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INCOME AND SCHOOLING

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ABSTRACT

Income and schooling

Whereas existing literature has documented strong correlations between national incomes and measures of schooling attainment, causality has been hard to pin down. Much of empirical work had tended to interpret these correlations as implying an effect of human capital on national income, but recent calibrated models have argued that most of the link works, in fact, the other way around. In this paper, therefore, we take a close look as to whether income growth causes schooling from an empirical perspective. We do so by focusing on within-country variation and using instrumental variables estimation to extract exogenous variation in countries' national incomes. We detect a significant causal effect of income growth on various measures of schooling attainment, more so in poor countries.

JEL Classification: I2 and O4
Keywords: national income and schooling attainment

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1. Introduction

There exist significant correlations, well documented, among others, in Barro (1991), Barro and Sala-i-Martin (1995), Benhabib and Spiegel (1994), Cohen and Soto, 2007, De la Fuente and Domenech, 2006, between schooling and economic growth in cross country data. As an illustration, Figure 1 plots against each other income per capita and secondary schooling attainment across countries in 2007, as well as over the entire period covered in this study, which clearly shows a close association between the two measures. A similar picture emerges when one of the variables is lagged, or for other measures of schooling.

Despite the strong positive correlations between income and schooling, identification obstacles have made it hard to disentangle the causality of this relationship. Does schooling cause growth, or is it the other way around? Are both income and schooling driven by other factors? As a consequence of the difficulty to establish causality with regression analysis, recent studies have been carried out using calibration techniques in the context of development accounting.[1] Bils and Klenow, 2000, in their calibration analysis, find that most of the above correlation (more than two thirds of it) is accounted for by the causal link from income growth to schooling, and the remaining part is due to the causal effect of schooling on growth. More recent calibration analyses, Cordoba and Ripoll, 2011, Restuccia and Vandenbroucke, 2011, also seem to support this.

1 See, for example, Bils and Klenow, 2000, Hall and Jones, 1999; Caselli and Ciccone, 2012, Cordoba and Ripoll, 2011, Gennaioli et al., 2012, and Resticcia and Vandenbroucke, 2011, are even more recent examples.

In this research, our goal is to empirically assess the causal link from income to schooling, identified in Bils and Klenow, 2000, and others as potentially important. To do so, one needs an exogenous and permanent source of variation in national income, in the context of panel cross country data that has been widely employed in this context. Consequently, we use oil price shocks, defined as the change in the log of the international oil price multiplied by countries' average net-export shares of oil in GDP, as an instrument for countries' national income, to identify such causal link. Most of the countries in our sample are price takers on the international oil market, so that variations in the international oil prices constitute a plausibly exogenous source of within-country variation in countries' national income. Moreover, as annual variation in the international oil price follows a unit root process, the oil price instrument captures variations in national incomes that are of permanent nature.

Schooling measurement issues have plagued the literature on education and growth since its beginning. In accordance with much of this literature, we use as our benchmark schooling attainment measure the share of population with secondary schooling. We also consider other measures of schooling attainment, such as, male vs. female enrollment, public and private expenditures on schooling, literacy rates, primary and tertiary school enrollment as well as average years of schooling based on Barro and Lee's (2010) dataset.

Our analysis of cross country panel data, which stretches over almost two generations, reveals that essentially in all specifications the instrumented income growth has a significant positive effect on schooling. For example, our point estimates indicate that a one percent increase in GDP per capita increases the secondary school enrollment rate by over 0.1 percentage points (or, around 0.2 percent) on average. We, further, find that these effects are larger for poor than for richer countries, particularly so for female enrollment.

It is important to note that our instrumental variables estimates are based on an instrument that is highly persistent (oil price shocks). Our instrumental variables estimates therefore capture the effects of permanent shocks to national income on schooling attainment. Different types of income shocks (transitory vs. permanent) are likely to have different effects on the incentives for acquiring additional years of schooling. In particular, shocks that have long-lasting effects on income are likely to have larger effects on schooling than transitory shocks. Our finding of a significant positive effect of permanent income on schooling thus does not contradict the finding in Méndez and Sepúlveda (2012) that skill acquisition is counter-cyclical.

All in all, our results reinforce Bils and Klenow's, 2000, suggestion that "growth causes schooling".[2] This should matter for studies that aim at uncovering long-run growth effects of education, as it suggests a potentially upward bias in existing estimates to the extent that they do not fully account for the endogeneity of schooling. Further, our estimates can be hopefully useful for work in development accounting that links schooling and income growth. As we are dealing here with a particular channel of income growth, our results are consistent with the work of Duflo, 2001, 2004, where in the context of Indonesia it is found

2 More recent calibration studies, such as Cordoba and Ripoll, 2011, and Resstuccia and Vandenbroucke, 2011, also find that income or productivity growth is an important explaining factor for schooling differences.

that oil revenues and the resulting economic boom in the aftermath of the 1973 oil shock have generated a massive expansion of schooling. Whereas Duflo, 2001, 2004 is concerned with the effects this expansion had on the composition of wages and individual returns to schooling, we are interested here in identifying the cause of this expansion.[3] Also related are Caselli and Michaels, 2012, where it is found that oil windfalls in Brazil generate an increase in spending on education (while not necessarily raising living standards, however); and Michaels, 2011, where it is shown that countries in the Southern US that benefited from oil discoveries enhanced the educational quality of their labor force. Acemoglu et al., 2012, employ oil price shocks to explore the effects of income on health in the context of the US states and is methodologically related.

Assessing the magnitude of the extent to which income growth drives schooling is not only important as a reverse causality mechanism behind the education and growth relationship, thereby potentially contributing to further studies of development accounting. Education is widely believed to be an essential ingredient of modern civility. For example, Lipset's, 1959, modernization hypothesis, stipulates that educated citizenry is essential for democracy, and Glaeser et al., 2007, reinforce this hypothesis; Verba, 1972, and Verba et al., 1995, stipulate that education makes voting more likely, and Milligan et al., 2004, provide support for the causal relationship. Botero et al., 2012, argue that educated citizenry improves

3 Duflo, 2004, dealing with the general equilibrium effects of schooling, comes up with somewhat ambiguous results.

the quality of the government. Because of these links, it is important to explore if education is a by product of economic prosperity.

We proceed as follows. The next section presents a simple framework to organize thoughts on the relationship between income growth and education. This is followed by data description, in Section 3. Section 4 contains the discussion of our estimation approach and presents the main results, which are then further extended in Section 5. Section 6 illustrates how our results can be used in the context of development accounting calculations. Finally, Section 7 concludes with brief remarks.

2. Analytical Framework

To illustrate, consider an economy that operates in discrete time periods t and is populated in each period by a measure one of identical successive households, each consisting of a parent and a child. Initial income, y_0 is given. In each period, income y_t has to be divided between consumption spending c_t and schooling spending, s_t implying the budget constraint of

$$y_t = c_t + s_t \tag{2.1}$$

Human capital h_{t+1} is produced via the production function[4]

$$h_{t+1} = q_t\, s_t, \tag{2.2}$$

4 We assume for simplicity that a dollar of spending generates one unit of schooling attainment, so that schooling spending and attainment are equivalent.

where q_t is interpreted as the quality of schooling, i.e., the way, schooling expenditure gets translated into effective, economically relevant, schooling outcomes. q_t conceivably hinges upon the school environment, such as teachers' quality and allocation efficiencies within the education system; but it also depends on factors that are not directly related to schools, such as parental educational input, or allocation efficiencies in the labor market that, in turn, are reflected in the education system.[5] Schoellman, 2012, in his calibration analysis, finds that education quality is particularly important for growth accounting. Income is produced using human capital,

$$y_t=A_t h_t^{\eta},\ A_t> 0,\ 0<\eta\leq 1 \qquad (2.3)$$

where A_t could in principle depend on the aggregate level of human capital, among other factors.[6]

Assuming the Stone-Geary isoelastic utility function from family consumption and human capital of the child, we write

$$U(c_t,h_{t+1}) = (c_t-\underline{c})^{1-\sigma}/(1-\sigma) + \beta(h_{t+1})^{1-\sigma}/(1-\sigma),\ \sigma\neq 1 \qquad (2.4)$$

5 See Pritchett, 2006, for convincing argumentation as to the importance of schooling quality and on its various interpretations.

6 Individuals' abilities could easily be incorporated into this framework, but for our purposes they are immaterial.

$$\ln(c_t - \underline{c}) + \beta \ln(h_{t+1}),\ \sigma = 1$$

where $\beta>0$, $1-\sigma$ is the elasticity and $\underline{c}$ is the subsistence requirement, and $c_t > \underline{c}$.

Utility maximization subject to the budget and the production constraints yields:

$$s_t = (y_t - \underline{c})\beta^{1/\sigma} q_t^{(1-\sigma)/\sigma},\ \text{if } y_t > \underline{c}, \text{ and 0 otherwise; } c_t = y_t - s_t, \tag{2.5}$$

which, upon further substitutions yields:

$$h_{t+1} = (y_t - \underline{c})\,(q_t\beta)^{1/\sigma}\ ,\ y_{t+1} = A_{t+1}\,(y_t - \underline{c})^{\eta}\,(q_t\beta)^{1/\sigma} \tag{2.6}$$

It then follows from the differentiation of s_t in (2.5) that the share of education spending out of income increases in the latter.

In particular, we can write:

$$\log(s_t) = \log(y_t - \underline{c}) + (1/\sigma)\log(\beta) + ((1-\sigma)/\sigma)\log(q_t) \tag{2.7}$$

Obviously, in this view, in addition to the direct effect of income, the magnitude of schooling expenditures (and outcomes) is also affected by the quality of schooling spending – positively so if $\sigma<1$, and negatively otherwise. In other words, depending on whether the utility elasticity is positive or negative, quality and the amount of schooling can in principle be either substitutes or complements.[7]

7 In any case, however, from (2.6) human capital, hence, next-period income increases in quality.

Importantly, q_t may likely be affected by factors such as the culture of learning and institutional schooling arrangements which could have an effect on income beyond schooling expenditures. The upshot is that q_t is endogenous, and that some of its components at least are unobservable. In particular, if $\sigma>1$ and the cross country components determining q_t are positively correlated with income, OLS estimation of (2.7) would generate a downward biased coefficient on income.

3. Data

Oil Price Shocks. The data on our oil price shock instrument are from Bruckner et al. (2012). In particular, data on the international oil price for the 1960-2007 period is from UNCTAD (United Nations Conference on Trade and Development) Commodity Statistics, and data on oil exports and imports is from the NBER-United Nations Trade Database. The UNCTAD oil price is an average of Dubai, U.K. Brent and W. Texas, equally weighted, averaged over the year over monthly data.

Because the level of the oil price displays a unit root (the AR(1) coefficient is 0.99; see also Bruckner et al. (2012) or Hamilton (2009) for further analysis), oil price shocks are identified by the change in the log of the international oil price:

$$OilPriceShock_{ct} = \Delta \ln(OilPrice)_t * \theta_c \qquad (3.1)$$

Equation (3.1) takes into account that the impact of a change in the international oil price is larger for countries that are very dependent on oil exports (imports) by weighting the change in the log of the international oil price by the average (i.e. time-invariant) share of net oil exports in GDP θ_c. The average share of net oil exports in GDP is computed as the period average value of oil exports minus imports divided by GDP. The sample maximum (minimum) value of θ_c is 0.18 (-0.03); the mean (median) is 0.009 (-0.001); and the interquartile range is [-0.005, 0.002].

We, furthermore, note that in our empirical analysis we are interested in exploiting variations in the international oil price as a plausibly exogenous shock to income growth. In particular, our instrumental variables analysis, under the assumption that variations in the international oil price are exogenous, does not require distinguishing between demand and supply-side driven changes in the international oil price. We are simply interested in the average marginal response of income growth to variations in the international oil price, and how this response varies depending on whether countries are oil importers or exporters. This stands in contrast to the monetary economics literature (e.g. Kilian, 2009), where distinguishing between demand and supply side shocks is a key issue; optimal monetary policy reaction to oil price shocks is not our focus here.

Schooling. Data on schooling are from the World Development Indicators (2012). We follow the growth literature (e.g. Barro, 1997, and Mankiw et al., 1992) and use the secondary school enrollment rate as our main measure of schooling. The secondary school enrollment rate is defined as the number of individuals enrolled in secondary school divided by the number of individuals who are of the corresponding secondary school enrollment age. The descriptive statistics in Table 1 show that the sample average secondary school enrollment rate is 0.57; the annual average change is 0.01.[8] For males the secondary school enrollment rate is somewhat higher than for females, although the difference is quantitatively not very large (0.03).

The available panel data on schooling expenditures per pupil are much sparser than for school enrollment rates. They cover less than 30 percent of the country-year observations for which data exist on school enrollment. With the data that are available for schooling expenditures, we see that expenditures per pupil are highest for tertiary education (exceeding $5000 per annum); this is followed by secondary school education (exceeding $2500 per annum) and primary school education (exceeding $2000 per annum). All three measures of schooling expenditures increased on average: seven percent, six percent, and two percent per annum for primary, secondary, and tertiary expenditures per pupil, respectively.

Even more sparse than schooling expenditures data are panel data on literacy rates. Literacy rates panel data cover less than ten percent of school enrollment data. For the literacy rates data available, we see that the sample average literacy rate is 0.72; the standard

8 All descriptive statistics are for the largest possible sample, given the availability of data on the oil price shock instrument.

deviation is 0.25. Literacy rates are higher for males than females: 0.66 for females and 0.78 for males.

As a robustness check on the WDI data, we will present results that use the Barro and Lee (2010) data on the average years of schooling of the population. This is a 5-year panel dataset from 1950-2010. The descriptive statistics in Table 1 show that the sample average years of schooling of the population is 5.8 (3.9 for primary schooling; 1.7 for secondary schooling and 0.2 for tertiary). Its five-year change is positive, exceeding 0.1 log points over a five year period (for primary schooling the five-year change amounts to about 0.1 log points; for secondary and tertiary schooling it amounts to about 0.2 log points).

GDP Data. Data on annual real per capita GDP are from the Penn World Table, version 7.1 (Heston et al. 2011).

4. Baseline Estimation

4.1 Framework

Our baseline analysis uses the following econometric model to estimate the within-country effect that changes in income per capita have on changes in schooling attainment:

$$\Delta\, Schooling_{ct} = a_c + b_t + \beta \Delta \ln(GDP_{ct}) + z_{ct} \qquad (4.1)$$

where a_c and b_t are country and year fixed effects; ΔSchooling is the year t-1 to t change in the school enrollment rate; Δln(GDP) is the year t-1 to t change in the log of real GDP per capita. In order to compute standard errors that are robust to arbitrary within-country serial correlation we cluster the error term, z_{ct}, at the country level. The panel comprises 138 countries (see Appendix Table 1 for a list of countries) and in the regression that uses the WDI school enrollment data spans the years 1970-2007.

Our main method of estimation is two-stage least squares. In the two-stage least squares estimation we instrument real GDP per capita by our oil price shock variable. By doing so, we use a plausibly exogenous source of variation in countries' GDP per capita to examine the link between income and schooling. Because year-to-year variations in the international oil prices are very persistent (see the discussion in Section 3), it is important to note that in the two-stage least squares estimation we identify the effects that permanent shocks to GDP per capita have on schooling attainment. The exclusion restriction for the two-stage least squares estimation is that oil price shocks should have no systematic effects on countries' schooling beyond their effects on the GDP. We will discuss and examine this exclusion restriction in detail in the next section.

4.2 Results

Least Squares Estimates

We begin the discussion of our empirical results by presenting, in Table 2, least squares results from a panel regression that relates the level of schooling to the level of GDP per capita. Column (1) shows that, without controlling for country fixed effects, the least squares coefficient on the log of GDP per capita is positive and highly significant. Column (2) shows that this continues to be the case when controlling for the *t*-1 level of schooling enrollment. A naive interpretation of the results in columns (1) and (2) would be that a one percent increase in GDP per capita increases the secondary school enrollment rate by 0.004 percentage points in the short-run, and by 0.2 percentage points in the long-run.

There are several econometric issues with the level specification in columns (1) and (2) of Table 2. First, schooling is a highly persistent variable. The R-squared in column (2) is in excess of 0.99. This is a red light that the relationship between income and schooling may be spurious due to both schooling and GDP per capita having a unit root. Indeed, the Maddala and Wu (1999) panel unit root test does not reject the null hypothesis of a unit root in schooling (p-value 0.82) and GDP per capita (p-value 0.51). The fact that the coefficients on current GDP per capita (Panel A) and lagged GDP per capita (Panel B) are nearly identical further suggests that shocks to GDP per capita are highly persistent and, thus, that unit roots in the level specification lead to spurious relationships.

A second issue with the level specification in column (1) of Table 2 is that it is prone to endogeneity biases. Reverse causality going from more schooling to higher income would imply that the least squares estimates are upward biased. This reverse causality bias could arise both from cross-country variation in schooling and from time-series variation. It may be plausible to assume that the long-run effects of schooling on income are larger than the short-run effects, hence we expect the cross-country reverse causality bias to be larger than the year *t* time-series bias. Endogeneity biases could also arise from omitted variables. These, for example, could be time-invariant variables related to countries' historical and geographical characteristics that affect both income and schooling. Once we control for these fixed country characteristics, see columns (3) and (4), we find that the year *t* effect of income and schooling turns insignificant.

Given the issue of unit roots in income and education as well as the endogeneity bias arising from unobservable time-invariant country-characteristics, our preferred estimating equation relates the within-country change in schooling to the within-country change in income. By taking first-differences, we eliminate fixed country characteristics and obtain time-series that are stationary.[9] We note that panel estimates which relate the year *t* change in schooling to the year *t* change in income reflect the short-run effect of income on schooling. We are, of course, also interested in the longer-run effects that income has on schooling. In order to examine these longer-run effects we include additional lags of GDP per capita growth (up to five years) on the right-hand side of the regression. The dynamic effects of income on schooling are captured in this unrestricted distributed lag regression by the

9 The Madalla and Wu panel unit root tests reject the null of the first-difference in schooling and income containing a unit root at the 1 percent significance level (p-value is 0.0000 for both series).

coefficients on GDP per capita at the different lags. The long-run effect is captured by the sum of the impact and lagged effects.

Table 3 contains the least squares estimates that are based on our baseline first-difference specification. The estimates in column (1) are based on a regression that controls for neither country nor year fixed effects. Columns (2)-(4) show estimates where we sequentially add to the right-hand side of the regression country and year fixed effects. The main result from these regressions is that the year *t* coefficient on GDP per capita is insignificant; however, the lags at year *t*-2 to *t*-5 are positive and significant at the conventional significance levels. This continues to be the case when including as additional regressor the year *t*+1 lead of GDP per capita growth (see column (5)). The year *t*+1 lead is positive but not significantly different from zero (p-value 0.30). Column (6) shows that the least squares coefficient on the year *t-5* to *t* change in GDP per capita, which reflects a change in income over a relatively long-term horizon, is also positive and significant at the 1 percent level.

In Table 3 the significant positive lagged effects on income are unlikely to be biased due to reverse causality: for there to be a reverse causality bias, it would have to be the case that current changes in income are a function of future (predictable) changes in schooling. Still, the least squares estimates in Table 3 may fail to identify causal effects of past income on schooling for a number of reasons.

One reason has to do with the measurement error in GDP per capita growth rates. Classical measurement error will attenuate the least squares estimates towards zero, thus leading to an understatement of the true causal effect that within-country changes in income have on within-country changes in schooling.

A second reason why the least squares estimates may fail to identify causal effects are omitted variables varying at the within-country level. These would have to be variables that affect school enrollment rates beyond their effect on income. One such example of a time-varying omitted variable, that directly follows from the theoretical model presented in Section 2, is the quality of schooling. If the quality of schooling has a positive effect on income, but has a negative effect on school enrollment beyond income (i.e., schooling and its quality are substitutes, which in the model hinges on the elasticity), then the least squares estimate of the income effect on school enrollment is downward biased.

Third, different types of income shocks (transitory vs. permanent) are likely to have different effects on the incentives for acquiring additional years of schooling.[10] In particular, shocks that have long-lasting effects on income are likely to have larger (more positive) effects on schooling than transitory shocks. The least squares estimate is an average effect; thus, endogeneity and measurement error issues aside, the least squares estimate reflects an average of the effects that transitory and permanent shocks to income have on schooling.

10 See Mendez and Sepulveda, 2011, who detect strong countercyclicality in schooling across the business cycle.

All of the above points imply that least squares estimation may well lead to an understatement of the true causal effect that permanent income shocks have on schooling attainment. Importantly, with a valid instrument for permanent income in hand all three issues can be addressed.

Instrumental Variables Estimates

In Table 4 we present the reduced-form effects that the oil price shock instrument has on the secondary school enrollment rate (see Section 3 for a description of the instrument). The structure of Table 4 is exactly the same as that of Table 3. In columns (1)-(4) we show estimates of the impact and lagged effects that oil price shocks have on schooling. We subsequently introduce country and year fixed effects as controls on the right-hand side of the regression. Similar to the least squares estimates of the effects that GDP per capita has on schooling, Table 4 shows that there are significant positive lagged effects of oil price shocks. The year *t* effect is quantitatively small and statistically insignificant. Column (5) also shows that the lead effect of oil price shocks on schooling is quantitatively small and statistically insignificant. Column (6) documents that oil price shocks over a period of five years exhibit a positive effect on schooling; and this reduced-form effect is significant at the 1 percent level.

Resonating with these significant positive reduced-form effects, Table 5 shows that two-stage least squares estimates which use oil price shocks (over a period of five years) as an instrument for GDP per capita growth yield a positive and significant effect on schooling.

We use oil price shocks over a period of five years in order to capture the effects that permanent shocks to GDP per capita have on schooling over a five-year period. The coefficients on GDP per capita in this 2SLS regression range between 0.10 and 0.18. Quantitatively, these point estimates suggest that a one percent increase in GDP per capita increases the secondary school enrollment rate by over 0.1 percentage points (or, of some 0.2 percent) on average. These results, it should be pointed out, are consistent with microeconometric estimates; see Haveman and Wolfe, 1995.[11]

In order to distinguish between the five-year impact and the long-run effect that income has on schooling we present in Table 6 dynamic panel estimates that control for the lagged dependent variable. We instrument the lagged dependent variable with its first lag in order to address biases in the dynamic panel regression (see Bond et al., 2010). Table 6 shows that the coefficients on the five-year impact effect of income on schooling range between 0.06 to 0.07; the cumulative effects range between 0.13 to 0.15. The five-year impact effect is significant at the 5 percent level while the cumulative effect is significant at the 1 percent level. Hence, the long-run effects are about twice as large as the five-year impact effects.

Discussion of Instrument Quality

11 The microeconometric elasticity estimates reported there falls in the range of 0.02-020.

We now discuss the quality of oil price shocks as an instrument for income. First, in terms of the first-stage fit, we note that the instrument performs reasonably well. Oil price shocks, as defined in equation (1), have a positive effect on income. This effect is in all specifications significant at the 1 percent level. Moreover, the Kleibergen-Paap first-stage F-statistic is well above the Staiger and Stock (1997) rule-of-thumb criteria of 10 for instruments to be declared weak.

The second important criterion for two-stage least squares estimation to yield consistent estimates is that the oil price shocks instrument should only affect schooling through income. To examine empirically whether oil price shocks are a valid instrument we use the lagged savings rate over five years as an additional instrument. The lagged savings rate has been employed as an instrument for income by Acemoglu et al. (2008) in the context of examining the effects of income on political institutions. In our context the assumption is that past savings, conditional on lagged schooling, are (i) exogenous to current schooling; (ii) do not exhibit significant effects on current schooling beyond income.

Columns (1) and (2) of Table 7 report two-stage least squares estimates that use both the oil price shock variable and the lagged savings rate as excluded instruments. The main finding in these regressions is that the 2SLS estimates on income are positive and significant. The coefficient on income is 0.08 in column (1) and 0.06 in column (2) where we also control for the lagged dependent variable. In terms of first-stage fit the Kleibergen-Paap F-statistic exceeds the critical value of 10. And the Hansen J test fails to find evidence that the instruments violate the exclusion restriction: the p-value of the joint hypothesis that the instruments are uncorrelated with the second-stage error term is well above 0.1.

To show also in a more intuitive way that, indeed, beyond income per capita the effects of oil price shocks on institutional quality are insignificant, we report in columns (3) and (4) instrumental variables estimates where we instrument income with the lagged savings rate and include the oil price shock variable on the right-hand side of the second-stage equation. The result is that the oil price shock variable does not exhibit a significant effect on schooling. Hence, conditional on income Table 7 shows that oil price shocks do not have a systematic average effect on schooling.

5. Extensions and robustness

5.1. Sensitivity analysis

Our identifying assumption for the two-stage least squares estimation is that, because the majority of the countries in our sample are price takers on the international oil market, variations in the international oil price are a plausibly exogenous source of variation in permanent income. To demonstrate that our results are robust to excluding those countries from our sample where changes economic conditions might have an effect on year-to-year variation in the international oil price, we report in columns (1) and (2) of Table 8 instrumental variables estimates that exclude large oil importing and oil exporting countries.[12] We find that in this case there continues to be a significant positive first-stage fit, with an F-statistic well in excess of 10. [13] Moreover, the second-stage coefficient on income is positive, 0.11, when excluding large oil importing countries and 0.14 when excluding large oil exporting countries.

In column (3) we show that the instrumental variables analysis produces similar results if we exclude those countries where oil companies are nationalized. This allows us to examine whether the response of schooling is a consequence of oil revenues accruing directly to the government sector (and much of schooling is publicly financed), or, whether the response reflects a more general average effect that permanent income shocks have on schooling. Column (3) of Table 8 shows that when we exclude countries where oil companies are in the hands of the government, estimates are similar to the baseline results: there is a positive and significant effect of within-country changes in income on schooling, with a 2SLS coefficient of around 0.15.

Beyond correcting for endogeneity and measurement error bias, another advantage of our two-stage least squares estimation framework that exploits the persistent nature of year-

12 Following Brueckner et al. (2012) large oil importing and exporting countries are identified as countries that export (import) more than 3 percent of world oil exports (imports). The large oil importing countries in the sample are China, France, United Kingdom, Italy, Japan, South Korea, Netherlands, and United States. The large oil exporting countries are Algeria, Canada, Indonesia, Iran, Iraq, Kuwait, Mexico, Nigeria, Norway, Oman, Qatar, Russia, United Arab Emirates, and Venezuela.

13 Note that, in column 2, the instrument is weaker that in the main specification, as should be expected, yet is still well above 10.

to-year variations in the international oil price is that it identifies the effect that permanent within-country changes in income have on schooling. To strengthen this point further, we report in column (4) of Table 8 two-stage least squares estimates for the sample period that excludes the years prior to 1973. Evidence on variations in international oil prices indicates that, in particular for the post-1973 period, these variations were highly persistent (see Kilian, 2009, and Dvir and Rogoff, 2010). Column (4) of Table 8 shows that when we focus on the post-1973 period the two-stage least squares coefficient is significant and positive. Quantitatively, the estimates indicate that a one percent increase in income was associated on average with an increase in the secondary school enrollment rate of over 0.1 percentage points.

In columns (5)-(7) of Table 8 we demonstrate that the estimates are robust to excluding data observations which could be deemed as outliers. In column (5) we exclude country-year observations in the top and bottom 1st percentile of the sample oil price shock distribution (i.e. large negative and positive oil price shocks). In column (6) we exclude country-year observations in the top and bottom 1st percentile of the within-country change in the secondary school enrollment rate (i.e. large negative and positive changes in schooling). In column (7) we exclude country-year observations in the top and bottom 1st percentile of the within-country change in (log) GDP per capita (i.e. large negative and positive GDP per capita growth observations). The second-stage coefficient on income continues to be positive and significant at the conventional significance levels in these regressions that exclude observations which could be deemed as outliers. Moreover, the first-stage relationship between oil price shocks and income continues to be strong with a Kleibergen Paap F-statistic exceeding 10.

Another robustness check that we have carried out is related to the construction of the oil price shock instrument. In equation (3.1) θ_c refers to countries' sample average net export share of oil in GDP. We are using in our benchmark regressions countries' average (and thus time-invariant) net export shares in order to make the instrument plausibly exogenous. One might be concerned that time-varying politico-economic variables affect industry structure and thus net oil exports as a share of GDP. Note that this effect is of order 1/T (T being the panel time-series length); thus, small in our sample as the average T is large (around 30). In order to further demonstrate that using average net exports of oil GDP shares in the construction of the oil price shock instrument does not lead to biased 2SLS estimates, we show in column (8) of Table 8 2SLS estimates that, alternatively, use as an instrumental variable the interaction between countries' initial (1970) share of net exports of oil in GDP and the year t-5 to t change in the international oil price. The main finding is that the second-stage coefficient on GDP per capita is positive and significant at the conventional confidence levels. Quantitatively, the size of the effect is also very similar to the benchmark 2SLS estimates reported in column (4) of Table 5.

5.2. Rich vs. Poor Countries

Part of the literature on income and schooling has noted heterogeneity in their relationship, depending on countries' development status (Psacharopoulos, 1994). One reason for why the income-schooling relationship may differ across countries depending on countries' development status is that credit frictions are typically more severe in poor countries. Another reason is that in rich countries secondary school enrollment rates are already very high (see Figure 1). Hence one may not expect much of an effect of income on secondary school enrollment in rich countries since in these countries the maximum level of secondary school enrollment is almost reached.

Table 9 shows that the effects of income on schooling are particularly large and significant for poor countries. The table shows this by splitting countries into the bottom 25th percentile, the bottom 50th percentile, the top 50th percentile, and the top 25th percentile based on countries' sample average GDP per capita. In the bottom 25th percentile the coefficient on income exceeds 0.3, in the bottom 50th percentile it exceeds 0.2. Both coefficients are significant at the conventional significance levels. On the other hand, in the sample of countries that are relatively rich the coefficient on income, while positive, is insignificant. In addition, we note that quantitatively the coefficient in the rich countries sample is less than half the size of the income coefficient in the poor countries sample.[14]

14 In Appendix Table 2 we document that similar results are obtained when we estimate an interaction model. The estimates from this interaction model suggest that the significantly larger effects of income growth on schooling in poor countries are not just due to poverty being correlated with lower levels of financial development and worse political institutions.

It is also interesting to note that, as Panel A of Table 10 shows, the larger effect of income on schooling enrollment in poor countries is particularly pronounced for female school enrollment. For example, comparing column (2) with column (3), the effect of income on female secondary school enrollment is three times larger in poor countries than in rich countries. For male secondary school enrollment (Panel B) the effects of income on school enrollment are also larger for poor countries than for rich countries, but the difference is smaller than for female enrollment. For example, comparing column (2) with column (3) shows that the effect of income on male secondary school enrollment is about twice as large in poor countries than in rich countries (for female enrollment it is more than three times as large). This difference in results for female and male enrollment is consistent with the literature that has found economic development (broadly defined) to have a positive effect on female education (see e.g. Field et al., 2009, for a recent micro study in this regard).

5.3. Alternative Measures of Schooling

Our main measure of schooling, in line with much of the existing work on economic growth determinants (e.g. Barro, 1997, Mankiw et al., 1992), is the secondary school enrollment. In this Section we discuss estimates of income effects on various alternative measures of schooling, such as, schooling expenditures, literacy rates, and years of schooling.

Data on schooling expenditures per pupil are sparser than for enrollment rates. This is one of the main reasons why we focus the analysis on school enrollment rates. In order to see what one would obtain with the limited observations on schooling expenditures, we present in Appendix Table 3 estimates that have the change in the log of school expenditures per pupil. Column (1) shows estimates for primary school expenditures; column (2) shows estimates for secondary school expenditures; and column (3) shows estimates for tertiary expenditures. The main finding is that the elasticity effect of income is positive and significant for all three expenditure measures. Quantitatively, the elasticity is largest for tertiary education. The estimated coefficient of 1.7 implies that a one percent increase in income increased expenditures on tertiary education by over 1.7 percent on average. Column (4) shows that the effect of income on total education expenditures is positive, with an overall elasticity of 2.4 that is significantly different from zero at the 1 percent level.

While measured school enrollment has prominently featured in policy debate, as reflected, for example, in the Millennium Development Goals (MDG) of the United Nations, goal number two being the achievement of universal primary schooling by the year 2015, enrollment data suffer from measurement errors as a measure of human capital (Pritchett, 2001, 2006). Consequently and following Hanushek and Kimko, 2000, recent attention has focused on qualitative measures, such as pertaining to cognitive skills. Existing work (see Ciccone and Papaioannou, 2009, Hanushek and Woessman, 2008, 2012) seems to suggest that these measures have a substantial effect on economic growth.

Unfortunately, panel data on school quality are very limited, in particular, for poor countries. Even data for literacy rates are much sparser than data on school enrollment. For the overlapping data that exist, the cross-country correlation between secondary school enrollment and literacy rates is high though (above 0.8). Appendix Table 4 shows that, with the 289 observations on literacy rates that exist, a pooled panel regression that instruments income with oil price shocks yields a positive and significant effect. The effect of income on literacy rates is positive and significant for both females and males. Quantitatively, the effect for females is about thirty percent larger than for males.

Another widely used dataset for country-level human capital (see e.g. Ciccone and Papaioannou, 2009) is the Barro and Lee (2010) dataset on the years of schooling in the population. This is as a 5-year panel, which uses the perpetual inventory method to construct internationally comparable attainment measures (see the source dataset for detailed explanations about data construction). Using this 5-year non-overlapping panel dataset, we present in Table 11 estimates that have the change in schooling over five years as a dependent variable. The explanatory variable is the change in GDP per capita over five years, and we instrument this variable by the change in the international oil price over five years weighted by countries' average net oil export shares during the sample period. The main finding here is that income has a significant positive effect on years of schooling. For example, the coefficient in column (1) of Panel A in Table 11 suggests that a one percent increase in GDP per capita leads to a 0.3 percent increase in the average number of years of schooling over a 5-year period. Panel B shows that the cumulative effect over a 10-year period is somewhat larger, going up to about 0.4 percent. Columns (2)-(4) show that similar elasticities are obtained when focusing separately on years of primary, secondary, and tertiary schooling.

5.4. An Alternative Instrument: Trade-Weighted World Income

In the previous sections our instrument for changes in countries' national incomes was oil price shocks. Year-to-year variations in the international oil price are highly persistent, thus, the oil price shock instrument is a convenient instrumental variable to study the effects of permanent income on schooling attainment. We now present instrumental variables estimates that are based on an alternative instrument for national income, trade-weighted world income; for a detailed discussion in regard to its construction, data sources, and exogeneity, see Acemoglu et al. (2008).

In columns (1)-(4) of Table 12 we present two-stage least squares estimates that use the change in trade-weighted world income as an excluded instrument for countries' GDP per capita growth. As in Acemoglu et al. the regressions are based on 5-year non-overlapping panel data, and schooling is measured as the average total years of schooling in the population aged 25 and over. The estimates reported in Table 12 are for the largest possible sample, given the availability of data on countries' real GDP per capita, total years of schooling, the trade-weighted world income instrument and the oil price shock instrument.

Column (1) shows that the first-stage effect of countries' trade-weighted world income is positive and highly significant. Given the literature on the transmission of international business-cycle shocks, discussed in Acemoglu et al. (2008), the positive and significant coefficient on trade-weighted world income is as expected. In the second stage we find that changes in countries' income have a significant positive effect on schooling. The coefficient on the log of GDP per capita is 1.05 and its standard error is 0.54. Quantitatively this semi-elasticity estimate of 1.05 suggests that a one percent increase in national income increases the population's average years of schooling by 0.01 years (or, by almost 0.2 percent); a doubling of national income approximately leads to an increase in schooling attainment by about 1 year.

In column (2) we repeat the two-stage least squares regression with the oil price shock instrument. The two-stage least squares regression is done for the same sample where the trade-weighted world income instrument is available. Consistent with our previous results, column (2) shows that national income has a positive and significant effect on years of schooling. The schooling-income semi-elasticity coefficient is around 0.8 with a standard error of 0.3.

The exclusion restriction in these IV regressions is that the oil price shock instrument and the world trade instrument only affect schooling attainment through their effect on aggregate income. As a first means to test this exclusion restriction, we present in column (3) two-stage least squares estimates that use both the oil price shock instrument and the world trade instrument as excluded instruments. The second stage coefficient in this overidentified IV regression is positive, around 0.9, and significantly different from zero at the 1 percent significance level. The Kleibergen-Paap F-statistic on the excluded instrument is 28.69. Thus, the instruments are strong. And the Hansen J-test on the joint hypothesis that the instruments are uncorrelated with the second-stage error term is 0.62. Hence the Hansen J test does not reject the validity of the excluded instruments.

In columns (4) and (5) of Table 12 we show in more intuitive way that there is no evidence of the instruments having systematic effects on years of schooling beyond national income. In column (4) we report two-stage least squares estimates that instrument national income with the trade-weighted world income instrument and include the oil price shock instrument directly in the second stage. In column (5) we report two-stage least squares estimates that instrument national income with the oil price shock instrument and include trade-weighted world income in the second stage. In both cases the coefficient on national income is positive and highly significant while the conditional (direct) effects of the instruments on schooling are insignificant.

Importantly, column (6) shows that, unconditional on countries' national income, the instruments do exhibit significant effects on schooling attainment in a reduced-form regression. This reduced-form regression shows that positive oil price and world income shocks have positive effects on years of schooling. Statistically, these effects are significant at the 5 percent level. Quantitatively, the reduced-form effects are also more than three times as large as the statistically insignificant conditional effects.[15]

15 In Appendix Table 5 we show that we obtain similar results when using the lagged savings rate as an instrumental variable.

6. Implications for Development Accounting: An Illustration

We now illustrate how our results may affect development accounting calculations. Letting y, k, and h denote the per capita values of income, physical and human capital, respectively, we write:

$$y = Ak^{\alpha}h^{1-\alpha} \tag{6.1}$$

and, moreover,

$$\log(h) = \log(z) + \beta\log(y) \tag{6.2}$$

where h is an attainment measure (say, the proportion of population with secondary schooling); z is the residual variation in attainment that is not due to y; β is our estimated coefficient that captures the income effect.

Let us define then, as in Caselli, 2005, the success rate (of production factors explaining the variance of national income across countries) as

$$\text{Successrate} = \text{var}[\log(y_{hk})]/\text{var}[\log(y)] \tag{6.3}$$

where

$$\log(y_{hk}) = \alpha\log(k) + (1-\alpha)\log(h) \tag{6.4}$$

Plugging equation (6.2) into (6.3) and then using (6.4) we obtain

$$\text{Successrate} = \text{Var}[\alpha\log(k) + (1-\alpha)\log(h)]/\text{Var}[\log(y)] =$$

$$\mathrm{Var}[\alpha\log(k) + (1-\alpha)(\log(z) + \beta\log(y))]/\mathrm{Var}[\log(y)] =$$

$$\{\mathrm{Var}\ [\alpha\log(k) + (1-\alpha)\log(z)] + (1-\alpha)^2\beta^2\mathrm{Var}\ \log(y) +$$

$$2\mathrm{Cov}[\alpha\log(k) + (1-\alpha)\log(z), (1-\alpha)\beta\log(y)]\}/\mathrm{Var}[\log(y)]$$

$$= (1-\alpha)^2\beta^2 +$$

$$\{\mathrm{Var}[\alpha\log(k) + (1-\alpha)\log(z)]+2\mathrm{Cov}[\alpha\log(k) + (1-\alpha)\log(z), (1-\alpha)\beta\log(y)]\}/\mathrm{Var}[\log(y)] \quad (6.5)$$

Hence, the explained variance by a model that does not take into account the feedback effects of income on schooling attainment is inflated by a function of β. Specifically, assuming that the covariance term in the above expression is positive, an assumption that appears plausible as both improvements in technology and increases in the capital stock tend to increase output per capita, a lower bound of the magnitude of this exaggeration effect is $(1-\alpha)^2\beta^2$.

As our estimated average β is around 0.2, and $\alpha = 1/3$ is commonly assumed, this yields $(4/9)(0.2)^2 = 0.02$, or two percentage points. For the subset of poor countries, however, the lower bound of the inflated effect is considerably larger. As for that group of countries the estimated β is almost 3/4, the bias is around $(4/9)(3/4)^2 = 1/4$, or 25 percentage points.

7. Concluding Remarks

Well-documented correlations between schooling attainment and national income have been hard to interpret. While early empirical research suggested that the former is the cause and the latter the effect, subsequent calibration analysis has suggested that the direction of the causal relationship is likely the other way around. Simple theoretical arguments suggest that the link can go both ways. In this paper, therefore, we take a fresh look at the extent to which schooling attainment is driven by income growth. To identify causal effects, we employ oil price shocks as an instrumental variable for income changes. Our results indicate that, indeed, income growth has a significant causal effect on schooling, however measured, and especially so in poor countries. In particular, a one percent of an increase in GDP per capita increases the secondary school enrollment rate by between 0.10 and 0.18 percentage points. Further, the average elasticity of years of schooling with respect to GDP per capita is about 0.3. The magnitude of such effects is well consistent with existing figures obtained from microeconomic analyses (Haveman and Wolfe, 1995). These results should be useful as a contribution to the literature on the macroeconomic determinants of education, which, in turn, is thought to affect various individual and social outcomes. In addition, an illustrative calibration exercise shows that, while our estimates of the average effect of national income on schooling attainment imply a relatively modest bias in development accounting, this bias can be quite large for the group of poor countries.

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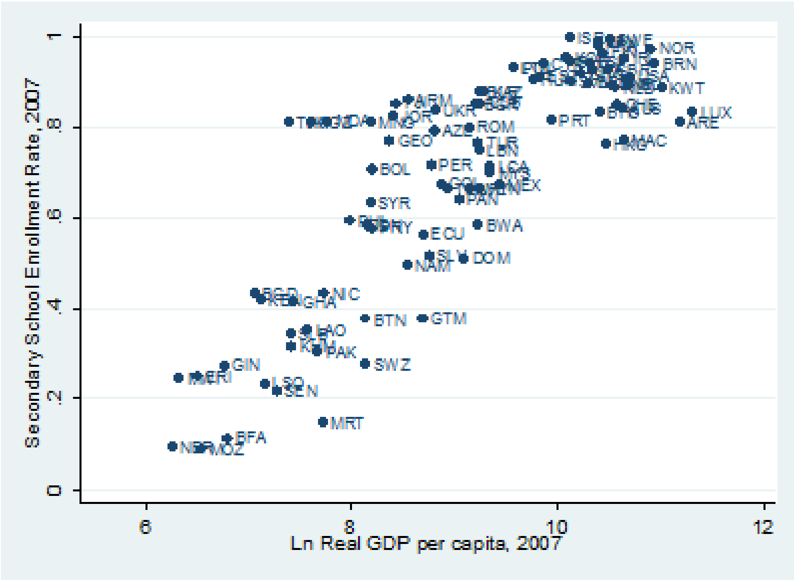
Figure 1. Income and Schooling



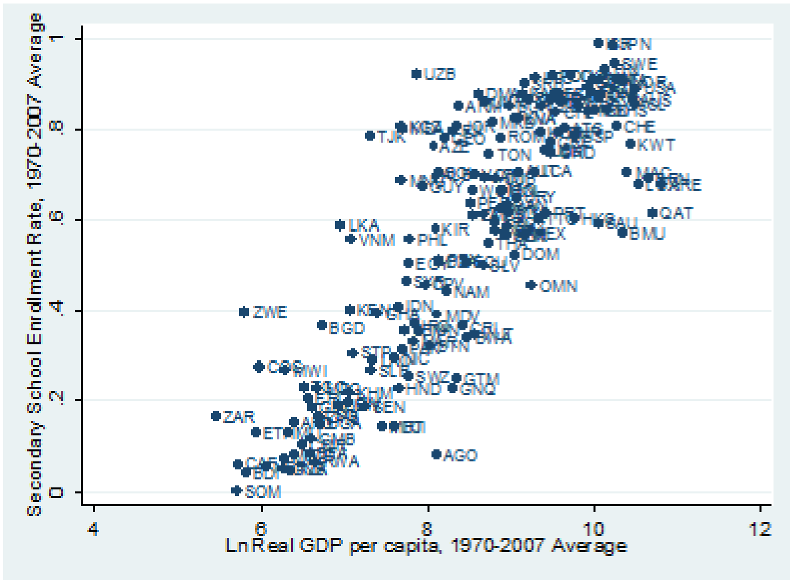


Table 1. Summary Statistics of Schooling Indicators

	Mean	Stdv	Observations
Secondary School Enrollment Rate, Total	0.566	0.342	3498
ΔSecondary School Enrollment Rate, Total	0.011	0.026	3121
Secondary School Enrollment Rate, Male	0.601	0.325	3111
ΔSecondary School Enrollment Rate, Male	0.010	0.030	2677
Secondary School Enrollment Rate, Female	0.567	0.369	3111
ΔSecondary School Enrollment Rate, Female	0.011	0.026	2677
Primary School Expenditures per Pupil	2059	2361	915
Δln(Primary School Expenditures per Pupil)	0.074	0.157	915
Secondary School Expenditures per Pupil	2680	2861	848
Δln(Secondary School Expenditures per Pupil)	0.065	0.152	848
Tertiary School Expenditures per Pupil	5022	4495	865
Δln(Tertiary School Expenditures per Pupil)	0.022	0.193	865
Literacy Rate, Total	0.721	0.246	289
Literacy Rate, Female	0.664	0.212	289
Literacy Rate, Male	0.780	0.284	289
Average Years of Schooling, Total	5.819	3.013	946
Δ5Years ln(Average Years of Schooling, Total)	0.116	0.099	946
Average Years of Schooling, Primary	3.857	1.805	946
Δ5Years ln(Average Years of Schooling, Primary)	0.091	0.098	946
Average Years of Schooling, Secondary	1.742	1.300	946
Δ5Years ln(Average Years of Schooling, Secondary)	0.188	0.173	946
Average Years of Schooling, Tertiary	0.220	0.248	946
Δ5Years ln(Average Years of Schooling, Tertiary)	0.214	0.276	946

Table 2. Income and Schooling
(Least Squares, Level Specification)

	Secondary School Enrollment Rate			
	(1)	(2)	(3)	(4)
	LS	LS	LS	SYS-GMM
	Panel A: Explanatory variable is ln GDP p.c., t			
ln GDP p.c., t	0.206***	0.004***	0.002	0.011
	(0.010)	(0.001)	(0.003)	(0.007)
Secondary School Enrollment Rate, t-1		0.981***	0.936***	0.951***
		(0.003)	(0.011)	(0.034)
R-squared	0.740	0.995	0.994	.
	Panel B: Explanatory variable is ln GDP p.c., t-1			
ln GDP p.c., t-1	0.207***	0.004***	0.001	0.009
	(0.010)	(0.001)	(0.003)	(0.007)
Secondary School Enrollment Rate, t-1		0.981***	0.936***	0.961***
		(0.003)	(0.011)	(0.033)
R-squared	0.739	0.995	0.994	.
Country FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	3247	3247	3247	3247
Countries	138	138	138	138

Note: The method of estimation in columns (1)-(3) is least squares; column (4) system-GMM. The dependent variable is the secondary school enrollment rate. Standard errors (shown in parentheses) are Huber robust and clustered at the country level. *Significantly different from zero at 10 significance, ** 5 percent significance, *** 1 percent significance.

Table 3. Income and Schooling
(Least Squares, First-Difference)

	ΔSecondary School Enrollment Rate					
	(1)	(2)	(3)	(4)	(5)	(6)
	LS	LS	LS	LS	LS	LS
Δln(GDP p.c.), t	0.002 (0.008)	0.004 (0.008)	0.002 (0.008)	0.006 (0.07)	0.005 (0.007)	
Δln(GDP p.c.), t-1	-0.004 (0.008)	0.004 (0.008)	-0.001 (0.008)	0.001 (0.009)	0.001 (0.009)	
Δln(GDP p.c.), t-2	0.016** (0.007)	0.017** (0.007)	0.016** (0.007)	0.019** (0.008)	0.018** (0.007)	
Δln(GDP p.c.), t-3	0.019*** (0.007)	0.020** (0.007)	0.019** (0.007)	0.022*** (0.007)	0.022*** (0.007)	
Δln(GDP p.c.), t-4	0.013** (0.006)	0.015** (0.008)	0.012* (0.007)	0.016** (0.007)	0.016*** (0.007)	
Δln(GDP p.c.), t-5	0.009 (0.006)	0.010 (0.007)	0.012* (0.006)	0.016** (0.006)	0.015** (0.007)	
Δln(GDP p.c.), t+1					0.008 (0.007)	
Δln(GDP p.c.), t to t-5 (5-Year Average)						0.012*** (0.003)
Country FE	No	No	Yes	Yes	Yes	Yes
Year FE	No	Yes	No	Yes	Yes	Yes
Observations	3107	3107	3107	3107	3107	3107
Countries	138	138	138	138	138	138

Note: The method of estimation is least squares. The dependent variable is the change in the secondary school enrollment rate. Standard errors (shown in parentheses) are Huber robust and clustered at the country level. *Significantly different from zero at 10 significance, ** 5 percent significance, *** 1 percent significance.

Table 4. Oilprice Shocks and Schooling
(Reduced Form)

	ΔSecondary School Enrollment Rate					
	(1)	(2)	(3)	(4)	(5)	(6)
OPS, t	-0.01 (0.03)	-0.01 (0.03)	-0.03 (0.03)	-0.02 (0.03)	-0.02 (0.03)	
OPS, t-1	0.13*** (0.05)	0.13*** (0.05)	0.11** (0.05)	0.10* (0.05)	0.10* (0.05)	
OPS, t-2	0.06 (0.05)	0.04 (0.05)	0.04 (0.05)	0.02 (0.06)	0.03 (0.05)	
OPS, t-3	0.10*** (0.03)	0.09*** (0.03)	0.07** (0.03)	0.06* (0.03)	0.06 (0.04)	
OPS, t-4	0.17*** (0.03)	0.16*** (0.03)	0.14*** (0.04)	0.13*** (0.04)	0.13*** (0.04)	
OPS, t-5	0.09*** (0.03)	0.09*** (0.03)	0.08** (0.03)	0.08** (0.03)	0.07** (0.03)	
OPS, t+1					-0.01 (0.04)	
OPS, t to t-5 (5-Year Average)						0.06*** (0.02)
Country FE	No	No	Yes	Yes	Yes	Yes
Year FE	No	Yes	No	Yes	Yes	Yes
Observations	3156	3156	3156	3156	3055	3156
Countries	138	138	138	138	138	138

Note: The method of estimation is least squares. The dependent variable is the change in the secondary school enrollment rate. Standard errors (shown in parentheses) are Huber robust and clustered at the country level. OPS is the change in the log of the international oil price multiplied with countries' average oil net-export GDP share. *Significantly different from zero at 10 significance, ** 5 percent significance, *** 1 percent significance.

Table 5. Income and Schooling
(Baseline, 2SLS Estimates)

	ΔSecondary School Enrollment Rate			
	(1)	(2)	(3)	(4)
Δln(GDP p.c.)	0.16***	0.18***	0.10**	0.11*
	(0.05)	(0.06)	(0.05)	(0.06)
Kleibergen-Paap F-Stat	62.31	46.96	95.03	88.97
		First-Stage Δln(GDP p.c.)		
OPS	0.52***	0.42***	0.66***	0.53***
	(0.06)	(0.06)	(0.07)	(0.06)
Country FE	No	No	Yes	Yes
Year FE	No	Yes	No	Yes
Observations	3121	3121	3121	3121
Countries	138	138	138	138

Note: The method of estimation is two-stage least squares. The dependent variable is the change in the secondary school enrollment rate. The instrumental variable is the 5-year average of the oil price shock; see Section 3 in the manuscript for a discussion of how this instrument is constructed. Standard errors (shown in parentheses) are Huber robust and clustered at the country level. *Significantly different from zero at 10 significance, ** 5 percent significance, *** 1 percent significance.

Table 6. Income and Schooling
(Impact vs. Cumulative Effects)

	ΔSecondary School Enrollment Rate			
	(1)	(2)	(3)	(4)
	2SLS	2SLS	2SLS	2SLS
Δln(GDP p.c.)	0.06**	0.07**	0.07**	0.07**
	(0.03)	(0.03)	(0.03)	(0.03)
Lagged ΔSecondary School Enrollment Rate	0.56***	0.57***	0.49***	0.50***
	(0.10)	(0.10)	(0.13)	(0.13)
Cumulative Effect Δln(GDP p.c.)	0.14***	0.15***	0.13***	0.14***
	(0.05)	(0.06)	(0.05)	(0.06)
Kleibergen-Paap F-Stat	30.04	30.10	28.20	46.60
Country FE	No	No	Yes	Yes
Year FE	No	Yes	No	Yes
Observations	2561	2561	2561	2561
Countries	135	135	135	135

Note: The method of estimation is two-stage least squares. The dependent variable is the change in the secondary school enrollment rate. The instrumental variable is the 5-year average of the oil price shock; see Section 3 in the manuscript for a discussion of how this instrument is constructed. Standard errors (shown in parentheses) are Huber robust and clustered at the country level. *Significantly different from zero at 10 significance, ** 5 percent significance, *** 1 percent significance.

Table 7. Income and Schooling
(Examination of Exclusion Restriction)

	ΔSecondary School Enrollment Rate			
	(1)	(2)	(3)	(4)
	2SLS	2SLS	2SLS	2SLS
Δln(GDP p.c.)	0.08*	0.06*	0.17*	0.13*
	(0.05)	(0.03)	(0.10)	(0.08)
Lagged ΔSecondary School Enrollment Rate		0.24***		0.24***
		(0.04)		(0.03)
OPS			-0.08	-0.07
			(0.09)	(-0.07)
Kleibergen-Paap F-Stat	57.48	60.41	27.72	46.28
Hansen J-test, p-value	0.37	0.31	.	.
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	3107	2809	3107	2809
Countries	138	137	138	137

Note: The method of estimation is two-stage least squares. The dependent variable is the change in the secondary school enrollment rate. In columns (1) and (2) the excluded instrumental variables are the 5-year average of the oil price shock and the 5-year average of the lagged savings rate; see Section 3 in the manuscript for a discussion of how the oil price shock instrument is constructed. In columns (3) and (4) the excluded instrumental variable is the lagged savings rate. Standard errors (shown in parentheses) are Huber robust and clustered at the country level. *Significantly different from zero at 10 significance, ** 5 percent significance, *** 1 percent significance.

Table 8. Income and Schooling
(Robustness Excluding: Large Oil Importers and Exporters; Countries where Oil Companies are Nationalized; Pre-1973 Period; Top and Bottom 1st Pctl. of Oil Price Shock, Secondary Education, and GDP p.c. Growth; 1970 Oil Net Export Shares)

	ΔSecondary School Enrollment Rate							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Excluding:	Large Oil Importers	Large Oil Exporters	Nationalized Oil Companies	Pre-1973 Period	Top and Bottom 1st Pctl. Oil Price Shock	Top and Bottom 1st Pctl. Education	Top and Bottom 1st Pctl. GDP p.c. Growth	Using1970 Oil Net Export Shares
Δln(GDP p.c.)	0.11*	0.14*	0.15*	0.11*	0.09*	0.09*	0.26*	0.11*
	(0.06)	(0.08)	(0.09)	(0.06)	(0.05)	(0.05)	(0.16)	(0.06)
Kleibergen-Paap F-Stat	84.26	27.49	10.65	89.89	85.35	24.50	18.62	100.50
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2848	2785	2334	2941	3058	3058	3058	3121
Countries	130	123	103	138	138	138	138	138

Note: The method of estimation is two-stage least squares. The dependent variable is the change in the secondary school enrollment rate. The instrumental variable is the 5-year average of the oil price shock; see Section 3 in the manuscript for a discussion of how this instrument is constructed. Standard errors (shown in parentheses) are Huber robust and clustered at the country level. Column (1) excludes large oil importing countries. These countries are China, France, Italy, Japan, South Korea, Netherlands, United Kingdom, and United States. Column (2) excludes large oil exporting countries. These countries are Algeria, Canada, Indonesia, Iran, Iraq, Kuwait, Libya, Mexico, Nigeria, Norway, Oman, Qatar, Russia, United Arab Emirates, and Venezuela. Column (3) excludes countries where oil companies are nationalized. The excluded countries are Algeria, Angola, Argentina, Bahrain, Bangladesh, Bolivia, Cambodia, Colombia, Republic of Congo, Ecuador, Egypt, Ethiopia, Gabon, Ghana, Guyana, India, Indonesia, Iran, Iraq, Kuwait, Libya, Malaysia, Morocco, Mozambique, Nepal, Nigeria, Oman, Pakistan, Peru, Philippines, Qatar, Russia, Trinidad and Tobago, Uganda, United Arab Emirates, Venezuela, Yemen, Zambia. Column (4) excludes from the sample the pre-1973 period. Columns (5)-(7) exclude from the sample the top and bottom 1st percentiles of the oil price shock, the change in the secondary school enrollment rate, and GDP per capita growth, respectively. Column (8) uses the initial (1970) oil net-export GDP share to construct the oil price shock instrument. *Significantly different from zero at 10 significance, ** 5 percent significance, *** 1 percent significance.

Table 9. Income and Schooling
(Poor vs. Rich Countries)

	ΔSecondary School Enrollment Rate			
	(1)	(2)	(3)	(4)
	2SLS	2SLS	2SLS	2SLS
	Bottom 25th Pctl. GDP p.c.	Bottom 50th Pctl. GDP p.c.	Top 50th Pctl. GDP p.c.	Top 25th Pctl. GDP p.c.
Δln(GDP p.c.)	0.37***	0.26**	0.07	0.09
	(0.08)	(0.10)	(0.06)	(0.08)
Kleibergen Paap F-stat	4.50	13.46	123.58	80.74
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	837	1569	1552	853
Countries	39	71	67	35

Note: The method of estimation is two-stage least squares. The dependent variable is the change in the secondary school enrollment rate. The instrumental variable is the 5-year average of the oil price shock; see Section 3 in the manuscript for a discussion of how this instrument is constructed. Standard errors (shown in parentheses) are Huber robust and clustered at the country level. *Significantly different from zero at 10 significance, ** 5 percent significance, *** 1 percent significance.

Table 10. Income and Schooling
(Males vs. Females)

	ΔSecondary School Enrollment Rate			
	(1)	(2)	(3)	(4)
	2SLS	2SLS	2SLS	2SLS
	Bottom 25th Pctl. GDP p.c.	Bottom 50th Pctl. GDP p.c.	Top 50th Pctl. GDP p.c.	Top 25th Pctl. GDP p.c.
		Panel A: Females		
Δln(GDP p.c.)	0.32***	0.18**	0.04	0.05
	(0.07)	(0.09)	(0.05)	(0.05)
Kleibergen Paap F-stat	2.40	8.26	110.98	30.71
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	699	1346	1331	682
Countries	38	75	62	29
		Panel B: Males		
Δln(GDP p.c.)	0.39***	0.30**	0.15*	0.29**
	(0.11)	(0.13)	(0.08)	(0.15)
Kleibergen Paap F-stat	2.40	8.26	110.98	30.71
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	699	1346	1331	682
Countries	38	75	62	29

Note: The method of estimation is two-stage least squares. The dependent variable is the change in the secondary school enrollment rate. The instrumental variable is the 5-year average of the oil price shock; see Section 3 in the manuscript for a discussion of how this instrument is constructed. Standard errors (shown in parentheses) are Huber robust and clustered at the country level. *Significantly different from zero at 10 significance, ** 5 percent significance, *** 1 percent significance.

Table 11. Income and Schooling
(Barro-Lee Dataset, 5-Year Non-Overlapping Panel)

	Δln(Schooling)			
	Total	Primary	Secondary	Tertiary
	(1)	(2)	(3)	(4)
	2SLS	2SLS	2SLS	2SLS
	Panel A: Δln(GDP p.c.), t			
Δln(GDP p.c.), t	0.34***	0.38***	0.24	0.43**
	(0.11)	(0.11)	(0.15)	(0.20)
Kleibergen Paap F-stat	27.87	27.87	27.87	27.87
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	946	946	946	946
Countries	120	120	120	120
	Panel B: Δln(GDP p.c.), t and t-1			
Δln(GDP p.c.), t	0.19	0.24**	0.11	0.31
[A]	(0.11)	(0.10)	(0.16)	(0.22)
Δln(GDP p.c.), t-1	0.24**	0.20**	0.26***	0.13
[B]	(0.09)	(0.09)	(0.09)	(0.20)
SUM [A]+[B]	0.43**	0.44***	0.37**	0.44*
	(0.14)	(0.13)	(0.17)	(0.26)
Kleibergen Paap F-stat	6.95	6.95	6.95	6.95
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	826	826	826	826
Countries	120	120	120	120

Note: The method of estimation is two-stage least squares. The dependent variable is the change in the log of average years of schooling (columns (2), (3), and (4) primary, secondary, and tertiary school school years, respectively). The instrumental variable is the 5-year average of the oil price shock; see Section 3 in the manuscript for a discussion of how this instrument is constructed. Standard errors (shown in parentheses) are Huber robust and clustered at the country level. *Significantly different from zero at 10 significance, ** 5 percent significance, *** 1 percent significance.

Table 12. Income and Schooling
(Acemoglu et al. 2008 Trade-Weighted World Income Instrument, 5-Year Non-Overlapping Panel)

	Δ Total Years of Schooling					
	(1)	(2)	(3)	(4)	(5)	(6)
	2SLS	2SLS	2SLS	2SLS	2SLS	LS
Δln(GDP p.c.)	1.06**	0.76**	0.87***	1.06**	0.76**	
	(0.54)	(0.34)	(0.29)	(0.54)	(0.34)	
ΔTrade-Weighted World Income					0.08	0.29**
					(0.17)	(0.13)
Oilshock				-0.66		1.66**
				(1.46)		(0.65)
Kleibergen Paap F-stat	40.67	21.41	28.69	35.61	21.46	.
Hansen J, p-value	.	.	0.62	.	.	.
			First-Stage Δln(GDP p.c.)			
ΔTrade-Weighted World Income	0.27***		0.28***	0.28***	0.28***	
	(0.04)		(0.05)	(0.05)	(0.05)	
Oilshock		2.14***	2.18***	2.18***	2.18***	
		(0.46)	(0.47)	(0.47)	(0.47)	
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	738	738	738	738	738	738
Countries	94	94	94	94	94	94

Note: The method of estimation in columns (1)-(5) is two-stage least squares; column (6) least squares. The dependent variable is the change in the average years of schooling. The excluded instrumental variable in columns (1) and (4) is the change in trade-weighted world income; see Acemoglu et al. (2008) for a detailed discussion on how this variable is constructed. The excluded instrumental variable in columns (2) and (5) is the oil price shock; see Section 3 in the manuscript for a discussion of how this instrument is constructed. The excluded instrumental variable in column (3) is the oil price shock and trade-weighted world income. Standard errors (shown in parentheses) are Huber robust and clustered at the country level. *Significantly different from zero at 10 significance, ** 5 percent significance, *** 1 percent significance.

Appendix Table 1. List of Countries
(Baseline Sample)

Afghanistan	Finland	Mozambique
Albania	France	Nepal
Algeria	Gabon	Netherlands
Angola	Gambia, The	New Zealand
Argentina	Georgia	Nicaragua
Armenia	Ghana	Niger
Australia	Greece	Nigeria
Austria	Guatemala	Norway
Azerbaijan	Guinea	Oman
Bahrain	Guinea-Bissau	Pakistan
Bangladesh	Guyana	Panama
Barbados	Haiti	Papua New Guinea
Belarus	Honduras	Paraguay
Belize	Hong Kong	Peru
Benin	Hungary	Philippines
Bolivia	Iceland	Poland
Brazil	India	Portugal
Bulgaria	Indonesia	Qatar
Burkina Faso	Iran	Romania
Burundi	Iraq	Russia
Cambodia	Ireland	Rwanda
Cameroon	Israel	Samoa
Canada	Italy	Senegal
Central African Republic	Jamaica	Sierra Leone
Chad	Japan	Slovenia
Chile	Jordan	South Africa
China	Kazakhstan	Spain
Colombia	Kenya	Tajikistan
Congo, Dem. Rep.	Kiribati	Tanzania
Congo, Republic of	Korea, Republic of	Thailand
Costa Rica	Kuwait	Togo
Cote d`Ivoire	Laos	Trinidad &Tobago
Croatia	Latvia	Tunisia
Cuba	Lebanon	Turkey
Cyprus	Libya	Uganda
Czech Republic	Lithuania	Ukraine
Denmark	Madagascar	United Arab Emirates
Djibouti	Malawi	United Kingdom
Dominican Republic	Malaysia	United States
Ecuador	Mali	Uruguay
Egypt	Malta	Uzbekistan
El Salvador	Mauritania	Venezuela
Equatorial Guinea	Mauritius	Vietnam
Estonia	Mexico	Yemen
Ethiopia	Mongolia	Zambia
Fiji	Morocco	Zimbabwe

Appendix Table 2. Income and Schooling
(Interaction Estimates)

	ΔSecondary School Enrollment Rate			
	(1)	(2)	(3)	(4)
	2SLS	2SLS	2SLS	2SLS
Δln(GDP p.c.)	0.35* (0.20)	0.21* (0.12)	0.09 (0.06)	0.25* (0.15)
Δln(GDP p.c.)* Avg(GDP p.c., in 1000s)	-0.016* (0.009)			-0.003** (0.001)
Δln(GDP p.c.)* Avg(Credit/GDP, in %)		-0.0047* (0.0027)		-0.006* (0.003)
Δln(GDP p.c.)* Avg(Democracy)			-0.08 (0.06)	0.01 (0.04)
Kleibergen Paap F-stat	5.38	16.16	68.80	11.48
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	138	135	128	126
Countries	3121	3056	2913	2876

Note: The method of estimation is two-stage least squares. The dependent variable is the change in the secondary school enrollment rate. The instrumental variable is the 5-year average of the oil price shock; see Section 3 in the manuscript for a discussion of how this instrument is constructed. Standard errors (shown in parentheses) are Huber robust and clustered at the country level. *Significantly different from zero at 10 significance, ** 5 percent significance, *** 1 percent significance.

Appendix Table 3. Income and School Spending Per Pupil

	Δln(Spending per Pupil)			
	Primary School	Secondary School	Tertiary	Total
	(1)	(2)	(3)	(4)
	2SLS	2SLS	2SLS	2SLS
Δln(GDP p.c.)	0.55**	0.96*	1.71***	2.43***
	(0.23)	(0.50)	(0.47)	(0.74)
Kleibergen Paap F-stat	6.43	19.35	4.32	7.25
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	915	848	865	690
Countries	84	79	82	67

Note: The method of estimation is two-stage least squares. The dependent variable is the change in log of expenditures per pupil. The instrumental variable is the 5-year average of the oil price shock; see Section 3 in the manuscript for a discussion of how this instrument is constructed. Standard errors (shown in parentheses) are Huber robust and clustered at the country level. *Significantly different from zero at 10 significance, ** 5 percent significance, *** 1 percent significance.

Appendix Table 4. Income and Literacy

	Literacy Rate		
	Total	Male	Female
	(1)	(2)	(3)
	2SLS	2SLS	2SLS
Δln(GDP p.c.)	2.05**	1.71***	2.27***
	(0.74)	(0.60)	(0.87)
Kleibergen Paap F-stat	11.53	11.53	11.53
Country FE	No	No	No
Year FE	No	No	No
Observations	289	289	289
Countries	115	115	115

Note: The method of estimation is two-stage least squares. The dependent variable is the literacy rate. The instrumental variable is the 5-year average of the oil price shock; see Section 3 in the manuscript for a discussion of how this instrument is constructed. Standard errors (shown in parentheses) are Huber robust and clustered at the country level. *Significantly different from zero at 10 significance, ** 5 percent significance, *** 1 percent significance.

Appendix Table 5. Income and Schooling
(Acemoglu et al. 2008 Lagged Savings Rate Instrument, 5-Year Panel)

	ΔTotal Years of Schooling					
	(1)	(2)	(3)	(4)	(5)	(6)
	2SLS	2SLS	2SLS	2SLS	2SLS	LS
Δln(GDP p.c.), t	2.05***	0.87***	1.33***	2.31**	0.69*	
	(0.76)	(0.30)	(0.32)	(1.06)	(0.37)	
Savings Rate, t-1					0.31	0.45***
					(0.20)	(0.15)
Oilshock, t				-3.07		1.29**
				(2.61)		(0.62)
Kleibergen Paap F-stat	17.77	19.42	11.70	8.55	15.79	.
Hansen J, p-value	.	.	0.18	.	.	.
			First-Stage Δln(GDP p.c.), t			
Savings Rate, t-1	0.24***		0.19***	0.28***	0.28***	
	(0.06)		(0.07)	(0.05)	(0.05)	
Oilshock, t		2.12***	1.90***	2.18***	2.18***	
		(0.48)	(0.48)	(0.47)	(0.47)	
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	843	843	843	843	843	843
Countries	110	110	110	110	110	110

Note: The method of estimation in columns (1)-(5) is two-stage least squares; column (6) least squares. The dependent variable is the change in the total years of schooling. The excluded instrumental variable in columns (1) and (4) is the lagged savings rate; see Acemoglu et al. (2008) for a detailed discussion on how this variable is constructed. The excluded instrumental variable in columns (2) and (5) is the oil price shock; see Section 3 in the manuscript for a discussion of how this instrument is constructed. The excluded instrumental variable in column (3) is the oil price shock and the lagged savings rate. Standard errors (shown in parentheses) are Huber robust and clustered at the country level. *Significantly different from zero at 10 significance, ** 5 percent significance, *** 1 percent significance.