Estimating the schooling-productivity profile using the production function approach

Abstract

This paper uses the production function approach to estimate the effect of schooling on the productivity of South African workers. Production functions are estimated for South African industries using a novel industry panel dataset that combines education and employment data from a series of household surveys with output and physical capital data from the South African Reserve Bank Quarterly Bulletins. The pooled ordinary least squares estimates indicate that the returns to education are substantial and concave, but these effects disappear when industry fixed effects are included in the regression. This result is similar to what has been found in other African production function studies. However, we demonstrate that the strong schooling effects are restored when making proper allowance for measurement error or parameter heterogeneity and cross-sectional dependence. The results suggest that industry productivity and the average schooling level of workers are highly correlated and difficult to identify simultaneously using only time series variation in the data.

1. Introduction

Despite the large number of studies that find a strong schooling effect on individual earnings levels, attempts to identify a similar effect from production function estimates on African firm or sector level data have generally been unsuccessful. The production function approach provides a more direct method of estimating the effect of schooling on labour productivity than the earnings function approach, and can produce consistent estimates of the schooling impact even where human capital externalities or institutional features of the labour market mean that workers are not paid their marginal revenue product.

This paper estimates industry-level production functions for nine South African industries over a twelve year period in order to determine the total monetary return to investing in human capital, rather than just the return that accrues to the individual. The estimation of these industry production functions are made possible by our unique dataset, which merges physical capital and output data from the SARB Quarterly Bulletins with industry employment and education estimates from a series of twenty-one consecutive StatsSA household surveys. Given the strong empirical evidence against the assumption that all sectors produce according to the same production function (Burnside, 1996; Eberhardt & Teal, 2008) or that production factors are uncorrelated to industry productivity (Söderbom & Teal, 2004), recent studies have preferred to use estimators that allow these features to be incorporated into their econometric models. However, by transforming away much of the variation in the regressors, such estimators may be more susceptible to measurement error and other types of biases not explicitly taken into consideration by the identifying assumptions. In certain cases, this may even lead to estimates that are less reliable than those of the pooled ordinary least squares estimator.

Section 2 below reviews the literature regarding the estimation of production functions. This is followed by a brief explanation of the data used in this study in section 0. Section 0 starts by describing our theoretical model, proceeds to outline the different types of endogeneity that could confound our analysis, and then analyses the characteristics of the various estimators that have been proposed to circumvent these problems. The results for these estimators are discussed in section 0, after which section 6 concludes.

2. The effect of education on labour productivity

There exists a substantial literature of Mincerian earnings regression studies for African countries that find substantial, positive and convex schooling returns. This is usually interpreted as evidence of the strong effect of education on labour productivity. Surprisingly, and somewhat disappointingly to proponents of greater education expenditure in African countries, production function studies have generally not been able to replicate this result.

Bigsten 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, the log of average worker education is insignificant in the production function regressions for all five countries[1]. Their results also include a capital coefficient estimate which is almost twice that of the employment coefficient, despite the fact that labour's share in income exceeds that of capital in most countries[2]. 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. They interpret this as evidence that the schooling level of workers is not very important in determining the productivity of African manufacturing firms. 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.

There are at least three ways to explain the very different results sometimes produced by the earnings function and production function approaches. Firstly, it is possible that the former only captures the

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[1] The sum of log education and log tenure is significant for four of the five countries, with coefficients varying between 0.1 and 0.29.

[2] If markets are perfectly competitive and production occurs with a constant returns to scale Cobb-Douglas production function, then the share of income accruing to labour and capital will be the labour and capital coefficients, respectively.

private monetary returns to schooling investment, whereas the latter also captures education externalities and spill-overs in the production process. In this case the difference between the two estimates can be interpreted as a reflection of human capital externalities. However, this requires assuming that individuals are paid their marginal revenue product, and that both methods provide unbiased estimates of schooling's effect on productivity. The second reason why earnings and production function schooling coefficients may diverge is because the former is affected by institutional characteristics of the labour market – such as union bargaining power, minimum wages or public sector wage premia – that drive a wedge between wages and marginal revenue product. Finally, it is also possible that data or specification problems produce biased estimates of the schooling coefficient in either or both approaches. The econometric issues that can confound the estimation of the schooling effect in earnings equations are very different from the identification issues that arise in production functions.

Although a number of studies have attempted to estimate Cobb-Douglas production functions for South Africa[3], data restrictions mean that such studies usually cannot control for human capital and use either time series data at the national level or cross-sectional data at the firm level. One notable exception is Behar (2010) who includes workers employed in the different skills categories as separate inputs in the production function. Although this is potentially informative about skills prices in the South African labour market, the skills categories are defined according to worker occupations rather than schooling levels. His research question also requires the estimation of a translog production function[4] rather than a Cobb-Douglas specification, so that his results are not directly comparable to ours.

The existing South African studies that most closely resemble our own is Fedderke (2002a; 2002b) which use a panel of South African manufacturing sectors to estimate the effect of human capital variables on total factor productivity growth. These studies use SARB data at a two-digit sector classification, for which output, physical capital and labour data are available, but for which no information on the average

[3] Examples include Arora and Bhundia (2003), Smit and Burrows (2002), Bhorat and Lundall (2004) and Bonga-bonga (2009).

[4] Although the added flexibility of these more sophisticated production functions are desirable, it introduces the issue of substitutability between the different factors of production which reduces the tractability of the model, the comparability with the results from earnings regressions and takes us beyond the scope of this paper.

schooling level of workers exists. Fedderke (2002a, p. 3) acknowledges the problems posed by labour quality differentials, but claims that "data limitations preclude the possibility of pursuing this line of inquiry further". Two approaches are used in an attempt to circumvent this problem: Fedderke (2002b) uses occupational information to classify workers into different skills categories, whereas Fedderke (2002a) employs a range of national-level education variables with only time series variation. Although his econometric model allows parameter heterogeneity in how the education variable affects sectoral productivity, this measure is derived as the residual from a regression in which the effects of capital and labour on output are assumed to be constant across all sectors. In this sense his specification is more restrictive than those considered in our own econometric analysis. Fedderke (2002a) finds that white school enrolment rates and the number of natural and engineering science degrees are positively correlated to productivity, whereas the logarithm of black enrolment rates is negatively correlated to productivity. This leads him to conclude that "[w]hat matters crucially in the long run is the quality of education" (Fedderke, 2002a, p. 29). However, it is also possible that the restrictiveness of his specification or data limitations may be precluding his regressions from accurately identifying the effect of his schooling quantity measures.

The difficulties in identifying a substantial schooling effect on output have also troubled researchers interested in determining the causes of cross-country productivity differentials. Exploring how this puzzle has been addressed in the cross-country literature may therefore offer insights into our own research question. There are at least three reasons why cross-country data would fail to find a large effect of education on labour productivity, whereas the bulk of earnings regressions show high returns to education. Firstly, if education merely serves as a signal of high inherent market ability without actually increasing one's productivity (Spence, 1973), then the highly educated will always earn more than those with lower levels of education, but an increase in a country's average level of education will not enhance the productivity of its workers. In this case the observed low cross-country return estimates will provide a more reliable reflection of the true effect of education on productivity.

Secondly, although cross-country data average away most individual level misreporting of schooling years within countries, this data also introduces other measurement issues such as aggregation bias or international differences in schooling quality. Krueger and Lindahl (2001) review a number of studies that investigate the cross-country relationship between education and economic growth, and find that such studies almost invariably suffer from schooling being measured with a great deal of error across countries. This produces the usual downward attenuation bias in the schooling coefficient and also – given the high positive correlation between human and physical capital across countries – an upwardly biased capital coefficient. Their hypothesis is supported by the findings of De la Fuente and Doménech (2006), who suggests that correcting for the quality of the schooling data substantially increases the estimated schooling coefficient in cross-country growth regressions. Krueger and Lindahl (2001) propose fixing the capital and labour coefficients to reasonable values, such as their respective shares of total income, in order to estimate more accurately the effect of education on labour productivity.

Thirdly, the effect of education may vary across groups of countries so that a regression model that assumes a constant education effect may suffer from misspecification bias (Temple, 1999, p. 132). Judson (1998) finds evidence that the effect of education on productivity depends on how efficiently educational institutions allocate their resources, which suggests that the education effect should be smaller in developing countries where inefficiencies in the school system are more likely to be an issue. This would also explain the failure of many African countries to convert increased levels of educational attainment into higher output growth. In a similar vein, Pritchett's (2001) observes that in many developing countries the highly educated are drawn to better paying but less productive public sector jobs, which would further reduce the productivity-enhancing character of education. These problems all suggest using a more flexible estimator that allows for parameter heterogeneity when attempting to accurately estimate the average schooling effect on labour productivity.

These problems are suggestive of the type of econometric problems that may confound the estimation of the schooling effect on worker productivity using African data. Properly accounting for measurement error and the differences in production technologies across firms or industries may be crucial in identifying this effect, and failing to do so may explain why production function studies of African data have hitherto failed to find any substantial schooling effect.

3. Data

Our literature review suggested 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 or firm-level panel dataset that contains a measure of worker education levels, has a time dimension that is sufficiently long to allow parameter heterogeneity across industries, and should ideally 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. 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. They also provide information that can serve as instrumental variables for employment, such as the share of industry workers that belong to trade unions or the average industry wage rate.

The SARB data are collected from South African firms, usually at either monthly or 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. This dataset also contains a measure of the long-term government bond yield which can be used as a measure of the cost of capital and hence as an instrument for the capital stock. The kind of economic activity that firms engage in is classified into nine different industries[5] using the ISO one digit categories. Although these variables are also available at the two-digit sector level (as used by Fedderke (2002a; 2002b)), 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

[5] 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.

problems 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 values[6] are recorded annually for all sectors (as well as quarterly for certain industries) and measured at constant 2000 prices. The industry output values[7] are recorded quarterly for all industries, measured at constant 2000 prices and unadjusted for seasonal fluctuations. These variables are combined with the employment and education data from the household surveys to construct a South African industry panel dataset spanning 13 years[8] and nine industries.

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 & Posel (2004), Kingdon & Knight (2005), Burger & Yu (2006) and 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.

[6] SARB time series codes 6140Y-6148Y.

[7] SARB time series codes 6631Y, 6632Y, 6634Y-6636Y, 6638Y-6640Y and 6642Y.

[8] The second bi-annual LFS (usually conducted in September or October) could potentially be used to add an additional observation per industry for each year between 2000 and 2007, although the panel time periods will then no longer be equally spaced. However, the industries for which the SARB only tracked the capital stock at an annual frequency would require the imputation of mid-year capital stock values. In order to avoid this additional complication we choose to use a panel dataset with an annual frequency.

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[9]. 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 also an 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 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, p. S127).

[9] This employment series is used most other South African production function studies, such as Fedderke (2002a; 2002b).

4. Production function and model identification

This paper is mainly concerned with identifying the average causal effect of additional schooling on the productivity of workers and particularly whether properly addressing measurement error and parameter heterogeneity can explain why so many African production function studies have failed to find a significant worker education effect. Our production function is of the human capital augmented Cobb-Douglas form, similar to that used in Hall and Jones (1999) and Bils and Klenow (2000) except that we allow non-constant returns to scale and also for schooling years to enter the production function non-linearly:

$$Y_{nt} = A_{nt} K_{nt}^{\alpha_n} \left(e^{\phi_{1,n} \bar{E}_{nt} + \phi_{2,n} \bar{E}_{nt}^2} L_{nt} \right)^{\gamma_n} \qquad [1]$$

The economy consists of N different industries (generically denoted n) each of which is observed over T different periods (indexed by t). Industries combine physical capital, K, the average education of workers, $\bar{E}$, labour, L, and Hicks-neutral technology, A, in order to produce output, Y. The technological parameters include the physical and human capital coefficients, α and γ, as well as the education coefficients ϕ_1 and ϕ_2. Allowing for parameter heterogeneity means that these coefficients will vary across industries. Industry productivity is generated according to $\log A_{nt} = a_{nt} = \bar{a} + \eta_n + \chi_n \tau_t + \varepsilon_{nt}$ where $\bar{a}$ is a constant, η_n is the time-invariant industry productivity effect, τ_t is a universal time shock, χ_n represents the industry's output response to this shock, and ε_{nt} is the remaining productivity innovation.

The input and technological parameter vectors are defined more succinctly as $\boldsymbol{x}_{nt} = [k_{nt}, l_{nt}, \bar{E}_{nt}, \bar{E}_{nt}^2]$, where k_{nt} and l_{nt} are the logs of capital and labour and $\boldsymbol{\beta}'_n = [\alpha_n, \gamma_n, \gamma_n \phi_{1,n}, \gamma_n \phi_{2,n}]$. The production parameters are random coefficients drawn from a distribution with $E(\boldsymbol{\beta}_n) = \boldsymbol{\beta}_*$ and the stochastic industry-specific technological parameter deviation is defined as $\boldsymbol{b}_n = \boldsymbol{\beta}_n - \boldsymbol{\beta}_*$.Whenever we allow for measurement error in the production factors the observed input values can be expressed as $\boldsymbol{x}_{nt} = \boldsymbol{x}_{nt}^* + \boldsymbol{e}_{nt}$, where $\boldsymbol{x}_{nt}^*$ is the vector of actual but unobservable factor input values and $\boldsymbol{e}_{nt}$ is the measurement error. Log output can thus be expressed as $y_{nt} = \boldsymbol{x}_{nt} \boldsymbol{\beta}_* + u_{nt}$, where

$$u_{nt} = \bar{a} + \eta_n + \chi_n \tau_t + \boldsymbol{x}_{nt} \boldsymbol{b}_n - \boldsymbol{e}_{nt} \boldsymbol{\beta}_n + \varepsilon_{nt}. \quad [2]$$

Although the industry productivity coefficients, $\boldsymbol{\beta}_n$, are all of interest in their own right, we are mainly interested in the population averages of these coefficients, $\boldsymbol{\beta}_*$.

Various estimators can be used to recover the technological parameters $\boldsymbol{\beta}_*$, depending on which set of identifying assumptions are most appropriate for our model. In section 0 we explore the coefficient estimates produced by a wide range of estimators. Before discussing each of these in turn, we consider a general class of least squares models that can be expressed as:

$$\widehat{\boldsymbol{\beta}}_M = (\boldsymbol{X}'\boldsymbol{M}'\boldsymbol{M}\boldsymbol{X})^{-1}\boldsymbol{X}'\boldsymbol{M}'\boldsymbol{M}\boldsymbol{y} = \boldsymbol{\beta}_* + (\boldsymbol{X}'\boldsymbol{M}'\boldsymbol{M}\boldsymbol{X})^{-1}\boldsymbol{X}'\boldsymbol{M}'\boldsymbol{M}\boldsymbol{u}$$

where $\boldsymbol{X}$ is the matrix of observable production factors (stacked across industries and periods), $\boldsymbol{y}$ is the output vector and $\boldsymbol{u}$ is the vector of unobservable productivity components. The $\boldsymbol{M}$ matrix performs a linear transformation on the data $(\boldsymbol{y}, \boldsymbol{X})$, and is usually chosen to ensure that $\widehat{\boldsymbol{\beta}}_M$ will be an unbiased estimator of $\boldsymbol{\beta}_*$. The objective of this transformation is to discard potentially endogenous variation from the observed production factors in $\boldsymbol{X}$. This logic has been used to justify a wide range of estimators that use different transformation matrices to rid the estimates of various types of endogeneity. However, if the error term contains many different unobserved factors, as in equation [18] above, then it is possible that a transformation can successfully remove the correlation between the regressors and one of the error components, while also increasing the estimator bias $(\boldsymbol{X}'\boldsymbol{M}'\boldsymbol{M}\boldsymbol{X})^{-1}\boldsymbol{X}'\boldsymbol{M}'E(\boldsymbol{M}\boldsymbol{u}|\boldsymbol{M}\boldsymbol{X})$. This will be the case if the estimator transforms away too much of the regressor variation $\boldsymbol{X}'\boldsymbol{M}'\boldsymbol{M}\boldsymbol{X}$ that serves to mitigate any remaining covariance between the errors and regressors.

4.1 Pooled OLS and random effects estimators

A natural point of departure for our econometric analysis is the pooled ordinary least squares (POLS) estimator: $\widehat{\boldsymbol{\beta}}_{POLS} = (\boldsymbol{X}'\boldsymbol{M}_P\boldsymbol{X})^{-1}\boldsymbol{X}'\boldsymbol{M}_P\boldsymbol{y}$,[10] where $\boldsymbol{M}_P = \left(\boldsymbol{I}_{NT} - \frac{1}{NT}\boldsymbol{i}_{NT}\boldsymbol{i}'_{NT}\right)$. This estimator will provide unbiased estimates of the technological parameters if $E(\boldsymbol{u}|\boldsymbol{X}) = \boldsymbol{0}$, which requires that all of the observed inputs are uncorrelated with the industry fixed effects η_n, the time shocks τ_t, the industry-specific time shock response χ_n, the industry technological parameter deviations $\boldsymbol{b}_n$, the measurement errors $\boldsymbol{e}_{nt}$ and any remaining productivity innovations ε_{nt}.

Clearly there are many reasons why the POLS estimator could produce biased results. The estimators discussed in the rest of this section attempt to remove the bias that occurs due to correlation between the factors of production and the different unobserved productivity components. Although the POLS estimator is sensitive to many types of endogeneity, it is also the only estimator that uses all of the available variation in the data with calculate the coefficient estimates. This is a drawback if some of the regressor variation is correlated with unobserved productivity, but a crucial advantage compared to estimators that discard information vital to accurately identifying the true production parameters.

Even if the conditions for POLS unbiasedness are met, the error structure of u_{nt} may still be exploited in order to provide more efficient estimates of the technological parameters if industry productivity differs in a way that does not affect employment or investment decisions. In the absence of measurement error and time shocks, the RE estimator can be estimated as $\widehat{\boldsymbol{\beta}}_{RE} = (\boldsymbol{X}'\boldsymbol{\Omega}^{-1}\boldsymbol{X})^{-1}\boldsymbol{X}'\boldsymbol{\Omega}^{-1}\boldsymbol{y}$, where $\boldsymbol{\Omega}$ is the block diagonal error covariance matrix.

[10] The POLS coefficients of the regressors can be recovered either by regressing output on the factors of production and a constant, or by regressing demeaned output on the demeaned factors of production, without a constant (as demonstrated by Frisch and Waugh (1933) and Lovell (1963)). The notation used here follows the latter method and is used in order to aid the comparability of the various estimators.

4.2 Fixed effects, time effects or two-way fixed effects estimators

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 will also coincide with employment and investment booms. The results from POLS estimators are therefore viewed with an understandable sense of suspicion in the production function literature (see Söderbom and Teal (2004) for example), and the use of panel data estimators that are unbiased in the presence of correlated industry effects has become standard econometric practice.

The fixed effects (FE) estimator is calculated by transforming away all time-invariant industry-specific variation in the data, using the idempotent within-groups transformation matrix, $\boldsymbol{M}_{WI} = (\boldsymbol{I}_{NT} - \boldsymbol{I}_N \otimes \boldsymbol{i}_T \boldsymbol{i}'_T)$[11], so that $\widehat{\boldsymbol{\beta}}_{FE} = (\boldsymbol{X}'\boldsymbol{M}_{WI}\boldsymbol{X})^{-1}\boldsymbol{X}'\boldsymbol{M}_{WI}\boldsymbol{y}$. Since the implied industry-demeaning removes potentially endogenous variation from the error and regressors, the conditions for FE estimator unbiasedness are less stringent than they are for POLS. Specifically, the existence of any correlation between the factors of production and industry fixed effects, $E(\eta_n|\boldsymbol{x}_{nt}) \neq 0$, will bias the POLS but not the FE estimators. The within-groups transformation also removes industry-specific measurement error and correlation between the industry technological coefficients and the factor inputs that could otherwise bias the POLS coefficient estimates. However, if some endogeneity remains after performing the within-group transformation – due to the importance of measurement error or global time shocks, for instance – then there is no guarantee that the FE estimates will be less biased than the POLS estimates. Whether this will be the case or not depends on how much of the regressor variation is removed by the within-groups transformation and how much endogeneity remains afterwards.

It is also possible to remove all the industry-invariant time-specific variation from the data with what can be called the "within-time periods" transformation $\boldsymbol{M}_{WT} = (\boldsymbol{I}_{NT} - \boldsymbol{i}_T \boldsymbol{i}'_T \otimes \boldsymbol{I}_N)$. This estimator – which we will for reasons of symmetry call the time effects (TE) estimator – will be biased in the presence of

[11] $\boldsymbol{I}_c$ is the identity matrix of dimension $c \times c$ and $\boldsymbol{i}_d$ is a $d \times 1$ vector of ones.

correlated fixed effects, but will produce unbiased results where correlation between the universal time shocks and the production factors would bias both the POLS and the FE estimates. Since this estimator does not explicitly allow the factor loading χ_n to vary by industry, it will only be unbiased if $\chi_n = \chi$ or if the effect of the time shocks on factor inputs and productivity is uncorrelated across industries. We will expand on this point in section 4.4 below. It also removes period-specific measurement error, something that the discussion of our data (in section 0) mentioned as a potentially important source of bias.

If the composite error term contains correlated time and industry effects, then the POLS, FE and TE estimators will all be biased. Combining the time and industry demeaning transformations, $\boldsymbol{M}_{2FE} = \boldsymbol{M}_{WT}\boldsymbol{M}_{WI}$, yields the two-way fixed effects (2FE) estimator, which now removes both industry and time-specific variation from the data. This estimator will therefore be unbiased in the presence of either correlated industry effects or time shocks, although any industry-specific productivity variation over time can still bias this estimator if it is also correlated with the factors of production.

Another candidate estimator, which requires very similar identifying conditions as the FE estimator, is the first-differenced estimator (FD), which differences the data before regressing production factors on output. In a panel data setup this estimator is defined as $\boldsymbol{M}_{FD} = \boldsymbol{I}_N \otimes \boldsymbol{M}_\Delta$ where M_Δ is the first differencing transformation matrix. Since the FD estimator differences away the between-industry variation in the data, the resulting estimates will suffer from the same decrease in estimator accuracy as the FE estimator if much of the regressor variation occurs across industries rather than time.

4.3 Parameter heterogeneity

The estimators discussed above all implicitly impose the restriction of slope homogeneity, but Burnside (1996) warns against exploiting this assumption when estimating cross-sector production functions. Using data for the U.S. manufacturing industry he finds that although this restriction seemingly improves estimator precision, the data strongly rejects its validity. We therefore introduce the mean group (MG) estimator – which explicitly allows for parameter heterogeneity – before considering how this affects the estimators discussed above.

The MG estimates are calculated by first obtaining estimates of $\boldsymbol{\beta}_n$ using OLS separately for each industry and then averaging these coefficient vectors across industries: $\widehat{\boldsymbol{\beta}}_*^{MG} = \frac{1}{N}\sum_{n=1}^{N}(\boldsymbol{X}_n{}'\boldsymbol{M}_P\boldsymbol{X}_n)^{-1}\boldsymbol{X}_n{}'\boldsymbol{M}_P\boldsymbol{y}_n$. Pesaran and Smith (1995) note that, unlike the estimators discussed above, the MG estimator explicitly estimates the expected value of the production parameters $\boldsymbol{\beta}_*$ as the average of the individual $\boldsymbol{\beta}_n$s, which is conceptually more consistent with the heterogeneous production process outlined at the beginning of this section. The variance for this MG estimator can be expressed as $Var\left(\widehat{\boldsymbol{\beta}}_n^{MG}\right) = \frac{\mathbf{1}}{\boldsymbol{N(N-1)}}\sum_{\boldsymbol{n=1}}^{\boldsymbol{N}}(\widehat{\boldsymbol{\beta}}_n^{MG} - \widehat{\boldsymbol{\beta}}_*^{\boldsymbol{MG}})^{\mathbf{2}}$, and the estimator bias as $\frac{1}{N}\sum_{n=1}^{N}(\boldsymbol{X}_n\boldsymbol{M}_P\boldsymbol{X}_n)^{-1}\boldsymbol{X}_n{}'\boldsymbol{M}_P\boldsymbol{E}(\boldsymbol{u}_n|\boldsymbol{X}_n)$, where the transformed error term now no longer contains the industry specific elements $\boldsymbol{x}_{nt}\boldsymbol{b}_n$, η_n or industry-specific measurement error. The MG industry-specific estimators only use data from single industries to estimate $\boldsymbol{\beta}_n$, so that correlation between the production factors and the industry fixed effects or coefficient deviations will no longer bias the individual industry estimates of $\boldsymbol{\beta}_n$ or, by implication, the average coefficient vector $\boldsymbol{\beta}_*$. On the other hand, the fact that only time series variation is exploited to estimate the production coefficients means that time dummies cannot be included in the regressions and the estimator will therefore be vulnerable to any correlation between global output shocks and the regressors. Discarding cross-sectional variation will also amplify any remaining endogeneity in the regressors in the same way that it does for the FE and FD estimators. If time series variation in the data is relatively unimportant compared to cross-industry variation, then this will leave us with very little confidence in the individual $\widehat{\boldsymbol{\beta}}_n^{MG}$'s, and only somewhat more in the cross-industry estimate of $\widehat{\boldsymbol{\beta}}_*^{MG}$.

If the parameters are heterogeneous but distributed independently of the regressors, then heterogeneity merely adds a specific type of heteroscedasticity to the error terms and will not affect the consistency properties of our estimators. Puzzlingly, Coakley, Fuertes, & Smith (2006) supposedly consider the effect of parameter heterogeneity on various heterogeneous panel data estimators, but never go beyond this rather restrictive type of heterogeneity. Not surprisingly, they conclude that parameter heterogeneity on its own is of little consequence. However, a more interesting case occurs if we allow "sorting on gains" as in the correlated random coefficient (CRC) model. If a firm's investment and employment decisions are affected by the returns to human or physical capital (as suggested by profit maximising behaviour), then this will induce a positive correlation between the error term in equation [18] and the untransformed model regressors via $\boldsymbol{x}_{nt}\boldsymbol{b}_n$, which violates the assumption of the (uncorrelated) random coefficient model. In this case the POLS and TE models will be biased, but any estimator that allows for industry-specific intercepts (such as FE, 2FE or MG) will be unbiased[12].

Much of the heterogeneous panel data literature is concerned with modelling dynamic features of the data generating process and producing consistent estimates of the model parameters with data that is characterised by non-stationarity. As explained in section 2, this study is in large part motivated by the African production function literature that has been unable to find schooling effects of a comparable magnitude to those estimated from earnings regressions. The production functions in these studies generally ignore dynamics in the production process and non-stationarity, and this is also the approach taken in our econometric analysis below. However, we do report tests of the time series properties of the different estimator residuals, and also discuss a few estimators that will produce consistent estimates with non-stationary data.

4.4. Cross-sectional dependence

A recent strand of the heterogeneous panel data literature considers the effect of cross-sectional dependence, with a particular focus on global time shocks that affect individual units differently. In our application that would imply industry heterogeneity not only in terms of how inputs affect production, but also in how common latent time shocks affect production. We briefly consider how the resulting cross-

[12] However, if the production parameters are correlated to non-linear functions of employment or investment levels, such as the variance, then none of the estimators we consider will be unbiased.

sectional dependence will affect the estimators discussed above, before introducing four estimators that could provide more reliable estimates under such conditions.

It is instructive to consider the process that generated the industry factor inputs; we follow Coakley, Fuertes and Smith (2006) in assuming that $\boldsymbol{x}_{nt}^{*} = \boldsymbol{\theta}_n \tau_t + \boldsymbol{q}_{nt}$, where $E(\boldsymbol{\theta}_n \tau_t | \boldsymbol{q}_{nt}) = 0$.[13] The $\boldsymbol{\theta}_n$ vector now captures the effect of a global productivity shock τ_t on industry employment and investment decisions, and $\boldsymbol{q}_{nt}$ represents the remaining determinants of these inputs. In this case the POLS, FE and MG estimators will only be unbiased if either $\chi_n = 0$ or $\boldsymbol{\theta}_n = \boldsymbol{0}$, that is if the universal time shocks either had no productivity effect, or had no effect on the input decisions of industries. The TE and 2FE estimators control for time effects without allowing for industry-specific factor loadings, so require somewhat weaker consistency conditions: $\chi_n = \bar{\chi}$ or $\boldsymbol{\theta}_n = \overline{\boldsymbol{\theta}}$ or $E\left((\chi_n - \bar{\chi})(\boldsymbol{\theta}_n - \overline{\boldsymbol{\theta}})\right) = \boldsymbol{0}$. These estimators therefore require one of three conditions to hold[14]: 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. In case the same industries are more responsive to global time shocks (like the business cycle) both in terms of output and employment or investment decisions, then none of the estimators considered so far will produce unbiased estimates of the model parameters.

If the POLS-FE-MG consistency conditions specified above are met, but the time shocks induce cross-sectional correlation in the model error terms, then Coakley et al. (2006) suggest combining the MG estimator with Zellner's (1962) seemingly unrelated regression (SUR) approach – estimating the $\boldsymbol{\beta}_n$'s for the different industries jointly, while allowing for non-zero cross-sectional covariance between the errors – in order to improve the efficiency of the estimates. This estimator will be no more robust to the various

[13] This process can also be conceived as a variation decomposition, in which case $\boldsymbol{\theta}_n$ is the regression coefficients $\frac{cov(\tau_t, x_{n,t}^{*})}{var(\tau_t)}$, and $E(\boldsymbol{\theta}_n \tau_t \boldsymbol{q}_{nt}) = 0$ by design.

[14] These conditions have not always been clearly stated in the heterogeneous panel literature (Coakley, Fuertes and Smith (2006) for example).

types of endogeneity that could bias the MG estimator, but the increased efficiency potentially afforded by the MG-SUR estimator could be important in small samples such as ours.

However, the MG-SUR estimator will still be biased in case $\chi_n \neq 0$ and $\boldsymbol{\theta}_n \neq \mathbf{0}$. Coakley et al. (2006) recommend using the demeaned mean group (DMG) estimator, which is calculated by estimating the MG estimator from period-demeaned data. The DMG-industry estimator will then be $\widehat{\boldsymbol{\beta}}_*^{DMG} = \frac{1}{N}\sum_{n=1}^{N}\left(\sum_{t=1}^{T}\tilde{x}_{nt}'\tilde{x}_{nt}\right)^{-1}\sum_{t=1}^{T}\tilde{x}_{n,t}'\tilde{y}_{nt}$, where $\tilde{\boldsymbol{x}}_{nt}$ is the period-demeaned $\boldsymbol{x}_{nt}$, and similarly for $\tilde{y}_{nt}$. This estimator will be unbiased under the same conditions as 2FE, but has the benefit of explicitly recognising the heterogeneity of the production parameters as part of the estimation procedure. Pesaran (2006) suggests using the common correlated effects mean group (CMG) estimator to provide consistent estimates of the model parameters under these conditions, which entails estimating the output equation separately for each industry using OLS but including output and input time means as regressors, i.e. estimating the following augmented regression equation:

$$y_{nt} = c_1\bar{y}_t + \boldsymbol{x}_{nt}\boldsymbol{\beta}_n + \overline{\boldsymbol{x}}_t\boldsymbol{c} + u_{nt}$$

where $\boldsymbol{c}' = [c_2, c_3, c_4, c_5]$. In the absence of measurement error, the average effect of the time shocks can be expressed as $\bar{\chi}\tau_t = \bar{y}_t - \overline{\boldsymbol{x}}_t\overline{\boldsymbol{\beta}} + v_{nt}$, where v_{nt} is sampling noise that will tend to zero as $N \to \infty$. The inclusion of $\overline{\boldsymbol{x}}_t$ and $\bar{y}_t$ as regressors will tend to absorb the effect of the time shocks, assuming that the cross-sectional dimension of the dataset is large enough. When adding measurement error to the model, the inclusion of these two terms will also control for any time-specific measurement errors. Unfortunately, this only controls for the homogeneous component of the time shocks, and the heterogeneous component $(\chi_n - \bar{\chi})\tau_t$ can still induce bias in the estimates in the same way that it can for the DMG or 2FE models. However, the simulation results in Coakley 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-DMG-CMG models. It bears noting that both the DMG and CMG estimators only exploit the within-industry variation in the data, and will therefore suffer the same estimator inaccuracy associated with the FE, 2FE or FD estimators.

Another estimator that will provide consistent estimates of $\boldsymbol{\beta}_*$ in the case of slope heterogeneity and cross-sectional dependence is the cross-section (CS) or between-groups estimator, which requires regressing the cross-sectional average of output on the cross-sectional average of the inputs, that is estimating $\bar{y}_n = \bar{\boldsymbol{x}}_n \boldsymbol{\beta}_* + \bar{u}_n$, where $\bar{u}_n = \bar{a} + \eta_n + \bar{\boldsymbol{x}}_n \boldsymbol{b}_n - \bar{\boldsymbol{e}}_n \boldsymbol{\beta}_n + \bar{\varepsilon}_n$. 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. On the other hand, it is the only estimator considered so far that will not be biased by time shocks (even if $E\left((\chi_n - \bar{\chi})(\boldsymbol{\theta}_n - \bar{\boldsymbol{\theta}})\right) = \mathbf{0}$) or two-way demeaned measurement error. Pesaran and Smith (1995) show that this estimator provides consistent estimates of $\boldsymbol{\beta}_*$ even if the error terms are distributed I(1). Since this estimator ignores all within-industry variation in the data, we would expect it to be fairly inaccurate 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.

4.5 Measurement error

As discussed in section 2, cross-country studies of the human capital-growth nexus have sometimes ascribed the low estimated schooling effect to problems in obtaining comparable measures of educational attainment. However, few production function studies on African or South African data have attempted to consider the effects of measurement error, which may explain the surprisingly low estimated schooling effects. Section 0 discussed some of the reasons why the variables in our dataset may have been inaccurately measured, and speculated that the industry employment variable compiled from the StatsSA surveys is likely to be less accurately measured than the capital or schooling variables. Although worker schooling levels are compiled from the same set of inconsistently sampled surveys, some of the sampling 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 no within-industry transformation will remove much of the resulting endogeneity. In fact, our discussion in section 0 suggests that $\boldsymbol{e_{nt}}$ will have a strong time-specific component, the elimination of which would require controlling for or averaging across time effects.

The classical errors-in-variables model suggests that the coefficient of the mismeasured regressor will suffer a downward attenuation bias proportional to the measurement error variance and inversely proportional to the regressor variation that is orthogonal to the other regressors. In the case where a second, correctly measured regressor is positively correlated to the mismeasured regressor, Thiel (1961) shows that the bias in the coefficient of the correctly measured variable will be higher the stronger the correlation between the two explanatory variables, and of the same sign as the mismeasured variable's coefficient.

Given that all of our production factors are positively correlated (not shown) and presumably measured with some error, this suggests that the coefficients for each variable will be ambiguously affected: the own-error bias will exert the usual attenuation bias while the other-variable-errors will tend to upwardly bias the estimates. The structure of the regressor covariance and the suspected magnitudes of the different measurement errors suggest that the own-error downward bias will be relatively larger for the labour coefficient, whereas the capital coefficient – which is fairly consistently measured over time, and highly correlated to both mismeasured labour and education – may well be upwardly biased. The net effect on the education coefficients is difficult to gauge without deeper analysis. These biases will tend to be exacerbated when using an estimator that transforms away much of the regressor variation without removing the measurement error.

There are at least three methods that we can use to identify the education effect in the presence of measurement error: the Grilliches-Hausman estimator, fixing certain production parameters to their "known" values (as per Krueger and Lindahl's (2001) suggestion) and instrumental variable techniques. The first approach was pioneered by Griliches and Hausman (1986), who noted that 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. If measurement error is important in explaining why the different estimators are producing contrasting results, then we would expect the results to converge as we make use of longer differencing.

The second method of attempting to circumvent the effects of measurement errors is to follow Krueger and Lindahl (2001) in fixing the labour and capital parameter coefficients to their shares of national income. We can check the robustness of the resulting human capital estimates by varying the capital and labour parameter values within a broad range of potentially realistic values.

Finally, if our dataset contains variables that are correlated to the true values of our factor inputs and uncorrelated to the measurement error, then IV estimators such as 2SLS can also recover consistent estimates of the production parameters. This estimator attempts to remove the endogeneity associated with measurement error by transforming away all of the endogenous variation in the regressors according to $\boldsymbol{M}_{IV} = \boldsymbol{Z}(\boldsymbol{Z}'\boldsymbol{Z})^{-1}\,\boldsymbol{Z}'$ so that $\widehat{\boldsymbol{\beta}}_{2SLS} = (\boldsymbol{X}'\boldsymbol{Z}\boldsymbol{Z}'\boldsymbol{X})^{-1}\boldsymbol{X}'\boldsymbol{Z}\boldsymbol{Z}'\boldsymbol{y}$. As with the estimators above, the downside of this approach is that the decrease in regressor variation sacrifices estimator accuracy and could amplify any remaining correlation between the regressors and unobserved productivity that was not successfully transformed away. The 2SLS estimator is known to be inaccurate in small samples, and this problem is exacerbated by using many or weak instruments (as discussed in Bound, Jaeger and Baker (1995), and Staiger and Stock (1997)). In this case the IV estimates will be biased in the direction of the POLS estimates and the actual size for hypothesis tests will be inflated relative to the stated test size

Where IVs are uncorrelated to the measurement error but correlated to industry fixed effects or other error components this IV transformation can be combined with other transformations in order to remove multiple sources of endogeneity. For example, the IV estimates on industry-demeaned data (which we will refer to as the IV-FE estimator) uses $\boldsymbol{M}_{IV-FE} = \boldsymbol{M}_{IV}\boldsymbol{M}_{WI}$ and will be unbiased in the presence of measurement error and correlated industry effects. Furthermore, this estimator can produce unbiased estimates of the model parameters where it would otherwise be affected by the simultaneity of output and factor inputs.

One popular IV candidate is the lagged values of the mismeasured production factors. The one-period lagged terms would be valid and informative if production factors were generated as an AR(1) processes – due, for example, to the gradual adjustment of employment levels that results from quadratic adjustment costs – and measurement error is serially uncorrelated. Other potential instruments include industry labour costs, unionisation rates and the cost of capital. Section 0 also alluded to an alternative measure of employment which suffered substantial measurement problems but in a way that was unrelated to the problems that plagued our own industry employment variable.

5. Empirical results

We now estimate the industry-level production function using the data discussed in section 0 and the estimators introduced in section 0. Table 1 reports the coefficient estimates obtained from using the POLS, TE, FE, 2FE, RE and FD estimators (as discussed in sections 4.1 and 4.2). The POLS estimates indicate that the marginal return to employing better educated workers is very high on average: about 31% at the mean schooling years. This is even higher than the large earnings regression estimates obtained in most earnings regression. The schooling coefficients also imply that returns are steeply decreasing in schooling years, which is surprising given the convex schooling-earnings profiles reported in most South African earnings regressions. The capital and labour coefficients are slightly below 0.3 and above 0.6 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 (for example, the studies cited in footnote 3). These coefficients suggest decreasing returns to scale (RTS) for the typical industry.

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

	POLS	**TE**	**RE**	**FE**	**2FE**	**FD**
Dependent variable	*Log output*	*Log output*	*Log output*	*Log output*	*Log output*	*D(Log output)*
Observations	117	117	117	117	117	108
R-squared	0.957	0.958	0.984	0.991	0.992	0.114
Coefficient estimates						
Constant	1.922***	1.623***	7.409***	5.895***	8.032***	0.019***
	(0.485)	*(0.493)*	*(0.991)*	*(1.106)*	*(1.136)*	*(0.007)*
Log capital stock	0.283***	0.285***	0.411***	0.86***	0.654***	0.831***
	(0.016)	*(0.016)*	*(0.059)*	*(0.110)*	*(0.106)*	*(0.181)*
Log employment	0.601***	0.605***	0.279***	0.039	0.062	-0.025
	(0.018)	*(0.018)*	*(0.075)*	*(0.084)*	*(0.072)*	*(0.077)*
Average education	0.784***	0.798***	0.104	-0.016	-0.05	0.047
	(0.089)	*(0.085)*	*(0.123)*	*(0.080)*	*(0.082)*	*(0.092)*
Average education^2	-0.037***	-0.038***	0.003	0.008*	0.006	-0.003
	(0.005)	*(0.005)*	*(0.007)*	*(0.005)*	*(0.005)*	*(0.005)*
Period dummies	No	Yes	No	No	Yes	No
Industry dummies	No	No	No	Yes	Yes	No
DW statistic	0.568	0.509	1.050	0.775	0.686	1.518
Implied RTS	0.885	0.890	0.690	0.898	0.716	0.806

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

The second column in Table 1 reports the results from a TE regression, and demonstrates that controlling for homogenous time shocks has little impact on the coefficient estimates and only marginally increases the regression R-squared. This suggests that global productivity shocks (or time-specific measurement error) induce a negligible amount of coefficient bias in the POLS estimates, at least under the maintained hypothesis of parameter homogeneity.

The estimators in Table 1 and the rest of this paper use the quadratic education specification in the human capital link function. Although this aids the comparability with the results presented in most of the South African earnings function literature and is therefore our preferred specification, we would still like to compare these results to those obtained when using alternative human capital link functions. Table 6 in the appendix presents the TE estimates with different specifications for average worker schooling years. The first column, which includes no measure of human capital, produces labour and capital coefficients that are very close to labour and capital's shares of total income. This result is consistent with previous South African production function studies that also failed to control for human capital (as discussed in section 4.1). The next three columns show the coefficient estimates for functions in which schooling was included logarithmically, linearly and quadratically, and in all three cases the human capital terms are highly significant and their inclusion leads to a marked improvement in the explanatory power of the model.

The education coefficient from the linear specification implies a rate of return in excess of 32% for each additional year of education, which is much higher than the estimates obtained from international individual-level studies (Psacharopoulos (1994), Case and Yogo (1999)) but close to the upper end of the range of such studies for South Africa (as surveyed in Keswell and Poswell (2004)). The coefficients of the logarithmic and quadratic specifications imply schooling returns at the mean schooling level of 32% and 31% respectively. The fact that the logarithmic function – which restricts the schooling effect to be a decreasing function of schooling year – is a better fit than the linear function suggests a convex schooling-production profile, and this is confirmed by the coefficients of the quadratic specification which also yields the highest R-squared.

Controlling for average worker schooling level does not substantially alter the coefficient on labour, although the physical capital coefficient is reduced. This suggests that the failure to control for the effect of human capital on production in previous South African studies may have over-estimated the effect of capital in the production process. However, it is worth bearing in mind that 80% of the variation in the average industry schooling variable occurs between 6.4 and 11.2 years of education: a much narrower range than what is observed for individuals. This variable may therefore be too crude to accurately identify the exact shape of the schooling-production profile for schooling years at sparser parts of this distribution.

Returning to the estimates in Table 1, the RE estimator allows for uncorrelated industry effects and exploits the error variance structure to provide more efficient production coefficient estimates. Surprisingly, the RE estimates (column 3) are markedly different from the POLS results, despite the fact that the two models require the same identifying assumptions for unbiasedness. The capital coefficient is now much closer to its share in total income, but the labour coefficient drops to below 0.3 and the education terms are both insignificant (although the implied returns are still sizable). Section 4.2 mentioned a few reasons why the industry fixed effects could be correlated with the production factors, in which case all three of the estimators discussed above would be biased. In contrast, the FE, 2FE and FD estimates could be unbiased even if industry effects are correlated to inputs. These estimates (reported in columns 4, 5 and 6) are very different from those of the POLS and TE estimators: the returns to education are now insignificant and more or less negligible in size. Furthermore, the capital coefficients are much higher than capital's share in income or the estimates produced in other South African studies, whereas the labour coefficients fall to something close to zero.

We are therefore confronted with two very different pictures of what the South African production process looks like. According to the POLS model the total returns to education (including production externalities) are extremely high and concave which – if true – would offer empirical evidence supporting policies to allocate more resources to improving access to education, and early education years in particular. On the other hand, the FE model suggests that education itself contributes almost nothing to productivity: some industries use production technologies that make them more productive and perhaps require hiring more educated workers, but merely hiring more educated workers has little effect on output.

From a purely mechanical perspective, removing the cross-sectional variation from our data clearly reduces the roles assigned to labour and schooling in the production process. One interpretation of this result would be that high levels of human capital merely reflect the important industry fixed effects, so that these variables are falsely assigned an important role when not allowing for different industry intercepts. Alternatively, removing the cross-sectional variation in the data could make the FE estimator very sensitive to any remaining endogeneity that arises from measurement error, parameter heterogeneity or cross-sectional dependence. If this noise makes it impossible to identify the schooling effect, then the regression will tend to overstate the importance of its correlates, notably physical capital and the industry fixed effects. This same pattern has emerged for other African countries as well (Söderbom & Teal (2004), for example) where it is typical to interpret the FE estimator as producing more credible results. This despite the fact that the capital coefficients produced by such regressions (Bigsten, et al., 2000) are – as in the FE model in Table 1 – much higher than capital's share of income. Furthermore, the FD estimator is the only model in Table 1 not to suffer from residual autocorrelation, as represented by the Durbin-Watson (DW) statistics. We therefore need to carefully investigate the effects of measurement error, parameter heterogeneity and cross-sectional dependence on our estimates.

Before turning to estimators that yield consistent estimates when industries are heterogeneous or inputs are measured with error, it is worth briefly considering the composition of the regressor variation in our data. As discussed in section 0, the FE estimator may be more susceptible to remaining sources of endogeneity *if* the within-groups transformation leads to a substantial reduction in regressor variation. Unless this is the case, there is little reason to think that the POLS results would be more robust than the FE model. Table 2 reports the results from decomposing the variation into cross-sectional, time and the remaining two-way variation. All of the production factors can be observed to have a very high proportion of industry-specific variation (99% for capital, 96% for labour and 94.5% for education), so that any estimator that transforms away the cross-industry variation is bound to suffer a large decrease in accuracy. If this transformation does not rid the data of all the covariance between the regressors and the error, then the POLS may have a lower relative bias than the FE model. In order to further explore this hypothesis, we now turn to methods that could provide unbiased estimates when endogeneity is not completely transformed away by industry- or time-demeaning.

Table 2: Variance decomposition for factors of production

	Industry-specific	Period-specific	Two-way demeaned
Log capital	99.0%	0.4%	0.6%
Log labour	96.0%	0.9%	3.1%
Education	94.5%	3.8%	1.7%

Source: Author's calculations from October Household Surveys 1995-99, Labour Force Surveys (March & September rounds) 2000-2007 (StatsSA: various years) and Quarterly Bulletin data 1995-2007 (South African Reserve Bank, various years)

5.1 Measurement error

We next investigate whether measurement error could be biasing our estimators by using the three approaches mentioned in section 4.5. Firstly, Griliches and Hausman (1986) showed that if the model errors are serially uncorrelated, then the measurement error bias in the FD estimates should diminish as longer differences are used. Columns 2, 3 and 4 in Table 2 compare the results from the FD estimator using 3 year, 5 year and 12 year lags, to those obtained from one year differenced data in column 1 (replicated from the FD coefficients in Table 1).

Table 3: Panel data estimates of production function coefficients

	FD	FD	FD	FD
Dependent variable	***D^i(Log output)***	***D^i(Log output)***	***D^i(Log output)***	***D^i(Log output)***
Differencing period	**1 year (i = 1)**	**3 years (i = 3)**	**5 years (i = 5)**	**12 years (i = 12)**
Observations	108	90	72	9
R-squared	0.197	0.422	0.421	0.280
Constant	0.066**	0.058**	0.112**	-0.029
	(0.033)	*(0.028)*	*(0.052)*	*(0.306)*
D^i(Log capital stock)	0.844***	0.725***	0.604***	0.414
	(0.184)	*(0.130)*	*(0.117)*	*(0.459)*
D^i(Log employment)	-0.012	0.037	0.098	0.398
	(0.073)	*(0.049)*	*(0.070)*	*(0.364)*
D^i(Average education)	0.031	-0.095	-0.052	0.533
	(0.080)	*(0.107)*	*(0.079)*	*(0.403)*
D^i(Average education^2)	-0.002	0.007	0.007	-0.011
	(0.004)	*(0.006)*	*(0.005)*	*(0.022)*
Period dummies	Yes	Yes	Yes	Yes
Industry dummies	No	No	No	No
DW statistic	1.502	0.525	0.427	.
Implied RTS	0.832	0.762	0.702	0.812

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

The capital coefficient can be seen to decrease as the data is differenced over longer lags, whereas the coefficient on labour increases. Using 12 year lags consumes almost all of our degrees of freedom, so that very little confidence can be placed in the precision of these point estimates, but it is still interesting to note how close the results from this regression – including the education coefficients – are to those obtained from the POLS estimator. This is supportive of the notion that measurement error is a source of bias in the one-period FD (and by implication the FE) estimates.

The second approach to estimating the human capital coefficients in the presence of measurement error (as discussed in section 4.5) is to fix the coefficient values of labour and capital to reasonable values. This will prevent the schooling coefficients from being biased by the correlation between education and other mismeasured inputs. Table 4 reports the FE and FD regression estimates from production functions in which the employment coefficient has been fixed to values of 0.5, 0.6 and 0.7.

Table 4: FE and FD production function regressions with fixed employment coefficient values

	2FE	2FE	2FE	FD	FD	FD
Dependent variable	*Log output*	*Log output*	*Log output*	*D(Log output)*	*D(Log output)*	*D(Log output)*
Observations	117	117	117	108	108	108
Coefficient estimates						
Constant	5.504***	4.926***	4.349***	0.141*	0.156*	0.171*
	(0.085)	*(0.093)*	*(0.102)*	*(0.080)*	*(0.090)*	*(0.100)*
Log capital stock	0.281***	0.196*	0.111	0.38	0.289	0.198
	(0.106)	*(0.115)*	*(0.126)*	*(0.327)*	*(0.381)*	*(0.436)*
Log employment	0.5	0.6	0.7	0.5	0.6	0.7
	.	.	.	.	.	.
Average education	0.139	0.183	0.226	0.179	0.208	0.237
	(0.127)	*(0.139)*	*(0.152)*	*(0.143)*	*(0.168)*	*(0.193)*
Average education^2	-0.001	-0.003	-0.004	-0.01	-0.012	-0.014
	(0.007)	*(0.008)*	*(0.009)*	*(0.008)*	*(0.009)*	*(0.010)*
Period dummies	Yes	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	No	No	No
DW test statistic	1.196	1.275	1.333	1.625	1.658	1.682

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

In case of both the FE and FD estimators, fixing this coefficient has the effect of producing physical and schooling coefficients that are rather similar to the POLS estimates in Table 1. The human capital coefficients indicate substantial returns and concavity, although the coefficients themselves are not significant. A higher employment coefficient is associated with a lower capital coefficient and schooling returns that are more concave, but the underlying picture that emerges from these regressions is fairly robust to the exact value of the employment coefficient. This evidence supports the notion that the POLS estimates are relatively accurate compared to the FE and FD estimates that are biased by the measurement error in the employment variable.

Finally, Table 8-Table 10 in the appendix report the results of a series of IV regressions aimed at addressing the problems of measurement error (and perhaps also simultaneity).[15] Table 8 presents the results of five different IV-TE regressions (including time, but not industry dummies in the regression). Columns 1, 2 and 3 in Table 8 all treat industry employment as the only endogenous regressor, since this

[15] In all cases the limited information maximum likelihood estimation technique was used rather than the 2SLS discussed in section 4.5, since Stock and Yogo (2005) find that the results for this estimator are less sensitive to the effects of weak instruments.

variable is considered to be the most severely affected by measurement error. The first regression instruments for our chosen employment measure using the alternative measure of industry employment (discussed in section 0), whereas the second regression adds a one-period lag of this alternative measure as well as the one- and two-period lags of our own employment series to the instrument set. The third column exploits the one- and two-period lags of the average industry wage rate as IVs. Compared to the TE estimates in Table 1, instrumenting has little effect on either the capital or labour coefficient (both are increased marginally). Furthermore, in all three regressions the schooling effect remains large and concave, but somewhat lower at the mean schooling level than suggested by the TE estimator. The two remaining regressions also consider the other production factors as endogenous and use as instruments the one-period lag of all the inputs (column 4) as well the average wage rate, the bond yield and the one-period lag of the industry unionisation rate (column 5). The results from these IV-TE regressions are virtually indistinguishable from the TE regressions that took all the regressors as exogenous. The Hansen J-test and F-test statistics indicate that the instrument sets are far from weak in all cases, and that there is little evidence that our instruments are themselves endogenous.

The IV regressions in Table 9 add industry dummies to the IV-TE regressions in Table 8 in order to control for correlated industry productivity differentials, and hence represent the IV-2FE estimates discussed in section 4.5. The implied data transformation now removes any variation from the data that is industry or period-specific, or that does not covary with the instrumental variables. This combination means that any weak instrument problems or remaining endogeneity will be exacerbated, something that is reflected in the low F-tests and inflated standard errors. On the other hand, this estimator can provide consistent estimates of the production parameters even where the POLS model was biased by time shocks, industry effects and measurement error. In general the capital, labour and human capital coefficients are relatively sensitive to the exact choice of instruments. However, the results also show that instrumenting for the regressors can produce large schooling effects, even when allowing for correlated industry effects.

Finally, Table 10 also reports the results of the IV-FD estimators corresponding to the endogenous variables and instruments sets in Table 8 and Table 9. This implied data transformation is very similar to that of the IV-2FE estimator, and we therefore expect the resulting coefficients to share the same strengths and weakness as the IV-2FE estimates. Again, this is confirmed by the low F-statistics and few significant coefficients. Much the same as for the IV-2FE model, the IV-FD estimator produces capital and labour coefficients that are often inaccurate and volatile. Although the schooling coefficients are generally insignificant, the implied returns are substantial (although lower than estimated by the POLS model) and concave. In general, the results from our instrumental variable regressions therefore support the notion that the POLS results actually offer a relatively accurate reflection of the schooling effect in the production process, but that this effect is hidden by the problems in measuring the industry input levels (particularly employment).

5.2 Parameter heterogeneity

The preceding results were produced with estimators that were developed under the assumption of parameter homogeneity, but the discussion in section 4.3 explained how randomness in the production parameters can bias these estimates. In order to investigate the validity of the constant coefficient assumption, Table 7 in the appendix reports the coefficient estimates obtained by running OLS on each industry separately (the MG-industry estimators) as well as the average and standard deviations of these industry coefficients (the MG estimator). The MG-industry results could scarcely provide less support for the notion of identical production parameters across industries. However, given the fact that these estimates are likely to be very inaccurate – due to the short time period and how little variation in the data occurs along the time series dimension – and sensitive to measurement error and common correlated time shocks, it is difficult to conclude with any confidence that the cross-industry variation in the production coefficients reflect true parameter heterogeneity. The fact that the industry point estimates of the labour and capital coefficients lie outside the unit interval as often as not further detracts from our faith in these results.

The MG estimator averages away some of the sampling error and is hence likely to be more accurate than the industry-specific estimates. However, the final column in Table 7 demonstrates the cost of attempting to estimate so many different parameters from so little variation in the data: although the production coefficients are often sizable, none of them differ significantly from zero. Although this is disconcerting, it is in line with the experience of Burnside (1996), who found that imposing slope homogeneity was without grounds and artificially increased estimate precision. Compared to the FE estimator, allowing for parameter heterogeneity slightly decreases the point estimate on the physical capital coefficient while the coefficients on labour and human capital both remain small and insignificant. The MG coefficient estimates more closely resemble the FE than the POLS estimates, though the large standard errors also demonstrate how ambitious it is to estimate the average of potentially heterogeneous coefficients using only within-industry variation in such a small data set.

Like the FE, 2FE and FD estimators, the MG estimator is unbiased in the presence of correlated industry fixed effects, correlated industry production coefficients or industry-specific measurement error but will be similarly vulnerable to other types of measurement error or common correlated time shocks due to the relatively small amount of variation in the data that is used to calculate the estimates. Additionally, the MG-estimator cannot control for pure time effects and will therefore be biased by time shocks that are correlated with production factors, even where these effects impact homogeneously across all industries. Nonetheless, there are two conclusions that can be drawn from the MG estimates in Table 7. Firstly, allowing for parameter heterogeneity but not cross-sectional dependence does not substantially alter the estimates produced by the FE estimator, and would seem to support the notion that POLS tends to over-estimate the effect of human capital in production because of its very restrictive identifying assumptions. Secondly, the results also highlight the importance of viewing the coefficient estimates obtained from

POLS or FE as providing a summary statistic of the distribution of the industry (or firm)-specific production parameters, rather than a constant parameter that is equally valid in every industry.

5.3 Cross-sectional dependence

Table 5 compares the results from the 2FE and MG estimators – which did not allow for cross-sectional dependence – to those produced with the MG-SUR, DMG, CMG and CS estimators (as discussed in section 4.4). The MG-SUR estimates are broadly similar to those of the MG and 2FE estimators. Allowing for cross-industry correlation in the error terms – while still assuming that these shocks are uncorrelated to the regressors – has the effect of slightly increasing all of the production function coefficients, including the effect of schooling. However, these estimates still suffer from the large standard errors that also characterised the MG estimates. The DMG estimator effectively controls for homogenous global time shocks, and is therefore more robust than either the MG or the MG-SUR estimators. The production function coefficient estimates also have smaller standard errors, although these are still much larger than those of the "homogeneous parameter" estimators. Its high physical and low human capital estimates are consistent with the FE interpretation of the data, although our discussion in section 4.4 warned that this model will also be inconsistent unless the time shocks have a homogenous effect across industries, its effect on industry inputs is constant, or its effect on productivity is uncorrelated to its effect on each of the inputs. Although none of the estimators we consider will be consistent unless at least one of the three conditions above are satisfied, the CMG model has been shown to perform better in small samples, and to be less sensitive to the violation of these conditions. In terms of evaluating whether cross-sectional dependence in the data can explain the low education returns reported in production function studies for Africa, we are therefore particularly interested in the results from this estimator. The coefficients (Table 5, column 5) show that the labour coefficient is still lower than what we would expect, but that the schooling coefficients are virtually the same as for the POLS estimator.

Finally, the CS estimator will also be unbiased under fairly general conditions, including parameter heterogeneity and I(1) errors, although the averaging transformation sacrifices a lot of efficiency and degrees of freedom in order to achieve this. These estimates, reported in column 6 of Table 5 closely resemble those of the POLS estimator. This may be because, apart from the POLS and TE estimators it is one of the few estimators that also exploits the (very informative) between-industry variation in the data.

Table 5: Various heterogeneous parameter panel data estimates of production function coefficients

	FE2	**MG**	**MG-SUR**	**DMG**	**CMG**	**CS**
Dependent variable	*Log output*	*Log output*	*Log output*	*Log output*	*Log output*	*Log output*
Observations	117	117	117	117	117	9
R-squared	0.992	0.998	0.996	0.998	1.000	0.964
Coefficient estimates						
Constant	8.032***	9.738	7.436	-0.056	-5.028	0.695
	(1.077)	*(9.604)*	*(8.859)*	*(0.497)*	*(4.079)*	*(1.994)*
Log capital stock	0.654***	0.474	0.609	0.867	0.498	0.297**
	(0.098)	*(0.629)*	*(0.516)*	*(0.528)*	*(0.545)*	*(0.073)*
Log employment	0.062	0.059	0.069	0.127*	0.128*	0.628***
	(0.055)	*(0.075)*	*(0.076)*	*(0.066)*	*(0.068)*	*(0.073)*
Average education	-0.05	-0.044	0.069	-0.009	0.824*	0.97**
	(0.078)	*(0.733)*	*(0.745)*	*(0.280)*	*(0.471)*	*(0.297)*
Average education^2	0.006	0.006	0.001	0.000	-0.045*	-0.048*
	(0.004)	*(0.037)*	*(0.036)*	*(0.016)*	*(0.023)*	*(0.018)*
DW statistic	0.511	2.102	1.876	1.627	1.964	.
Implied RTS	0.716	0.533	0.678	0.993	0.626	0.925

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

Like most other firm or industry-level datasets available for Africa, ours does not have the dimensions required to conclusively determine whether cross-sectional dependence has downwardly biased the estimated education effect in other studies. However, the fact that the CMG and CS education estimates are so similar to those obtained from the POLS and measurement error models certainly suggest that cross-sectional dependence may have biased – and potentially severely biased – the FE, FD and 2FE results reported in other studies.

6. Conclusion

This paper has studied the way in which nine broadly defined South African industries used human capital in the production process during the twelve years following political transition. It does so by constructing a dataset from SARB Quarterly Bulletins and StatsSA household surveys. This novel dataset allows us to explore issues that previous studies on African and particularly South African data were unable to do. It also investigated the results from a wide range of estimators, in order to investigate the effect of measurement error and cross-industry differences in production technology on the predicted schooling effects.

The POLS results suggests labour and physical capital coefficients of approximately 0.6 and 0.3 respectively, and that the returns to education were high and concave. The FE and FD estimates, on the other hand, found that the returns to both labour and schooling were much lower, and the effect of physical capital somewhat higher. This is consistent with the pattern observed in other African production function studies. Our analysis of the data shows that the industry-demeaning transformation discards between 95 and 99% of the variation in the regressors, which makes the FE and FD estimates much less accurate and highly sensitive to any biases (including measurement error bias or differences in production technologies across industries) that are not successfully transformed away. The POLS estimates, on the other hand, may be more robust to the presence of these types of biases.

Using longer differences in the FD estimator or fixing the employment coefficients to reasonable values, both suggest that the FE and FD estimates suffered from large measurement error bias, and that the POLS estimates provide a more reliable account of the production process in the typical industry. Using instrumental variables to explicitly account for measurement error also produced results that generally, although not uniformly, supported the original POLS estimates rather than the FE and FD estimators. The fact that the IV-FE estimator assigns a smaller role to schooling in the production process than POLS suggests that average worker education levels are correlated with industry productivity, which could complicate efforts to simultaneously identify both of these two effects.

Although the FE and FD estimators allow industries to produce with different levels of productivity, they still restrict output to respond identically to changes in the inputs or global time shocks. Using estimators that exploit less restrictive identifying assumptions, we show that accounting for parameter heterogeneity and (particularly) cross-sectional dependence also produces estimates that more closely resemble the POLS coefficients estimates that those from the FE and FD estimators. The results also highlight the importance of viewing the coefficient estimates obtained from POLS or FE as providing a summary statistic of the distribution of the industry specific production parameters, rather than a constant parameter that is equally valid in every industry.

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8. Appendix

Table 6: POLS estimates of production function coefficients, with various human capital link functions

	TE	TE	TE	TE
Dependent variable	*Log output*	*Log output*	*Log output*	*Log output*
Observations	117	117	117	117
R-squared	0.822	0.944	0.930	0.958
Coefficient estimates				
Constant		3.421***	5.329***	1.623***
	(0.558)	*(0.325)*	*(0.352)*	*(0.530)*
Log capital stock	0.416***	0.236***	0.226***	0.285***
	(0.031)	*(0.021)*	*(0.025)*	*(0.021)*
Log employment	0.552***	0.571***	0.557***	0.605***
	(0.040)	*(0.023)*	*(0.025)*	*(0.020)*
Log average education		1.492***		
		(0.099)		
Average education			0.182***	0.798***
			(0.014)	*(0.076)*
Average education^2				-0.038***
				(0.005)
Period dummies	Yes	Yes	Yes	Yes
Industry dummies	No	No	No	No
Durbin-Watson statistic	0.071	0.287	0.224	0.422
Implied returns to scale	0.97	0.81	0.78	0.89

Table 7: OLS time series regression estimates of production function, by industry

	Agri-culture	**Mining**	**Manu-facturing**	**Con-struction**	**Utilities**	**Internal trade**	**Transport-ation**	**Financial services**	**Community services**	**Total Average**
Dependent variable	*Log output*	*Log output*	*Log output*	*Log output*	*Log output*	*Log output*	*Log output*	*Log output*	*Log output*	*Log output*
Observations	13	13	13	13	13	13	13	13	13	117
R-squared	0.594	0.737	0.965	0.474	0.969	0.984	0.974	0.954	0.930	0.998
Coefficient estimates										
Constant	67.686*	7.636**	-5.932	22.998**	18.801***	12.584	-13.464	-37.23*	14.558	9.738
	(30.242)	*(2.728)*	*(7.038)*	*(7.296)*	*(4.732)*	*(9.556)*	*(10.470)*	*(18.799)*	*(19.978)*	*(9.604)*
Log capital stock	-4.207	0.65***	1.393***	-0.208	0.463***	1.157***	2.023***	1.896**	1.097***	0.474
	(2.554)	*(0.144)*	*(0.232)*	*(0.329)*	*(0.105)*	*(0.233)*	*(0.263)*	*(0.594)*	*(0.182)*	*(0.629)*
Log employment	-0.42***	-0.035	0.405***	0.068	0.174	0.046	0.007	0.246	0.041	0.059
	(0.116)	*(0.026)*	*(0.109)*	*(0.090)*	*(0.110)*	*(0.079)*	*(0.159)*	*(0.248)*	*(0.090)*	*(0.075)*
Average education	0.693	0.526	0.077	-1.094	-2.426**	-1.893	0.852	4.786	-1.919	-0.044
	(0.630)	*(0.420)*	*(1.060)*	*(0.898)*	*(0.996)*	*(1.638)*	*(2.003)*	*(3.050)*	*(3.245)*	*(0.733)*
Average education^2	-0.072	-0.032	0.001	0.057	0.16**	0.101	-0.035	-0.21	0.082	0.006
	(0.056)	*(0.026)*	*(0.055)*	*(0.044)*	*(0.063)*	*(0.083)*	*(0.099)*	*(0.133)*	*(0.143)*	*(0.037)*
DW statistic	2.579	2.189	2.479	2.055	2.674	1.858	1.567	1.041	0.976	2.102
Implied RTS	-3.934	1.142	1.875	-1.235	-1.790	-0.690	2.882	6.928	-0.781	0.489

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

Table 8: IV-TE estimates of production function coefficients

	IV_TE	IV_TE	IV_TE	IV_TE	IV_TE
Dependent variable	*Log output*	*Log output*	*Log output*	*Log output*	*Log output*
Observations	104	88	99	108	108
R-squared	0.970	0.974	0.970	0.965	0.965
Coefficient estimates					
Constant	3.754***	3.035***	1.513**	1.638***	1.625***
	(0.811)	*(0.814)*	*(0.634)*	*(0.520)*	*(0.520)*
Log capital stock	0.317***	0.316***	0.292***	0.286***	0.286***
	(0.018)	*(0.018)*	*(0.019)*	*(0.020)*	*(0.020)*
Log employment	0.611***	0.614***	0.619***	0.618***	0.618***
	(0.017)	*(0.017)*	*(0.032)*	*(0.020)*	*(0.020)*
Average education	0.306*	0.492***	0.811***	0.806***	0.806***
	(0.161)	*(0.160)*	*(0.077)*	*(0.076)*	*(0.076)*
Average education^2	-0.014*	-0.024***	-0.038***	-0.038***	-0.038***
	(0.008)	*(0.008)*	*(0.005)*	*(0.005)*	*(0.005)*
F-test	1000.725	1133.293	22.749	193.115	108.492
J-test	0.000	3.739	0.876	0.000	2.271
p-value	.	0.291	0.349	.	0.518
Period dummies	Yes	Yes	Yes	Yes	Yes
Industry dummies	No	No	No	No	No
Implied RTS	0.929	0.931	0.911	0.904	0.904
Endogenous regressors	Log employment	Log employment	Log employment	All factors of production	All factors of production
Instrumental variables	Log employment (alternative measure)	Log employment (alternative measure, lags 0 & 1), log employment (lags 1 & 2)	Log wage (lags 1 & 2)	All factors of production (lag 1)	All factors of production (lag 1), log wage, bond yield, union membership (lag 1)

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

Table 9: IV-FE estimates of production function coefficients

	IV-2FE	IV-2FE	IV-2FE	IV-2FE	IV-2FE
Dependent variable	*Log output*	*Log output*	*Log output*	*Log output*	*Log output*
Observations	104	88	99	108	108
R-squared	0.983	0.993	0.994	0.991	0.945
Coefficient estimates					
Constant	3.898	6.613***	5.623***	5.191***	-0.8
	(2.431)	*(1.418)*	*(1.289)*	*(1.861)*	*(7.387)*
Log capital stock	-0.076	0.087	0.27	0.402*	-0.549
	(0.334)	*(0.269)*	*(0.178)*	*(0.238)*	*(1.077)*
Log employment	0.841***	0.584***	0.542***	0.463*	1.523
	(0.289)	*(0.188)*	*(0.162)*	*(0.246)*	*(1.175)*
Average education	0.457	0.401	0.087	-0.007	0.451
	(0.364)	*(0.289)*	*(0.097)*	*(0.145)*	*(0.569)*
Average education^2	-0.016	-0.018	-0.001	0.012*	0.006
	(0.018)	*(0.015)*	*(0.005)*	*(0.007)*	*(0.017)*
F-test	8.208	5.574	7.882	1.345	0.808
J-test	0.000	8.337	1.091	0.000	5.346
p-value	.	0.040	0.296	.	0.148
Period dummies	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes
Implied RTS	0.764	0.671	0.811	0.866	0.974
Endogenous regressors	Log employment	Log employment	Log employment	All factors of production	All factors of production
Instrumental variables	Log employment (alternative measure)	Log employment (alternative measure, lags 0 & 1), log employment (lags 1 & 2)	Log wage (lags 1 & 2)	All factors of production (lag 1)	All factors of production (lag 1), log wage, bond yield, union membership (lag 1)

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

Table 10: IV-FD estimates of production function coefficients

	IV-FD	IV-FD	IV-FD	IV-FD	IV-FD
Dependent variable	*D(Log output)*	*D(Log output)*	*D(Log output)*	*D(Log output)*	*D(Log output)*
Observations	96	80	108	99	99
R-squared	0.157	0.420	0.119	0.007	0.215
Coefficient estimates					
Constant	0.028	0.024	0.017	0.024	0.015
	(0.022)	*(0.016)*	*(0.020)*	*(0.021)*	*(0.014)*
D(Log capital stock)	0.49	0.603**	0.691***	1.001***	1.09***
	(0.507)	*(0.272)*	*(0.261)*	*(0.268)*	*(0.195)*
D(Log employment)	0.245	0.162	0.157	0.097	-0.084
	(0.494)	*(0.213)*	*(0.166)*	*(0.347)*	*(0.091)*
D(Average education)	0.127	0.163	0.08	0.2	0.188
	(0.100)	*(0.159)*	*(0.089)*	*(0.169)*	*(0.153)*
D(Average education^2)	-0.008	-0.009	-0.005	-0.018	-0.013
	(0.006)	*(0.008)*	*(0.005)*	*(0.012)*	*(0.009)*
F-test	0.277	0.973	4.270	0.180	1.401
J-test	0.000	.	0.000	0.000	1.863
p-value	.	0.485	.	.	0.601
Period dummies	Yes	Yes	Yes	Yes	Yes
Industry dummies	No	No	No	No	No
Implied RTS	0.735	0.765	0.848	1.098	1.005
Endogenous regressors	Log employment	Log employment	Log employment	All factors of production	All factors of production
Instrumental variables	D(Log employment) (alternative measure)	D(Log employment) (alternative measure, lags 0 & 1), D(log employment) (lags 1 & 2)	D(Log wage)	D(All factors of production) (lag 1)	All factors of production (lag 1), D(log wage), D(bond yield), D(union membership)

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