THE EFFECT OF SCHOOL QUALITY ON EDUCATIONAL ATTAINMENT AND WAGES

Lorraine Dearden
Javier Ferri
Costas Meghir

THE INSTITUTE FOR FISCAL STUDIES
WP 00/22

The Effect of School Quality on Educational Attainment and Wages

Lorraine Dearden*, Javier Ferri† and Costas Meghir‡

December 2000

Abstract

The paper examines the effects of school pupil-teacher ratios and type of school on educational attainment and wages using the British National Child Development survey (NCDS). The NCDS is a panel survey which has followed a cohort of individuals born in March 1958, and has a rich set of background variables recorded throughout the individuals life. The results suggest, that once we control for ability and family background, the pupil-teacher ratio has no impact on educational qualifications or on male wages. It has an impact on women's wages at the age of 33, particularly those of low ability. We also find evidence that those who attend selective schools have better educational outcomes and in the case of men, higher wages at the age of 33. The impact is higher for the type of individuals who are less likely not to attend selective schools, but for whom a comparison group does exist among those attending.

Acknowledgements

We thank two anonymous referees, Christian Dustmann, Paul Johnson, James Heckman, the editor Robert Moffitt, Steve Pischke, Chris Pissarides and Petra Todd for useful comments and stimulating discussions. Participants in seminars at the CREST-INSEE and CEP-LSE provided useful comments. Costas Meghir's work on this paper was largely undertaken while he was visiting the CREST - INSEE in Paris. Lorraine Dearden's work on this paper was funded under Leverhulme Trust Research Grant F/386/G. We thank the CREST, the ESRC Centre for the Microeconomic Analysis of Fiscal Policy at the IFS, and the Leverhulme Trust for financial support.

*Institute for Fiscal Studies.

†Universitat de València.

‡University College London and Institute for Fiscal Studies.

1. Introduction

This paper examines the impact of measures of inputs into schooling (often refereed to as "school quality") on educational attainment, hourly wage rates at 23 and 33 years of age and on employment. We use a unique data set for this purpose which due to its rich array of characteristics, allows us to address many of the concerns raised in the school quality literature. This data set follows a cohort of individuals born in a week of 1958.

There is much controversy over whether particular aspects of school quality which are directly affected by government policy have significant effects on an individual's future educational achievement and earnings. The measures of school quality or school inputs which are typically central to this debate include pupil-teacher ratios, expenditure per pupil, and measures of teacher quality such as average teacher salaries. Most of the literature looking at school quality stems from the US and began with the publication of the Coleman Report in 1966 (Coleman et. al. (1966)). The controversial finding of this report was that measured school quality had very little effect on pupil achievement once family background and school composition effects had been taken into account. The subsequent US literature looking at this issue has, on the whole, tended to confirm this somewhat surprising finding or at best found only weak effects of school quality on pupil achievement (see for example, Hanushek (1986) and Hanushek, Rivkin and Taylor (1996)).

As Moffit (1996) points out there has also been a separate strand of the school quality literature which has instead focused on the impact of school quality on later earnings. The findings from this strand of the school quality debate have on the whole found significant impacts of school quality on later earnings in distinct contrast to the pupil achievement literature (see for example the papers by Johnson and Stafford (1973) and Card and Krueger (1992)). Some quite recent contributions to this literature have, however, found no significant effects (for example Betts (1995) and Heckman, Layne-Farrar and Todd (1996a)). There are a

number of differences between the various studies in type of data and in the way its used that could be at the root of the differences.

A possible explanation for the different findings is that the impact of school quality has been declining over time and/or is less important for younger workers than older workers. Most of the analyses looking at pupil achievement have focused on relatively young cohorts of individuals born in the 1950s or later while they are early in their career whereas the studies focusing on earnings have tended to concentrate on older cohorts of individuals who are later in their working life (aged 30 or above). Card and Krueger (1992), for example, focus on a cohort of individuals born in the 1920s, 1930s and 1940s aged between 30 and 60 years in 1980 whereas the study by Betts (1995) focuses on men aged 32 years or younger. Loeb and Bound (1996) look at the issue of whether school quality effects on achievement have been declining over time by examining a cohort of men born in the US in the 1920s, 1930s and 1940s. They find that state level measures of school quality have a significant effect on pupil achievement for this group of individuals. They argue that this finding may in part reflect differences across cohorts but also may reflect the extent of aggregation involved in measuring school inputs in their study. Betts (1996a) looks at the issue of whether school quality effects increase with age and labor market experience. He uses both individual level and state level census data and finds that there is no evidence of age dependence.

Another explanation relates to the importance of factors like family background and ability in determining choice of school, as well as pupil achievement and earnings. If this is not taken into account then the estimated relationship between school quality, and earnings and/or achievement, may be spurious. Very few studies in the school quality literature have directly addressed this problem because of lack of suitable data. The original Coleman Report explicitly controlled for individuals' socio-economic characteristics as well as school composition and found that these factors were much more important determinants of pupil achievement than quality measures. Two recent studies have used sibling (Altonji and

Dunn (1996)) and twin (Behrman, Rosenzweig and Taubman (1996)) data to look at this issue. If some siblings or twins attend different schools then the data can be used to look at the impact of differences in school quality on differences in earnings. In these models unobserved family effects with the same impact on all siblings will be differenced out. Altonji and Dunn (1996) find that school quality effects on earnings increase when such family fixed effects are controlled for, whereas Behrman, Rosenzweig and Taubman (1996) find that college quality effects on earnings are decreased.

In this paper we use data from England and Wales[1] to examine the impact of the pupil teacher ratio and type of school on educational achievement, employment and hourly wages at two different ages (23 and 33). We use a unique data set, the National Child Development Survey (NCDS), which is a continuing longitudinal study of all subjects living in Great Britain who were born in the week between 3 and 9 March 1958. There have been five follow up surveys for this cohort, the latest in 1991 when the individuals were aged 33. The surveys have detailed information on the individual's educational achievements (both school and post-school), family background and labor market history. Importantly, for the purpose of this paper, the data also has information obtained from the individual's school at the ages of 7, 11 and 16 on measures such as the pupil-teacher ratios, class sizes, type of school (e.g. government selective, government non-selective or private, single-sex or co-educational) results of numerous ability tests undertaken by the individuals at the time of each of the follow-up surveys as well as the family's financial circumstances and composition.

The paper looks at the impact of the pupil-teacher ratio and school type on educational achievement and earnings at two points in the life-cycle - age 23 and 33. The importance of controlling for ability and family background which are often unobserved, can therefore be assessed directly. The age dependence of the

[1]We exclude individuals from Scotland because we use local education authority data in the paper and this is only available on a consistent basis for England and Wales. Also the Scotish schooling system differs from that operating in England and Wales.

impact of the pupil-teacher ratio can also be explicitly examined.[2]

Other studies that consider the effects of School Quality on outcomes are Dustmann, Rajah, Van Soest (1997) who focus on the effects of school quality on continuation of education beyond 16, Robertson and Symons (1996) who attempt to measure the effects of peer groups in school performance and Feinstein and Symons (1996) who study attainment in secondary schools. Our focus is on outcomes later on, i.e. on the ultimate educational attainment and on labor market performance.

In Section 2 of the paper we outline our methodological approach. In Section 3 we take a closer look at the NCDS data used in the paper. In Section 4 of the paper we discuss our empirical results. Section 5 concludes and considers the important policy implications of our analysis.

2. Methodological Approach

Our methodology involves a sequential approach. We begin by examining the effect of school quality measures and other factors on educational attainment. We then examine the effects of our school quality measures on wages at two points in the life-cycle ten years apart, conditional on the highest qualification obtained up to that point. Finally we look at the impact of school quality on the probability of being employed. By taking this sequential approach we can assess the effects of school quality on outcomes through three main channels: educational attainment, the level of wages and the returns to potential experience. We also consider the impact of school inputs on wages without conditioning on qualifications. This allows us to measure the total impact, comprising of the direct effect and the effect that works through educational qualifications. We also examine the impact

[2]In an earlier version of the paper we also considered the impact of the expenditure per pupil and the average teacher salaries in each local authority, on the outcomes. We found that these had no clear impact and were always jointly insignificant. Multicollinearity seemed to be responsible for some uninterpretable and imprecise results. Thus we no longer report results based on these input measures. Our earlier results are available upon request.

of the pupil-teacher ratio in primary and in secondary school as well as the type of school such as single sex schools, private schools and different types of government schools (selective and non-selective).

There are a number of important endogeneity issues that need to be addressed in looking at the impact of school quality on both educational attainment and earnings. First, parents with a greater interest in their child's education may locate close to schools they consider better. They may use the pupil-teacher ratio as a factor. They may also choose single sex schools for a girl and a mixed school for boys simply because this is thought to be 'best'. Since an active interest in the child's education may lead to better educational attainment and higher earnings, such self-selection may generate an upward bias on school quality measures. If this is the case then our estimate of the pupil-teacher ratio effect may be too large due to non-random assignment to schools. Alternatively a downward bias may be generated if parents whose children attend the better schools invest less of their time on their child's education. Second, some types of schools select pupils by ability, which in itself is likely to imply that pupils from such schools will perform better and obtain higher qualifications. This is certainly the case for most private schools and for the state Grammar schools. In the two examples given above, the bias is generated by the active choices of parents and schools.

Another source of bias may originate in the way that education is financed in the UK. In England and Wales the responsibility for schooling is shared between Central government and Local Education Authorities (LEAs). In 1974 there were 117 LEAs in England and Wales. Central government provides LEAs with resources to fund education (and other local services) and LEAs with an economically disadvantaged population receive higher government grants[3]. It is up to the LEA to determine how much of this money they allocate to schools.

[3]The Education Reform Act of 1988 allowed schools to opt out of LEA control. Schools can now become grant maintained which means that they receive funds directly from central government if they receive approval from a ballot of parents. This reform was not in operation for the NCDS cohort.

Private schools do not receive government money and are funded by endowments and fee paying pupils. Some of the extra education money given to LEAs with a more disadvantaged population is spent on providing things like free school meals to disadvantaged pupils and this forms part of the education budget. It may also be spent on reducing the pupil-teacher ratio. But children from deprived neighborhoods may perform worse, thus generating a downward bias on the effect of the pupil-teacher ratio on educational attainment. Finally, the local socio-economic environment that the child lives in may affect educational attainment and/or future earnings (e.g. through role model or peer effects). If such characteristics are also correlated with measures of school quality then omitting them may generate a further source of bias.

In the absence of some obvious experimental framework as in Krueger (1999) allocating pupils randomly to different types of school one way of solving such endogeneity problems would be to use some instrumental variables procedure. This requires exclusion restrictions. However, in this context it is very hard to argue that any of the available background, family or local variables determine school allocation but not educational attainment and wages. All such variables are potential inputs in the production of human capital.

In our view the best way to deal with the endogeneity issues with data such as ours is to control for the variables that are likely to be driving school selection before the relevant treatment occurs. Hence on the basis of our discussion above we need:

1. Family background variables to control for differences in parental circumstances and tastes for education (X_1).

2. Individual characteristics and test scores to control for differences in ability (X_2).

3. Characteristics of the local authority to control for variation in education expenditures related to the amount of deprivation in the area (X_3).

4. Neighborhood characteristics (X_4).

The NCDS data used in this study explicitly allows us to control for all of these effects which makes the matching approach we use here credible. Such rich data has not been previously used in the school quality literature.

Formally, we wish to estimate the effect of school input variables (Q_i) on schooling or education (s_i) and (log) wages (w_i). We assume that any selection takes place on the basis of observable variables $Z_i = [X_{1i},\ X_{2i},\ X_{3i}, X_{4i}]$. We assume that conditioning on the observables Z_i is sufficient to control for the endogenous choice of school type and input (Q_i). More formally we have the following simple sequential model for two time periods ($j = 1, 2$):

$$s_{ji} = \delta_{j0} + \delta'_{j1} Q_i + \delta'_{j2} Z_i + u_{ji} \qquad j = 1, 2 \tag{2.1}$$

$$w_{ji} = \beta_{j0} + \beta'_{j1} Q_i + \beta'_{j2} Z_i + \rho^{'} s_{ji} + v_{ji}, \qquad j = 1, 2 \tag{2.2}$$

where the δ_{j1} and β_{j1} measure the effect of school quality on our outcome variables s_{ji} and w_{ji} in 1981 ($j = 1$) and in 1991 ($j = 2$) respectively. In this model we assume that individuals who are the same in the observable dimension Z_i but who attended schools characterized by different values of Q_i do not to differ on average in the unobserved dimension u_{ji} and v_{ji}. Formally this means that $E(u_{ji}|Q_i,\ Z_i) = E(u_{ji}|\ Z_i)$ and $E(v_{ji}|Q_i,\ Z_i, s_{ji}) = E(v_{ji}|\ Z_i)$.

We can extend this simple model to allow the effects of school quality to be heterogeneous in the population (i.e. $\delta_{j1i} = \delta_{j1} + \upsilon_{ji}$ where $Var(\upsilon_{ji}) > 0$ and $\beta_{j1i} = \beta_{j1} + \varepsilon_{ji}$ where $Var(\varepsilon_{ji}) > 0$). We assume that although the effects of Q_i may be heterogeneous in the population (i.e. $Var(\upsilon_{ji}) > 0$ and $Var(\varepsilon_{ji}) > 0$), only the average population values of δ_{j1i} and β_{j1i}, conditional on the observables, are known by the person undertaking the choice of Q_i for the child. In other words, we assume that the parent does not know the precise return of Q_i to his own child. Formally we assume that $E(\upsilon_{ji}|Q_i,\ Z_i)Q_i = E(\upsilon_{ji}|\ Z_i)Q_i$ and $E(\varepsilon_{ji}|Q_i,$

$Z_i, s_{ji})Q_i = E(\varepsilon_{ji}|\ Z_i)Q_i$. Hence the average school quality effects $\delta_{j1}$and β_{j1} can be identified from the following sequential regression models

$$s_i = \delta_{j0} + \delta'_{j1}Q_i + \delta'_{j2}Z_i + \delta'_{j3}(Z_i \otimes Q_i) + u_{ji} \tag{2.3}$$

$$w_i = \beta_{j0} + \beta'_{j1}Q_i + \beta'_{j2}Z_i + \beta'_{j3}(Z_i \otimes Q_i) + \rho' s_i + v_{ji} \tag{2.4}$$

where $E(u_{ji}|Q_i,\ Z_i) = 0$ and $E(v_{ji}|Q_i,\ Z_i, s_{ji}) = 0$. In equations (2.3) and (2.4) the coefficients δ_{j3} and β_{j3} capture the heterogeneity in the effects of Q_i. The arguments used here are similar to the arguments made for matching estimators (see Heckman, Ichimura and Todd, 1997) although our approach is more restrictive in the sense that we use linear matching.

As pointed out in the model above, we present results for wages with education achieved up to that point as a control variable. This is done to isolate the impact of the pupil-teacher ratio and type of school over and above their effect on the qualification obtained. However, we also present results for wages without the educational qualifications included.

When using matching estimators one should control for variables in the information set of agents making the decision that will affect treatment (here the pupil-teacher ratio and type of school). When we consider the primary school pupil-teacher ratio we condition on parental background and local characteristics describing the broader area and the local neighborhood. Test scores at 7 are obtained at the early stages of primary school and they may already contain the effect of the primary pupil teacher ratio. Thus we report results with test scores at seven included and not included.

In the next part of the empirical results we focus our attention on the impact of the secondary school pupil-teacher ratio and type of secondary school. When we do this we control for test scores at 7 and 11 as well as the other background variables. Of course the test scores are themselves probably endogenous and a function of parental background and earlier school inputs. Nevertheless, when the secondary schooling decision is made the test scores at 7 and 11 are known

by the decision-makers (parents and schools) and consequently the selection takes place possibly using these variables. Omitting them may confound the impact of the subsequent school inputs with the selection on these ability scores. In fact our assumption that all selection is on observables (the matching assumptions) relies on including all those observables variables that are likely to affect the treatment decision. The fact that our data allows us to do this is the strength of our approach. However, we also report results that do not include some or all of the test scores.

Given the above matching assumptions the wage equation can be estimated by Ordinary Least Squares (OLS). The standard errors must be estimated using White's (1982) adjustment for heteroskedasticity, if only because the heterogeneous returns imply that the variance of u_{ji} and v_{ji} will depend on Q_i.

For educational qualifications we use an ordered probit. The basic assumption is that we can explain all education choices using a single index given by the right hand side of the regression in equation (2.1). Cameron and Heckman (1998) provide conditions under which this is a valid approach. We also need to assume that our errors are homeskedastic. This assumption does not sit comfortably with the possibility of heterogeneous responses which depend on unobservables. A multinomial choice model incorporating seven education levels is, however, computationally complex. Instead we assess the validity of the ordered probit assumption by comparing the results we get with those obtained by a simple probit model where the dependent variable is "obtain some qualifications" versus none. We also look at the top of the educational distribution by estimating another probit model for obtaining a degree versus no degree. Under the null hypothesis, the results of the two approaches should be similar. Under the alternative they would differ because the probit does not impose the single index assumption across all education choices.

An important issue, particularly when we consider the causal impact of school

type,[4] is whether the composition of the population going to different types of schools is such that we can actually form comparison groups. In a separate section we examine this issue and we also construct a non-parametric matching estimator for the impact of school type, taking particular care to impose common support when we compare children in selective and non-selective schools. We do this because we feel that school type may reflect important inputs in education.

3. The Data

For this study we use, the National Child Development survey (NCDS), which charts the development of all children born in a week of March 1958. The data set contains information on the parents and a wealth of information on the subjects at 6 points in the life-cycle: birth, 7, 11, 16, 23 and 33. The data contain information on family background, ability test scores, on the characteristics and types of school attended at each interview date, on educational qualifications and training, on the area of residence at each survey date, on wages and hours worked (at ages 23 and 33) and occupational information. The initial sample covered 17,414 individuals, but there has been quite a lot of attrition since. In the subsequent waves the sample sizes were 15,468, 15,503, 14,761, 12,537 and 11,409 respectively. In 1978 exam results were obtained for 14370 subjects directly from their schools. Dearden et. al. (1997) show that attrition has tended to take place among individuals with lower ability and lower educational qualifications[5]. The sample used in this paper

[4]There are three types of state schools. Comprehensive schools, which are non-selective secondary school with an academic curriculum; secondary modern schools, which are lower ability secondary schools; and Grammar schools which are selective state schools where pupils are admitted on the basis of an exam at 11. Comprehensive schools were first introduced in 1968 and were meant to replace selective education in the state sector. This reform was still continuing in 1974 and indeed some LEAs still have selective state education today. These issues are considered in detail in the paper by Harmon and Walker (1997). The final type of schools are private schools, known in the UK as 'Public' schools. The comprehensive type is the omitted category in all our regressions.

[5]See Dearden et. al. (1997) Tables 2a and 2b, pp. 53-54. The nature of attrition in the NCDS sample is discussed in detail in the documentation accompanying the various surveys.

under-represents individuals in the bottom of the ability distribution.[6] Attrition need not bias our results however, to the extent that it depends on observables only. Given the large array of characteristics relating to ability and background we have reasonable grounds to believe that in our analysis attrition is exogenous, given the observables.

3.1. Variables used in the analysis

School quality variables >From the NCDS surveys we observe the pupil-teacher ratio in the child's school at 11 (end of primary school) and 16 (end of compulsory schooling) both collected directly from the school. Class size often reflects the needs of the particular class since schools use streaming by ability and they tend to place children with greater learning difficulties in the smaller classes. Since we cannot directly control for this with the NCDS data we decided to use the overall pupil-teacher ratio in the school which does not suffer from this endogeneity problem. While it is still true that schools in more deprived areas will tend to have lower pupil-teacher ratios because of the extra funding they receive from central government, the data allows us to directly control for the level of deprivation in the child's immediate neighborhood.

Rather than excluding pupils who went to private schools as is often done in school quality studies (e.g. Card and Krueger (1992)), we keep them in the sample and include controls for the type of school in some of the regressions. With our large array of information on test scores and family background we can control for the main relevant factors that govern selection into schools. We also control for whether the secondary school is single sex or not. This dimension of schooling is an important issue in the UK.

[6] For example, only 16.47 per cent of individuals (15.2% of men and 17.0% of women) in the sample used in this paper were in the bottom quintile of the maths ability test undertaken at the age of 7 whereas 22.41 per cent of individuals (24.6% of men and 20.4% of women) were in the top quintile of this maths ability test (see Table 0.2 in the appendix)

Family Background Variables We use data from the second and third waves of the survey to construct variables identifying the fathers' occupation in 1974; the years of full-time education undertaken by the child's mother and father; a variable identifying individuals who had no father figure in 1974; whether the child was receiving free school meals in 1969 and/or 1974; whether the family was experiencing serious financial difficulties in 1969 and/or 1974; and the number of siblings and older siblings the individual had in 1974. We also included indicators of the parents' interest in the child's education as assessed by the primary school teacher.

Ability Variables We utilize the results from reading and mathematical ability tests undertaken when the person was aged 7 and 11. From these reading and mathematical ability tests we construct dummy variables ranking the individual's results in each of the tests by quintiles[7].

Local Authority and Neighborhood Characteristics All regressions presented include indicators for the ten broad administrative regions as well as a dummy for the inner London and outer London regions. We also include a set of variables that describe the immediate social environment in the neighborhood of the child, as well as the overall deprivation level of the local authority (municipality). These variables are taken from the 1971 census, and relate to the enumeration district and to the local authority where the child lived in 1974. The enumeration district is small enough to pick up the characteristics of the child's immediate neighborhood. The local authority variables, cover a much larger area and are included to control for the fact that central government grants to local authorities (including education grants) relate to the level of LEA deprivation. Finally we include a set of variables describing the size of the local authority and

[7]We choose quintiles, as 20 per cent of individuals in 1965 when the tests were undertaken obtained maximum marks in the reading ability test. The quintiles refer to quintiles at the time the test was taken and not in our final sample (see Table 0.2 in the Appendix).

its 'needs'[8].

Wage and Education Data We use data from the fourth and fifth waves of the survey to construct real hourly gross wage data measured in 1995 prices. We limit our sample to individuals who are employees at the time of the 1981 and/or 1991 survey. Since all individuals in the sample are born in the same week of March 1958 age (or potential labor market experience) is controlled for in all of our models. Our other outcome variable is highest educational qualification (based on both school *and* post-school qualifications) at the age of 23 in 1981 and 33 in 1991. A full description of how this is constructed is given in Table 0.1 in the Appendix.

Individuals often work in different areas to the one in which they attended school. When estimating the wage equations we include nine region of schooling dummies at age 16. In the paper we estimate two sets of wage equations, one at the age of 23 and one at the age of 33. In each case we also include the region of residence dummies for that age to control for the effects of the local labor market. We also include the highest qualification obtained by that age.

3.2. Descriptive information from the final sample

Table 3.1 shows the educational achievement of for males and females by the age of 33. In the last two columns we show the real log hourly wage in 1991 (in January 1995 prices) for each of the educational categories for the subsample of individuals for whom we have valid wages data.

A significant number of individuals in this cohort have ended up with no serious qualification, i.e. over 40 per cent of men and over 50 per cent of women if we

[8]The enumaration district variables we include are: the proportion of owner occupiers and of council tenants, the average persons per room, the proportion lacking an inside WC, the proportion unemployed and the proportion of unskilled manual workers. These variables are also included at the Local Authority level. We also include the primary and secondary school populations in 1969 (age 11) and 1974 (age 16) (per 10 of the Local Authority Population) and the local authority population in 1969 and 1974 (divided by 10,000,000).

	Frequency (%)				Log Hourly Wages	
Highest Education Qualification:	Females		Males		Females	Males
No Qualifications	173	(7.2)	126	(5.7)	1.32	1.67
Other	331	(13.7)	217	(9.7)	1.40	1.79
Lower Vocational	795	(33.0)	566	(25.4)	1.50	1.93
Middle Vocational	304	(12.6)	488	(21.9)	1.71	1.99
A Levels	165	(6.8)	130	(5.8)	1.83	2.19
Higher Vocational	335	(13.9)	373	(16.7)	1.96	2.19
University Degree	309	(12.8)	332	(14.9)	2.16	2.37

Table 3.1: Educational Qualifications and log Wages in 1991: Men and Women

take the first three categories. Nevertheless the rest have some sort of qualification which, as we see from the log wages, are associated with large pay advantages. As an interesting aside, note that in the raw data there are very large male/female wage differentials at all educational levels, although these are likely to be explained in part by the differing levels of labor market experience at the age of 33.

>From Table 0.2 in the Appendix we see that the pupil-teacher ratio is much more dispersed for primary schools than for secondary schools. For the largest sample, average primary pupil-teacher ratio is 23.8 with a standard deviation of 9.5 compared to an average of 17.1 and a standard deviation of 2.0 for secondary schools. In Table 3.2 we break down the pupil-teacher ratio in secondary schools by school type and sex. There are four types of school. The 1968 *Education Act* allowed LEAs to establish non-selective government schools called 'comprehensives'. Prior to this Act, pupils had to sit an exam at 11. The successful pupils (those in the top 10 to 20 per cent) went on to a grammar school while the rest attended a secondary modern school.[9] Our cohort went through the education system as this reform was being implemented. In fact grammar schools still survive in some areas today and their revival is at the centre of the education policy debate. The final category are the private or independent schools, known

[9]There were also technical schools, though these were not very common. Technical schools provided a more vocationally oriented education up until the age of 16.

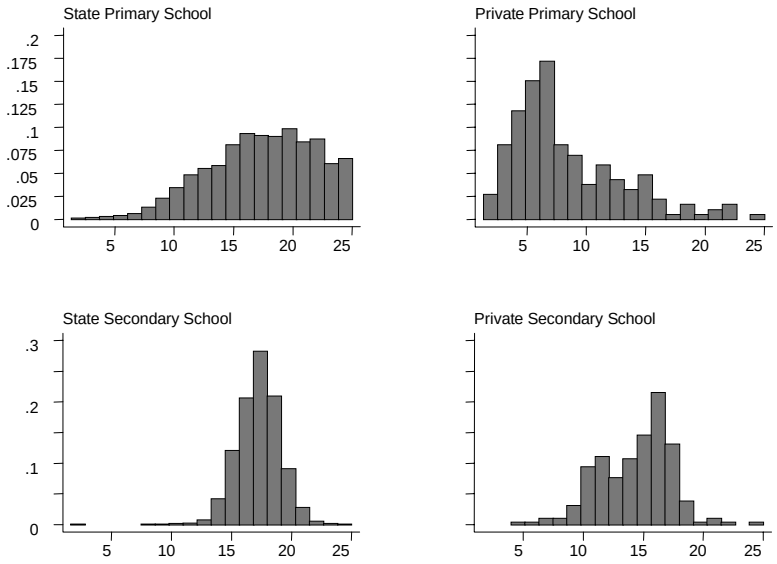

Figure 3.1: The pupil-teacher ratio in primary and secondary school for the state and the private sector.

in the UK as 'public' schools. From Table 3.2 it seems that private schools have the lowest pupil-teacher ratio though the degree of dispersion is relatively large compared to the government sector. Secondary modern schools have the highest average pupil-teacher ratio. Women went through schools with a slightly higher ratio, particularly in grammar schools. For a more complete picture of the variability of the pupil-teacher ratio we present a histogram by state or private schools for the primary and secondary schools in Figure 3.2

In our sample about 57 per cent of children attend comprehensives, 25 per cent secondary moderns, 13 per cent grammar schools and 5 per cent private schools.

	Males		Females	
Type of School	Mean	(S.D.)	Mean	(S.D.)
Comprehensive	17.1	(1.6)	17.3	(1.9)
Secondary Modern	18.3	(1.7)	18.3	(1.6)
Grammar School	15.9	(1.5)	16.3	(1.3)
Private	14.5	(2.5)	14.6	(3.2)
All schools	17.1	(1.9)	17.2	(2.1)

Table 3.2: Pupil-teacher ratio in secondary schools (1974 NCDS)

Comprehensive and secondary modern schools are usually mixed sex (90 per cent and 75 per cent mixed respectively). Only 33 per cent of grammar schools and 20 per cent of private schools are mixed sex.

4. Empirical Results

In earlier versions of our work we carried out a number of experiments with various school input measures. These included the expenditure per pupil, the average teacher salaries and the pupil-teacher ratio in both primary and secondary school at the LEA level. The expenditure measures and the teacher salaries were never jointly significant and the estimates were not precise in any of the outcome equations (qualifications and wages at 23 and 33) probably because of lack of sufficient variation within the broad LEA regions. In the Tables presented below we do not report any of the results with these variables included as in all cases we could not identify any impact on education attainment or wages[10].

4.1. The pupil-teacher ratio in primary school

In Table 4.1 we present the estimated impact (coefficients and marginal effects) of the pupil-teacher ratio in primary school on qualifications by the age of 33[11]. The results are from an ordered probit which controls for parental background

[10]These results are available from the authors.

[11]We also looked at the determinants of highest educational qualifications at 23 and highest school qualifications, but this did not change the results obtained.

	Men		Women	
Specification	1	2	1	2
Pupil-teacher ratio (primary)	-0.005	-0.002	-0.003	0.002
	(0.002)	(0.0025)	(0.0023)	(0.0024)
Marginal effect on obtaining some qualification ×100	0.7%	0.2%	0.3%	-0.2%
Test scores at 7	No	Yes	No	Yes

Notes:
(i) Regressions include region of schooling and residence, family background, local authority and census enumeration district characteristics.
(ii) Asymptotic standard errors in parentheses.

Table 4.1: The impact of the primary pupil-teacher ratio on educational attainment

variables, the local authority and neighborhood variables, and the region of residence. Results with and without the test scores at 7 are included. The results presented in Table 4.1 show that the impact of the primary pupil-teacher ratio is precisely estimated to be zero when test scores at 7 are included (specification 2) and very small when they are excluded (specification 1). This result is also robust to whether we control for type of primary school (private or government). This result is achieved, despite the very large variability of the pupil-teacher ratio in primary school (see Figure 3.2).

We also find that the primary pupil-teacher ratio has no effect on wages at 23 and 33 as shown in Table 4.2. Including the primary school pupil-teacher ratio together with the corresponding secondary school ratio reduced precision of our estimates but did not significantly affect the estimated impact of the secondary school pupil-teacher ratio on both education and wages. In what follows we concentrate our attention on the impact of the secondary school pupil-teacher ratio as well as variables describing the type of secondary school attended.

	Men		Women	
Specification	1	2	1	2
Pupil-teacher ratio (primary)	-0.0006	-0.0003	-0.0014	-0.0013
	(0.001)	(0.001)	(0.0014)	(0.0014)
Test scores at 7	No	Yes	No	Yes

Notes:
(i) Regressions include region of schooling and residence, family background, local authority and census enumeration district characteristics.
(ii) Asymptotic standard errors in parentheses.

Table 4.2: The impact of the primary pupil-teacher ratio on wages at 33

Specification	1	2	3	4
Pupil-teacher ratio 1974	-0.065	-0.046	-0.015	-0.016
	(0.012)	(0.013)	(0.014)	(0.014)
Single sex school	0.223	0.070	-0.015	-0.044
	(0.057)	(0.058)	(0.06)	(0.064)
Secondary modern school			-0.15	-0.108
			(0.059)	(0.060)
Grammar school			0.68	0.31
			(0.083)	(0.088)
Private school			0.715	0.559
			0.126	(0.128)
P-value: Local area characteristics	0.000	0.054	0.01	0.09
P-value: Family Background	0.000	0.000	0.000	0.000
P-value: Test Scores at 7		0.000		0.000
P-value: Test Scores at 11		0.000		0.000
P-value: Test Scores at 7 & 11		0.000		0.000
Log Likelihood	-3738.5	-3523.0	-3689.9	-3507.7
Pseudo R^2	0.032	0.131	0.090	0.135
Number of observations	2232	2232	2232	2232
Asymptotic standard errors in parentheses				

Table 4.3: School Quality and Male Educational Qualifications

4.2. The effect of the pupil-teacher ratio and school type on educational qualifications

4.2.1. Men

The results for educational qualifications for men are presented in Table 4.3. They are based on an ordered probit for qualifications obtained by the age of 33[12]. The highest qualification (degree) is given the highest rank. Thus a positive coefficient increases the chance of a higher qualification.

In the table (and the following tables) we present four sets of results which all

[12] Again, the results when we instead used highest qualification at 23 and highest school qualification were essentially the same.

include regional dummies, the local area characteristics described in the previous section and the parental background variables.[13] The local area characteristics include characteristics of the municipality (Local authority) and the census enumeration district (census track) which should capture the effects of local deprivation and general level of resources. At the bottom of each table we list the *p-value* for the additional controls included in our 4 different specifications. When no *p-value* is presented this indicates that these additional controls were not included in the regression.[14]

In specification 1, no test scores are included. In this regression the pupil-teacher ratio measured at 16 years of age has a large and significant effect on educational attainment. The estimated marginal effect using the mean characteristics of those who do not obtain qualifications, suggest that an increase in the pupil-teacher ratio by one increase the chance of ending up with no qualifications by 7.5 percentage points. The other notable result based on this regression is that being in a single sex school has a significant and positive effect on men's educational attainment. The probability of ending up with no qualifications is 3.4 percentage points lower on average for those attending a single sex school compared to those attending a co-educational school.

In specification 2 we control for test scores at both 7 and 11. All these extra controls are highly significant and reduce both the pupil-teacher ratio effect and the single sex school effect. The impact of the pupil-teacher ratio is now reduced substantially but is still significant. The impact of an increase of one in the pupil-teacher ratio increases the probability of having no qualifications by 4.7 percentage points while at the other extreme the probability of obtaining a degree

[13]The family background variables include father's social class, mothers and fathers years of education, whether the parents were in serious financial difficult when the child was 11 and when the child was 16, indicators for the religion of the child when he/she was 23 (parental religion is not available), whether the child was receiving free school meals at 11 and at 16, the number of siblings and the number of older siblings. The test scores relate to reading and mathematical ability. They are a series of dummy variables identifying the child's quintile in the distribution of scores.

[14]We always include regional indicators but we do not report the p-values.

declines by 0.7 percentage points (from -2.0 percentage points in the previous specification).[15] At the same time the negative effect of being in a single sex school on the probability of obtaining any qualification decreases to 1.1 percentage points (from 3.4 percentage points).[16]

In specification 3 we include school type variables but exclude test scores. In specification 4 we include both school type variables and test scores. In a sense these are the results most comparable to those from the US where typically private schools are excluded from the data. Here we keep them in the sample but we control for them. When we do this, the pupil-teacher ratio and the single-sex school effect both decline further. This is despite the fact that there is considerable variation in the pupil-teacher ratio within both the state and the private sector.[17] As can be seen from the Table, the standard error of the estimate hardly increases.[18] The effect of increasing the pupil-teacher ratio by one increases the chance of having no-qualifications by 1.8 percentage points and has practically no impact on the chances of obtaining a degree. Hence we can detect an impact on individuals with characteristics that lead them to have low or no qualifications, but this impact is not statistically significant. The impact on educational qualifications of attending a state selective school (grammar) or a private school is large and significant even after controlling for tests at 11 which are the primary selection mechanisms into these schools.

We carried out a number of experiments in which we checked whether the pupil-teacher ratio had a larger effect in state schools or for children of different levels of ability. None of these interaction effects had any sizeable impact or was in any way significant. We also checked whether the pupil-teacher ratio had a larger impact on school qualifications and qualifications at 23 (rather than highest qualifications at 33 reported in the Table). Our conclusions are the same for these

[15] These probabilities are evaluated at the average characteristics of the relevant group.

[16] Conditioning on tests scores at 7 only does not alter this result.

[17] In a simple regression of the pupil-teacher ratio on school type, the school type explains 20% of the variance.

[18] A full set of results for specifications 3 is given in Table ?? in the Appendix.

outcome variables as well.

Does the single index assumption bias the results? There is the question of whether the ordering of education levels and the imposition of a single index model for educational attainment is biasing the results. In particular it is an issue whether the impact of the pupil teacher ratio at the low end of the educational distribution is biased by forcing it to explain the impact at the upper end with the same coefficient on the linear index. To test for this we compare the coefficients and marginal effects derived from the third specification of Table 4.3 with the ones derived using a simple probit of obtaining no qualification versus obtaining some qualifications. The probit is less restrictive than the ordered probit since it does not force the same index to explain the progression between the higher levels of qualification. It does, however, nest the ordered probit as far as the estimation of the probability of the first or last category is concerned. When we use the probit, the marginal effect of a change in the pupil teacher ratio on not obtaining a qualification is zero (-0.08 percentage with standard error 0.1 percentage points). Checking the upper end of the education distribution by estimating the probability of obtaining a degree versus not obtaining one, the marginal effect of the pupil teacher ratio is -0.2 percentage points (standard error 0.26). Thus there is no evidence to suggest that the single index assumption is seriously distorting the results in the sense that it is masking a strong effect either at the top or the bottom of the distribution. The single index assumption does, however, substantially improve precision.

4.2.2. Women

The results for womens' educational attainment are presented in Table 4.4. The overall pattern of results for women is very similar to those of men. The pupil teacher ratio has a significant and large impact when we do not control for ability, implying a marginal effect of 4.9 percentage points on the probability of ending up with no qualifications and -0.7 percentage points of ending up with a university

Specification	1	2	3	4
Pupil-teacher ratio 1974	-0.051	-0.027	-0.018	-0.0113
	(0.011)	(0.011)	(0.012)	(0.0121)
Single sex school	0.36	0.212	0.090	0.080
	(0.053)	(0.054)	(0.060)	(0.060)
Secondary modern school			-0.074	-0.027
			(0.057)	(0.058)
Grammar school			0.82	0.421
			(0.75)	(0.079)
Private school			0.606	0.391
			(0.116)	(0.12)
P-value: Local area characteristics	0.000	0.000	0.000	0.000
P-value: Family Background	0.000	0.000	0.000	0.000
P-value: Test Scores at 7		0.000		0.000
P-value: Test Scores at 11		0.000		0.000
P-value: Test Scores at 7& 11		0.000		0.000
Log Likelihood	-4112.6	-3590.7	3790.1	-3573.6
Pseudo R^2	0.058	0.177	0.132	0.181
Number of observations	2412	2412	2412	2412
Asymptotic standard errors in parentheses				

Table 4.4: School Quality and Female Educational Qualifications

degree (specification 1). The size of the effect is reduced substantially to 2.4 percentage points on the probability of having no qualifications when we include test scores at 7 and 11 and family background (specification 2). Finally, as was the case for men, controlling for the type of school reduces this marginal impact to 1.3 percentage points which is not significant (specification 4). Similarly the large single sex school is small and insignificant in specification 4.

Finally to test whether the single index assumption is biasing the women's results we compared again our results with those from a probit for obtaining some qualification versus none and for obtaining a degree versus none. The marginal effect of an increase in the pupil-teacher on the probability of obtaining a qualification is 0 (standard error 0.05%). The degree probit implies a marginal effect of -0.1% (standard error 0.2%). Hence again there is no evidence that the single index assumption is leading us to the wrong conclusions.

Thus as for boys, the strongest evidence we have that school inputs might matter is in the impact of the type of school attended. Attending either a grammar school or private school seems to lead to better educational outcomes, even conditional on ability, family background and neighborhood effects. We are not

able to distinguish which aspect of grammar schools and private schools enhances educational outcomes. The fact that pupils in these schools seem to do better, even conditional on our observables, may have something to do with the way teaching is organized, or possibly with the type and quality of teachers that such schools attract. If this could be shown to be the case there are important lessons to be learned from such schools. An alternative possibility is that by selecting high ability pupils, the schools create an environment of highly motivated pupils generating strong peer pressure to achieve. This is a view expressed in Robertson and Symons (1996) and Feinstein and Symons (1997). If the latter is the reason for the success of such schools it is not easy to see what can be learned from such settings for the purpose of improving the overall educational outcomes in the population. We look more closely at the effect of school type on wages using propensity score matching techniques below.

4.3. Wages and the pupil-teacher ratio

We now consider whether educational inputs affect wages, both conditional on and not conditional on qualifications obtained. Better educational inputs may offer other qualities to a worker, enhancing the ability to learn at any qualification level. In looking at the effect of school quality variables on wages, we once again consider 4 specifications. In specification 1 we control for the individuals highest educational qualification, local area characteristics, family background variables and region of residence. In specification 2 we also control for ability tests undertaken at the ages of 7 and 11. In specification 3 we also control for the type of school attended in 1974 at the age of 16. Specification 4 is the same as specification 3, except that we no longer control for highest qualification.

4.3.1. Men

The first set of results, for males at 23, are shown in Table 4.5. The dependent variable is the real log hourly wage rate in 1981 (in January 1995 prices). All

Specification	1	2	3	4
Pupil-teacher ratio 1974	0.004	0.003	0.004	0.003
	(0.005)	(0.004)	(0.005)	(0.005)
Single sex school	-0.029	-0.020	-0.0260	-0.027
	(0.021)	(0.020)	(0.022)	(0.022)
Secondary modern school			-0.002	-0.001
			(0.0210)	(0.022)
Grammar school			0.029	0.024
			(0.029)	(0.028)
Private school			-0.006	-0.010
			(0.0434)	(0.044)
Highest Education (No qualification is base group) by 1981				
Other	0.060	0.046	0.047	
	(0.033)	(0.033)	(0.033)	
Lower Vocational	0.122	0.101	0.102	
	(0.030)	(0.032)	(0.032)	
Middle Vocational	0.160	0.132	0.133	
	(0.029)	(0.320)	(0.032)	
A-Level	0.106	0.085	0.082	
	(0.038)	(0.041)	(0.041)	
Higher Vocational	0.182	0.158	0.158	
	(0.035)	(0.037)	(0.037)	
Degree	0.126	0.098	0.096	
	(0.041)	(0.043)	(0.044)	
P-value: Local area characteristics	0.000	0.000	0.000	0.000
P-value: Family Background	0.37	0.41	0.44	0.21
P-value: Test Scores at 7		0.23	0.25	0.15
P-value: Test Scores at 11		0.68	0.69	0.32
P-value: Test Scores at 7 & 11		0.34	0.40	0.017
R^2	0.109	0.119	0.120	0.106
Number of observations	1700	1700	1700	1700
Hetereoskedasticity consistent standard errors in parentheses				

Table 4.5: School Quality and Male Wages at 23

regressions control for region of residence at 23 and at 16 as well as for parental background and the local authority and the characteristics of the census enumeration district as outlined earlier. When we control for qualifications we use those obtained by the age of 23. [19]

The results are very striking. At 23 years of age, the principal determinant of wages are educational attainment. The influence of family background variables and test scores is not significant. The type of school has no obvious independent influence either. In fact the school type variables are jointly and individually insignificant at conventional levels of significance. The coefficient is both low and precisely estimated. Finally, the impact of the pupil-teacher ratio is very small and insignificant.[20] Good schooling is valuable to the extent that it leads to qualifications that are valued by the market. No other aspect of measured school quality seems to matter at this stage. Excluding qualifications (specification 4) does not overturn these results.

In Table 4.6 we present the results for the wages of this cohort measured when they were 33.[21] In this specification the relevant qualifications are those obtained by the age of 33. The full set of results for specification 3 is given in Table **??** in the Appendix. The pupil-teacher ratio still has no effect on wages[22]. However the ability scores at 7 and the school type and single sex indicators, are now significant[23], but family background remains insignificant conditional on qualifications. Hence it seems that school type does affect wage growth. This may reflect better access to on the job training or a better ability to learn by men who went through private or grammar schools (even conditional on test scores

[19] The full set of results are available from the authors on request.

[20] When we remove all controls the regression coefficient becomes -0.017 with a standard error of 0.004.

[21] We experimented with using individuals employed at both dates. The overlap is very large and this selection made little difference to the results but of course reduced precision slightly.

[22] When we exclude all controls the coefficient of the pupil teacher ration for wages at 33 is -0.04 with a standard error of 0.007.

[23] When we exclude the single sex indicator the school type effect is only significant at the 9% level. In this sample there seems to be considerable collinearity between the two.

Specification	1	2	3	4
Pupil-teacher ratio 1974	-0.005	-0.004	0.002	-0.0013
	(0.005)	(0.005)	(0.006)	(0.006)
Single sex school	-0.019	-0.035	-0.069	-0.069
	(0.024)	(0.024)	(0.026)	(0.028)
Secondary modern school			0.003	-0.013
			(0.03)	(0.027)
Grammar school			0.058	0.086
			(0.035)	(0.036)
Private school			0.195	0.22
			(0.052)	(0.054)
Highest Education (No qualification is base group) by 1991				
Other	0.048	0.007	0.010	
	(0.056)	(0.054)	(0.052)	
Lower Vocational	0.195	0.121	0.121	
	(0.053)	(0.052)	(0.052)	
Middle Vocational	0.231	0.142	0.143	
	(0.054)	(0.054)	(0.054)	
A-Level	0.393	0.270	0.253	
	(0.068)	(0.068)	(0.068)	
Higher Vocational	0.421	0.316	0.321	
	(0.056)	(0.056)	(0.056)	
Degree	0.566	0.433	0.422	
	(0.057)	(0.058)	(0.058)	
P-value: Local area characteristics	0.36	0.30	0.31	0.20
P-value: Family Background	0.52	0.53	0.53	0.12
P-value: Test Scores at 7		0.003	0.007	0.000
P-value: Test Scores at 11		0.12	0.09	0.000
P-value: Test Scores at 7 & 11		0.000	0.000	0.000
R^2	0.309	0.334	0.341	0.277
Number of observations	1523	1523	1523	1523
Heteroskedasticity consistent standard errors in parentheses				

Table 4.6: School Quality and Male Wages at 33

at 11 which is he principal selection mechanism for admittance to such schools). Removing qualifications from the regression (column 4) has no significant impact on the results. The school type results may be biased, however, if the type of children attending selective schools (private and grammar schools) are different to those attending non-selective schools. By different we mean that the observed characteristics of children in selective schools have little or no overlap with those of children attending non-selective schools (i.e. there is lack of common support). We consider this issue in detail in section 4.4 below.[24]

We have also considered interaction effects of the pupil-teacher ratio with low ability and with the type of schools. Such interaction effects are jointly insignificant. However, the pupil-teacher ratio impact for men who went through the secondary modern school is quite large (a -1.5 per cent effect on wages for an unit increase in the pupil-teacher ratio with a standard error of 1.3) although the effect is not significant.

4.3.2. Women

In Table 4.7 we present regressions for female wages at 23. All regressions include dummies for the region of residence and the region of schooling at 16 as well as the set of local authority and enumeration district characteristics described before and the parental background variables.

As for men there is no effect of the pupil-teacher ratio whose effect is in fact quite precisely estimated at zero.[25] The main difference with men is that for women the test scores at 11 are strongly related to hourly wage rates at 23. We also checked whether interaction effects with ability and type of schooling were important and found no significant differences across different groups. Female wages

[24] This could also be an issue when evaluating the impact of the pupil-teacher ratio on outcomes. We have investigated this by estimating a propensity score for being in a school with a small (below the mean) versus a large (above the mean) pupil-teacher ratio and undertaking nearest neighbour non-parametric matching (see Heckman, Ichimura and Todd (1997)). Our results are similar although precision is considerably reduced.

[25] In the regression with no additional controls the pupil-teacher ratio has a coefficient of -0.017 with a standard error of 0.004.

Specification	1	2	3	4
Pupil-teacher ratio 1974	-0.0032	-0.0001	0.0008	0.00002
	(0.004)	(0.004)	(0.004)	(0.004)
Single sex school	0.024	0.019	0.024	0.026
	(0.020)	(0.020)	(0.022)	(0.023)
Secondary modern school			-0.026	-0.029
			(0.022)	(0.022)
Grammar school			-0.008	0.030
			(0.024)	(0.026)
Private school			-0.012	0.011
			(0.040)	(0.042)
Highest Education (No qualification is base group) by 1981				
Other	0.082	0.074	0.076	
	(0.046)	(0.044)	(0.044)	
Lower Vocational	0.145	0.106	0.108	
	(0.036)	(0.036)	(0.035)	
Middle Vocational	0.242	0.181	0.180	
	(0.038)	(0.038)	(0.038)	
A-Level	0.247	0.177	0.176	
	(0.039)	(0.040)	(0.040)	
Higher Vocational	0.375	0.315	0.316	
	(0.039)	(0.039)	(0.039)	
Degree	0.428	0.345	0.345	
	(0.043)	(0.045)	(0.045)	
P-value: Local area characteristics	0.33	0.22	0.30	0.33
P-value: Family Background	0.48	0.35	0.34	0.000
P-value: Test Scores at 7		0.82	0.83	0.42
P-value: Test Scores at 11		0.000	0.000	0.000
P-value: Test Scores at 7 & 11		0.000	0.000	0.000
R^2	0.251	0.280	0.280	0.225
Number of observations	1486	1486	1486	1486
Heteroskedasticity consistent standard errors in parentheses				

Table 4.7: School Quality and Female Wages at 23

at 23 are determined by qualifications, ability at 11 and region of residence which reflects the characteristics of the local labor market. The conclusions relating to the effect of the pupil-teacher ratio on wages at 33 when we do not condition on highest qualifications are shown in specification 4. The results are once again not significantly different to those obtained when we control for qualifications.

In Table 4.8 we present results for women at 33. The full set of results for specification 3 is given in Table **??** in the Appendix. Unlike our earlier results, we find a significant and relatively large impact of the pupil-teacher ratio on female wages at 33.[26] A decrease of one in the pupil-teacher ratio in secondary school is

[26] In the regression with no additional controls the pupil-teacher ratio has a coefficient of -0.039 with a standard error of 0.007.

associated with a 1 per cent increase in wages. This is true in all specifications we tried and in particular it remains true regardless of whether we condition on test scores or on family background or on the type of school. Given the large variability in the pupil teacher ratio in the data, the results imply that it could be responsible for some large wage differentials. Moreover, there appears to be no strong school type effect for women at 33, although again these results could be biased if we do not have common support. This issue is explored in detail in section 4.4 below. What seems to matter for wages is ability measured at 11, family background, qualifications and to some extent the pupil-teacher ratio. When we exclude qualifications (specification 4) the impact of a unit increase in the pupil teacher ratio increases to -1.2 per cent. Moreover the single sex school effect becomes large and significant but the other school type variables remain unimportant.

Next we considered whether interaction effects are important. The results are presented in Table 4.9. In the Table, we interact the pupil-teacher ratio with low and high ability.[27] The results suggests that the pupil-teacher ratio may be more important for the wage outcomes of low ability women than high ability women and this holds whether we include qualifications (specification 3) or not (specification 4) in the regression. In specification 4 (column 1) the difference in the coefficients is significant at the 12.6 per cent level while in specification 3 (column 2) the difference is significant at the 9.6 per cent level. The impact for higher ability women is smaller but not negligible. We also tried interactions with the type of school. These were insignificant (p-value 24 per cent).

The result that the pupil-teacher ratio has an impact at a later age is similar to some US results showing that the impact of quality effects is stronger at older ages. The significance of our result is that we control for cohort. Hence for women

[27] High ability people are defined to be those in the top two quintiles of either the reading or maths ability test at the age of 7.

Specification	1	2	3	4
Pupil-teacher ratio 1974	-0.0095	-0.009	-0.01	-0.0122
	(0.0058)	(0.006)	(0.006)	(0.0064)
Single sex school	0.0032	0.021	0.029	0.061
	(0.028)	(0.028)	(0.030)	(0.031)
Secondary modern school			0.002	-0.004
			(0.03)	(0.035)
Grammar school			-0.026	0.057
			(0.04)	(0.043)
Private school			-0.026	0.037
			(0.060)	(0.065)
Highest Education (No qualification is base group) by 1991				
Other	0.020	0.004	0.004	
	(0.046)	(0.05)	(0.05)	
Lower Vocational	0.118	0.077	0.076	
	(0.043)	(0.048)	(0.048)	
Middle Vocational	0.286	0.224	0.226	
	(0.055)	(0.061)	(0.061)	
A-Level	0.386	0.328	0.330	
	(0.064)	(0.068)	(0.068)	
Higher Vocational	0.544	0.493	0.495	
	(0.052)	(0.057)	(0.058)	
Degree	0.679	0.594	0.598	
	(0.056)	(0.063)	(0.064)	
P-value: Local Authority	0.63	0.72	0.75	0.87
P-value: Test Scores at 7		0.49	0.50	0.20
P-value: Test Scores at 11		0.006	0.005	0.000
P-value: Test Scores at 7 & 11		0.009	0.007	0.000
P-value: Family Background	0.000	0.000	0.000	0.000
R^2	0.387	0.401	0.402	0.285
Number of observations	1324	1324	1324	1324
Heteroskedasticity consistent standard errors in parentheses.				

Table 4.8: School Quality and Female Wages at 33

Specification	4	3
Pupil-teacher ratio 1974× high ability	-0.0106 (0.0066)	-0.008 (0.006)
Pupil-teacher ratio 1974× low ability	-0.0152 (0.0067)	-0.012 (0.006)
Single sex school	0.062 (0.031)	0.030 (0.030)
Secondary modern school	-0.004 (0.035)	0.001 0.03
Grammar school	0.060 (0.043)	-0.023 (0.04)
Private school	0.039 (0.065)	-0.024 (0.06)
Qualifications included	NO	YES
R^2	0.287	0.403
Number of observations	1324	1324

Notes:
(i) Controls as in specification 3 of Table 4.8.
(ii) Heteroskedasticity consistent standard errors in parentheses

Table 4.9: School Quality and Female Wages at 33

there seems to be some evidence that quality effects have an impact at a later age, and particularly for less able women.

4.4. The effect of selective education on wages at 33

An interesting and potentially important result in the regressions presented above has been the positive impact of selective schools (private and state grammar schools) on wages at 33 as well as on qualifications. This may well be an indication that inputs matter, or that peer effects are important. However, to give a causal interpretation of this result, we need to make sure that the impact of selective schools is measured using comparable pupils in selective and non-selective schools. Of course we must assume (as before) that selection takes place on observables. Given the vast array of parental, local and individual characteristics (including test scores) at our disposal, this appears to be a reasonable assumption.

We use propensity score matching (see Rosenbaum and Rubin (1983) and Heckman, Ichimura and Todd (1997)). The propensity score for attending a selective school is estimated using parental background variables, local characteris-

tics, test scores, regional indicators, the pupil teacher ratio and school's single sex status as covariates but *not* qualifications. Hence the impact we measure is the impact of selective schools on wages at 33 *including* that which operates through qualifications (i.e. the covariates of specification 4). The approach we follow is non-parametric. The conditional expectation of the counterfactual outcome is estimated using a Gaussian kernel. This is followed by nearest neighbor matching where observations that can not be matched closely enough are excluded. This ensures that the comparison takes place over a common support for the treated and the non-treated group.[28]

In Table 4.10 we report the impact of selective schools on wages at 33 for those who attended a selective school (treatment on the treated) and for those who did not (treatment on the non treated). For the purposes of inference we present the 95% confidence interval computed using the bootstrap. This takes into account the fact that the propensity score is estimated. We also present the standard deviation of the bootstrap estimates.

In all cases the impact of being in a selective school is positive. However the 95% confidence interval for the treatment on the treated parameter does contain zero and the results are generally quite imprecise. This is obviously due to the relative small sample sizes involved. When considering the impact on the non-treated the impact for men is large and significantly different from zero, although still quite imprecisely estimated. This suggests that it is those children less likely to attend a selective school (say because of coming from a poorer socio-economic

[28]The effect we estimate is $\alpha = E_{F^1(P(X))}(Y_i - E(Y_i^0|P(X_i), D_i = 0)|D_i = 1)$ where Y_i^1 and Y_i^0 represent the outcome in the treatment and non-treatment state respectively (in our cases the log wage at 33). $P(X_i)$ is the probability of being treated, i.e. the propensity score, evaluated at the Xs of the i_{th} treated individual. $E(Y_i^0|P(X_i), D_i = 0)$ is the expected outcome conditional on $P(X_i)$ in the non-treated state and that is estimated using a Gaussian kernel on the non-treated sample. Finally, the notation $E_{F^1(P(X))}$ denotes expectation over the distribution of the propensity score in the treatment group. We use only those treated observations for which a match based on $P(X_i)$ is close enough. The maximum score difference is 5 % points for the smallest treated samples and 0.8% points for the larger smaples. In the former case 90% of the sample have score differences of less than 1.5 % points. For the effect of treatment on the non-treatment just reverse the definition of treatment and control.

background) and who do have a comparison group within the selective sector, who would have benefited the most from the type of education offered by the selective sector. In fact the matched sample for estimating the impact of treatment on the non-treated has a mathematics test score at 7 1/3 of a standard deviation higher than the children in the non-selective sector and 0.5 a standard deviation less than the average child in the selective sector. In terms of background the selective sector contains 0.76 white collar workers. For the non-selective sector the proportion of white collar workers is 0.36, while for this matched sample the proportion is 0.53. The wide confidence bands reflect partly the fact that some individuals in the comparison group are used a number of times. In particular for treatment on the non-treated in the case of males, there are only 88 distinct individuals in the matched control group (children in selective schools) and they are used repeatedly (each is used 7.3 times on average). The same happens in all other cases in that table. Obviously the approach remains silent on the effect of selective schooling on children for whom there is no comparison group in the selective sector. The fact that we have smoothed the expected outcome for the control group before removing the unmatched observations helps improve precision considerably (see Heckman, Ichimura and Todd, 1997).

Overall the results point to a positive causal impact on wages of selective schools, subject to the assumption that all selection is on observables. Discovering the causes of such an impact is important, since it may hold the key to how best to spend resources in schools.

4.5. The effect of the pupil-teacher ratio and school type on employment at 33

A final outcome of interest is the level of employment. First, employment together with wage rates determine earnings. Second by examining the employment equation we can find out if it is likely that the estimated impact of the pupil-teacher

	Male	Female	All
Average impact of a selective school on	0.0987	0.0352	0.0823
the population attending one (treatment on the treated)	(0.0785)	(0.0781)	(0.0567)
	[-0.052, 0.204]	[-0.204, 0.115]	[-0.061, 0.161]
Number of observations in selective schools matched	237	240	486
Average impact of a selective school on	0.2080	0.0821	0.1248
the population attending a non-selective one	(0.0657)	(0.0765)	(0.0560)
(treatment on the non-treated)	[0.0850, 0.322]	[-0.143, 0.156]	[0.051,0.300]
Number of observations in non-selective schools matched	645	557	1272

Note: Bootstrap 95% confidence interval in square brackets [ ]. Standard deviation of the bootstrap in round brackets ( ). 100 replications for the bootstrap

Table 4.10: Impact of Selective Schooling on log Wages at 33

ratio may have been biased by composition effects (see Dearden (1999) for more details). In Table 4.11 we present the marginal effects from a simple probit for employment of men and women at the age of 33 (i.e. in 1991).[29] These probits include the same controls as the third specification in Table 4.8.[30]

Quite clearly, the school quality and type variables have no impact on the employment probability for either men or women. Hence to a first order approximation under joint normality the results obtained in the wage regressions are not the outcome of having ignored composition effects. For men, only the higher educational qualifications matter for employment. Moreover, while test scores do not matter, family background and local neighborhood characteristics do. For women, working is related to having qualifications and the higher the qualification level the higher the probability of employment. This is consistent with a positive wage effect on labor supply as well as with the possibility that women who do not intend to work, do not obtain qualifications. Finally for women, test scores do not seem to matter. The relationship between educational qualifications and

[29] Full set of coefficients for these probits are available on request.
[30] Interacion effects were completely isignificant.

Specification	1. Men	2. Women
Pupil-teacher ratio 1974	-0.006	-0.002
	(0.006)	(0.006)
Single sex school	0.01	-0.024
	(0.026)	(0.028)
Secondary modern school	0.005	-0.023
	(0.024)	(0.027)
Grammar school	0.045	0.006
	(0.034)	(0.036)
Private school	-0.082	-0.067
	(0.057)	(0.055)
Highest Education (No qualification is base group) by 1991		
Other	0.074	0.095
	(0.04)	(0.043)
Lower Vocational	0.03	0.120
	(0.04)	(0.043)
Middle Vocational	0.08	0.118
	(0.039)	(0.047)
A-Level	0.09	0.111
	(0.04)	(0.053)
Higher Vocational	0.141	0.198
	(0.034)	(0.041)
Degree	0.135	0.200
	(0.037)	(0.045)
P-value: Local area characteristics	0.02	0.44
P-value: Family Background	0.038	0.33
P-value: Test Scores at 7 & 11	0.67	0.84
R^2	0.067	0.033
Number of observations	2232	2412
Heteroscedasticity consistent standard errors in parentheses.		

Table 4.11: Employment at 33

employment implies that the returns to education for both men and women are likely to be underestimated (see Dearden (1999)).

4.5.1. The returns to education for men and women

An interesting by-product of our analysis is a set of returns to qualifications. Dearden (1999) uses this cohort to examine these in some detail. Here we note that returns to qualifications are significantly reduced when we include ability scores. We also note that the returns increase significantly between the ages of 23 and 33. At 23 the workers with higher qualifications have much lower labor market experience than the lower educated ones. Moreover the returns to

education probably increase with experience. Finally, from the fact that higher qualifications increase the employment probability, we can infer to a first order approximation under joint normality, that the returns are underestimated by the wage equation if wages and employment are conditionally positively correlated (see Dearden (1999)).

5. Conclusions

In this paper we have used data from the 1958 National Child Development Survey (NCDS) to investigate the impact of the pupil-teacher ratio and school type on educational and labor market outcomes. The outcomes we consider is the highest level of educational qualification, wages at 23 and 33 and employment at 33.

Our major findings are:

1. The primary pupil-teacher ratio has no impact on any of the outcome variables over the range that it varies in the data, once we condition on test scores for mathematical and verbal ability at 7. Even when we do not condition on these variables the impact is very small and only significant for men.

2. The secondary pupil-teacher ratio has no impact on educational attainment for either men or women, once we control for test scores and type of school attended.

3. While the secondary pupil-teacher ratio is found to have no impact on wages at 23, we find evidence of some impact on wages at 33, particularly for women. We also find evidence that low ability women benefit more from lower pupil-teacher ratios than high ability women. The results lend some support to some US findings that school input measures matter more for outcomes measured later in life (i.e. they may be age dependent).

4. Wages at 23 for men depend only on qualifications and local labor market indicators. For women test scores also matter. However none of the measured school quality variables are important at that age.

5. Wages at 33 for both men and women depend on qualifications and ability. For women family background also matters. Thus it seems that ability as measured by test scores affects earnings growth, either through a learning/on-the-job screening mechanism and/or through a complementarity between ability and learning-by-doing or professional training.

6. We also find that attending a selective school (either a government grammar school or a private school) impacts positively and significantly on educational outcomes for both men and women and the wages of men at the age of 33. We check the robustness of this finding using non-parametric propensity score matching techniques. We find evidence suggesting that the selective school impact on wages at 33, is highest on the types of individuals who predominantly attend non-selective schools but who do have a comparison group among those going to a selective one. These individuals are more able and come from better off backgrounds than the average child in the non-selective sector, but are less well off and less able than the average selective sector child. Obviously we have nothing to say for the types of individuals from poorer backgrounds and/or with lower ability who have no comparison group among the selective school children.

7. The probability of employment does not depend on any of the school input or school type variables, conditional on qualifications.

The upshot of these results is that the pupil-teacher ratio in secondary schools matters somewhat for the wages of women at the age of 33 but has no obvious impact for men. The fact that we find an impact for lower ability women is in line with some of the recent literature where the pupil teacher ratio is found to have

a larger impact on outcomes for pupils from a disadvantaged background (see Krueger and Whitmore, 2001). It has been argued by Lazear (1999) that lowering the pupil teacher ratio is likely to be most effective for lower ability pupils who are not disruptive. This conjecture is broadly consistent with our results. It may well be that the greater maturity of young women coupled with the need for greater attention at the lower end of the ability distribution makes lower pupil-teacher ratios effective for girls.

The results for men do not, however, imply that academic outcomes and wages would be the same, regardless of whether the pupil-teacher ratio was one or one hundred. The results in this paper relate to the impact of differences in this ratio observed in our data.

These results may raise concerns that some selection is occurring. We have, however, taken great care to exploit the richness of this data set to control for both family background *and* neighborhood composition at both LEA and census enumeration district (approximately 500 households). This should control for the increased resources deprived areas receive from central government to fund the state school system. Moreover, the use of the pupil-teacher ratio avoids problems related to class size, where sometimes disadvantaged pupils within a school are placed in smaller classes. Our measure captures an average across the school mitigating this problem. However it is possible that there are other selection mechanisms that we are not able to control for. In particular, there is the possibility that the pupil-teacher ratio is correlated with unobservable inputs which could bias our results. Hanushek (1998) has emphasized the importance of teacher quality which we do not observe. If this is important and schools with higher pupil-teacher ratios have better teachers (either because better teachers allow schools to have higher pupil-teacher ratios other things being equal or because the schools respond to a resource constraint by screening teachers better), then the pupil-teacher ratio effect may be biased downwards. While we feel that this is not likely, it is only with better data that the importance of this potential

bias can be assessed. Equally of course better teachers may self-select to schools with lower pupil-teacher ratios since the working conditions there are better.

The results do indicate that inputs of other kinds may, however, be important. The *type* of secondary school does matter for qualifications obtained and the wages of men at 33, even after controlling for test scores up to the age of 11 and for detailed family background variables. In particular those attending private schools and selective state schools have significantly better outcomes. These result may imply that school inputs matter in a way that cannot be captured by the pupil-teacher ratio or the other input measures we considered. One explanation may be that the quality of the teachers is better in these schools since conditions of service are generally better and salaries in the private sector generally higher. Clearly better teacher quality data is needed to explore this important issue. Understanding why the type of school attended matters is of considerable policy importance, but unfortunately beyond the scope of our data set. The UK government is currently in the process of seeking advice on how to best ensure that these important issues can be addressed in the future. The results from our study provide a useful starting place, but it will only be with even better data and/or research design that we will be able to ascertain in exactly what ways school quality matters and what the policy priorities should be in this important area.

Appendix

Table 0.1: Description of Highest Education and Highest School Qualification

Variable	Description
Highest Education Qualification:	
Degree	University or CNAA first degree; CNAA Post-graduate Diploma; or University or CNAA Higher Degree.
Higher Vocational	Highest Vocational: Full professional qualification or part of a professional qualification; Polytechnic Diploma or Certificate (not CNAA validated); University or CNAA Diploma or Certificate; Nursing qualification including nursery qualification; non-graduate teaching qualifications; Higher National Certificate (HNC) or Diploma (HND); BEC/TEC Higher Certificate or Higher Diploma; City and Guilds Full Technological Certificate.
A Levels	At least one GCE A Level, Scottish Leaving Certificate (SLC), Scottish Certificate of Education (SCE), Scottish University Preliminary Examination (SUPE) at Higher Grade, Certificate of Sixth Year Studies.
Middle Vocational	Middle Vocational or 5+ O Levels: City and Guilds Advanced or Final; Ordinary National Certificate (ONC) or Diploma (OND); BEC/TEC National, General or Ordinary; at least five GCE O Level passes or grades A–C, or CSE Grade 1 or equivalent.
Lower Vocational	Lower Vocational or O Levels: City and Guilds Craft or Ordinary; a Royal Society of Arts (RSA) awards, stage 1, 2 or 3; other commercial or clerical qualifications; at least one GCE O Level passes or grades A–C, or CSE Grade 1 or equivalent.
Other	Miscellaneous Qualifications: All other courses leading to some sort of qualification which are not identified above including CSE grade 2–5 or equivalent and miscellaneous apprenticeship qualifications.
None	No qualifications including those with no formal schooling.
Highest School Qualification:	
A Levels	At least one GCE A Level, Scottish Leaving Certificate (SLC), Scottish Certificate of Education (SCE), Scottish University Preliminary Examination (SUPE) at Higher Grade, Certificate of Sixth Year Studies.
5+ O Levels	At least five GCE O Level passes or grades A–C, or CSE Grade 1 or equivalent.
O Levels	At least one GCE O Level passes or grades A–C, or CSE Grade 1 or equivalent.
CSEs	CSE grade 2–5 or equivalent school qualification.
None	No school qualifications.

Table 0.2: Summary Statistics

Variable	Males 2232 Observations		Females 2412 Observeations	
	Mean	(Std Dev.)	Mean	(Std Dev.)
Real log hourly wage 1981	1.582	(0.322)	1.442	(0.341)
Real log hourly wage 1991	2.058	(0.422)	1.688	(0.490)
Valid wage data 1981	0.762	(0.426)	0.616	(0.486)
Valid wage data 1991	0.682	(0.466)	0.549	(0.498)
Employed 1981	0.873	(0.333)	0.674	(0.469)
Employed 1991	0.924	(0.265)	0.725	(0.447)
Highest Qualification 1991:				
None	0.056	(0.231)	0.072	(0.258)
Other	0.097	(0.296)	0.137	(0.344)
Lower Vocational	0.254	(0.435)	0.330	(0.470)
Middle Vocational	0.219	(0.413)	0.126	(0.332)
A Levels	0.058	(0.234)	0.068	(0.252)
Higher Vocational	0.167	(0.373)	0.139	(0.346)
Degree	0.149	(0.356)	0.128	(0.334)
Highest Qualification 1981:				
None	0.093	(0.291)	0.108	(0.310)
Other	0.147	(0.354)	0.165	(0.372)
Lower Vocational	0.233	(0.423)	0.303	(0.460)
Middle Vocational	0.238	(0.426)	0.143	(0.350)
A Levels	0.080	(0.271)	0.079	(0.270)
Higher Vocational	0.093	(0.290)	0.092	(0.289)
Degree	0.117	(0.321)	0.109	(0.312)
Single sex school 1974	0.249	(0.433)	0.291	(0.454)
Pupil-teacher ratio 1974	17.068	(1.923)	17.196	(2.077)
Comprehensive school 1974	0.573	(0.495)	0.550	(0.498)
Secondary modern school 1974	0.246	(0.431)	0.238	(0.426)
Grammar school 1974	0.127	(0.333)	0.155	(0.362)
Private school 1974	0.054	(0.226)	0.056	(0.231)
High ability	0.574	(0.495)	0.613	(0.487)
Maths ability at 7:				
1st quintile (lowest)	0.159	(0.366)	0.170	(0.375)
2nd quintile	0.177	(0.382)	0.219	(0.414)
3rd quintile	0.203	(0.403)	0.206	(0.405)
4th quintile	0.214	(0.410)	0.201	(0.401)
5th quintile (highest)	0.246	(0.431)	0.204	(0.403)
Reading ability at 7:				
1st quintile (lowest)	0.205	(0.404)	0.127	(0.333)
2nd quintile	0.212	(0.409)	0.174	(0.379)
3rd quintile	0.209	(0.407)	0.215	(0.411)
4th quintile	0.200	(0.400)	0.220	(0.414)
5th quintile (highest)	0.173	(0.379)	0.264	(0.441)
Maths ability at 11:				
1st quintile (lowest)	0.154	(0.361)	0.139	(0.346)
2nd quintile	0.203	(0.402)	0.174	(0.379)
3rd quintile	0.189	(0.391)	0.217	(0.412)
4th quintile	0.219	(0.413)	0.221	(0.415)
5th quintile (highest)	0.232	(0.422)	0.242	(0.428)
Reading ability at 11:				
1st quintile (lowest)	0.177	(0.382)	0.115	(0.319)
2nd quintile	0.208	(0.406)	0.165	(0.371)
3rd quintile	0.194	(0.396)	0.211	(0.408)
4th quintile	0.205	(0.404)	0.231	(0.421)
5th quintile (highest)	0.212	(0.409)	0.273	(0.445)
Missing ability at 11	0.004	(0.060)	0.006	(0.079)

Continued next page ...