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Jonathan Haskel, Denise Hawkes and Sonia Pereira

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Jonathan Haskel, Queen Mary, University of London; AIM, CeRiBA, IZA and CEPR
Denise Hawkes, Centre For Longitudinal Studies, Institute of Education and University of London
Sonia Pereira, UCL, Barnard college and CeRiBA

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Centre for Economic Policy Research
90–98 Goswell Rd, London EC1V 7RR, UK
Tel: (44 20) 7878 2900, Fax: (44 20) 7878 2999
Email: cepr@cepr.org, Website: www.cepr.org

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ABSTRACT

Skills, human capital and the plant productivity gap: UK evidence from matched plant, worker and workforce data*

Using two matched plant level skills and productivity datasets for UK manufacturing we document that (i) more productive firms hire more skilled workers: in 2000, plants at the top decile of the TFP distribution (controlling for their four-digit industry) hired workers with, on average, around 1/3rd of a year of additional schooling compared to firms in the bottom decile and (ii) in an accounting sense the skills gap between the firms in the top and bottom deciles of the TFP distribution accounts for 3 to 10% of the TFP gap depending on the specification used.

JEL Classification: D24, J24 and L6
Keywords: productivity and skills

Jonathan Haskel
Department of Economics
Queen Mary
University of London
London
E1 4NS

Tel: (44 20) 7882 5365/5095
Fax:(44 20) 8455 7923
Email: j.e.haskel@qmul.ac.uk

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Denise Hawkes
Centre for Longitudinal Studies (CLS)
Institute of Education
20 Bedford Way
LONDON
WC1H 0AL

Tel: (44 20) 7612 6624
Fax:(44 20) 7612 6880
Email: dh@cls.ioe.ac.uk

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Sonia Pereira
Visiting Fellow
Economics Department
University College
London
WC1H 0AX

Email: s.pereira@ucl.ac.uk

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1 Introduction

A number of studies have documented large and quite persistent differences in productivity between businesses (Bartelsman and Dhrymes, 1998, Bailey, Campbell and Hulten, 1992, Foster, Haltiwanger, Syverson, 2003, Syverson, 2004). Few business-level data sets however have information on skills. This paper matches business-level productivity and skill data to attempt to answer two questions: (a) do more productive businesses employ a more skilled workforce? (b) how much of the variation in productivity is associated with variation in skills?

A number of studies of the relation between productivity levels and skills are based on cross-country data. Hall and Jones (1999), for example, document that the five poorest countries in their data had, in 1998, 8 years less average years of education than the five richest. Based on a 10% return to schooling they calculate that the five richest should be 2.2 times more productive for this reason. But the five richest are in fact 32 times more productive (in terms of labour productivity, seven times in terms of TFP). O'Mahony and DeBoer (2002), who study relative productivity between the UK, US, France and Germany, using standardised qualifications find, for example, that in 1999 Germany had 20% of workers with low qualifications whereas the UK and US had 57% and 54% respectively. However, they also find that bringing UK workers up to German levels would raise labour productivity by 3.6%, whereas the actual gap is 19%. In these two studies therefore differences in schooling account for about 20% and 4% of differences in labour productivity, respectively (although a higher fraction of TFP).[1]

Very much less work has been done at the plant level. Plant level studies are a useful complement to the cross-country and cross-industry studies. First, cross-country studies face the formidable problem of comparing different education systems. Second, industry productivity can change due to skill upgrading in firms but also the sorting process in the market (entry, exit, changes in market share see e.g. Foster, Haltiwanger and Krizan, 2001, Disney, Haskel and Heden, 2003), making it hard to ascribe the effect of skills on plant productivity with industry data. Third, just as there is large variation in productivity/TFP between countries, which researchers have sought to explain, there is also enormous variation in productivity/TFP between businesses within a single country, which cross-country studies cannot of course shed light on. A major objective of this paper is to see how much of this variation is associated with skill variation.

The first challenge to investigating the relationship between skills and productivity at the business-level is that the principal business-level productivity datasets (from accounting or official

[1] These studies look at the level of education and the level of productivity. A number of other cross-country studies look at the effect of the level of education on the growth of productivity, e.g. Barro (1991). A number of industry studies look at the growth of education and the growth of productivity, see e.g. Jorgenson et al (1987), O'Mahony and Oulton (2000), Lau and Vaze (2002) for US and UK industry work, from which one can infer a level/level relationship, but this is typically not the focus of this type of study.

production *Census* data) typically have no data on skills. Some *Census*-based plant-level data sets in some countries measure workers as manuals (or production) and non-manuals (or non-production). Whilst this is correlated with skills levels, it is generally acknowledged as being rather crude.[2] In this paper we construct better skill data by matching worker and plant level data. Specifically, we construct two data sets by matching UK business *Census*[3] data with two skill data sets. The first is the 2001 Department for Education and Skills (DfES) *Employer Skills Survey* (ESS) which surveys around 30,000 businesses and, for this cross-section, asks detailed information on qualifications. We then relate business-level productivity to this measure of skills.

Such a match, although a step forward from previously available data, suffers from at least two weaknesses. First, whilst qualifications measure hard/formal skills, it is widely conjectured that soft/informal skills (e.g., timekeeping, motivation and interpersonal characteristics) are also important, which the ESS does not measure. Second, the data are a single cross-section and hence association between skills and productivity might be biased due to the correlation between skills and unobservable business characteristics that affect productivity.

To overcome these two problems we build a data for a panel of businesses, whose skill data includes informal skills. Thus, our second match with business *Census* data is with the *New Earnings Survey* (NES), currently available for 1994-2000[4]. Unlike the ESS, the NES contains no qualification data. It has however, worker-level information on wages, occupation and age, and the resulting match forms an employee-employer panel. Following Abowd et al (2002), we estimate a panel wage equation controlling for age, person and firm fixed effects and use the estimated person effects and the implied experience effect as a proxy for the workers' skills. This measure thus captures both hard and soft skills insofar as they are valued in the marketplace. We then relate business-level productivity to this other measure of skills.

How does our work relate with others in the literature? To the best of our knowledge no such UK dataset has been used to analyse these questions[5]. Regarding productivity and qualifications, for the US,

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[2] In addition, the policy implications from such an analysis are not clear. For example, should policy be designed to increase the supply of non-manual/non-production workers?

[3] The UK business-level *Census* data is the Office for National Statistics *Annual Business Inquiry* which contains extensive data on outputs and inputs and can therefore be used to measure labour productivity and TFP. It is the micro data that, in aggregated form, is the basis of the UK industry and whole economy studies cited above. Its only skill data is manual/non-manual (discontinued in 1996). Neither of the other surveys that we use have any productivity data.

[4] 1997 can not be matched because the firm identifiers in the *New Earnings Survey* are missing from the archive files.

[5] The only work we are aware of on a UK matched employee/employer data set is the pioneering work of Hildreth and Pudney (1998 and 1999). They too matched the New Earning Survey with the Annual Business Inquiry and we follow their matching method, detailed in their 1998 paper. At that time they only had 1994 and 1995 data available, they did not estimate panel wage equations controlling for person and firm effects as we do neither did they look at their effect on productivity. In a preliminary version of this paper, Haskel, Hawkes and Pereira (2003), focussed more on data issues and presented an OLS estimate of a Cobb-Douglas production function.

Hellerstein, Neumark and Troske (1999) and Hellerstein and Neumark (2004) match a business-level US data set with worker information for one cross section and estimate production functions using worker qualification, age and gender information. They find a positive relation between productivity and skills and our cross-section match with the ESS qualification information is similar to this method.[6]

Regarding productivity and wage-based skill measures, we follow Abowd, Kramarz and Margolis (1999) and Abowd, Haltiwanger, Jarmin, Lane, Lengermann, McCue, McKinney, and Sandusky, (2004). Using French and US data respectively, these studies find a strong positive correlation between skills (measured from employer/employee wage equations) and productivity. Abowd at al (2002) also find effects from the structure of skills at an organisation (namely that the most and least skilled workers have a disproportionate positive and negative impacts on productivity).

Using these new UK data, we establish three findings. First, we document that more productive firms do indeed hire more skilled workers. In 2000, in our sample of manufacturing business, plants at the top decile of the TFP distribution (controlling for their four-digit industry) hired workers with, on average, around 1/3rd of a year of additional schooling compared to firms in the bottom decile.[7] Second, we also find strong positive correlations between different measures of skills and productivity in both our cross-section and panel estimates. Third, our estimates suggest that skill differences account for between 3 and 10 percent of the TFP gap between firms in the top and bottom deciles of the TFP distribution. Interestingly, our findings are very similar to the US findings in Abowd et al (2002) and the French findings in Abowd et al (1999). They find a one standard deviation change in their skills index implies a change in labour productivity of 11 and 15 log points respectively: we find an implied change of 16 log points.

As well as producing novel results for the UK however, we believe our work goes beyond the extant literature in three main ways. First, we have both a panel of workers and a panel of plants. Abowd et al (2002) build their US human capital regressions from a panel of workers 1996-2000 but their productivity data is based on a 1997 cross-section. Abowd et al (1999) have a panel of French firms but pool their data when looking at the relation between human capital and productivity. We estimate cross-section, pooled and fixed-effects regressions of (various measures of) productivity on skills and we find that the correlation between productivity and skills is robust to the various specifications. Second, as well as looking at labour productivity, as these studies have done, we look at Total Factor Productivity (TFP) using a method, based on Klette (1996) that is robust to heterogeneous output elasticities, imperfect competition, measurement error of capital, non-adjustment of capital to its equilibrium value, non-constant returns to scale and unobservable plant-specific price indices. Third, to explore the causal

[6] Their prime interest is to calculate implied marginal productivities from a production function and compare to those implied by a wage equation. Jones (2001) undertakes a similar study using matched employee/employer data for Ghana. Both find that skill, as measured by education, is positively correlated with productivity.
[7] Not controlling for the four-digit industry of the plant gives a difference of about 2 years.

effect of skills on TFP we experiment with (within-industry) regional skill shortages as an instrument, and discuss the conditions under which it is valid. We find a significant effect of skills on TFP which is somewhat larger than the OLS estimates, but we interpret these with caution since they can be upward biased.

There are (at least) two limitations with our work. First, the analysis is for manufacturing only due to data limitations at time of writing and uncertainty about the correct productivity measure for many service sector industries. Second, our wage-derived skill measure is based on a partial sample of the firm's workers. For this reason, unlike Abowd et al (2002), we cannot look at the distribution of skills within the firm with this skill measure.

This paper proceeds as follows: section 2 describes the data, the data matching and the skills measures used, section 3 sets out the methods used, section 4 presents the regression results, section 5 explores a number of robustness checks and section 6 concludes.

2 Skills measures and data matching

2.1 Qualifications-based skill measures from the Employers Skills Survey (ESS)

The basic skill information from the ESS is drawn as follows. First, firms are asked to report the fraction of workers who are in each of the following 9 occupational groups: managers, professionals, associates, administrators, skilled manual, personal services, sales, machine operatives and elementary occupations (see appendix 1). Second, firms are asked to specify the most common qualification held by their employees in each of these nine occupational groups. The list of qualifications given is set out in Table 1 (see also appendix 1).

This enables us to build three skill indices. The first imputes years of education to each qualification level as set out in Table 1. This inevitably involves some measurement error since the questionnaire groups academic qualifications (which typically have a prescribed learning time) and non-academic qualifications (National Vocational Qualifications, NVQs[8]) which do not have such a time. We therefore create a second measure that simply allocates an index from 1 to 6 to the different qualifications, where 6 stands for the "highest" qualifications, again as set out in column 2 of Table 1. With the exception of levels 1 and 2, differences in this measure can be interpreted as differences in NVQ levels. Our third measure is to simply use for each firm the proportions of workers with each

[8] NVQs are a qualification, often obtained via assessment of on-the-job performance in a technical or practical subject designed to show that a person has a range of skills useful for employment, see http://www.dfes.gov.uk/nvq/what.shtml.

qualification level described in Table 1, which we combined into levels 4, 3, 2, 1, other and none (column 3).

2.2 Wage-based skill measures from the New Earnings Survey (NES)

Our second broad measure of skills derives from wages in the NES. Qualifications measure only formal skills, yet it might be that informal/soft skills are also important. If they are part of human capital and are valued in the marketplace they will be reflected in wages. Wages might however diverge from the productivity-enhancing aspects of human capital for a number of reasons: e.g. efficiency wages (companies may pay higher wages to reduce turnover, increase productivity, etc.), product market rents, workers' bargaining power. Thus wages would depend both on worker characteristics that affect their productivity but also on firm characteristics, such as wage policy and product market competitiveness.

To attempt to control for this, we therefore follow Abowd, Kramarz and Margolis (1999) and regress the (log) hourly wage of person p in business i at time t ($\ln w_{pt}$) on a person fixed effect f_p, a employer fixed effect $\Psi_{i(p,t)}$, an age (we do not have experience) polynomial interacted with a gender dummy variable[9] AGE'_{pt} and a set of time dummies λ_t:

$$\ln w_{pt} = f_p + \alpha AGE'_{pt} + \Psi_{i(p,t)} + \lambda_t + \varepsilon_{pt} \qquad (1)$$

where the subscript $i(p,t)$ indicates the employer i of individual p at time t and ε is an iid error. Firm-specific effects on the wage such as efficiency wages, profit sharing etc. are therefore captured by $\Psi_{i(p,t)}$, enabling us to construct three measures of human capital that correct for these effects. They are (i) the experience component, αAGE_{pt}; (ii) the fixed labour quality person effect f_p , and (iii) 'total' human capital h_{pt} which is the sum of these two

$$h_{pt} = \alpha AGE'_{pt} + f_p \qquad (2)$$

[9] We included an interacted gender dummy variable because the relationship between age and labour market experience differs on average between men and women. We could also include a non-interacted female dummy because if women are discriminated against in the labour market their wages in relation to males' wages underestimate their productivity all else constant. However, with this specification, the individual fixed effects might overestimate the relative productivity of females if part of the raw male-female wage differential is explained by workers' and job characteristics such as education, actual experience and occupational sorting. Since the inclusion of the female dummy did not affect the productivity regressions in our empirical section, we choose to keep the simplest form and not to include the non-interacted female dummy.

The basic idea then is to measure worker human capital as the market's valuation of an individual's experience αAGE_{pt}, and the remaining portable component of their skills f_p, controlling for the firm component in the wage.

A number of points are worth noting. First, as Abowd, Kramarz and Margolis (1999) emphasise, many studies estimating wage equations omit $\Psi_{i(p,t)}$ in (1). $\Psi_{i(p,t)}$ captures the component of the wage that is specific to the establishment, due to rent sharing, wage profiles etc. We therefore hope that the workers' individual effects will offer a much better approximation of the workers' human capital than measures which omit $\Psi_{J(i,t)}$. Second, identification of person and employer fixed effects requires that the employer has at least one worker with at least one job change; in a worker-business pair in which the worker has no other job spells observed in the data, one can not identify how much of the unobserved component in wages is due to the employer or to the worker. Thus identification rests on mobility. We follow the literature and assume that it is exogenous to the included regressors.

Third, since identification comes from mobility the person and firm effects are identified for person A moving between firms 1 and 2 let us say, likewise person B moving between firms 3 and 4. Unless person A then moves to firms 3 and 4 or person B to firms 1 and 2 there are therefore different "groups" of "connected" individuals and employers (Abowd, Creecy and Kramarz, 2002) [10]. In fact, because individual fixed effects are estimated by including a set of person dummies in (1) and establishment effects by including a set of business dummies, the two full sets of dummies and the constant term are not uniquely identified and so cannot be compared across groups. One possible way to proceed is to set both the mean on the individual effects and the mean of establishment effects equal to zero for all groups. However, because the data set includes various separate groups of connected individuals and employers, this identifying restriction would have to be applied to each group of connected individuals and employers. Given that in our data, there are many small groups, setting the means on the individual effects and the establishment effects equal to zero for each of these small groups can be problematic, since some groups can comprise workers with very different average skills and/or very different average establishment characteristics[11]. In fact, if this is the case individual and establishment effects are not comparable across different groups of connected individuals and employers. To avoid having to make such assumptions we simply estimate (1) on the largest group of connected

[10] A group of connected persons and employers contains all the workers who are observed to have ever worked for any of the employers in the group and all the employers at which any of the workers were ever employed. The groups of connected individuals and establishments can be determined by applying methods from the graph theory. The program identifying the groups of inter-connected firms and individuals used in this paper was kindly provided by Ralf Martin, and it applies the grouping algorithm in Abowd, Creecy and Kramarz (2002).

[11] Given that in our data we only observe a small percentage of workers for each firm (on average 1.4), our data is characterised by a larger number of very small groups, when compared to other data where all workers are observed for each firm.

individuals and employers rather than for all possible groups as Abowd et al (2002) do.[12] We then use (1) to construct (2) which is similar to that used in Abowd et al (2002). Finally, one limitation of this technique is that the estimation of fixed effects by definition only captures the workers' wage component that is fixed over time. This does not capture the heterogeneous accumulation of human capital over time (human capital is allowed to increase overtime with experience, but equation (1) assumes that the impact of years of experience – proxied by age – is similar for all individuals with the same gender).

2.3 Data matching

We match the UK business *Census* data, called the *Annual Business Inquiry* to two sources of skill data - the *Employers Skill Survey* and the *New Earnings Survey*.

2.3.1 The Annual Business Inquiry (ABI)

The ABI is an annual business survey that covers almost all production and construction activities as well as distribution and some other service activities[13] (see Criscuolo, Haskel and Martin, 2003, for an extensive description). Information on the universe of UK businesses is maintained by the Office for National Statistics using the Interdepartmental Business Register (IDBR). The ABI is a sample drawn from the IDBR according to IDBR employment: surveys all businesses with 250 employees or more, and a sample of smaller businesses according to stratification rules based on size and industry. The ABI collects information on output, employment, materials, investment, region, industry and business structure (presence of other plants in the firm) and occasional questions such as R&D, e-commerce and computer expenditure.[14]

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[12] This is also driven by the binding constraint that all our computing has to be performed at the ONS micro data lab and this group exhausts the available computing power. For information, there are about 11,000 groups. The largest group in fact consists of about 30% of all matched workers and 12% of firms. The second largest group has only 0.3% of all the workers.

[13] It does not cover some sectors, notably, the public administration and defence, agriculture, fishing, financial intermediation, non-private education, private households with employed persons and only has limited coverage on health and social work.

[14] It is compulsory to return data if sampled. We investigated increasing the sample using non-returned data. For businesses that are not sent a survey (predominantly the smaller firms since all large firms are surveyed) we have their IDBR data. The IDBR records the businesses' region, industry, business structure, turnover and employment. Thus we cannot calculate TFP for these businesses since we have no material and capital data. Can we calculate labour productivity using the turnover and employment data? For these businesses, turnover and employment is calculated from their tax records (VAT and PAYE records). If they have only VAT or PAYE records but not both, the missing employment or turnover data is interpolated. According to ONS (2001, table 2), in October 2000 66% of businesses on the IDBR (1,260,098) had never been sent a survey, of whom 74% (931,378, 49% of the total number of businesses on the register) had only PAYE or VAT information but not both, ruling these firms out for labour productivity analysis. Businesses who have been sampled have either been covered by the ABI, and so we use their data, or by the Annual Register Inquiry (ARI), a bigger sample than the ABI used to update IDBR information but which only asks for employment data and not the for the more detailed information on output, materials etc. that the ABI asks for. However such employment information is out of date (ONS, 2001, p.56) since small firms are resampled only every four years (to reduce compliance costs) and other administrative data are not, by convention, used to update the ABI data. Thus we believe the only reliable source of TFP data is from the ABI returned data. See Crisculo, Haskel and Martin (2003) for further details.

To reduce compliance costs, multi-plant businesses have some degree of choice in the way they report the information to the ABI. They can report on all the plants individually or on one or various groups of plants. Data is therefore collected at what the ONS calls the *reporting unit* level, where a reporting unit is a plant or a group of plants. Since this is the level at which data is collected, that is the level we work with in this paper. In sum, our ABI data over the relevant period potentially available for matching consists of about 12,000 businesses, 10,000 of whom are single plant firms, accounting for about ½ of total employment (Criscuolo, Haskel and Martin, 2003).

Due to the lack of availability of price and capital data for the service sector as well as doubts about the appropriate output measure in many service industries, our empirical work is for manufacturing. Capital stock is derived from ABI investment, using the perpetual inventory method (see Martin (2002)).

2.3.2 The Employers Skills Survey (ESS)

The ESS is a survey sent to businesses, conducted annually since 1999. The 2001 ESS (to which we have been given access) was sent to around 27,000 businesses and covered all sectors of the economy for plants with one or more employees (subsequent surveys were much smaller). The survey covers a range of subjects including product market characteristics, product strategy & skills, recruitment problems, skills & proficiency and training. For the purpose of this study we shall focus on the workforce skill questions from the ESS, which ask employers about the skill level of the workforce at the sampled business.[15]

The ESS 2001 consisted of a pilot in October 2000 and was in the field November 2000 - April 2001. As the ABI is conducted mainly by financial year we match the ESS 2001 to the ABI 2000. Details of the matching process can be found in Hawkes (2002). The key issue is that the ESS is carried out at plant level (it is a telephone survey and therefore calls an individual plant). Recall that ABI reporting units can be single plants but they can be multiple plants as well. For single plants the ESS/ABI match is straightforward; for these reporting units, the skills information in the ESS applies to the complete workforce in the reporting unit. For multi-plant businesses each match with the ESS produces skill information for only one plant.

In the results below we use only the ESS plants matched to ABI reporting units where the reporting unit consists of only one plant. This smaller sample of single plant businesses may lead to selection bias, but the additional matches with multi-plant reporting units have the great disadvantage of introducing measurement error due to the partial sampling of skills at the reporting unit. The results using matches with multi-plant reporting units (available on request) were somewhat less well determined (as one would expect with measurement error), but very similar to the ones reported.

[15] For more information on the ESS see IFF Research Ltd (2002).

2.3.3 *The New Earnings Survey* (NES)

The NES is a one percent sample of all employees who are members of the British PAYE (Pay As You Earn) tax scheme[16], carried out in April of each year. It selects all individuals whose National Insurance number ends with a specific pair of digits. Since the same pair of digits has been used since 1975, workers are followed over time, as long as they remain in paid employment. The questions cover earnings, incentive pay, working hours (including full and part time), occupation, pension arrangements, collective bargaining, age and gender. Crucially for us, the NES identifies the enterprise where the employee works and we use this information to match to the ABI data (see Haskel and Pereira (2002) for details of the NES/ARD match[17]). We have information on matched workers and firms for the years 1994-6 and 1998-2000 (the PAYE identifiers are missing on the 1997 NES data archive).

2.4 Descriptive statistics: do more productive firms employ more skilled workers?

Table 2 sets out a description of the basic data. Panel 1a shows sample means and standard deviations of employment and skills for the 292 ESS/ABI plants[18]. As the top two left hand cells show, mean employment is 161 employees, with 12.3 years of education. Recall that this sample consists of firms with one plant only, and therefore its average firm size is rather small. Looking at columns 4 to 8, on average, 11% of workers are skill level 1, 38% level 2, 20% level 3, 16% level 4 and 10% have no or other qualifications. To control for industry effects, panel 1b shows these variables in terms of deviations from their four-digit industry means. Averages (relative to the four-digit industry mean) are shown for

[16] Though the NES coverage of full-time adult employees is virtually complete, the coverage of part-time employees is not comprehensive, since many of those with earnings below the income tax threshold (for example, in 1998 this is equivalent to full-time earnings of £80.67 a week or £349.58 per month) are not covered, which excludes mainly females with part-time jobs and a small proportion of young workers (ONS, 1998).

[17] Matching is somewhat complex, since the PAYE identifies the enterprise at which the worker is employed, which can consist of more than one ABI reporting unit. An enterprise is defined as a business or group of businesses with common ownership. Enterprises are free to report their information to the ABI at plant level or separately for certain groups of plants. Note that regarding the discretion given to enterprises in the way they report their information, reporting units are consistent over time. The matching is therefore done in three sequential steps. First, we match the PAYE code with the enterprise group identifiers (this is the only correspondence available) and collect those individuals for whom the enterprise group identifiers corresponds to one reporting unit. Second, the NES also reports the Industry code (SIC) of each employee. Thus, those individuals not matched in step one are matched to a reporting unit whose SIC corresponds uniquely to the SIC of the worker in the same enterprise. Third, the NES reports the postcode of workers' workplaces. Thus, those individuals not matched in the two previous steps are matched where it is possible to obtain a unique match to a reporting unit using the enterprise identifier and post code.

[18] The reasons why we lose such a high number of observations relative to the raw ESS and NES are set out in Hawkes (2002) and Haskel and Pereira (2002) respectively. The main reason is that our ABI data here are for manufacturing which is now a small fraction of employment. Furthermore, the ABI does not have complete employment, materials and investment data for all manufacturing businesses, but rather a sample (a sample of smaller firms and all firms above 250) which results in a further loss of data.

firms in the top and bottom deciles of the log TFP distribution. Column 11 shows that TFP in the top decile plants is 37.4% higher than their industry mean and in the bottom decile is 33.7% lower. Columns 2 and 3 show that plants in the lowest TFP decile employ below average skill levels whether measured by years of education or the ordinal skill index. The years of education measure indicates a gap of about 1/3rd of a year of education between top and bottom plants. Looking at the share of level 1 employees, this is about the same in the top and bottom decile plants, but the top plants employ a higher share of workers with levels 3 and 4 skills. Panel 1.c multiplies all these numbers by the plant four digit industry average labour share (for ease of interpretation in the regressions in the subsequent sections). The pattern is very similar, since labour shares do not vary strongly across plants. Thus this table suggests that more productive plants do indeed employ higher skilled workers (in terms of their qualifications).

Panel 2 describes the data for the 1,187 reporting units in the NES/ABI match. We restrict our sample to those reporting units with at least 7 workers matched, which given that the NES is a 1% sample means restricting ourselves to large firms. Panel 2a confirms this, average employment is 1,679 in the NES sample, in sharp contrast with 161 in the ESS sample. Panel 2.b confirms that more productive plants do indeed employ higher skilled workers, when skill measure (human capital) is derived from individual wages (although the implied value of the experience difference is very small).

From this table, we can compute an initial estimate of the fraction of variation in productivity that variation in skills might account for. Using wage equations, Sianesi (2003, Table 1) reports the following raw returns to qualifications in the U.K.: degree, 69%[19], A levels, 43% and O levels 28%. Table 1, panel b, column 8, shows the highest and lowest lnTFP deciles had a differential of about 20 percentage points in the share of workers qualified to level 3 and 4 , i.e. A levels or a degree (column 8). If the average return to such individuals is (69+43)/2=56% and the output elasticity of labour is 0.25 (roughly the share of all labour types in gross output) then a 20 percentage point difference predicts an output differences of 0.20*0.56*0.25= 0.03 difference in output which is 0.03/(0.37-(-0.34))=4% of the TFP gap between the top and bottom firms. Of course, the above calculations assume that returns to skills are correctly identified from wage equations. In the next sections we shall attempt to identify returns to skills from productivity equations as follows.

3 Skills and productivity

We attempt to estimate the effect of skills on productivity via a production function. In doing so, we try to account for at least some of the legion of criticisms of production functions. We therefore allow for heterogeneous technology, imperfect competition, mis-measurement of capital, non-adjustment of capital to its equilibrium value, non-constant returns to scale and unobservability of a plant-specific

price index. This framework is based on Klette (1996), Grilliches and Klette (1996), Klette (1999) and Melitz (2001). We also allow for potential endogeneity of skills by IV methods and for endogeneity of capital by Olley and Pakes (1996) methods.

3.1 Skills and productivity with heterogeneous technology, imperfect competition, mis-measurement of capital, non-adjustment of capital to its equilibrium value, non-constant returns to scale and unobservability of a plant-specific price index

Following Klette (1996), write the production function for plant *i* in industry *I* at time *t* as

$$Y_{it} = A_{it} F_t(M_{it}, K_{it}, QL_{it}) = A_{it} F_t(M_{it}, K_{it}, (SKILL_{it} \times L_{it})) \tag{3}$$

where *QL* is the quality of labour inputs *L*, which we have written as *(SKILL×L)* where *SKILL* converts each labour input into efficiency units according to their skill. We will be more explicit about the *SKILL* variable below. Note that *F* is subscripted by *t*, thus allowing for any type of technical change (neutral, factor augmenting etc.) over time common to plants in an industry; A_{it} is idiosyncratic, firm-specific technology. Using the mean value theorem, (3) can be written generally in log deviations from industry mean as

$$(y_{it} - y_{It}) = (a_{it} - a_{It}) + \sum_{j=M,K,L} \overline{\gamma}_{it}^{j}(x_{it}^{j} - x_{It}^{j}) + \overline{\gamma}_{it}^{L}(skill_{it} - skill_{It}) \tag{4}$$

where lower case letters are logs and for any input or output *X*

$$\overline{\gamma}_{it}^{j} \equiv \left[\frac{X_{it}^{j}}{F_t(X_{it}^{j})} \frac{\partial F_t(X_{it}^{j})}{\partial X_{it}^{j}} \right]_{X_{it} = \overline{X}_{it}} \tag{5}$$

i.e. γ is the output elasticity evaluated at $\overline{X}_{it}$ in the interval between X_{it} and X_{It} and, from (3), $\overline{\gamma}_{it}^{QL} = \overline{\gamma}_{it}^{L}$. This then allows the production function to be of any functional form of which the Caves et al. (1982) form is a special case. Note too that the output elasticities, $\overline{\gamma}_{it}^{j}$ vary by plant and year.

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[19] These are the cumulated numbers from GCSEs, A levels and first degree in column 3 of table 1.

We now wish to allow for non-constant returns to scale, imperfect competition, capital mismeasurement, non-adjustment of capital to its equilibrium value and unobserved plant-specific price indices. We do this as follows. For any paid input X^j we have a first-order condition for the output elasticity

$$\bar{\gamma}_{it}^{j} = s_{it}^{j}\mu \tag{6}$$

where $s^j = (\bar{W}^j_{it}X^j_{it})/(\bar{P}_{it}Y_{it})$ which is the cost share of input j relative to total revenue, again evaluated at $\bar{X}_{it}$, and $\mu \geq 1$ is the ratio of price to marginal cost thereby capturing any form of competition. We assume (6) holds for labour and materials, $j=L, M$. For capital, K, using (6) to measure γ_K requires us to measure the price and quantity of capital accurately in order to calculate s_K and, also, since (6) is assumed to hold period by period that capital adjusts to its equilibrium value costlessly. We wish to relax these assumptions which we can do as follows. The elasticity of scale in production, $\bar{\eta}_{it}$ can be written as the sum of the various output elasticities

$$\bar{\eta}_{it} = \sum_{j=M,L,K} \bar{\gamma}_{it}^{j} = \sum_{j=M,L} \bar{\gamma}_{it}^{j} + \bar{\gamma}_{it}^{K} \tag{7}$$

hence one can write

$$\begin{aligned}(y_{it} - y_{It}) = (a_{it} - a_{It}) + \mu_{It}\left(\sum_{j=M,L} \bar{s}_{it}^{j}(x_{it}^{j} - x_{It}^{j}) + (1 - \bar{s}_{it}^{L} - \bar{s}_{it}^{M})(x_{it}^{K} - x_{It}^{K})\right) + \\ (\bar{\eta}_{it} - \mu_{It})(x_{it}^{K} - x_{It}^{K}) + \mu_{It}\bar{s}_{it}^{L}(skill_{it} - skill_{It})\end{aligned} \tag{8}$$

Thus (8) allows for heterogeneous production, non-constant returns to scale, imperfect competition and capital mis-measurement/non-adjustment.

Finally, how do we account for unobserved plant-specific price indices? As emphasised by Griliches and Klette (1996), among others, we wish to measure $y_i = r_i - p_i$ where r_i is log plant revenue ($r_i = y_i + p_i$) and p_i a log plant deflator. In fact, we only have available to us deflators at the industry level p_I and hence we measure $r_i - p_I$ which equals $y_i + p_i - p_I$. Assume that demand for firm i can be written

$$\frac{Y_i}{Y_I} = \left(\frac{D_i}{D_I}\right)^{1/(\mu_I - 1)} \left(\frac{P_i}{P_I}\right)^{-\mu_I/(\mu_I - 1)} \tag{9}$$

where D_i, the extent of product differentiation, scales the share of industry output earned by the firm at a given relative price and μ is consistent with μ in (6).[20] Using this we can write plant-revenue that we observe in terms of deviations from industry averages,

$$(r_{it} - p_{It}) - (r_{It} - p_{It}) = \frac{1}{\mu_{It}}(y_{it} - y_{It}) + \frac{1}{\mu_{It}}(d_{it} - d_{It}) \tag{10}$$

Substituting (10) into (8) and rearranging gives:

$$(rtfp_{it} - rtfp_{It}) = \left(\frac{\overline{\eta}_{it} - \mu_{it}}{\mu_{It}}\right)(x_{it}^K - x_{It}^K) + \overline{s}_{it}^L(skill_{it} - skill_{It}) + \frac{1}{\mu_{It}}(a_{it} - a_{It}) + \frac{1}{\mu_{It}}(d_{it} - d_{It}) \tag{11}$$

where

$$(rtfp_{it} - rtfp_{It}) = ((r_{it} - p_{It}) - (r_{It} - p_{It})) - \left(\sum_{j=M,L} \overline{s}_{it}^j (x_{it}^j - x_{It}^j) + (1 - \overline{s}_{it}^L - \overline{s}_{it}^M)(x_{it}^K - x_{It}^K)\right) \tag{12}$$

where (12) says that the left hand side of (11) is the deviation from the industry mean of output less the share-weighted deviation from industry means of capital, material and labour inputs, but called *rtfp* to make clear that it is calculated from revenues.

3.1.1 Relation to previous literature

Equation (11) is the basis for our estimating equation and a number of points are worth making. First, as shown in (12), the left-hand side of (11) is the standard measure of TFP, namely output less the share-weighted inputs (except it is all in deviation from mean form) with the shares calculated as mean shares, an index enabling a proper comparison of TFP within-industries (see e.g. the discussion of TFP indices in Harrigan, 1999). Indeed with perfect competition and constant returns *($\mu=\eta=1$)*, and no skill

[20] The micro foundations of (9) are set out in e.g. Melitz (2001). All firms in an industry face an elasticity μ which means that they all earn a mark-up of μ but have different market shares according to their relative quality $D_i - D_I$. We comment on the functional form in note 22 below.

or quality differences, (11) reduces to the Caves et al (1982) measure of TFP for measuring technology differences.[21]

Second, it is often felt that TFP as a measure of technology differences is flawed under conditions of imperfect competition, since only under perfect competition do input factor shares equal input elasticities, see (6). Equation (11) shows this is indeed the case, since the last two terms show that differences in *rtfp* reflect differences in both *a* and *d*. However, it is worth noting that although imperfect competition throws a wedge between input factor shares and the input elasticity with respect to *y*, the unavailability of plant-level prices also creates a wedge between the observed output based on *r* and *y*. With the particular specification of demand, the two wedges give the simple specification in (11).[22]

Third, (11) gives four sources of possible difference in measured TFP between firms in the same industry. Moving term by term on the right hand side, the first difference arises from differences in capital, which disappear with perfect competition and constant returns $(\mu=\eta=1)$. The second relates to differences in skill which is standard when employment is measured in heads but consists of employees with different marginal productivity. The purpose of our paper is of course to measure skill. Finally, differences occur with differences in d, product quality. This arises from the fact that firm revenues are deflated by p_I and not p_i the wedge between which is, from (9), product quality.

Fourth, there is of course an extensive plant-level literature which regresses *y* on *k*, *m* and *l* and is concerned with biases to the resulting output elasticities, especially on *k*, (due to endogeneity and/or selection) see e.g. Olley and Pakes (1996), Levinsohn and Petrin (2003) and Griliches and Mairesse (1995) for a critique of some methods adopted. Olley and Pakes (1996) assume the industry produces a homogenous product and so the question of non-observable p_i does not arise.[23] They derive exit and investment conditions for the firm and concentrate on the endogeneity and selection problems in estimating a regression of *y* (value added) on *k* and *l*. As Griliches and Mairesse (1995, section 7) and

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[21] The (physical) capital term appears on the right-hand side of (11) interacted with terms in μ and η but not the human capital (skill) term since we assume the capital cost share is mismeasured but the labour cost share is not.

[22] Klette (1999) writes down (8), estimates it using r_{it}-p_{it} as dependent variable (to measure y_{it}) and inspects the coefficient on the summed share-weighted input term (the second term on the right-hand side of (8)) as a measure of μ. He finds it close to one for most Norwegian manufacturing sectors. Given that one cannot observe y_{it}, (11) and (12) show that one would expect a coefficient of 1 on this term even if $\mu\neq 1$. Klette's (1996) paper, which derives (11) without the skill term (see his equation 11, p. 507), concentrates on estimation of *a*. Katayama, Lu and Tybout (2003) point out that (9) is restrictive and propose estimating the production function jointly with a general form of demand. The specification, estimation and identification of an additional equation is of interest, but raises a number of fresh complications that take it beyond the scope of this current paper.

[23] "We assume the industry produces a homogenous product with Cobb-Douglas technology, and that the factors underlying profitability differences among firms are neutral efficiency differences" (Olley and Pakes, p.1273). Their precise assumption regarding μ is not clear. The homogenous product assumption typically is tantamount to $\mu=1$ (in the long run with constant returns, a standard Bertrand model with no product differentiation would predict that the lowest cost firm expands to meet all the demand and other firms exit). An alternative interpretation is that there are homogenous products but either short run varying μs due to a time it takes to exit (firms with high costs do not exit instantly since there are temporary shocks to costs as well as long run cost differences) or long run μs due to decreasing returns.

(11) suggests, even with these corrections such an equation is also likely to be biased if p_i is unavailable. Our analysis relaxes the Olley-Pakes assumption of a homogeneous product. Combined with the first-order conditions for inputs (l and m), we avoid these estimation issues to a great extent by having TFP as our dependent variable (essentially we avoid stage 1 of their procedure). Note this also has the considerable advantage of not imposing constant output elastiticies across all firms in the sample as the regression method does. Of course, k still appears as a regressor and is potentially biased in OLS, just as Olley and Pakes point out. This bias is not however our central interest here. In our robustness checks section we implement the Olley/Pakes procedure and find little impact on skills, so for the moment we shall ignore it and focus on the bias to the skill. [24]

Fifth, there is some literature which seeks to model a_{it} (Hicks neutral TFP) or its changes by, for example, R&D (Klette, 1996). We do not have R&D data in our panel and so use fixed effects to partially control for a_{it} . We remark on the possible sources of biases below.

3.1.2 Measuring skill in (11)

There are a number of different ways of representing QL in (3) and hence skill in (11) depending on what skill variable is available. Suppose first one has a measure of the average schooling level at the plant, ED_{it}. Thus QL can be written

$$QL = \exp\{\phi ED\} L \tag{13}$$

where L is total employment at the firm and ϕ converts a year of schooling into the quality of labour. This form is popular in the cross-country growth literature where one has data on the average schooling level of a country and it ensures that the log of human capital is related to the level of schooling, which fits the Mincer earnings regression form (see and Krueger and Lindahl (2000), for example).[25] Thus (11) becomes

[24] It is worth noting the major source of bias that Olley and Pakes (1996) find. Looking at Table VI of Olley and Pakes (1996), in their data the bulk of the bias to l and k occurs from selection induced either from using a balanced panel of surviving firms or a within estimator (which implicitly drops firms in the period they exit). Cross-section estimates of y on l, k, age and time (see their column 3) are very similar to their selection and endogeneity corrected estimates (see columns 6-8). Below we find also our skill term is robust to the Olley/Pakes procedure (as do Hellerstein and Neumark, 2004).

[25] Note that in this literature the functional form is crucial: regressing log productivity on log schooling typically produces an insignificant coefficient on log schooling, whereas regressing log productivity on the level schooling typically produces a significant coefficient.

$$(rtfp_{it} - rtfp_{It}) = \left(\frac{\overline{\eta}_{it} - \mu_{It}}{\mu_{It}} \right)(x_{it}^K - x_{It}^K) + \overline{s}_{it}^L \phi(ED_{it} - ED_{It}) + \frac{1}{\mu_{It}}(a_{it} - a_{It}) + \frac{1}{\mu_{It}}(d_{it} - d_{It}) \tag{14}$$

where ϕ is the average marginal return to schooling across workers of different schooling levels in the firm and $(ED_{it} - ED_{It})$ deviation of firm average education from the industry level.

Suppose second that one has data on proportions of workers with different schooling or skill levels. Hellerstein, Neumark and Troske (1999) and Jones (2001) write *QL* as,

$$QL = L_0 + \sum_{p=1}^{n} (\sigma_p + 1) L_p \tag{15}$$

where there are n different worker types and (σ_p+1) is the marginal product of worker *p* relative to the least skilled worker *0*. Note that this additive formula assumes, like the average schooling formulation, that workers of different schooling types are infinitely substitutable (Appendix 2 shows the case with a more general functional form). Using $L=L_0+\Sigma L_p$ and $SKILL=QL/L$, substitution into (11) gives

$$(rtfp_{it} - rtfp_{It}) = \left(\frac{\overline{\eta}_{it} - \mu_{It}}{\mu_{It}} \right)(x_{it}^K - x_{It}^K) + \overline{s}_{it}^L \left[\sum_{p=1}^{n} \sigma_p \left((L_p / L)_{it} - (L_p / L)_{It} \right) \right] + \frac{1}{\mu_{It}}(a_{it} - a_{It}) + \frac{1}{\mu_{It}}(d_{it} - d_{It}) \tag{16}$$

3.2 Estimating the relation between skills and TFP

This section discusses how we estimate (14) or (16). The next section considers IV estimates. Let us first consider some generic problems in estimating (14) or (16). First, concerning μ_I and η_{it} we do not have enough observations to estimate industry by industry allowing different coefficients per industry and hence we have to assume that they are constant across industries and time. Thus we write an amendment of (14) namely

$$(rtfp_{it} - rtfp_{It}) = \alpha_1(x_{it}^K - x_{It}^K) + \alpha_2(\overline{s}_{it}^L(ED_{it} - ED_{It}) + \alpha_3(a_{it} - a_{It}) + \alpha_4(d_{it} - d_{It}) + \varepsilon_{it} \tag{17}$$

where the α_i are parameters to be estimated and we have relegated to the equation error, ε_{it}, $\left((\eta_{it}/\mu_{It})-(\eta_t/\mu_t)\right)(x_{it}^K - x_{It}^K)$. Thus there is a bias to our term of interest, $\hat{\alpha}_2$ to the extent that the within-industry share-weighted education difference is correlated with the between-industry ratio of plant-specific returns to scale to industry mark-ups difference. This correlation of within-industry differences to between-industry differences is very hard to sign a priori

Second, an additional problem is that in what follows we do not have an explicit measure of (a_{it}- a_{It}) (we do have a measure on the ESS of (d_{it} –d_{It}) but find little effect on $\hat{\alpha}_2$ so we omit it explicitly from the current discussion). Thus $\hat{\alpha}_2$ is biased if there is a correlation between $\overline{s}_i^L(ED_i - ED_I)$ and the omitted (a_{it}- a_{It}) i.e. if technology differences, relative to the industry mean, are correlated with the skill mix.[26] If (a_{it}- a_{It}) is fixed over time, fixed effects would solve this bias, but to the extent that it does not, we discuss the likely biases here.

To analyse the possible correlation between $\overline{s}_i^L(ED_i - ED_I)$ and (a_{it}- a_{It}), consider the determinants of $(ED_i - ED_I)$. The first order conditions for choice of skills would suggest that a firm has a different skill mix relative to the industry if it faces (a) a different relative wage or (b) if its skill mix has a different relative productivity. The latter might be due to e.g. different levels of skill-biased technology from an unobservable input that both raises *(a_{it}- a_{It})* and the relative demand for skilled labour (computers or organisational capital are two examples often cited in the literature). What are the directions of bias? Concerning computers, to the extent that computer use is captured in the capital measure, this will be accounted for. To the extent that it is not, it depends on the way in which computers bias demand towards skilled labour. Measured TFP is a combination of Hicks neutral and biased parameters (Berndt and Wood, 1982)[27]. Following Johnson (1997), if computers are an intensive skill-biased technology they raise skilled demand leaving unskilled demand unaffected and so raise measured TFP, inducing a positive correlation between $\overline{s}_i^L(ED_i - ED_I)$ and (a_{it}- a_{It}). As Johnson (1997) argues however, the apparent acceleration in skill-biased technical progress in the 1980s and 1990s did not correspond to an acceleration in TFP which he suggests is consistent with extensive skill-biased technical change[28] that would affect $\overline{s}_i^L(ED_i - ED_I)$ but, being overall neutral to TFP, would not affect (a_{it}- a_{It}) meaning no bias.

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[26] Note that we can rule out cross-industry effects e.g. aerospace is more productive than pencils due to better technology and also employs more skilled workers.

[27] Except in simple functional forms such as Cobb-Douglas where, due to its multiplicative structures, biased and neutral parameters are not separately identified. In translog for example, lnTFP is a share-weighted average of the biased technology parameters.

[28] Which is technical change that makes skilled workers better at tasks previously performed by the unskilled (e.g. typing a paper) and hence does not necessarily raise TFP since it raises the productivity of the skilled at the expense of lowering that of the unskilled.

Regarding organisational change, Brenahan et al (2002) has argued that TFP increases come with organisational change, which raises both $\overline{s}_i^L(ED_i - ED_I)$ and (a_{it}- a_{It}). To the extent that such firms employ better paid or qualified workers, $\hat{\alpha}_2$ overstates the causal impact of our skill measures on productivity (since it picks up part of the better organisational capital of firms). Note however that it is also suggested that during the transition to higher TFP the necessary investment in organisational change might lower measured TFP as workers learn about the new organisation etc. Thus $\hat{\alpha}_2$ would be downward biased, as high skilled firms have temporarily lower TFP.

Overall this discussion suggests the following. First, biases to $\hat{\alpha}_2$ arise only to the extent that omitted variables affecting to skill mixes also affect TFP which may or may not be the case. Second, $\hat{\alpha}_2$ is likely biased upwards if higher skilled firms have also higher TFP due to other unmeasured factors, but it might be biased downwards if such higher skilled firms have temporarily lower TFP as they re-organise production.[29]

4 Regression Results

4.1 Estimating (17) on the ESS

Table 3 sets out regression estimates of (17) by OLS for our cross-section (which implicitly assumes that a_i and d_i are randomly distributed around their industry average, i.e. captured in the *iid* error) omitting measures of *d* but using different skill measures. The top row shows the coefficient on $(X_{it}^K - X_{It}^K)$, the deviation of capital from its four-digit industry mean, which is negative and significant. Recalling that the coefficient is $(\eta-\mu)/\mu$, the coefficient of around –0.03 is consistent with constant returns $(\eta=1)$ and $\mu=1.03$, or $\mu=1$ and slightly decreasing returns $(\eta=0.98)$.

Turning to our main variables of interest, columns 1 and 2 show the skill variable as defined by school years and the qualifications index (each, as in all the variables are deviations from the 4-digit industry mean). Both are positive, with the second statistically significant. Column 3 enters the share of skill levels 1 to 4 (the share of workers with no or other qualifications are omitted). All coefficients are

[29] There is of course an extensive literature on X-inefficiency and productivity. Omitting measures of X-inefficiency, or its determinants, such as competition, causes a bias but only to the extent that competition causes differences in *relative* employment of skilled and unskilled workers. A priori there is no particular reason to suppose this is the case. Finally, note that if we measure skill by qualifications, and higher ability results in higher qualifications, $\hat{\alpha}_2$ overstates the causal effect of *qualifications* on productivity since it is driven by ability. It is however a sufficient statistic for the average effect of ability and the productivity enhancing effect of qualifications via formal qualifications.

positively signed, with ascending returns but only levels 3 and 4 are significant. Columns 4 and 5 explore different combinations of the level 3 and 4 terms.

Equation (17) also includes measures of product quality as discussed in section 3 (recall from equation 17 that $(rtfp_{it}\text{-}rtfp_{It})$ includes a term in $(d_{it} - d_{It})$ which might be measured by product quality). The ESS asks firms to rate their product quality in terms of three measures, all relative to the industry and consist of a categorical rating of the whether the product is a volume or specialised, how the quality of the product is rated (low to high relative to the industry) and complexity (not complex to complex). We added these variables to those in Table 3 but the coefficients on skills were not greatly altered.

What of the magnitudes of the skill coefficients in Table 3? First, if one uses the coefficients as estimates of marginal returns that are comparable to wage equations, column 4 suggests returns to level 3 and 4 qualifications of 54% and 67% respectively, which compares well with Sianesi's (2003) estimated returns from wage equations of 43% and 69% to A levels and a degree.

Second, what fraction of variation in productivity can variation in skills account for according to these estimates? The TFP gap between plants in the top and bottom deciles is as in Table 2, namely (0.375-(-0.337))=0.712 points. Consider the share of workers with A levels or more, from column 5 of Table 3, who have a coefficient of 0.566. The fraction of the TFP gap accounted for is thus 0.566 times the gap between $\overline{s}_i^L(ED_i - ED_I)$ for the top and bottom decile (which is 0.074, Table 2, panel 1c, column 8). Thus the fraction of the TFP gap is (0.566*0.074)/(0.712)=6.0%. This is very close to 4% from the numbers using the wage-based returns to schooling.

4.1.1 Estimating (17) on the NES by OLS and fixed effects

Table 4 sets out regression estimates of (17) starting with OLS. In column 1, the capital term is insignificant, but the NES skill term is well specified. Column 2 splits up the skill measure into the person and experience effect and suggests that the person effect is more significant. To preview our fixed effects results, column 3 is an OLS regression for plants who appear at least twice in the dataset (since a fixed effects regression discards variation from plants only appearing once) and column 4 for those appearing every period (six times); the skill terms are both significant, with the point estimate being higher in column 4. Thus these simple OLS regressions support the idea that skill is statistically significantly correlated with TFP (again all in terms of deviations from four-digit industry means).

Column 5 estimates (17) including plant fixed effects, which implicitly assumes that the deviation of a_i and d_i from their four-digit industry average deviations has a fixed and randomly distributed component and all the other terms can be thought of as deviations from the average deviation from their four-digit industry average. The skill term is still significant, with point estimate slightly higher than in columns 1 and 3. Finally, column 6 sets out a fixed effect regression with skill separated

into person and experience effects; neither term is well determined, but the person effect is slightly more precisely estimated. Note too that the capital term is well determined in columns 5 and 6; if μ=1, η=0.95 (slightly decreasing returns), if η=1, μ=1.05 (an average mark-up of 5%).

A number of points are worth making regarding these results. First, let us again calculate the fraction of variation in lnTFP that variation in this skills measure accounts for. Consider column 1, Table 3. The TFP gap between the top and bottom deciles is as in Table 2, namely (0.274-(-0.347))=0.621 points. The share of skills that accounts for this is the coefficient, 0.243, times the gap between $\overline{s}_i^L(ED_i - ED_I)$ for the top and bottom decile (which is 0.051, Table 2, panel 2c). Thus the fraction of the gap is (0.243*0.051)/(0.62)=2.0%. Thus the fraction of productivity accounted for by skills, on this measure, is only slightly lower than that using the ESS. If we use the estimates from columns 3 and 4 we get 1.9% and 3.0% respectively (since the samples are different the gaps are different as well the coefficients). If we use the FE estimate in column 5 we also obtain 3.0%. Thus the fraction of productivity accounted for by skills, is (a) of a similar magnitude regardless of estimating method and (b) similar to that found with the ESS.

Second, why does the fixed effect coefficient exceed the OLS coefficient? It must be noted at the outset that there is in fact no significant statistical difference between them.[30] However, the point estimate does rise, just as it rises in the OLS results as one estimates on plants that survive for longer (compare columns 3 and 4, the coefficient on plants appearing at least twice is 0.330 (t=4.34)). Analysis of the underlying data shows that plants who survive longer have progressively higher TFP and higher skills.[31] Thus it could be that the main impact of fixed effects is to raise the marginal impact of skills because of the induced selection of longer-lived plants. Such plants might have a higher impact because, for example, longer-established firms have built up the organisational capital enabling them to get a larger marginal impact on TFP from skill. But it must be remembered that the effects are not statistically different and the share of TFP variance accounted for is still very similar.

5 Robustness checks, comparison with other studies and IV estimates

So far we have found, using both data sets a statistically significant effect of skills on productivity that explains about 2-6% of the lnTFP gap between plants in the top and bottom deciles of the lnTFP distribution. Before moving to some IV results, we carried out a number of tests of the robustness of these initial conclusions. These are all set out in table 5 which starts in column 1 by repeating as a memo item the coefficient from Table 4, column 5.

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[30] A Hausmann test for a significant difference between the coefficients in columns 5 and 1 is $\chi^2(1)$=1.47 (difference 0.12 with standard error 0.099).

[31] The exception is that plants who are only in the data once have relatively high TFP and but low skills which could be due to the perhaps special circumstances of start-ups.

First, we weighted the NES observations by the share of workers observed in the plant (mean 1.4%, standard deviation 1.6%), since that might be correlated with measurement error in plant-based human capital. Column 2 shows that this gave a coefficient on skill of 0.218 (t=236), similar to our original estimate. Second, we varied the cut off point for the number of workers matched to each ABI plant from 3 to 10 giving coefficients, t-statistics and sample sizes of 0.116(t=3.22, N=2,539) and 0.151 (t=1.83, N=761), see column 3 and 4. Third, we re-estimated the equation by regressing log gross output on log employment, log capital, log materials and the skill index (not pre-multiplied by the labour share) and a set of 4-digit dummies; the coefficient on the NES skill index was 0.098, see column 5 (for the ESS index the coefficient ESS index 0.202 (t=3.27)). Both these are somewhat lower than the deviation-based indices but recall that these estimates impose equal output elasticities in all firms. Fourth, column 6 shows robustness to dropping the capital term with the coefficient in skills rising to 0.507 (t=2.93) (our ESS results here were a coefficient of 0.249 (t=3.45)).

Fifth, TFP data is notoriously noisy. We therefore averaged all variables across all observations for each plant and re-estimated the NES results on this resulting cross-section (of 416 plant-averages). As column 7 shows, the coefficient is 0.613 (t=3.22) which is somewhat higher than before and this raises the share of the spread to 8.8%. A higher coefficient is consistent with attenuation bias in our previous estimates due to measurement error.

Sixth, column 8 reports on an Olley/Pakes type estimate where we include squares in the capital and skill terms and interactions of all levels and squares as additional regressors. The coefficient on skill is hardly affected.

Finally, we report on a number of other checks not set out in the table. First, we repeated our ESS estimates on the large sample that includes some reporting units for whom we do not have complete workforce data: the results were not in fact much altered, but somewhat noisier. Second, to calculate industry TFP we only used industries with positive capital shares. For firm TFP we allowed firm capital shares to be negative, since the method is supposed to be robust to mis-measured capital. Restricting firm capital shares to be positive did not greatly change results. Third, in case firm-based shares are error ridden, we replaced the shares for all firms with their industry shares, rather than the average industry and firm shares as theory suggests. The results were very similar, as they were when we smoothed the data by averaging firms over 1994-6 and 1997-2000 and then calculating relative TFP and skills and re-computing the data relative to their three digit industry.[32]

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[32] The last two sets of checks were for the NES. Finally, it might be asked whether OLS estimation is suitable with a matched data set. Hildreth and Pudney (1999) find that that (cross section wage) OLS estimates are very close to estimates correcting for the non-normalities arising from the creation of matched data. We note too that we have not corrected the standard errors on the skills variable due to the fact that skills is a generated variable (though we do use heteroscedastic-consistent White standard errors). This follows Abowd, Kramarz, Margolis and Troske (2001, note 7) but see Abowd, Kramarz and Margolis (1999) for discussion. We cannot directly use conventional correction methods e.g. Murphy and Topel (2002) since the generated regressor is not one but an average of the h_{pt}

From the robustness checks done we conclude that both the statistical significance of the *SKILL* term and the share of the productivity spread that skills contributes to are robust to different specifications.

5.1 Comparison with other studies of productivity and human capital wage-based measures

For the US Abowd et al (2002) report a one-standard deviation in their wage-based skill index, which is the fraction of workers with wage-based human capital above the economy-wide median, accounts for a 11 log point change in manufacturing labour productivity (which itself has as standard deviation of 89 log points), controlling for capital intensity and industry effects for 1997 (see their table 5.1 column 4 and 5.2, column E).[33] We re-ran our specification to match theirs and obtained a predicted one-standard deviation skills of 16 log points with manufacturing labour productivity having a standard deviation of 72 log points. These numbers are remarkably similar. In Abowd, Kramarz and Margolis (1999, Table IX and X, pp.297-8), on French data, the standard deviation of the average person effect per firm is 0.66 per firm. With a coefficient of 0.22 on the person effect in a regression of log value added per employee a one-standard deviation change is 0.15 (the standard deviation of log value added per employee is 1.11). These numbers are again very similar.

5.2 IV: the causal effects of skills

Whilst we have been able to control for some unobservables, endogeneity of $(ED_i - ED_I)$ remains a concern. A valid instrument would help establish the causal effect of skills on TFP (for the group of workers or firms to whom the instrument relates). We require an instrument that is correlated with the skill intensity variable, but uncorrelated with the unmeasured technology/intangible or quality shocks. This suggests using shocks to labour supply, since it is likely to be too hard to identify convincingly skill-neutral shocks to labour demand. An ideal natural experiment then is to randomly supply workers of different skill levels to different firms. One possible way to get close to this experiment might be via skill shortage data. Firms faced exogenously with different levels of skill shortages would under otherwise similar circumstances employ exogenously different levels of skilled workers. Under these assumptions, the variation in skill intensity due to variation in skill shortages

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variables and unlike the other employee/employer papers it is only for a subset of workers. The direction of bias is not clear, but note that if our standard errors are too small so that the "true" effect of skills is zero, this would be in line with the overall "small" impact of skills on explaining the productivity spread.

would constitute an exogenous source of variation, since shortages would be correlated with the skill proportion variable, but uncorrelated with the equation error.[34] The ESS also asks plants whether they have hard-to-fill vacancies and this can be used to construct an instrument.

There are two obvious initial problems with this strategy. The first is that skill shortages are likely to vary endogenously by industry. This could be due to industry-specific (biased) productivity variation that causes both cross-sectional differences in skill mixes and also TFP (for example differences in technology between the aircraft and pencil industry might cause aircraft to be more skill intensive and have higher TFP). Thus we must remove industry variation from the instrument.

The second problem is that shortages are likely to vary endogenously by firm. Whilst it might be the case that some regions or industries for example face shortages for exogenous reasons (e.g. poor quality of local skills, the arrival of a large rival firm demanding many workers etc.), firms might face shortages due to a choice, for example by paying below the market wage. If firms with good productivity shocks paid higher wages and so experienced lower shortages for example, then the instrument would be correlated with the equation error. Thus using firm-level variation in skill shortages also seems inadmissible.

Since we cannot use industry or firm variation, we therefore decided to use, as an instrument, geographic variation in skill shortages net of industry means. To do this, we first calculated two terms: the fraction of firms, by three-digit industry and travel to work area reporting (a) any hard-to-fill vacancies and (b) skilled related hard-to-fill vacancies. Our two instruments are these two variables less the fraction in the industry so reporting (all multiplied by the plant-industry labour share average). In sum, we are using geographical variation within industries to instrument for between-plant variation in the skill inputs within industries. Hence we attempt to identify the causal effect of skills on productivity using that part of the variation in skills within industries due to variation in geographical skill shortages within industries.

Another problem is of course that this instrument is invalid if it is correlated with the omitted technology shocks that we think might be the source of the problem. For this, we need to assume that geographical variation within industries in shortages is not correlated with geographical variation within industries in productivity shocks. It seems plausible that industries have similar productivity shocks.

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[33] Their table 5.1 shows, for manufacturing, a standard deviation of log labour productivity (gross output per worker) of 0.888 and of the human capital measure of 0.225. The coefficient on human capital is 0.512, table 5.2, column E, controlling for capital intensity and two digit industry dummies.

[34] What is the interpretation of a skill or labour shortage in an equilibrium economy? If hiring is via a matching process firms wait in equilibrium for vacancies to be filled. Thus a firm reporting a labour shortage is either reporting that it takes time to fill vacancies or that there is a longer than expected waiting time.

However, it seems somewhat less plausible that these shocks vary systematically by region within the particular industry.

Another objection to the instrument might be that location is endogenous. What would the bias be in this case? If plants with a high a_{it} shock re-locate in response to high skill shortages to areas of low shortages this creates a correlation in the data between low geographic shortages and high plant TFP. This would cause the IV estimate to be upward biased, since our assumed effect is that low shortages raise skill and so give high plant TFP. Note that this would be the case if adjustments to location occur faster than adjustments to skill. This seems unlikely for incumbent firms but might affect start-ups. Since start-ups have a high failure rate however, they typically are not dominant in our data.

Two further points about our instruments must be noted. First, they must be correlated with $(ED_i - ED_I)$ and so we shall test for this. Second, whilst valid IV estimation returns a causal estimate it does of course return an estimate of the marginal effect of skills driven by the particular variation of the instrument. Deviations of skill shortages from their industry means might be driven for example by critical workers, in which case we might expect IV estimates to be higher than OLS.[35]

Table 6 reports our IV results for the ESS and the NES. Columns 1 and 2 use the ESS where the skill regressor is the fraction of workers with levels 3 and 4 qualifications, corresponding to column 5 in Table 3 (where the coefficient was 0.566). The instruments are in turn the share of hard-to-fill vacancies in column 1 and the share of skilled hard-to-fill vacancies in column 2. The latter is larger and somewhat more significant than the former, though both imprecisely estimated. The fraction of TFP difference between plants in the top and bottom deciles accounted for by skills in column 4 is 8.6% ((0.815*0.074)//(0.712)), only slightly higher than our least squares estimates. Note the penultimate row that reports an F test of the significance of the instrument in a regression of $(ED_i - ED_I)$ on *lnK* and the instruments. This is suggested by Staiger and Stock (1997) as a check on weak instruments: in both cases the F test is significant (although both are under 10, but note that we use only one instrument).

The remaining columns use the NES. Columns 3 shows the OLS regression on the 803 plant observations where the instrument is available (the shortage data is only for England not Scotland and Wales), giving a coefficient on $\overline{s}_i^L(ED_i - ED_I)$ of 0.333. Columns 4 and 5 show the IV estimates (again weak instruments are not a problem). Interestingly, once again the IV coefficients rise quite sharply; (and are significantly different from zero); around 3½ times the coefficients from column 7. The fraction of TFP variance accounted for is 11% and 13% respectively (1.133*0.068/0.67 and 1.299*0.068/0.67).

Finally, we explored the effects on the averaged data, to minimise attenuation bias due to measurement error in the data. Here an IV regression (not reported in Table 6), using the share of skilled vacancies as the instrument returned a coefficient of 1.70 (t=1.99) on 270 observations giving a share

[35] Our instruments are only available for 2001 and thus they are assumed to be the same across all years of the NES.

(calculated on these 270 observations) of 25% (1.70*0.023/0.236). This increased figure is in line with the two effects discussed above i.e. the increase in the coefficient when averaging and with IV. Since, however, as we discussed above higher IV estimates may be upwardly biased, we regard these numbers with some caution.

6 Conclusions

In relation to the three questions posed in the introduction we find the following: first, we document that more productive firms do indeed hire more skilled workers. In our sample of manufacturing business for the year 2000, plants in the top decile of the productivity distribution hired workers with, on average, around 1/3rd of a year of extra schooling relative to plants in the bottom decile (controlling for four-digit industry).

Second, we find that both "hard" skill measures (based on qualifications) and "hard and soft" skill measures (based on double-fixed effect wage regressions) are significantly associated with productivity.

Third, we estimate that the skills gap between firms in the top and bottom deciles in the productivity distribution explains, in an accounting sense, for about 3-10% of the TFP gap. Our IV results returned somewhat higher estimates, but may be upwardly biased. In addition, we find the similarity of this figure to micro studies using the same method for France and the US reassuring.[36]

There are a number of caveats regarding our work that we wish to explore in future. First, we would like to control better for unmeasured technology, perhaps using a technology survey (although the sample sizes on a three-way match between the business census, technology and skills surveys may be too small). Second, we have not looked at whether level of skills raises investment or productivity growth/innovation (our estimates here are for the effect of skills on TFP i.e. for given capital and for the level of skills on the level of productivity). Third, our estimates are for manufacturing; at least some parts of the service sector may well rely more on skills than manufacturing and this would be interesting to investigate.

[36] The larger fraction of the TFP gap accounted for at the cross-country level might be due to education externalities (e.g. firm's productivity in given areas being boosted by education in the region).

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Table 1: Qualification questions for ESS firms

Qualification	(1) Years of education (imputed)	(2) Skill index (allocated)	(3) Skill level
Higher level of qualification such as degree or equivalent (e.g. NVQ level 4/ Nursing/ HND/ HNC/ Higher diploma)	17	6	4
Intermediate level of qualification such as A levels or equivalent (e.g. NVQ level 3/ BTEC National/ /OND/ City and Guilds Advanced Craft)	14	5	3
Basic level of qualification such as G.C.S.Es or equivalent (NVQ level 2/ O levels/ BTEC first or general diploma/ Intermediate GNVQ/ City and Guilds Craft)	12	4	2
Lower level of qualification such as NVQ Level 1 or equivalent (BTEC first or general certificate/ basic vocational training/ RSA/ Foundation GNVQ)	11	3	1
Other qualifications (SPECIFY)	11	2	Other
None	10	1	None

Notes to table: (i) firms are asked to specify the typical qualification, out of the above list for the 9 occupational groups they have specified, the 9 groups being occupations are managers, professions, associates, administrators, skilled manual, personal, sales, machine operatives and elementary occupations. (ii) The years and number allocated columns are the years of education and index number we have allocated to each qualification level, see text. For the relation between academic and NVQs (non-vocational qualifications) see <http://www.dfes.gov.uk/nvq/what.shtml >.

Table 2: Basic data for ESS and NES matched plants and reporting units

Panel 1. ABI/ESS (292 plants)

	(1) Employ-ment	(2) Skill (years of educ.)	(3) Skill (qualif. index)	(4) Share level 1	(5) Share level 2	(6) Share level 3	(7) Share level 4	(8) Share 3&4	(9) Share other qualif.	(10) Share no qualif.	(11) Relative TFP
a.all plants											
Mean	161	12.3	3.94	0.112	0.383	0.199	0.159	0.358	0.022	0.078	
SD	168	2.55	1.19	0.249	0.35	0.26	0.207	0.467	0.128	0.222	
b. deviation from four digit industry mean, by TFP decile (30 plants in each decile)											
Below 10% TFP		-0.103	-0.241	0.022	0.071	-0.050	-0.048	-0.097	0.000	-0.005	-0.337
Above 90% TFP		0.201	0.368	0.024	-0.094	0.088	0.020	0.107	0.000	0.000	0.375
c. as b, multiplied by labour share											
Below 10% TFP		-0.143	-0.041	0.002	0.034	-0.019	-0.018	-0.037	0.000	-0.001	
Above 90% TFP		0.087	0.054	0.006	-0.033	0.035	0.002	0.037	0.000	0.000	

Panel 2. ABI/NES (1187 reporting units, with at least 7 workers matched)

	Employment	Human capital	Individual fixed effects component	Experience component	TFP
a. all reporting units					
Mean	1,679	3.1	0.008	3.1	
SD	2,403	0.52	0.63	0.12	
b. deviation from four digit industry mean, by TFP decile (105 reporting units in each decile)					
Below 10% TFP		-0.165	-0.158	-0.007	-0.347
Above 90% TFP		0.105	0.106	-0.001	0.274
c. as b, multiplied by labour share					
Below 10% TFP		-0.042	-0.040	-0.001	
Above 90% TFP		0.001	0.001	0.000	

Notes to table: (i) labour share is average of plant and four digit wages and salaries share of gross output (ii) TFP is in logs, calculated as equation (12); deviation of plant output from four digit industry mean output less share-weighted deviations of labour, capital and material inputs from four-digit industry mean inputs.
Source: authors' calculations using ABI, ESS and NES.

Table 3: Estimates of (17) using skill measures from ESS

(dependent variable $rtfp_i$-$rtfp_I$, deviation of logTFP from plant at the 4 digit industry mean; all independent variables are deviations from 4-digit industry means multiplied by plant-industry average labour shares)

Deviation from 4-digit industry mean of:	(1)	(2)	(3)	(4)	(5)
lnK_i	-0.025	-0.026	-0.028	-0.027	-0.027
	(2.03)	(2.14)	(2.41)	(2.27)	(2.20)
School years	0.042				
	(1.44)				
Qualif index		0.114			
		(2.12)			
Level 1			0.224		
			(0.80)		
Level 2			0.176		
			(0.86)		
Level 3			0.670	0.535	
			(2.85)	(2.96)	
Level 4			0.821	0.671	
			(2.23)	(2.16)	
Level 3& 4					0.566
					(3.34)
Observations	292	292	292	292	292
R-squared	0.04	0.04	0.07	0.07	0.07

Notes to table: Robust *t* statistics in parentheses, estimation by OLS.

Table 4: Estimates of (17) using skill measures from NES

(dependent variable: $rtfp_i$-$rtfp_I$ deviation of lnTFP from plant at the 4 digit industry mean; all independent variables are deviations from 4-digit industry means, multiplied by plant-industry average labour shares)

Deviation from 4-digit industry mean of:	(1)	(2)	(3)	(4)	(5)	(6)
			Appear >1	Appear >5	Fixed effects	Fixed effects
$\ln K_i$	0.003	0.004	0.004	-0.017	-0.047	-0.045
	(0.40)	(0.55)	(0.49)	(0.62)	(4.07)	(3.91)
NES skill	0.243		0.239	0.621	0.362	
	(3.26)		(3.09)	(2.24)	(2.93)	
Separated into:						
Person effect		0.159				0.100
		(2.80)				(1.57)
Experience component		0.378				-0.056
		(1.67)				(0.28)
Observations	1187	1187	1091	144	1187	1186
R-squared	0.01	0.01	0.01	0.04		

Notes to table: Robust t statistics in parentheses. Columns 3 to 4 select firms that appear more than once and in all periods respectively. Columns 1 to 4 include a constant, not reported. F test that all $u_i=0$, i.e. that fixed effects are not statistically significant is F(415, 769) = 3.95 in column 5 and F(414, 768) = 3.93 in column 6. Columns 1-4 estimated by OLS, 5-6 by LSDV.

Table 5: Robustness checks on skill term in Table 4

	1	2	3	4	5	6	7	8
Deviation from 4-digit industry mean of:	Table 4, col 5	Weighted	>3 workers matched	>10 workers matched	Cobb-Doug	No capital	Averaged data	Olley/Pakes
NES skill	0.243	0.218	0.116	0.151	0.098	0.507	0.613	0.239
	(3.26)	(2.36)	(3.22)	(1.83)	(3.47)	(2.93)	(3.22)	(3.27)
Observations	1,187	1,187	2,539	761	1,187	1,187	416	1,187

Notes to table: Table reports coefficient on NES skill term only in different versions of Table 4, column 1. All regressions include capital term, except column 6. All estimates are by OLS except column 8 which is by IV. Column 8 includes as additional regressors squares in capital and skill and interactions of all levels and squares. See text for details on each regression.

Table 6: IV estimates of (17) using skill measures from ESS and NES

(dependent variable: $rtfp_i$-$rtfp_I$, deviation of logTFP from plant at the 4 digit industry mean; all independent variables are deviations from 4-digit industry means, multiplied by plant-industry average labour shares)

Regressors (devs from four-digit means)	ESS		NES		
	(1) IV-all HTF vacs	(2) IV-skilled HTF vacs	(3) OLS on IV sample	(4) IV-all HTF vacs	(5) IV-skilled HTF vacs
Level 3 &4	0.387	0.815			
	(0.35)	(1.06)			
Human capital			0.333	1.133	1.299
			(3.98)	(2.44)	(2.32)
LnK	-0.030	-0.033	-0.009	-0.023	-0.026
	(2.46)	(3.05)	(0.92)	(1.81)	(1.83)
Stage 1 regression F test row	F(1, 270) =4.14 Prob > F = 0.0429	F(1, 270) =8.29 Prob > F = 0.0043		F(1,801)=22.61 Prob > F = 0.0000	F(1, 801) =12.01 Prob > F = 0.0006
Observations	272	272	803	803	803

Notes to table: robust *t* statistics in parentheses. Instruments denoted in column headings; they are fraction of plants in the region *and* industry reporting hard-to-fill vacancies and skilled hard-to-fill vacancies less fraction of plants in the industry reporting hard-to-fill vacancies and skilled hard-to-fill vacancies. *F* statistics and *P* values shown in the stage 1 regression *F* test row are the *F* statistic and *p* value for the significance of the instrument at the top of the column in a regression of the endogenous skill variable (deviation from industry mean) on *lnK* (deviation from mean) and the instrument.

Appendix 1. ESS questionnaire on skills

This is the questionnaire asked to firms from which we construct our ESS skills measure.

A1 I'd like to ask you to break down your workforce into nine specific categories. These categories are… [LIST CATEGORIES WITH EGs]
Would you like to record staff details as a percentage or as actual numbers of staff?
Approximately, what proportion of staff at this establishment are employed as/How many of your staff are employed as… ?
READ OUT

Managers and senior officials e.g. directors, senior government officials, senior police officers	_____%
Professional occupations e.g. professional engineers, scientists, accountants, teachers, solicitors, architects, librarians	_____%
Associate Professional and technical occupations e.g. laboratory technicians, junior police officers, design and media professionals, nurses, artists	_____%
Administrative and secretarial occupations e.g. clerks, computer operators, secretaries, telephonists	_____%
Skilled trades occupations e.g. fitters, electricians, farmers, computer engineers, bricklayers	_____%
Personal service occupations e.g. catering staff, hairdressers, domestic staff, caretakers	_____%
Sales and customer service occupations Till operators, telesales staff, call centre staff, market traders	_____%
Process, plant and machine operatives e.g. machine operators, drivers, scaffolders, assembly line workers	_____%
Elementary occupations e.g. labourers, cleaners, security guards, postal workers, bar staff, shelf fillers, waiters	_____%
	100%

FOR EACH OCCUPATION GROUP MENTIONED AT QD1

D1a Thinking about your current workforce, what is the most common level of qualification amongst your ….(OCCUPATION AT QD1) ?
PROMPT IF NECESSARY. Would you say that they typically have ….?
READ OUT. SINGLE CODE ONLY

Higher level of qualification such as degree or equivalent (e.g. NVQ level 4/ Nursing/ HND/ HNC/ Higher diploma)									
Intermediate level of qualification such as A levels or equivalent (e.g. NVQ level 3/ BTEC National/ /OND/ City and Guilds Advanced Craft)									
Basic level of qualification such as G.C.S.Es or equivalent (NVQ level 2/ O levels/ BTEC first or general diploma/ Intermediate GNVQ/ City and Guilds Craft)									
Lower level of qualification such as NVQ Level 1 or equivalent (BTEC first or general certificate/ basic vocational training/ RSA/ Foundation GNVQ)									
Other qualifications (SPECIFY)									
None									

Appendix 2: Using a different functional form for QL.

A functional form that relaxes the assumed infinitely elastic substitution between efficiency units (here measured by schooling) of workers within the firm is

$$QL = \prod_{p=1}^{n} L_p^{\delta_p} \tag{18}$$

for n classes of labour at the firm, where $\gamma\delta_p$ is the output elasticity of type *p* workers. Substitution into (11) gives

$$\widehat{rtfp}_{it} = \left(\frac{\overline{\eta}_{it} - \mu_{It}}{\mu_{It}} \right) \hat{x}_{it}^{K} + \overline{s}_{it}^{L} \left[\sum_{i=1}^{n} (\delta_i - \delta_0) \frac{\widehat{L_i}}{L} \right]_{it} + \frac{1}{\mu_{It}} (\hat{a}_{it}) + \frac{1}{\mu_{It}} (\hat{d}_{it}) \tag{19}$$

where a "hat" indicates a deviation from an industry mean and the coefficient on L_i/L, $\gamma(\delta_i - \delta_0)$ gives the difference in output elasticities between worker type *i* and type *0*. Thus the two different specifications of *QL* in (15) and (18) give different interpretations of the coefficient on $\overline{s}\, L_i/L$, compare (16) and (19). Combing (15) and (3), the functional form of (15) gives that ϕ is both the marginal and the average product of a skilled worker. The production functions (16) and (19) return estimates of average products, whilst, if wages equal marginal products, the Mincer-type equations return an estimate of marginal products. If the form of the production function is such that marginal and average products are equal then one can directly compare σ_i from a Mincer equation and σ_i from (16) or (19) and this is the approach of Jones (2001) and Hellerstein et al (1999) who estimate equations like (16) along with Mincer-type wage equations with measures of worker types by education.. However, combing (18) and (3) shows that the δ coefficients in (18) are relative output elasticities. In general there is no reason that relative output elasticities should equal relative output marginal products but (15) imposes this restriction. Since we are not specifically interested in comparing wage and production functions we shall estimate equations of the form of (16) and (19) and, if required, they can be interpreted as giving the information on marginal products of the different labour types.