Skill-Biased Technical Change and Wages: Evidence from a Longitudinal Data Set.

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Abstract

The widening of the wage structure in the 1980s has been attributed in large part to the impact of skill-biased technical change. Yet very few studies have shown a clear link between an individuals wage and their use of skill-biased technology. One important exception is Krueger (1993) who demonstrates a strong positive correlation between computer usage and wages. This paper explores this correlation in more detail and examines the impact on wages of other indicators of skill-biased technology. Using a longitudinal data set for the UK we show that computer use and other measures of skill are strongly correlated with earnings, appear to have a productivity enhancing interpretation and are not merely capturing unobserved characteristics. Furthermore the increased importance of these skills can explain a large fraction of the increase in the returns to education over the course of the 1980s.

1 Introduction

> "the principal reason for the increases in wage differentials by educational attainment and the decrease in the gender differential is a combination of skilled-labor-biased technical change and changes in unmeasured labor quality" (Bound and Johnson, 1992).

The 1980s witnessed a dramatic widening of the wage structure in both the United States and United Kingdom. At the same time the returns to education rose in both countries. Two principal explanations have been provided for these observations. Both are based on rising relative demand for skilled labor. First, skill-biased technical change associated primarily with the computer revolution is claimed to have favored the more skilled. Second, increased exposure to international trade and the rising importance of low-skill manufactured imports from the newly industrialized countries has led to a fall in the demand for unskilled workers in the industrialized world.

Much of the evidence in favor of the skill-biased explanation rests on showing that the data is not consistent with other explanations (see for example Bound and Johnson, 1992). This is hardly a very compelling method of proving one's case and it would be more satisfactory if it could be shown that workers using these new technologies and possessing relevant skills were receiving a wage premium in the marketplace. This is exactly what Alan Krueger (1993) has sought to demonstrate. He shows that US workers obtained a wage premia in the 1980s if they used computers in their workplace. Controlling for numerous observable characteristics did not qualitatively affect the results. As with all cross-section wage equations however, we must be cautious in interpreting these results. It is possible that computer usage is positively correlated with characteristics that are unobserved in the data set and that generate wage premia.

In this case the coefficient on the computer dummy is biased upwards and the extent of this bias is unknown. In a recent paper, DiNardo and Pischke (1996) have analyzed a German data set and argue that the observed correlation between computer usage and wages is indeed capturing unobserved heterogeneity among workers.

In this paper we make use of a longitudinal data set for the UK. This data set has wage data on individuals in 1981 and again in 1991. The 1991 survey also contained questions on computer usage and other technical skills used in the workplace. Furthermore the data set contains a vast array of information on the individual and the employer so that we are likely to be particularly successful in controlling for other characteristics in our empirical work. More importantly perhaps is that we exploit the longitudinal element of the wage data directly to test for the bias of unobserved characteristics.

The structure of this paper is as follows. In the next section we discuss the data and present some descriptive statistics on the prevalence of different technical skills in the workplace. Section 3 then estimates standard cross-section wage equations for the 1991 wage observations adding in these differing measures of technical skill. In Section 4 we make use of additional controls and the panel element of the data set to try and determine the extent to which the cross-section estimates are biased due to omitted variables. Section 5 estimates the impact of skill-biased technology on the change in the wage structure that occurred during the 1980s. Our conclusions are then given in Section 6.

2 The Data and Summary Statistics

The data for this study are drawn from the National Child Development Study (NCDS). This study is a continuing longitudinal study which is seeking to follow the lives of all those living in Great Britain who were born between 3 and 9 March, 1958. To date, there have been five major waves of the survey conducted in 1965 (when aged 7), in 1969 (age 11), in 1974 (age 16), in 1981 (age 23) and in 1991 (age 33). Information on wages and jobs was collected in the final two waves. Our wage measure is the gross hourly wage and we sample all full-time males and females.

We have a number of variables from the 1991 sweep that provide information on the technical skills required in the respondents employment. Information was obtained about a range of abilities and where these abilities were most used (e.g. at work, home etc.). We focus on four of these abilities:

- using computers.
- mathematical calculations.
- reading plans and diagrams.
- running an organization, group or firm.

These variables have the advantage of allowing for a much richer view of skill-biased technical change than is commonly given in the literature. While the use of computers in the workplace has taken centre stage in the debate on technical change, other changes such as the introduction of team working are also an important feature of the changing nature of work in the 1980s. An additional reason to emphasize other skills is because of their complementarity with the use of computers. For example, in the model of Helpman and Trajtenberg (1994) the benefits of computer technology only occur after the development of complementary inputs. They argue that

> "it is becoming quite clear that in order to reap the benefits from computerization firms have to redesign the organization of work (e.g. emphasize team-work rather than hierarchical links), ..., the history of technology suggests that changes in technology and changes in organization and institutions are intimately related" (Helpman and Trajtenberg, 1994)

We begin by reporting some simple statistics showing the characteristics of those with particular skills. Table 1 reports the percentage of respondents in particular groups who use the specified skill in the workplace. Focusing on the first column, it is clear that the use of computer technology is concentrated among white-collar, well-educated workers. A similar pattern emerges for the other skills considered though in general the differences between occupational and educational groups is narrower. Men have a significantly higher probability of using mathematical calculations and reading plans and diagrams in the workplace but there is less of a difference for computer use and organizational ability. The only strong effect of establishment size occurs with computer usage and is clearly positive.[1]

3 Cross-Section Estimates

3.1 The Cross-Sectional Return to Computer Use

We begin by replicating the results of Krueger (1993) to show that those who used computers at work in 1991 in the UK obtained a *ceteris paribus* wage advantage. We then explore the other skill measures and try to identify which are particularly important for wages. We estimate simple log-linear wage equations of the form

$$\ln w_{91} = \alpha + \beta X_{91} + \delta C_{91} + \varepsilon_{91} \qquad (1)$$

where all the variables are subscripted by the year of the observation. Hence wages are a function of standard human capital variables (the components of the X_{91} vector) and a dummy variable, C_{91}, which takes the value of one if the worker uses a computer in the workplace and zero otherwise. Notice that a standard age-earnings profile would

[1] This contrasts with the results of Hirschorn (1988) who shows no strong relationship between establishment size and computer use.

not be identified in this data set since all respondents were born in the same week. Furthermore while potential labor market experience does differ between individuals due to different schooling levels, potential experience is a linear function of years of schooling and is therefore also unidentified.

Table 2 reports OLS estimates of various specifications of equation (1). The first column includes only the computer dummy. The dummy is highly significant and implies a raw differential of 49.8% between those who do and those who do not use a computer.[2] This compares with the 1989 US differential of 38.4% reported by Krueger. The subsequent columns of Table 2 add several other explanatory variables to this regression. In Column 2 we add controls for gender, marital status, years of schooling, union status, and health status. Furthermore, all regressions except for column 1 include 10 regional dummies. Adding these controls reduces the computer premium to 35.4% with a t-ratio of 20.2. Column 3 then also adds some information on employment characteristics. It may be argued that the computer dummy is correlated with employer characteristics and is in fact capturing rent-sharing effects. Our data set provides a significant amount of information on the workplace. We include 3 employer size dummies, 3 type of employer dummies (private sector, nationalized industry, government), a dummy if the individual has supervisory responsibilities and dummies for evening, night, early morning, Saturday and Sunday work. We also include a dummy if the job allows for a significant proportion of the work to be done at home. For brevity we only report the establishment size and supervisory dummies in Table 2. It is clear from Column 3 that these controls are important and more significantly they reduce the wage premium on computer usage to 25.0% with a t-ratio of 14.9. In column 4 we add in 55 industry dummies. Once again the computer premium falls somewhat to 21.4% with a t-ratio of 10.8.[3]

[2] i.e. (exp(0.404) - 1).

[3] The observant reader will note that the sample size declines significantly between columns 3 and 4. This reflects missing data on industry of employment. To check that this is not driving the reduction

Finally in column 5 we include 15 occupation dummies. As Krueger argues, whether this is appropriate or not is a debatable issue. Workers with computer skills may well be placed in occupations that traditional pay more precisely because of their computer skills. Hence to get the true return to computer skills one would need to model the effect of these skills on occupational category. In some sense the previous regressions have done this by estimating the reduced-form of such a model. Still, when these occupation dummies are included, the computer premia falls again to 17.0% with a t-ratio of 8.3. Indeed even if we include 53 occupation dummies, the premia is estimated to be 14.8% with a t-ratio of 7.1. The most comparable figure from Krueger's study for 1989 is 17.6%. We have already shown that this is likely to be biased upward due to the omission of employer characteristics that we find reduce the computer use premium quite significantly.

Our results therefore point to a significant wage premia associated with the use of computers at work. Controlling for a range of characteristics of the individual and the job reduces the premia significantly but does not eliminate it. Our cross-section results are very similar to those reported by Krueger for the US which is perhaps unsurprising given the rapid adoption of computer technology throughout the industrialized world.

3.2 Is the Observed Return Really Productivity Enhancing?

We have been implicitly assuming that the observed return to computer use reflects productivity effects. In this section we show that our data is indeed consistent with such an interpretation. This contrasts with results in DiNardo and Pischke (1996) who find that the computer differential is entirely explained by computer knowledge not actual computer use.

Our data set allows respondents to say whether their computer skills were used at

in the computer dummy coefficient between the two columns, we re-estimated column 3 on the reduced sample. The coefficient on the computer dummy was 0.216 (s.e. 0.018).

work, or whether they have computer skills that are not used at work. We therefore estimate equations of the form:

$$\ln w_{91} = \alpha + \beta X_{91} + \delta_1 CWork_{91} + \delta_2 CAbility_{91} + \varepsilon_{91} \tag{2}$$

where $CWork$ is a dummy equal to one if the worker uses a computer at work and $CAbility$ equals one if the individual claims they have the ability to use computers. The coefficient on $CAbility$, δ_2, will be positive if the observed correlation between computer usage and earnings is driven by computing ability rather than the productivity effects of computer use. Table 3 reports the results of re-estimating the model with the specification given by equation (2). The upper panel of the table includes industry dummies while the lower panel includes industry and occupation dummies. The first column reports the estimates using just $CWork$, the second column uses just $CAbility$, and the third column includes both measures simultaneously. The ability to use computers is correlated with earnings though it appears to be a less useful measure than the computer use dummy. Most importantly, column 3 shows that once we control for using a computer at work, there is no effect from having the ability to use a computer. These results are strongly supportive of a productivity-enhancing interpretation of the computer wage premium.

3.3 Cross-Sectional Return to Other Skills

We now test the significance of the other skill measures available to us in the data set. While the growth in the use of computers has been pinpointed as a major source of technical change in the 1980s other skills are likely to have become more important as well. For example Lindbeck and Snower (1996) emphasize the change in the culture of the workplace and the growing importance of non-Tayloristic work practices. We estimate wage equations that include dummies if the respondent uses mathematical calculations in their job ($Math$), a dummy if they are required to read plans or diagrams ($Plans$) and a dummy if they have some organizational responsibility ($Organize$). Table 4 includes industry dummies but not occupation dummies. All regressions include the full set of controls used in Column 4 of Table 2.[4]

The table show large premia associated with all types of observable skill. The coefficients are all well-defined and statistically significant. It may be thought that the organization dummy is merely picking-up the additional pay that goes with supervisory responsibility. There are two reasons to doubt this interpretation. First, the regressions already include a dummy for whether the worker supervises others in the workplace. The coefficient on this variable is positive and significant but does not eliminate the importance of the organization dummy. Secondly, the correlation between the two measures is 0.384 which indicates a significant difference between the two. So for example 24% of the sample report supervisory responsibility but no organizational responsibility.

The final column of the table includes all the measures of skill in the same equation, including the computer dummy. In this specification, all the measures except *plans* remain significant and the premia are quite large. These results suggest that the concentration on the computer revolution is to some extent mis-leading. Other skills such as the ability to perform mathematical calculations and the competence to organize are also important.

We also tested whether these additional skills had a productivity-enhancing interpretation. We used the same procedure as that outlined above for testing the productivity effects of computer use. For both math calculations and organizational ability, the dummy for having such a skill was significant when entered without the dummy for whether the skill was actually used at work. For the plans variable, the dummy

[4]In a lone effort to save the rainforests we omit results where both industry and occupation dummies are included. Suffice it to say that the results remain qualitatively similar. Results can be obtained upon request.

for having the skill was insignificant. When both the skill dummy and the use at work dummy were entered together, the skill dummy was insignificant while the use at work dummy was positive and strongly significant in all cases.[5] Once again we interpret these results as favoring a productivity-enhancing interpretation of the wage premium.

4 Is the Observed Return Real?

The fundamental problem with the results reported above is that our measure of particular skills in the workplace may be positively correlated with unobserved characteristics that also generate wage premia. In this case the coefficients will be biased upwards. It is certainly true that the coefficients fall significantly as we add more covariates. However even when we include industry and occupation dummies, the estimated premia on a range of measures are large and remain strongly significant.

In this section we explore a number of ways of assessing the likely size and significance of this bias. Our first method adds test scores that were obtained from the respondents when they were aged 16. A unique aspect of this data set is that a uniform, specially constructed test was given to respondents that tested their reading and mathematics comprehension. These scores are likely to be strongly correlated with ability. An alternative approach is to use the longitudinal element of the data set. There are two obvious ways of making use of this aspect of the data set. First, we estimate a cross-section wage equation for the 1981 wage data using the 1991 measures of skill. If these measures are capturing unobserved ability then we would expect them to be significant in the 1981 cross-section. Second we estimate a difference equation between 1981 and 1991 for the full sample and various sub-samples of workers. This

[5]The coefficients and standard errors were as follows:

Math: Use at Work = 0.133 (0.022); Ability = 0.060 (0.042)

Plans: Use at Work = 0.104 (0.020); Ability = -0.031 (0.036)

Org: Use at Work = 0.144 (0.023); Ability = 0.017 (0.023) allows us to eliminate the fixed-effect associated with individual characteristics and in the sub-samples we can also remove firm-level fixed effects.

4.1 Using Test Scores to Control for Unobserved Characteristics

A standard method of controlling for unobserved ability is to use standardized test scores. The NCDS reports two such tests that were conducted at age 16. A reading comprehension test with a score range of 0-35 and a math comprehension test with a score range of 0-31. These tests were conducted at school and were identical for all respondents. They are thus likely to provide a good measure of ability at age 16. In this section we simply re-estimate some of the models reported in the previous section including these test scores as additional explanatory variables.

We focus first on the computer premia. Table 5 reports the results of re-estimating columns 4 and 5 of Table 2 but including the two test scores. It must be noted straight away that the sample size declines again due to missing data on test scores. For comparison, column 1 re-estimates column 4 of Table 2 (i.e. without including test scores) on the reduced sample. The coefficient on the computer dummy is 0.183 compared to 0.194. Hence the reduction in sample size has no appreciable effect on the coefficient estimates. Columns 2 and 3 examine the effect of the test scores on the computer dummy while the remaining columns use all the skill variables. The two test scores enter with the right sign and are statistically significant. This contrasts with the results reported by Krueger using the High School and Beyond Survey. He finds that achievement test scores are negatively correlated with subsequent earnings. This makes one rather sceptical about the value of the measures in controlling for unobserved ability. No such problem occurs with our measures. The inclusion of test scores does reduce the coefficient on the computer use dummy but not dramatically. The main effect is, not surprisingly, on the years of schooling coefficient which falls significantly. These results do not point to any significant unobserved ability bias in the cross-section.[6]

In the final two columns of Table 5 we repeat the exercise for all the measures of skill. Except for the *plans* dummy, all our measures of skill remain significantly positive after the inclusion of the test scores. This is particularly interesting with respect to the *math* dummy. It may obviously have been the case that this was simply picking up general mathematical ability rather than the practical use of mathematical calculations in the workplace. Our regressions show that even controlling for the level of math ability of the individual, there is a wage premium associated with jobs that require mathematical reasoning. The coefficients on the computer dummy in the final two columns of Table 5 are lower than those in earlier tables. This is primarily due to the significant positive correlations between computer usage and the other skills we are considering.[7] Hence in cross-sections that only include a computer dummy, some of the positive correlation is picking up other technical skills.

[6]We tried numerous robustness tests at this stage. We used test results reported at age 11 as instruments to control for potential measurement error. This had the effect of reducing the coefficient on the reading test measure but raising the math test coefficient. It had no important effect on the technical change dummies. We also split the test measures into quintiles of attainment to allow for non-linearities but this appears unimportant.

[7]The correlation coefficients between the various skill measures are all positive and significant with a p-value of less than 0.001. The correlation matrix is:

	Comp	Math	Plans	Org
Comp	1.000			
Math	0.350	1.000		
Plans	0.231	0.357	1.000	
Org	0.269	0.318	0.234	1.000

4.2 Using the 1981 Cross-Section to Control for Unobserved Characteristics

In this section we use the wage data from the 1981 wave of the survey to try and establish whether our measures of skill are actually measuring unobserved characteristics. To do this we exploit a simple idea. Suppose our measures are indeed capturing unobserved but marketable characteristics of the individual. Then assuming these characteristics were present in 1981, our skill measures should be significantly positive in the 1981 cross-section. Hence we estimate an equation of the form:

$$\ln w_{81} = \alpha + \beta X_{81} + \pi C_{91} + \varepsilon_{81} \tag{3}$$

and test whether $\pi > 0$. Of course π may be positive because C_{91} is correlated with computer usage in 1981 that was rewarded. The same observation applies, *mutatis mutandis,* to the other measures of skill. We therefore require either that these skills were not present in 1981 or they were not rewarded.

Fortunately our data set allows us to shed some light on this issue. In the 1991 survey the respondents were asked whether their competence in the particular skill had got better, stayed the same or deteriorated over the previous 10 years. We can therefore create dummy variables for the particular skills for the 1981 cross-section that equal one if their competence has stayed the same or deteriorated, since these individuals are claiming to have been equally or more competent with these technologies in 1981 than in 1991. We find that 15.0% of the sample claim to have had computer skills in 1981, 17.7% to have had organizational skills, 57.2% math skills and 44.2% plans skills. It is interesting to note that the two most rewarded skills in 1991, computing and organization, are the skills that also experienced the largest increase in supply over the decade. We estimate an equation identical to that given in Column 4 of Table 4 for the 1981 cross-section using these retrospective dummies on skill. We find that the coefficients on the four skill measures in this equation are:

- *computer dummy* : -0.008 (s.e. 0.014)
- *math dummy* : 0.035 (s.e. 0.010)
- *plans dummy* : -0.010 (s.e. 0.010)
- *organize dummy* : 0.001 (s.e. 0.013)

It is clear that except for the math dummy, there was no observed market return for these skills in 1981. Hence we proceed under this assumption since it is not inconsistent with the data that we have.

Table 6 estimates a variety of models on the 1981 cross-section. In column 1 we simply have the computer dummy as an independent variable. Clearly it is positively correlated with 1981 wages and highly significant. This is hardly surprising and cannot be taken as evidence against our interpretation of the computer dummy in the 1991 regression. After all, we know the dummy is strongly positively correlated with education. When we add in more explanatory variables the coefficient falls dramatically and is statistically insignificant. Indeed in the most general specification given in column 5 there is no evidence that any of our measures of skill are correlated with previous earnings. It is therefore difficult to maintain that our measures are simply capturing unobserved heterogeneity amongst workers. It must be acknowledged however at this point that we are only able to explore the bias associated with omitted individual characteristics. Omitted firm-level characteristics may still be driving the correlation in the 1991 data, though our earlier regressions did include a range of firm-level data.

Of course statistical significance is not the only criteria that should be applied in this case. Suppose that the dummies on the skill variables are measuring unobserved ability. Then in a single index model of skills the coefficients on these measures are rather like the coefficients on the test dummies. To see this consider the model of Card and Lemieux (1996). Let k_i be the skill index of individual i and assume the log wage of i in period t is a linear function of k_i: $\beta_t k_i$. Let $\beta_0 = 1$ for a base period 0 (in this case 1981). The observed log wage of individual i is w_{it}, where

$$w_{it} = \beta_t k_i + \varepsilon_{it}$$

If $\beta_t > 1$, then the return to skill increased between the base period and period t. To estimate this model on the data we assume that

$$k_i = x_i \theta + \alpha_i$$

where x_i is a vector of observables (e.g. test scores) and α_i is the unobserved component of skill. Combining these two equations implies a set of linear regressions with time-dependent coefficients:

$$w_{it} = x_i \delta_t + \eta_{it}$$

where $\delta_t = \beta_t \theta$ and $\eta_{it} = \alpha_i + \varepsilon_{it}$. The crucial point here is that an increase in the return to skill implies a uniform re-scaling of the regression coefficients associated with observed skill attributes.

Now suppose we compare column 3 of table 6 with column 3 of table 5. These two equations are the same except for the year in which they are estimated. Notice that the coefficients on both the reading and math test scores rise from 0.004 in 1981 to 0.006 in 1991. In other words the coefficients have been re-scaled by 50%. Now suppose we apply the same re-scaling to the coefficient on the computer dummy in 1981. This would imply a rise from 0.024 to 0.036. In fact the coefficient on the computer dummy is 0.112 in 1991. Hence even if we believe the above argument, the value of the computer premia in 1991 cannot be easily explained by a rising return to ability.

4.3 Using the Panel to Control for Unobserved Characteristics

Our final test uses the panel element of the data set to control as far as possible for unobserved characteristics. Suppose we have the two cross-section wage equations given below:

$$\ln w_{81} = \alpha_{81} + \beta_{81} X_{81} + \gamma_{81} A + \varepsilon_{81} \tag{4}$$

$$\ln w_{91} = \alpha_{91} + \beta_{91} X_{91} + \gamma_{91} A + \delta C_{91} + \varepsilon_{91} \tag{5}$$

where A is a vector of unobserved characteristics which may be individual or firm-level. Once again, we implicitly assume that our skill measures, C, where either not present in the 1981 sample or where not rewarded. Since we cannot observe A in the cross-section, any positive correlation between A and C will impart upward bias on δ in the 1991 cross-section. If we convert these cross-section regressions into a difference equation, it is easily seen that

$$\begin{aligned} \Delta \ln w \;\; &= \;\; (\alpha_{91} - \alpha_{81}) + (\beta_{91} - \beta_{81}) X_{91} + \beta_{81}(X_{91} - X_{81}) \\ &\quad + (\gamma_{91} - \gamma_{81}) A + \delta C_{91} + (\varepsilon_{91} - \varepsilon_{81}) \end{aligned} \tag{6}$$

The unobserved characteristics only enter the difference equation if the coefficient associated with these characteristics changes over time. We cannot do much about such time-varying parameters but notice that the difference equation will certainly reduce any biases arising from unobserved characteristics compared to the cross-section equation. The difference model includes the observed characteristics in both 1981 and 1991 (i.e. the X_{81} and X_{91}). These enter in two ways. The first term $(\beta_{91} - \beta_{81})X_{91}$ represents the change in the return to a fixed characteristic (e.g. the increase in the return to education that occurred in the 1980s) while the second term $\beta_{81}(X_{91} - X_{81})$ captures the change in wages associated with a change in observed characteristic (e.g. moving into a different occupation category). In our empirical estimates of the difference equation we allow for changing coefficients on the test scores, years of schooling, and the female dummy. We also allow for changes in the occupational status of the worker over the period and for a change in union status. Furthermore, all our results include a dummy variable if the worker has been promoted while in the job.

Table 7 reports a collection of difference equations. Columns 1 and 2 estimate the difference equation on the full sample. Missing data reduces the sample to 2,208. The results show that the coefficients on the skill variables are very similar to those obtained in the 1991 cross-section. Again there is little evidence that unobserved ability are driving these results. In columns 3 and 4 we restrict the sample to those workers who worked for the same firm in both 1981 and 1991. This enables us to remove the fixed-effect associated with firm-specific characteristics. This generates a sample of just under 1000 workers. Since such stayers are unlikely to be a random sample from the whole data set we estimated a new 1991 cross-section wage equation using the same specification as in Column 4 of Table 2. We omit the *plans* dummy as it appeared the least important measure of skill in the previous sections. We find that the coefficients and standard errors on the computer, math and organize dummies are 0.077 (0.029), 0.022 (0.031) and 0.073 (0.028) respectively. Compared to the estimates on the full sample, the dummies are significantly smaller in this sample. However both the computer and organize dummies still demonstrate a significant wage premium attached to these skills. Notice that under a strict interpretation of equation (6), the coefficient on the skill measures should be the same in both the level and difference equation. We are unable to reject this hypothesis in our data set and the differenced model gives little support for unobserved heterogeneity at either the individual of firm-level. The coefficient on the computer dummy falls from 0.077 in the 1991 cross-section to 0.047 in the difference model, suggesting that a maximum of 40% of the premia can be explained by unobserved heterogeneity.

As a final test we restrict the sample to males who left school at the minimum school-leaving age. For these workers only we have information on their starting weekly wage in their first job. Since this wage refers to a job obtained in 1974 it is clear that there is no way that the worker could have been using a computer in this job. By 1991 however 34% of these workers report using a computer at work. This sub-sample also has the advantage of being reasonably homogenous as a group. Column 5 of Table 7 estimates a difference equation between 1991 and 1974 of the log weekly wage. Once again there is strong support for the existence of wage premia associated with the use of computers at work. There are also large returns to maths and organizational ability.[8]

5 Skill-Biased Technology and the Changing Wage Structure

How much of the observed change in the UK wage structure over the course of the 1980s can be attributed to the wage premia associated with the technical skills we have highlighted in this paper? In this penultimate section we examine how the change in the return to education is affected when we control for skill-biased technical change. One problem that we encounter with our data set is that years of schooling and potential experience are co-linear since all the sample are the same age and started school at the same time. Hence the coefficient on the years of schooling variable used in the previous sections is a complex combination of the returns to education and experience.

[8]We would not like to push this argument too far. Much has happened to these workers since 1974 that we are not capturing in our regressions. The results are however certainly suggestive.

To get around this problem and assess the effect of skill-biased technical change on the wage structure, we use measures of educational qualifications instead of years of schooling. This then allows us to identify both education and experience terms in our regressions.[9] We also include the two test scores in all the regressions and evaluate the educational qualification differentials using the mean test scores within the relevant education group.

We estimate cross-section wage equations in 1981 and 1991 and then estimate a 1991 equation that includes the skill dummies. We then compare the change in the return to different educational qualifications over the 1980s using the two different 1991 equations. These estimates provide an upper bound on the impact of skill-biased technology on the changing wage structure since we are forced to assume that they either were not present in the workplace in 1981 or had little effect on the returns to education. We have already shown however that our data tends to support the hypothesis that these skills did not generate significant wage premia in 1981 and the 1991 skill measures have no effect on the estimates of the 1981 returns to education.

Table 8 reports our estimates. All the regressions in the table include a full set of controls and a quadratic in experience. Column 1 estimates the wage premia associated with educational qualifications in 1981. The qualification variables are:

- $Qual\ 1$: CSE Qualification (obtained usually at 16) (14% of the sample)
- $Qual\ 2$: O Level Qualification (obtained usually at 16) (36%)
- $Qual\ 3$: A Level Qualification (obtained usually at 18) (17%)
- $Qual\ 4$: Higher Qualification (10%)
- $Qual\ 5$: University Degree or equivalent (10%)

[9] We re-estimated all the models in the previous sections using educational qualification dummies and a quadratic in experience instead of years of schooling. None of our results are sensitive to this alternative specification.

with no qualifications (13%) as the omitted level of education. Column 2 then estimates the same equation using the 1991 data. As expected the return to educational qualifications rose considerably over the course of the 1980s. The wage premia associated with a University degree rose by 0.138 log points over the decade. All the coefficients show a relative gain compared to those with no qualifications though the gains are mostly located in the upper end of the qualifications range (see Schmitt, 1995 for similar results on the change in the education premia). In column 3 we add the computer dummy to the 1991 regression. This dramatically reduces the rise in the returns to education over the decade. 41% of the rise in the return to a University degree is eliminated by introducing the computer dummy and similar large reductions occur for other education levels. In the final column we introduce the other measures of skill simultaneously. We now explain 57% of the rise in the return to a University degree over the 1980s. These results are indicative of large skill-biased technical change effects on the education premia over the 1980s. The magnitude of the effect is broadly similar to that reported by Krueger for the US.

6 Conclusion

The large increases in the returns to education in the US and UK over the 1980s has been attributed by most commentators to the pervasive effect of skill-biased technical change most commonly associated with the computer revolution. Yet little hard evidence has been produced to demonstrate this link. The justification for the role of technology is in reality based upon studies showing that other explanations are inconsistent with the data. This is to say the least an unusual way of demonstrating a linkage in empirical economics and cannot be expected to convince even the mildly sceptical.

A more direct approach has been followed by Krueger (1993). He shows that workers in the US who use computers at work receive a wage premium. He interprets this as evidence of the productivity enhancing effect of computers in the workplace and argues that much of the rise in the returns to education can be accounted for by the proliferation of computers. But his results are inevitably susceptible to the criticism that he is in fact capturing unobserved heterogeneity amongst workers that is correlated with computer use. This is the point made most clearly by DiNardo and Pischke (1996) who suggest that all the results of Krueger are consistent with the heterogeneity explanation.

This paper uses a longitudinal data set for the UK to try and isolate the true return to computer use by controlling for heterogeneity among workers. We also emphasize a wider range of technical skills than is common in the literature. Using a variety of techniques we show that wages are positively related to these skills and that there is little evidence that unobserved characteristics of either the individual or the firm are driving this correlation. Furthermore it is the use of these skills in the workplace that is important for wages not simply the ability. This suggests that a productivity enhancing interpretation is the most appropriate. Furthermore our results suggest that up to one-half of the increase in the return to education over the decade can be attributed to the various measures of technical skill that we concentrate on.

One may wonder why such premia exist. After all workers should observe these premia being paid in the marketplace and excess demand for such jobs should result. This will drive down the wage premia associated with these skills. Our data show that the two most rewarded skills, computer use and organizational ability, did indeed show the largest increase in supply over the 1980s. It must therefore be that employers' demand for such skills rose at an even faster rate than the supply. Whether this race between supply and demand will produce the same outcome in the 1990s is an open question.

7 References

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Table 1. Technical Skills and Personal Characteristics

	Comp	Math	Plans	Org
All Workers	48.7	69.6	60.4	42.2
Males	50.2	77.5	70.4	46.7
Females	46.4	57.4	44.6	35.2
Professional & Managerial	70.3	87.7	70.5	71.7
Skilled Manual	23.0	66.6	72.2	23.6
Semi- and Unskilled Manual	15.0	44.3	43.4	15.8
11 or less Years of School	37.1	65.8	59.5	34.5
12-16 Years of School	59.3	71.8	59.2	47.5
16+ Years of School	69.3	80.1	67.1	60.5
Manufacturing	45.6	71.5	66.7	32.1
Distribution, Hotels, Catering	43.3	72.2	53.4	49.4
Transport and Communication	52.2	72.3	63.3	38.7
Banking, Finance, Insurance	64.6	77.5	54.4	50.0
Union Member	53.0	72.1	64.7	39.6
Non-Union Member	45.9	68.1	57.6	43.9
25 or fewer workers	38.1	67.4	55.4	47.1
26-99 workers	49.3	72.9	60.9	44.9
100-499 workers	51.5	68.5	61.8	39.2
500+ workers	58.2	70.5	64.3	37.2

Notes: Figures are percentages giving positive response.

Table 2. 1991 Wage Regression Results

	(1)	(2)	(3)	(4)	(5)
Constant	1.694	0.822	0.855	0.797	0.880
	(0.010)	(0.054)	(0.070)	(0.301)	(0.305)
Computer Dummy	0.404	0.303	0.223	0.194	0.157
	(0.014)	(0.015)	(0.015)	(0.018)	(0.019)
Years of School		0.065	0.046	0.041	0.026
		(0.004)	(0.004)	(0.005)	(0.005)
Female		-0.161	-0.154	-0.157	-0.141
		(0.025)	(0.024)	(0.030)	(0.030)
Married		0.147	0.128	0.109	0.102
		(0.020)	(0.019)	(0.023)	(0.022)
Female*Married		-0.199	-0.186	-0.172	-0.153
		(0.031)	(0.030)	(0.036)	(0.035)
Union Member		0.105	0.080	0.087	0.102
		(0.015)	(0.016)	(0.020)	(0.020)
Health Problem		-0.084	-0.077	-0.061	-0.052
		(0.031)	(0.030)	(0.035)	(0.034)
Supervisor			0.157	0.163	0.132
			(0.015)	(0.018)	(0.018)
26-99 workers			0.081	0.096	0.096
			(0.020)	(0.023)	(0.023)
100-499 workers			0.153	0.146	0.142
			(0.020)	(0.024)	(0.024)
500+ workers			0.194	0.182	0.181
			(0.021)	(0.026)	(0.026)
Industry Dummies	No	No	No	Yes	Yes
Occupation Dummies	No	No	No	No	Yes
N	5695	4863	4863	3459	3382
R^2	0.121	0.265	0.320	0.335	0.365

Notes: The dependent variable is the log of gross hourly wages. Standard errors in parentheses.

Table 3. Computer Usage and Productivity

	(1)	(2)	(3)
(i) with Ind Dummies			
Computer at Work	0.194		0.204
	(0.018)		(0.023)
Computer Ability		0.118	-0.016
		(0.019)	(0.024)
N	3459	3459	3459
R^2	0.335	0.320	0.334
(ii) with Ind & Occ Dummies			
Computer at Work	0.157		0.167
	(0.019)		(0.024)
Computer Ability		0.085	-0.018
		(0.019)	(0.024)
N	3382	3382	3248
R^2	0.365	0.356	0.365

Notes: The dependent variable is the log of gross hourly wages. Standard errors in parentheses. All regressions contain the full set of controls used in Column 4 of Table 2.

Table 4. Other Skills and Wages

	(1)	(2)	(3)	(4)
Math Dummy	0.160			0.085
	(0.020)			(0.021)
Plans Dummy		0.096		0.035
		(0.019)		(0.019)
Organize Dummy			0.157	0.114
			(0.019)	(0.020)
Computer Dummy				0.147
				(0.019)
N	3358	3358	3358	3358
R^2	0.322	0.315	0.323	0.344

Notes: The dependent variable is the log of gross hourly wages Standard errors in parentheses. All regressions include the full set of controls used in Column 4 of Table 2.

Table 5. 1991 Wage Regressions with Test Scores

	(1)	(2)	(3)	(4)	(5)
Computer Dummy	0.183	0.156	0.133	0.112	0.101
	(0.021)	(0.022)	(0.022)	(0.023)	(0.023)
Math Dummy			0.070	0.062	
			(0.025)	(0.025)	
Plans Dummy			0.042	0.022	
			(0.022)	(0.022)	
Organize Dummy			0.114	0.092	
			(0.023)	(0.023)	
Years of School	0.038	0.022	0.013	0.021	0.014
	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
Reading Test		0.007	0.006	0.006	0.005
		(0.002)	(0.002)	(0.002)	(0.002)
Math Test		0.006	0.004	0.006	0.004
		(0.002)	(0.002)	(0.002)	(0.002)
Industry Dummies	Yes	Yes	Yes	Yes	Yes
Occupation Dummies	No	No	Yes	No	Yes
N	2537	2537	2537	2464	2464
R^2	0.324	0.335	0.360	0.347	0.367

Notes: The dependent variable is the log of gross hourly wages Standard errors in parentheses. All regressions include the full set of controls used in Column 4 of Table 2.

Table 6. 1981 Wage Regressions

	(1)	(2)	(3)	(4)	(5)
Computer Dummy	0.124	0.031	0.024	0.024	0.018
	(0.010)	(0.013)	(0.013)	(0.012)	(0.013)
Math Dummy			-0.006		-0.005
			(0.014)		(0.014)
Plans Dummy			0.033		0.017
			(0.013)		(0.013)
Organize Dummy			0.015		0.018
			(0.013)		(0.013)
Reading Test		0.005	0.004	0.005	0.004
		(0.001)	(0.001)	(0.001)	(0.001)
Math Test		0.004	0.004	0.003	0.003
		(0.001)	(0.001)	(0.001)	(0.001)
Industry Dummies	No	Yes	Yes	Yes	Yes
Occupation Dummies	No	No	No	Yes	Yes
N	6041	3685	3564	3678	3564
R^2	0.025	0.256	0.258	0.281	0.283

Notes: The dependent variable is the log of gross hourly wages. Standard errors in parentheses. All regressions include the full set of controls used in Column 4 of Table 2.

Table 7. Difference Models

	(1)	(2)	(3)	(4)	(5)
Computer Dummy	0.149	0.120	0.075	0.047	0.134
	(0.018)	(0.018)	(0.022)	(0.022)	(0.046)
Math Dummy		0.047		0.038	0.110
		(0.020)		(0.024)	(0.050)
Organize Dummy		0.104		0.136	0.137
		(0.018)		(0.021)	(0.046)
Reading Test	0.004	0.003	0.006	0.005	0.022
	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)
Math Test	0.001	0.001	0.000	-0.001	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.005)
N	2208	2208	990	990	848
R^2	0.189	0.206	0.111	0.152	0.125

Notes: The dependent variable is the first difference of the log of gross hourly wages. Standard errors in parentheses.

Table 8. Skill-Biased Technology and the Wage Structure

	(1)	(2)	(3)	(4)
Computer Dummy			0.143	0.118
			(0.014)	(0.014)
Math Dummy				0.064
				(0.015)
Organize Dummy				0.080
				(0.014)
Qual 1	0.062	0.092	0.077	0.072
	(0.022)	(0.031)	(0.031)	(0.031)
Qual 2	0.131	0.203	0.171	0.158
	(0.019)	(0.027)	(0.027)	(0.027)
Qual 3	0.191	0.259	0.223	0.205
	(0.022)	(0.030)	(0.030)	(0.030)
Qual 4	0.225	0.371	0.323	0.298
	(0.024)	(0.031)	(0.031)	(0.031)
Qual 5	0.314	0.452	0.395	0.373
	(0.030)	(0.038)	(0.037)	(0.037)
N	4292	3482	3482	3482
R^2	0.205	0.429	0.446	0.455

Notes: The dependent variable is the log of gross hourly wages in the specified year. Standard errors in parentheses. All regressions include the set of controls in Column 3 of Table 2 and a quadratic in experience. Column (1) is the 1981 regression.