sample to males, private-sector males and full-time private-sector males. The last assumes that all workers begin work at age fifteen or whenever they finish schooling, which is another plausible assumption given our observations in Figure A.4 and hence sets *experience = age – schooling – 6* for all individuals with nine or more years of schooling, and *experience = age – 15* for other workers. The correlations range from 0.45 to 0.58 when experience is assumed to start accruing at age six and is 0.65 when experience begins only at age fifteen. In all cases, the correlations are statistically significant at below the 1% level. Thus, our main result is not an artifact of our choice of definition for experience.

Returns to Schooling Finally, we re-estimate the experience-earnings profile with alternative measurements of the returns to schooling. This checks that our results are not driven by the fact that we estimate the returns to schooling from our data in place of using other conventional strategies in the literature. To check that our results are not sensitive to this difference, we first estimate the returns to experience under the restriction that returns to schooling satisfy the non-linear function used by Hall and Jones (1999).[14] Then, we follow Hsieh and Klenow (2010) and estimate the returns to experience by restricting the returns to schooling to be a constant 10%. The last panel of Table 1 shows that the correlations between the profile heights and the log of GDP per capita in the two exercises range between 0.63 and 0.64, and are both statistically significant. Thus, our main result is not an artifact of our choice for estimating the returns to schooling.

Measurement Error in Age In Bangladesh and India, two of the poorer countries in our sample, we observe age-heaping in the data, where individuals seem to be rounding their ages to the nearest five years (see for example India in Figure A.2). This is presumably due to survey respondents not knowing their true ages. Since random measurement error in age will attenuate our estimates

[14] They impose diminishing returns by assuming that $g(s)$ is a piecewise linear function with slope 0.13 for $s \leqslant 4$, 0.10 for $4 < s \leqslant 8$ and 0.07 for $8 < s$.

for the returns to experience and this is more likely to be a problem in poor countries than rich countries, one may be concerned that a significant part of the cross country differences in profiles is due to differences in measurement error.

To investigate the potential quantitative effect of bias caused by heaping, we construct a new auxiliary dataset for the United States where we replace the age of a certain percentage of workers with their age rounded to the nearest five years. We then re-estimate the returns to experience with this auxiliary dataset. Figure 6a shows that increasing the fraction of the sample to which we introduce measurement error does bias downward the profiles, but the effect is not quantitatively large. Even in the extreme case when we allow 90% of the U.S. population to mis-report their age, the profile is still far above that of India. Thus, our main cross-country results are not driven by biases induced through age-heaping.

Measurement Error in Education Years For most countries, direct measures of the years that individuals spent in school are not available. We therefore had to rely on educational attainment data (e.g., "secondary school degree" or "college degree") in order to construct the "years of schooling" variable. Moreover, the precision of the educational attainment data differs across countries. For example, for the U.S. we obtained *Current Population Survey* (CPS) data (in addition to the data we use in our main exercise) and in this dataset we have fifteen education groups from zero to 21 years of schooling. In contrast, for China we only have six education groups, with respectively six, nine, twelve, fifteen, sixteen or nineteen years of education. In the data, there is no correlation between the precision of education years estimates and the level of a country's GDP. Nevertheless, we here show that measurement error in education years is not able to generate quantitatively large differences in experience profiles.

To investigate the potential quantitative effect of bias caused by mismeasuring education years, we use the United States CPS data with its very precise educational attainment variable to construct a new auxiliary dataset in which we replace the education years of individuals with a fictitious educational attainment variable with wider year intervals. More specifically, we assume that we only have data on completed degrees and thus substitute zero years of education for all individuals with education less than five (no degree), five years of education for all individuals with education above or equal to five and below eight (primary school degree) and so on. After having constructed this new educational attainment variable, we recalculate potential experience for all individuals and re-estimate the experience-earnings profiles. Figure 6b shows that the experience-earnings profiles using the modified schooling measures are different, but the differences are not quantitatively large compared with the differences between the profiles of India and the United States.[15] This is also true if we recode years of education assuming to have information only on attained degrees ("degrees only") and if we recode it using the coarser categorization used in IPUMS ("IPUMS categories"). Thus, our main cross-country results are not driven by biases induced through measurement error in education.

Additional Sensitivity Tests Given the large labor economics literature that studies the returns to experience using the *Current Population Survey* (CPS), we replicate our estimates with these data to check that our results for the U.S. experience-earnings profiles are not driven by our choice of data. Figure 6c shows that the experience-earnings profiles estimated from Census data and from CPS data are nearly identical.

We also show that profiles are flatter in poor countries than rich countries when using age instead of experience (see Appendix Section C).

For brevity, we do not motivate or present the results for a large number of other robustness checks that we performed, which include showing that our results are robust to: different functional forms for estimating the returns to schooling, in particular higher order polynomials and fully flexible returns to education; alternative imputation methods for hours worked in countries with no hours data; restricting the sample to only include household heads; and restricting the maximum years of experience at 40, 45 or 50 years. These results are available upon request.

4 Aggregate Human Capital and Development Accounting

Having estimated experience-earnings profiles across countries, we now calculate individual and aggregate human capital stocks. We then conduct a standard development accounting exercise with our improved measures of aggregate human capital stocks and show that taking into account cross-country differences in returns to experience substantially increases the fraction of cross-country income differences that can be explained by human and physical capital stocks.

[15] The profile with artificial measurement error is steeper than that in our benchmark exercise. This is because measurement error in education affects profile steepness through two mechanisms: (i) it results in measurement error in potential experience and hence induces classical attenuation bias, (ii) it results in attenuation bias of the coefficient on *schooling* which in turn increases the steepness of the experience profile.

4.1 Human Capital from Experience

We begin by decomposing individual human capital stocks into the components due to experience and schooling $h_{it} = h_{it}^S h_{it}^X$, where

$$h_{it}^X = \exp(f(x_{it})), \qquad h_{it}^S = \exp(g(s_{it})).$$

Analogously, the part of aggregate human capital due to experience only is just the average of the individual stocks across individuals and over time

$$H^X = \frac{1}{T}\sum_{t=1}^{T}\frac{1}{N_t}\sum_{i=1}^{N_t} h_{it}^X. \tag{7}$$

Our estimates for aggregate human capital stocks due to experience are simply the integral of the estimated experience-earnings profiles from the previous section (i.e., the area beneath the profiles) using the distribution of work experience from the data.[16]

Figures 7a-7d plot the implied human capital from experience against per capita GDP for each country during this period. For these figures, the human capital stocks from experience are calculated using the quintic specification in equation (6) and each figure corresponds to a different combination for the inclusion of cohort and year controls. These figures show clearly that there is a strong positive relationship between human capital from experience and income levels, with correlations ranging from 0.58 to 0.61 and always significant at the 1% level. The estimates of experience human capital stocks for each country that are used in the figures are reported in Table A.2 in the appendix.

4.2 Total Human Capital Stocks from Schooling and Experience

We define the total human capital stock (due to both schooling and experience) in a country to be the average of individual human capital stocks, $h_{it} = \exp(g(s_{it}) + f(x_{it}))$,

[16] The distributions of work experience in our six representative countries are displayed in Figure A.2. Figure A.3 plots the average potential experience for all countries in our sample against GDP per capita. Our data show little variation in the average experience level across countries, as in the findings of Caselli (2005) and Bils and Klenow (2000).

$$H = \frac{1}{T}\sum_{t=1}^{T}\frac{1}{N_t}\sum_{i=1}^{N_t} h_{it}. \tag{8}$$

The estimates of these human capital stocks are based on our estimated returns to schooling and experience from our quintic specification.

Table 2 Panel (a) summarizes our country-specific estimates of aggregate human capital stocks and presents two measures of the cross-country dispersion of total human capital stocks. The first measure we use is the log variation in human capital stocks, and the second measure is the ratio of the human capital stock of the country at the 90th percentile in the world income distribution to one at the 10th percentile. The 90th percentile country has an experience human capital stock that is 1.96 times as large as that of the 10th percentile. For the sake of comparison, we also present the difference in the human capital stock from schooling, which equals 1.87. This is the traditional measure used by the literature. These results show that cross-country differences in human capital due to experience (as measured by the 90/10 ratio) are roughly as big as those due to schooling. When dispersion is instead measured using the log variation in column (1), it becomes somewhat larger for schooling than for experience human capital stocks, but both are of the same order of magnitude.[17]

Finally, the third row of Panel (a) in Table 2 reports the dispersion of total human capital stocks, where we take into account both schooling and experience. It shows that taking into account cross-country differences in returns to experience (going from row one to three) roughly doubles the dispersion in human capital across countries. Panel (b) of Table 2 shows that these numbers change somewhat when we include cohort and year effects, but that our main finding – that allowing returns to experience to vary across countries increases cross-country human capital gaps – is robust to their inclusion. The results reported in Table 2 are constructed from the country-specific estimates reported in Appendix Tables A.2 and A.3.

[17] Note that while cross-country differences in human capital due to schooling and experience are similar in magnitude, there is an important asymmetry in how those differences arise. In the case of schooling, it is well known that returns to a year of school completed are roughly similar across countries, while average years of schooling completed are higher in richer countries. For experience, as we document, returns to a year of experience are higher in rich countries, while the average level of experience is roughly the same across countries.

4.3 Development Accounting

To make the development accounting exercise comparable to the existing development accounting literature, we use the same accounting method as in the survey by Caselli (2005). Our accounting procedure uses a Cobb-Douglas aggregate production function $Y = K^{\alpha}(AH)^{1-\alpha}$, where Y is a country's real GDP, K is its physical capital stock, A is total factor productivity and H is our measure of the human capital stock. The capital share is assumed to equal one-third, $\alpha = 1/3$.

We follow Caselli (2005) and calculate his measures $success_1$ and $success_2$ that are designed to measure the fraction of cross-country income differences explained by factors of production

$$success_1 = \frac{\mathrm{var}(\ln Y_{KH})}{\mathrm{var}(\ln Y)},$$
$$success_2 = \frac{Y_{KH}^{90}/Y_{KH}^{10}}{Y^{90}/Y^{10}},$$

where $Y_{KH} = K^{\alpha}H^{1-\alpha}$ is the component of output explained by factors of production.

Intuitively, $success_1$ is the fraction of the variance of log GDP per capita that is explained by human and physical capital. $Success_2$ is the fraction of the 90th-to-10th percentile ratio of GDP that is explained by these factors of production.

Table 3 presents these measures of success for different measures of the aggregate human capital stocks. When human capital is identified by only schooling as in most of the literature, the two measures of success are between forty and fifty percent. Recall that in Table 2, we showed that cross-country differences in experience human capital are roughly as big as those in schooling human capital. This is reflected in Table 3: comparing the first and the second rows reveals that schooling and experience human capital are roughly equally important determinants of cross-country income differences. Finally, when both schooling and experience are taken into account as in the third row, both measures of success increase dramatically. Physical and human capital taken together now account for roughly two-thirds of the variation in cross-country income differences as compared to less than half when experience is not taken into account.

We can also conduct our development accounting exercise "country-by-country". To do this, we report a slightly modified version of $success_2$

$$success_2^j = \frac{Y_{KH}^{US}/Y_{KH}^j}{Y^{US}/Y^j}. \quad (9)$$

This is the fraction of the income gap between the United States and a poorer country j that can be explained by factors of production only. Table A.4 first reports Caselli's numbers for output and physical capital in columns (1) and (2). The estimates for $success_2^j$ are presented in columns (3)-(7). The results indicate that taking into account cross-country differences in returns to experience when calculating aggregate human capital stocks allows one to account for a substantially larger fraction of cross-country income differences than does the existing literature. Note that for some countries (e.g., Italy) and under some specifications our estimates even modestly over-predict the GDP gap with the United States when taking experience into account. Taken literally, this implies that the estimated human capital in these countries is so low relative to the United States that the only way of accounting for the observed income gap is for these countries to have "higher" TFP than the United States.

4.4 Comparison to Existing Accounting Exercises

We now summarize our development accounting results with a series of accounting exercises that precisely illustrate the effects of each departure that we take from existing accounting exercises and their influences on our final result. All exercises use our data and are shown in Table 4. We begin with a specification that is similar to the one used in Klenow and Rodriguez-Clare (1997) (Panel (a)) and proceed step-by-step to the specification in our benchmark exercise (Panel (e)), adding one element at a time.

The specification in Panel (a) computes human capital stocks using a linear-quadratic Mincer specification and the average returns to schooling and experience as in Klenow and Rodriguez-Clare (1997).[18] $Success_1$, when taking into account human capital due to both schooling and experience, is only 0.41 (Panel (a), third row, third column), which is considerably less than the result of one-half that we have cited as the upper-bound from the existing literature.

Panel (b) uses the same specification, but imposes diminishing returns to schooling as in Hall and Jones (1999).[19] This is also similar to Bils and Klenow (2000). $Success_1$ is essentially unchanged

[18] In our sample of 36 countries, the average coefficients on *schooling*, *experience* and *experience*^{2} are 0.09211, 0.04775 and -0.000758, which are similar to those in Klenow and Rodriguez Clare (1997).

[19] See footnote 14.

at 0.39. Panel (c) allows returns to experience to vary across countries, but retains the quadratic specification for estimating the returns to experience. This induces an increase in $success_1$ from 0.39 to 0.48. Panel (d) allows the returns to experience to vary across countries *and* uses our main quintic functional form for estimating the returns to experience. This causes $success_1$ to further increase from 0.48 to 0.62. Panel (e) additionally allows returns to schooling to vary across countries (estimated using a linear control for the years of schooling). This produces our main results shown in Tables 2 and 3: $success_1$ is 0.63.

The results in Table 4 show that our finding that human and physical capital contribute to two-thirds instead of less than one-half of cross-country income differences is due to our allowing the returns to experience to vary across countries *and* the more accurate approximation of the experience-earnings profile from using a quintic functional form. To illustrate the importance of the flexible functional form more clearly, Figure 8 repeats our empirical exercises from Section 3, but uses a linear-quadratic specification for estimating the experience-earnings profiles. Panel (a) shows that the quadratic experience-earnings profiles provide a poor approximation of the fully flexible ones whereas a quintic specification is much more accurate. Moreover, Panel (b) plots the height of both the quadratic and quintic experience-earnings profiles at twenty years of experience against countries' income levels and shows only a weak relationship for the quadratic profiles (black line) whereas it is much stronger for the quintic ones (light grey line).

4.5 Composition Effects

In this section, we attempt to shed light on the underlying forces of our cross-country findings by examining the extent to which the estimated cross-country differences in experience-earnings profiles are due to differences in worker compositions across workers.

Agriculture A key difference between rich and poor countries is that poor countries tend to have a much larger share of workers in agriculture than rich countries. This could affect our estimates of average experience-earnings profiles for each country as Herrendorf and Schoellman (2011, Figure 4b) document that profiles are generally flatter among agricultural workers than non-agricultural workers in the United States.

To consider the role that this composition difference may play in our development accounting exercise, we extend our simple model in Section 3.1 to allow for differences in human capital accumulation across sectors. In particular, we now allow the human capital production functions (1) to be different for agriculture (sector A) and non-agriculture (sector N). Human capital of individual i at time t in country j is now

$$h_{itj} = \exp(g_j(s_{itj}; D_{itj}) + f_j(x_{itj}; D_{itj})),$$

where $D_{itj} \in \{A, N\}$ is the sector that individual i is active in at time t. As before, the functions $f_j(\cdot\ ; A)$ and $f_j(\cdot\ ; N)$ can be identified from the experience-earnings profiles for agriculture and non-agriculture, i.e. we estimate equation (4) separately for each of the two sectors. Having done so, we construct aggregate human capital from experience in sector $D \in \{A, N\}$ and country j as

$$H^X_{D,j} = \frac{1}{T} \sum_{t=1}^{T} \frac{1}{N_{Dt,j}} \sum_{i: D_{itj} = D} \exp(f_j(x_{itj}; D)), \tag{10}$$

where $N_{Dt,j}$ is the number of individuals in country j at time t that are employed in sector $D \in \{A, N\}$. Aggregate experience human capital in country j is then simply a weighted average of the sectoral experience human capital stocks

$$H^X_j = \ell_{A,j} H^X_{A,j} + (1 - \ell_{A,j}) H^X_{N,j}, \tag{11}$$

where $\ell_{A,j}$ is the employment share in agriculture in country j. Figure 9a shows the height of the experience-earnings profile at twenty years of experience in agriculture plotted against that in non-agriculture. It can be seen that all countries except Italy, France and Australia lie below the 45 degree line (i.e., for all countries except Italy, France and Australia, the experience-earnings profiles in agriculture are flatter than those in non-agriculture).

To assess the quantitative importance of the cross-country differences in the proportions of workers engaged in agriculture for the cross-country differences in experience-earnings profiles, we conduct the following counterfactual exercises. We ask: what would a country's experience human capital be if that country had the United States' employment share in agriculture? We compute the following counterfactual experience human capital stock for each country j

$$\tilde{H}^X_j = \ell_{A,US} H^X_{A,j} + (1 - \ell_{A,US}) H^X_{N,j}. \tag{12}$$

If all of the cross-country differences in experience human capital stocks were due to sectoral differences, then this counterfactual would eliminate all such differences. Figure 9b graphs the counterfactual human capital stocks (those using U.S. agricultural employment shares) against the actual human capital stocks. If composition effects explained all of cross-country differences in experience human capital stocks, all countries would lie on a straight horizontal line at the level of the U.S. human capital stock. But instead, all countries lie near the 45 degree line – i.e., counterfactual human capital stocks are very similar to the actual ones. Thus, differences in agricultural employment shares across countries do not appear to be driving our results.

To make this point more rigorously, we decompose the variance of the logarithm of experience human capital stocks that we reported in Table 2 as follows

$$\underbrace{\operatorname{var}\left(\ln H^X\right)}_{\text{dispersion in } H^X} = \underbrace{\operatorname{var}\left(\ln H^X\right) - \operatorname{var}\left(\ln \tilde{H}^X\right)}_{\substack{\text{part due to} \\ \text{composition effects}}} + \underbrace{\operatorname{var}\left(\ln \tilde{H}^X\right)}_{\substack{\text{part due to} \\ \text{within-sector diff's in } H^X}} . \tag{13}$$

Panel (a) of Table 5 reports the results of this decomposition. Differences in employment shares between agriculture and non-agriculture explain a modest thirteen percent of the log variation in experience human capital stocks across countries.

Schooling Another important compositional difference between rich and poor countries is that workers in poor countries attain fewer years of schooling, which could drive our cross-country results since several studies have shown that college graduates have steeper *age*-earnings profiles than high school graduates (Carroll and Summers, 1991, Figures 10.7a and 10.8a; Guvenen, 2007, Figure 2; Kambourov and Manovskii, 2009, Figures 3,6,8 and 10; Elsby and Shapiro, 2012, Figure 3).

We explore the extent to which this difference drives our results by allowing the returns to experience to vary by the different levels of schooling in the human capital production function in equation (1). We allow for a functional form that is non-separable in schooling and experience, $h_{it} = \exp(m(s_{it}, x_{it}))$. If the function m has a positive cross-derivative, then schooling and experience are complements, which captures the idea that one has to "learn (in school) how to learn (on the job)." If, instead, m has a negative cross-derivative, this would suggest that schooling and experience are substitutes.

We work with a simple cutoff specification that allows for different returns to experience according to whether a worker has "high" (H) or "low" (L) educational attainment, i.e. whether his years of schooling are larger or smaller than some cutoff $\bar{s}$ that is common across countries

$$h_{itj} = \exp(g_j(s_{itj}; D_{itj}) + f_j(x_{itj}; D_{itj})) \quad \text{where} \quad D_{itj} = \begin{cases} L, & s_{itj} \leq \bar{s} \\ H, & s_{itj} > \bar{s} \end{cases}. \tag{14}$$

We define the threshold to be at ten years of schooling, $\bar{s} = 10$, which is the highest level for which we have a sufficient number of observations above and below the threshold in all countries (it is also approximately the average years of schooling in our set of countries). The aggregate experience human capital stock for a given schooling category is then again simply the average human capital across all individuals in that category, that is calculated as in equation (10). Figure 9c shows the height of the experience-earnings profile for workers with low schooling (less than ten years) plotted against the height for those with high schooling (more than ten years). We find that in some countries, experience profiles are flatter for workers with low educational attainment, but in many other countries they are steeper.[20]

Next, we conduct a similar counterfactual exercise as earlier and compute the implied experience human capital stocks if all countries had the same share of highly educated individuals as the United States. Figure 9d plots the counterfactual human capital stocks against the actual stocks. Many countries lie *below* the 45 degree line, suggesting that the cross-country gaps in the counterfactual human capital stocks are even larger than those in the actual human capital stocks. Panel (b) of Table 5 reports the fraction of cross-country dispersion in experience human capital stocks that is due to schooling composition effects. The number is negative. Thus, our results are not driven by differences in the educational composition of workers across countries.

Other Composition Effects Using the same basic approach as above, we have explored composition effects along other dimensions that may differ systematically between rich and poor countries: services versus non-services, manufacturing versus non-manufacturing, public- versus private-sector employment, male versus female, urban versus rural, and full- versus part-time employment. We

[20]For the United States, experience profiles are not substantially different by schooling level. This is consistent with Heckman et al. (2006) who show that a simple model of lifecycle earnings dynamics ("Mincer's accounting identity model") predicts that experience-earnings profiles are parallel across education groups, while age-earnings profiles fan out, and find that these hypotheses are supported in U.S. census data for white males.

also explored compositional effects for different combinations of these categories. None of these decompositions are important for explaining cross-country differences in the returns to experience. The fraction of cross-country dispersion in experience human capital explained by composition effects never exceeds thirteen percent. These results are not reported, for brevity, and are available upon request.

5 Why Are Experience-Earnings Profiles Flatter in Poor Countries?

In this section, we discuss possible explanations for flat experience-earnings profiles in poor countries. The discussion serves two purposes. First, we note that our finding is consistent with several bodies of existing theoretical work and discuss the implications of our findings in the context of these theories. Second, these theories provide a framework for considering the extent to which our interpretation that flat experience-earnings profiles reflect low human capital depend on the development accounting assumptions that we made. We consider two groups of possible explanations. The first includes different theories of human capital accumulation, and the second are alternative explanations in which factors other than human capital accumulation (e.g., long-term contracting) affect earnings dynamics. We argue that while the latter group of factors may affect the slope of experience-earnings profiles, only a few of them have the potential to generate the *cross-country* pattern that profiles are flatter in poor countries.

5.1 Theories of Human Capital Accumulation

TFP as a Determinant of Human Capital Accumulation Our empirical findings are consistent with the class of theories in which TFP and experience human capital accumulation are complementary, in the sense that an increase in TFP raises the returns to the accumulation of experience human capital. A recent example from this class of theories is a study by Manuelli and Seshadri (2010). Their framework is a Ben-Porath (1967) type model in which human capital accumulation requires both time and non-time inputs (i.e., goods inputs, for example books, computers, or buildings). Low TFP thus implies that the price of non-time inputs is high relative to the wage per unit of human capital. This, in turn, implies that individuals purchase fewer non-time inputs and accumulate less human capital, both in school and on the job. A symptom is flat experience-earnings profiles. This class of theories also makes clear that the main result of our development accounting exercise, that TFP explains a smaller fraction of cross-country income differences than previously thought, is only true in an accounting sense and does not imply that TFP is less important than human capital as the root cause of cross-country income difference. A similar argument is made by Erosa et al. (2010) whose thesis is that the accumulation of schooling human capital amplifies TFP differences across countries.

Other Determinants of Human Capital Accumulation Another potential cause of lower returns to experience in poor countries is that the higher prevalence of extractive institutions in poorer countries (emphasized by Acemoglu et al. (2001) among others) discourages workers from accumulating human capital for which the returns could be confiscated in one way or another. This logic is consistent with recent evidence that higher taxation of labor income in Europe can explain a substantial fraction of European-U.S. differences in wage inequality and lifecycle wage growth (Guvenen et al., 2011). Similarly, it could be allocative distortions in poor countries that reduce human capital accumulation over the lifecycle (Bhattacharya et al., 2012).

More broadly, workers in developing countries may simply have less opportunity to improve their skills over their lifetimes than their counterparts in rich countries. One class of theories in this vein emphasizes the importance of social interactions for learning (Lucas, 2009; Lucas and Moll, 2011; Perla and Tonetti, 2011). These theories posit that human capital is accumulated through social interactions with others such that an individual learns more when interacting with someone more knowledgeable than herself and more or better interactions lead to steeper age-earnings profiles. Within this framework, all determinants of the frequency or quality of such social interactions, such as the quality of communication technology, are therefore also potential determinants of cross-country differences in returns to experience.

Lower skill accumulation by workers could also be the result of worse management practices in developing countries, as documented by Bloom and Van Reenen (2007) and Bloom et al. (2010). More generally, the same factors which cause firms to grow less quickly over the lifecycle in poor countries (Hsieh and Klenow, 2011) may explain why workers' earnings grow less quickly. In this spirit, Seshadri and Roys (2012) propose a theory that can potentially explain both facts simultaneously: workers and managers accumulate human capital and a firm is a match of a manager and some workers. Human capital accumulation and matching interact and jointly determine the lifecycle of both firm size and workers' earnings.

Relationship to Development Accounting Assumptions Under the standard assumptions of development accounting, a worker's human capital is proportional to his wage (see equation (2)), and hence we could identify individual human capital stocks directly off individual wages. These assumptions are that workers earn their marginal products, that they supply their entire human capital to the labor market, and that human capital is valued in efficiency units. Some of the theories of human capital accumulation we just discussed also make these assumptions, but others do not. Examples of theories that satisfy the assumptions of development accounting are theories of on-the-job learning in which human capital is accumulated passively, for example through social interactions, or theories of learning-by-doing with spot labor markets.

Other theories do not satisfy these assumptions. Notably, Ben-Porath (1967) type models depart from the assumption that individuals supply their entire human capital to the market. Individuals instead use some of their time for human capital accumulation, for example in on-the-job training. Therefore, upward-sloping experience-earnings profiles may partly reflect a declining fraction of time devoted to training.[21] While such theories leave intact the *qualitative* result of our development accounting exercise (i.e., accounting for country-specific returns to experience increases the human-capital dispersion across countries), they would change the *quantitative* results (i.e., accounting for country-specific returns to experience increases the contribution of human and physical capital to cross-country income differences from less than half to almost two thirds). Put differently, these theories postulate a different *quantitative mapping* from experience-earnings profiles to human capital stocks than our development accounting exercise.[22] Another example of a theory that would change this quantitative mapping is a theory of learning-by-doing in which different employers offer different learning opportunities and potential employees find their employers through competitive search. Then, employees will pay firms for good learning opportunities by accepting starting wages

[21]In these models, individuals devote a fraction ℓ_{ict} of their time endowment to human capital accumulation and the remaining $1 - \ell_{ict}$ to working. The mapping from wages to human capital (2), therefore, has to be modified to include the fraction of time (and therefore human capital) supplied to the market: $w_{ict} = \omega_{ct} h_{ict} (1 - \ell_{ict}) \exp(\varepsilon_{ict})$. A rising w_{ict} may thus be due to a declining ℓ_{ict}.

[22]For example, Kuruscu (2006) argues that in a calibrated Ben-Porath model, relatively large fractions of time devoted to training have only small effects on lifetime income and human capital accumulation. Therefore, large cross-country differences in experience-earnings profiles may reflect smaller differences in human capital profiles (instead of the two being proportional as in our benchmark exercise). But the exact quantitative mapping would depend on (i) the exact type of Ben-Porath model used (for example, how the human capital production function depends on time- and non-time inputs), (ii) what exactly drives cross-country differences in time devoted to training (for example, whether these are caused by differences in human capital accumulation technologies or tax policies), and (iii) the exact functional forms and parameter values that are being used.

below their marginal product, thus violating one of the assumptions of development accounting.[23]

5.2 Alternative Explanations

There are also alternative theories that not only depart from the assumptions of development accounting outlined above, but also postulate that factors that are different from human capital accumulation affect the shape of experience-earnings profiles.

Long-Term Contracting If workers and firms form long-term contracts, like in the work of Lazear (1979), wages may not equal workers' marginal product of labor, thus violating one of the assumptions of development accounting. In the presence of moral hazard or commitment frictions on the part of workers, firms may "backload" wage payments to incentivize their employees. This results in a wage profile that is steeper than a worker's human capital profile. Similarly, Michelacci and Quadrini (2009) postulate that financially constrained firms that sign optimal long-term contracts with workers may implicitly borrow from their workers, thereby offering steeper wage profiles than in the frictionless case.

Since moral hazard and commitment frictions for workers and credit constraints for firms are likely to be more pronounced in poor countries, these theories would predict more backloading in poor countries.[24] They imply that true human capital profiles in poor countries are even flatter than what we observe, which means that our estimates understate the human capital dispersion between rich and poor countries, and thus understate the contribution of human capital to cross-country income differences.

There are also theories that suggest that there is more "front-loading" in poor countries. For example, studies such as Azariadis (1988) and Bernhardt and Timmis (1990) propose that long-term contracts can resolve financial constraints for workers. The firm can act as a "lender of last resort" for its workers, implicitly lending to them by offering a wage profile that is flatter than in the frictionless case. If workers in poor countries are more financially constrained, then these

[23] These two examples discuss departures from two development accounting assumptions, that workers supply their entire human capital to the labor market and that they earn their marginal products. Similarly, one could imagine departing from the third development accounting assumption that human capital is valued in efficiency units, i.e. that different skill types are perfect substitutes. We do not explore this possibility here and instead refer the reader to Jones (2011) who discusses this in great detail and argues that relaxing this assumption can substantially amplify the role of human capital in accounting for cross-country income differences (and even close the entire income gap between rich and poor countries if high and low skill types are sufficiently complementary).

[24] In a similar spirit, in a study comparing different provinces within Italy, Guiso et al. (2010) find that firms operating in less financially developed provinces offer steeper wage-tenure profiles.

models predict that the observed experience-earnings profiles for workers in poor countries are flatter than actual profiles (relative to rich countries). This would cause us to over-estimate the true cross-country human capital dispersion and the contribution of human capital for explaining cross-country income differences.

For assessing the likelihood that long-term contracts will cause our results to under- or over-state the contribution of human capital to cross-country income dispersion, it is important to note two facts. First, all of the theories we discuss above are actually about the returns to tenure (experience at a specific firm). For the United States, a recent estimate of median tenure is 4.6 years (Bureau of Labor Statistics, 2012). Therefore, if tenure in other countries is of similar or shorter duration, it is unlikely that front-loading in long-term contracts is the driver of flat experience-earnings profiles in poor countries and that this causes our estimates to over-state the true contribution of human capital.

Second, note that our development accounting exercise does not require us to correctly estimate individual human capital stocks. Rather, it relies on our correctly estimating *aggregate* human capital stocks, and it turns out that this is still possible even if wages do not equal marginal products of labor. In particular, as long as a worker's average wages over her lifecycle equal her average productivity, our estimation strategy identifies correct aggregate human capital stocks. The intuition is straight-forward. In such an environment, the area under the measured experience profile will be equal to the area under the profile that reflects the worker's true productivity.[25] In light of these two arguments, the presence of long-term contracts that do not pay wages equal to marginal products of labor does not play an obviously important role in driving our cross-country results.

Job Shopping and Job Seniority Two other potential determinants of earnings dynamics over the lifecycle are job shopping and job seniority. A large literature in labor economics using U.S. data has examined experience-earnings profiles and the extent to which they reflect human capital accumulation over the lifecycle as opposed to these other factors. Consistent with our focus on potential experience, studies such as Altonji et al. (2009) find that human capital accounts for most of the growth of earnings over a career and that job seniority and job mobility play decidedly smaller roles. While other studies such as Topel and Ward (1992) and Bagger et al. (2011) have argued that

[25]We prove this formally in Appendix B and illustrate our point in a numerical example.

the contribution of job search is larger than that postulated by Altonji et al. (2009), they all agree that human capital accumulation is the most important source of wage growth at least in the early phase of workers' careers, which is also the phase in which the cross-country differences in returns to experience that we document are most pronounced. These findings lead us to believe that it is unlikely that flat experience-earnings profiles in poor countries are entirely due to cross-country differences in job shopping or job seniority.

6 Conclusion

This paper uses newly available large-sample micro data from 36 countries to document a new fact: experience-earnings profiles are flatter in poor countries than rich countries. We show that the fact is robust to a range of sample restrictions and to controlling for time or cohort effects, to the extent that our data allow. When we apply this fact to a development accounting exercise by allowing the returns to experience to vary across countries, we find that the contribution of the observable factors of production to cross-country income differences rises from less than one-half as in previous studies (e.g., Caselli, 2005 and Hsieh and Klenow, 2010) to up to two-thirds. The intuition behind our finding is straightforward. By restricting returns to experience to be similar across countries, previous development accounting exercises understated the importance of cross-country differences in human capital from experience. In contrast, our exercise implies that international differences in human capital from experience are in fact substantial.

The results of this study suggests several interesting avenues of future research. First, it is obviously important to understand the determinants of cross-country experience-earnings profiles. For example, in a companion study, we investigate the extent to which experience-earnings profiles are determined by portable human capital that workers can take with them versus local institutions by comparing the experience-earnings profiles across immigrants from different countries in the United States to the profiles of their native countries (Lagakos et al., 2012). Another interesting avenue of research is to understand whether the slower lifecycle earnings growth for workers in poor countries is related to the slower lifecycle growth of firms, emphasized by Hsieh and Klenow (2011), and if so how.

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Appendix

A Data Appendix

The surveys we employ in our analysis are listed below for each country. All surveys are nationally representative unless noted. We attempted to obtain data for every country in the world with a population greater than one million people. We obtained a number of surveys from the Food and Agriculture Organization's (FAO) Rural Income Generating Activity (RIGA) database; these surveys are available here: www.fao.org/economic/riga/riga-database/en/. We obtained a number of other surveys through the Integrated Public Use Microdata Series (IPUMS) (Minnesota Population Center, 2011; King et al., 2010), which can be found here: www.ipums.org. The remaining surveys were made available to us by the statistical agencies of the countries in question or other sources, as listed below.

- Argentina: *Encuesta Permanente de Hogares,* 2003, 2007 and 2010, from the Instituto Nacional de Estadística y Censos; *representative of urban areas.*

- Australia: *Household Income and Labour Dynamics in Australia,* yearly from 2001 to 2009, from the Australian Department of Families, Housing, Community Services and Indigenous Affairs, available from the Cornell Department of Policy Analysis and Management.

- Bangladesh: *Household Income and Expenditure Survey,* 2000, from the Bangladesh Bureau of Statistics, available from the FAO RIGA database.

- Bolivia: *Encuesta de Hogares,* 2005, from the Bolivian Instituto Nacional de Estadística.

- Brazil: *Recenseamento Geral do Brasil, Censo Demográfico,* 1970 (5% sample), 1980 (5% sample), 1991 (5.8% sample), and 2000 (6% sample), from the Instituto Brasileiro de Geografia e Estatística (IBGE), available from IPUMS, and *Pesquisa Nacional por Amostra de Domicilios,* yearly from 2001 to 2010, from IBGE.

- Canada: *Census of Canada,* 1971 (1% Sample), 1981 (2% Sample), 1991 (3% Sample) and 2001 (2.7% Sample), available from IPUMS.

- Chile: *National Socioeconomic Characterization Survey (CASEN),* 2000 and 2009, from the Chilean Ministry of Planning and Cooperation.

- China: *Urban Household Surveys* (0.01% of urban households, 27 cities), year from 1989 to 2005; *representative of urban areas.*

- Colombia: *XIV National Population and III Housing Census* by Departmento Administrativo Nacional de Estadística (DANE), 1973 (10% of households), available from IPUMS.

- Ecuador: *Estudio de Condiciones de Vida,* 1995, from the Instituto Nacional de Estadística y Censos, available from the FAO RIGA database.

- Egypt: *Labor Market Panel Survey, 2006* from the Egyptian Central Agency for Public Mobilization and Statistics.

- France: *Enquete Emploi*, yearly from 1993 to 2001, from the Ministre de l'Économie de l'Industrie et de l'Emploi.

- Germany: *German Socioeconomic Panel (SOEP),* yearly from 1991 to 2009, from the German Institute for Economic Research (DIW Berlin).

- Guatemala: *Encuesta Nacional de Condiciones de Vida,* 2000 and 2006*,* from the Instituto Nacional de Estadistica.

- Honduras: *Encuesta Permanente de Hogares de Propósitos Multiples,* 2005, from the Secretaria de Trabajo y Seguridad Social.

- India: *Socio Economic Survey* by National Sample Survey Organization, 1993 (0.07% of households), 1999 (0.07% of households), 2004 (0.06% of households), available from IPUMS.

- Indonesia: *Family Life Survey,* 2000, from RAND, available from the FAO RIGA database; National Labour Force Survey (SAKERNAS), 2007, 2008, 2009, 2010, 2011, from the Indonesia Badan Pusat Statistik

- Italy: *Survey on Household Income and Wealth*, 1991, 1993, 1995, 1998, 2000, 2002, 2004, 2006, 2008, 2010, from the Bank of Italy.

- Jamaica: *Population Census*, 1982, 1991 and 2001, (10.0% samples) from the Statistical Institute of Jamaica, available from IPUMS.

- Mexico: *XI General Population and Housing Census*, 1990 (10% sample); *Population and Dwelling Count*, 1995 (0.4% of sample); *XII General Population and Housing Census*, 2000 (10.6% of sample), available from IPUMS.

- Netherlands: *DNB Household Survey,* yearly from 1994 to 2010, available from centERdata.

- Nicaragua: *Encuesta Nacional de Hogares sobre Medición de Nivel de Vida,* 1998 and 2001, from the Instituto Nacional de Estadística y Censos, available from the FAO RIGA database.

- Panama: *Censo Nacional de Población y de Vivienda de Panamá*, 1990 (10% sample), available from IPUMS, and the *Encuesta de Condiciones de Vida,* 2003, from the Dirección de Estadística y Censos de Panamá, available from the FAO RIGA database.

- Paraguay: *Encuesta Permanente de Hogares*, 2011, from the Direccion General de Estadistica.

- Peru: *Encuesta Nacional de Hogares,* 2004 and 2010, from the from the Instituto Nacional de Estadística y Informática.

- Puerto Rico: *Census of Population and Housing,* 1970 (1% Sample), 1980 (5% Sample), 1990 (5% Sample) , 2000 (5% Sample) ; *American Community Survey*, 2005 (1% Sample), available from IPUMS.

- Russia: *Russia Longitudinal Monitoring Survey,* yearly from 2000 to 2010, available from the Carolina Population Center at the University of North Carolina, Chapel Hill.

- South Africa: *Labor Force Survey,* 2000, 2001 and 2002 from Statistics South Africa.

- South Korea: *Korea Labor and Income Panel Study,* yearly from 1999 to 2008, from the Korea Labor Institute, available from the Cornell Department of Policy Analysis and Management.

- Switzerland: *Swiss Household Panel*, yearly from 1999 to 2009, from the Swiss Foundation for Research in Social Sciences, available from the Cornell Department of Policy Analysis and Management.

- Taiwan: *Survey of Family Income and Expenditure*, yearly from 1995 to 2003, available from the Research Program in Development Studies at Princeton University.

- Thailand: *Thailand Socioeconomic Survey,* 1990, 1992, 1994, 1996, 1998 and 1999, available from the Research Program in Development Studies at Princeton University.

- United Kingdom: *British Household Panel Survey,* yearly from 1992 to 2009, from the Institute for Social & Economic Research at the University of Essex.

- United States: *Census of Population and Housing,* 1960 (1% Sample), 1970 (1% Sample), 1980 (5% Sample), 1990 (5% Sample) , 2000 (5% Sample); *American Community Survey*, 2005 (1% Sample); *Current Population Survey,* yearly from 1980 to 2010; all available from IPUMS.

- Uruguay: *Extended National Survey of Households*, 2006, from the Uruguay National Institute of Statistics, available from IPUMS.

- Vietnam: *Living Standards Survey*, 1998 and *Household Living Standards Survey,* 2002, both from the General Statistics Office of Vietnam, available from the FAO RIGA.

All calculations in our analysis are weighted using the applicable sample weights for each survey. We express all earnings and wage data in local currency units of the most recent year in the data using the consumer price index of the country in question, taken from the IMF's International Financial Statistics database. In each survey we drop the top and bottom 1% of earners to remove potential outliers, and to minimize the impact of potential cross-country differences in top-coding procedures.

For most countries, we measure hours as the actual hours worked in the past week (or in some recent reference week.) For the United States, Brazil (the census data), Italy and Puerto Rico, we measure hours as the *usual* weekly hours worked (which is what is available). For China, India, Panama (the census data), Taiwan and Thailand, we have no hours data available, and impute hours as the average hours worked in all other countries for the individual's level of experience.

For most countries, labor earnings and hours worked are for both primary and secondary jobs. In Argentina, Chile, France, South Korea and Uruguay, labor earnings and hours worked are for just the primary job. For Brazil (the census data) and Switzerland, we measure labor income as the total income earned of individuals reporting to be primarily wage earners (as opposed to self employed.) In most countries, earnings are reported at the monthly frequency. The exceptions are Australia, Canada, Germany, Jamaica, South Korea, and the United States, in which earnings are measured at the annual frequency, and India, in which earnings are measured at the weekly frequency. In all surveys, earnings are before taxes.

The numbers for per capita GDP at PPP that we use in some of our calculations and figures are taken from the Penn World Tables (Heston et al., 2011).

B Identifying Aggregate Human Capital Stocks when Wages≠MPL

The purpose of this Appendix is to argue that even if individuals' wages do not reflect their marginal products, our methodology still correctly identifies *aggregate* human capital stocks under certain conditions. Assume that human capital is produced according to (1). Depart from the assumption that individuals are paid their marginal products in efficiency units of human capital and instead assume that an individual's wage is

$$w_{ict} = \omega_{ct} h_{ict} (1 + \tau_{ict}) \exp(\varepsilon_{ict}), \tag{15}$$

where τ_{ict} captures deviations from the wage equals marginal product assumption (in Section 3.1, $\tau_{ict} \equiv 0$). An identification problem arises if this transfer depends on schooling and/or experience. We therefore assume that

$$1 + \tau_{ict} = \exp(\tilde{\tau}(s_{ict}, x_{ict})). \tag{16}$$

The following Lemma shows that this departure from the assumption that wage equals marginal product is not an issue for *aggreggate* human capital estimates if the deviations satisfy

$$\frac{1}{T} \sum_{t=1}^{T} \frac{1}{C_t} \sum_{c=1}^{C_t} \frac{1}{N_{ct}} \sum_{i=1}^{N_{ct}} \tau_{ict} h_{ict} = 0, \qquad \tilde{\tau}(0,0) = 0. \tag{17}$$

This condition says that deviations take the form of zero-sum transfers across individuals and that no transfers are received by individuals with zero experience and schooling. If the distribution over age and schooling choices is stationary, (17) has the alternative interpretation of zero-sum transfers in expectation *over the lifecycle of a given individual.* This is because in a stationary environment the fraction of individuals with a given experience and schooling level is also the probability of an individual living up to the age where she attains that same experience and schooling levels.

Lemma: Assume wages equal marginal products *plus* transfers, $\tau_{ict} \neq 0$, but that these satisfy

(17). Then the procedure in Sections 3 and 4 produces biased estimates of *individual* human capital stocks, h_{ict}, but correct *aggregate* human capital stock estimates, H.

Proof: Combining equations (1), (15) and (16), the analogue of our estimating equation (4) is now

$$\log w_{ict} = \log \bar{\omega} + g(s_{ict}) + f(x_{ict}) + \tilde{\tau}(s_{ict}, x_{ict}) + \gamma_t + \psi_c + \varepsilon_{ict}$$

Our identification strategy still correctly identifies the intercept $\log \bar{\omega}$ because $\tilde{\tau}(0,0) = 0$. But we can no longer separately identify the functions g,f and $\tilde{\tau}$. The slope of the experience-earnings profile are therefore biased, and we obtain biased estimates of *individual* human capital stocks

$$\hat{h}_{ict} = \exp(g(s_{ict}) + f(x_{ict}) + \tilde{\tau}(s_{ict}, x_{ict})) = h_{ict}(1 + \tau_{ict}).$$

However, *aggregate* human capital stocks are still correctly identified:

$$\begin{aligned}\hat{H} &= \frac{1}{T}\sum_{t=1}^{T}\frac{1}{C_t}\sum_{c=1}^{C_t}\frac{1}{N_{ct}}\sum_{i=1}^{N_{ct}}\hat{h}_{ict} = \frac{1}{T}\sum_{t=1}^{T}\frac{1}{C_t}\sum_{c=1}^{C_t}\frac{1}{N_{ct}}\sum_{i=1}^{N_{ct}}h_{ict}(1+\tau_{ict}) \\ &= \frac{1}{T}\sum_{t=1}^{T}\frac{1}{C_t}\sum_{c=1}^{C_t}\frac{1}{N_{ct}}\sum_{i=1}^{N_{ct}}h_{ict} = H.\end{aligned}$$

where the second to last equality follows from (17).$\square$

Numerical Example. For simplicity abstract from cohort and year effects and set $\gamma_t = \psi_c = 0$. Let human capital production be given by (1) and the functions g and f by those we estimated for the United States in Section 3.2. Consider the example of long-term labor contracts and assume that transfers depend on experience in a linear-quadratic fashion, $\tau_i h_i = \beta_1 x_i + \beta_2 x_i^2$, and the coefficients β_1 and β_2 are such that (17) is satisfied.[26] Panel (a) of Figure A.5 plots two examples of such transfer functions. Panel (b) plots the implied experience-earnings profiles (dashed lines) and the underlying correct human capital profile for sake of comparison (solid line). If our empirical exercise estimated either of the two dashed experience-earnings profiles, we would still identify the correct *aggregate* human capital stock, that is the one corresponding to the solid experience-earnings

[26]We assume for simplicity that experience is distributed uniformly between 0 and $\bar{x} = 45$. Therefore, β_1 and β_2 satisfy

$$\int_0^{\bar{x}} [\beta_1 x + \beta_2 x^2]\frac{1}{\bar{x}}dx = 0 \quad \Leftrightarrow \quad \beta_1 + \beta_2\frac{2\bar{x}}{3} = 0.$$

profile.

C Age-Earning Profiles

Experience-earnings profiles are closely linked to age-earnings profiles. In the absence of cross-sectional variation in educational attainment, potential experience is a linear tranformation of age, and thus experience and age profiles are identical. Introducing differences in education across individuals breaks this equivalence, but the two profiles remains strongly tied. It is therefore natural to expect age profiles to show a cross-country pattern similar to the one we have documented for experience profiles. In this section we document that this conjecture is correct: age-experience profiles are steeper in richer coutries.

In order to calculate age-earnings profile we restrict the sample to include all individuals between 18 and 63 years old and estimate the following specification

$$\log w_{ict} = \alpha + \theta s_{ict} + \sum_{k=1}^{5} \eta_k \left(age_{ict} - 18\right)^k + \varepsilon_{ict}$$

The age-earnings profiles for our six large and representative countries are plotted in Figure A.6a, and the height of the profiles at age 38 for all countries is plotted in Figure A.6b. These figures show that age-earnings profiles are steeper in richer countries. The correlation between the height of the profile at age 38 and GDP is 0.69.

Table 1: Correlation between Height of Profiles and GDP per Capita under Alternative Sample Restrictions and Experience Measures

Sample Restriction/Experience Measure	corr(GDP,$height_{20}$)	var($height_{20}$)	max($height_{20}$)	min($height_{20}$)
Benchmark	0.60***	0.09	1.67	0.23
Male Workers	0.68***	0.13	1.82	0.26
Private Workers	0.58***	0.10	1.67	0.29
Full-time Workers	0.66***	0.12	1.87	0.27
Male Private Workers	0.63***	0.15	1.82	0.27
Male Private Full-time Workers	0.66***	0.18	1.96	0.32
Non-agricultural Workers	0.53***	0.09	1.67	0.23
Start Work at Age 6	0.45**	0.09	1.67	0.23
Start Work at Age 6 - Male	0.56***	0.11	1.77	0.26
Start Work at Age 6 - Male Private	0.50**	0.13	1.77	0.27
Start Work at Age 6 - Male Private Full-time	0.58***	0.16	1.93	0.32
Start Work at Age 15	0.65***	0.10	1.71	0.23
Hall and Jones Return to Schooling	0.63***	0.10	1.72	0.22
Constant 10% Return to Schooling	0.64***	0.09	1.64	0.28

Notes: *significant at 5% level, **significant at 1% level, ***significant at 0.1% level

Table 2: Dispersion of Aggregate Human Capital Stocks

	Dispersion Measure					
	Var(ln H)			90-10 Ratio		
Human Capital Measure	(a) Cross-sectional Results					
Schooling	0.130			1.87		
Experience	0.089			1.96		
Schooling + Experience	0.290			3.68		
	(b) Year and Cohort Controls					
	Year	Cohort	Cohort+GDP	Year	Cohort	Cohort+GDP
Schooling	0.108	0.118	0.105	1.93	1.83	1.90
Experience	0.108	0.083	0.111	1.98	1.75	1.98
Schooling + Experience	0.316	0.293	0.319	3.83	3.20	3.77

Table 3: Development Accounting

	Success Measure					
	$success_1$			$success_2$		
Human Capital Measure	(a) Cross-sectional Results					
Schooling	0.39			0.44		
Experience	0.41			0.46		
Schooling + Experience	0.63			0.70		
	(b) Year and Cohort Controls					
	Year	Cohort	Cohort+GDP	Year	Cohort	Cohort+GDP
Schooling	0.34	0.34	0.33	0.45	0.44	0.45
Experience	0.40	0.36	0.39	0.46	0.42	0.46
Schooling + Experience	0.59	0.55	0.57	0.72	0.63	0.71

Table 4: Relation to Literature

	Dispersion Measure		Success Measure	
Human Capital Measure	Var(ln H)	90-10 Ratio	*success$_1$*	*success$_2$*
	(a) Klenow and Rodriguez-Clare			
Schooling	0.060	1.74	0.39	0.42
Experience	0.002	1.04	0.23	0.30
Schooling + Experience	0.071	1.83	0.41	0.44
	(b) Hall-Jones Schooling + Klenow and Rodriguez-Clare Experience			
Schooling	0.049	1.63	0.37	0.41
Experience	0.002	1.04	0.23	0.30
Schooling + Experience	0.059	1.72	0.39	0.42
	(c) Hall-Jones Schooling + Country-Specific Quadratic Returns to Exp			
Schooling	0.049	1.63	0.37	0.41
Experience	0.034	1.54	0.30	0.39
Schooling + Experience	0.117	2.54	0.48	0.54
	(d) Hall-Jones Schooling + Country-Specific Quintic Returns to Exp			
Schooling	0.049	1.63	0.37	0.41
Experience	0.098	1.95	0.42	0.46
Schooling + Experience	0.216	3.22	0.62	0.64
	(e) Country-Specific Returns to Schooling (Linear) and Exp (Quintic)			
Schooling	0.133	1.88	0.40	0.44
Experience	0.092	1.98	0.41	0.46
Schooling + Experience	0.297	3.72	0.64	0.70

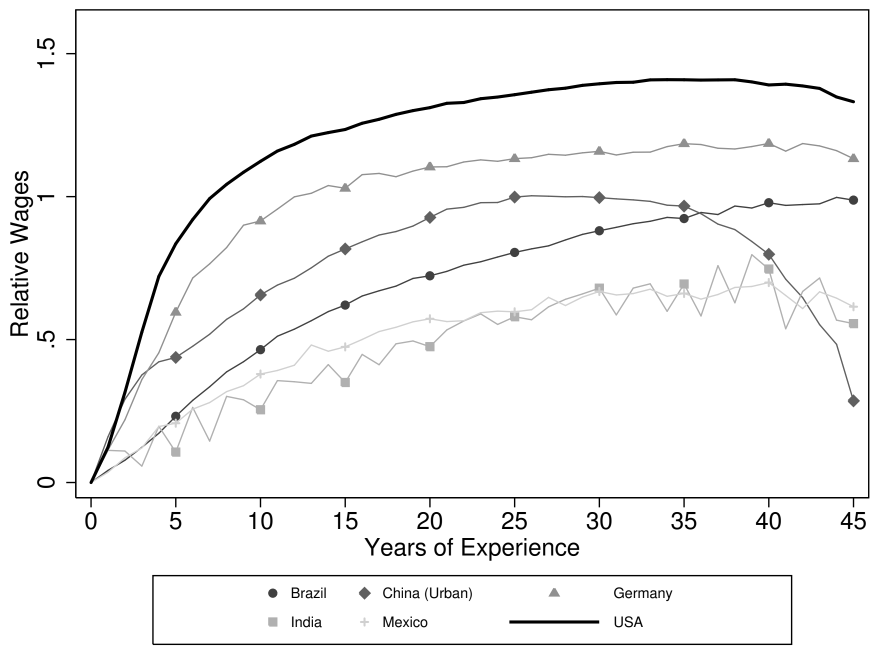
Notes: Panel (a) computes human capital stocks using a linear-quadratic Mincer specification and using the *average* returns to schooling and experience as in Klenow and Rodriguez-Clare (1997). In our sample of 28 countries the average coefficients on schooling, experience and experience^2 are 0.09211, 0.04775 and -0.000758 which is similar to those in Klenow and Rodriguez Clare (1997). Panel (b) uses the same specification except for imposing *diminishing returns to schooling* using the methodology of Hall and Jones (1999), namely assuming that g(s) is piecewise linear with slope 0.13 for s<=4, 0.10 for 4<s<=8 and 0.07 for 8<s. This is also similar to Bils and Klenow (2000). Panel (c) allows returns to experience to vary across countries, but retains a quadratic specification whereas Panel (d) uses a quintic specification. Panel (e) additionally allows returns to *schooling* to vary (linearly) across countries.

Table 5: Importance of Composition Effects

	Var(ln H^X)
(a)Counterfactual: U.S. Employment Share in Agriculture	
Data	0.102
Counterfactual	0.089
Fraction due to Composition Effect	0.13
Fraction due to Within-Sector Diffs	0.87
(b) Counterfactual: U.S. Share of Workers with Low Schooling	
Data	0.096
Counterfactual	0.100
Fraction due to Composition Effect	-0.05
Fraction due to Within-Sector Diffs	1.05

Figure 1: Fully Flexible Experience-Earnings Profiles, Cross-Sectional Estimates

(a) Experience-Earnings Profiles for Select Countries



(b) Height of Profiles at 20 Years of Experience versus Income

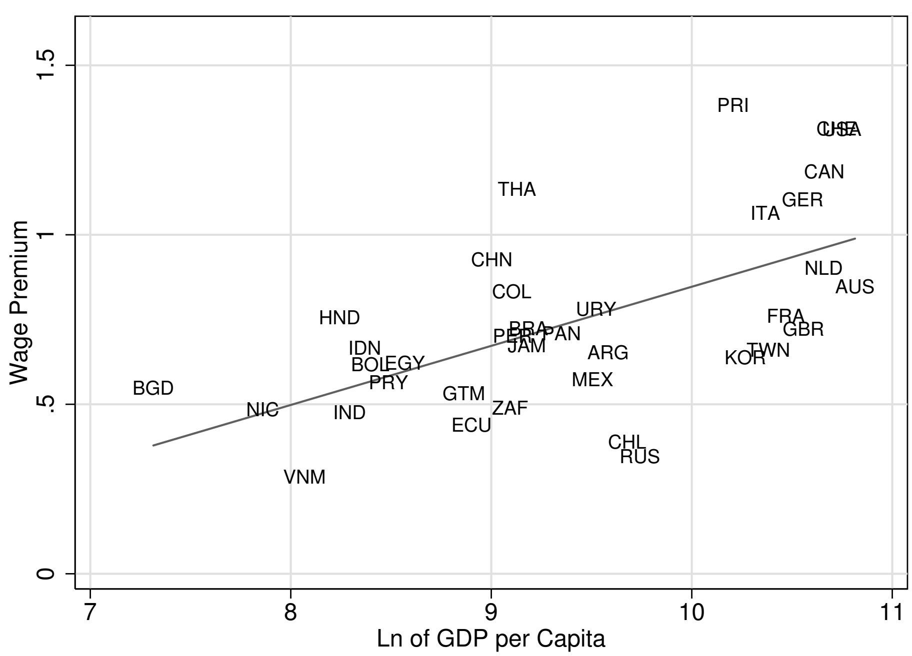
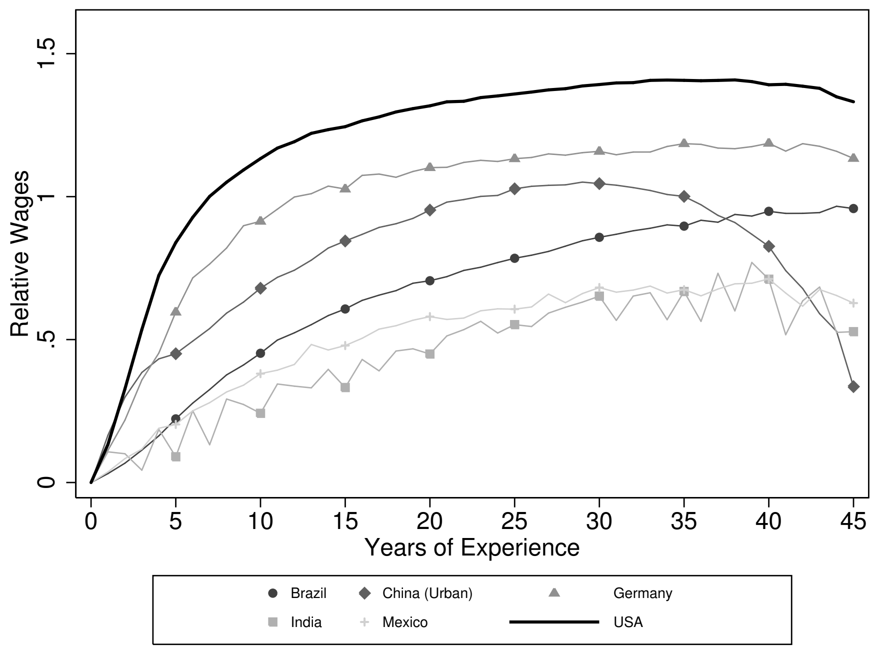


Figure 2: Fully Flexible Experience-Earnings Profiles, Estimates with Time Effects

(a) Experience-Earnings Profiles for Select Countries



(b) Height of Profiles at 20 Years of Experience versus Income

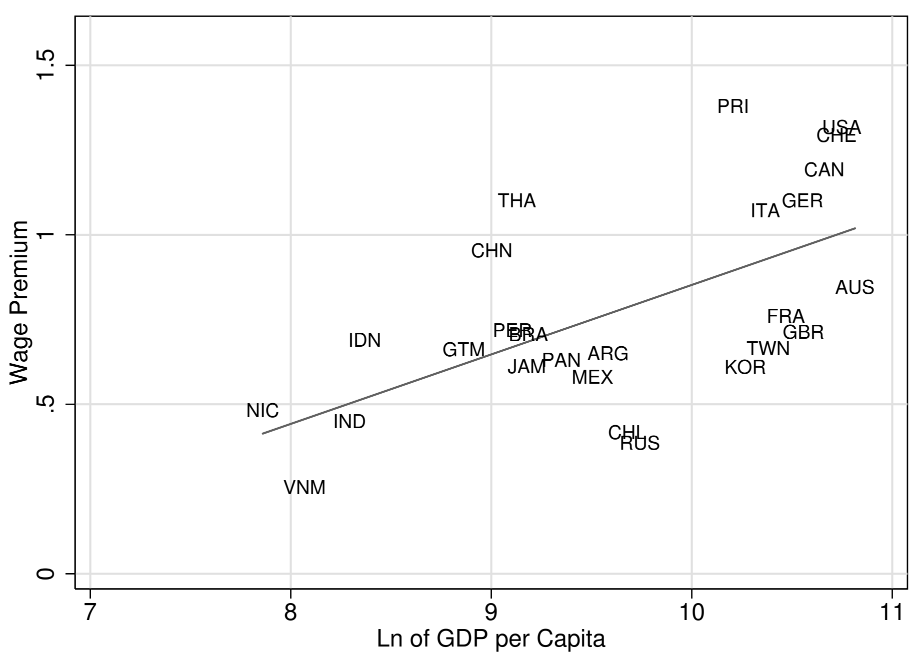
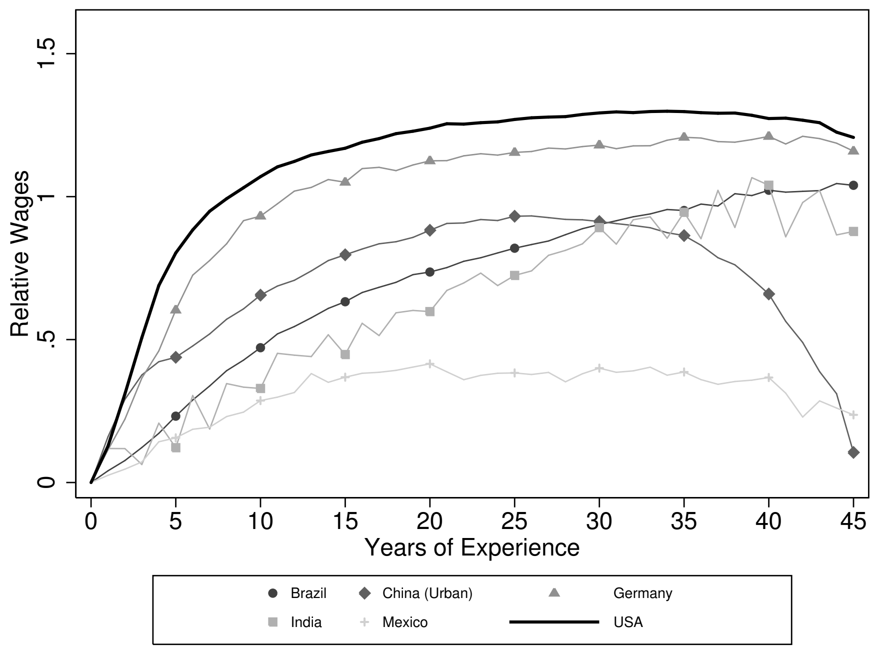


Figure 3: Fully Flexible Experience-Earnings Profiles, Estimates with Cohort Effects

(a) Experience-Earnings Profiles for Select Countries



(b) Height of Profiles at 20 Years of Experience versus Income

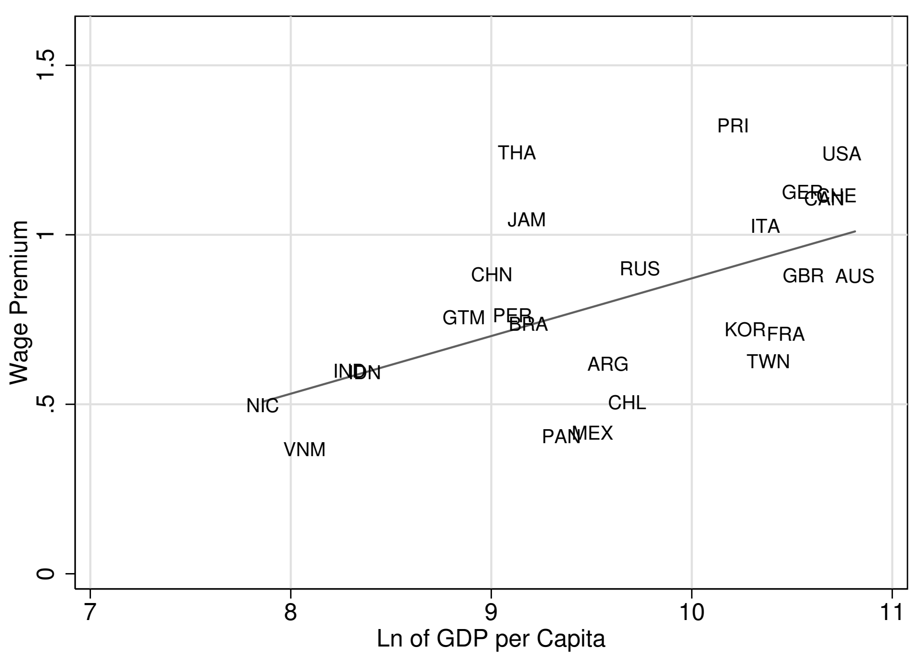
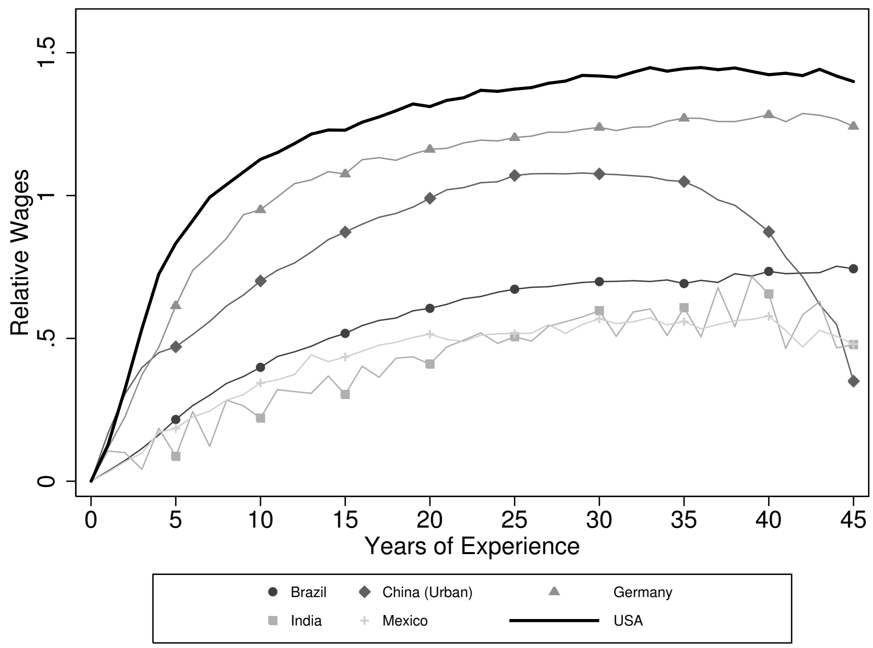


Figure 4: Fully Flexible Experience-Earnings Profiles, Estimates with Cohort Effects and Controlling for GDP

(a) Experience-Earnings Profiles for Select Countries



(b) Height of Profiles at 20 Years of Experience versus Income

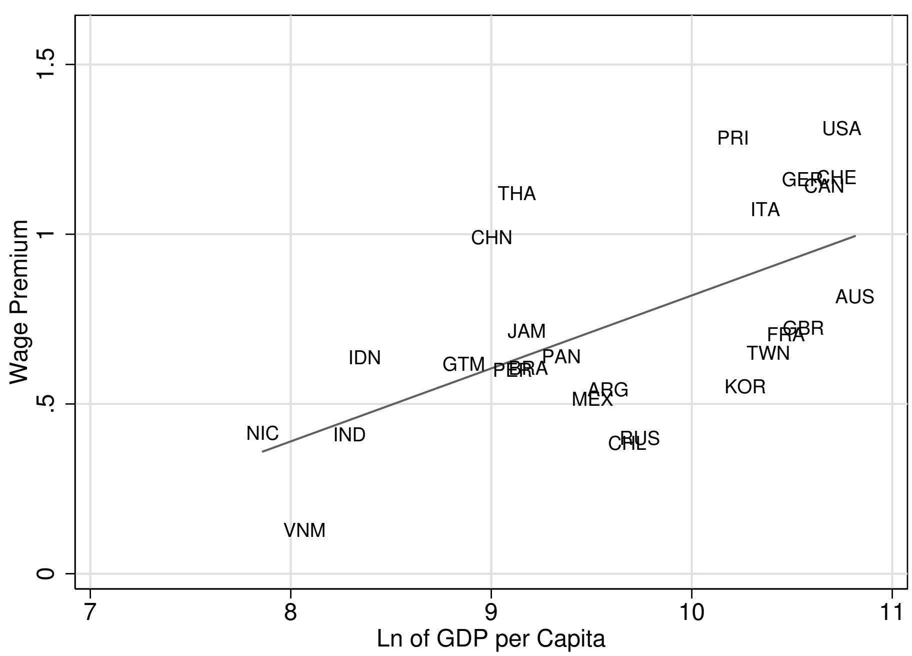
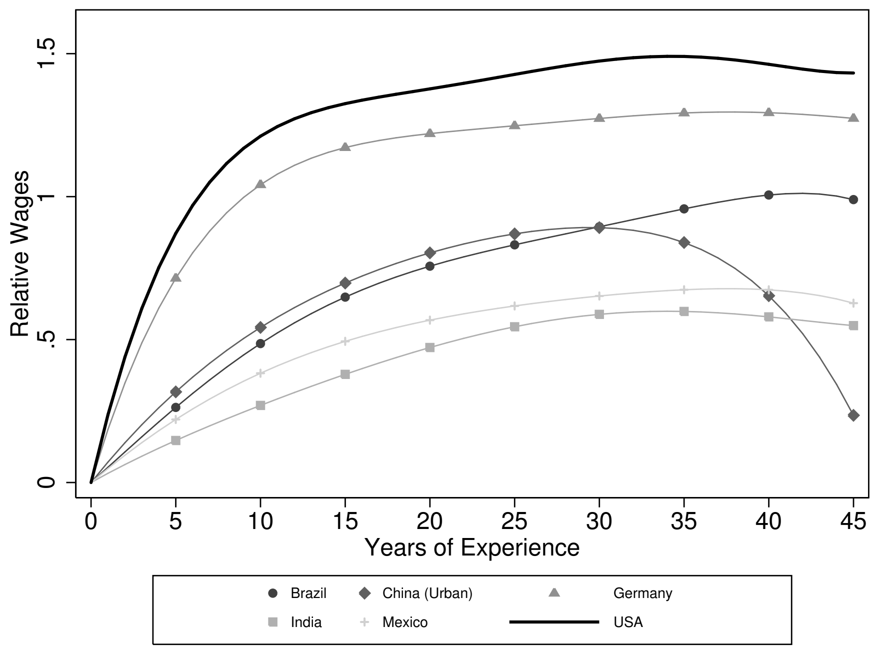


Figure 5: Quintic Experience-Earnings Profiles, Cross-sectional Estimates

(a) Experience-Earnings Profiles for Select Countries



(b) Height of Profiles at 20 Years of Experience versus Income

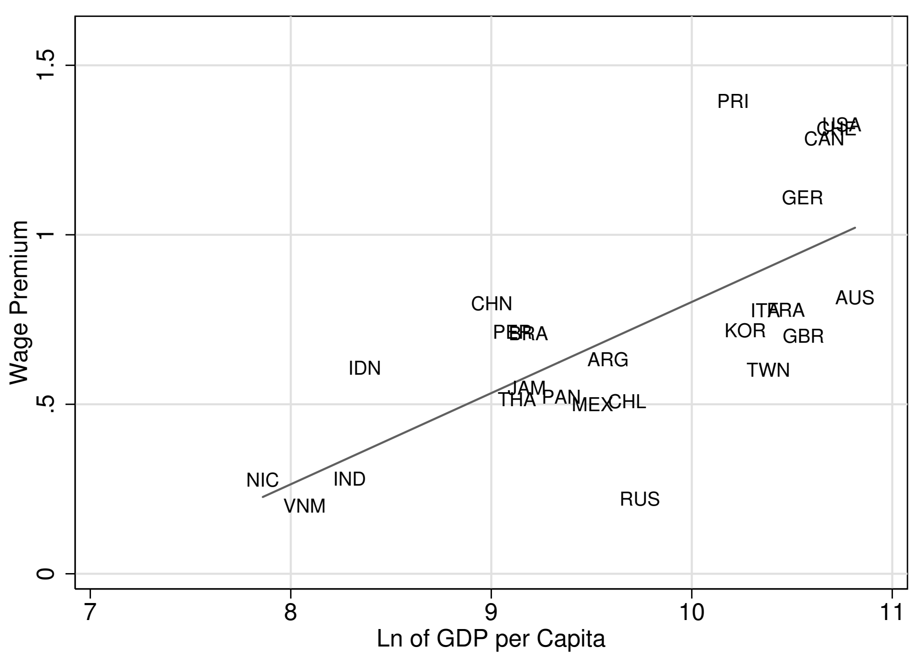
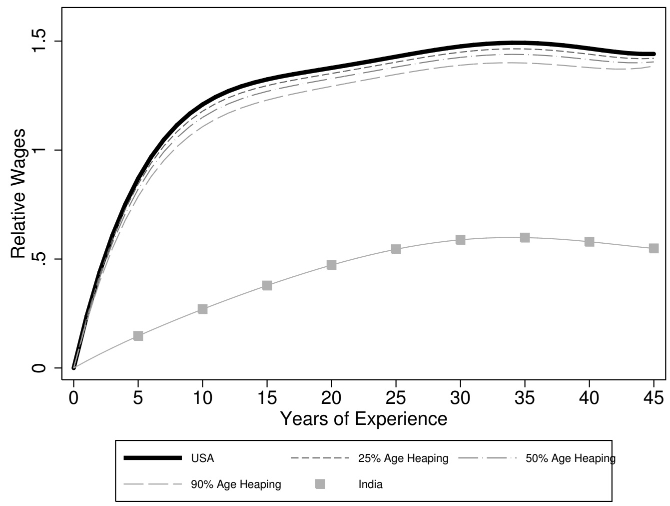
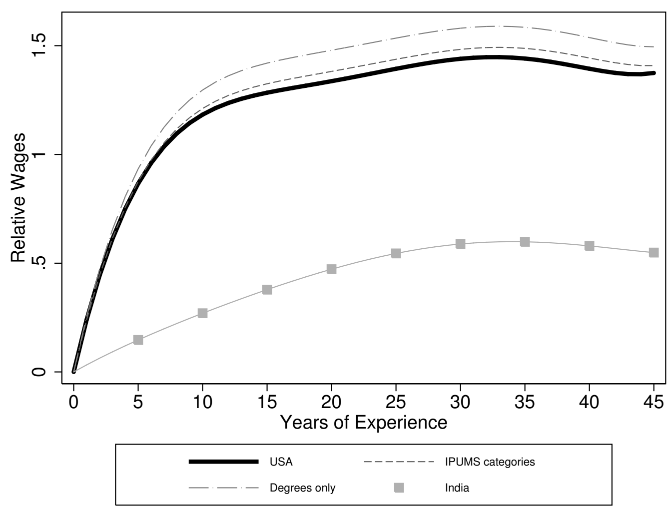


Figure 6: Returns to Experience – Robustness to Measurement Differences

(a) Adjusted for Age Heaping



(b) Adjusted for Differences in Education Reporting



(c) U.S. Census and CPS Data

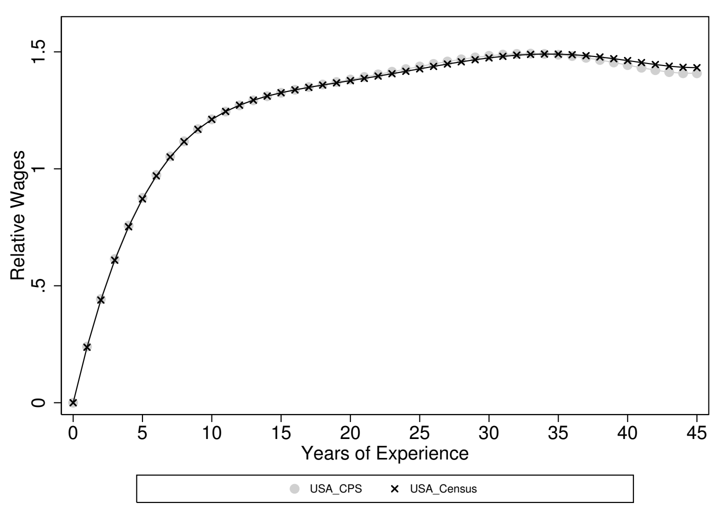
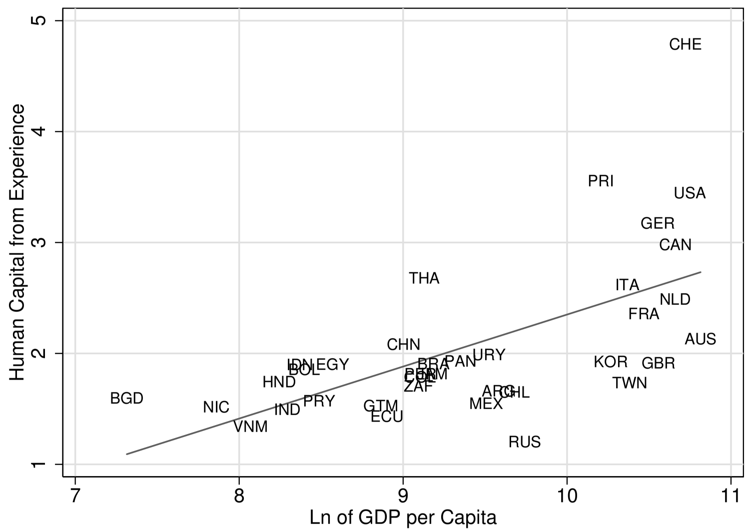
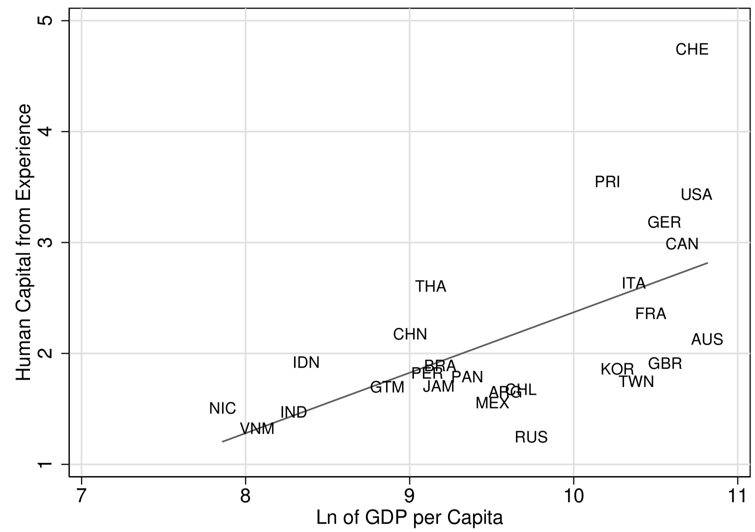


Figure 7: Implied Human Capital from Experience versus Income

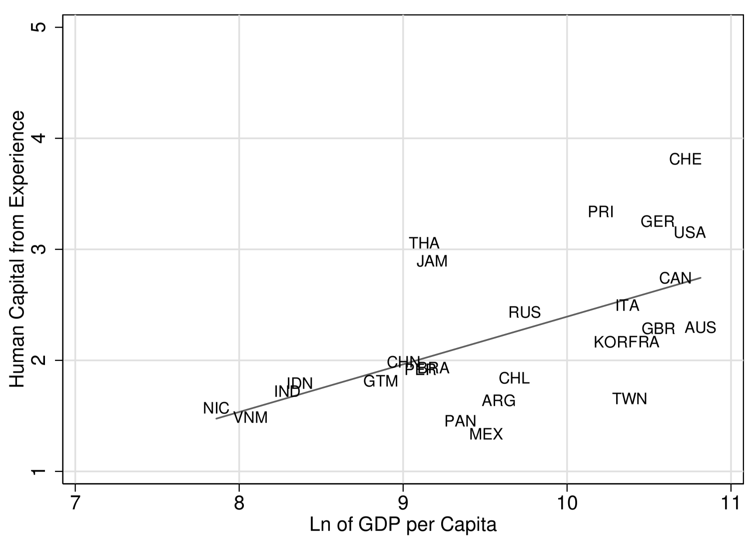
(a) From Quintic Estimate



(b) From Quintic Estimates with Time Effects



(c) From Quintic Estimates with Cohort Effects



(d) From Quintic Estimates with Cohort Effects and Controlling for GDP

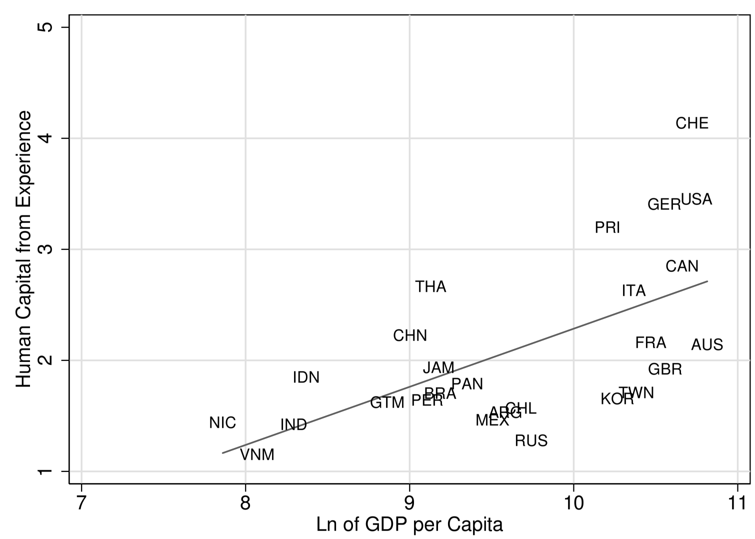
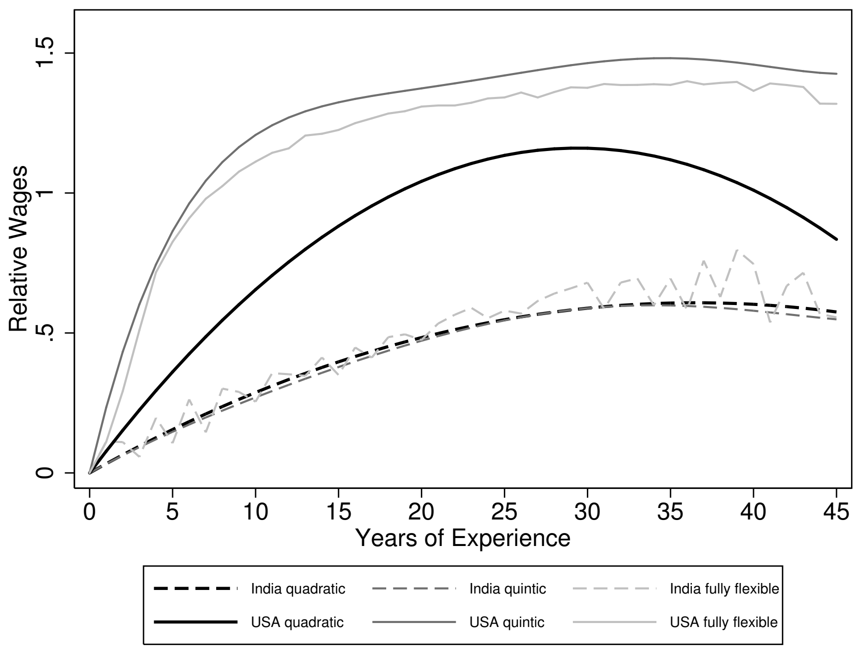


Figure 8: Quadratic Experience-Earnings Profiles, Cross-Sectional Estimates

(a) Experience-Earnings Profiles for Select Countries



(b) Height of Profiles at 20 Years of Experience versus Income

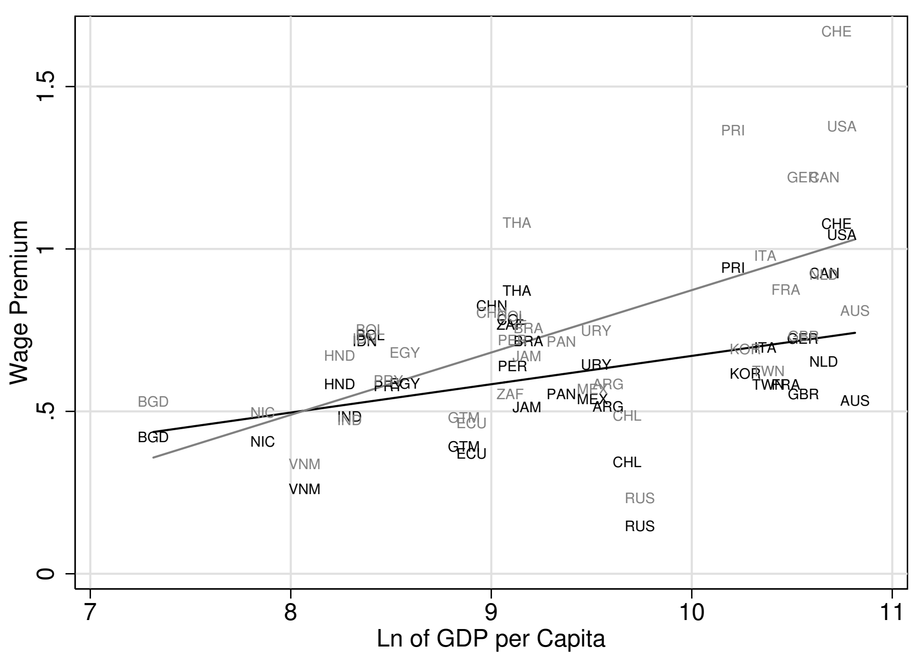
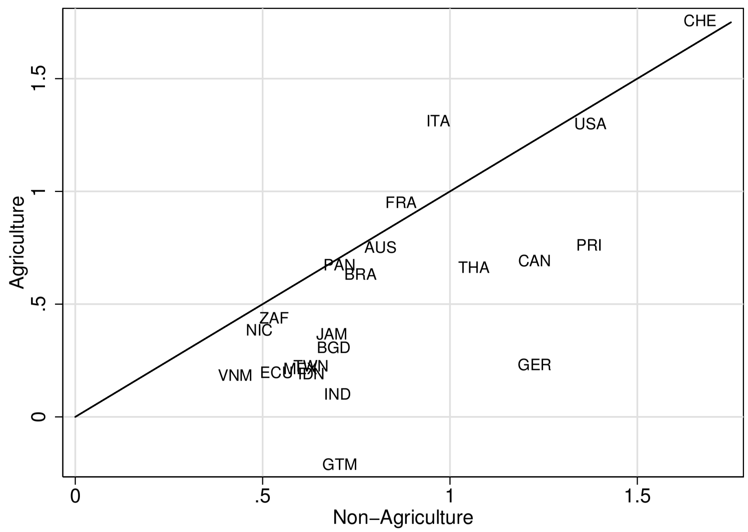
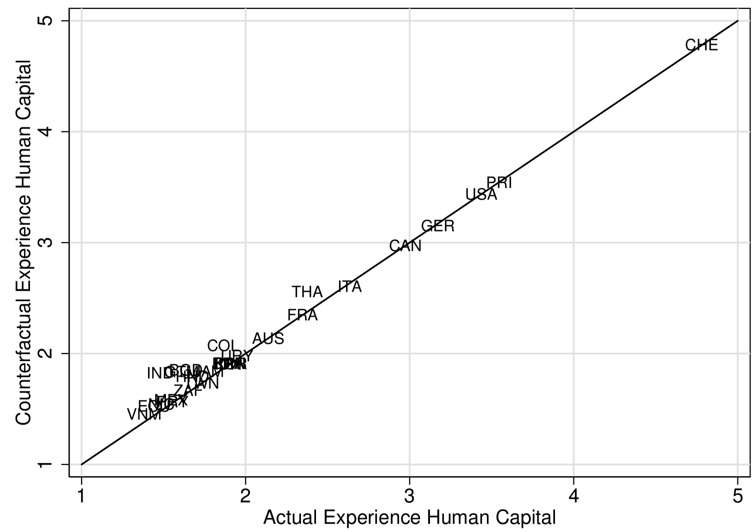


Figure 9: Worker Composition

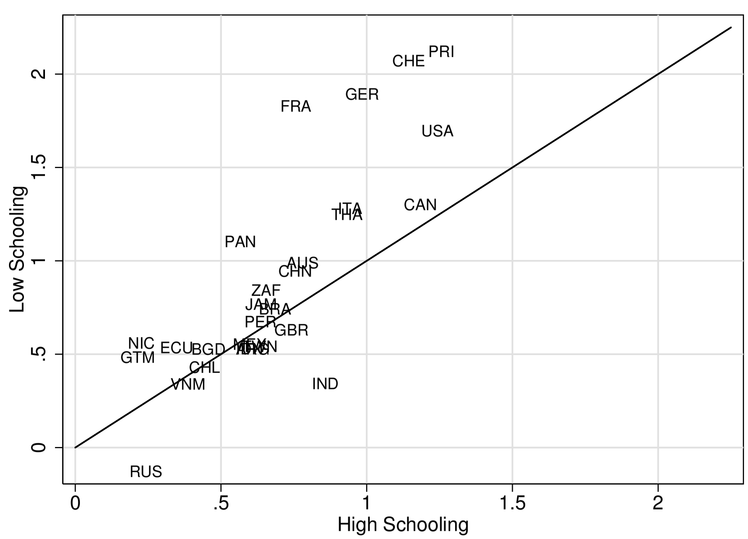
(a) Height of Profiles at 20 Years of Experience: Agriculture vs. Non-Agriculture



(b) Counterfactual: U.S. Employment Share in Agricultu



(c) Height of Profiles at 20 Years of Experience: High versus Low Schooling



(d) Counterfactual: U.S. Share of Workers with Low Schooling

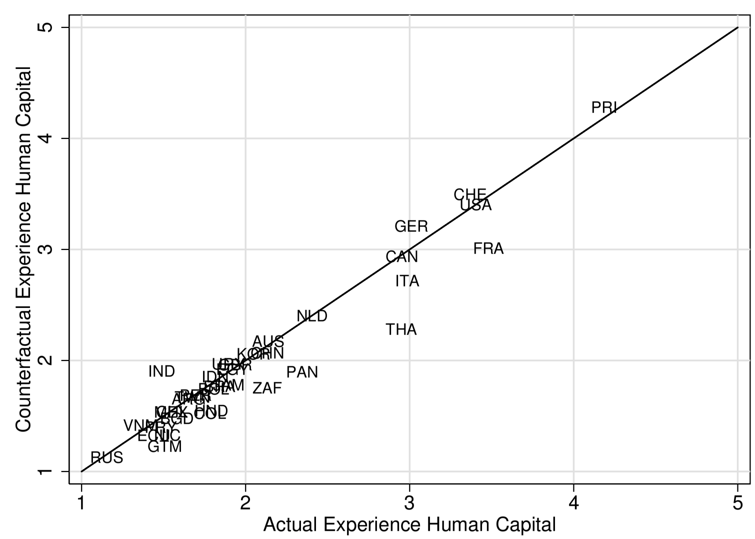


Table A.1: Robustness to Dropping Countries for which Cohort Effects May be Important

Drop Five Countries with Largest Change in:	corr(GDP,$height_{20}$)	var($height_{20}$)	max($height_{20}$)	min($height_{20}$)
No Countries Dropped (Benchmark)	0.60***	0.09	1.67	0.23
Total School Years	0.59***	0.09	1.67	0.23
Male School Years	0.59***	0.09	1.67	0.23
Female School Years	0.60***	0.09	1.67	0.23
Life Expectancy at Birth	0.59***	0.09	1.67	0.23
Infant Mortality	0.60***	0.09	1.67	0.23
Per Capita GDP Growth	0.67***	0.10	1.67	0.23
Gini Coefficient	0.62***	0.10	1.67	0.23

Notes: *significant at 5% level, **significant at 1% level, ***significant at 0.1% level

Table A.2: Aggregate Human Capital from Experience

	Fully Flexible Specification			Quintic Specification			
	Cross-Section	Time Controls	Cohort Controls	Cross-Section	Time Controls	Cohort Controls	Cohort+GDP
Country	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Experience Human Capital Relative to U.S.						
Argentina	0.56	0.56	0.60	0.48	0.48	0.52	0.44
Australia	0.69	0.69	0.79	0.62	0.62	0.73	0.62
Bangladesh	0.54			0.46			
Bolivia	0.51			0.54			
Brazil	0.58	0.57	0.64	0.55	0.55	0.61	0.49
Canada	0.90	0.90	0.90	0.86	0.87	0.87	0.83
Chile	0.49	0.50	0.60	0.48	0.49	0.58	0.45
China	0.73	0.75	0.75	0.60	0.63	0.63	0.64
Colombia	0.54			0.52			
Ecuador	0.44			0.42			
Egypt	0.59			0.55			
France	0.66	0.65	0.67	0.68	0.69	0.69	0.63
Germany	0.88	0.88	0.97	0.92	0.93	1.03	0.99
Guatemala	0.49	0.55	0.67	0.44	0.49	0.58	0.47
Honduras	0.53			0.51			
India	0.48	0.47	0.60	0.43	0.43	0.55	0.41
Indonesia	0.58	0.58	0.58	0.55	0.56	0.57	0.54
Italy	0.90	0.91	0.93	0.76	0.77	0.79	0.76
Jamaica	0.57	0.53	0.93	0.53	0.50	0.92	0.56
Mexico	0.48	0.48	0.45	0.45	0.45	0.42	0.42
Netherlands	0.77			0.72			
Nicaragua	0.44	0.44	0.50	0.44	0.44	0.50	0.42
Panama	0.60	0.55	0.48	0.56	0.52	0.46	0.52
Paraguay	0.52			0.46			
Peru	0.57	0.57	0.65	0.53	0.53	0.61	0.48
Puerto Rico	1.13	1.12	1.15	1.03	1.03	1.06	0.93
Korea, Rep.	0.59	0.56	0.71	0.56	0.54	0.69	0.48
Russian Federation	0.39	0.40	0.83	0.35	0.36	0.77	0.37
SouthAfrica	0.49			0.49			
Switzerland	1.08	1.05	0.96	1.39	1.38	1.21	1.20
Taiwan	0.56	0.56	0.58	0.50	0.51	0.52	0.49
Thailand	0.85	0.82	1.04	0.78	0.76	0.97	0.77
United Kingdom	0.59	0.59	0.76	0.55	0.56	0.73	0.56
Uruguay	0.64			0.58			
Vietnam	0.41	0.40	0.49	0.39	0.39	0.47	0.33

Notes: The aggregate human capital estimates in columns (1)-(3) are based on returns to experience estimates from fully flexible specifications (e.g., years of experience dummies). Those in columns (4)-(7) are based on a quintic specification. In columns (2) and (5), we include controls for calendar year dummy variables. In columns (3) and (6), we include categorical controls for birth cohorts (e.g., dummy variables indicating twenty-year intervals for birth year). In column (7) we include birth cohort dummies and the logarithm of per capita GDP.

Table A.3: Aggregate Total Human Capital

	Total Human Capital (Schooling + Experience)				
	No Experience	Cross-Section	Time Controls	Cohort Controls	Cohort+GDP
Country	(1)	(2)	(3)	(4)	(5)
	Human Capital Relative to U.S.				
Argentina	0.84	0.40	0.37	0.42	0.33
Australia	0.83	0.51	0.47	0.62	0.48
Bangladesh	0.38	0.18			
Bolivia	1.11	0.59			
Brazil	0.95	0.51	0.47	0.56	0.41
Canada	0.85	0.74	0.71	0.71	0.66
Chile	0.94	0.44	0.41	0.54	0.37
China	0.78	0.47	0.48	0.47	0.49
Colombia	0.69	0.35			
Ecuador	0.52	0.22			
Egypt	0.52	0.28			
France	0.68	0.46	0.43	0.44	0.39
Germany	0.79	0.73	0.69	0.81	0.76
Guatemala	0.72	0.32	0.30	0.40	0.29
Honduras	0.95	0.47			
India	0.61	0.26	0.24	0.32	0.23
Indonesia	0.88	0.48	0.45	0.47	0.42
Italy	0.66	0.50	0.48	0.50	0.47
Jamaica	0.89	0.46	0.31	0.74	0.38
Mexico	0.71	0.32	0.30	0.29	0.28
Netherlands	1.02	0.73			
Nicaragua	0.46	0.20	0.19	0.22	0.18
Panama	0.72	0.40	0.40	0.33	0.39
Paraguay	1.19	0.53			
Peru	1.30	0.68	0.62	0.79	0.53
Puerto Rico	1.24	1.26	1.19	1.27	1.03
Korea, Rep.	0.94	0.51	0.43	0.63	0.37
Russian Federation	0.87	0.30	0.26	0.92	0.27
SouthAfrica	1.48	0.69			
Switzerland	1.03	1.44	1.35	1.14	1.13
Taiwan	0.92	0.45	0.43	0.45	0.42
Thailand	2.01	1.43	1.30	1.81	1.31
United Kingdom	1.34	0.74	0.65	1.02	0.64
Uruguay	0.53	0.30			
Vietnam	0.40	0.16	0.14	0.19	0.12

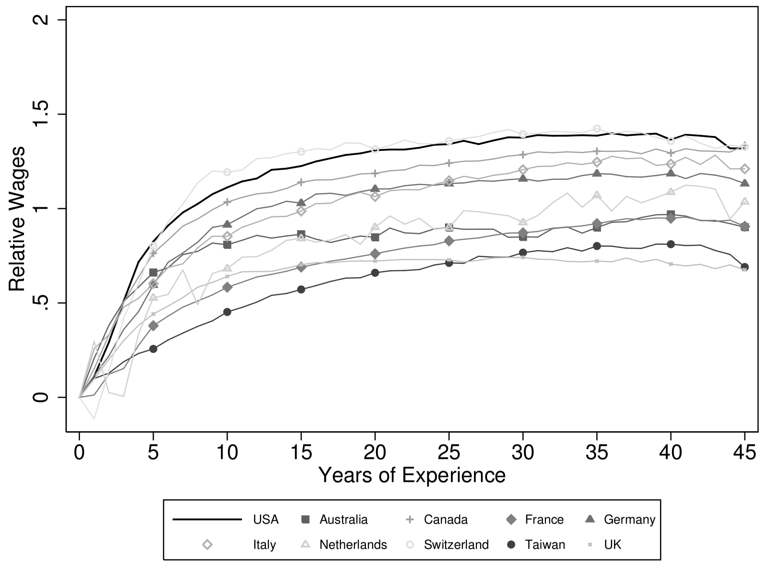
Notes: The aggregate human capital estimates in columns (1)-(5) are based on a quintic specification. In columns (3), we include controls for calendar year dummy variables. In columns (4), we include categorical controls for birth cohorts (e.g., dummy variables indicating twenty-year intervals for birth year). In columns (5) we include birth cohort dummies and the logarithm of per capita GDP.

Table A.4: Development Accounting

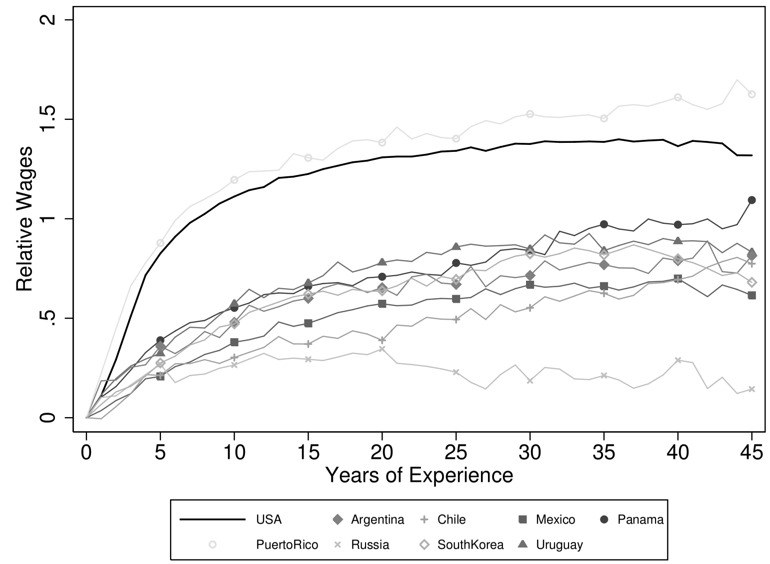
	Data from Caselli (2005)		Country-specific $success_2^j$ relative to U.S.				
	Y	K	No Experience	Cross-Section	Time Controls	Cohort Controls	Cohort+GDP
Country	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Argentina	0.45	0.39	0.69	1.13	1.19	1.09	1.28
Australia	0.81	0.95	0.94	1.29	1.36	1.14	1.35
Bangladesh	0.11	0.05	0.57	0.95			
Bolivia	0.12	0.06	0.28	0.44			
Brazil	0.33	0.31	0.50	0.76	0.81	0.72	0.88
Canada	0.79	0.98	0.89	0.98	1.00	1.00	1.05
Chile	0.41	0.29	0.64	1.05	1.11	0.93	1.18
China	0.09	0.06	0.26	0.37	0.36	0.37	0.35
Colombia	0.21	0.12	0.55	0.86			
Ecuador	0.22	0.20	0.58	1.05			
Egypt	0.22	0.06	0.86	1.29			
France	0.79	1.08	1.00	1.29	1.35	1.32	1.45
Germany							
Guatemala	0.23	0.09	0.66	1.13	1.16	0.97	1.20
Honduras	0.12	0.08	0.29	0.46			
India	0.09	0.04	0.37	0.65	0.69	0.56	0.71
Indonesia	0.17	0.11	0.39	0.58	0.60	0.59	0.63
Italy	0.89	1.11	1.14	1.38	1.41	1.37	1.43
Jamaica	0.13	0.14	0.28	0.43	0.56	0.31	0.49
Mexico	0.37	0.35	0.66	1.14	1.17	1.22	1.24
Netherlands	0.80	0.98	0.80	1.00			
Nicaragua	0.10	0.08	0.39	0.68	0.71	0.63	0.74
Panama	0.27	0.25	0.53	0.78	0.79	0.89	0.80
Paraguay	0.21	0.11	0.39	0.67			
Peru	0.18	0.18	0.26	0.41	0.43	0.37	0.48
Puerto Rico							
Korea, Rep.	0.60	0.78	0.68	1.02	1.14	0.89	1.26
Russian Federation							
SouthAfrica							
Switzerland	0.77	1.27	0.70	0.56	0.58	0.65	0.66
Taiwan	0.62	0.44	0.87	1.39	1.43	1.39	1.47
Thailand	0.23	0.30	0.22	0.28	0.29	0.24	0.29
United Kingdom	0.71	0.70	0.66	0.97	1.07	0.79	1.07
Uruguay	0.36	0.24	0.90	1.30			
Vietnam							

Notes: Columns (1) and (2) report GDP per worker and physical capital stocks from Caselli (2005), that we use as inputs into our accounting exercise. Columns (3)-(7) report $success_2^j$ which is the fraction of a given country's income gap to the U.S. that can be explained by physical and human capital. In column (5), we include controls for calendar year dummy variables. In columns (6), we include categorical controls for birth cohorts (e.g., dummy variables indicating twenty-year intervals for birth year). In column (7) we include birth cohort dummies and the logarithm of per capita GDP.

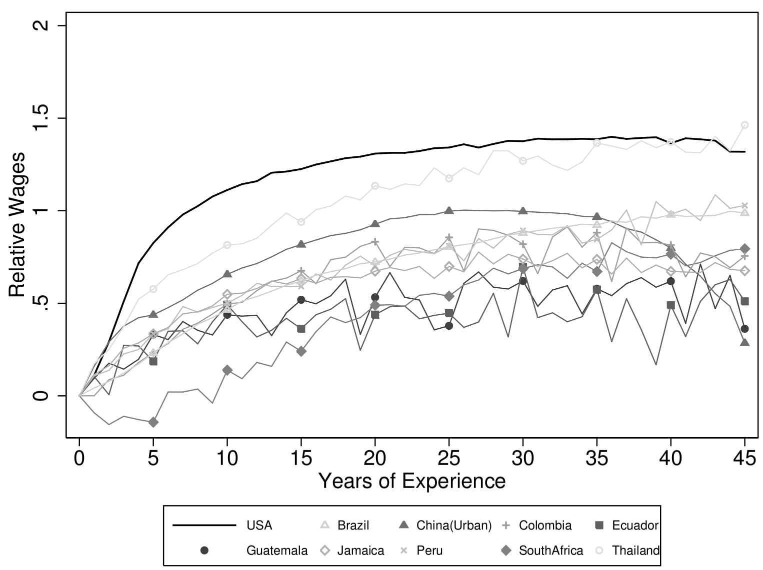
Figure A.1: Fully Flexible Experience-Earnings Profiles by Income Quartile (Relative to the USA)



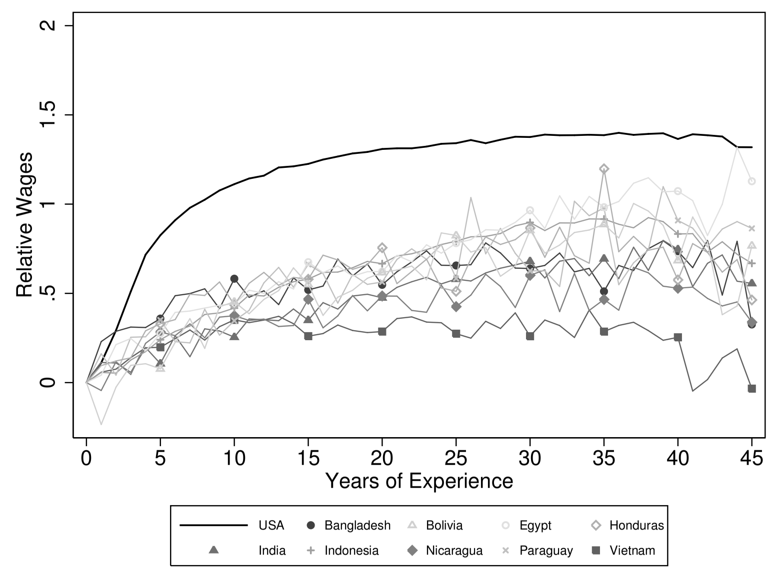
(a) Top Quartile



(b) Second Quartile

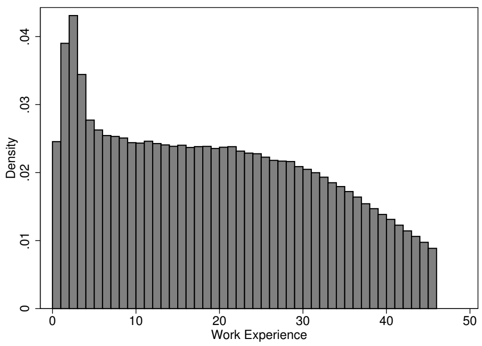


(c) Third Quartile

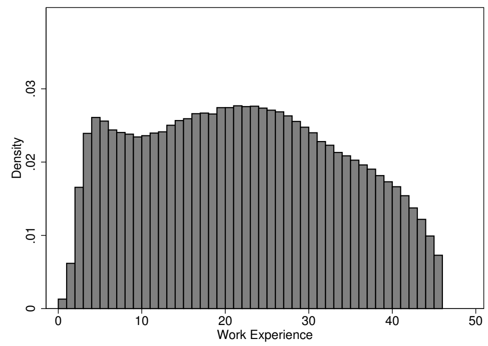


(d) Poorest Quartile

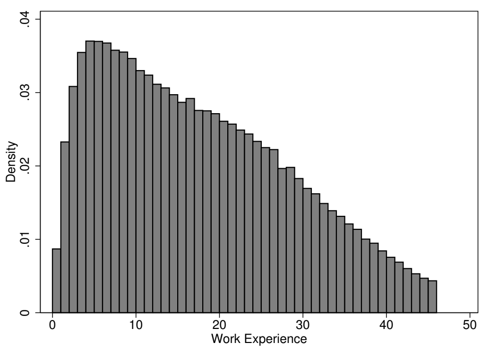
Figure A.2: Experience Histograms (for Six Select Economies)



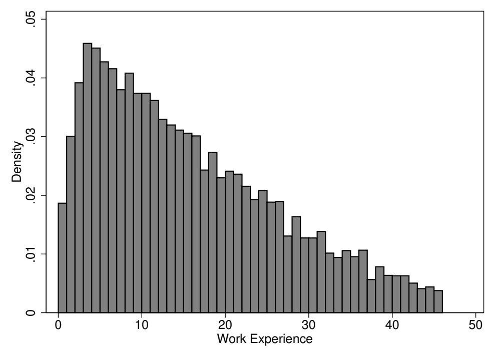
(a) USA



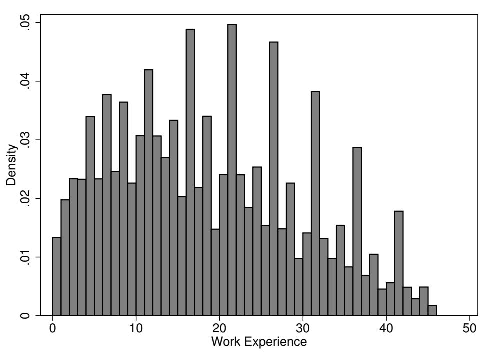
(b) Germany



(c) Brazil



(d) Mexico



(e) India

(f) China

Figure A.3: Average Experience versus Income

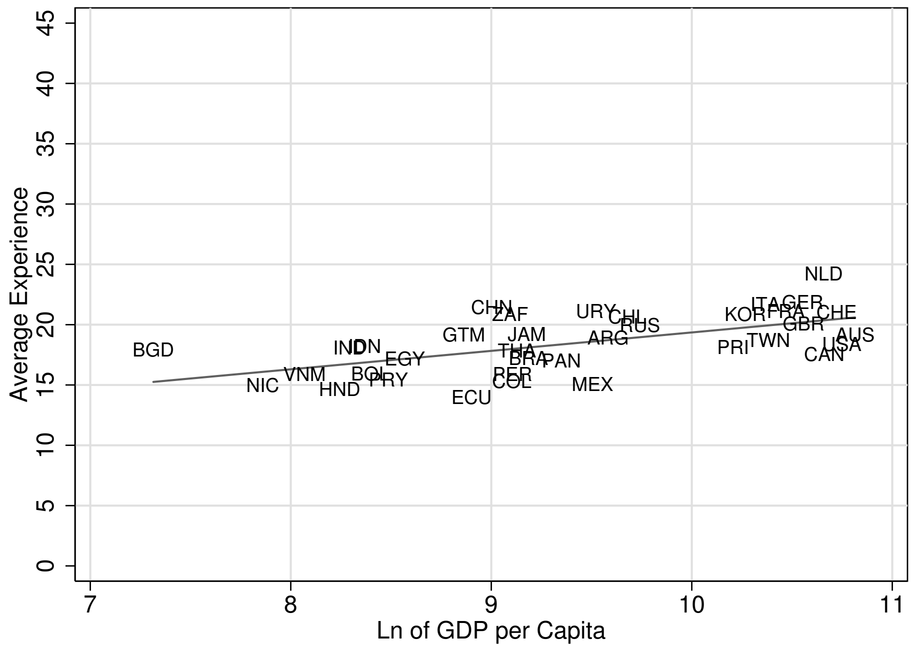


Figure A.4: Fraction of Individuals with Positive Wage Earnings by Age

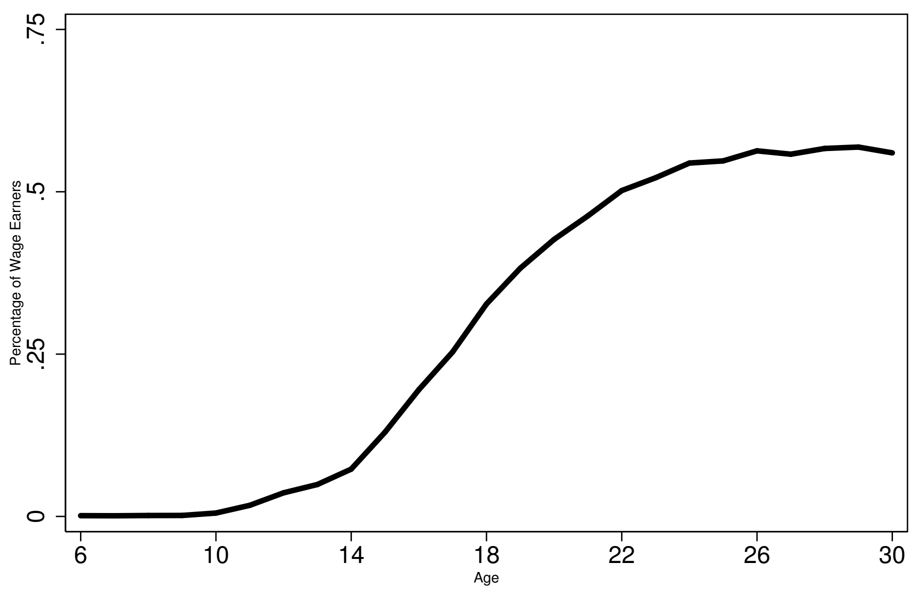
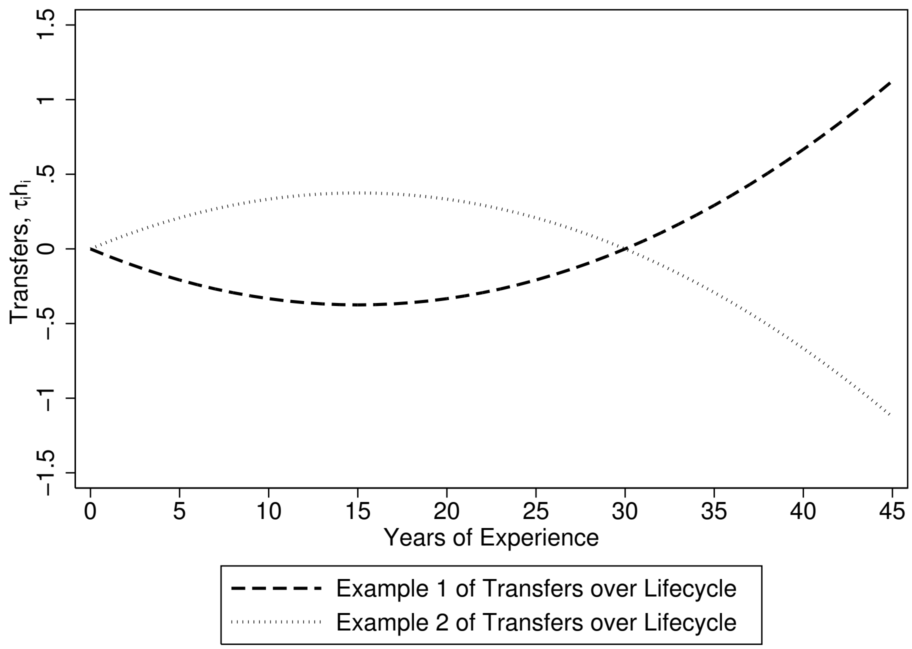


Figure A.5: Acceptable Violations of $w = MPL$

(a) Transfers over Lifecycle



(b) Implied Experience-Earnings Profiles with Transfers

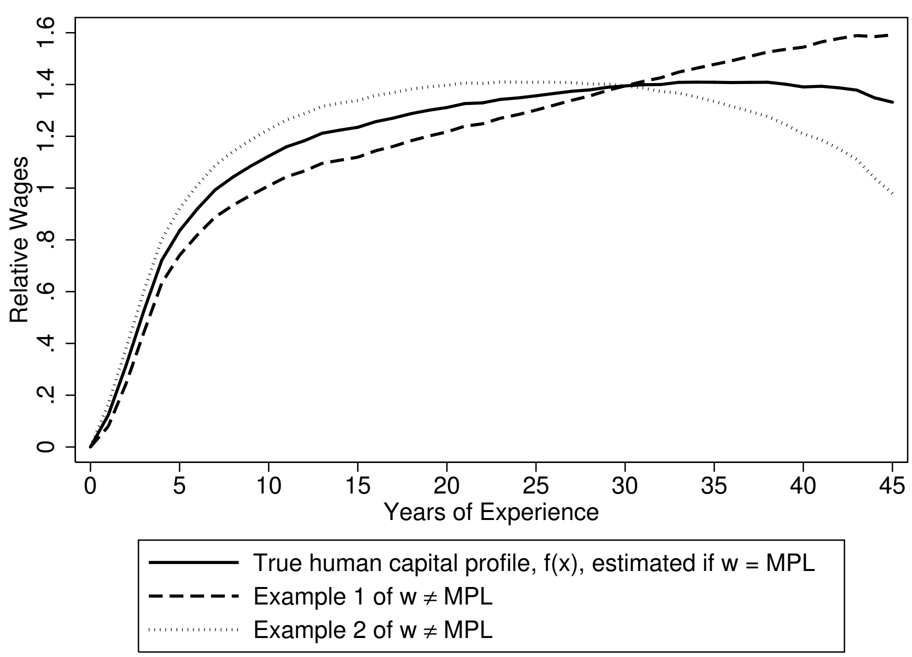
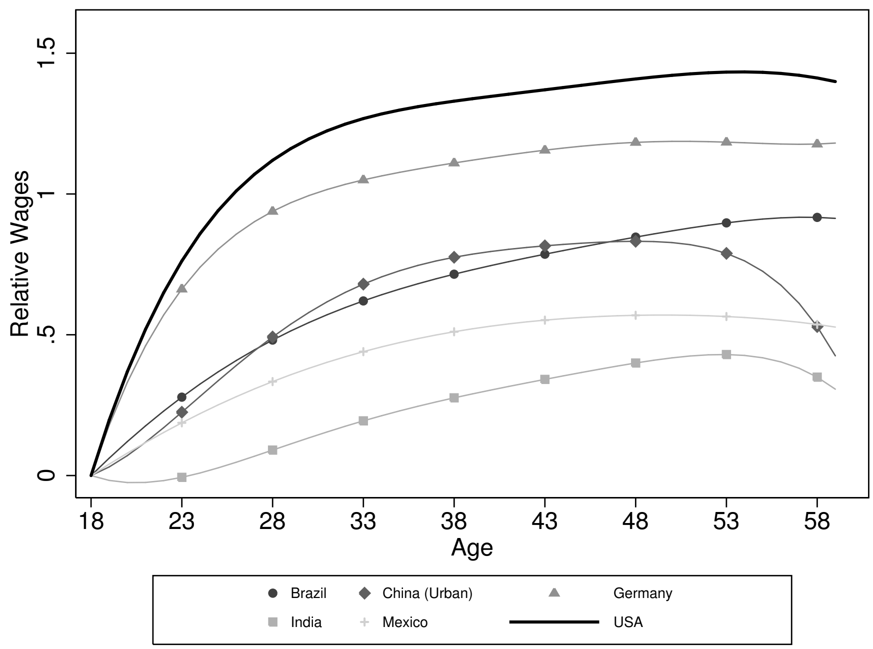


Figure A.6: Age-Earnings Profiles, Cross-Sectional Estimates

(a) Age-Earnings Profiles for Select Countries



(b) Height of Profiles at 38 Years of Age versus Income