THE EFFECT OF SCHOOLING ON WORKER PRODUCTIVITY: EVIDENCE FROM A SOUTH AFRICAN INDUSTRY PANEL

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Abstract

This paper estimates the effect of schooling on worker productivity using a South African industry panel dataset that combines education and employment data from a series of thirty-six consecutive household surveys with output and physical capital data from the South African Reserve Bank Quarterly Bulletins. The novelty of this dataset is that it spans a longer period than typical firm-level panels, while containing cross-sectional variation in worker education that is absent from most sector or industry-level panels. Using data from different sources presents measurement error concerns that are explicitly addressed. Unlike other studies that have used African firm or sector level data, we find a large schooling effect. This suggests that using data with sufficient time series variation in the schooling variable is crucial in accurately estimating its effect. Furthermore, the coefficient estimates suggest that the schooling-productivity profile is concave. These results are highly robust across different estimators that allow for correlated industry effects, heterogeneous production technologies, cross-sectional dependence and measurement error.

Keywords: Human capital, labour demand, panel data econometrics, South Africa

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1. Introduction

A number of studies have now found a strong schooling effect on individual earnings in African countries, and this is often interpreted as evidence of the impact of education on worker productivity. This would be good news for the many African countries that have invested heavily in education (Collier and Gunning 1999) in the hope of putting themselves on the road to economic development. However, although this expenditure reprioritisation has had the desired effect on educational attainment (UNESCO 2011), it has generally failed to translate into improved labour market outcomes amongst younger, better educated cohorts. This outcome is consistent with the results obtained by empirical African production function studies which find that worker education levels do not contribute significantly to productivity.

This earnings and production function studies therefore paint two very different pictures of the causal effect of schooling on worker productivity, and it is not clear which method produces more reliable results. On the one hand, the production function approach provides a more direct method of estimating the effect of schooling on labour productivity and can produce consistent estimates of the schooling impact even where workers are not paid their marginal revenue product. On the other hand, African production function studies have either used firm-level datasets with a very short time dimension or sector-level data without cross-sectional variation in education, which means that the measures of schooling may not have sufficient variation to provide accurate estimates of the parameter of interest.

This paper estimates industry-level production functions for South Africa over a sixteen year period in order to determine the effect of human capital investment on worker productivity. The estimation of these industry production functions are made possible by our unique dataset, which merges physical capital and output data obtained from the South African Reserve Bank (SARB) Quarterly Bulletins with industry employment and education estimates from a series of thirty-six consecutive Statistics South Africa (StatsSA) household surveys. Importantly, our panel dataset has a much longer time series component than is typical for African firm level datasets. Furthermore, our schooling measure varies across industries, unlike the sector level panels used by previous South African studies. The schooling coefficients are compared for a wide variety of estimators using different identifying assumptions that allow for correlated industry effects, measurement error, industry heterogeneity and cross-sectional dependence.

Section 2 below reviews the literature regarding the estimation of production functions in African countries, and South Africa in particular. This is followed by a brief explanation of the data used in this study in section 3. Section 4 defines our estimable model and section 5 discusses the results from a wide range of estimators. Section 6 concludes.

2. The effect of education on labour productivity

There are a number of earnings function studies that find substantial, positive and convex schooling returns in African countries (Carnoy 1995, Appleton, Hoddinott et al. 1996, Siphambe 2000, Whaba 2000, Nielsen and Westergard-Nielsen 2001, Teal 2001), including South Africa (Keswell and Poswell 2004). This is often interpreted as evidence of the strong effect of education, and tertiary education in particular, on worker productivity. However, attempts to replicate this result using production data have generally been unsuccessful (Appleton and Balihuta 1996, Bigsten, Isaksson et al. 2000, Söderbom and Teal 2004, Fedderke 2005, Kleynhans and Labuschagne 2012), which casts some doubt over the validity of this interpretation.

Bigsten, Isaksson et al. (2000) estimate firm-level production functions for the manufacturing sectors of five sub-Saharan African countries: Cameroon, Ghana, Kenya, Zambia and Zimbabwe. Despite finding high returns to education in their earnings regressions, average worker education is insignificant in the production function regressions for all five countries. Söderbom and Teal (2004) find that schooling is significant in a Cobb-Douglas production function of Ghana's manufacturing sector when estimated using pooled OLS, but that this effect disappears when estimated with a fixed effects estimator. This leads them to conclude that worker education levels "appear not to be quantitatively very important in determining productivity" (Söderbom and Teal 2004). Appleton and Balihuta (1996) review studies that estimated the effect of education on labour productivity in the agricultural industries of African countries, and find that the effect is usually either insignificant or small in magnitude. Their own estimates for Uganda show that although primary education has a significantly positive effect in raising agricultural production, the returns to secondary school are insignificant and the overall returns are much lower than those usually found in earnings regressions.

In the absence of South African firm-level panel data, production function estimates have either used time series data at the national level (Smit and Burrows 2002, Arora and Bhundia 2003, Bonga-bonga 2009) or cross-sectional data (Bhorat and Lundall 2004). Data limitations mean that such regressions generally did not control for human capital. An important exception is Fedderke (2005) who uses a panel of South African manufacturing sectors to estimate the effect of human capital variables on total factor productivity (TFP) growth. This study use SARB data at a three-digit sector classification, for which output, physical capital and employment data are available, but for which no information on the average schooling level of workers exists. He proceeds to estimate the effect of a range of national-level education variables (which do not vary across sectors) on sectoral productivity, and finds that measures of "human capital quantity" are either negatively or insignificantly related to TFP growth whereas measures of "human capital quality" have a positive and significant effect. This leads him to conclude that "it is the quality of human capital rather than the quantity of human capital that is important for TFP growth" (Fedderke 2005). Similarly, Kleynhans and Labuschagne (2012) study a cross-section of South African manufacturing firms from the World Bank enterprise survey and find that average worker schooling levels contribute insignificantly to production.

It is possible that the divergence between the estimated effects of schooling in earnings and production function regressions is evidence of the role of education as a signal of high inherent market ability (Spence 1973). In this case the highly educated will earn more than those with lower levels of education in equilibrium, but education itself has no causal effect on worker productivity. However, it is also possible that data limitations have precluded production function studies from accurately identifying the positive effect of schooling on productivity. Education usually changes slowly over time, which means that there may not be enough time series variation in the average schooling level of employees to accurately estimate its effect while allowing for correlated firm effects. This concern is raised by Bigsten, Isaksson et al. (2000), who report that in their firm-level panel "education is close to being a firm fixed effect". A similar issue arises when using education variables with no cross-sectional variation (as in Fedderke (2005)): given how slowly education changes over time, there may simply not be enough variation in such measures to provide a reliable estimate of its effect on productivity.

The empirical literature on the role of human capital in production alerts us to two other concerns that should be taken into consideration when choosing an identification strategy: measurement error and parameter heterogeneity. At the cross-country level, Krueger and Lindahl (2001) find that measurement error in the schooling variable causes substantial attenuation bias in the schooling coefficient. At a cross-sector or industry level, consistent measurement of schooling is likely to be less of a concern. However, other data quality problems may still induce a bias into the schooling coefficient, particularly where the schooling variable changes slowly over time or is highly correlated with other factors of production. This problem is exacerbated when using a differenced estimator (Griliches and Mairesse 1998). Krueger and Lindahl (2001) find that more accurate estimates of the effect of education on labour productivity are obtained by either fixing the capital and labour coefficients to reasonable values (such as their respective shares of total income) or using longer differences in a differenced estimator.

Misspecification bias may be a problem if industries with very different technologies are assumed to all produce according to the same production function. There is substantial empirical evidence against the assumption of parameter homogeneity at the sectoral level (Burnside 1996, Eberhardt and Teal 2011). The heterogeneous effect of education on productivity has also been put forward as an explanation for low estimated schooling effects at the cross-country level (Judson 1998, Pritchett 2001) and in agricultural production functions (Appleton and Balihuta 1996). These problems suggest using a more flexible estimator that allows for parameter heterogeneity, and perhaps also heterogeneity in how sectors respond to global productivity shocks (i.e. cross-sectional dependence).

3. Data

Our literature review suggests measurement error and parameter heterogeneity as two potential sources of endogeneity that could bias the estimated effect of worker education on labour productivity. Exploring these issues requires a South African industry, sector or firm-level panel dataset that contains a measure of worker education that varies across the units of observation and that has a time dimension that is sufficiently long to allow parameter heterogeneity across industries. Ideally, it should also contain additional variables that can be used as instruments for potentially mismeasured input variables. No such a dataset exist, so we construct an industry level panel using two separate data sources: the SARB Quarterly Bulletin and the StatsSA household surveys.

The StatsSA household surveys offer the richest source of medium-term South African labour market trends. We use data from a series of surveys that were conducted in the post-apartheid era using a comparable sampling frame and survey design. The October Household Surveys were administered on an annual basis between 1995 and 1999, before being replaced by the bi-annual Labour Force Surveys from 2000 to 2007. In 2008 StatsSA launched the Quarterly Labour Force Survey, for which we use the data until the third quarter of 2011. This provides us with thirty-six consecutive, but unevenly spaced surveys spanning the years from 1995 to 2011. These surveys include individual responses to questions regarding employment, years of schooling completed and industry of occupation that can be used to estimate the number of formal sector employees working in different industries as well as their average years of completed schooling.

The SARB data are collected from South African firms at quarterly intervals. This data includes variables for "gross value added by kind of economic activity" and the "fixed capital stock by kind of economic activity", which we will use as our measures of output and physical capital respectively. The kind of economic activity that firms engage in is classified into nine different industries[3] using the ISO one digit categories. Although these variables are also available at the two-digit and three-digit sector level (as used by Fedderke (2005)), constructing the employment and schooling variables at this lower level of aggregation would mean using fewer observations for each estimate and compounding any measurement error and sampling variation in the data. These nine industries are therefore used as the cross-sectional units of observation for our production function model. The industry capital stock and output values are recorded quarterly and measured at constant 2000 prices. These variables are combined with the employment and education data from the household surveys to construct a balanced South African industry panel dataset spanning thirty-six periods and nine industries. These datasets also provide information that can potentially serve as instrumental variables for employment (such as the share of industry workers that belong to trade unions or the average industry wage rate) and physical capital (the long-term government bond yield).

Given the important role assigned to measurement error in explaining certain results in the cross-country human capital-growth literature, it is worth briefly discussing the nature of the measurement issues that affect our data. Some studies have questioned the reliability of the SARB data – for example, Van Walbeek (2006) highlights the large revisions that are periodically made to the SARB data, and the substantial impact that these changes can have on empirical studies – but much more attention has focused on the problems in comparing the Stats SA household surveys. Many papers (Casale, Muller et al. 2004, Kingdon and Knight 2005, Burger and Yu 2006, Altman 2008) discuss the effect that modifications in questionnaire design and sampling methodology may have had on the comparability of the household surveys over time. The most serious comparability problems occur for informal sector or self-employed workers, so that the effect of these inconsistencies can be limited by omitting these workers from the sample and restricting our dataset to formal sector employees only. Since the SARB firm surveys are almost certain to ignore the bulk of industry output arising from (and the capital stock owned by) firms operating in the informal sector, omitting these workers is also likely to improve the internal consistency of our dataset. Although worker schooling levels are compiled from the same set of surveys, some of the sampling and measurement problems will be mitigated in variables that are constructed as averages rather than the totals. If these measurement errors mainly derive from comparability problems across the different surveys, then we would expect it to contain a strong time-specific component.

Altman (2008) investigates the StatsSA household data by comparing industry employment trends to those obtained from alternative sources, including employment data derived from a series of establishment surveys. This series, known successively as the Survey of Total Employment and Earnings, the Survey of Employment and Earnings and the Quarterly Employment Survey, also underwent numerous changes in surveying methodology and sampling frames, and is therefore unlikely to provide a more accurate measure of industry employment trends[4]. Assuming that these changes were independent of the questionnaire design and sampling adjustments that affected the household surveys, this second employment measure offers a useful benchmark to which the StatsSA employment variable can be compared, and possibly another instrumental variable that can be used to identify our model parameters in the presence of measurement error. Altman (2008) finds evidence of substantial errors in the industry employment totals

[3] The nine industries are 1) agriculture, forestry and fishing, 2) mining and quarrying, 3) manufacturing, 4) electricity, gas and water, 5) construction, 6) wholesale and retail trade, catering and accommodation, 7) transport, storage and communication, 8) finance, insurance, real estate and business services, and 9) community, social and personal services.

[4] This employment series is used most other South African production function studies, such as Fedderke, J. W. (2005). Technology, Human Capital and Growth, Economic Research Southern Africa..

derived from the household surveys – particularly in the agriculture, mining and community, social and personal services industries – but her analysis still indicates that these household surveys provide the "most comprehensive and reliable sources of employment trend data for the past decade" (Altman 2008).

4. Production function and model identification

In this paper we endeavour to estimate the effect of schooling on the productivity of workers. In doing so, the literature review suggest we should avoid exploiting identifying conditions that assume that factors of production are uncorrelated with unobserved industry-specific effects, that variables are measured without error, or that all industries produce with an identical production technology. Our econometric model is based on standard production theory: industries combine physical capital, K, labour, L, and Hicks-neutral technology, A, to produce output, Y. Labour is inherently heterogeneous, which we incorporate by allowing the labour input to be augmented by the average education of workers in the industry, $\bar{E}$. The production function is of the (human capital augmented) Cobb-Douglas form:

$$Y_{nt} = A_{nt} K_{nt}^{\alpha} \left(e^{\phi_1 \bar{E}_{nt} + \phi_2 \bar{E}_{nt}^2} L_{nt} \right)^{\gamma} \qquad [1]$$

where N different industries (generically denoted n) are observed over T periods (indexed by t). This is similar to the production function employed by Hall and Jones (1999) and Bils and Klenow (2000), except for three important generalisations. Firstly, we allow non-constant returns to scale in production. Secondly, in our specification the average years of schooling augments the labour input in a non-linear way, and its schooling coefficients are estimated rather than assumed. The production function in [1] can be manipulated to produce a "macro-Mincer equation":

$$\ln \frac{Y_{nt}}{L_{nt}} = \ln A_{nt} + \alpha \ln \frac{K_{nt}}{L_{nt}} + (\gamma + \alpha - 1) \ln L_{nt} + \gamma \phi_1 \bar{E}_{nt} + \gamma \phi_2 \bar{E}_{nt}^2$$

The fact that log output per worker depends is a quadratic function of average education levels is in line with the empirical earnings function literature that finds an important role for a quadratic schooling term in explaining the individual earnings distribution. Thirdly, in our most general specification we also allow the technological parameters to vary across industries in a way that accommodates a high degree of production heterogeneity.

Industry productivity is determined according to $A_{nt} = e^{\eta_n + \chi_n \tau_t + \varepsilon_{nt}}$ where η_n represents unobservable time-invariant industry productivity effects, τ_t is a universal time shock, χ_n represents the industry's output response to this shock, and ε_{nt} denotes all remaining productivity innovations. Defining the logged vector of observable production inputs as $\boldsymbol{x}_{nt}$ and the technological parameter vector as $\boldsymbol{\beta}_n$, the most general specification of our model can be expressed as

$$y_{nt} = \boldsymbol{x}_{nt} \overline{\boldsymbol{\beta}} + \eta_n + \chi_n \tau_t + \boldsymbol{x}_{nt} \left(\boldsymbol{\beta}_n - \overline{\boldsymbol{\beta}} \right) - \boldsymbol{e}_{nt} \boldsymbol{\beta}_n + \varepsilon_{nt} \qquad [2]$$

where y_{nt} is log industry output, $\overline{\boldsymbol{\beta}} = E(\boldsymbol{\beta}_n)$ is the production parameters for the "average" industry, and $\boldsymbol{e}_{nt} = \boldsymbol{x}_{nt} - \boldsymbol{x}_{nt}^*$ is the measurement error that arises due to the difference between observed and actual but unobservable factor input values, $\boldsymbol{x}_{nt}^*$. Although the industry productivity coefficients, $\boldsymbol{\beta}_n$, are all of interest in their own right, we are primarily interested in the population averages of these coefficients, $\overline{\boldsymbol{\beta}}$.

This formulation is general enough to simultaneously allow for correlated unobservable industry- or time-specific effects, measurement error, parameter heterogeneity and cross-sectional dependence. These issues will now be discussed in turn, with specific reference to the estimators that are meant to address them.

A natural point of departure for our econometric analysis is the pooled ordinary least squares (POLS) estimator. In the absence of measurement error and parameter heterogeneity, this estimator will be consistent if the production inputs are uncorrelated with the unobservable industry fixed effects and time shocks. Even if these conditions are met, a random-effects (RE) estimator can be used to obtain estimates that are both consistent and asymptotically efficient. However, in a simple model where firms maximise current period profits and all firms face the same factor costs, those in high-productivity industries will employ more workers and invest more in physical capital than those in low-productivity industries. By the same logic high productivity periods should also coincide with employment and investment booms. In such cases, the production function coefficient estimates from POLS and RE estimators will yield biased estimates of the causal productivity effects of the factors of production. In contrast, fixed-effects (FE) and first-difference (FD) estimates will be consistent regardless of whether unobservable industry-specific effects are correlated to the factors of production or not. This comes at the cost of less precise parameter estimates, particularly for explanatory variables with limited time-series variation like education. Adding time dummies to the POLS, FE or FD regression will provide estimates that will be consistent despite correlation between global productivity shocks and industry production factors as long as this does not cause cross-sectional dependence.

If some of our explanatory variables are measured with error, this affects the properties of the estimators. By transforming away all cross-sectional variation in the variables, the FE and FD estimators are known to be highly sensitive to measurement error (Griliches and Mairesse 1998), particularly where the time series variation in the regressors has a low signal-to-noise ratio. The discussion in section 3 suggested that our employment measure may be especially vulnerable to measurement error. Theil's (1961) multivariate measurement error formula suggests that this will also induce a downward bias in the estimated schooling coefficients[5]. If industry employment has a high degree of autocorrelation relative to its measurement error, then we would expect this bias to be more severe in the FD than the FE estimates (Wooldridge 2002).

There are at least three methods to estimate the education effect more accurately in the presence of measurement error. Firstly, the values of certain production parameters can be fixed to reasonable values, such as their shares of national income in a Cobb-Douglas specification. Secondly, if variables are measured with serially uncorrelated errors, then estimators based on longer differences will be less severely biased than those based on "short" differenced results (Griliches and Hausman 1986). Finally, we can attempt to use instrumental variables for the true values of the incorrectly measured production factors. Krueger and Lindahl (2001) found that the first and, to a lesser extent, the second approaches work well in obtaining more accurate estimates of the schooling coefficient in a cross-country context.

The estimators discussed above all implicitly impose the restriction of slope homogeneity, whereas our review of the literature warned against using estimators that exploit this restriction as part of an identification strategy. The mean group (MG) estimator obtains estimates of each $\boldsymbol{\beta}_n$ vector by estimating an OLS production function on each industry's time series data, before averaging these coefficient vectors

[5] This is based on the assumption that the effects of labour and schooling are both positive, and on the observation that in our sample industry employment and worker education are negatively correlated after controlling for capital and industry fixed effects.

across industries to calculate an estimate $\overline{\boldsymbol{\beta}}$. Conceptually this approach is more consistent with the notion of heterogeneous production processes across industries (Pesaran and Smith 1995). This estimator only uses within-industry variation and will therefore be consistent even where the production factors are correlated with unobservable industry effects or production parameters, but will be similarly sensitive to measurement error as the FD and FE estimators.

Industry heterogeneity may arise not only in terms of how inputs affect production, but also in how common latent time shocks affect production. If global productivity shocks either affect industry productivity or the accumulation of factor inputs homogeneously, then estimators that control for time effects (such as the 2FE or POLS with time dummies, but not the MG estimator) will produce consistent estimates of the technological parameters. More specifically, such estimators require one of three conditions to hold: the effect of the time shocks on productivity should be constant across industries, its effect on industry inputs must be constant, or its effect on productivity should be uncorrelated to its effect on each of the inputs. However, where the error term and regressors have correlated factor loadings the resulting cross-sectional dependence in the error terms can lead to coefficient bias in all the hitherto discussed estimators. For example, if the same industries are more responsive to global time shocks both in terms of output and employment or investment decisions, then none of the estimators considered so far will produce consistent estimates of the model parameters.

In such cases Pesaran (2006) suggests using the common correlated effects mean group (CMG) estimator, which entails estimating the output equation separately for each industry using OLS but including output and input cross-sectional averages as regressors. The inclusion of these additional controls will tend to absorb the effect of the time shocks, as well as any survey-specific measurement error. This is an important advantage over the MG estimator, which does not allow controlling for any time-specific effects. Although this estimator will generally not be consistent under correlated factor loadings, the simulation results in Coakley, Fuertes et al. (2006) suggests that the CMG model performs better in smaller samples, and is more robust to the type of cross-sectional dependence that violates the identifying assumptions of the 2FE model. On the other hand, the CMG estimator only exploits the within-industry variation in the data, and will therefore suffer the same decreased estimator precision associated with the FE, 2FE and FD estimators.

Two additional estimators that will provide consistent estimates of $\overline{\boldsymbol{\beta}}$ in the case of slope heterogeneity and cross-sectional dependence are the augmented mean group estimator (AMG) and the cross-section (CS) or between-groups estimator. The AMG estimator was developed in Eberhardt and Teal (2010), and simulation results (Eberhardt and Bond 2009) suggest that it performs as well the CMG estimator in the presence of non-stationary variables or cross-sectional dependence. The CS estimator requires regressing the cross-sectional average of output on the cross-sectional average of the inputs. Although this estimator will be biased by correlated industry fixed effects, correlated random coefficients and industry-specific measurement error in the same way as the POLS or TE estimators, it is the only estimator considered so far that will not be biased by correlated factor loadings or two-way demeaned measurement error. Since this estimator ignores all within-industry variation in the data, we would expect it to be fairly imprecise in a dataset with as few cross-sectional observations as ours. The fact that it does not discard the between-industry variation means that it provides an interesting benchmark for our analysis, especially if we have reason to suspect that much of the informative variation in our data occurs along the cross-section dimension.

5. Empirical results

Table 1 reports the coefficient estimates obtained from a variety of panel data estimators. The pooled OLS estimates in column 1 indicate that the marginal return to employing better educated workers is very high at low levels of schooling, but decreases as the workforce becomes better educated. This result is surprising given the convex schooling-earnings profiles reported in most South African earnings regressions. The capital and labour coefficients are a little below 0.4 and 0.5 respectively, which is close to their shares of total income and is similar to what has been found for other countries and for South Africa using different approaches or data in the past. The coefficients suggest decreasing returns to scale for the typical industry.

Table 1: Estimates of production function coefficients, using various panel data estimators

	POLS	POLS	RE	FE	2FE	FD
Dependent variable	Log output	Log output	Log output	Log output	Log output	Log output
Log capital stock	0.373***	0.383***	0.343***	0.347***	0.327***	0.115
	(0.031)	*(0.033)*	*(0.056)*	*(0.058)*	*(0.052)*	*(0.087)*
Log employment	0.494***	0.493***	0.495***	0.494***	0.314***	0.068***
	(0.027)	*(0.029)*	*(0.033)*	*(0.033)*	*(0.032)*	*(0.024)*
Average education	1.607***	1.627***	0.289***	0.287***	0.174***	0.057
	(0.155)	*(0.163)*	*(0.051)*	*(0.051)*	*(0.042)*	*(0.063)*
Average education^2	-0.084***	-0.086***	-0.010***	-0.010***	-0.011***	-0.004
	(0.009)	*(0.009)*	*(0.003)*	*(0.003)*	*(0.002)*	*(0.003)*
Constant	1.481	1.013	7.789***	7.701***	11.687***	0.028***
	(1.243)	*(1.325)*	*(1.238)*	*(1.262)*	*(1.156)*	*(0.004)*
Control for industry effects	No	No	No	Yes	Yes	Yes
Control for time effects	No	Yes	No	No	Yes	No
Observations	324	324	324	324	324	288
R-squared	0.73	0.74		0.81	0.89	0.05

Notes: Standard errors in parenthesis. *Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

The second column in Table 1 reports the results obtained from adding period controls, and demonstrates that controlling for time shocks has little impact on the coefficient estimates and only marginally increases the regression R-squared. The random effects and fixed effects regressions in columns 3 and 4 show that controlling for uncorrelated or correlated industry effects produces a much flatter but still significantly concave schooling-productivity profile. This suggests that it is possible to identify the schooling effect from only within-industry variation in our longer panel dataset, and that the positive correlation between worker schooling and production is not driven only by between-industry correlation in worker education and productivity. Simultaneously controlling for period and industry effects further flattens but does not eliminate the concave schooling-productivity profile.

The coefficient estimates from a first-differenced estimator are shown in the final column of Table 1. These are very different from the other estimates: the capital and employment coefficients are implausibly low and the schooling effect is now insignificant. However, this estimator is known to be particularly sensitive to the effects of measurement error. The fact that our variables are compiled from different data sources may introduce precisely this problem, so it is worth exploring whether this can explain the divergence in results. Our discussion in section 3 suggested that our employment measure may be especially susceptible to measurement error, so treating this as the mismeasured variable seems like a natural point of departure. Table 2 reports the regression estimates from first differenced estimators that constrain the employment coefficient to be 0.5, 0.6 and 0.7 respectively. Higher employment coefficients are associated with schooling returns that are initially higher but also reveal stronger concavity. The basic result is robust within the range of plausible employment coefficients, and similar to what was observed from the other estimators in Table 1: the effect of schooling is substantial, statistically significant and concave. Fixing the capital coefficient produces schooling coefficients (not reported here) that are qualitatively similar, although less precisely estimated and hence not statistically significant. The final two columns in Table 2 use longer differences – 2 and 3 year differences respectively – to estimate the production function coefficients. Compared to the one year differenced estimates in Table 1, the labour and capital coefficients can both be seen to increase to more plausible values. The pattern of a significant and concave education effect on productivity is also restored. The results from Table 2 are therefore supportive of the notion that measurement error is a source of bias in the FD estimates.

Table 2: Panel data estimates of production function coefficients

	FD	**FD**	**FD**	**FD**	**FD**
Dependent variable	Log output	Log output	Log output	Log output	Log output
Differencing period	1 year	1 year	1 year	2 year	3 year
Log capital stock	0.032	0.012	-0.007	0.184***	0.204***
	-0.127	-0.144	-0.162	-0.066	-0.06
Log employment	0.5†	0.6†	0.7†	0.162***	0.241***
				-0.027	-0.029
Average education	0.231**	0.272***	0.312***	0.125*	0.120*
	-0.091	-0.103	-0.115	-0.067	-0.067
Average education^2	-0.012**	-0.014**	-0.016**	-0.008**	-0.008**
	-0.005	-0.005	-0.006	-0.004	-0.004
Constant	0.025***	0.025***	0.024***	0.054***	0.077***
	-0.006	-0.007	-0.008	-0.006	-0.008
Observations	288	288	288	252	216

Notes: Standard errors in parenthesis. *Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.
† denotes parameter restrictions.

We also estimated a number of regressions in which employment is instrumented using a variety of instrumental variables for employment (as discussed in section 3). When not controlling for industry effects, the results are almost identical the POLS results in Table 1, regardless of whether time dummies are included or not. This result is robust to the choice of instruments, or to replacing our employment measure with the alternative employment measure. When only exploiting within-industry variation in the data (by either differencing or including industry effects), the estimates reveal the familiar symptoms of weak instruments. Although the results are generally consistent with a schooling effect that is substantial and concave, the coefficients are imprecisely estimated and sensitive to the choice of instruments.

The preceding results were produced by estimators developed under the assumption of parameter homogeneity. However, these estimators may be susceptible to misspecification bias if the different industries produce using very different technologies. In order to investigate the effect of parameter heterogeneity and cross-sectional dependence on the estimated schooling effect, Table 3 reports the coefficient estimates from four estimators that explicitly acknowledge the heterogeneity in industry production. The MG coefficient estimates are reported in column 1 of Table 3. Compared to the FE and 2FE estimators, allowing for parameter heterogeneity produces schooling coefficients that are less precisely estimated, but that confirm the essential result of a schooling effect on worker productivity that is substantial, concave and statistically significant. The same result is obtained when using the CMG, AUG or CS estimators, although the point estimates are now even less precisely measured.

Table 3: Various heterogeneous parameter panel data estimates of production function coefficients

	MG	**CMG**	**AUG**	**CS**
Dependent variable	Log output	Log output	Log output	Log output
Log capital stock	0.743***	0.219	0.331	0.43
	(0.243)	*(0.158)*	*(0.328)*	*(0.264)*
Log employment	0.232***	0.067	0.088*	0.516*
	(0.085)	*(0.057)*	*(0.046)*	*(0.226)*
Average education	0.869*	0.226	0.45	2.471
	(0.488)	*(0.405)*	*(0.416)*	*(1.577)*
Average education^2	-0.039*	-0.011	-0.024	-0.133
	(0.020)	*(0.020)*	*(0.020)*	*(0.088)*
Constant	-14.271	2.354	9.122	-3.962
	(13.797)	*(6.863)*	*(8.055)*	*(11.770)*
Observations	324	324	324	9

Notes: *Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

6. Conclusion

This paper investigated the way in which nine broadly defined South African industries used human capital in the production process during the sixteen years following political transition. It does so by constructing a dataset from SARB Quarterly Bulletins and StatsSA household surveys. We compare the education coefficients obtained from a wide range of estimators and find that schooling has a substantial direct effect on production, and that this effect decreases with the education level of workers. This result is robust to estimators that explicitly allow for correlated industry effects or period-specific productivity fluctuations. The results from the first-differenced estimator also supports our conclusion, but only after explicitly making allowance for measurement error by either fixing the employment parameter to reasonable values or using longer differences. Further confirmation is provided by the mean-groups estimator, which allows for heterogeneous production technologies across industries. Estimators that also allow for cross-sectional dependence yield similar point estimates for the education coefficients, although these are imprecisely estimated.

The novelty of our dataset is that it spans a longer period than typical African firm-level panels, while containing cross-sectional variation in worker education that is absent from most African sector or industry-level panels. Given that other studies on African countries have generally been unable to find a significant effect for education in production functions, we conclude that the additional variation in education in our dataset is crucial in correctly identifying the effect of education on worker productivity. Our results suggest that conclusions that the quantity of education is relatively unimportant in the production process may have been premature.

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