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**Education and Growth: Where All the
Education Went**

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Education and Growth: Where All the Education Went

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Abstract

We investigate why the economics literature often finds a negative relationship between increased schooling and GDP growth over short periods. We show that increases in GDP in 98 countries during five-year intervals are correlated with the increases in adults' average schooling during the prior 40 years. We find that an additional year of schooling of the work force raised GDP by 7% on average during 1980-2005, but its initial effect on GDP was much smaller. The delayed effect of increased schooling on national productivity explains why recent increases in schooling cannot explain near-term increases in GDP.

Key Words: Education; Economic Growth; Multi-country; Human Capital; Production Function

JEL Codes: O47; I25

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I. Introduction

For over 25 years researchers have used cross-country data to estimate the relationship between increased schooling and GDP growth. In recent studies researchers have presented cross-sectional evidence showing that increases in average schooling attainment are associated with increases in GDP that are substantially larger than the estimated effect of increased schooling on workers' earnings [Breton, 2013a, and 2015, Gennaioli, La Porta, Lopez-de-Silanes, and Shleifer, 2013, and Sunde and Vischer, 2015].

But in studies within countries over five or ten-year periods, researchers consistently find that there is no relationship or a negative relationship between increases in schooling and changes in GDP/worker. Pritchett [2001] presented these results in his well-known article, "Where Has All the Education Gone?"

Recently, Delgado, Henderson, and Parmeter [2014] used a non-parametric model to analyze the relationship between changes in schooling and economic growth during five and ten-year intervals. Over several time periods, with several data sets and with different groups of countries, they again find either no relationship or a negative relationship between additional schooling and growth.

Krueger and Lindahl [2001] investigated why cross-sectional and time-series estimates of the effect of schooling are so different. They concluded that the national data for schooling attainment (available at the time) had too much measurement error to permit the identification of any effect over five-year intervals. They found that over such short intervals differencing virtually eliminated any signal in the data.

Since that time Cohen and Soto [2007] and Barro and Lee [2013] have revised the cross-country schooling data to reduce the measurement error. But Delgado, Henderson, and Parmeter's empirical results using these more recent data indicate that if measurement error is the problem, these data revisions did not solve it.

Hanushek and Woessmann [2008] argue that the human capital measurement problem is much bigger than simple mis-measurement of national levels of schooling. They argue that schooling attainment is an inherently flawed measure because schooling quality varies dramatically across countries.

But differences in schooling quality do not necessarily invalidate average schooling attainment as a proxy for cross-country levels of human capital. As cited above, researchers have shown that differences in average schooling across countries explain differences in levels of GDP extremely well. Breton [2011 and 2013a] shows that across countries schooling quality

is correlated with average schooling attainment, so to some degree schooling attainment explains differences in both the quantity and the quality of schooling.¹

Even if schooling attainment did not account for differences in schooling quality *across* countries, this limitation would not explain why researchers find no effect from increases in schooling *within* countries. If human capital matters, schooling quality within countries should be sufficiently stable to ensure a correlation between increases in a country's level of adult schooling and increases in GDP/worker over short periods of time.

We think there is a better explanation than inconsistent schooling quality for the negative relationship between increased levels of adult schooling and GDP over short periods. In all of the existing studies, researchers assume that the entire effect on GDP of an increase in adults' average schooling occurs immediately. As a consequence, they include only the recent changes in adult's average schooling when they estimate schooling's relationship to GDP. If increases in schooling affect GDP slowly over time, then studies that examine only the immediate effect would find a small or negligible effect, even though the long run effect is large.

In this article we test the hypothesis that national increases in adults' average schooling have a small initial effect on GDP and a much larger effect over time. Or expressed in another way, we test the hypothesis that the GDP growth observed in the most recent five-year period is due to the increases in adults' average schooling that occurred over the prior 40 years.

The implication of our hypothesis is that a country's average schooling attainment is a poor measure of its human capital over short intervals because it does not account for *how long* workers have had this level of schooling, and, therefore, for how many years their level of education has interacted with experience to improve their productivity on the job. Countries whose workers were educated only recently could have the same average schooling as countries whose workers have been educated for a long time, but their level of human capital would be lower.

Our analysis in this article is focused entirely on the effect of increases in schooling *quantity* on growth. But it is important to point out that any delay between increased schooling and increased worker productivity on the job would apply equally to the effect of increases in students' skills, such as those measured in international tests.² Countries whose

¹ Breton [2010, 2013a, and 2015] shows that across countries average schooling attainment is highly correlated with PPP-adjusted cumulative investment in schooling, so it implicitly accounts for schooling quality differences to some degree.

² It is important not to equate student test scores with school quality. Student test scores are substantially affected by family characteristics and cultural practices (e.g., private tutoring) both within and across countries, so test scores cannot be attributed entirely to schooling quality [See Breton, 2015].

students achieved high test scores only recently would have less human capital than countries whose students have had high test scores for a long time.

Delayed effects in macroeconomic analyses are usually estimated using VAR models. But average schooling levels change so slowly and steadily that VAR models cannot identify the time pattern for schooling's effect. Estimates of schooling's lagged effects in these models exhibit oscillating patterns of positive and negative signs that change with the number of lagged periods included in the model.

We employ an alternative strategy to identify the relationship between changes in schooling and subsequent changes in GDP over time. The increase in workers' earnings with experience at different levels of schooling measures how schooling-related human capital increases as workers obtain experience on the job. We use these relationships in workers' earnings data to convert the average schooling of the work force to an experience-weighted measure of human capital in each country. We then estimate the effect on GDP/worker of changes in this measure of human capital and in physical capital/worker over five-year intervals.

We show that increases in this experience-weighted measure of human capital during five-year intervals are correlated with increases in GDP in 98 countries over the 1980-2005 period. We also show that after only minor adjustments to the experience weights, changes in the human capital variable and changes in GDP are associated at the 1% level.

Our 2SLS estimates of a standard production function indicate that during the first five-year interval, the effect of increased schooling on GDP is only 30% of its eventual effect, which occurs after 35 years. These estimates indicate that an additional year of adult schooling increases GDP by only 3% during the first five years, even though it increases GDP by 7% on average over 40 years.

The implication of this finding is that schooling-based measures of human capital that do not take into account the interactive effect of schooling and experience have considerable measurement error. This error affects estimates of the effect of schooling on GDP to different degrees, depending on the structure of the growth model, the statistical technique employed, and the period of estimation. In OLS regressions using panel data and data differences over short intervals, this measurement error severely attenuates or completely eliminates any estimated effect of schooling.

The rest of this article is organized as follows: Section II presents data showing the relationship between workers' earnings and experience at different levels of schooling. Section III presents the details of the methodology used in this study. Section IV presents the results.

Section V compares the estimates in this study to estimates in other studies. Section VI concludes.

II. Schooling and Workers' Earnings

Existing empirical studies of the effect of increased adult schooling on GDP assume the entire effect is immediate, but they do not include any justification for this assumption. One possible explanation is that this same assumption is used for the effect of physical capital, and it greatly simplifies the analysis compared to an assumption that schooling has a lagged effect.

The more likely rationale behind this assumption is that it parallels the assumption in the simplest version of the Mincer earnings model [Heckman, Lochner, and Todd, 2003]. In this model the effects of schooling and experience are independent, so schooling's entire effect on earnings is immediate:

$$1) \quad \text{Log}(\text{earnings}) = \alpha_0 + \alpha_1 \text{ schooling} + \alpha_2 \text{ experience} + \alpha_3 \text{ experience}^2$$

This model has been estimated throughout the world, and the empirical results for different countries are remarkably similar and consistently statistically-significant [Montenegro and Patrinos, 2014]. In these studies an additional year of schooling typically is associated with a 10 percent increase in earnings.

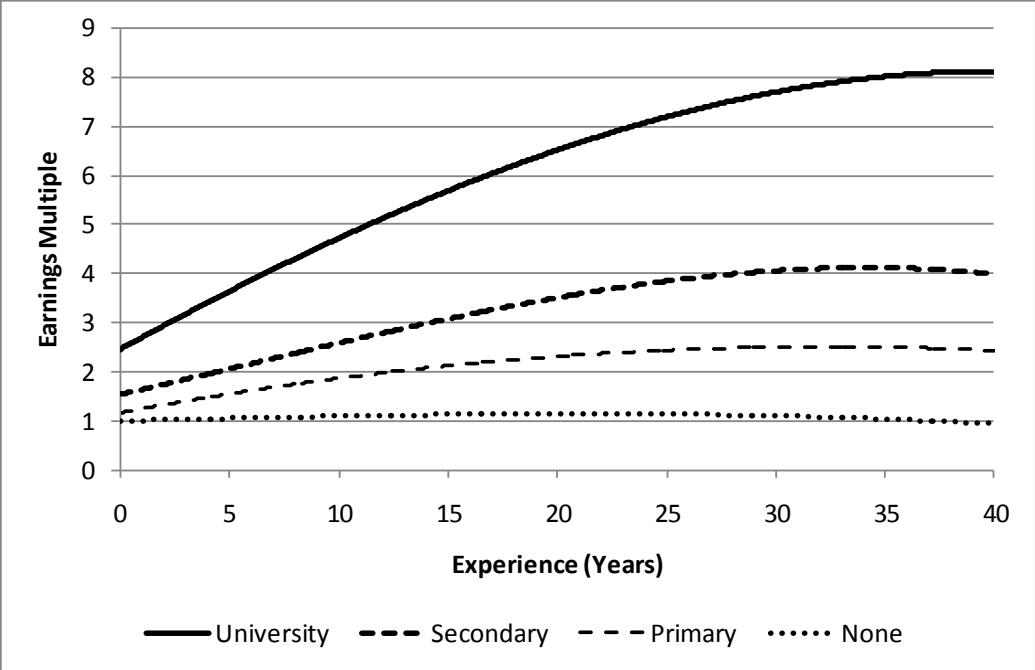
Even though the basic Mincer model provides consistent statistical results, its assumption that schooling and experience are independent appears to be incorrect. There is considerable evidence that earnings increase more with experience at higher levels of schooling, which means that a conceptually-correct earnings model has a positive interactive term between schooling and experience. In other words, some of the effect of additional schooling on earnings is delayed. If workers' earnings are related to their productivity on the job, as assumed in neoclassical theory, then some fraction of the effect of additional schooling on GDP also must be delayed.

Heckman, Lochner, and Todd [2003] present evidence for the U.S. that the effects of schooling and experience on earnings were not independent in 1980 and 1990. Heckman, Lockner, and Todd [2008] present evidence that experience has a greater effect on U.S. workers' incomes at higher levels of schooling. However, since incomes tend to rise with experience at all levels of schooling, and all U.S. workers have some schooling, it is not clear from the U.S. data whether the effect of experience on earnings is mostly related to schooling, or whether it is mostly independent of schooling.

The magnitude of the interactive effect between schooling and experience can only be ascertained in countries where a substantial share of the work force has no schooling. Figure 1

shows Breton’s [2013a] estimate of the average relationship between workers’ earnings and experience at three levels of schooling and with no schooling in four middle income countries in 1990. The relationship is presented as an index, with the initial earnings of a worker with no schooling as the base (1.0). The patterns in the figure show very clearly that earnings rise with experience, *but only for workers who have completed at least primary schooling*. The earnings of workers with no schooling show almost no increase with experience.³

Figure 1
Workers’ Earnings vs. Experience by Level of Schooling



The implication of these patterns is that increases in earnings on the job are a *delayed effect* of an increase in a worker’s level of schooling. While it is undoubtedly true that the rising salaries are a result of increasing worker productivity related to experience and training on the job, the data indicate that the increase in productivity with experience is almost entirely dependent on having completed some level of schooling. So the effect of schooling on the earnings of workers after 20 years of experience is much larger than the effect of schooling on

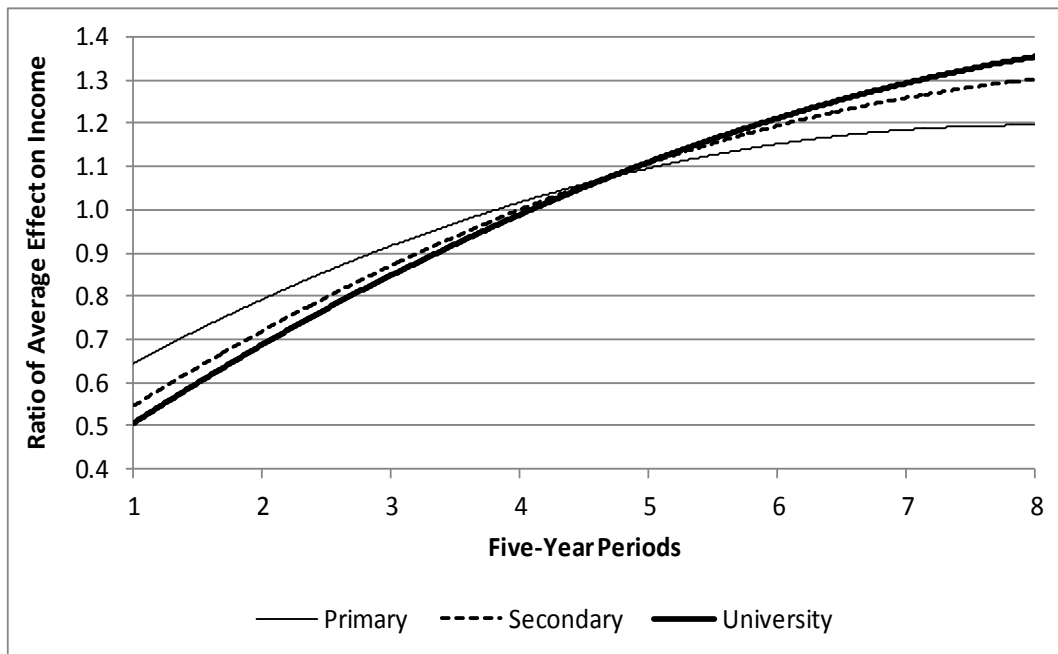
³ The patterns in Figure 1 are based on employee salaries in the public and private sectors. They do not include workers’ earnings in the informal sector, but they are indicative of the relationships between earnings and levels of schooling and experience in the overall labor market.

the earnings of workers with no experience. This delayed effect of additional schooling on workers' earnings is likely to be reflected in a similar delay in its effect on GDP.

Figure 2 converts the data in Figure 1 to a ratio of a worker's earnings in each five-year interval to the worker's average earnings over his/her working life. It shows that during the first five years on the job, a worker with a university education earns only half of his average lifetime salary, and this salary is only 37% of his final salary after 35 years. The data also show that workers with only primary and secondary schooling have lower increases in earnings over time relative to their initial earnings.

Figure 2

Ratio of Workers' Earnings Over Time to Average Lifetime Earnings



The implication of these patterns is that the average delay in the effect of schooling on GDP is slightly higher in more educated countries, since these countries have a higher share of workers with more schooling. Still, the delays in the increase in earnings are similar enough across the three levels of schooling that a cross-country analysis using a single pattern to convert schooling by age cohort to experience-weighted human capital should provide a much more accurate measure of human capital than average adult schooling with no adjustments for experience.

III. Methodology

The conceptual model used in the analysis is a standard Cobb-Douglas production function, in which GDP/worker (Y/L) across countries is a function of the stocks of physical capital/worker (K/L), human capital/worker (H/L), and total factor productivity ($A_0 e^{gt}$). We estimate the model in log form:

$$2) \quad \log(Y/L)_{it} = \alpha \log(K/L)_{it} + \beta \log(H/L)_{it} + (1-\alpha-\beta) \log(A_0) + (1-\alpha-\beta)g t$$

We use either average schooling attainment or the experience-weighted average attainment of the adult population (years) to represent the human capital of the work force, assuming a log-linear relationship between human capital and either measure of schooling:

$$3) \quad \text{Log}(H/L) = c + \gamma/\beta \text{ Schooling}$$

Breton [2013 and 2015] shows that across countries the stock of human capital/adult (H/L) estimated from cumulative investment in schooling fits this log-linear relationship with average years of schooling extremely well. This relationship holds because the (average) unit costs of schooling typically rise exponentially with increases in a country's average level of schooling.⁴

Substitution of the relationship in (3) into (2) yields a log-linear "macro-Mincer" production function [Krueger and Lindahl, 2001]:

$$4) \quad \text{Log}(Y/L) = c + (1-\alpha-\beta)g t + \alpha \log(K/L) + \gamma \text{ schooling} + \varepsilon_i$$

Estimation of this model over different time periods does not provide consistent estimates of α and γ . $\log(K/L)$ and average schooling attainment are highly correlated across countries (0.84 in this study), so measurement error in the variables changes the covariance matrix in econometric estimations, which causes substantial variation in the estimated coefficients.

Since estimates of K/L generally have less measurement error than the schooling proxy for human capital, OLS estimates of (4) yield estimates of α that are biased upward and estimates of γ that are biased downward. The downward bias in γ may be offset by upward bias due to the endogeneity of schooling. Both types of bias can be reduced using instruments for the physical capital and schooling variables.

Improvements in the accuracy of the schooling-based measure of human capital should lead to less attenuation bias in the OLS estimate of γ , raising γ and reducing α . Our

⁴ It is this log-linear relationship between GDP and average schooling that permits cross-country analyses to find a large effect of schooling on GDP even though the quality of schooling is higher in countries with higher average schooling attainment.

methodology relies on this phenomenon to determine whether the effect of increased adult schooling on GDP is delayed and to identify the lag pattern. Superior specifications of the lag pattern should increase the estimate of γ and reduce the variance in its estimate, particularly in differenced estimates of the model over short time intervals.

We estimate the production function using data for GDP/adult, physical capital/adult and schooling/adult for the period 1975-2005. The economic data are from Penn World Table (PWT) 6.3 and 7.1 [Heston, Summers, and Aten, 2009 and 2012]. We calculate the physical capital stock in 1975, 1985, 1990, 1995, 2000, and 2005 using the PWT investment rates (ci) during the prior 25 years and a 0.05 annual depreciation rate. We limit the calculation period for these stocks to 25 years because the investment data begin in 1950.

We create the experience-weighted human capital data from the Barro and Lee [2015] data for the schooling of the population over age 25 during 1975-2005. We use these data rather than the over age 15 data because in most countries many students are still in school between the ages of 15 and 25.

Barro and Lee's over-25 data are excellent for our purpose because they include the average schooling in each five-year age cohort and the size of the cohort for the population between the ages of 25 and 64 in five-year intervals. These data permit a very accurate calculation of the experience-weighted level of human capital across countries as each five-year cohort increases its productivity with experience on the job. We use the age 25-64 schooling data to represent the schooling of the work force in each country over our estimation period. Each five-year cohort's human capital is estimated as its fraction of schooling's eventual full effect of schooling on its productivity. The fraction in the age 60-64 final cohort is equal to 1.0 and the fractions in the earlier, less-experienced cohorts are less than 1.0. This measure for the population is an estimate of the relative human capital of the work force in each country compared to a work force with the same average schooling composed entirely of workers with 35-40 years of experience.

We would not expect to find a statistically significant empirical relationship between factor inputs and GDP in economies whose production is not determined primarily by profit maximization with markets for inputs and outputs. For this reason we exclude countries from the analysis that were not market economies throughout the period. We also exclude countries that lacked sufficient data to calculate the physical capital stock during at least the 1985-2005 period or that were not included in the Barro and Lee [2015] data.

This left us with an unbalanced panel of 98 countries. Since PWT 6.3 and 7.1 do not have investment rates for some low-income countries prior to 1960, only 57 of the 98 countries

in our panel have capital stock data in 1975 and only 66 have these data in 1980. The data are complete for 1985-2005.

We began the analysis by specifying an experience-weighted pattern for human capital similar to the pattern of observed increases in workers' earnings in Figure 2. In this pattern the human capital in each five-year cohort of workers is about 40, 55, 65, 75, 85, 93, 98, and 100 percent of the eventual level in the 60-64 age cohort for an average level of schooling. We examined the effect of this measure on GDP and then examined the effect of slightly different patterns until we identified a pattern that provides an estimate of the effect of additional schooling on GDP with high statistical significance.

The earnings pattern in Figure 2 indicates that a worker's productivity during the initial five-year period is 40 percent of his/her eventual productivity.. We found that patterns beginning with a lower fraction of eventual productivity have slightly larger estimated effects on GDP and/or higher statistical significance than the observed pattern. We show the statistical results for the observed pattern and for a pattern beginning with 30 percent of the eventual level of productivity. Patterns with even lower initial fractions (20-25 percent) yield similar statistical results.

We also performed an analysis using the over-25 schooling attainment data five years earlier to represent the schooling of the age 20-24 cohort. We found that changes in the schooling of this cohort do not explain the changes in GDP over the estimation period.

Since the experience-weighting calculation of human capital reduces the average years of schooling in the first seven cohorts, the average values of the experience-weighted measures of human capital are lower than a country's average years of schooling. These lower average values increase the estimated effect of a year of schooling in the regressions that use these measures. We adjust the average effect of a year of schooling on GDP for the experience-adjusted measures so the comparison with the effect of average schooling is comparable in the results.

Economic time series typically are non-stationary of degree one. Although the number of time periods in our panel is short, the time series components could have unit roots, which could create bias in the estimated coefficients.

Since our interest is in examining whether changes in schooling within countries affect GDP, we estimate our models in differences to eliminate the cross-sectional effects in the data. This differencing has the added benefit that it eliminates any trends in the data that could bias the estimated effects of physical capital and schooling. We tested the differenced data using the Im-Pesaran-Shin test and confirmed that for the three variables in the model the null hypothesis that all the data series contain a unit root is rejected at the 1% level.

Our physical capital and schooling variables could be endogenous, so we estimated the with RE GLS and with 2SLS with instruments created from lagged values of the capital and schooling variables. We performed our analysis first with PWT 6.3 data and then checked the robustness of the results using PWT 7.1 data.

IV. Results

Table 1 presents estimates of the model using different assumptions and statistical techniques with three human capital measures. The measures are average schooling attainment and the two experience-weighted schooling measures described above. The first experience-weighted measure assumes human capital in the first five-year cohort is 40 percent of the human capital in the age 60-64 cohort. The second measure assumes the first cohort is 30 percent of the human capital in the age 60-64 cohort. The table is organized to facilitate a comparison of the effects, with the three measures side by side for each estimation method.

	1	2	3	4	5	6	7	8	9
Observations	515	515	515	417	417	417	417	417	417
Technique	RE GLS	RE GLS	RE GLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
	AvSch	405565	305065	AvSch	405565	305065	AvSch	405565	305065
Instruments				L1DS L3DS L1DK	L1DLS L3DLS L1DK	L1DLS L3DLS L1DK	L1DK	L1DK	L1DK
D.Log(K/A)	0.48* (.06)	0.48* (.06)	0.48* (.06)	0.33* (.08)	0.28* (.09)	0.28* (.09)	0.30* (.09)	0.29* (.09)	0.28* (.09)
D.Schooling Measure	0.03 (.01)	0.06* (.02)	0.07* (.02)	0.01 (.03)	0.10 (.04)	0.11* (.04)	0.02 (.01)	0.06* (.02)	0.07* (.02)
R ²	.26	.27	.27	.22	.21	.21	.21	.22	.22
α	.48*	.48*	.48*	.33*	.28*	.28*	.30*	.29*	.28*
Adj. γ^a	.03	.04*	.04*	.01	.07	.07*	.02	.04*	.05*
*Statistically significant at the 1 percent level.									
^a Average 30-50-65 fraction is 0.64 and 40-55-65 fraction is 0.67 of average schooling.									
Note: Robust standard errors in parentheses									

In our production function α is the share of national income that accrues to physical capital, which Gollin [2002] estimates is about 0.35 on average across countries. We include our estimate of this measure in the table to show whether each model provides acceptable estimates of α . We also include our estimate of γ , adjusted to account for the lower average value of our experience-weighted measures. Since micro earnings studies show that each year

of schooling raises workers' earnings about 10%, and these earnings in the aggregate are 70% of GDP, we expect adjusted γ to be at least 0.07.

The first three columns show the results for random effects generalized least squares. The estimates of α are similar for all three measures (0.48), and they substantially exceed the expected level of 0.35. The estimates of γ are all positive, but they are lower than the expected level of 0.07. The effect with the two experience-weighted measures is larger (.04) than with the average schooling measure, and both experience-weighted measures are statistically significant at the 1% level.

The next three columns (4-6) show the results with 2SLS using the first lag of the change in physical capital/adult and the first and third lags of the schooling measures as instruments. We do not use the second lag of the schooling measures because a Sargan test showed that it is not a valid instrument. Using the first lag of the capital variable as an instrument shortens the estimation period by five years to 1980-2005, reducing the sample size from 515 to 417. Including the lags for the schooling measures as instruments does not affect the sample size because the schooling data are available for a longer historic period.

The 2SLS model provides much more acceptable values for α (0.28 -0.33), and these estimates continue to be statistically significant. This model finds no effect from average schooling attainment, but it finds the expected effect (0.07) for the experience-weighted schooling measures. The estimate with the 40-55-65 measure is statistically significant at the 5% level, and the estimate with the 30-50-65 measure is statistically significant at the 1% level. We interpret these results to indicate that the effect of increased schooling on GDP is substantially delayed and the effect in the initial five-year interval is only 30% of its eventual effect.

The standard STATA post-estimation tests for 2SLS estimation indicate that the physical capital variable is endogenous and, somewhat surprisingly, the schooling-based variables are not. The tests also indicate that the three instruments are strong ($F > 10$) and that they are valid. In theory the rejection of the null for the endogeneity of the schooling-based variables means that they do not require instrumentation to control for endogeneity bias.

Columns 7-9 show the 2SLS results that include an instrument only for the physical capital variable. The estimates of α are similar and statistically significant, but the estimates of γ with the experience-based measures are lower (0.05) than the instrumented estimates. Again the estimates using the experience-weighted measures are larger than with the average schooling measure and statistically significant at the 1% level. The adjusted estimate of γ with the 30-50-65 measure is larger than with the 40-55-65 measure, which we interpret to mean

that the 30-50-65 measure has less measurement error, so its estimated coefficient exhibits less attenuation bias.

Since instruments reduce attenuation bias, we interpret the entire set of results to indicate that the 30-50-65 experience-weighted measure is the most accurate measure of human capital of the three measures and that the 2SLS estimate of its effect is the least biased. This measure indicates that an additional year of schooling raises GDP by 7% over the working life of a cohort of workers.

Table 2 shows the same model estimates using PWT 7.1 data. The estimates of α and γ are slightly different, but they exhibit the same patterns for the three human capital measures as in the PWT 6.3 data, with the 30-50-65 measure providing the best results. These estimates show that the effect of the experience-weighted schooling variables is robust to changes in the economic data series used for the analysis.

Table 2									
Effect of Schooling on GDP with PWT 7.1 Data									
[Dependent variable is D.log(GDP/adult)]									
	1	2	3	4	5	6	7	8	9
Observations	515	515	515	417	417	417	417	417	417
Technique	RE GLS	RE GLS	RE GLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
	AvSch	405565	305065	AvSch	405565	305065	AvSch	405565	305065
Instruments				L1DS L3DS L1DK	L1DLS L3DLS L1DK	L1DLS L3DLS L1DK	L1DK	L1DK	L1DK
D.Ln(K/A)	0.44* (.05)	0.43* (.05)	0.43* (.05)	0.28* (.08)	0.22* (.08)	0.21* (.08)	0.26* (.08)	0.23* (.08)	0.23* (.08)
D.School Attainment	0.03 (.01)	0.06* (.02)	0.07* (.02)	0.03 (.03)	0.12* (.05)	0.14* (.05)	0.03 (.01)	0.07* (.02)	0.08* (.02)
R ²	.23	.24	.24	.19	.17	.17	.18	.19	.19
α	.44*	.43*	.43*	.28*	.22*	.21*	.26*	.23*	.23*
Adj. γ^a	.03	.04*	.04*	.03	.08*	.09*	.03	.05*	.05*
*Statistically significant at the 1 percent level.									
^a Average 30-50-65 fraction is 0.64 and 40-55-65 fraction is 0.67 of average schooling.									
Note: Robust standard errors in parentheses									

Nevertheless, the estimates of the models using the PWT 7.1 data are less satisfactory from a conceptual standpoint. The estimates of α are consistently lower by about 0.06 compared to the PWT 6.3 data, so they are considerably lower than expected. In addition, all of the models explain less of the variation in GDP/adult than the models using the PWT 6.3 data.

The estimates of γ are higher using the PWT 7.1 data. In the 2SLS estimate of the 30-50-65 experience-weighted measure, each additional year of schooling raises GDP by 9%. This estimate appears to be biased upward, because the estimated coefficient on physical capital (0.21) is so low that it must be biased downward.

There is considerable evidence that the PWT 7.1 data are less accurate than the PWT 6.3 data. In PWT 7.1 all of the national accounts data from 1950 to 2010 were adjusted using the prices collected in ICP 2005, rather than the prices collected in earlier ICP surveys that correspond in time to the data [Breton, 2012]. This change appears to have reduced the accuracy of the data in PWT 7.1. In addition, construction prices appear to be underestimated in many developing countries in ICP 2005, which causes an overestimate of investment rates [Breton and Garcia, 2016]. Deaton and Aten [2014] identify other estimation problems that biased the prices in ICP 2005. Since the ICP 2005 prices were used to adjust the entire 1950-2010 period, they appear to substantially bias the PWT 7.1 data. Overall, we conclude that the empirical results from the models using the PWT 6.3 data are more accurate.

V. Comparison with Cross-Sectional Estimates

Earlier we observed that cross-sectional estimates of the relationship between schooling and GDP typically produce estimates that are larger than the effect of additional schooling on workers' earnings. But the estimates in the literature often are not comparable because their magnitude is affected by the form of the income or growth model used for the estimation. Reduced forms of the production function and models that do not include physical capital provide larger estimates of the effect of schooling. These estimates must be adjusted to compare them to the effect of schooling in the standard production function.

The estimated production functions in the literature are in two forms, the standard form shown in (4) and a reduced form that is a function of the capital/output ratio:

$$5) \quad \text{Log}(Y/L) = C + (1-\alpha-\beta)g/(1-\alpha) t + \alpha/(1-\alpha) \log(K/Y) + \gamma/(1-\alpha) \text{ schooling}$$

In this model the coefficient on schooling is $\gamma/(1-\alpha)$, so estimates using this function must be reduced by the factor $1-\alpha$ to compare them to the estimated coefficient in the standard function.

Some analyses omit the physical capital variable altogether. These models are misspecified unless they have explanatory variables that substitute for physical capital. The estimated coefficient on schooling in these models is biased upward because schooling is highly correlated with the missing physical capital variable. In these models the estimated coefficient on schooling is about 2γ . Mankiw, Romer, and Weil [1992]] show that when one type of capital

is excluded in a cross-sectional analysis, the estimated coefficient on the remaining capital variable approximately doubles.

Table 3 presents the estimated coefficients on schooling in six recent cross-sectional estimates of national income models. The period and form of the model used for these estimates varies, but all of the estimates are cross-sectional or panel estimates that include the cross-country relationship. As a consequence, all of these estimates implicitly estimate the long-run effect of schooling.

Study	Period	Coefficient Estimated			Implied
		γ	$\gamma/(1-\alpha)$	2γ	
Cohen & Soto, 2007	1960-90		.13		.08
Gennaioli, et. al, 2013	2005			.24*	.12
Breton, 2013a	1990	.12**			.12
Breton, 2013b	1910-2000		.16		.10
Sunde & Vischer, 2015	1970-2000	.13			.13
Breton, 2015	1985-2005	.09**			.09

*Adjusted down from 0.28 to account for their lower measure of schooling.
 **Converted from estimates of β using $\gamma/\beta = 0.32$ [Breton, 2013a]

Gennaioli, et. al. [2013] found that each additional year of schooling is associated with a 28% increase in regional income. Their estimate is not comparable to the other estimates because their measure of average schooling attainment only includes years related to completion of a degree. Since their average schooling level is 16% lower than Cohen and Soto's estimate of average schooling, a comparable estimate of the effect of an additional year of schooling on income in their study is 24%. Since they did not include physical capital in their model, this estimate is about double the implied value of γ , which is about 0.12.

Breton [2013a and 2015] estimates the effect of investment in schooling (β), rather than the effect of years of schooling (γ), but Breton [2013a] presents evidence that for the Cohen and Soto [2007] schooling data, $\beta/\gamma = 0.32$. Using this conversion ratio his estimates of the effect of a year of schooling are 0.09 and 0.11.

Cohen and Soto [2007] and Breton [2013b] estimated the reduced form model in (5), so their estimated coefficients are higher by a factor of $1-\alpha$. The implied values of γ in these estimates are not that different from the estimate in workers' earnings studies.

So while estimated coefficients in the literature vary considerably, the implied values of γ in these estimates all range from 0.08 to 0.13. These estimates are larger than the 0.07 estimate of the effect of schooling on GDP in workers' earnings studies and in this study, but they are relatively similar. This summary should counter the common belief that cross-sectional estimates of the relationship between schooling and GDP are several times larger than the estimates in workers' earnings studies.

Overall the empirical research now indicates that an additional year of schooling, holding physical capital/worker constant, increases GDP by about 3% in the short run and 7-13% in the long run. This finding is a huge change from the situation a short time ago when Pritchett [2001] concluded that on average across countries increased schooling reduces economic growth.

VI. Conclusions

For over 20 years researchers have tried without success to find an effect on GDP from increases in schooling over five-year periods. After performing one of these analyses and finding only negative correlations, Pritchett [2001] famously asked, "Where has all the education gone?" In this paper we provide an answer to this question.

The existing analyses that fail to find any effect assume that the entire effect of schooling is immediate. We examine whether the effect of schooling on GDP may be substantially delayed to determine whether this may explain the failure to find any effect.

We first present data showing that increases in schooling affect workers' earnings differently depending on their level of schooling. We also show that workers' earnings in middle income countries only increase with experience if they have prior schooling. We conclude that increases in worker productivity and in earnings on the job are a delayed effect of their prior schooling.

We then examine whether a pattern with a delayed effect similar to the one observed for workers' earnings may characterize the relationship between increased schooling and GDP. We find that this pattern can explain changes in GDP. But we find that a pattern in which the initial effect of schooling on GDP is slightly lower than in the earnings studies (30% of its eventual effect) provides results that are more statistically significant. The clear implication is that the increase in GDP during a five-year period is due to the increases in schooling during the prior 40 years.

We find that an additional year of schooling in the population age 25-64 raises GDP by 7% on average over a 40-year period, but the effect associated with this additional year in the

initial five-year period is only 3%. Since average schooling typically increases by less than a year over a five-year period, the initial effect of increased adult schooling on GDP is very small.

So this is where the education went. It had a small initial positive effect on GDP and then contributed steadily to workers' productivity as they gained experience over their working lives. Even though their improvement in productivity occurred on the job, it was not independent of their prior schooling. As a consequence, it is appropriate to consider the productivity improvements on the job as a delayed effect of the workers' earlier schooling.

The results in this article highlight the reality that increasing a country's productivity through education is a very long-term process. It begins by investing in children's pre-schooling and continues through primary, secondary, and post-secondary schooling. If there is any effect on GDP during the schooling period, it is negative since students forego work to attend school. Even if the students entering the work force have more schooling than the existing workers, the positive effect of this additional schooling materializes slowly. In this study the initial effect did not occur until after age 25 and then it continued to increase until age 65.

But the positive effects of additional adult schooling are not limited to this 40-year period. There is considerable evidence that student achievement in school is positively affected by their parents' level of education [Fuchs and Woessmann, 2007]. In addition, students with educated parents stay in school longer and eventually are more productive on the job [Hanushek and Woessmann, 2008]. This continuing positive effect of additional schooling beyond an adult's working life may explain why the effect of schooling on GDP in cross-sectional analyses is greater than the effect found in this study.

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