Education and Economic Growth: Where All the Education Went

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Abstract

We investigate why neoclassical analyses in the literature have found no effect from increased schooling on GDP over short periods. We show that in 98 countries increases in GDP over five-year intervals during 1980-2005 are positively correlated with increases in adults´ schooling attainment during the prior 45 years. Additional schooling has an effect on GDP that is similar to its effect on workers' earnings as they acquire experience on the job. We find that an additional year of schooling in the age 20-24 cohort raised GDP by 6-7% after 45 years but by only 3% during the first five years.

Key Words: Education; Economic Growth; Human Capital; Production Function; Mincer model

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I. Introduction

For over 25 years researchers have used cross-country data to estimate the cross-sectional relationship between schooling and GDP in the augmented Solow model. Recent cross-sectional studies show that increases in average schooling attainment are associated with increases in GDP/worker that are substantially larger than their estimated effect on workers' earnings [Breton, 2013a, and 2015, Gennaioli, La Porta, Lopez-de-Silanes, and Shleifer, 2013, and Sunde and Vischer, 2015].

In contrast, in longitudinal studies over five or ten-year periods, estimates of this model consistently find that there is no relationship or even a negative relationship between increases in schooling and changes in GDP/worker. Pritchett [2001] presented some of these results in his well-known article, "Where Has All the Education Gone?"

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 an effect over five-year intervals. They showed that over such short intervals differencing virtually eliminated any signal in the data.

Subsequently, Cohen and Soto [2007] and Barro and Lee [2013] revised the cross-country schooling data to reduce the measurement error. Using these data and a non-parametric model, Delgado, Henderson, and Parmeter [2014] analyzed the relationship between changes in schooling and economic growth over five and ten-year intervals. In analyses over different time periods and with different groups of countries, they again found either no relationship or a negative relationship between additional schooling and GDP growth. So if measurement error was the problem, the data revisions did not solve it.

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

But their concerns appear to be exaggerated. As cited above, differences in average schooling explain cross-country differences in GDP extremely well. Moreover, Breton [2011 and 2013a] has shown that across countries schooling quality is correlated with average schooling attainment, so to some degree average attainment accounts for differences in both the quantity and the quality of schooling.[1]

More importantly, even if schooling attainment did not account for differences in schooling quality *across* countries, this limitation would not explain why researchers have been unable to find any effect from increases in schooling *within* countries. If schooling creates human capital, schooling quality should be sufficiently stable within countries over short periods to ensure a positive correlation between increases in the average level of adult schooling and increases in GDP/worker. Even with measurement error in the data, estimates using valid instruments should find an effect.

We think there is a better explanation for the failure to find a positive relationship over short periods. In the existing studies researchers assume the effect of increased schooling on GDP is immediate, so they include only recent changes in schooling when they estimate schooling's effect. If an increase in schooling affects GDP gradually over a long period, then studies that examine only its 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 an increase in a country's average schooling attainment affects GDP gradually over the life of a cohort of workers. We begin by quantifying the cross-sectional relationship in middle-income countries between schooling and workers' earnings over their working lives. We then investigate whether this micro cross-sectional relationship explains the macro longitudinal relationship. We find that increases in schooling in 98 countries over the prior 45 years can explain GDP growth over a series of five-year periods.

The implication of our findings is that the average schooling attainment of the population is a poor measure of human capital because it does not account for *how long* workers have had this level of schooling, and, therefore, for the number of years that their education and experience have interacted to improve their productivity on the job. Countries who are raising their average level of schooling could have a work force with the same average attainment as countries whose workers have been educated for a longer time, but their level of human capital would be lower.

Delayed effects in macroeconomic analyses are usually estimated using VAR models, but average schooling levels within countries are so highly correlated over short intervals that VAR models cannot identify the lag pattern for schooling's effect on GDP. Estimates of schooling's

[1] Breton [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.

lagged effects in these models invariably have oscillating signs on the lags that change with the number of periods included in the model.

We employ an alternative strategy to identify the relationship between changes in schooling and associated changes in GDP. Workers' earnings increase with experience at different rates depending on their levels of schooling. We use these relationships in workers' earnings data in three middle-income countries to convert the average schooling of the work force to an experience-weighted measure of human capital in each country.[2] We then estimate the effect on GDP/worker of changes in this measure, in physical capital/worker, and world TFP over five-year intervals.

We show that five-year increases in this experience-weighted measure of human capital are correlated with increases in GDP in 98 countries over the 1980-2005 period. Our estimates of a standard production function indicate that during the first five-year interval, the effect of increased schooling on GDP is 50% of its eventual effect, which occurs after 45 years. These estimates indicate that an additional year of adult schooling increases GDP by only 3% during the first five years, even though it eventually increases GDP by 6-7% after 45 years.

The implication of this finding is that schooling-based measures of a country's human capital that do not account for the interactive effect of schooling and experience have considerable measurement error. This error biases 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 eliminates the estimated effect of schooling. This measurement error affects cross-sectional estimates much less than time series estimates because all countries experience similar lags in the effect of additional schooling on GDP.

Our analysis is focused on the effect of increases in schooling attainment on growth. But it is important to point out that the observed delay between increased schooling and the effect on GDP could apply equally to the effect of increases in students' skills, such as those measured in international tests.[3] Countries whose workers achieved high test scores only recently could have less human capital (and lower GDP) than countries whose workers have similar average scores but whose older workers have higher scores.

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[2] Thanks to Diego Restrepo-Tobon for suggesting this approach to estimate the lagged effects.

[3] 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 differences in test scores cannot be attributed entirely to schooling quality [See Breton, 2015].

This article makes several contributions to the literature. First, it quantifies the schooling-related increases in workers' earnings with age by level of schooling in a group of middle-income countries. Second, it shows that the lagged relationship between increased schooling and increased earnings at the micro level is reflected at the macro level. Third, it provides an estimate of the time pattern for the initial and eventual increase in GDP resulting from an additional year of average schooling attainment. Fourth, it shows that while cross-sectional estimates of the effect of increased schooling on GDP in the literature are larger than the direct effect on workers' earnings, the difference is not at large as many researchers believe.

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 the cross-sectional estimates in other studies. Section VI concludes.

II. Schooling and Workers' Earnings

Existing empirical studies assume that any effect of increased adult schooling on GDP is immediate, but they do not include any justification for this assumption. One possible rationale is that this same assumption is used for the effect of increased physical capital. But the more likely rationale is that this assumption is consistent with the relationship in the simplest version of the Mincer earnings model, in which schooling and experience have independent effects on log(earnings) [Heckman, Lochner, and Todd, 2003].

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

As a consequence of the mathematical structure in this model, the entire effect of increased schooling on log(earnings) is immediate.

This version of the Mincer model has been estimated using cross-sectional data throughout the world over a long period of time. The average estimated effect of an additional year of schooling across countries has been quite consistent over the years, raising workers' incomes by about 10% on average. However, the variation in this effect across countries has changed over time.

In recent analyses the effect of a year of schooling is similar regardless of country income level [Montenegro and Patrinos, 2014]. But in older studies, which are more relevant for a time series analysis, the effect of a year of schooling was higher in less educated countries. Psacharopoulos and Patrinos [2004] and Psacharopoulos [1994] found that a year of schooling was associated with salary increases of about 11% in low-income countries and about 7% in high-income countries.

Even though the simplest Mincer model provides results that are easy to interpret and statistically significant, its assumption that schooling and experience have independent effects on log(earnings) appears to be incorrect. There is considerable evidence that log(earnings) increases more with experience at higher levels of schooling, which means that a conceptually-correct model would include a positive interactive effect between schooling and experience. The implication is that some of the effect of additional schooling is lagged.

Dougherty and Jimenez [1991] examined whether the effects of primary and secondary schooling on log(earnings) are independent of experience in Brazilian survey data for 1980. They found that the coefficients on the interaction terms between primary schooling and experience and experience^2 and between secondary schooling and these variables are all statistically significant at the 1% level.

Heckman, Lochner, and Todd [2003] present evidence that the effects of schooling and experience on log(earnings) in the U.S. were not independent in 1980 and 1990. Heckman, Lockner, and Todd [2008] present evidence that in the U.S. experience has a greater effect at higher levels of schooling.

Since incomes tend to rise with experience at all levels of schooling, and since all U.S. workers have some schooling, it is not clear from the U.S. data whether the effect of experience on log(earnings) is mostly related or mostly independent of schooling. The magnitude of the interactive effect between schooling and experience can only be ascertained in countries in which a share of the work force has no schooling.

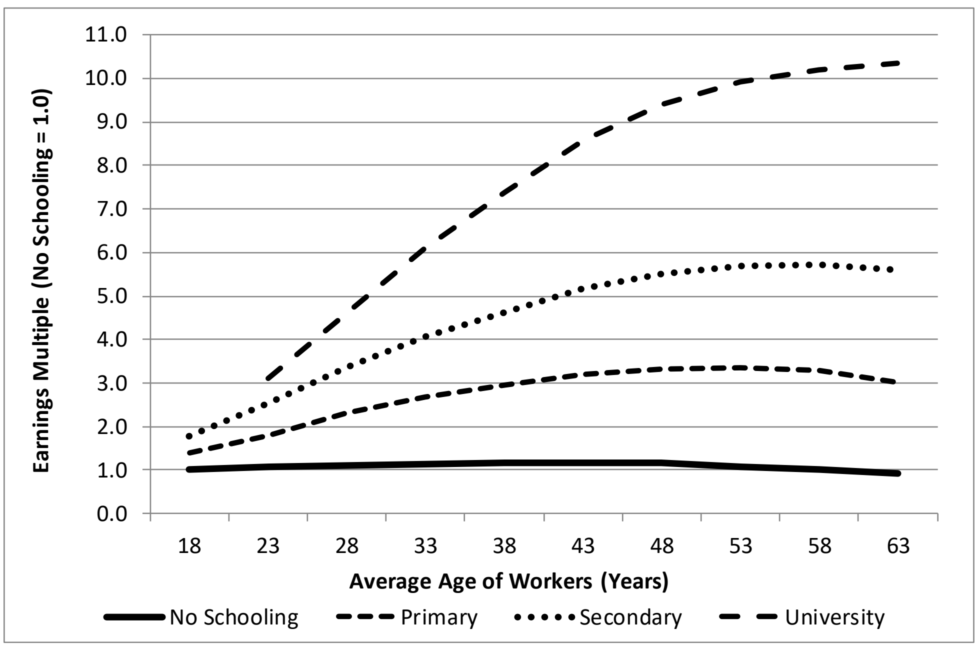
Figure 1 shows the quadratic trends in the relationship between workers' earnings and experience at three levels of schooling and with no schooling in three middle-income countries around 1990. The three countries are Ecuador, Paraguay, and Uruguay, and the data are from Gomez-Castellanos and Psacharopolous [1990], Psacharopoulos and Velez [1994], and Psacharopolous, Velez, and Patrinos, [1994].

The relationship in the figure is presented as an index, with the initial earnings of a worker with no schooling and no experience 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 and decline after age 48.[4]

The implication of these patterns is that increases in earnings on the job are at least partly a *delayed effect* of an increase in a worker's level of schooling. While it is undoubtedly true that rising salaries are a result of increasing worker productivity related to experience and training on the job, the data in Figure 1 indicate that the increase in productivity with experience is dependent on having completed some level of schooling. This delayed effect of additional schooling on workers' earnings is likely to be reflected in an analogous delay in its effect on GDP.

Figure 1

Workers' Earnings vs. Age by Level of Schooling



We use the data for Ecuador, Paraguay, and Uruguay to estimate a Mincer model with interactive terms to estimate the lag between increases in the schooling of the work force and increases in worker productivity. Table 1 presents the results from a series of Mincer-type models that include years of schooling, age, age^2, and interactive terms between years of

[4] 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, which could be different.

schooling and the age terms. Age is used as a proxy for experience to create results that are applicable to the age-related cohort data in the macro analysis.

The basic model indicates that an additional year of schooling in these middle-income countries raised earnings on average by 12% over the life of a worker. But when interactive terms are added, it becomes evident that about half of this effect is delayed. The model results in column 2 show that experience (age) alone has no effect on earnings; all of the effect of experience is associated with prior completion of some level of schooling.

Effect of Additional Schooling on Workers' Earnings in Middle-Income Countries [Dependent variable is log(earnings)]

	1	2	3
Schooling (years)	.120* (.008)	.073* (.018)	.068* (.012)
Age	.029** (.012)	.0040 (.0199)	
Age^2	-.0003 (.0003)	-.00004 (.00035)	
Schooling*Age		.0032 (.0021)	.0035* (.0012)
Schooling*Age^2		-.00003 (.00005)	-.00003 (.00003)
Constant	-.28** (.12)	.083 (.171)	.139** (.069)
R^2	.73	.76	.76

Figure 2 shows the fraction of log(earnings) at age 63 received by workers as a function of age, calculated using the regression results in column 3 of Table 1. The graph in the figure shows the relationship between the productivity of workers by age relative to their productivity at age 63, as they gain experience on the job. The relationship is estimated using seven years of schooling, which was the average level of attainment in the three countries in 1990. The pattern indicates that half of the effect of additional schooling on productivity was immediate and half was delayed up to 40 years.

Figure 2

Effect of Experience on Worker Productivity in Middle Income Countries

III. Methodology Used for the Growth Analysis

The model used in the analysis is the standard augmented Solow model, 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) that increases at a constant rate over time ($A_t = A_0e^{gt}$):

2) $(Y/L)_{it} = (K/L)_{it}^{\ \alpha} (H/L)_{it}^{\ \beta} (A_0e^{gt})^{1-\alpha-\beta}$

Converted to log form:

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

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:[5]

4) $\text{Log}(H/L)_{it} = c + \gamma/\beta \text{ Schooling}_{\ it}$

[5] γ/β is specified as the coefficient, so that γ is the effect of a year of schooling on log (Y/L).

Breton [2013a 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 very well. This relationship holds because the (average) unit costs of schooling typically rise exponentially with increases in a country's average level of schooling.[6]

Substitution of the relationship in (4) into (3) yields the log-linear "macro-Mincer" production function [Krueger and Lindahl, 2001]:

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

Estimation of this model across countries over different time periods does not provide consistent estimates of α and γ. Log (K/L) and average schooling attainment are structurally related in the augmented Solow model, which makes them highly correlated across countries (0.81 - 0.84 in this study). 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) often 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. Alternatively, α and/or γ may be biased downward, biasing the TFP residual (1-α-β)g upward. Endogeneity bias and attenuation bias due to random measurement error 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 or (1-α-β)g.

We evaluate the validity of the model's estimated coefficients based on *a priori* expectations for the values of α and (1-α-β)g. In the augmented Solow model α is the share of GDP allocated to physical capital, which should be about 0.35 [Gollin, 2002]. The coefficient on time, the TFP growth rate, should be positive, since world productivity increases over time. A more accurate specification of the lag pattern for the effect of schooling 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 cross-country data for GDP/adult, physical capital/adult and schooling/adult for the period 1975-2005. Since the estimated coefficient on the schooling variable is sensitive to non-random measurement error in the physical capital data, the estimate differs when the production function is estimated with different sets of

[6] Since the quality of schooling is higher in countries with higher average schooling, the estimated effect of average schooling (quantity) on GDP implicitly includes the average effect of schooling quality differences.

economic data. We estimate the production function using economic data from Penn World Table (PWT) 6.3, 7.1, and 8.1 [Heston, Summers, and Aten, 2009 and 2012, and Feenstra, Inklaar, and Timmer, 2015a] to provide a thorough test of our hypothesis. We use the na version of PWT 8.1 for this test, as recommended by Feenstra et. al [2015a] for growth analyses.

Each PWT data set adjusts National Accounts data for differences in purchasing power, but in different ways. The PWT 6.3 data are adjusted using International Comparison Program (ICP) prices collected between 1970 and 1996. The PWT 7.1 data are adjusted using ICP prices collected in 2005. The PWT 8.1 na data are adjusted using ICP prices collected in 2005 and 2011 [Feenstra, Inklaar, and Timmer, 2015b].

Breton [2012] shows that the 2005 investment prices used in PWT 7.0 for lower-income countries are very different from earlier prices. Breton and Garcia [2016] and Breton [2016] present evidence that the ICP 2005 and 2011 methodologies underestimate construction prices in lower-income countries. As a consequence, growth models estimated using PWT 7.1 and 8.1 data exhibit very different results than models estimated using PWT 6.3 data.

Most of the analyses in the literature use PWT versions 6.1 to 6.3. For this reason, as well as for the more accurate estimates of construction prices, we use the PWT 6.3 data as the initial data set for our analysis and then check the robustness of our results by performing the same analyses with the PWT 7.1 and 8.1 data.

The PWT 6.3 and 7.1 data series have the same variables and the same countries. PWT 8.1 has different variables and a different set of countries, so we use the data from these series in different ways. With the data in PWT 6.3 and 7.1, 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 to 25 years because the investment data begin in 1950. The PWT 8.1 na data do not include the investment rate variable, but they include estimates of the physical capital stock, so we use these stock estimates in our analysis.

We create the experience-weighted human capital data from the Barro and Lee [2015] over-25 data set for the average schooling of the population during 1975-2005. These data are excellent for our purpose because they include the average schooling attainment and the size of each five-year age cohort for the population between the ages of 15 and 75+ in five-year intervals. These data enable us to calculate the experience-weighted level of human capital in each country as each five-year cohort increases its productivity with experience on the job. We use the schooling data for the five-year cohorts from age 20 to age 64 to represent the schooling of the work force, since the earnings data in Figure 1 indicate that most individuals enter the work force prior to age 25.

Since other studies assumed that the effect of schooling was independent of experience, they were not particularly concerned about the age group used to compare the effect of attainment across countries. These studies use data for the average schooling attainment in the population over age 15 or over age 25, since the existing data sets provide average attainment data for these age ranges. Since we are estimating the longitudinal effect of changes in schooling on GDP over short periods, it is critical for our analysis that we utilize a range of cohorts that matches the age of the work force. Since some individuals are still in school at age 20, our estimates of the changes in the schooling of workers between 20 and 25 have some measurement error. But if we excluded this cohort from the analysis, the measurement error in the schooling data would be much greater.

In creating our data for each country, we estimate each five-year cohort's human capital as the fraction of its eventual maximum human capital, assumed to occur in the age 60-64 cohort. Each country's average human capital/adult is its experience-weighted average level of schooling, with the experience fraction set equal to 1.0 in the age 60-64 final cohort and the fractions in the younger, less-experienced cohorts less than 1.0.

The Barro and Lee [2015] data provide the size and average schooling of each five-year population cohort, which is not the same as the size and average schooling of the cohort in the work force. Since we do not have the information required to create estimates for the work force, we assume that the work force has the same schooling distribution as the adult population. This clearly creates measurement error in the data, as does the failure to adjust for labor participation and unemployment rates.

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 in 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 three panels of data with two compositions. The data sets created using PWT 6.3 and 7.1 data are unbalanced panels that include 98 countries. The appendix contains a list of these countries. Our three middle-income countries are in the middle of the income distribution for these countries, with two above (Ecuador and Uruguay) and one below (Paraguay) the median of GDP/adult in 1980. 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 these panels have capital stock data in 1975 and only 66 have these data in 1980. The data are complete for 1985-2005. The data set created from PWT 8.1 is a balanced panel of 94 countries for the full period 1975-2005.

Our experience-weighted patterns for the human capital of the work force are taken directly from the workers' earnings pattern in Figure 2. The human capital in each five-year cohort of workers from 20 to 64 in this pattern is 50, 59, 68, 76, 82, 88, 93, 97, and 100 percent of the human capital in the age 60-64 cohort.

Since the experience-weighting adjustment reduces the average years of schooling in the first eight cohorts of the work force, the average values for this measures are lower than a country's average years of schooling attainment. Over the 1980 to 2005 period, the average schooling attainment measure has a mean of 6.79 years, while the experience-weighted measure has a mean of 4.64 years. The lower values of the experience-weighted measure increase the estimated effect of a year of schooling in the regressions that use this measure. In our interpretation of the results using these measures, we adjust the average effect of a year of schooling downward by a factor of 0.68 so that the effect of an additional year of average schooling on GDP is comparable for both measures.

Economic time series typically are non-stationary of degree one. Although the number of time periods in our panel is small, the time series could have unit roots, which could create bias in the estimated coefficients. Since our interest is in examining whether changes in schooling affect GDP over five-year periods, we estimate our model in five-year differences. This differencing has the added benefit that it eliminates any linear 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 them first with OLS and then with 2SLS using instruments created from lagged values of the difference in physical capital/adult and the difference in our experience-weighted schooling variable. Our instrument for the change in the physical capital/worker over a five-year period is the change in this variable over the prior five-year period. Conceptually, the growth in GDP during a five-year period is unlikely to cause growth in the physical capital stock during a period five years earlier, so this instrument complies with the exclusion restriction.

Our instrument for the change in experience-weighted schooling is the five-year lag of this variable. This variable is highly correlated with current changes in experience-weighted schooling, but it is a problematic instrument. Since it contains changes in workers' schooling in cohorts that affect GDP directly, it may not comply with the exclusion restriction. We address this problem in the next section.

IV. Results

Table 2 presents estimates of the model with the PWT 6.3 data. Columns 1-2 present the results with average schooling attainment. Columns 3-6 present the results with the experience-weighted schooling measure. Five-year changes are denoted D, while five -year lags of these variables are denoted L5.D.

Table 2
Effect of Changes in Schooling on Changes in GDP with PWT 6.3 Data
[Dependent variable is D.log(GDP/adult]

	1	**2**	**3**	**4**	**5**	**6**
Technique	OLS	2SLS	OLS	2SLS	2SLS	2SLS
Instruments		L5.DKA		L5.DKA	L5.DKA	L5.DKA L5.DSch
Observations	515	417	515	417	417	417
Variable	**Schooling**		**Experience-Weighted Schooling**			
DLog(K/A)	0.48* (.06)	0.30* (.08)	0.47* (.04)	0.28* (.09)	0.28* (.09)	0.28* (.09)
DSchool	.031** (.014)	.023 (.014)	.062* (.021)	.063* (.019)	.062* (.019)	.065 (.046)
L5.DSch					.002 (.021)	
Constant	.008 (.011)	.022 (.012)	-.001 (.011)	.008 (.012)	.008 (.014)	.007 (.021)
R^2	.26	.22	.27	.22	.22	.22
α	.48*	.30*	.47*	.28*	.28*	.28*
Adj. γ^a	.03**	.02	.04*	.04*	.04*	.04
TFP growth	.008	.022	-.001	.008	.008	.007

*Statistically significant at the 1 percent level.
**Statistically significant at the 5 percent level.
[a]The average value of the experience-weighted measure is 0.68 of average schooling attainment.

Note: Robust standard errors in parentheses

In each column we summarize the three key coefficients for each model; α, the adjusted γ, and the average TFP growth rate. Reasonable results require feasible values for these estimated coefficients. In particular, α should be about 0.35, and the TFP growth rate should be positive. Breton [2013b] estimates that annual TFP growth was in the range of .003 to .005 over the 1910-2000 period. Since micro earnings studies show that each year of schooling raises workers' earnings about 7% in high-income countries and 11% in low-income countries, and workers' earnings are about 65% of GDP, we expect adjusted γ to be in the 5-7% range if schooling has no external effects.

Column 1 shows the OLS results with the standard schooling measure. The value of α is clearly too high in these results, while the value of adjusted γ is low (.03). Since investment in physical capital affects the stock so quickly, it is likely that changes in the physical capital stock are endogenous with changes in GDP over five-year periods.

Column 2 shows 2SLS results with Dlog(K/A), instrumented with a five-year lag of this variable (L5.DKA). In these results the value of α is reasonable (0.30), while the value of γ declines and loses statistical significance. The Durbin and Wu-Hausman tests for this model clearly reject the null that DKA is exogenous. The empirical results showing no statistically-significant effects from increases in schooling are consistent with the results in the literature.

Columns 3 and 4 present the results for the same two regressions using the experience-weighted schooling variable. As mentioned earlier, when the model is estimated with experience-weighted schooling, the estimate of γ must be adjusted to account for the lower average value of this measure compared to average schooling attainment.

The effect of changes in physical capital is similar in these regressions, but the adjusted effect of changes in schooling (D.School) is larger (.04) and statistically significant at the 1% level in both the OLS and 2SLS results. The 2SLS model provides acceptable values of α (.28) and the TFP growth rate (.008). The adjusted effect of additional schooling is somewhat smaller than its average effect on workers' earnings across countries (.065), but the effect on GDP is similar to its effect on workers' personal income, both in magnitude and in its lagged pattern.

The experience-weighted D.School variable is unlikely to be endogenous, since it is created from increases in average adult schooling attainment during the prior 45 years, the investment to school these adults occurred even earlier, and the weights are largest on the oldest cohorts. Nevertheless, Bils and Klenow [2000] argued that prior schooling could be endogenous in an income model if investments were made in anticipation of future increases in income.

Bils and Klenow employed their reverse causality argument to explain why cross-sectional estimates of the effect of additional schooling on GDP are *larger* than the effects on workers' earnings. In our case their argument does not apply, since our estimates of the effect of additional schooling on GDP are *smaller* than the effect on workers' earnings. In the context of five-year changes in GDP, we think the possibility that experience-weighted schooling is endogenous is extremely unlikely, but we have attempted to test for this possibility.

In theory if the change in human capital is estimated correctly in our experience-weighted schooling variable, then including lagged values of this variable in the model would not have any additional effect on GDP and when used as instruments, these lagged values would comply with the exclusion restriction. We can test whether a five-year lag of this variable affects GDP by including this variable in an OLS estimate of the model.

Column 5 shows the results. The coefficient on the five-year lag of the experience-weighted schooling variable is very small and not statistically significant at all. In addition, the estimates of the other coefficients are virtually unaffected, as is the R^2.

Column 6 shows the 2SLS model using this lagged variable (L5.DSch) and L5.DKA as instruments. The estimated coefficient on the experience-weighted schooling variable is not statistically significant, but its magnitude is similar to the estimate with the un-instrumented variable. Durbin and Wu-Hausman tests of the endogeneity of the D.School variable provide strong evidence that the experience-weighted schooling variable is exogenous. The schooling instrument is strong with an F value of 52.

We interpret these tests and the consistent estimate of the effect of experience-weighted schooling on GDP (.04 after adjustment) to provide strong evidence that experience-weighted schooling is exogenous, that increased schooling attainment does raise GDP, but that half of the effect is lagged. In addition, the similarity of the coefficients on D.School in the OLS and 2SLS estimates provide no evidence that the OLS estimates suffer from attenuation bias due to random measurement error. The results provide evidence that on average over 40 years an additional year of schooling (in cohort 20-24) raises GDP by about 4%, while the weights on the cohorts indicate that the effect is 3% in the first five years, rising to 6% over the following 40 years.

4.1 Results with Other Data Sets

Table 3 presents the results for the same models in columns 1-6 using the PWT 7.1 data. The results and the statistical tests have similar patterns. The effect of a year of average schooling attainment when D.log(K/A) is instrumented is small and statistically insignificant. The effect of a year of experience-weighted schooling is larger and statistically significant at the 1% level. In these results the 2SLS estimate of the effect of an additional year of schooling on GDP/adult is 5% when physical capital/adult is instrumented.

The adjusted effect of an additional year of schooling rises to 6% when the experience-weighted schooling variable is instrumented with a five-year lag of this variable, but this effect is not statistically significant at the 5% level. The endogeneity tests provide results that are similar to the PWT 6.3 results, and the F tests indicate the instruments are strong.

The similarity of the estimated effect of experience-weighted schooling with the PWT 6.3 and 7.1 data sets is reassuring. Nevertheless, the overall empirical results with the PWT 7.1 data are less satisfactory, since the explained variation in the growth rates (R^2) in all the models is considerably lower, and the estimated effect of physical capital (α = 0.23) is far below its expected value. These less satisfactory estimates of the augmented Solow model are consistent with the analyses cited earlier that identify problems with the PWT 7.1 data.

Table 3
Effect of Changes in Schooling on Changes in GDP with PWT 7.1 Data
[Dependent variable is D.log(GDP/adult]

	1	**2**	**3**	**4**	**5**	**6**
Technique	OLS	2SLS	OLS	2SLS	2SLS	2SLS
Instruments		L5.DKA		L5.DKA	L5.DKA	L5.DKA L5.DSch
Observations	515	417	515	417	417	417
Variable	**Schooling**		**Experience-Weighted Schooling**			
DLog(K/A)	0.44* (.04)	0.26* (.07)	0.43* (.04)	0.24* (.08)	0.23* (.08)	0.23* (.08)
DSchool	.025 (.013)	.025 (.013)	.057* (.017)	.066* (.018)	.063* (.018)	.080 (.048)
L5.DSch					.008 (.022)	
Constant	.012 (.010)	.023 (.010)	.002 (.010)	.008 (.010)	.006 (.013)	.003 (.021)
R^2	.23	.18	.24	.18	.18	.18
α	.44*	.26*	.43*	.24*	.23*	.23*
Adj. γ^a	.03	.02	.04*	.05*	.05*	.06
TFP growth	.012	.023	.002	.008	.006	.003

*Statistically significant at the 1 percent level.
**Statistically significant at the 5 percent level.
[a]The average value of the experience-weighted measure is 0.68 of average schooling attainment.

Note: Robust standard errors in parentheses

Table 4 presents the analogous results for the same models using the PWT 8.1 data. The estimates with the PWT 8.1 data have patterns that are similar to the results with the other data sets, but different in several important respects. First, the explained variation in the growth rates (R^2) is much higher than in either of the other two data sets. Second, the effect of changes in physical capital on growth is larger. Third, with these data the effect of changes in average schooling attainment (column 2) is positive and statistically significant at the 5% level. An additional year of schooling raises GDP by 2% over each five-year period.

Table 4
Effect of Changes in Schooling on Changes in GDP with PWT 8.1 Data
[Dependent variable is D.log(GDP/adult]

	1	**2**	**3**	**4**	**5**	**6**
Technique	OLS	2SLS	OLS	2SLS	2SLS	2SLS
Instruments		L5.DKA		L5.DKA	L5.DKA	L5.DKA L5.DSch
Observations	564	564	564	564	564	564
Variable	**Schooling**		**Experience-Weighted Schooling**			
DLog(K/A)	0.58* (.04)	0.32* (.06)	0.57* (.04)	0.31* (.07)	0.31* (.07)	0.32* (.07)
DSchool	.012 (.012)	.024** (.012)	.032** (.016)	.057* (.018)	.061* (.018)	.044 (.035)
L5.DSch					-.009 (.019)	
Constant	.005 (.009)	.018** (.009)	-.001 (.009)	.008 (.009)	.010 (.010)	.013 (.015)
R^2	.34	.28	.35	.28	.28	.28
α	.58*	.32*	.57*	.31*	.31*	.32*
Adj. γ[a]	.01	.02**	.02**	.04*	.04*	.03
TFP growth	.005	.018	-.001	.008	.010	.013

*Statistically significant at the 1 percent level.
**Statistically significant at the 5 percent level.
[a]The average value of the experience-weighted measure is 0.68 of average schooling attainment.

Note: Robust standard errors in parentheses

When we replace average schooling attainment with experience-weighted schooling, the (average) effect of an additional year of schooling doubles to 4% and this estimate is significant at the 1% level. The post-estimation tests confirm the earlier results with the PWT

6.3 data set. The DKA variable is endogenous, the experience-weighted schooling variable is exogenous, and the instruments are strong.

Overall with these three PWT data sets, the effect of an additional year of experience-weighted schooling is 4-5% on average, with effects that range from 3% during the initial five-year period to 6-7% after 40 years. These effects are consistently significant at the 1% level. The other estimated parameters are acceptable, except in PWT 7.1 where the effect of physical capital (α) is consistently too low.

These estimates of the average effect of an additional year of schooling are lower than the expected value of 6.5%. The TFP growth rates are relatively high in all the regression results, suggesting that the effect of schooling is underestimated. In a sensitivity analysis (not shown), we found that productivity patterns that are lower initially (below 50% in the 20-24 age cohort) and rise more slowly provide larger estimates of the average effect of additional schooling on GDP growth. These estimates are closer to 6.5% and still yield a positive TFP growth rate. We have not shown these results because they do not have a micro foundation related to workers' earnings and could be spurious.

4.2 Results for Subsets of the Data

Table 5 examines whether there are non-linearities in the effect of schooling on GDP as a function of the level of GDP/worker. All of the models are estimated with 2SLS with an instrument only for the D.log(K/A) variable.

The first column shows the 2SLS results for the full PWT 6.3 data sample from Table 2 for comparison purposes. Columns 2 and 3 show the results for two subsamples of these data for the low-income and high-income countries, separated at $13,581/adult (2005 USD). When the data set is divided, the samples are smaller and the variables have less variation. As a result, the effect of physical capital is lower and less statistically significant in both subsamples. In the low-income countries, the adjusted effect of schooling is similar to the effect in the full sample but not statistically significant. In the higher-income countries, the effect of schooling is smaller than in the full sample (.03) but not statistically significant.

Columns 4 and 5 show the results for the subsamples using the PWT 7.1 data. The empirical results are similar to the results with the PWT 6.3 data, but the effect of physical capital is lower and the effect of experience-weighted schooling is higher and statistically significant at the 5% level.

Columns 6 and 7 show the results for the two income groups using the PWT 8.1 data. As with the full data set, these PWT 8.1 subsamples are larger than the subsamples using the PWT 6.3 and 7.1 data. The effect of physical capital is larger and statistically significant in both subsamples. The effect of a year of experience-weighted schooling is larger (5%) and significant at the 5% level in the low-income group and smaller (only 2%) and not significant in the high-income group. For all the data sets the lower effect of additional schooling in the higher-income countries is consistent with the smaller relative effect of an additional year of schooling on workers' earnings in these countries (7% vs. 11%).

Table 5
Effect of Schooling on GDP in Countries at Different Levels of GDP/Adult
[Dependent variable is D.log(GDP/adult]

	1	**2**	**3**	**4**	**5**	**6**	**7**
PWT series	6.3	6.3	6.3	7.1	7.1	8.1	8.1
Observations	417	209	208	209	208	300	264
Technique	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Instrument	L5.DKA	L5.DKA	L5.DKA	L5.DKA	L5.DKA	L5.DKA	L5.DKA
Sample	**All**	**Lo Inc**	**Hi Inc**	**Lo Inc**	**Hi Inc**	**Lo Inc**	**Hi Inc**
D.Log(K/A)	0.28* (.09)	0.23 (.12)	0.24 (.12)	0.19 (.11)	0.20 (.12)	0.30* (.09)	0.23** (.12)
D.School	.063* (.019)	.066 (.038)	.039 (.021)	.080** (.034)	.040** (.020)	.074** (.033)	.032 (.019)
Constant	.008 (.012)	-.014 (.020)	.044* (.013)	-.015 (.017)	.043* (.013)	-.010 (.015)	.044* (.013)
R^2	.22	.16	.19	.14	.16	.27	.20
α	.28*	.23	.24	.19	.20	.30*	.23
Adj. γ[a]	.04*	.05	.03	.06**	.03**	.05**	.02
TFP growth	.008	-.015	.044*	-.015	.043*	-.010	.044

*Statistically significant at the 1 percent level.
**Statistically significant at the 5 percent level.
[a]The average value of the experience-weighted measure is 0.68 of average schooling attainment.

Note: Robust standard errors in parentheses

The other result in these analyses is that TFP growth is much higher in the high-income group than in the low-income group. The TFP growth rate is over 4%/year in the high-income countries, while an additional year of schooling increases GDP by only 2-3%. In these countries the additional schooling is at the post-secondary level. We suspect that the composition of this schooling may be important for explaining growth. Since this information is not in the model, its effect appears in the residual TFP growth rate, along with all other unexplained causes of growth.

In the low-income countries, the issue is how to explain the negative TFP growth rate. One interpretation is that even though additional schooling raised growth rates, the low average level of schooling (or other characteristics) in these countries prevented them from taking complete advantage of the skill-based productivity improvements occurring in the high-income countries.

The effects of an additional year of schooling on GDP in these various estimates are somewhat lower than the effects reported in workers' earnings studies. There are numerous potential explanations for this difference. One possibility is that since the estimates in workers' earnings studies are cross-sectional, our lower effects could be due to changes in the cross-sectional earnings patterns over time.

Another possibility is that measurement error in the schooling data is causing attenuation bias in our estimates of the effect of increased schooling. This does not seem likely, however, since the estimates of the effects of the instrumented D.School variable using the full data sets are similar to the un-instrumented effects.

Another possibility is that the PWT economic data have measurement error due to the adjustments made to equalize purchasing power (PP) across countries. If the adjusted levels of GDP or physical capital are mis-estimated in a non-random manner, the estimated effects of changes in physical capital and/or schooling on GDP would be biased in our regressions. The very different estimates of the effect of physical capital in the different PWT data sets strongly indicate that errors in the PP-adjustment process are affecting the results.

Another likely possibility is that the relationship between schooling and workers' earnings in Figure 2 is not an accurate representation of this relationship in the 98 countries in over the growth period. Even in the three countries used to estimate the relationship, the data only include the workers in the formal labor market, so it may not be an accurate representation of the productivity pattern in the full work force.

V. Comparison with Cross-Sectional Estimates

Earlier we observed that cross-sectional estimates of the relationship between schooling and GDP in the literature are larger than the estimates of the effect of additional schooling on workers' personal earnings. These cross-sectional estimates often are not comparable because their magnitude is affected by the form of the growth model and the particular human capital data used in the estimation.

Reduced forms of the production function and models that do not include physical capital provide larger estimates of the effect of schooling on GDP. The estimates from these models must be adjusted to compare them to the effect of schooling in the standard production function.

The estimated production functions in the literature using schooling attainment data are typically in two forms, the standard form shown in (5) and a reduced form that is a function of the capital/output ratio:

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

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

Some analyses omit the physical capital variable altogether. These models are mis-specified unless they have explanatory variables that substitute for physical capital. The estimated coefficient on schooling in these models is biased upward quite substantially because schooling is highly correlated with the missing physical capital variable. In these models a rough estimate of the coefficient on schooling is 2γ. Mankiw, Romer, and Weil [1992]] showed that when one type of capital is excluded from the production function in a cross-sectional analysis, the estimated coefficient on the remaining capital variable approximately doubles.

Breton [2013a and 2015] used financial measures of human capital stocks and flows to estimate the production function. These measures correspond to the standard production function in (2) or to a dynamic version of this model. These models produce an estimate of β that must be converted to a comparable estimate of the effect of average schooling attainment.

Table 6 presents the estimated coefficients on schooling in five recent cross-sectional estimates of the effect of increased schooling on GDP.[7] 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. All of these estimates implicitly estimate the long-run effect of schooling, but they differ in whether they control for endogeneity and for the effect of institutions. Studies in differences implicitly control for the relatively constant effect of institutions on growth, while studies using instruments and 2SLS control for the potential endogeneity of schooling in the growth or income model.

[7] Sunde and Vischer [2015] obtain slightly higher estimated values of γ, but their results are not comparable to the others because they include additional variables in their growth model.

Table 6 Estimated Coefficients on Schooling in Cross-Sectional Income Models						
Study	**Period**	**Method**	**Coefficient Estimated**			**Implied**
			β	**γ/(1-α)**	**2γ**	**γ**
Gennaioli, et. al, 2013	2005	OLS			.24*	.12
Cohen & Soto, 2007	1960-90	Δ/2SLS		.13		.08
Breton, 2013a	1990	2SLS	.36			.12
Breton, 2013b	2000	2SLS		.16		.10
Breton, 2015	1985-2005	OLS	.28			.09

*Reduced from 0.28 to account for their lower measure of average schooling attainment.

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 could be about double the implied value of γ, which would be 0.12.Breton [2013a and 2015] estimates the effect of human capital (β) on GDP, rather than the effect of years of schooling (γ), but Breton [2013a] presents evidence that for the Cohen and Soto [2007] schooling data, β/γ = 0.32. Using this conversion ratio his estimates of the effect of a year of schooling are 0.09 and 0.12.

Cohen and Soto [2007] and Breton [2013b] estimated the reduced form model in (6), so their estimated coefficients are higher by a factor of 1-α. The implied values of γ in these estimates (.08 and .010) are only slightly larger than the estimates in workers' earnings studies.

So while estimated coefficients in the recent literature vary considerably, after adjustment the implied values of γ in these cross-sectional estimates range from 0.08 to 0.12. These estimates are larger than the equivalent cross-sectional estimates of the effect of schooling on workers' earnings (.05 to 0.07) and larger than the longitudinal estimates in this study (.04 -.05), so they imply that the additional schooling has substantial external effects on GDP.

Acemoglu, Gallego, and Robinson [2014] argue that cross-sectional estimates of the effect of increased schooling on GDP are biased upward because they include effects that should be attributed to differences in institutions. Our estimates of the effect of additional schooling in this study exclude any effects actually due to institutions in the cross-sectional estimates because differencing eliminates the effect of relatively stable institutions. Our estimates support Acemoglu et al.'s contention.

Nevertheless, it is possible that our long-term effects of schooling on GDP, which occur after 45 years, may not capture the full eventual effect of additional schooling. We doubt that the characteristics of a country's institutions are entirely independent of a country's level of schooling. It may be that increases in average schooling attainment may be a necessary but insufficient condition for raising the quality of a nation's institutions, even if this process is considerably lagged.

VI. Conclusions

For over 25 years researchers have sought but have been unable to find a positive 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 use micro data from middle-income countries to answer this question.

The existing neoclassical analyses assume that the entire effect of additional schooling on GDP is immediate. We examine whether lags in the effect can explain the difference between the short and long run estimates in the literature.

We first show that workers' earnings in three middle-income countries only increase with experience if the workers have prior schooling. We conclude that increases in worker productivity on the job are at least partly a delayed effect of their prior schooling.

We then examine whether the pattern of increases in worker productivity in these middle-income countries characterizes the relationship between increased schooling and GDP in 98 countries. We find that this pattern explains a portion of GDP growth during five-year intervals over the 1980-2005 period.

The implication of our analysis is that the increase in GDP during a five-year period is caused by the increase in the average schooling attainment of the population during the prior 45 years. We find that an additional year of schooling in the age 20-24 cohort eventually raises GDP by 6-7% after 45 years, but the effect in the initial five-year period is only 3%, and the average effect over 45 years is only 4-5%.

Since average schooling typically increases by less than a year over a five-year period, the near-term effect of an increase in a country's average level of schooling attainment is quite small. So this is where all the education went. It had a positive effect on GDP, but this effect occurred over a very long period of time.

We also examine the analogous estimates of the effect of additional schooling in cross-sectional analyses and show that they are not as large or as varied as they appear to be, once adjustments are made to account for differences in the structure of the model used in their estimation. Our adjusted cross-sectional estimates indicate that in the long term an additional year of schooling is associated with an increase in GDP of 8-12%.

It seems likely that the larger cross-sectional estimates are biased upward because they include effects on GDP that are due to cross-country differences in institutions. Still, it seems unlikely that increases in schooling have no effect on a country's institutions, so the effect of schooling could be larger than what we found in our analysis. Additional research is required to further reconcile the longitudinal and cross-sectional estimates of the effects of additional schooling on GDP in countries with differing institutional characteristics.

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Appendix

98 Countries Included in the Analysis

Algeria	Greece	Pakistan
Argentina	Guatemala	Panama
Australia	Haiti	Papua New Guinea
Austria	Honduras	Paraguay
Bangladesh	Hong Kong	Peru
Barbados	Iceland	Philippines
Belgium	India	Portugal
Benin	Indonesia	Rwanda
Bolivia	Iran	Senegal
Botswana	Ireland	Sierra Leone
Brazil	Israel	Singapore
Burundi	Italy	South Africa
Cameroon	Jamaica	South Korea
Canada	Japan	Spain
Central Afr Republic	Jordan	Sri Lanka
Chile	Kenya	Sweden
Colombia	Lesotho	Switzerland
Congo	Luxembourg	Syria
Costa Rica	Malawi	Taiwan
Cote d`Ivoire	Malaysia	Tanzania
Cyprus	Mali	Thailand
Dem Rep of the Congo	Mauritania	Togo
Denmark	Mauritius	Trinidad &Tobago
Dominican Republic	Mexico	Tunisia
Ecuador	Morocco	Turkey
Egypt	Mozambique	Uganda
El Salvador	Namibia	UK
Fiji	Nepal	Uruguay
Finland	Netherlands	USA
France	New Zealand	Venezuela
Gabon	Nicaragua	Zambia
Gambia	Niger	Zimbabwe
Ghana	Norway	