Revisiting the Role of Education for Agricultural Productivity

Malte Reimers and Stephan Klasen

Various recent cross-country regressions have detected insignificant or even surprisingly negative effects of schooling on agricultural productivity. Applying advanced panel econometric techniques to a sample of 95 developing and emerging countries from 1961 to 2002, we show that these results are due to a problematic reliance on enrollment and literacy indicators. Using data on educational attainment, we instead find a sizable and significant impact of schooling (avg. increase of approx. 3.2% per year of schooling) on agricultural productivity that is robust to estimation methods and model specification. We also find that returns from schooling are higher in technologically more advanced countries.

Key words: agricultural productivity, agricultural production function, cross-country regression, education, human capital.

JEL codes: I20, I25, O13, O15, O47, Q10.

Early studies on the determinants of agricultural productivity[1] across countries typically found significant positive coefficients for education variables, implying that higher levels of schooling lead to higher productivity (e.g. Hayami and Ruttan 1970; Nguyen 1979; Kawagoe, Hayami, and Ruttan 1985; Lau and Yotopoulos 1989[2]). However, these findings are contrary to those of some more recent studies that apply more sophisticated econometric methods that either did not include education variables at all in the model (e.g. Frisvold and Ingram 1995) or found insignificant (Vollrath 2007) or even puzzling negative coefficients for the education variables used (Craig, Pardey, and Roseboom 1997). Hence, the existing literature can be judged as rather inconclusive regarding the role of education for agricultural productivity in the international context.[3]

This is surprising given that the majority of micro studies find a significant positive effect for education (e.g. Ali and Flinn 1989; Young and Deng 1999; Alene and Manyong 2006). Indeed, Phillips (1994, p. 149) even states that "there is a general consensus that education has a positive effect on agricultural productivity." Regarding the mechanism leading education to affect agricultural productivity, various arguments have been proposed and empirically tested in the literature. First, education is

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[1] The cited literature uses the term of agricultural productivity rather inconsistently to refer to either the (partial) productivity of land or labor (broadly defined as output per hectare and worker, respectively). In our analysis, we follow the example of the relatively recent articles by Frisvold and Ingram (1995) and Vollrath (2007), and will refer to the (partial) productivity of land when speaking of agricultural productivity. However, it should be noted that in our regression framework below, the two concepts are closely related. In fact, if one uses labor productivity as a dependent variable and uses land per worker (and other conventional inputs likewise per worker) instead of workers per ha (as we do in the land productivity regressions) as an additional covariate, the coefficients for education are identical to the ones reported here for land productivity.

[2] The article of Fulginiti and Perrin (1993) can also be counted to this literature even though its focus is rather on explaining changes in total factor productivity (TFP) than in the (partial) productivity of land or labor.

[3] There is a related but somewhat distinct debate on the role of education on income growth in developing countries. See, for example, Pritchett (2001).

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supposed to let farmers become better "managers" by enhancing their decision-making skills (Asadullah and Rahman 2009). Second, education improves the peasant's access to information and therefore should allow him/her to potentially pay and receive better prices for the inputs used and the outputs sold (Jamison and Lau 1982). Third, various empirical studies have shown that on average, better educated farmers are adopting promising new technologies faster, and therefore have a first-mover advantage (Feder, Just, and Zilberman 1985; Hossain et al. 1990; Lin 1991; Asfaw and Admassie 2004; Weir and Knight 2004). Last, it is regularly argued that as a consequence of improved decision-making skills, better-educated peasants generally prefer riskier production technologies (typically promising higher returns) since they are able to adequately evaluate the implied opportunities and risks (Asadullah and Rahman 2009).

Given the preceding list of arguments supporting the view that rural education should enhance agricultural productivity, it remains an open question why cross-country studies using advanced econometric techniques were not able to find such an effect. In this article, we show that these studies fail to detect the expected impact because they are using inadequate variables (enrollment and literacy rates) to approximate the stock of education. Using a panel of 95 developing and emerging countries, together with the newest version of the Barro-Lee educational attainment dataset (Barro and Lee 2010), we show that there is indeed a positive impact of educational attainment on agricultural productivity that is robust to changes in the control variables and in the econometric methods applied. Furthermore, distinguishing between different levels of education reveals that only primary and secondary schooling attainments have significant positive impacts on agricultural productivity. In addition, the prominent argument claiming that education leads to higher agricultural productivity, particularly in the presence of rapid technical progress (Nelson and Phelps 1966; Schultz 1975; Rosenzweig 1995; Foster and Rosenzweig 1996), is tested empirically in the cross-country framework. Findings indicate that the returns to education (in terms of augmentations of the agricultural productivity) are higher for countries with higher levels of income. We not only show these effects using our data set, but are also able to show that the inclusion of our measure of educational attainment is robust to the use of data sets from other studies that previously had found no impact of education. Furthermore, we submit our results to extensive robustness checks and find very robust results in terms of magnitude and significance of effects.

The article proceeds as follows. Section 2 provides an overview of the literature on education and its effects on agricultural productivity. Section 3 describes the indicators previous cross-country studies have included in their regression to control for education, and argues why the average years of schooling as obtained from Barro and Lee (2010) are a conceptually superior proxy. Section 4 provides a description of the methodological approach and the data used. In section 5, the results of the fixed and random effects, as well as feasible generalized least squares (FGLS) models are discussed, and some further extensions of the models are introduced. Section 6 shows the results of diverse robustness checks. Finally, in section 7, the main results are summarized and conclusions are drawn.

Schooling and Agricultural Productivity: Mechanisms and Micro Evidence

Before starting a systematic review of the existing literature dealing with the question of why increased schooling could have a positive impact on agricultural productivity, it is necessary to discuss what is considered to be the effect of education in general. Nelson and Phelps (1966, p. 69) provide a widely-cited, relatively simple answer to this question by stating that education "enhances one's ability to receive, decode, and understand information." In addition, Schultz (1975, p. 835) argues that "education—even primary schooling—enhances the ability of students to perceive new classes of problems, to clarify such problems, and to learn ways of solving them." Welch (1970, p. 42) related the effect of education to agricultural production and identified two distinct phenomena through which schooling can have a productive value, namely, the "worker effect"[4] and the "allocative effect." According to Welch, the former describes the phenomenon that well-educated workers are simply able to use a given amount of

[4] The phenomenon that Welch (1970) labeled as the "worker effect" is conceptually almost equivalent to what more recent studies typically describe with the term "technical efficiency" (Azhar 1991).

resources more efficiently. In contrast, the latter is characterized by the ability of an educated worker to sufficiently "acquire and decode information about costs and productive characteristics of other inputs" (Welch 1970, p. 42). As a result, the highly educated farmer will regularly use a different mix of inputs compared to a relatively low-skilled peasant, that is, the more-educated farmer allocates resources more efficiently. This phenomenon can hence be called the "allocative effect." With regard to the relevance of these two effects for agriculture, there is nowadays a consensus among researchers that farmers' schooling generates its productive value mainly as a consequence of the allocative effect, and only to a relatively limited extent from the worker effect (Huffman 1999).

Given that the concepts of the two above-described effects are still relatively vague, more recent literature has tried to further clarify the (often interrelated) transmission channels through which rural education may enhance agricultural productivity. However, it is important to emphasize that the basic notion of education as provided by Nelson and Phelps (1966) and Schultz (1975) is still relevant for these studies. The first argument commonly used to justify a potentially positive impact of education on agricultural productivity is a direct consequence of the above definitions. If one accepts that education allows farmers to make better use of the information available, to perceive new classes of problems, and to autonomously find solutions to them, it directly follows that those peasants will possess superior decision-making skills and will hence be better "managers" who allocate their resources more efficiently (Asadullah and Rahman 2009).

As a second argument, it is often claimed that well-educated farmers are not only capable of using available information more competently, but also that they have better access to required information. Against the background that in many developing countries the majority of farmers have not received any schooling and are hence illiterate, it is easy to imagine that this lack of education is a severe obstacle for those peasants when seeking information. Thus, education provides peasants with the ability to disengage themselves from the "tight grip of […] inefficient 'word-of-mouth' communication patterns" (Welch 1970; Asfaw and Admassie 2004, p. 216). Taking this argument a step further, Jamison and Lau (1982) even argue that well-educated farmers potentially pay and receive better prices for their inputs and outputs, indicating that education can be a remedy to prevailing information asymmetries in the market.

A third argument that has received considerable attention in the literature suggests that well-educated peasants are more likely to adopt new technologies or products early since they have superior access to related information and are more capable of distinguishing between promising and unpromising innovations. In contrast, farmers with little schooling will often prefer not to introduce a new technology until its profitability has been proven, for example, waiting until other farmers have successfully adopted the innovation (Nelson and Phelps 1966). This provides the educated farmer with a first-mover advantage, making the new technology even more profitable and thus attractive for adoption. This argument is in line with the seminal contribution of Schultz (1975), who postulated that traditional agricultural societies are—in the absence of modernization—in an economic equilibrium since they have continuously optimized the use of the available resources over the generations. The occurrence of (exogenous) technological progress then pushes those societies away from the stationary state and allows them to achieve a superior equilibrium. However, the adjustment process takes time and its duration depends crucially on the population's ability to respond efficiently to the prevailing disequilibrium which can—according to Schultz (1975)—be enhanced through education. Consequently, the returns to education are expected to be higher in societies experiencing greater technical progress due to the henceforth increased level of complexity involved in the production process. On the contrary, in very traditional agricultural settings where tasks are rather simple, one would expect schooling to have only minor impacts on productivity (Schultz 1975; Schultz 1981; Rosenzweig 1995; Yang 1997). To the extent that income levels are correlated with levels and change of technology, this argument would suggest that education has a larger impact on agricultural productivity in richer countries.

In recent years, various authors have empirically tested the above-described argument that educated farmers are more likely to adopt new technologies early and have found overwhelming support for its validity (e.g., Feder, Just, and Zilberman 1985; Hossain et al. 1990; Lin 1991; Asfaw and Admassie 2004; Weir and Knight 2004). Most prominently, Foster and

Rosenzweig (1996) analyzed data from the green revolution period (specifically 1968–1981) in India and found increasing positive returns to schooling during this time of rapid technological progress. As an extension, the authors were even able to estimate the returns to schooling separately for areas faced with different levels of technological change.[5] The obtained results were in line with the above-described theoretical considerations from Schultz (1975) and a theoretical model provided by Rosenzweig (1995), indicating that the returns to schooling increased significantly more in those areas where a high degree of technological progress took place.

In recent years, a fourth argument has emerged from the literature that is strongly linked to the previous argument. To wit, it is claimed that educated farmers adopt new technologies earlier because they are more willing to adopt riskier production technologies if these technologies provide higher expected returns (Knight, Weir, and Woldehanna 2003; Asadullah and Rahman 2009). Hence, education is supposed to decrease the perceived level of uncertainty and therefore to reduce the farmer's aversion towards endogenous risk[6], that is, risks arising from the peasant's choice of production technology. Knight, Weir, and Woldehanna (2003) tested this hypothesis empirically using household data from rural Ethiopia, and found a significant reduction of risk aversion if the household head had received at least some schooling. This result implies that providing education to farmers not only lets them adopt *new* technologies earlier, but it may also change their attitude towards relatively risky *traditional* production technologies (e.g. crops they did not dare to plant previously). As a consequence, the farmer may–after having received some schooling–optimize his mix of crops (including also riskier crops if they provide high expected returns) based on an improved ability to evaluate the associated risks and opportunities.

In contrast to the above-described four major arguments from the literature regarding why increased education should have a positive effect on agricultural productivity, one could also put forth theoretical reasons why studies find the returns to education to be small or even absent. First, it could be—following an argument of Pritchett (2001)—that the quality of education is just too low to effectively increase cognitive abilities and, ultimately, also productivity. Second, the skills provided in formal education may simply be too unspecific to positively affect agricultural productivity. However, this would immediately raise the question of why most of the above-described micro studies were then able to find a productivity-enhancing effect of formal education. Third, and probably most importantly, recent micro literature has emphasized that estimates of the agricultural productivity returns to schooling may potentially be downwardly biased if the analysis does not adequately account for the endogeneity of activity choice by the farmers (e.g. Taylor and Yunez-Naude 2000; Yang and An 2002; Jolliffe 2004; Yang 2004). For instance, as illustrated in a two-activity model by Taylor and Yunez-Naude (2000), it may be beneficial for rural households to allocate at least parts of the available investment resources (e.g. labor or land) away from crop production towards non-crop production if the marginal effect of schooling on the net income-productivity of investments of the latter exceeds that of the former. Applied to our case, this implies that we may potentially underestimate the total returns to schooling because we are limiting our analysis to the agricultural sector and will therefore not fully capture the returns to schooling of those educated individuals who decide to allocate at least parts of their human capital to sectors other than agriculture (where the returns, to their knowledge, are probably larger).

Besides the above-described literature on the relationship between overall education and agricultural productivity, various relatively recent studies have focused on particular aspects of this nexus. For instance, Asadullah and Rahman (2009) distinguished between different levels of education obtained by rice-producing households in Bangladesh. Their results show—not entirely surprisingly—that basic education (defined as primary and secondary schooling) is relatively more important for agricultural productivity than higher education. Other studies have focused on the question of whether agricultural households are benefitting from externality effects. While the findings consistently affirm the existence

[5] This extension was possible since the contemplated Indian areas differed substantially with regard to (exogenous) weather and soil conditions, and therefore did not have the same ability to exploit the new seeds profitably (Foster and Rosenzweig 1996, p. 932).

[6] The literature typically subdivides risks into endogenous and exogenous (Knight, Weir, and Woldehanna 2003). While the former arises directly from the peasant's choice of technology (e.g. mix of crops), the farmer is not able to influence the latter risk, which is thus exogenous to him (e.g. variability in rainfall).

of intra-household externalities (Yang 1997; Asfaw and Admassie 2004), implying that there is indeed a knowledge-spillover among different members of agricultural households (even when the educated person is not involved in farming activities), the evidence for extra-household externalities (meaning that better-educated neighbors have a positive impact on a household's productivity) is rather ambiguous (Foster and Rosenzweig 1995; Knight, Weir, and Woldehanna 2003; Asadullah and Rahman 2009). Given the highly aggregated nature of our country-level data, it is not possible to test adequately for the existence of externalities. However, we will make efforts to distinguish between different levels of education in our analysis in section 5.

As shown above, the literature clearly demonstrates a range of plausible mechanisms linking education to higher agricultural productivity, and these mechanisms are often found to empirically play a role in micro studies.

Problems and Issues with Measuring Education in Macro Studies

In the macro-level cross-country and panel literature on the determinants of agricultural productivity worldwide, there are four human capital measures that are regularly included in the production function to account for differences in the quality of labor. Of these four, every author typically uses two indicators in each model: one is to allow for differences in farmers' health (either life expectancy at birth or the total fertility rate), and the other is to control for differentials in education (either the adult literacy rate or the gross/net enrollment ratio for primary/secondary education). Given the focus of this article, in the following we will only discuss the appropriateness of the two education measures.

Despite their widespread use in the literature (e.g. Hayami and Ruttan 1970; Nguyen 1979; Kawagoe, Hayami, and Ruttan 1985; Lau and Yotopoulos 1989; Fulginiti and Perrin 1993; Vollrath 2007[7]), both gross and net enrollment ratios (GER and NER, respectively)[8] are rather inappropriate indicators for the current level of schooling of the working-age population in a country.[9] First, the data quality is often relatively poor since the enrollment rates are typically obtained from administrative records from schools which have a strong incentive to overstate the number of students in order to receive more resources for their institution. Second, the enrollment rates usually reflect the number of registered students at the beginning of the school year, and thus do not take into account how many pupils drop out of class in the course of the year, that is, they fail to adequately capture actual school attendance. Third and most importantly, enrollment ratios by definition only measure the *flow* of schooling and therefore provide information about the *future* and not the *current stock* of education in a country (Barro and Lee 1993). Only in the very particular case of stable enrollment rates for all countries the GERs/NERs would be able to mirror the steady-state stock of education correctly. However, this assumption is rather implausible given the substantial but heterogeneous increases in developing countries' enrollment ratios in recent years (Schultz 1988; Pritchett 2001). Hence, enrollment ratios–gross or net–do not adequately reflect what the productivity literature wants them to reflect, which is the current stock of education available in a country.

As an alternative, the adult literacy rate, typically defined as the share of the population aged 15 and above having "the ability to read and write with understanding a simple statement related to one's daily life" (UNESCO 2011), has been used by various authors to approximate the population's level of schooling (e.g. Hayami and Ruttan 1970; Kawagoe, Hayami, and Ruttan 1985; Lau and Yotopoulos 1989; Craig, Pardey, and Roseboom 1997). This is not surprising given that the adult literacy rate possesses several features one would expect from a perfect measure for the level of education. First, the concept is relatively simple and the data are available for a wide array of countries. Second, the adult literacy rate indeed gives an idea of the current stock of education among adults in a country, and is thus preferable to the enrollment ratios. However, there are also several

[7] Vollrath (2007) states in footnote 5 of his article that he tried to include primary enrollment ratios, but found them to be insignificant.

[8] According to UNESCO (2011), the NER is defined as the "enrollment of the official age group for a given level of education expressed as a percentage of the corresponding population," as opposed to the GER being defined as the "total enrollment in a specific level of education, regardless of age, expressed as a percentage of the eligible official school-age population corresponding to the same level of education in a given school year."

[9] See also Barro and Lee (1993) for an extensive discussion of these issues.

drawbacks. Most importantly, the adult literacy rate must be judged as a relatively "crude measure of schooling" (Huffman 1999, p. 31) since it only refers to the "first stage in the path of human capital formation" (Barro and Lee 1993, p. 367) and hence does not sufficiently allow us to assess the full extent of education. As a consequence, the adult literacy rate—if used as an indicator for the quality of labor in a productivity analysis—has the inherent problem that it implicitly assumes that any education higher than the most elementary level will not have any productive value (Barro and Lee 1993). Furthermore, it is by definition bounded above, that is, it is impossible to achieve literacy rates higher than 100%. Because of this feature, the variation between countries is artificially reduced, particularly when contemplating middle- or high-income countries. These last two drawbacks can, for instance, be exemplified with data for the Maldives and Israel. While these two countries are almost equal in terms of the adult literacy rate (97.0% and 97.1%, respectively (UNDP 2009)), the average years of schooling differ substantially, with a Maldivian adult receiving an average of 6.14 years of schooling compared to an average of 11.33 years for an Israeli (both numbers from Barro and Lee 2010). This obvious discrepancy in the educational attainment in the two countries is not reflected sufficiently in the data and is thus ignored when taking the adult literacy rate to approximate the current stock of schooling.

Considering the problems of the two schooling indicators from an econometric point of view, one can consider enrollment and literacy rates as variables that measure the true stock of education with error. As is well-known, measurement error leads to a downward bias in estimated coefficients, which might explain the failure to find effects using these proxies.

In short, the two above-described measures both suffer from severe methodological weaknesses and do not adequately reflect the stock of education currently available in a country. Against this background, in the early-1990s Barro and Lee (1993) introduced their educational attainment data set, which has since then been methodologically improved and regularly updated (Barro and Lee 1996; 2001; 2010). This data set is mostly based on reported school attainment data from census and household surveys (mainly compiled by UNESCO and Eurostat) which are then projected using robust simulation methods to generate the achievement data for the benchmark years. In particular, Barro and Lee (2010) calculate—as a first step—the educational attainment of the population by 5-year age groups, and then split the distribution into four rather broad attainment categories.[10] As a next step, forward and backward extrapolation is used to fill in missing observations, with each group being assigned an age- and education-specific mortality rate (hence not assuming a uniform mortality). Currently, the variables from the data set are widely accepted as providing the most reliable proxies for the stock of education for a large number of countries.[11]

For the analysis conducted in this article, we therefore decided to use the newest version of their data set (Barro and Lee 2010), which offers 5-year averages of educational attainment for 146 countries from 1950 to 2010. In particular, we will use the average number of years of schooling (s_t) for the population aged 15 and above, which the two authors defined as:

$$(1) \qquad s_t = \sum_{a=1}^{A} l_t^a s_t^a \quad \text{with} \quad s_t^a = \sum_j h_{j,t}^a Dur_{j,t}^a$$

where l_t^a denotes the share of age group a in the population aged 15 and above, s_t^a is the average number of years of schooling of age group a, h_j^a corresponds to the share of the age group a having attained the schooling level $j =$ *primary*, *secondary, tertiary*, and *Dur* represents the duration in years corresponding to the respective level of education (Barro and Lee 2010, p. 7).

We argue that this indicator is methodologically superior to the measures previously used in the literature on the determinants of agricultural productivity worldwide because it shares the desirable characteristics of the adult literacy rate (relatively simple concept, good availability of data, actually measuring the *current* stock of education), and additionally has the advantage of not being restricted to the most basic level of education. Therefore, the variable accounts more adequately for the full depth of education.

However, it is important to emphasize that the Barro-Lee measures still do not meet two requirements that one would expect of the

[10] Namely, no formal education, primary education, secondary education, and tertiary education.

[11] For example, the UNDP recently replaced the adult literacy rate with the total years of schooling variable from Barro-Lee in the education component of the widely-noted Human Development Index (UNDP 2009).

"perfect" measure of education in our particular context. First, the indicators are exclusively focused on the quantity of schooling received by the population and only partly account for quality differences. In particular, only to the extent that a student's achievements were insufficient to pass a grade will this be reflected in the educational attainment indicator, which measures years passed (rather than years attended); quality differences beyond passing or failing a grade are not considered. Second, it would be highly desirable for the measure to be disaggregated into rural and urban areas since the vast majority of agricultural labor resides in rural areas. When testing the robustness of our results in section 6, we will attempt to overcome this shortcoming. Despite these caveats, it is clear that taking the average years of schooling, as of Barro and Lee (2010), presents a crucial improvement to indicators previously used in the literature to approximate a country's stock of education.

From here onward, the argumentation of this article is as follows. Based on the extensive theoretical considerations provided in section 2, as well as the empirical evidence found in two early meta studies (Lockheed, Jamison, and Lau 1980; Phillips 1994[12]) and numerous micro studies (e.g. Ali and Flinn 1989; Young and Deng 1999; Alene and Manyong 2006), the hypothesis is that rural education increases, on average, agricultural productivity. However, this stands in sharp contrast to recent cross-country studies applying modern econometric methods (particularly panel estimation techniques including time and country dummies), which either did not include any education variables in the model (e.g. Frisvold and Ingram 1995), or found insignificant (Vollrath 2007) or even puzzling negative coefficients for the education variables used (Craig, Pardey, and Roseboom 1997). Against the background of the above-described inadequacy of the education indicators used in those articles (adult literacy rate or gross/net enrollment ratio for primary/secondary education), we argue—in line with Huffman (1999)—that the inability to detect the expected robust positive impact for education in the cross-country framework is rather due to data problems than to the absence of real effects. This hypothesis will be tested in the following empirical part of the article using the newest version of the Barro-Lee educational attainment dataset, but using the same advanced econometric framework and covariates of the recent studies that had failed to find an effect.

Methodology and Data

Our methodological approach is generally in line with the recent cross-country and panel literature on the determinants of agricultural productivity (e.g. Craig, Pardey, and Roseboom 1997; Vollrath 2007). We are assuming that the production process for the ith country at time t follows a common Cobb-Douglas production function. In particular, we estimate the following specification:

$$\text{(2)} \qquad ln\, y_{it} = \alpha + \beta_x ln\, X_{it} + \beta_E E_{it} + \beta_V V_{it} + \beta_C C_i + \gamma_i + \delta_t + \varepsilon_{it}$$

where the dependent variable is the natural logarithm of the output per ha and X_{it} is a vector of conventional agricultural inputs taken in per hectare terms. The variable E_{it} is the above-described indicator for the average years of schooling as obtained from Barro and Lee (2010)[13]. Thus, β_E is the coefficient of main interest, reflecting the partial productivity effect of education, and it is expected to be positive. In addition, we also include V_{it}, which represents a vector of time-varying with C_i being a vector of time-invariant controls in the model (only when using random effects). Lastly, γ_i and δ_t, respectively, are country- and time-specific fixed effects typically included in panel models, and ε_{it} is the potentially heteroskedastic error term. In some specifications, we also use random effects to reproduce other results from the literature and to be able to include C_i, our time-invariant country characteristics.

Following standard practice in this area of the literature, we take the total value of all agricultural production after deductions for feed and seed (all expressed in 1999–2001 international $) divided by the total agricultural area in ha (both obtained from the Food and Agricultural Organization of the United Nations' statistical database [FAOSTAT]) as the dependent variable (see the supplementary appendix online for the exact specifications and

[12] However, the results of such meta-studies should always be regarded with the necessary caution, since they implicitly assume that the methods and models of all contemplated studies were appropriate.

[13] Against the background that the average years of schooling are only available in five-year intervals, while all other variables in our model are disposable on an annual basis, we decided to linearly interpolate the schooling data.

sources of all variables). The X_{it} vector contains four conventional inputs typically included in production functions, namely labor, fertilizer, tractors, and livestock (all in per hectare terms).[14] Data for the conventional inputs are all obtained from FAOSTAT, whereat the livestock data is converted into cattle equivalents using weights from Hayami and Ruttan (1985).[15]

The vector of time-varying controls V_{it} can be subdivided into up to four categories. The first group of variables intends to account for differences in the quality of land. Therefore, we included the share of agricultural land equipped for irrigation and the percentage of agricultural land that is used as permanent meadows and pastures (both obtained from FAOSTAT) into our regression.[16] To further allow for differentials in climate, we additionally used satellite data reflecting average precipitation on agricultural land in year t for country i (data from Williams and Breneman (2009)). As a second group of time-varying controls, two road traffic-related variables are used to ensure that the human capital variables do not only capture the potentially positive effect of a well-developed infrastructure. Typically, for this purpose researchers use road density, defined as the total road length in km per 100 square kilometers of land area (data taken from Canning (1998) and WDI online, respectively). However, we argue that this concept is too narrow since it is not the pure disposability of roads that generates a productive value, but rather the effective ability of the economy to regularly use these roads. Therefore, we additionally attempt to use the per capita road sector energy consumption[17] (data from WDI online) to account for differentials in infrastructure. Thirdly, it is important to rule out the possibility that differences in the quality of institutions are driving the results. To allow for this, we further include the political risk index taken from the International Country Risk Guide (ICRG), which is a commonly used indicator for a country's political stability (Political Risk Services 2005). The last category contains additional human capital variables not included in E_{it}. In particular, two alternative measures are, in line with the literature, used to account for differences in the population's health level, namely life expectancy at birth and total fertility rate. In addition to the above-described time-varying controls, dummy variables for the legal origin as derived by La Porta et al. (1999) were included in the C_i vector of our model (only in the random effects specification) to allow for time-invariant differences in the legal system of the countries.[18] To give an overview on the data used for the analysis, table 1 presents summary statistics for the above-described variables.

A standard assumption in cross-country regressions trying to explain differences in agricultural productivity is that there exists a common production function that is applicable to all countries in the sample–a so-called "meta-production function" (Hayami and Ruttan 1970; Kawagoe, Hayami, and Ruttan 1985). Without any doubt, this assumption is strong and it can plausibly be argued that the agricultural production process differs between industrialized and developing countries. Taking such objections seriously, the 34 current Organisation for Economic Co-operation and Development (OECD) member countries were dropped from the dataset in order to reduce its heterogeneity, so that we are left with a sample of developing and emerging countries. In a robustness analysis below, we further trim the sample to reduce heterogeneity. We further excluded countries/territories with very small agricultural areas or labor forces to minimize measurement error since the corresponding data from FAOSTAT are generally rounded to thousands, leading to a severe bias for countries having only small values for these variables.[19]

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[14] Land is not included as a separate input in the equation since constant returns to scale are assumed and the variable thus cancels out.

[15] Their widely-used weighting scheme allows us to transform the headcounts of different animals into comparable units by assuming that 1 horse = 1 mule = 1 buffalo = 1.25 cattle = 1.25 asses = 0.9 camels = 5 pigs = 10 sheep = 10 goats = 100 chickens = 100 ducks = 100 geese = 100 turkeys.

[16] We also tried the land quality index from Peterson (1987), which is time-invariant and would thus belong to the vector C_i. However, it turned out that this variable greatly reduced our sample without adding any meaningful information.

[17] The correlation ρ between the two measures in our dataset is approximately 0.45, implying that it is in fact possible to include both variables at the same time without introducing a multicollinearity problem.

[18] According to La Porta et al. (1999), it is possible to classify a country's legal origin in one of the five following groups: English common law, French commercial code, German commercial code, Scandinavian commercial code, and Socialist/Communist law.

[19] The countries/territories dropped due to these two exclusion criteria are: American Samoa, Andorra, Anguilla, Aruba, Bahamas, Bahrain, Barbados, Bermuda, British Virgin Islands, Brunei, Cook Islands, Falkland Islands, Faroe Islands, French Guiana, Gibraltar, Greenland, Guadeloupe, Holy See, Kuwait, Liechtenstein, Luxembourg, Malta, Martinique, Monaco, Montserrat, Nauru, Netherlands Antilles, Niue, Norfolk Island, Northern Mariana Islands, Qatar, Palau, Saint Helena, Saint Kitts and Nevis, San Marino, Seychelles, Singapore, Tokelau, Turks and Caicos Islands, Tuvalu, United States Virgin Islands, and the Wallis and Futuna Islands.

Table 1. Summary Statistics

Variable	Observations	Mean	SD	Min.	Max.
Net production per ha (intl. $)	3,282	271.87	296.28	2.68	2,063.74
Workers per 1,000 ha (number)	3,282	424.94	605.59	1.99	4,498.84
Tractors per 1,000 ha (number)	3,282	3.50	10.25	0.00	117.36
Livestock per 1,000 ha (in cow equivalents)	3,282	477.91	428.12	4.40	2,801.47
Fertilizer per 1,000 ha (in tons)	3,282	20.26	36.86	0.00	337.84
Irrigated land (in % of total)	3,282	6.26	10.10	0.00	67.12
Land in pasture (in % of total)	3,282	58.00	28.80	0.71	99.50
Precipitation on agricultural land (in mm)	2,184	1,164.65	701.55	6.00	3,738.00
Road density (km of roads per 100 sq. km of land)	1,654	17.24	24.32	0.26	220.13
Road sector energy consumption p. c. (kt of oil equivalent)	1,970	0.16	0.26	0.00	2.53
Life expectancy at birth (in years)	3,282	57.98	10.16	26.41	78.88
Total fertility rate (children per women)	3,273	5.16	1.80	1.09	8.73
Total years of schooling	3,282	4.40	2.52	0.04	10.80
Years of primary schooling	3,282	3.13	1.68	0.04	7.31
Years of secondary schooling	3,282	1.15	1.00	0.00	5.64
Years of tertiary schooling	3,282	0.12	0.15	0.00	1.07
Political Risk Index (ICRG)	1,288	56.34	12.30	14.08	81.67

Finally, we also made efforts to clean our sample of all observations that are biased due to major natural disasters and/or armed conflicts. To account for the former, for each year we divided the number of inhabitants affected by earthquake, floods or droughts (obtained from Emergency Disasters Database (2011)) by the total population of the country. All observations where this ratio exceeded the threshold of one-third were then excluded from the analysis since under these circumstances we consider efficient agricultural production to be practically impossible. In addition, we took battle deaths data from the Centre for the Study of Civil War (Lacina and Gleditsch 2005), and analogously calculated the share of the population that was killed in a specific year due to armed conflicts. We argue that a share of 0.1% (i.e. one in 1,000 inhabitants), together with the associated flow of refugees, is sufficient to impede efficient agricultural production. Hence, we dropped all corresponding observations from the dataset (see the supplementary appendix online for a list of observations that were dropped due to the two exclusion criteria). As an alternative to dropping these observations, one could also include a dummy variable to account for natural disasters or armed conflicts. The results for this second alternative do not differ from those obtained when dropping the affected observations.[20]

The result of these modifications is a non-balanced panel covering 95 countries[21] from 1961 to 2002 (in most specifications our panel only reaches from 1976 to 2002 due to the unavailability of data for some of the covariates for early years). Using a non-balanced panel for the estimation could generally cause the coefficients for different points in time to differ due to the fact that different samples are used. However, we do not see this as a major problem in our case for two reasons. First, all our regressions include country and year fixed effects, which should capture the vast majority of the variation caused by changes in the sample used. Second, it is not the goal of this article to compare the effect of education over time (in that case a non-balanced panel could severely bias the results), but rather to estimate the *average* effect of education on agricultural productivity.[22]

[20] The results for this second alternative can be seen in the supplementary appendix online.

[21] A detailed list of the countries and the number of observations is provided in the supplementary appendix online.

[22] To further test whether the use of a non-balanced panel affects our results, we re-estimated table 2 using a quasi-balanced panel (only including those countries in the sample where we have data

Table 2. Results of the Panel Regressions

	(1) RE	(2) RE	(3) FE	(4) FE	(5) FGLS	(6) FGLS
(log) Livestock per ha	0.294***	0.291***	0.291***	0.293***	0.269***	0.291***
	(7.035)	(6.294)	(6.650)	(5.255)	(16.722)	(13.577)
(log) Fertilizer per ha	0.076***	0.056***	0.066***	0.060***	0.014***	0.017***
	(5.055)	(3.012)	(5.170)	(3.186)	(5.360)	(4.130)
(log) Tractors per ha	0.082***	0.095***	0.061***	0.076***	0.060***	0.063***
	(3.553)	(3.407)	(2.919)	(2.814)	(10.372)	(8.285)
(log) Workers per ha	0.162***	0.264***	0.106*	0.209***	0.177***	0.236***
	(2.830)	(4.281)	(1.738)	(2.766)	(8.023)	(8.067)
Area equipped for irrigation (%)	0.006**	0.004*	0.004*	0.004	0.005***	0.005***
	(2.264)	(1.918)	(1.691)	(1.579)	(5.493)	(4.650)
Permanent meadows and pastures (%)	−0.007**	−0.012***	−0.006*	−0.010***	−0.006***	−0.008***
	(−2.208)	(−4.557)	(−1.785)	(−2.670)	(−6.399)	(−7.068)
Life expectancy at birth	0.010**	0.013***	0.011***	0.012***	0.011***	0.011***
	(2.549)	(3.428)	(3.030)	(3.173)	(8.814)	(7.239)
Total years of schooling	0.060**	0.065***	0.053**	0.063**	0.033***	0.032***
	(2.442)	(2.991)	(2.020)	(2.512)	(4.325)	(3.542)
Road sector energy consumption		0.254**		0.226*		0.264***
		(2.302)		(1.838)		(3.615)
(log) Precipitation (mm)		0.035		0.027		0.021**
		(1.268)		(1.161)		(2.394)
Political Risk Index	−0.012	0.005				
	(−1.366)	(0.717)				
Dummy for French legal origin[a]	0.095	0.388***				
	(0.688)	(3.564)				
Dummy for Socialist legal origin	−0.004	−0.001				
	(−0.018)	(−0.007)				
Constant	6.789***	5.555***	5.843***	5.874***	5.405***	5.561***
	(12.044)	(12.369)	(20.720)	(14.498)	(44.843)	(37.108)
Observations	2,791	1,609	3,282	1,685	3,282	1,685
Number of countries	79	69	95	74	95	74
Country controls included	yes	yes	no	no	no	no
Time fixed effects	yes	yes	yes	yes	yes	yes
Country fixed effects	no	no	yes	yes	yes	yes
ε_{it} autocorrelation	none	none	none	none	AR(1)	AR(1)
Hausman test statistic[b]			175.09	68.57		
Hausman test p-value			0.00	0.00		
Wooldridge test statistic[c]			27.28	18.12		
Wooldridge test p-value			0.00	0.00		
Augm. Dickey-Fuller test statistic[d]	229.20	269.48	293.16	275.63	293.16	275.63
Augm. Dickey-Fuller test p-value	0.00	0.00	0.00	0.00	0.00	0.00
Phillips-Perron test statistic[d]	306.73	279.17	438.77	284.96	438.77	284.96
Phillips-Perron test p-value	0.00	0.00	0.00	0.00	0.00	0.00
R^2	0.891	0.936	0.840	0.912	n.a.	n.a.

Notes: The dependent variable is the logarithm of the net agricultural production per ha (in intl. $). Robust z-statistics are given in parentheses. Single asterisk (*) denotes significance at the 10% level, double asterisk (**) denotes significance at the 5% level, and triple asterisk (***) denotes significance at the 1% level.

[a]The sample does not include any countries of German or Scandinavian legal origin; the left-out category is British legal origin.

[b]Hausman test statistic is distributed as χ^2_{42} in column (3) and χ^2_{30} in column (4).

[c]Wooldridge test statistic is distributed as F(1,94) in column (3), and F(1,73) in column (4).

[d] For the unit root tests, we applied the trend (due to a clear upward trend in avg. productivity worldwide) and the demean (to mitigate the potential impact of cross-sectional dependence) options. Choice of the lag structure was based on Akaike's and Schwarz's Bayesian Information Criteria (AIC and BIC, respectively) which uniformly recommended applying an AR(1) structure for the tests. Test statistics are distributed as χ^2_{158} in column (1), as χ^2_{138} in column (2), as χ^2_{190} in columns (3) and (5), and as χ^2_{148} in columns (4) and (6).

Regression Results

As a first step of the analysis, two random-effect models (RE) are applied to the data (table 2). In the most basic specification (column 1), we include, in addition to the four conventional agricultural inputs (X_{it}),the share of the agricultural land equipped for irrigation and the percentage used as permanent meadows and pastures. The coefficients of all variables are statistically significant and show the expected positive signs. The statistically significant negative coefficient for permanent meadows and pastures is also not surprising, given that a high value for this indicator is typically an indication of relatively low quality of the agricultural land, which is presumably the reason for extensive land use as meadows and pastures.[23] We further include the ICRG political risk indicator (taken as an average for each country and assumed to be stable over time[24]) and the La Porta et al. (1999) legal origin dummies as two time-invariant country controls (C_i), as well as year dummies (δ_t). The political risk indicator has no significant impact, while the legal origin dummy has a significant impact in one specification. Of course, the variables of main interest are those measuring human capital, namely, life expectancy at birth and, particularly, the average years of schooling, which both show highly significant and positive coefficients. This implies that a higher level of education increases agricultural productivity, which is in line with the theoretical considerations from section 2. The point estimate suggests that an additional year of schooling improves agricultural productivity by around 6%, a sizable effect that is not only statistically but also economically significant.

It could still be argued that this finding is biased, as no controls for differences in climate or infrastructure across the countries were included in the model. However, as can be seen in column 2, this is apparently not the case since the inclusion of corresponding variables, that is, the road sector energy consumption and the natural logarithm of average precipitation, does not materially change the results for the schooling variable in terms of size and significance (due to unavailability of data for early years, the inclusion of these two covariates reduces our sample to the years 1976 to 2002).[25] Instead, this even increases its statistical significance to the 1% level and enhances the explanatory power of our model.

As a second step, we re-estimate the model using a fixed-effects specification (FE) with time dummies, thereby dropping the time-invariant controls (C_i). As can be seen in columns 3 and 4, using the fixed-effect instead of the random-effect specification does not materially change the results of our analysis. In particular, the coefficient of the schooling variable remains consistently positive, of similar size (5-6% return for a year of schooling), and highly significant. The Hausman specification test suggests that only the fixed-effect estimator is consistent and thus preferable.

The two estimation methods used so far do not control for serial correlation in the error terms. In line with Vollrath (2007), this assumption is questionable in the context of agricultural productivity analysis since various types of shocks are probably persistent over time (e.g. adverse weather conditions). To account for this possibility, the Wooldridge test for serial correlation (see Wooldridge 2002, p. 282) is applied, and in both cases the hypothesis of no first-order autocorrelation is strongly rejected. Hence, it is necessary to allow ε_{it} not only to be heteroskedastic (by calculating robust standard errors), but also to permit the error structure to follow an AR(1) process of the type $\varepsilon_{it} = c + \rho\varepsilon_{i,t-1} + \eta_{it}$, with ρ having a value between 0 and 1, and η_{it} being a white noise process with zero mean and variance σ^2_η. With regard to the parameter ρ, there are generally two possibilities. On the one hand, it could be presumed that the errors follow a *unit-specific* first-order autoregressive process (thus using ρ_i instead of ρ in the equation). On the other hand, it is also possible to assume the parameter to be *homogenous* across countries (consequently using ρ).

for at least 40 years for the simplest specification). Even though this drastically reduced our sample, the coefficients for the average years of schooling variable remained highly significant and positive in the preferred specifications. This finding further increases our confidence that the use of a non-balanced panel is rather unproblematic in our case.

[23] This is consistent with the fact that the variable is highly negatively correlated ($\rho \approx -0.75$) with the land quality index from Peterson (1987).

[24] This assumption is in line with the literature (e.g. Vollrath 2007), and in our case is necessary since the political risk index is only available for the years 1984 onwards, and we would thus lose all observations prior to this year. However, relaxing the assumption does not materially change our results (see robustness checks in table 6).

[25] In addition, we estimated the specifications of columns 1, 3, and 5 using the reduced sample from 1976 to 2002 to investigate whether changes in the sample are responsible for changes in the estimated coefficients. However, results indicate that this is rather not the case.

Beck and Katz (1995) convincingly showed–using Monte Carlo simulations–that using feasible generalized least squares (FGLS) under the assumption of a unit-specific ρ_i leads to severely underestimated standard errors, implying extreme overconfidence in the coefficients, when T is not at least as large as N. Given that T in our dataset is considerably smaller than N (27 years compared to 95 countries), we decided to assume ρ to be homogenous across countries.

Hence, the two variants of the model are–as a third step–re-estimated using feasible generalized least squares methods with time and country dummies included in all specifications (columns 5 and 6). As can be seen, the FGLS results are generally in line with those obtained using the RE- and FE-models. However, the coefficients of some of the variables changed in magnitude and/or in statistical significance. Most notably, the t-value of the total years of schooling variable increases substantially when allowing for first-order autocorrelation, while the absolute magnitude of the coefficient almost halves to 0.032 (column 6). Nevertheless, the impact of education on agricultural productivity is still sizeable, implying that if each member of the population obtained an additional year of schooling, the agricultural productivity of the country would *ceteris paribus* increase by approximately 3.2%. To illustrate the economic relevance of the estimated effect, we also calculate the total contribution of the actually observed changes in the level of education to the observed changes in agricultural productivity. This is done by multiplying the total increase in the years of education between 1976 and 2002 with the estimated coefficient, and dividing this product by the change in the log of the agricultural productivity between 1976 and 2002: $\frac{(6.48-3.55)\times 0.032}{(5.27-4.82)} \approx 20.84\%$. Using this approach, the change in the years of education accounted for more than 20% of the increase in agricultural productivity in the time period under investigation, which is indeed a sizeable contribution.

To meet objections that we may have a spurious correlation problem since we neglect the time series properties of our data (given that our time dimension is relatively large, $T = 27$ in our preferred model), we conduct unit root tests to check whether our dependent variable is a non-stationary series, that is, whether it is integrated of order one. The results of the Fisher tests (as proposed by Maddala and Wu (1999)) are unambiguous: both the augmented Dickey-Fuller and the Phillips-Perron test clearly reject the null hypothesis of an existing unit root (see table 2). Hence, spurious correlation should not influence inference in our case.[26]

As a next step, we try various extensions of our model (table 3).[27] First, we add two variables regularly used in the literature, namely the total fertility rate and road density (see, e.g. Craig, Pardey, and Roseboom 1997; Vollrath 2007). Both variables remain insignificant at all conventional levels, while the coefficient of the average years of schooling remains relatively unaffected. Furthermore, including these two additional controls greatly reduces our sample from 1,685 to only 737 observations. Consequently, we do not consider this extension as an improvement, and therefore do not continue to include these two variables in our model. Second, we substitute our standard schooling variable by more disaggregated data reflecting the average years of schooling separately for primary, secondary, and tertiary education (also obtained from Barro and Lee (2010)). The results indicate that the effect of an additional year of schooling conspicuously differs by type of education. In particular, in our preferred model (FGLS) we find the returns to primary and secondary education to be positive and statistically significant at the 5% level, whereas the effect of tertiary schooling on agricultural productivity is not significantly different from zero.

With regard to the magnitude of the coefficients, it is a bit surprising that the coefficient for secondary education exceeds that of primary schooling. However, one explanation for this finding could be that it is not just the pure ability to read and write that causes the greatest impact on agricultural productivity, but rather advanced analytical skills (not provided in primary schools) which become—as extensively discussed in section 2—particularly

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[26] Furthermore, we used the Pesaran CD test (Pesaran 2004) to check for cross-sectional dependence in the error terms. The test indicated that there is indeed some cross-sectional dependence which may potentially lead to underestimated standard errors. However, it turns out that the results of our analysis remain largely unchanged, even when using the "correct" standard errors (following the widely used approach of Driscoll and Kraay (1998)). To additionally investigate whether any structural breaks are present in our data, we conducted various graphical inspections, but were not able to find any indication for such breaks.

[27] Given that the Hausman specification test clearly negates consistency for all random effects estimations, we only show the FE and FGLS results, whereas the FGLS estimates are preferable for the reasons discussed above.

Table 3. Extensions of the Panel Model

	(1) FE	(2) FGLS	(3) FE	(4) FGLS	(5) FE	(6) FGLS
(log) Livestock per ha	0.304***	0.220***	0.293***	0.290***	0.271***	0.266***
	(3.248)	(6.531)	(5.047)	(13.481)	(4.821)	(12.234)
(log) Fertilizer per ha	0.054**	0.015**	0.060***	0.016***	0.049**	0.012***
	(2.241)	(2.573)	(3.252)	(4.020)	(2.498)	(3.249)
(log) Tractors per ha	0.107**	0.088***	0.076***	0.064***	0.068***	0.061***
	(2.659)	(6.812)	(2.843)	(8.311)	(2.684)	(7.984)
(log) Workers per ha	0.074	0.220***	0.216**	0.236***	0.339***	0.281***
	(0.488)	(4.353)	(2.524)	(7.758)	(4.060)	(9.057)
Area equipped for irrigation (%)	0.002	0.006***	0.003	0.005***	0.004**	0.005***
	(0.621)	(3.296)	(1.537)	(4.548)	(2.058)	(4.907)
Permanent meadows and pastures (%)	−0.011	−0.013***	−0.010***	−0.008***	−0.009**	−0.006***
	(−1.570)	(−5.973)	(−2.698)	(−6.952)	(−2.368)	(−5.419)
Life expectancy at birth	0.013*	0.013***	0.012***	0.011***	0.010**	0.009***
	(1.879)	(4.020)	(3.101)	(7.222)	(2.157)	(6.235)
Total years of schooling	0.056*	0.048***				
	(1.755)	(3.251)				
Road sector energy consumption	0.302	0.310**	0.219*	0.272***	0.073	0.212**
	(1.556)	(2.532)	(1.841)	(3.705)	(0.197)	(2.427)
(log) Precipitation (mm)	0.031	0.024*	0.027	0.021**	0.025	0.023**
	(1.031)	(1.883)	(1.159)	(2.375)	(1.025)	(2.559)
Total fertility rate	−0.055	−0.000				
	(−1.228)	(−0.025)				
Road density	−0.001	0.000				
	(−0.997)	(0.951)				
Years of primary education			0.052	0.029**		
			(1.414)	(2.268)		
Years of secondary education			0.073	0.040**		
			(1.464)	(2.303)		
Years of tertiary education			0.110	−0.046		
			(0.432)	(−0.603)		
Income quintile 1 (poorest) * schooling					0.025	0.021**
					(1.062)	(2.269)
Income quintile 2 * schooling					0.039	0.028***
					(1.653)	(3.099)
Income quintile 3 * schooling					0.055**	0.035***
					(2.386)	(3.915)
Income quintile 4 * schooling					0.053**	0.032***
					(2.078)	(3.576)
Income quintile 5 (richest) * schooling					0.060**	0.032***
					(2.332)	(3.556)
Constant	6.191***	6.185***	5.908***	5.557***	6.165***	5.568***
	(10.021)	(15.759)	(13.702)	(36.370)	(14.351)	(37.583)
Observations	737	736	1,685	1,685	1,556	1,556
Number of countries	57	56	74	74	73	73
Time fixed effects	yes	yes	yes	yes	yes	yes
Country fixed effects	yes	yes	yes	yes	yes	yes
ε_{it} autocorrelation	none	AR(1)	none	AR(1)	none	AR(1)
Wooldridge test statistic[a]	8.97		18.12		15.28	
Wooldridge test p-value	0.00		0.00		0.00	
R^2	0.883	n.a.	0.913	n.a.	0.906	n.a.

Notes: The dependent variable is the logarithm of the net agricultural production per ha (in intl. $). Robust t-statistics are given in parentheses. Single asterisk (*) denotes significance at the 10% level, double asterisk (**) denotes significance at the 5% level, and triple asterisk (***) denotes significance at the 1% level.
[a]Wooldridge test statistic is distributed as F(1,48) in column (1), as F(1,73) in column (3), and as F(1,72) in column (5).

important when adopting new technologies. This would provide further support for our approach of using years of schooling rather than adult literacy, which captures much more basic education.

As a third extension, we use the GDP per capita (PPP) from the Penn World Tables 7.0 (Heston, Summers, and Aten 2011) to subdivide our sample into five income quintiles (with quintile 1 being the poorest and quintile 5 the richest).[28] We then generate dummy variables for each income quintile and multiply those with the average years of schooling indicator. This allows us to estimate the effect of an additional year of schooling separately for the five income groups while at the same time maintaining the assumption of a common meta-production function for all countries. The aim of this exercise is to empirically test the above-described hypothesis that the returns to education are generally higher in those societies that experience greater technical progress since the involved tasks in such settings become more complex and thus require a higher level of education (Schultz 1975; Rosenzweig 1995; Foster and Rosenzweig 1996), using GDP per capita levels as proxies for the level of agricultural technologies available to farmers.

Our results (columns 5 and 6) generally confirm the predictions of the above-described hypothesis. In the fixed effects specification, the coefficient of education is statistically significant and positive only for the richest three quintiles while remaining statistically insignificant for the poorest two quintiles. In our preferred model (FGLS) the results are slightly different, indicating that the effect of an additional year of schooling is in fact highly significant and positive for all income quintiles. However, with regard to the magnitude of the coefficients, both models reveal a general trend of smaller coefficients for the schooling variable for poorer income quintiles. We interpret these results as support for the claim, already discussed in section 2, that in very traditional agricultural settings where tasks are typically rather simple, one would expect the returns to education to be smaller (Schultz 1975; Schultz 1981; Rosenzweig 1995; Yang 1997). To address the problem that per capita GDP might only be an imperfect proxy of the technological level, in a robustness check we show that using other proxies of the level of agricultural technologies from the literature does not materially affect the results.[29]

Robustness Checks

As a first robustness check to our findings, in table 4 we test the question of whether it would have been possible to find the above-described positive impact of education on agricultural productivity when using – instead of the average years of schooling from Barro and Lee (2010) – the measures typically used in the literature to approximate the current stock of education in a country (namely, gross/net enrollment ratios and adult literacy rates).

While columns 1 and 2 reproduce our positive findings from table 2 (columns 4 and 6), the results change conspicuously when we simply substitute the total years of schooling with the gross enrollment ratio (GER) for primary education (columns 3 and 4) or the adult literacy rate[30] (columns 5 and 6). In particular, when using the GER as a proxy for the current stock of education, the FE-model indicates a highly significant, *negative* effect of

[28] The easiest way to undertake such a classification would be to simply take either the initial or average GDP per capita for each country for the whole time period and rank the countries accordingly. However, this procedure has the drawback that all observations of a country are assigned to exactly the same group, which makes the results of the analysis very sensitive to the allocation of countries to the income groups. Therefore, we decided to pursue a slightly different, though likely methodologically superior approach by first subdividing our sample into five-year intervals, and then for each of the intervals using the GDP per capita in the first year to assign the observations belonging to that five-year interval to one of the income groups. This procedure is repeated for all intervals and has the great advantage of assigning the countries to income quintiles more flexibly, thus allowing the countries to switch the income quintile over time. In our opinion this is the most appropriate way to account for the very differential growth performances worldwide (compare, e.g., Southeast Asia with Sub-Saharan Africa) that have increased the farmer's access to technological innovations in some countries more rapidly than in others. One could suspect that we may create an endogeneity problem when ranking the countries according to their GDP while using the net agricultural productivity per ha as our dependent variable, since these two measures may be highly correlated. However, the correlation between these indicators is in fact relatively low ($\rho = 0.22$). In addition, we argue that if there was a bias due to endogeneity, it would skew the estimates for the poorest quintiles downwards and not upwards, as countries with rising agricultural productivity are more likely to leave this quintile.

[29] In particular, as alternative proxies for the level of agricultural technology, we followed the suggestion of Self and Grabowski (2007) and tried the fertilizer intensity (i.e. the amount of fertilizer used per hectare of agricultural land), as well as the interaction term of fertilizer intensity and tractor intensity (analogously defined as the number of tractors per hectare of agricultural land) to subdivide our sample into quintiles. It turned out that the corresponding results are very much in line with those using the GDP per capita as the grouping criterion (table 3), likewise indicating higher returns to schooling for technologically more advanced countries.

[30] Given that the available adult literacy data include many gaps, we decided to linearly interpolate the existing data.

Table 4. Robustness Checks 1

	(1) FE	(2) FGLS	(3) FE	(4) FGLS	(5) FE	(6) FGLS
(log) Livestock per ha	0.293***	0.291***	0.341***	0.291***	0.316***	0.338***
	(5.255)	(13.577)	(5.252)	(13.652)	(4.498)	(14.053)
(log) Fertilizer per ha	0.060***	0.017***	0.062***	0.020***	0.062**	0.022***
	(3.186)	(4.130)	(3.422)	(4.754)	(2.551)	(4.344)
(log) Tractors per ha	0.076***	0.063***	0.059**	0.047***	0.053*	0.051***
	(2.814)	(8.285)	(2.147)	(5.677)	(1.822)	(6.873)
(log) Workers per ha	0.209***	0.236***	0.224***	0.261***	0.264**	0.254***
	(2.766)	(8.067)	(2.891)	(8.560)	(2.183)	(6.027)
Area equipped for irrigation (%)	0.004	0.005***	0.003	0.005***	0.004	0.003***
	(1.579)	(4.650)	(1.160)	(3.523)	(1.542)	(3.136)
Permanent meadows and pastures (%)	−0.010***	−0.008***	−0.011***	−0.008***	−0.010**	−0.007***
	(−2.670)	(−7.068)	(−2.926)	(−6.972)	(−2.135)	(−5.205)
Life expectancy at birth	0.012***	0.011***	0.013***	0.011***	0.010*	0.010***
	(3.173)	(7.239)	(2.973)	(6.829)	(1.925)	(6.197)
Road sector energy consumption	0.226*	0.264***	0.221	0.274***	0.181	0.180**
	(1.838)	(3.615)	(1.445)	(3.714)	(1.161)	(2.164)
(log) Precipitation (mm)	0.027	0.021**	0.012	0.022**	0.006	0.018**
	(1.161)	(2.394)	(0.585)	(2.552)	(0.255)	(1.963)
Total years of schooling	0.063**	0.032***				
	(2.512)	(3.542)				
Gross enrollment ratio			−0.002**	−0.000		
			(−2.495)	(−0.561)		
Adult literacy rate					0.003	0.001
					(0.735)	(1.261)
Constant	5.874***	5.561***	6.453***	4.661***	6.238***	5.742***
	(14.498)	(37.108)	(14.972)	(29.045)	(9.925)	(34.886)
Observations	1,685	1,685	1,575	1,574	1,321	1,313
Number of countries	74	74	85	84	84	76
Time fixed effects	yes	yes	yes	yes	yes	yes
Country fixed effects	yes	yes	yes	yes	yes	yes
ε_{it} autocorrelation	none	AR(1)	none	AR(1)	none	AR(1)
Wooldridge test statistic[a]	18.12		18.12		14.70	
Wooldridge test p-value	0.00		0.00		0.00	
R^2	0.912	n.a.	0.872	n.a.	0.894	n.a.

Notes: The dependent variable is the logarithm of the net agricultural production per ha (in intl. $). Robust t-statistics are given in parentheses. Single asterisk (*) denotes significance at the 10% level, double asterisk (**) denotes significance at the 5% level, and triple asterisk (***) denotes significance at the 1% level.
[a] Wooldridge test statistic is distributed as F(1,73) in column (1), as F(1,82) in column (3), and F(1,72) in column (5).

education. In contrast, we find the coefficient not to be significantly different from zero in the FGLS model.[31] Similarly, when taking the adult literacy rate, the coefficient for education remains insignificant at all conventional levels in both models. Given our arguments in section 3 about the methodological weaknesses of these indicators, it is striking that the use of these indicators can actually impede the detection of the existing positive effect of education on agricultural productivity.[32]

Secondly, we want to test whether it would have been possible to find the above-described significant positive impact of education with datasets that previous authors have used to

[31] We also used the net enrollment ratio (NER) for primary schooling instead of the average years of schooling. The results are very similar to using GER and are shown in the supplementary appendix online.

[32] As a further robustness check, we also tested the empirical relevance of the variable share of the adult population with no schooling (also available in the Barro-Lee dataset). When we include it as an additional covariate, it hardly ever is significant and does not change the results on our preferred education indicator. When it is included instead of our preferred indicator, it has a negative and sometimes significant effect which is not very robust. Therefore, our preferred total years of schooling indicator is not only conceptually preferred but also empirically more robust.

explain differences in agricultural productivity worldwide. Therefore, we used the dataset of Vollrath (2007) and exactly replicated the panel results of his analysis (see table 5, columns 1 to 3; these correspond to columns 4, 2, and 6 in table 6 of Vollrath's article). We then re-estimated the model, including the interpolated average years of schooling variable (everything else remained unchanged), which minimally reduced the sample due to the unavailability of education data. As can be seen, not only in our sample but also in the one Vollrath used, schooling has a highly significant positive effect on agricultural productivity, regardless of the estimation technique (RE, FE, or FGLS), and regardless of whether one further controls for the agricultural R&D expenditures per ha (see table 5, column 7; this corresponds to column 7 in table 6 of Vollrath's article). In fact, including such an education indicator raises the explanatory power of the model (see the increases in the R^2 between columns 1 and 4, and columns 2 and 5, respectively). These results are particularly interesting given that Vollrath states in footnote 5 of his article that he tried to include primary school enrollment rates as a proxy for the level of education, which did not add any meaningful information to the regressions and were thus left out. Against the background of the methodological superiority of the Barro-Lee measure over the enrollment rates (see section 3), as well as the results in table 4, the contrasting results are not surprising. Instead, we interpret the findings of this second robustness check as support for the claim that the results are dependent neither on our particular dataset nor on the empirical methodology applied.

As a third robustness check, we relax the assumption commonly made in the literature of a stable institutions index (table 6, columns 1 and 2). Given that the ICRG political risk index is only available from 1984 onwards, this modification greatly reduces our sample to only 1,120 observations. It turns out that the political risk variable itself does not have any significant impact on agricultural productivity. In addition, the effect of the average years of schooling remains positive and statistically significant at the 5% level in our preferred model (FGLS), and only slightly misses significance in the FE specification.

Fourth, the model is re-estimated using five-year averages instead of annual data to minimize the effects of persistent temporary shocks (see table 6, columns 3 and 4). Yet this modification only slightly alters the magnitude of the coefficients for the education variable, but does not affect its statistical significance.

Fifth, it was argued earlier in the article that the average years of schooling as provided by Barro and Lee (2010) are methodologically superior to the measures previously used in the literature to approximate for the current stock of education in a country. However, the indicator still has the disadvantage of not solely measuring the education of the rural population, which would be highly desirable given that the vast majority of agricultural labor comes from pastoral surroundings. Ulubaşoğlu and Cardak (2007) made an effort to address this issue and combined data from the UNESCO Educational Yearbooks and the World Bank Education Statistics in order to calculate the average years of schooling separately for urban and rural areas.[33] As a robustness check to our analysis, we take these data and use them to predict the average years of schooling for the rural population by first regressing the rural on the national years of education and its square (both from Ulubaşoğlu and Cardak (2007)). The resulting coefficients are then used to predict the average years of schooling for the rural population for all countries of our sample. As an alternative approach, as a first step we regress the ratio of rural to urban years of education on the nation's average years of education and then use the formula

$$s_{rural} = \frac{s_{national}}{\frac{\omega_{urban}}{r} + \omega_{rural}}$$

to predict the average years of education for the rural population. The resulting predicted data are highly correlated with the Barro-Lee indicator for the average years of education used in the main part of our analysis ($\rho \approx 0.99$ and $\rho \approx 0.92$, respectively), and it is therefore not entirely surprising that replacing the Barro-Lee measure with the predicted values for the rural population does not significantly change our results (the regression results obtained from these two alternatives, as well as the derivation of the above-described formula, are provided in the supplementary appendix online).

Sixth, we also examine whether the assumption of a common production function is driving our results. We do this by progressively

[33] However, due to data limitations this was only possible for a relatively small sample (76 observations from 56 countries).

Table 5. Robustness Checks 2 (Replication of Vollrath [2007])[a]

	(1) RE	(2) FE	(3) FGLS	(4) RE	(5) FE	(6) FGLS	(7) FGLS
Gini coefficient	−0.49***	−0.50***	−0.48***	−0.51***	−0.49***	−0.07	−0.47***
	(−4.58)	(−4.22)	(−4.82)	(−4.72)	(−4.01)	(−0.80)	(−6.44)
Log avg. farm size	0.02	0.02	−0.06***	0.02	0.03	−0.03	−0.00
	(1.30)	(1.23)	(−2.88)	(1.28)	(1.50)	(−1.46)	(−0.27)
Inputs							
Log livestock per ha	0.41***	0.39***	0.32***	0.42***	0.39***	0.32***	0.42***
	(11.13)	(9.76)	(12.18)	(11.67)	(9.35)	(12.59)	(20.80)
Log fertilizer per ha	0.04***	0.04***	0.01**	0.04***	0.04***	0.02***	0.01
	(4.03)	(4.13)	(2.27)	(4.09)	(4.17)	(3.53)	(1.47)
Log tractor per ha	0.03***	0.03***	0.07***	0.03***	0.03***	0.08***	0.09***
	(3.15)	(2.85)	(6.93)	(3.53)	(2.67)	(7.86)	(8.85)
Log labor per ha	0.09***	0.07**	0.26***	0.11***	0.07**	0.39***	0.30***
	(3.43)	(2.53)	(7.67)	(4.23)	(2.50)	(12.26)	(9.51)
Land quality							
% Irrigated	1.27***	1.37***	0.34**	1.23***	1.49***	0.17	0.51***
	(8.59)	(8.84)	(2.19)	(7.68)	(9.15)	(1.38)	(5.07)
% Permanent pasture	−0.17	0.24	−0.56***	−0.35**	0.19	−0.71***	−0.49***
	(−0.98)	(1.07)	(−6.37)	(−2.24)	(0.82)	(−7.77)	(−7.39)
Total years of schooling				0.03***	0.04***	0.13***	0.08***
				(3.18)	(3.00)	(12.30)	(7.36)
Research effort							
Log. agric. R&D expended per ha							0.04***
							(4.77)
Constant	2.52***	3.29***	2.94***	3.27***	3.51***	4.66***	3.92***
	(5.86)	(7.48)	(9.51)	(7.85)	(7.84)	(15.30)	(15.04)
Observations	1,159	1,159	1,159	1,128	1,128	1,128	993
Number of countries	54	54	54	52	52	52	42
Country controls (Z)[b] included	Yes	No	Yes	Yes	No	Yes	Yes
ε_{it} autocorrelation	none	none	AR(1)	none	none	AR(1)	AR(1)
R^2	0.864	0.749	n.a.	0.886	0.788	n.a.	n.a.

Notes: The dependent variable is the logarithm of the net agricultural production per ha (in intl. $). Robust t-statistics are given in parentheses. Single asterisk (*) denotes significance at the 10% level, double asterisk (**) denotes significance at the 5% level, and triple asterisk (***) denotes significance at the 1% level. This table was created using STATA Version 9 to achieve an exact replication of the results of Vollrath (2007).

[a] All specifications include the total fertility rate, life expectancy, and year dummies.

[b] Z includes the Kaufmann, Kraay, and Zoido (2002) index of institutions, dummies for legal origin from La Porta et al. (1999), and the land quality index from Peterson (1987).

trimming the sample by excluding the most and least capital-intensive observations (as approximated by the tractor intensity and the fertilizer intensity—each defined as stated in footnote 29). It turns out that the results of our analysis are quite robust in the sense that the coefficient of the average years of schooling variable remains highly significant and positive even in substantially trimmed datasets.

Last, Schultz (1999) and Wouterse (2011) claimed that a farmer's increased human capital will not instantaneously translate into higher agricultural productivity, and it is therefore necessary to consider lagged values of those variables. We took these objections seriously and included two-year lags for the life expectancy and the average years of schooling instead of current values in our model (results not shown). However, it turned out that this does not materially affect the results of our analysis.[34]

[34] As an additional test, some previous authors included the natural logarithm of the agricultural R&D expenditures per hectare in their models to account for the country's research effort. Despite a very poor availability of data, we did the same as a robustness check (results not shown) using data from the ISNAR Agricultural Research Indicator Series (Pardey and Roseboom 1989). While the magnitude of the coefficient for the average years of education remained relatively unchanged, the variable lacked statistical significance. However, we argue that this is due to the dramatically reduced sample of only 272 observations from 49 countries (compared to 1,685 observations from 74 countries prior), rather than being a consequence of the inclusion of the R&D expenditures

Table 6. Robustness Checks 3

	(1) FE	(2) FGLS	(3) FE	(4) FGLS
	annual data		5-year averages	
(log) Livestock per ha	0.232***	0.243***	0.303***	0.295***
	(4.539)	(9.889)	(4.715)	(13.418)
(log) Fertilizer per ha	0.046**	0.021***	0.096***	0.084***
	(2.098)	(3.903)	(3.930)	(11.571)
(log) Tractors per ha	0.090***	0.064***	0.054**	0.047***
	(2.940)	(6.535)	(2.236)	(5.514)
(log) Workers per ha	0.262***	0.284***	0.197***	0.207***
	(3.229)	(7.710)	(2.711)	(8.307)
Area equipped for irrigation (%)	0.004*	0.002*	0.002	0.001
	(1.794)	(1.888)	(0.960)	(0.924)
Permanent meadows and pastures (%)	−0.009**	−0.005***	−0.005	−0.006***
	(−2.043)	(−3.665)	(−1.342)	(−4.246)
Life expectancy at birth	0.012***	0.011***	0.011**	0.011***
	(3.122)	(6.681)	(2.565)	(10.452)
Road sector energy consumption	−0.060	0.064	0.212*	0.196**
	(−0.203)	(0.657)	(1.987)	(2.472)
(log) Precipitation (mm)	0.040	0.021**	−0.026	0.008
	(1.361)	(2.010)	(−0.351)	(0.219)
Total years of schooling	0.046	0.027**	0.056**	0.044***
	(1.525)	(2.341)	(2.097)	(5.135)
Political Risk Index	−0.000	0.000		
	(−0.020)	(0.643)		
Constant	5.800***	5.532***	6.089***	5.746***
	(11.921)	(29.443)	(9.216)	(20.398)
Observations	1,120	1,120	396	395
Number of countries	69	69	74	73
Time fixed effects	yes	yes	yes	yes
Country fixed effects	yes	yes	yes	yes
ε_{it} autocorrelation	none	AR(1)	none	AR(1)
Wooldridge test statistic[a]	14.79		21.94	
Wooldridge test p-value	0.00		0.00	
R^2	0.902	n.a.	0.913	n.a.

Notes: The dependent variable is the logarithm of the net agricultural production per ha (in intl. $). Robust t-statistics are given in parentheses. Single asterisk (*) denotes significance at the 10% level, double asterisk (**) denotes significance at the 5% level, and triple asterisk (***) denotes significance at the 1% level.
[a] Wooldridge test statistic is distributed as F(1,68) in column (1), and F(1,70) in column (3).

Conclusion

In this article, we re-examine the role of education for agricultural productivity in a cross-country framework. It was claimed that recent cross-country studies using sophisticated econometric methods failed to detect a statistically significant, positive impact of schooling as a consequence of inadequate proxies used to measure a country's stock of education. Using a large panel of 95 developing and emerging countries, together with the newest version of the educational attainment dataset of Barro and Lee (2010), we find that education in fact has a significant positive impact on agricultural productivity worldwide, which is robust to changes in both specification and estimation methods. The effect is sizeable, implying that an additional year of schooling for the whole population would increase agricultural productivity by approximately 3.2% in the preferred FGLS model. Furthermore, we find that only primary and secondary education has a statistically significant positive impact on agricultural productivity. Finally, the effect of schooling was estimated separately for countries of

variable, which always remained statistically insignificant with t-values below 0.30. To support our claims of sample size problems, we can show that the schooling variable in this particular reduced subsample was not statistically significant, even when applying our most basic regressions (without agricultural R&D expenditures), and the inclusion of the additional control variable did not materially change any of the coefficients. Hence, it is rather the smaller and apparently biased subsample that caused the education variable to be insignificant and not the effect of the additional control variable for R&D expenditures.

different income levels. Our results suggest that the effect of education is generally smaller for the poorest countries. These findings are in line with the arguments proposed by Schultz (1975), Rosenzweig (1995), and Foster and Rosenzweig (1996), which claim that in very traditional agricultural settings where tasks are typically rather simple, one would expect the returns to education to be smaller (compared to countries facing rapid technical change).

The policy implications of our article are relatively straightforward. The positive impact of schooling on agricultural productivity found in our analysis supports the view that education is indeed one of the key ingredients that enhance productivity in developing and emerging countries. Hence, even governments of nations relying to a great extent on the primary sector should maximize efforts to increase the population's level of education. However, particularly for the poorest countries, our findings underline the complementarity of capital investments in the agricultural and educational sectors, since technical progress is needed to fully exploit the productivity-enhancing potential of schooling. Or, in the words of Foster and Rosenzweig (1996, p. 951):

> [T]he returns to investment in technical change will in general be higher when primary schooling is accessible and the returns to investment in schooling will be higher when technical change is more rapid.

We conclude with some caveats and further suggestions. First of all, our results are based on cross-country regressions which ought to be viewed with the necessary caution since they all rely on relatively strong assumptions (e.g. the existence of a common meta-production function). However, we did our best to reduce the heterogeneity of the sample and are therefore relatively confident that this assumption is in our case justifiable. The fact that our macro findings are much more in line with the micro literature than previous macro findings further supports our contention. Second, we would have liked to account for differences in the quality of schooling in our analysis instead of solely focusing on its quantity. Eric A. Hanushek and Ludger Wößmann have worked extensively on this topic and have compiled a dataset of test scores for approximately 50 countries worldwide (Hanushek and Wößmann 2007). However, this dataset has—at least from our perspective—the drawback that a large part of these countries are industrialized nations, which we intentionally excluded from our sample (see section 4). In addition, according to Hanushek and Wößmann (2007) it is necessary to take an average of the test scores over at least the last 40 years to obtain a reliable proxy for the educational performance of the entire labor force, and not just a measure of the quality of current students. When one does so, one ends up with just one observation per country, thus having a time-invariant quality of schooling indicator, which in our preferred models would be simply intercepted by the country fixed effect. In short, the unavailability of a time series schooling quality indicator unfortunately prevented us from accounting for differences in the quality of education in our analysis. Third, as indicated earlier in this article, our results may actually underestimate the "full" impact of education on agricultural productivity, since in our macro framework we are not able to adequately account for the endogeneity of activity choice by the farmers (e.g. Taylor and Yunez-Naude 2000; Yang and An 2002; Jolliffe 2004; Yang 2004). In particular, it may well be that individuals decide—as a consequence of education—to seek work (either partially or fully) in the non-agricultural instead of the agricultural sector, where returns to their level of knowledge are potentially higher. Given that our analysis is limited to agricultural productivity, we are obviously not able to (fully) capture the returns from schooling for those individuals.

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