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Institutional Quality Mediates the Effect of Human Capital on Economic Performance

Jonathon Adams-Kane
Jamus Jerome Lim

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Abstract

This paper considers the relationship between institutional quality, educational outcomes, and economic performance. More specifically, it seeks to establish the linkages by which government effectiveness affects per capita income, via its mediating effect on human capital formation. The empirical approach adopts a two-stage strategy that estimates national-level educational production functions that include government effectiveness as a covariate, and then uses these estimates as instruments for human capital in cross-country regressions of per capita income. The results identify a significant and positive effect of human capital on per capita income levels, and partially resolves the inconsistency between macro- and micro-level studies of the effect of human capital on income. The results also remain robust to alternative specifications, extension to a panel setting, subsamples of the data, and fully endogenous institutions.

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Institutional Quality Mediates the Effect of Human Capital on Economic Performance

Jonathon Adams-Kane and Jamus Jerome Lim*

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1 Introduction

One of the enduring puzzles in the study of human capital and income has been the apparent inconsistency between the empirical micro- and macro-econometric evidence. Studies using Mincer (1974)-style earnings functions generally find that the educational level—when used as a measure of human capital—is one of the strongest predictors of lifetime income, but this intuitive result does not generally survive aggregation: The cross-country evidence supporting a strong effect of educational attainment on national income is surprisingly mixed.

Indeed, while earlier studies that have considered the contribution of human capital to economic performance (Barro 1991; Mankiw, Romer & Weil 1992) have typically found a large and significant influence of such capital—as proxied by enrollment rates—on income per capita, later papers (Benhabib & Spiegel 1994; Pritchett 2001) have not only found an insignificant contribution, but in some cases have actually established a *negative* relationship between human capital and income.

This stands in stark contrast to a very large body of microeconometric labor research that has found a strong and persistent relationship between educational levels and wage rates. Although estimates are noisy and may depend on the time period chosen, the general result that earnings increase linearly with schooling completion has been found to hold for both U.S. (Heckman, Lochner & Todd 2006) as well as international (Peracchi 2006) data.

This micro-macro incongruence has led to various efforts aimed at resolving the paradox. One approach argues that human capital is either poorly measured or mismeasured. This approach stresses how existing education stock data may either suffer from systematic data deficiencies (Cohen & Soto 2007; Doménech & de la Fuente 2006), fail to capture important quality dimensions (Behrman & Birdsall 1983; Hanushek & Kimko 2000), or be subject to high rates of measurement error when first-differenced (Krueger & Lindahl 2001). Accounting for these measurement issues would then resolve the paradox.

Another school of thought has stressed the importance of educational governance failures. Factors such as teacher absenteeism, informal payments, and corruption in schools can severely erode the productivity of the education sector (Reinikka & Svensson 2005; Rogers 2008) and reduce the incentives for human capital accumulation (Gupta, Davoodi & Tiongson 2001). This institutional failure has implications for growth outcomes (Acemoğlu, Johnson & Robinson 2005). Given the poor institutional environment in which learning occurs, the failure of traditional educational statistics to capture the actual stock of human capital is hardly surprising.

These two resolutions are not unrelated; governance failures often imply poor quality of education. Nonetheless, authors have tended to stress one approach over another.[1]

[1] Pritchett (2001) further argues that the results could be due to stagnant demand for education labor in developing countries. This explanation is less likely, however, given both international (Berman, Bound & Machin 1998) and plant-level evidence that suggests that the demand for skilled labor is reasonably strong in many developing countries

The major challenge in the empirical study of the role of human capital in growth is centered of the endogeneity of human capital. While there is a strong theoretical basis for how human capital can drive growth in both neoclassical (Lucas 1988) and endogenous (Romer 1990) models, there is also the possibility of reverse causality, possibly through a discount rate channel (Bils & Klenow 2000), or more generally through improvements in human development (Suri, Boozer, Ranis & Stewart 2011). This endogeneity suggests that naïve attempts to measure the contribution of human capital will almost certainly encounter a bias in their estimates.

Our empirical analysis adopts a two-stage strategy: First, we estimate national-level educational production functions that include institutional governance and inputs to schooling as covariates. Second, we use these estimates from the first stage as instruments for human capital in cross-country regressions of (steady-state) income. This method not only provides new cross-country estimates of the impact of governance measures on educational outcomes, but also addresses the endogeneity concerns that arise when using direct measures of education in regressions of this nature.

Moreover, our use of instrumental variables (IV) allows us to reconcile the two major explanations that have been advanced to resolve the micro-macro human capital puzzle. By including governance measures in the education production function, we directly account for the institutional framework in which human capital accumulation occurs. The methodology also allows us to sidestep the concerns surrounding the mismeasurement of human capital, so long as our instruments are chosen carefully and satisfy the necessary validity conditions. Finally, in our robustness checks we also demonstrate that the results remain robust to fully endogenizing institutions in a System Generalized Method of Moments (System GMM) setting.

The two papers closest in spirit to our own are the ones by Dias & Tebaldi (2012) and Hanushek & Kimko (2000). Like us, the first paper is interested in the relationship between institutions, human capital, and economic performance. It develops a theoretical model that links institutions to human capital, but the econometric analysis does not adopt a two-step approach to estimating the mediating effect that governance plays in its effect on human capital, as we do here. The latter paper does use a similar two-step estimation procedure, but the first stage does not include our key conditioning variable of interest, institutional quality. Moreover, unlike these papers, our empirical work follows directly from a theoretical model, which leads us to estimate the first stage in *levels*, rather than growth *rates*.

Our approach is also complementary to the work of Glaeser, La Porta, López-de Silanes & Shleifer (2004) as well as Bhattacharyya (2009). The former uses a two-stage strategy to argue that human capital, rather than institutions, is a stronger predictor of per capita income, while the latter unbundles institutions to resolve the multicollinearity problem between institutions and human capital. Unlike both of these papers, we employ a different choice of instruments, and

(Fajnzylber & Fernandes 2008; Harrison & Hanson 1999; Pavcnik 2003).

our substantive concern is driven by a neoclassical growth model, rather than an approach that stresses the fundamental drivers of growth (Rodrik, Subramanian & Trebbi 2004) approach. We regard our treatment of human capital as a proximate—rather than fundamental—determinant of cross-country economic performance as a more intuitive one, and one that is more consistent with the theoretical literature.

Our main results are supportive of the notion that human capital is central to cross-country incomes. Our benchmark specifications find that a 1 percent increase in human capital contributes 3.02–3.33 percent to income per capita, and this contribution outstrips that of physical capital. In our robustness tests, we also show that this result survives the inclusion of additional explanatory variables in the second stage, as well as the use of alternative specifications in the first stage, including specifications allowing governance to be endogenous to income and/or endogenous to human capital.[2] We also demonstrate that the main results follow even when we alter our specification to exploit panel data.

Our findings are of considerable academic and policy interest. Empirical studies of human capital have frequently been hampered by the difficulty of isolating the causal impact of education on per capita income. Furthermore, to the extent that institutions are themselves subject to change, corroborating the body of microeconomic evidence on governance and education provides further impetus for institutional reform in developing countries.

The rest of the paper is organized as follows. Section 2 will present the motivating theoretical model. We then report the empirical results in Section 3, which includes a host of robustness tests that allow variations to the benchmark in terms of specification, sample, and fully endogenous institutions. A final section concludes with policy implications.

2 A Simple Model of Per Capita Incomes, Human Capital, and Governance

Our motivating theoretical model is an augmented Solow (1956) growth model, expanded to allow for three reproducible factors: Labor, L, physical capital, K, and human capital, H (Mankiw *et al.* 1992). Output at time t is generated by the production function

$$Y_t = K_t^{\alpha} H_t^{\beta} \left(A_t L_t\right)^{1-\alpha-\beta}, \quad 0 < \alpha, \beta < 1, \tag{1}$$

where A is the current level of (exogenous) technology, and we assume decreasing returns to all capital, so that $\alpha + \beta < 1$.

[2] Lipset (1960) argues that both economic growth and human capital accumulation cause institutional change, a hypothesis supported by Glaeser *et al.* (2004).

The micro literature on the education production function (Todd & Wolpin 2003) argues that cognitive achievement for a given individual i is determined by innate ability, η, family inputs, F, and school inputs, S. At the individual level, human capital at time t is therefore a function

$$H_{it} = h\left(\eta_i, F_{it}, S_{it}; G_t\right),$$

where G is the (exogenous) institutional environment whereby learning takes place, and we assume that individual ability is time-invariant. Aggregating over all effective units of labor gives

$$\begin{aligned} H_t &= \int_1^{A_t L_t} h\left(\eta_i, F_{it}, S_{it}; G_t\right) di \\ &= F_t \gamma S_t^{\epsilon} \left(A_t L_t\right)^{1-\gamma-\epsilon} \cdot G_t^{\phi}, \quad 0 < \gamma, \epsilon < 1, \end{aligned} \tag{2}$$

where we further assume a Cobb-Douglas form and decreasing returns to inputs with $\gamma + \epsilon < 1$. Note the omission of the ability term at the aggregate level; this amounts to assuming that innate ability is distributed normally across countries at the global level, such that there are no significant cross-country differences. Taking logarithms of (2) gives the (steady-state) amount of human capital per effective unit of labor:

$$\ln\left[\frac{H_t}{L_t}\right] = \ln A_0 + gt + \gamma \ln f + \epsilon \ln s + \phi G, \tag{3}$$

where we follow convention and rewrite $f \equiv \frac{F}{AL}$ and $s \equiv \frac{S}{AL}$ in intensive form, representing family and school inputs per unit of effective labor.

Technology progresses and labor grows at exogenous rates described by

$$A_t = A_0 e^{gt}, \quad L_t = L_0 e^{nt},$$

giving accumulation according to the ordinary differential equations

$$\dot{k}_t = s_k y_t - (n + g + \delta)\, k_t, \tag{4a}$$

$$\dot{h}_t = s_h y_t - (n + g + \delta)\, h_t, \tag{4b}$$

where s_k and s_h are, respectively, the investment shares of physical and human capital, δ is the rate of capital depreciation, and as before $y \equiv \frac{Y}{AL}$, $k \equiv \frac{K}{AL}$, and $h \equiv \frac{H}{AL}$ are in intensive form.

The steady state levels of physical and human capital are straightforward, and given by

$$k^* = \left[\frac{s_k^{1-\beta} s_h^{\beta}}{n + g + \delta}\right]^{\frac{1}{1-\alpha-\beta}}, \quad h^* = \left[\frac{s_k^{\alpha} s_h^{1-\alpha}}{n + g + \delta}\right]^{\frac{1}{1-\alpha-\beta}}.$$

Substitution into (1), taking logarithms, and re-substituting the steady-state share of human capital back into the resulting equation yields steady-state income per worker given by

$$\ln\left[\frac{Y}{L}\right] = \ln A_0 + gt + \frac{\alpha}{1-\alpha}\ln s_k + \frac{\beta}{1-\alpha}\ln h^* - \frac{\alpha}{1-\alpha}\ln(n+g+\delta). \tag{5}$$

Together, (3) and (5) are the system of two equations that we take to the data.

3 Empirical Tests of Income, Education, and Institutions

3.1 Empirical Model

Our empirical model is based on the system of equations summarized by (3) and (5):

$$\ln\left[\frac{H_{jt}}{L_{jt}}\right] = \theta_0 + \mu_j + \theta_1 G_{jt} + \ln\left[\frac{\mathbf{F}_{jt}}{L_{jt}}\right]\mathbf{\Theta}_2 + \ln\left[\frac{\mathbf{S}_{jt}}{L_{jt}}\right]\mathbf{\Theta}_3 + \varepsilon_{jt}, \tag{6}$$

$$\begin{aligned}\ln\left[\frac{Y_{jt}}{L_{jt}}\right] &= \pi_0 + \rho_j + \pi_1 \ln s_{k,jt} + \pi_2 \ln\left[\frac{H_{jt}}{L_{jt}}\right] - \pi_3 \ln(n+g+\delta) \\ &\quad + \mathbf{X}_{jt}\mathbf{\Pi}_4 + \nu_{jt},\end{aligned} \tag{7}$$

where G_{it} is governance, $\mathbf{F}_{jt}$ and $\mathbf{S}_{jt}$ are vectors of family and school inputs to human capital production for country j at time t, respectively, H_{jt} is human capital, $s_{k,jt} = \frac{I_{jt}}{Y_{jt}}$ is the investment share of GDP, $(n+g+\delta) = n + 0.05$ is the net rate of depreciation of effective units of labor,[3] $\mathbf{X}_{it}$ is a vector of additional controls, Y_{it} is GDP, μ_i and ρ_i are time-invariant country fixed effects, and $\varepsilon_{it} \sim N\left(0, \sigma_\varepsilon^2\right)$ and $\nu_{it} \sim N\left(0, \sigma_\nu^2\right)$ are i.i.d. disturbance terms. The theoretical prior for our main coefficient of interest, π_2, is positive.

In our robustness section, we populate the vector $\mathbf{X}_{it}$ with several other controls that have been found to be important in cross-country growth regressions. Similarly, we have entered family and school inputs as vectors, to accommodate the fact that the education production function literature has identified a host of possible candidates for important inputs to student performance. In our benchmark specifications, we maintain parsimony with only one input for F and S; we relax this restriction in our robustness section. Since the benchmark estimates that we consider are cross-sectional in nature, the t subscript above would not apply. However, time subscripts in (6) and (7) would be operative in the panel analyses.

3.2 Estimation and Identification Strategy

In our benchmark tests, we employ three main variables in our first-stage regressions. We contend that, of these three, two can be treated as plausibly exogenous, and could thus function as instru-

[3] We follow Mankiw *et al.* (1992) and assume that g and δ are constant across countries and their sum is approximated by calibrated data of 0.02 and 0.03, respectively.

ments; the third may suffer from simultaneity concerns, and is only used in conjunction with our other instruments.

Our first, and primary, instrument is government effectiveness.[4] Although there are potentially many channels by which an effective government bureaucracy can affect economic outcomes, we contend that the primary means by which this occurs is through service delivery, and in particular the delivery of educational services. In many countries, especially developing ones, educational expenditure is one of—if not the—largest components of total public expenditure, and education at the primary and secondary level is largely publicly-provided.[5] If government effectiveness does matter to per capita incomes, there is a strong likelihood that it does so mainly through its mediating effect on the delivery of education. Although we make no claims of causality at this stage, the strong relationship between governance and human capital is nevertheless visually captured in Figure 1.

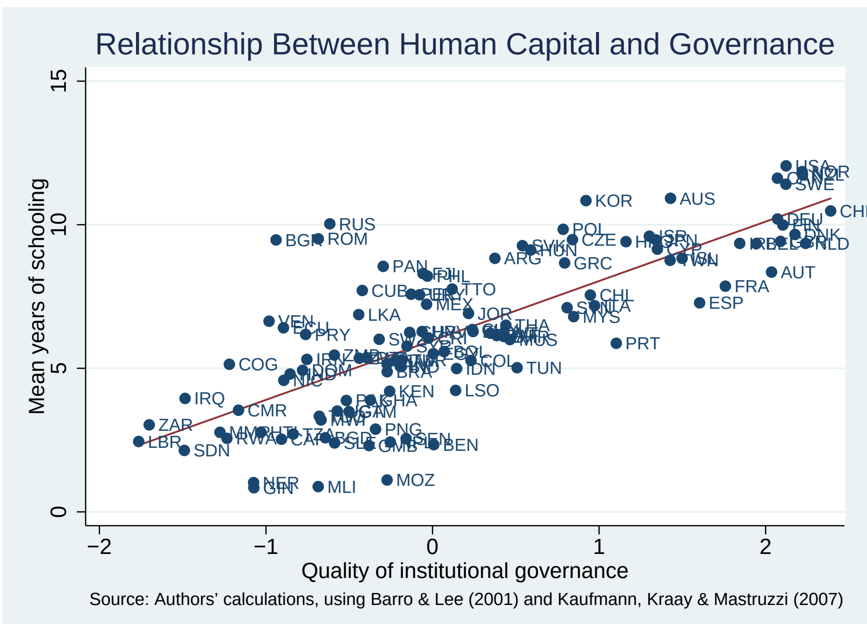

Figure 1: Positive relationship between quality of institutional governance and mean years of schooling, 2000, with fitted regression line. The (bivariate) regression is significant at conventional levels.

There are two other main channels by which effective government may affect economic out-

[4]We are not the only authors to recognize that the institutional setting can be an important instrument for education. Hanushek & Wößmann (2012) exploit the institutional structure of the educational *system*—specifically, the use of external exit exams, extent of school choice, and degree of local school autonomy—as instruments for cognitive skills.

[5]Education expenditures typically account for about 14% of government expenditures, which is typically (though not always) the largest single budget item (with the exception of social security in a small minority of countries).

comes. The (ostensibly) most obvious channel is through policy, especially (but not limited to) macroeconomic policy. While this may be a plausible theoretical consideration, this seems to be less of an issue in practice. There is fairly abundant evidence that policy variables in general do not exert a systematic influence on economic growth, at least at the margin (Levine & Renelt 1992; Sala-i-Martin 1997).[6]

The second channel is through the public financial management. As is the case for policy, while severe *mis*management of public finances—in the form of corruption—have been found to affect growth directly, empirical work has struggled to establish a strong first-order effect of government expenditures on growth, especially when expenditures are untempered by the quality of governance (Rajkumar & Swaroop 2008). Even in the narrower sphere of the size of public education expenditures, the evidence that such spending has an impact on educational outcomes is decidedly mixed.[7] But more importantly for our purposes, public education expenditures do not display a robust relationship with growth (Levine & Renelt 1992; Sala-i-Martin, Doppelhofer & Miller 2004). As a consequence, the quality of public financial management is unlikely to have a direct effect on economic growth.

In the analysis that follows, we perform a number of falsification tests that help rule out the possibility that these additional channels may invalidate the exclusion restriction. Finally, remaining simultaneity concerns are addressed by using a lagged specification of the effectiveness variable. Overall, we are reasonably confident that government effectiveness satisfies the exclusion restriction in the first stage. For completeness, however, we also provide several additional tests of the strength of this particular assumption when we discuss the benchmark results.

While an obvious candidate for household inputs is income per capita, it is essentially the same as the left-hand-side variable in the second stage regression, and thus clearly not exogenous. Thus, the second instrument that we use is the consumption-investment ratio, which acts as a proxy for family inputs into education.[8] To the extent that household educational expenditures is an investment good, the C/I ratio offers a plausibly exogenous instrument for family inputs that is not, theoretically, systematically related to the level of income per capita.[9] Moreover, the correlation between the two is relatively low, at -0.37.

Our final variable is the pupil-teacher ratio, which is our proxy for school inputs. We choose this variable, instead of other candidates, in part due to the strong case made for class size as a key

[6]This claim should be qualified. There is some evidence that very *bad* policy choices—such as financial repression or severe trade restrictions—may negatively affect country performance (Easterly & Levine 1997). However, policies that can be more directly associated with government effectiveness—such as monetary and fiscal policy—tend to be insignificant in standard cross-country growth regressions.

[7]Contrast, for example, the arguments in Hanushek (2003) to that of Lee & Barro (2001).

[8]Educational spending is generally classified as consumption expenditure by households; *ceteris paribus*, higher educational expenses would be associated with higher C/I ratios.

[9]While consumption and investment are, independently, jointly determined with income, we are unaware of any theoretical models that posit a systematic link between the *ratio* of the two with income.

determinant of schooling outcomes due to school resources (Krueger 2003), and in part because of its availability across countries and time. There are some legitimate concerns of simultaneity bias in including this variable: Countries with higher incomes per capita are likely to be able to afford to increase schooling resources, lowering the pupil-teacher ratio. Without a measure of school inputs, the tradeoff is reduced efficiency of the estimates due to a poorer fit in the first stage; we report specifications with and without the inclusion of this variable.

The remaining endogeneity issue is that of omitted variable bias. While it is possible that government effectiveness or the consumption-investment ratio can influence income per capita through an intervening omitted variable, or is affected by an omitted variable that also affects income per capita, this is not suggested by our theoretical model. Moreover, we are inclined toward a fairly parsimonious model, given the general lack of robustness of other, atheoretical explanatory variables that have been advanced in the literature. In any case, we take steps to address this issue in our robustness section.

Estimation of the model is via two-stage least squares, using two-step GMM and adjusted for heteroskedasticity-robust standard errors. For robustness tests using panel data, we run both fixed effects IV-GMM with correction for heteroskedasticity and serial correlation, and system GMM using the orthogonal deviations transformation for the endogenous regressors (Arellano & Bover 1995) and Windmeijer-corrected standard errors.[10] In most of our specifications, our model is overidentified, and we accordingly report the Hansen J-test of overidentifying restrictions.[11]

3.3 Data Description

Our cross-country macroeconomic data are drawn mainly from the World Bank's *World Development Indicators*. This includes the main controls: the investment share of output, and the net rate of depreciation (which is the sum of the population growth rate and 0.05, as discussed in Section 2). We supplement these with data from several other sources. Our primary measure of the human capital stock is the Barro & Lee (2001) dataset on educational attainment, which captures the average educational attainment of the population aged 15 and older. Our supplementary educational data—such as the pupil-teacher ratio and educational expenditures—were mainly from the UNESCO Institute for Statistics' *Global Education Statistics* database. Our primary governance

[10] System GMM allows for control of (weak) endogeneity using internal instruments, which is a distinct advantage for addressing possible endogeneity in the governance variable. System GMM was chosen over difference GMM (Arellano & Bond 1991) due to the discontinuous nature of the discontinuities nature the annual data.

[11] There are additional issues associated with the practical estimation of the augmented Solow model, many of which have been raised before by other authors (Dowrick & Rogers 2002; Hall & Jones 1999). These include, *inter alia*, assumptions of homogeneous cross-country technology and a failure to distinguish between the effects of diminishing returns and technology transfer. We do not propose to resolve these additional issues here—doing so would go far beyond the scope of this paper—but we wish to reiterate that the focus here is on resolving the human capital puzzle, not on testing the Solow model.

data were the *Worldwide Governance Indicators* (Kaufmann, Kraay & Mastruzzi 2011), which not only provides disaggregation into the subcomponents that we need, but are also, in our view, the highest-quality data available. For reasons discussed in Subsection 3.2, our preferred measure is government effectiveness, although we consider aggregate measures of governance as well.

The government effectiveness measure is central to our analysis, and it is worthwhile describing it briefly here. The measure captures, *inter alia*, perceptions regarding the quality of public services and the quality of the civil service (Kaufmann *et al.* 2011). Key for our application is that this variable is a reasonably good proxy for the quality of educational service delivery, as distinct from other institutional quality measures—such as the rule of law or voice and accountability—that are likely to have a far weaker relationship with educational attainment, if at all (further details on the construction of the variable are provided in the annex).

The benchmark sample comprises 64 developed and developing economies, for the year 2000. This year selection was dictated by data limitations: it is the only year where there is overlap between our preferred human capital measure and the governance measure used as an instrument. In Subsection 3.6, we consider an alternative setup that comprises an unbalanced panel of 445 observations, for the years 1998, 2000, 2002–2006 (annual). More details on the specific measures employed, as well as other data sources and additional controls used in the robustness tests, are described in full in the data appendix, along with additional summary statistics and the list of countries included in the sample.

3.4 Main Findings

In Table 1 we report the main results of our benchmark model, which is a cross-section using 2000 data. Specification (*B1*) is the least squares estimates for the augmented Solow model consistent with (7).[12] The sample comprises 103 countries, and the model provides a reasonably good fit. The human capital contribution is statistically significant, and enters with the expected sign. However, endogeneity concerns lead us to discount these results.

The top half of column (*B2*) reports the IV estimates for the baseline specification. In this specification, we use the pupil-teacher ratio as a proxy for school inputs, and the consumption-investment ratio as a proxy for family inputs. Due to data limitations, the full sample falls to 64 countries. Our main coefficient of interest, π_2, remains positive and statistically (and economically) significant. The contribution of physical capital is also consistent with the theoretical prior, but

[12]Breton (2010) has argued that consistency with the Mincerian framework requires that, in contrast to the Mankiw *et al.* (1992) specification, human capital be measured without transformation into logarithms. Since human capital is instrumented in our setup, the significance of the coefficient (although not the magnitude) is determined primarily by our instrument, rather than the form of the variable. We nevertheless provide estimates with a log-linear specification for human capital in the annex, and it is clear that the qualitative nature of our main result—that human capital is a significant determinant of cross-country income—remains unaffected.

Table 1: Benchmark regressions of GDP per capita†

	(B1)	*(B2)*	*(B3)*	*(B4)*	*(B5)*	*(B6)*
			Second stage income equation			
Investment	0.432	0.836	1.097	-0.002	0.689	0.255
share	(0.34)	(0.47)*	(0.48)**	(0.42)	(0.27)**	(0.32)
Net rate of	-0.900	0.815	0.801	1.889	0.744	1.695
depreciation	(0.63)	(0.99)	(1.02)	(0.74)**	(0.98)	(0.69)**
Human	1.840	3.125	3.142	3.329	3.024	3.250
capital	(0.23)***	(0.48)***	(0.44)***	(0.39)***	(0.41)***	(0.32)***
Constant	4.111	7.231	7.547	8.616	6.954	8.545
	(1.58)**	(2.35)***	(2.56)***	(1.84)***	(2.33)***	(1.79)***
			First stage human capital equation			
Family		-0.359	-0.753		-0.377	
resources		(0.29)	(0.32)**		(0.24)	
School		-0.557	-0.626	-0.548		
resources		(0.20)***	(0.22)***	(0.17)***		
Governance		0.136		0.137	0.251	0.277
		(0.06)**		(0.05)***	(0.05)***	(0.04)***
Broad			0.116			
governance			(0.07)*			
Constant		1.657	1.749	1.760	-1.971	-1.694
		(1.39)	(1.50)	(1.03)*	(0.96)**	(0.71)**
Adj R^2	0.715	0.534	0.498	0.434	0.591	0.508
Anderson LR		31.544***	29.135***	40.837***	27.779***	39.049***
Cragg-Donald F		12.315	11.252	24.789	15.615	45.639
Hansen J		1.717	1.535	0.255	0.032	-
N	103	64	60	83	78	103

† Notes: All variables were transformed to logarithmic form. Huber-White (robust) standard errors reported in parentheses. First stage regressions included second stage controls as instruments, but are not reported. Hansen statistics for exactly identified models are replaced with a dash. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level.

only marginally significant.[13] The Sargan-Hansen J statistic ($\chi^2 = 2.59, p = 0.27$) indicates that the instruments are valid. The Anderson LR statistic for underidentification is significant, and the Cragg-Donald F for weak instruments is reasonably high ($F = 12.32$, Stock-Yogo $F^{crit} = 9.08$ for 10% relative bias); both suggest that the instruments satisfy the relevance condition. Finally, the partial R^2 of the first-stage regression (not reported) is reasonably strong ($R^2 = 0.39$); since there is only one endogenous regressor, this result further corroborates the test for weak identification ($F = 9.78, p = 0.00$).

The bottom half of column (*B2*) reports the corresponding first stage results. While these estimates are of secondary interest, we note that the coefficients are consistent with the expected

[13] There is a valid concern that investment in physical capital may in fact be endogenous to government effectiveness, perhaps through the efficiency of government bureaucrats in processing investment-related procedures. While this appears to be important at the microeconomic level (Djankov, La Porta, López-de Silanes & Shleifer 2001), we are less convinced that this channel operates at the macroeconomic level, given that the correlation between the investment share and (lagged) government effectiveness is a low 0.2.

signs (recall that the pupil-teacher ratio is expected to be negatively related to human capital), and both school inputs and governance are significant at the 5% level. Finally, it is helpful to point out that, unlike Rogers (2008), our empirical strategy introduces the governance dimension directly as a covariate into the education production function, instead of separating the data into subsamples according to their level of governance. Besides being implied by our theoretical model of Section 2, we also regard this approach as a more direct test of the role that institutional governance might (or might not) play in the determination of human capital accumulation.

For reasons of identification, we have chosen to restrict our measure of governance to government effectiveness. Other than econometric reasons, there is a theoretical reason for doing so. The use of the more comprehensive definition of governance runs the risk of being tautological: If good institutions are *defined*, *ex ante*, as those structures and mechanisms that are most likely to enhance growth, then it is small wonder that, *ex post*, institutions are found to directly affect growth. Governance then becomes significant because we have defined it to be so. However, in order to allay concerns regarding the possibility that our choice of governance indicators are *ad hoc*, in column (*B3*) we repeat the above specification, but with one change: We expand the governance measure to all the six dimensions listed in Kaufmann *et al.* (2011). Our results are essentially unchanged. However, the adjusted R^2 for the first stage is lower, and the coefficient in this case is only weakly significant. We consider this a validation of our choice of a narrower definition of governance.

To account for remaining econometric concerns concerning our choice of instruments, we take three further steps: First, we exclude family inputs altogether, treating all measures of income as endogenous to the model. Second, we exclude school inputs, which as we discussed earlier may suffer from simultaneity bias. Third, we exclude all family and school inputs and rely solely on governance to identify the effect of human capital on income level and growth. These are reported in columns (*B4*) through (*B6*), respectively. The coefficient π_2 remains robust through these three changes, although these are not directly comparable due to changes in the sample size that result from differential data availability.

Taken together, the IV results reported in Table 1 suggest that a 1 percent increase in human capital contributes between 3.02–3.33 percent to income per capita. By way of comparison, physical capital—the only other control variable to feature some significant coefficients across the different specifications—has a contribution that is about three to five times smaller, ranging from 0.69–1.10 percent. As is common for cross-country growth regressions, the large and significant constant term suggests that a substantial unexplained component remains.

These specifications also satisfy the primary diagnostic tests for instrument validity. We note that the Hansen J cannot be computed for specification (*B6*), since the specification is just identified; this specification thus relies on the validity of the exclusion restriction (as discussed in

Subsection 3.2). To formally test the validity of this important assumption, we exploit a recent procedure developed by Kraay (2012), which utilizes Bayesian inference to explicitly characterize the extent to which prior uncertainty about the assumption affects the posterior distribution of π_2.[14]

We report these tests in Table 2, for differing assumptions with regard to the strength of the prior belief that the exclusion restriction holds exactly. This strength is given by the parameter ω, with higher (lower) values representing greater (lesser) certainty that the exclusion restriction is valid. The support—for the 2.5th and 97.5th percentiles—is chosen to correspond to a 95 percent confidence interval; changes in the interquantile range are also reported.

Table 2: Tests of validity of exclusion restriction for governance†

	$\omega = 5$	$\omega = 10$	$\omega = 100$	$\omega = 200$	$\omega = 500$	$\omega = \infty$
	Posterior distribution for π_2					
2.5th percentile	1.49	2.02	2.70	2.74	2.80	2.82
Mode	3.52	3.54	3.53	3.54	3.55	3.55
97.5th percentile	6.12	5.62	4.83	4.86	4.80	4.75
Change in interquantile range	4.63	3.60	2.13	2.12	2.00	1.93

† Notes: Posterior distributions calculated assuming that the distribution of prior probabilities that the exclusion restriction holds at 10% level. Corresponding supports are |0.46|, |0.34|, |0.12|, |0.08|, |0.05|, and 0, respectively.

Relative to the case where there is no prior uncertainty about the exclusion restriction ($\omega = \infty$), the support for the posterior distribution widens (from 1.93 to 4.63) as there is greater uncertainty ($\omega \to 5$), as expected. However, the mode remains stable, and even in the case of extreme uncertainty about the validity of the exclusion restriction ($\omega = 5$), the interval does not include zero, signifying the strength of the instrument. An alternative way of looking at this result is captured in Figure 2; here, while greater uncertainty over instrument validity leads to a wider dispersion in possible π_2 values, this change in the distribution is sufficiently small that the contribution of human capital continues to matter.[15]

We perform one final set of tests for the exclusion restriction, based on the two other possible channels where government effectiveness might be expected to operate. We perform a number of falsification tests by introducing a measure of either quality of macroeconomic policy or public

[14] The details of the analysis are described briefly in Appendix A.3.

[15] An important caveat to the tests are what the results would be *if* the distribution of *priors* was not centered on zero; in particular, if it were centered on a positive value. In this case, Kraay (2012) suggests that the nonzero mean would need to be subtracted out from the posterior distribution, which would result in a lower value for the 2.5th percentile that may include zero. While this is a theoretical possibility, we regard the likelihood of this as fairly low, since the expected prior of the effect would have to be quite large: at least greater than 1.49 (the lowest 2.5th percentile value). Estimates of the direct effect of governance on income—whether including controls or not—yield coefficients that are comfortably below 1.49 (these are available on request). As such, it appears unlikely that the caveat would be operative in practice.

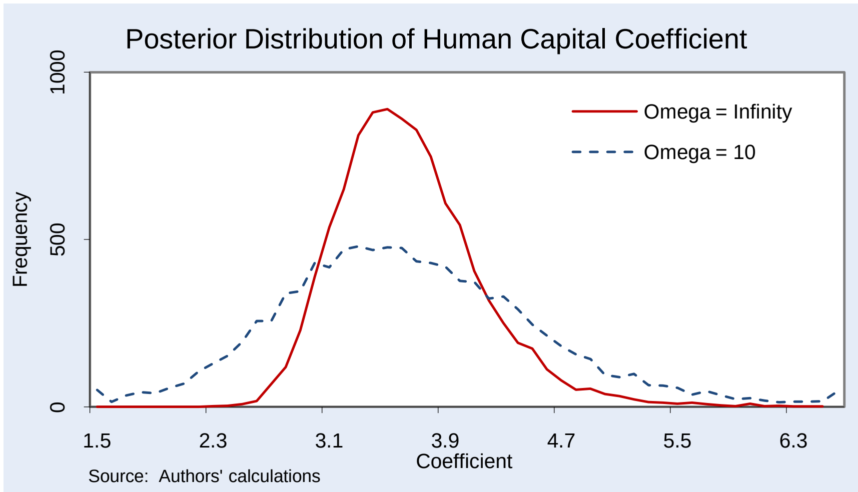

Figure 2: Posterior distribution for coefficient of human capital, with alternative assumptions about the validity of the exclusion restriction. Lower values of ω indicate greater prior uncertainty that the instrument satisfies the orthogonality condition. Even with high levels of uncertainty, the posterior distribution of the slope coefficient does not include zero.

financial management, alongside the human capital measure. For policy, we utilize the first principal component of an index of primary balances, gross debt, and inflation, similar to the policy measure proposed in Burnside & Dollar (2000). For public financial management, we adopt an index of the quality of public investment management developed by Dabla-Norris, Brumby, Kyobe, Mills & Papageorgiou (2011).[16] By and large, these variables display low correlations with human capital (-0.20 for macro policy, 0.19 for structural policy, and 0.19 for financial management), which alleviates concerns of multicollinearity. We find that, in regressions where we include both human capital and another alternative channel, the coefficient on the human capital variable remains significant, while those for either macroeconomic policy or financial management are insignificant.[17] Thus, we are fairly confident that these other main alternative channels are not responsible for the effect of government effectiveness on income.

3.5 Robustness of Benchmark Specification

In the benchmark models, we did not introduce any additional controls to explain cross-country income per capita. Here, we allow $\mathbf{X}$ to include variables that the literature has identified as important. More specifically, we draw on a selection of the variables that Levine & Renelt (1992)

[16]We also consider several alternative measures of each. For policy, for example, we used the World Bank's CPIA rating for macroeconomic management as well as structural policies; for public finance, the World Bank's CPIA ratings of quality of budget management and efficiency of revenue mobilization.

[17]These results are available on request.

and Sala-i-Martin (1997) argue are robust empirical relations: The trade share of GDP, geographic location, and infrastructure.[18] To this we include some relatively more recent candidates in the empirical growth literature: Ethnolinguistic fractionalization (Easterly & Levine 1997), democratic development (Barro 1996), and social capital (Knack & Keefer 1997). These are reported in columns (*R1*)–(*R6*) of Table 3.[19]

Table 3: Regressions of GDP per capita with additional controls†

	(*R1*)	(*R2*)	(*R3*)	(*R4*)	(*R5*)	(*R6*)
Investment	0.931	0.752	0.663	0.879	1.527	1.195
share	(0.51)*	(0.44)*	(0.74)	(0.47)*	(0.73)**	(0.47)**
Net rate of	0.853	0.997	0.779	0.935	1.303	0.444
depreciation	(1.00)	(0.88)	(1.13)	(1.10)	(1.56)	(0.89)
Human	3.160	2.901	2.992	3.203	4.076	2.646
capital	(0.49)***	(0.49)***	(0.54)***	(0.50)***	(0.77)***	(0.44)***
Trade share	-0.092					
	(0.14)					
Geography		0.152				
		(0.10)				
Infrastructure			0.079			
			(0.10)			
Ethnolinguistic				0.131		
fractionalization				(0.20)		
Social					1.541	
capital					(1.51)	
Democracy						0.206
						(0.14)
Constant	7.805	7.565	6.854	7.400	6.924	7.326
	(2.56)***	(2.11)***	(3.13)**	(2.74)***	(3.78)*	(2.33)***
Adj R^2	0.517	0.590	0.498	0.478	0.523	0.678
Anderson LR	31.099***	28.409***	20.051***	24.981***	17.367***	34.238***
Cragg-Donald F	11.888	10.636	7.044	9.084	5.984	13.677
Hansen J	1.702	2.148	1.957	1.320	0.980	2.773
N	64	63	54	63	39	58

† Notes: All variables were transformed to logarithmic form. Huber-White (robust) standard errors reported in parentheses. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level.

The significance of the coefficient on human capital survives the inclusion of all these additional controls. As before, while the coefficients are not directly comparable, we note that the human capital contribution is statistically and economically significant, with a range $[2.65, 4.08]$. The coefficient on physical capital is occasionally statistically significant, but its contribution is never greater than 1.53 percent, and is always dominated by the human capital contribution. None of the

[18] We used road density as a proxy for infrastructure, but we also explored alternative proxies such as the share of rural population and a weighted average of the percentage of population with access to water and sanitation facilities. Our qualitative results were affected by these alternatives.

[19] In addition to these controls, we considered a range of additional variables that could be of interest, especially in the context of the effect these may have on human capital accumulation. These include: dummies for states operating under a federal system, regional dummies, and government ideology (left/right). In the interest of space, we do not report these results (although they are available on request), the inclusion of these additional variables did not qualitatively change our main findings.

other variables that have been identified as important enter significantly.[20] Also, the instruments pass both the under- and over-identification tests, and in most cases satisfy the tests for weak instruments as well.

We now proceed to consider alternative variables for and permutations of our exogenous instruments.

Table 4: Regressions of GDP per capita with alternative controls†

	(Z1)	*(Z2)*	*(Z3)*	*(Z4)*	*(Z5)*
Investment	1.636	0.550	0.013	-1.067	0.970
share	(0.49)***	(0.23)**	(0.51)	(0.10)***	(0.42)**
Net rate of	1.672	0.821	4.464	-1.313	0.306
depreciation	(1.17)***	(0.65)	(1.07)***	(0.61)**	(0.82)
Human	3.556	3.242		1.788	3.073
capital	(0.48)***	(0.36)***		(0.20)***	(0.40)***
Alternative			7.981		
human capital			(0.85)***		
Constant	10.827	6.490	-17.418	0.491	6.067
	(1.75)***	(1.72)***	(3.07)***	(1.46)	(1.92)***
Adj R^2	0.528	0.649	0.173	0.832	0.541
Anderson LR	26.546***	53.722***	31.075***	36.005***	47.987***
Cragg-Donald F	7.460	39.036	18.248	15.237	15.911
Hansen J	4.955***	2.776	0.276	4.775	12.681***
N	54	63	68	11	64
	(Z6)	*(Z7)*	*(Z8)*	*(Z9)*	*(Z10*
Investment	0.761	0.972	0.895	-0.311	0.805
share	(0.43)*	(0.37)***	(0.41)**	(0.47)	(0.49)*
Net rate of	0.853	0.997	0.779	-1.392	1.303
depreciation	(1.00)	(0.88)	(1.13)	(0.66)**	(1.56)
Human	3.070	3.139	2.538	0.707	1.897
capital	(0.46)***	(0.48)***	(0.75)***	(0.36)*	(1.14)*
Governance			0.203	1.654	0.320
			(0.22)	(0.30)***	(0.55)
Constant	6.886	7.493	7.096	3.179	5.700
	(2.20)***	(2.29)***	(1.98)***	(1.91)*	(1.79)***
Adj R^2	0.550	0.529	0.696	0.726	0.812
Anderson LR	31.628***	31.680***	9.507***	13.461***	1.091
Cragg-Donald F	9.108	9.127	3.096	4.508	0.245
Hansen J	1.907	1.933	1.521	0.034	4.395
N	64	64	64	23	64

† Notes: All variables were transformed to logarithmic form. Huber-White (robust) standard errors reported in parentheses. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level.

An alternative way to qualify family inputs in the education production function is to recognize that families with a greater share of parental authority invested in the mother—usually due to

[20]The coefficient that is of tangential interest is the one on the infrastructure variable. One could make an argument that government effectiveness—in terms of the quality of public financial management—may affect growth through the infrastructure channel. However, we see here that infrastructure is an insignificant predictor of income levels in the second stage.

higher levels of education attained by them—are more likely to invest a greater share of family resources on education (Carneiro, Meghir & Parey 2013). We use this variable as an additional instrument to proxy for family inputs. We report this specification in column (*Z1*) of Table 4. In this case, the instruments are somewhat weak, but human capital remains positive and significant.[21]

Some authors have made a case for how the general intelligence quotient factor (Spearman's g) can affect growth, either as a proxy for human capital (Jones & Schneider 2006) or as an indicator of unobservable individual ability in the process of human capital formation (Weede & Kämpf 2002). There have been numerous criticisms of the use of g as a reliable indicator of general intelligence.[22] For our purposes, it is sufficient to note two important reservations, both of which we regard as critical.

The first is methodological. The theoretical foundation for g is premised on the emergence of a single general factor from hierarchical factor analysis of test scores. The problem with inferring that general intelligence exists as a consequence is that a general factor will always result whenever the correlation structure of all intelligence tests are positive (Thomson 1916), which is always true by design. The low power of such tests, especially with limited sample sizes, casts doubt as to whether g does truly exist, or even if it does, whether it can be accurately measured with IQ tests.

The second concern is that measures of g and their growth rates are not stable across time; in particular, they demonstrate a positive time trend. These have been extensively documented both between ethnic groups within countries, as well as between countries (Flynn 2007). Although many resolutions have been proposed to explain this effect, persuasive arguments have been advanced that changes in the cognitive or nutritional environment are responsible. Importantly for our purposes, this implies that IQ itself may be endogenous to the level of economic development of a country.

With these reservations in mind, we nonetheless include in our empirical tests a measure of intelligence, due to Lynn & Vanhanen (2002),[23] as a strong proxy for all resource inputs (so that the instrument set includes only IQ and government effectiveness).[24] This is reported in column (*Z2*). As before, our results are largely unchanged.

In the specifications listed in Table 1, we shied away from using achievement data (in the form of test scores). By and large, the international comparability across different test types and

[21] We also explored *replacing* the family input variable altogether, and while our qualitative results were unchanged, the instrument set did not satisfy the exclusion condition.

[22] We will not delve too deeply into the large (and contentious) literature on the psychometric measurement of intelligence and cognitive ability. Devlin, Fienberg, Resnick & Roeder (1997) provides a good summary of the key issues in the debate.

[23] The measures themselves have also been subject to dispute. The source data used in the construction of the dataset have been criticized as being based on excessively small, unrepresentative samples of national populations, and concerns have been raised about the accuracy of the reported scores and about the normalization methods employed to render the scores internationally comparable.

[24] Alternatively, we could have included it in (6) as a measure of innate ability, η, which we now allow to differ between nations. Doing so did not affect the qualitative nature of our results, but the instrument set fails the Hansen J test.

time periods is suspect, and where comparable data are available, they are often only for a very limited set of (mostly developed) countries. Moreover, our instrumental variables strategy already accounts for issues of mismeasurement, conditional on our instruments satisfying the necessary exclusion conditions. Nonetheless, we use a recently-compiled database of comparable achievement data (Altinok & Murseli 2007) to examine how our results change when we utilize a more accurate measure of human capital quality. The results are reported in column (*Z3*).[25] Human capital remains significant, and in this case its contribution more than doubles, so that a 1 percent increase in human capital leads to an almost 8 percent increase in output per worker. We do note the far poorer fit of the specification, however, which we feel justifies our decision not to use this measure as our primary measure of human capital.

The microeconometric literature on education production functions suggests that, in addition to the pupil-teacher ratio, several other inputs have been important (Hanushek 2003; Pritchett & Filmer 1999). We include, as additional instruments, a selection of the determinants that have been found to be more consistently significant: The percentage of trained teachers (as a macroeconomic proxy for teacher ability, usually measured with teachers' years of schooling or experience), and public education expenditures (a macroeconomic proxy for resources devoted to teacher salaries and school infrastructure). This specification is reported in column (*Z4*).[26] Although the results are once again similar, we note that the specification suffers from a small sample problem, which may limit inference.[27]

The next three columns, (*Z5*)–(*Z7*), introduce interaction terms between governance and resource inputs. These are for governance and school inputs, governance and family inputs, and family and school inputs, respectively. Although not fully justified by our theoretical model, the interaction term allows for the possibility that the efficacy of school inputs may be conditional on the institutional environment. This is intuitively plausible, and the interaction term also serves as a possible instrument that is orthogonal to the error term in the second stage. Adding these interaction terms, however, does not modify our principal conclusions concerning the coefficient for human capital, which remains relatively stable throughout. Note, however, that (*Z5*) does not satisfy the overidentification test.

Our final three specifications endogenize the most potentially problematic instrumental variable: Government effectiveness. Column (*Z8*) uses lagged government effectiveness (from 1996) as an instrument for contemporaneous (year 2000) governance. The magnitude of the human capital contribution falls, but remains significant at the 1 percent level, while the coefficient for physical

[25] We are again forced, by virtue of satisfying the overidentification test, to exclude family inputs from the instrument set.

[26] The microeconomic literature also finds that teacher quality is a very important source of variation in student performance (Hanushek 2003). Unfortunately, there is close to no international data available for teacher quality.

[27] Other permutations and combinations of these additional school inputs yielded similar significant coefficients for human capital, but typically did not satisfy the overidentification test.

capital is also significant at the 5 percent level. Interestingly, government effectiveness is *insignificant* when included in the second stage, while lagged effectiveness is significant and positively signed in the first stage human capital equation. This gives us some limited confidence that the effects of good governance—at least when measured with government effectiveness—operates primarily through its mediating role on human capital.[28] This is also the argument first raised in Glaeser *et al.* (2004), although they arrive at their claim from a different angle.

In column (*Z9*) we use a measure of the pervasiveness of informal payments as an instrument for government effectiveness. There are several reasons why we choose not to use this instrument more extensively. First, the correlation between informal payments and both government effectiveness and human capital is very low.[29] Second, the sample size—even in the attenuated sample, is extremely small. Finally, the instrument is relatively weak.[30] Nonetheless, we note that in this specification, human capital remains marginally significant, and government effectiveness is positive and highly significant.

The fairly large literature on institutions and growth that emerged following the paper by Acemoğlu, Johnson & Robinson (2001) has utilized, as instruments for institutions, settler mortality. We are somewhat reluctant to use these instruments, however, for two reasons. First, while a convincing case can be made for how the historical disease environment is a plausibly exogenous instrument for contemporary property rights institutions—or broader definitions of institutions—the linkage is, in our view, weaker when institutions are defined, as we do here, as the efficacy of the current government bureaucracy. Second, recent work has questioned the quality of the settler mortality data (Albouy 2012), and corrections to these data lead to settler mortality becoming a weaker instrument.

In any case, for comparability with the rest of the literature, we follow Acemoğlu *et al.* (2001) and Hall & Jones (1999) and include in our instrument set instruments corresponding to the fraction of the population of European descent (we maintain as instruments family and school inputs). This is reported in column (*Z10*). As expected, the quality of the combined instrument set is suspect: The specification does not pass the underidentification test, and the Cragg-Donald F statistic suggests

[28] Another possibility that would give rise to our result is that contemporaneous government effectiveness is strongly correlated with the other regressors included the second stage. While this is not an issue for the investment share and net depreciation rate—the correlation coefficients are 0.29 and -0.34, respectively—this could be the case for human capital ($\rho = 0.70$). There may be reasons why this result could be spurious, however. It is difficult see how an increase in the current level of human capital accumulation can lead to a simultaneous increase in contemporaneous government effectiveness; after all, improvements in human capital generally take time to diffuse into the workforce, including the public sector.

[29] The correlation coefficients are -0.25 and -0.24, respectively. This is very likely due to the very poor quality of the cross-sectional data. The data are typically not available for the year in question, and are generally cobbled from several different sources, which may use slightly different data collection methodologies; see the data appendix for more details.

[30] Using an alternative micro-based governance indicator, teacher absenteeism, is even worse; the sample size falls to 10, and the instruments fail both the over and underidentification tests.

that the instruments are extremely weak. Human capital does show up marginally significant, and governance remains an insignificant predictor of income, but we heavily discount this result due to poor test statistic performance.[31]

3.6 Extension to Panel Setting

Due to data limitations, the estimates that have been presented thus far have been cross-sectional in nature. It is possible to expand the sample to a panel, but it is important to keep in mind two considerations. First, while the educational attainment data are available for five-year intervals from 1960–2000, the panel is unbalanced, and consequently the 116-country sample has an average of only 4 observations per country. We report the fixed effects regression, analogous to (*B1*), in column (*P1*) of Table 5.[32]

Second, given that the governance and educational attainment data overlap for only one year (2000), we need to use an alternative measure of human capital if we wish to expand the panel in a way that allows us to preserve the use of government effectiveness as an instrument. We do so by substituting our human capital measure with data on enrollment rates. The panel with enrollment rates alone is much larger—176 countries, with an average of 7 years—and for reasons of comparability we report the fixed effects regression using this human capital measure in column (*P2*).

The coefficients for human capital in both of these specifications are relatively small: 0.409 and 0.323, respectively, although both are statistically and economically significant. Physical capital also appears significant in both of these specifications, although the magnitudes of their coefficients are also correspondingly smaller. As before, however, we discount these estimates because of endogeneity concerns.

Our benchmark panel, which uses enrollment data but is otherwise analogous to (*B2*), is reported in column (*P3*). It comprises 95 countries, with an average of about 5 time periods per country. As noted in the introduction, the danger that enrollment is a poor proxy measure for human capital is less of a concern as long as our instruments are valid. The Anderson and Hansen tests confirm that this is indeed the case, although it is important to point out that we are forced to use contemporaneous (instead of lagged) government effectiveness as an instrument; it is perhaps for this reason that the coefficient on governance in the first stage is indistinguishable from zero.

The results largely corroborate the findings of the cross section estimates, with the coefficients on human capital being statistically significant. While the magnitude of the contribution is somewhat

[31] We also explored including the settler mortality instrument, with even more disastrous results: The instrument fails *both* the exclusion and relevance conditions, and none of the variables in the second stage are statistically significant.

[32] The Hausman test detects systematic differences between coefficients and hence a preference for fixed over random effects.

Table 5: Panel regressions of GDP per capita†

	(*P1*)	(*P2*)	(*P3*)	(*P4*)	(*P5*)	(*P6*)
			Second stage income equation			
Investment share	0.162 (0.05)***	0.152 (0.04)***	0.111 (0.08)	0.126 (0.08)	0.031 (0.11)	0.139 (0.10)
Net rate of depreciation	-0.098 (0.13)	0.043 (0.13)	0.337 (0.18)*	0.349 (0.19)*	0.733 (0.28)***	-0.612 (0.46)
Human capital	0.409 (0.11)***					
Alternative human capital		0.323 (0.05)***	1.503 (0.44)***	1.546 (0.47)***	2.183 (0.72)***	-0.937 (1.12)
Constant	8.263 (0.39)***	8.101 (0.37)***				
			First stage human capital equation			
Family resources			0.029 (0.11)	0.309 (0.11)		0.011 (0.09)
School resources			-0.253 (0.08)***	-0.261 (0.08)***	-0.180 (0.06)***	
Governance			-0.047 (0.06)		-0.030 (0.04)	-0.081 (0.06)
Broad governance				-0.032 (0.07)		
F	9.018***	21.990***	6.173***	5.980***	5.478***	1.261
Anderson LR			13.012***	12.015***	12.627***	4.395*
Cragg-Donald F			4.356	4.017	6.342	2.188
Hansen J			3.256	4.024	2.407	1.294
N	511	1175	435	435	658	536

† Notes: All variables were transformed to logarithmic form. Heteroskedasticity and autocorrelation-robust (asymptotic) standard errors reported in parentheses. With the exception of the pooled specification, regressions included country and time fixed effects. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level.

smaller, it is still economically significant: A 1 percent increase in human capital leads to a 1.5 percent increase in per capita income. This decline is probably due to the inclusion of country fixed effects, which would capture a good deal of idiosyncratic country-specific variation.

In columns (*P4*)–(*P6*), we make several minor perturbations to this benchmark. Specification (*P4*) replaces government effectiveness with the broad measure of governance, while columns (*P5*) and (*P6*) limit the instrument set by dropping, respectively, family and school inputs as instruments. While dropping family inputs as an instrument or using the broad measure of governance does not affect our results in any qualitative fashion, the instrument set is weakened considerably by the absence of school inputs. Specification (*P6*) satisfies the relevance condition only marginally, and the utility of the model—as given by the F test—is very low. While we report the estimates in this final model for completeness, we are inclined to heavily discount them in our analysis.

3.7 Fully Endogenizing Institutions

Our final robustness check seeks to endogenize as many of the instruments that we have used as possible; of particular concern is the possibility that governance may be endogenous to the income equation. To do so, we exploit the temporal nature of the panel to retrieve internal instruments based on the lags of the endogenous variables. Table 6 reports these results using the panel with enrollment rates as a proxy for human capital, and contemporaneous government effectiveness as the measure of governance.

The specifications are as follows: (*S1*) System GMM estimates of (5), with governance, with one-period lagged GMM-style internal instruments and family and school resources treated as fully exogenous IV-style instruments;[33] (*S2*) Specification (*S1*), but without family and school inputs as exogenous instruments; (*S3*) Specification (*S1*), but with year dummies as additional exogenous instruments; (*S4*) Specification (*S1*), but with two-period lagged GMM-style internal instruments; (*S5*) Specification (*S1*), but with a broad governance measure; and (*S6*) All variables in (3) included as explanatory variables in (5), with one-period lagged GMM-style internal instruments.

Table 6: Regressions of GDP per capita with internal instruments†

	(*S1*)	(*S2*)	(*S3*)	(*S4*)	(*S5*)	(*S6*)
Investment	-0.317	-0.352	0.189	0.147	0.553	-0.791
share	(0.37)	(0.42)	(0.20)	(0.20)	(0.26)**	(0.35)**
Net rate of	0.190	0.554	0.412	-0.332	-0.043	-0.013
depreciation	(1.15)	(0.78)	(1.22)	(0.73)	(0.75)	(0.78)
Human	1.651	1.674	1.471	1.277	1.323	0.816
capital	(0.34)***	(0.32)***	(0.30)***	(0.22)***	(0.18)***	(0.48)*
Governance	0.774	0.519	0.698	0.700		0.352
	(0.26)***	(0.15)***	(0.21)***	(0.13)***		(0.18)*
Broad					0.740	
governance					(0.11)***	
Family						-0.550
resources						(0.29)*
School						-1.086
resources						(0.56)*
Constant	2.255	3.134	4.246	2.984	4.130	8.718
	(3.42)	(2.38)	(2.83)	(1.66)*	(1.73)**	(5.12)*
F	22.405***	30.411***	61.002***	77.530***	48.714***	60.737***
Arellano AR(1)	1.022	0.198	-1.170	-1.381	-0.106	-0.857
Arellano AR(2)	1.016	1.072	0.560	1.377	1.959***	-0.052
Hansen J	39.123	53.451***	45.149	88.030	82.110	63.305
N	445	808	445	445	511	445

† Notes: All variables were transformed to logarithmic form. Heteroskedasticity- and autocorrelation-robust (asymptotic) standard errors reported in parentheses. A constant term and time dummies were included in the regressions, but not reported. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level.

[33] Strictly speaking, system GMM also uses first differences of endogenous regressors as additional instruments, but this difference structure does not vary since additional lagged differences would lead to redundant moment conditions.

We make three comments about the results. First, the instrument set is reasonably sound. With the exception of specification (*S2*), the instruments satisfy the overidentifying restrictions, and the Arellano-Bond test for both AR(1) and AR(2) autocorrelation is satisfied (exempting AR(2) serial correlation in specification (*S5*)). Although not reported, the difference-in-Sargan tests for the (strict) exogeneity of the instrument subsets are generally satisfied.

Second, the coefficient on human capital is significant across all the specifications, ranging from 0.816–1.651 (with the lower bound only marginally significant). As before, the human capital contribution swamps the physical capital share, and in many cases by an order of magnitude.[34] Once again, we have validation that human capital is an economically crucial determinant of income patterns.

Third, our measure of governance enters significantly across the different specifications as well, with magnitudes that are about half that of the coefficient on human capital. This stands in contrast to our findings reported in the cross-section (Table 4), and deserves some explanation. The crucial difference to note is that our measure of governance in this case is contemporaneous, rather than lagged, government effectiveness.

Why might this lead to problems? Our estimation method (system GMM) relies on weak exogeneity—the assumption that current explanatory variables are not affected by future innovations in income—as an identification strategy. While this may be plausible for human and physical capital, the fact that the current stock of human capital is likely to be affected by past realizations of governance quality means that the simultaneity problem is not completely eliminated when we include current levels of governance as a covariate on the right hand side. In other words, we cannot rule out the possibility that anticipated future levels of income may affect current governance levels, which violates the assumption of weak exogeneity. This may account for the significance of the governance variable, although we cannot completely rule out the possibility that our theoretical model suffers from misspecification concerns.

3.8 Subsample Analysis

Given the centrality of institutional differences, we perform one final set of analyses to tease out the mechanism driving our results. We dissect the panel into subsamples corresponding to the following: (a) The subsamples above and below the median; (b) Half a standard deviation above and below the mean; (c) One standard deviation above and below the mean, all with respect to the broad institutional governance measure.[35] These are reported in Table 7.

[34] Although investment share is incorrectly signed in some specifications, these estimates are generally statistically indistinguishable from zero. In the one specification where the coefficient on physical capital is significant, it is correctly signed.

[35] We also explored subsamples pivoted about the mean, and with larger deviations from the mean, but these subsamples did not yield any additional qualitative insight, and in some cases were not estimable due to small sample

Table 7: Panel regressions of GDP per capita, by institutional quality†

	$< p50$	$> p50$	$< \mu - \frac{1}{2}\sigma$	$> \mu + \frac{1}{2}\sigma$	$< \mu - \sigma$	$> \mu + \sigma$
Investment	0.115	0.047	0.169	0.152	-0.080	0.139
share	(0.07)*	(0.12)	(0.11)	(0.09)*	(0.16)	(0.11)
Net rate of	0.228	0.155	0.162	-0.148	-0.789	-0.050
depreciation	(0.20)	(0.22)	(0.15)	(0.13)	(1.59)	(0.15)
Human	0.674	2.497	0.568	1.290	0.170	-0.512
capital	(0.29)**	(0.95)***	(0.27)**	(0.78)*	(0.14)	(1.02)
F	5.298***	4.919***	6.069***	5.044***	0.619	0.486
Anderson LR	10.491***	11.354***	17.929***	4.524	16.056***	3.508
Cragg-Donald F	3.501	3.798	6.221	1.471	6.059	1.116
Hansen J	1.882	2.216	1.369	6.780**	0.325	2.085
N	203	224	130	144	30	95

† Notes: All variables were transformed to logarithmic form. Heteroskedasticity- and autocorrelation-robust (asymptotic) standard errors reported in parentheses. Sample sizes above and below the median differ because not all controls were available for full-sample estimation. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level.

We offer three remarks about the results. First, compromising the sample size typically reduces the strength of the instruments, as reflected in both the over and under-identification tests (especially for the specification in column four), as well as the Cragg-Donald weak instrument tests. This gives us less confidence that endogeneity problems have been fully addressed, and this may also account for the generally smaller point estimates for the coefficient on human capital.

Second, this reduction in sample size also significantly reduces the explanatory power of the model in general. The F statistics in the final two columns are insignificant, as are all the coefficients on the covariates. First stage results (not reported) further suggest a very poor fit for instruments, with low F statistics and insignificant controls.

Third, and most interestingly, human capital appears to matter in institutional environments that are either relatively strong or relatively weak. While this may simply be a consequence of the restricted sample, there is reason to believe otherwise. Subsample regressions that dissect the data into income groups also find significant coefficients on human capital, despite these subsamples possessing even smaller sample sizes.[36] What is more likely is that countries that fall in the extremes of the institutional quality distribution face systematically different challenges in translating human capital investments into growth outcomes.

For countries with extremely poor quality of institutions—countries such as Guinea, Lao PDR, and Sudan—improvements in human capital alone are unlikely to make a dent in growth, unless accompanied by institutional improvements that render such investments productive in the context of the broader economy. At the other end of the spectrum, countries that have already accumu-

sizes.

[36]These are reported in reported Appendix A.5.

lated a large stock of human capital—such as Belgium, Finland, and Sweden—may face strong diminishing returns to additional investments in education. While schooling may still matter for lifetime incomes at the individual level, the marginal returns to an additional unit human capital at the country level would be much smaller.

More generally, the results in Table 7 can be interpreted in the light of equations (3) and (5). In countries with low quality of institutions and ineffectual governments (low G), the marginal productivity of effective human capital (h) is likely to be low, such that the binding constraint to per capita income growth is in (3). As countries improve their governance levels, this constraint is relaxed, such that human capital makes a positive and significant contribution to income per capita. Finally, for countries with strong institutional frameworks (high G), (3) no longer acts as a constraint to growth. Instead, continued output growth bumps into diminishing marginal productivity, as embodied in the coefficient of human capital in (5).

4 Conclusion

In this paper, we take an alternative approach to reconciling the apparent paradox between micro- and macro-level studies of the role of human capital in income. Specifically, we have argued that the quality of institutions is central to learning and education, so that the role of governance in a country's growth process operates primary though its intervening effect on human capital. Using a range of empirical identification strategies, we have taken this theory to the data, and found support for this conjecture at both the cross-sectional and panel level.

From a policy perspective, our findings stress the critical importance of ensuring that human capital accumulation through national educational systems occurs in an institutional environment that reflects a high quality of governance. From the perspective of a developing country, this typically involves improving the quality of educational service delivery through well-designed systems that align the incentives of teachers and administrators with those of students; in practical terms, this means reducing chronic absenteeism from schools—often one of the primary sources of educational governance failure (Chaudhury, Hammer, Kremer, Muralidharan & Rogers 2006)—and (if necessary) improving the quality of school infrastructure (Duflo 2001). Importantly, while direct interventions—including conditional cash and noncash transfers (Filmer & Schady 2008; Schultz 2004), as well as direct and indirect subsidies (Angrist, Bettinger, Bloom, King & Kremer 2002)—have proven positive effects on schooling attendance and enrollment, our research suggests that the efficacy of such interventions could be muted, so long as there are no changes in the quality of educational institutions.

Future research will consider more carefully the mechanisms underlying changes in institutional quality, and its interactions with growth. In particular, by allowing for a dynamic process of institutional change, it may be possible to obtain steady-state expressions for not just human and physical capital, but also institutions, and the interactions between these economic and political factors. Empirical opportunities include directly testing the role of governance in education with micro-level indicators of governance, such as teacher absenteeism rates or the pervasiveness of informal payments in schooling, using micro-level data on student performance.

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Appendix

A.1 Detailed Data Description

Educational attainment is the mean years of primary, secondary, and post-secondary education received by the population aged 15 and older, normalized for differential duration of education across countries. The (gross) *enrollment rate* is the share of pupils enrolled at the secondary level, regardless of age, relative to the theoretical age group for that level.

The *consumption-investment ratio* is total household and government consumption expenditure divided by gross fixed capital formation (gross of changes in the level of inventories), in constant 2000 U.S. dollars.

The *pupil-teacher ratio* is the number of pupils enrolled in primary school, divided by the number of primary school teachers. The additional school input is *public education expenditure*, which is the current and capital government spending on educational institutions (both public and private), education administration, as well as educational subsidies for private entities, such as households.

Kaufmann *et al.* (2011) collect governance data according to six dimensions: Voice and accountability, political stability, government effectiveness, regulatory quality, rule of law, and control of corruption. As discussed in the text, the measure of *governance* that we employ for most specifications includes only the variable most likely to operate through human capital accumulation: government effectiveness. Estimates for this variable are assumed to be drawn from a normal distribution centered on zero with support $[-1, 1]$. We use the lagged effectiveness variable from the year 1996. For the fuller governance measure, we equally weight the 6 dimensions in the composite score to obtain an aggregate governance measure.

Three additional instruments were used for governance in the robustness section. The first is the pervasiveness of informal payments in education, which was collected from multiple survey sources—mostly Afrobarometer Round 3, AmerciasBarometer 2006, Transparency International, and World Bank diagnostic reports—-over a range of years between 2000 and 2006. To maximize the number of observations, we utilize the nearest year to 2000, if 2000 data were not available. The other two are common to those in Acemoğlu *et al.* (2001) and Hall & Jones (1999): The mortality rates of early European settlers and the fraction of the population of European descent, specifically those speaking English and other European languages.

For additional controls introduced in the robustness section: *Trade* openness is taken to be net exports as a share of GDP, *geography* is the longitudinal distance from the equator, and *infrastructure* is proxied by road density, measured as kilometers of road per 100 square kilometer of land area. These were all from the WDI. In addition, we obtained fractionalization data from Alesina, Easterly, Devleeschauwer, Kurlat & Wacziarg (2003), democracy data from the Polity IV project Marshall & Jaggers (2008), and social capital data from the World Values Survey. *Ethnolinguistic* *fractionalization* is the sum of the ethnic and linguistic fractionalization measures, which in turn were computed as one minus the Herfindahl indices of the respective group shares in the population. The theoretical distribution has the range $[0, 2]$, with higher values indicating greater fractionalization. *Democracy* is a composite indicator of the competitiveness of executive recruitment and political participation, the openness of executive recruitment, and the strength of constraints on the chief executive; it has the integer range $[0, 10]$, with higher values indicating greater democracy. *Social capital* is a measure of trust in the society, which is calculated from the response to the question,

> "Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?"

The indicator is binomial, distribution on support $[1, 2]$, with lower values indicating greater levels of trust. Following the literature, we assumed that trust was time-invariant, and so countries with more than one survey were collapsed into a single score by simple averaging.

For alternative variables used in the robustness section: *Parental authority* is the father's share of parental authority, which ranges from 0 (half share) to 1 (full). This was obtained from the OECD's Gender, Institutions, and Development database. *Ability* was calculated average national IQ estimates, adjusted to account for time differences as a result of the Flynn (2007) effect. This was due to Lynn & Vanhanen (2002). *Achievement* is the sum of the student performance in math and reading tests, adjusted for cross-country and cross-test comparability, from Altinok & Murseli (2007).

We used *adult schooling* as an alternative measure of family input; this is the mean schooling of the population aged 25 and over, and it serves as a proxy for parental education as a family input into the education process. Adult (youth) *literacy* is the percentage of the population aged 15 and older (aged 15–24) that is able to read and write a short, simple statement on their everyday life.

A.2 Descriptive Statistics

As discussed in the text, the data sample we employ varies according to the availability of both key variables as well as additional controls. We report here descriptive statistics for the benchmark cross-section (*B2*) and panel (*P3*) regressions (Table A.1a). The specific list of countries included is listed in Table A.1b.

A.3 Bayesian Analysis of Exclusion Restriction

First, projections of the dependent variable Y/L, endogenous regressor H/L, and instrument G on the exogenous variables in the first stage, namely s_k and $(n + g + \delta)$. Second, residuals corresponding to these projections were then collected, and the variance of residuals corresponding to the

Table A.1a: Summary statistics†

	Mean	Std dev	Min	Max
	Cross section			
Output per worker	9.221	1.429	6.380	10.975
Investment share	-1.508	0.214	-1.969	-0.854
Net depreciation rate	-2.770	0.183	-3.231	-2.161
Human capital	1.756	0.594	-0.174	2.489
Family resources	1.320	0.268	0.724	1.896
School resources	3.201	0.499	2.315	4.179
Governance	0.426	1.009	-1.234	2.221
	Panel			
Output per worker	9.087	1.403	5.780	11.773
Investment share	-1.514	0.306	-2.413	-0.489
Net depreciation rate	-2.746	0.170	-3.352	-2.136
Alternative human capital	4.099	0.617	1.803	5.077
Family resources	1.339	0.406	-0.784	2.966
School resources	3.212	0.496	2.261	4.278
Governance	0.183	0.981	-1.767	2.234

† Notes: Total number of observations in the cross section and panel were 64 and 445, respectively.

Table A.1b: List of countries†

Albania	Equatorial Guinea	Lao PDR	**South Africa**
Algeria	Eritrea	Lebanon	**Spain**
Argentina	Ethiopia	**Lesotho**	St. Vincent
Austria	**Finland**	Luxembourg	Sudan
Bangladesh	**France**	Macao, China	**Swaziland**
Belgium	Gabon	Madagascar	**Sweden**
Belize	**Gambia**	**Malaysia**	Switzerland
Benin	**Germany**	Maldives	**Syria**
Bhutan	**Ghana**	**Mali**	Tajikistan
Bolivia	**Greece**	Mauritania	*Tanzania*
Botswana	**Guinea**	**Mauritius**	**Thailand**
Brunei	Guinea-Bissau	**Mexico**	Timor-Leste
Burkina Faso	**Guyana**	Morocco	**Togo**
Cambodia	**Honduras**	**Mozambique**	**Trinidad and Tobago**
Cameroon	**Hong Kong SAR, China**	Namibia	**Tunisia**
Canada	**Hungary**	**New Zealand**	**Uganda**
Cape Verde	**Iceland**	Norway	United Arab Emirates
Chad	**India**	Paraguay	**United Kingdom**
Chile	**Indonesia**	**Peru**	**United States**
China	**Iran**	Philippines	**Uruguay**
Comoros	**Ireland**	**Portugal**	Venezuela
Congo-Kinshasa	**Italy**	**Romania**	Vietnam
Cote d'Ivoire	**Japan**	**Russia**	West Bank and Gaza
Croatia	Jordan	**Rwanda**	Yemen
Denmark	**Kenya**	**Senegal**	**Zambia**
Dominican Republic	**Korea, Rep.**	**Slovakia**	
Egypt	Kyrgyz Republic	**Slovenia**	

† Notes: Countries in **bold** appear in both the cross section and panel. Countries in *italics* appear only in the cross section.

instrument was normalized to one. Third, 10,000 draws were taken from the posterior distribution of π_2, for alternative values of ω. Finally, the 2.5th, 50th, and 97.5th percentiles of this distribution were computed, together with the interquantile ranges. The procedure is described in greater detail in Kraay (2012).

A.4 Estimation of the Benchmark in Loglinear Form

Here we report, without additional comment, the benchmark results in Table 1 with human capital not transformed into logarithmic form.

Table A.2: Benchmark regressions of GDP per capita with level human capital†

	(B1)	*(B2)*	*(B3)*	*(B4)*	*(B5)*	*(B6)*
	Second stage income equation					
Investment share	0.463	1.156	1.404	0.243	0.737	0.389
	(0.31)	(0.46)**	(0.48)***	(0.37)	(0.20)***	(0.27)
Net rate of depreciation	-0.412	1.185	1.258	1.830	-0.208	1.017
	(0.62)	(0.97)	(1.06)	(0.86)**	(0.82)	(0.69)
Human capital	0.400	0.601	0.588	0.633	0.494	0.548
	(0.03)***	(0.07)***	(0.07)***	(0.07)***	(0.06)***	(0.05)***
Constant	6.111	10.289	10.969	10.517	6.442	8.984
	(1.63)***	(2.65)***	(2.93)***	(2.19)***	(2.02)***	(1.72)***
	First stage human capital equation					
Family resources		-1.945	-3.653		-1.865	
		(1.44)	(1.68)**		(1.23)	
School resources		-1.475	-1.665	-1.591		
		(0.77)*	(0.79)**	(0.65)**		
Governance		1.207		1.163	1.567	1.642
		(0.32)***		(0.27)***	(0.24)***	(0.17)***
Broad governance			1.318			
			(0.35)***			
Constant		-3.012	-2.321	-2.015	-10.995	-10.843
		(6.86)	(7.34)	(5.11)	(4.60)**	(3.24)***
Adj R^2	0.741	0.687	0.679	0.596	0.689	0.779
Anderson *LR*		31.544***	29.135***	40.837***	27.779***	39.049***
Cragg-Donald *F*		19.187	16.948	38.089	34.193	94.825
Hansen *J*		1.717	1.535	0.255	0.011	0.000
N	103	64	60	83	78	103

† Notes: All variables, except human capital, were transformed to logarithmic form. Huber-White (robust) standard errors reported in parentheses. First stage regressions included second stage controls as instruments, but are not reported. Hansen statistics for exactly identified models are replaced with a dash. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level.

A.5 Additional Subsamples

Here we report, without additional comment, panel regressions of subsamples of the data divided by income level level, with countries grouped into high income (including both OECD and non-OECD countries), lower-middle and upper-middle income, and low income (Table A.3).

Table A.3: Panel regressions of GDP per capita, by income level†

	Low	Lower middle	Upper middle	High
Investment	0.034	0.121	0.248	0.133
share	(0.06)	(0.13)	(0.10)**	(0.10)
Net rate of	0.128	-0.140	0.161	-0.173
depreciation	(0.22)	(0.25)	(0.10)*	(0.20)
Human	0.561	1.073	1.860	1.949
capital	(0.30)*	(0.30)***	(0.64)***	(1.15)*
F	3.689***	9.429***	4.786***	16.100***
Anderson LR	10.264***	12.344***	8.166***	4.915
Cragg-Donald F	3.423	4.168	2.668	1.598
Hansen J	1.327	1.967	6.138**	2.510
N	121	111	71	132

† Notes: All variables transformed to logarithmic form. Heteroskedasticity- and autocorrelation-robust (asymptotic) standard errors reported in parentheses. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level.