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Using Boundary Changes to Estimate the Impact of School Competition on Test Scores

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Abstract
We study the impact of school choice on test score outcome. It has generally proved difficult to isolate exogenous differences in the degree of competition faced by schools. We run a difference-in-difference analysis, exploiting a local government reorganisation to provide identification. This reorganisation changed the boundaries of education markets. We analyse one cohort of children passing through secondary school before the change, and one afterwards, both for the treated (re-organised) area and for similar control areas. Our point estimates suggest that the fall in competition experienced reduced test scores, but the estimates are not statistically significant.

Keywords: School choice, school competition, educational outcomes

JEL Classification: I21, I28

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1. Introduction

Raising educational attainment is a central policy aim of many governments. Many are considering the role of structural reforms as an alternative to allocating higher resources. The role of school choice in intensifying competition between schools and thereby raising standards is one of these policies. The British government has recently proposed legislation designed to do just that[1]. Yet, in terms of the available evidence, this remains a controversial area. There is no strong consensus on the likely effects of increasing choice in education. School choice affects the distribution of attainment through two principal channels – the sorting of pupils across schools, and an impact on the effort schools put in to teaching, or school value-added. This paper focuses on the latter[2].

It has proved difficult to isolate exogenous differences in the degree of competition faced by schools. In this paper, we run a difference-in-difference analysis, exploiting a local government reorganisation to provide identification. During the late 1990's the structure of local government in England changed considerably, and the boundaries of many Local Education Authorities (LEAs) were affected as a result. This has a significant effect on education since LEAs essentially define school markets. Almost all pupils (certainly outside London) attend a school in their LEA in which they live. This policy change therefore meant that many schools faced an exogenous change in competition during this period, as the boundaries of their markets changed. In our main analysis, we focus on a case where a large LEA was split up into six. Many schools therefore faced a significant decline in the degree of competition they faced – some schools that once provided alternatives for a student were no longer available after the boundary change. Other schools in the original LEA, those not near the borders of the new LEAs, had no change in competition. We exploit this factor in our estimation, alongside using other LEAs as controls. The data enables us to analyse one cohort of children passing through secondary school before the change, and one afterwards, both for the treated (re-organised) area and for similar control areas. We measure the impact of the fall in competition on pupil progress, controlling for initial pupil attainment, other pupil characteristics and school effects. The timing of the

[1] See http://www.dfes.gov.uk/publications/educationandinspectionsbill/

[2] As part of an on-going research programme at CMPO, we have also characterised choice and sorting in schools in England (Burgess et al, 2004), Burgess and Briggs (2006).

change and our data means that both cohorts of pupils choose their schools under the old boundaries, so any difference we measure is purely about teaching effort. That is, there is no confounding effect from changes in peer groups arising from the boundary changes.

Our results show no significant effect of the decline in competition on pupil progress. In all specifications, the point estimate is negative, but it is insignificantly different from zero. This may be simply because the data are insufficient to precisely estimate the effect. We do find a significantly negative effect for a small sub-set of schools, Foundation or Voluntary Aided schools. These are schools with more control over their own admissions than regular community schools. However, inference on this issue is only based on a few observations, so may not be very secure.

We also exploit a longer run of data on two additional areas with boundary changes, but without data on pupil prior attainment. This is also a difference-in-difference analysis, but studying gross output rather than value-added. In one of the three areas we do find significant evidence of a competition effect, insignificant, but of the expected sign in the other two.

There is a long tradition of studying school choice: from a tool to remedy market inefficiencies (Friedman, 1955), through choice to empower the disadvantaged (Jencks, 1970), to choice as a governance issue (Chubb and Moe, 1990)[3]. We can divide recent empirical contributions into those studying specific targeted choice schemes (for example, Cullen, Jacob and Levitt, 2006; Howell, 2004), and those examining a generalised system of school choice (for example, Hoxby, 2000, in the US and Sandstrom and Bergstrøm, 2002, in Sweden). We can also characterise studies as analysing the partial equilibrium impact on the pupils making choices (for example, Howell, 2004 on the New York school choice programme), or focussing on the impact on the school system as a whole (Hoxby, 2003b). Bayer and McMillan (2005) model the general equilibrium of residential and school choice. Lavy (2006) seems to be the only study looking at both effects on individuals and on the system as a whole. Finally, we can differentiate the systemic impact into the impact on sorting across schools (for example, Söderström and Uusitalo, 2004, Burgess et al, 2006), and the impact of competitive pressure on general levels of attainment (Lavy, 2006). This

[3] Recent contributions surveying the field include Howell and Peterson (2002), Hoxby (2003a), Ladd (2002) and Neal (2002).

paper fits into the category of studies of the impact generalised school choice and competition on overall levels of attainment.

Different strategies have been followed to isolate exogenous differences in the degree of competition faced by schools. Hoxby (2000) uses geographical features to instrument for education market boundaries in a cross-sectional analysis, though this is controversial (Rothstein, 2005, Hoxby, 2005). Others use a policy change to identify a change in competition (Lavy, 2006). There are very few studies for Britain. Bradley et al (2000) show that a school's own exam performance is positively related to the exam performance of competitors in preceding years. The educational outcomes here are raw exam results, not value added, so they may include some effect from sorting. Identification requires that there be no common local factors raising or lowering school test scores. More recently, Clark (2004) exploits the Grant Maintained schools policy. He finds that schools located near to opting-out schools, which therefore arguably faced increased competition, did not respond by improving outcomes. Finally, Gibbons, Machin and Silva (2006) use a cross-section of primary schools and pupils and instrument their measure of competition using distance from the market boundary. They find little effect of the degree of competition on outcomes.

In section 2 we set out the modelling framework and briefly describe the English school system. Section 3 describes the data and the nature of the boundary changes we exploit, and section 4 presents the results. We conclude the paper with a discussion of why we might expect competition to 'work' in some areas and not in others.

2. Modelling Framework

a) *The Education System in England*

The analysis in this paper relates to state secondary schools, which educate pupils from the age of 11 to 16, when the exams at the end of compulsory schooling are taken. Schools in England have operated in a quasi-market since the Education Reform Act of 1988. In principle, parents have some choice over which school their child attends, as catchment areas overlap. Schools have incentives to attract pupils as devolved budgets are calculated on a per capita basis and so schools must attract sufficient pupils to maintain resources.

The competitive system works as follows. Parents are interested in the quality of education that a school would provide, and schools can signal this through the annual publication of exam results. This is in a standardised form across all schools, and has operated since the early 1990s. The key measure for secondary schools is the percentage of pupils gaining at least 5 GCSE qualifications at level C or above. Typically, students take GCSE exams in 8 to 10 subjects. The annual publication of the "school league tables" is widely publicised in national and local press, and by schools themselves. Thus the competitive environment provides a clear way for schools to signal their achievements, and provides incentives for them to attract pupils. Schools may also gain indirectly from attracting more able pupils if peer effects are important. It also allows parents to choose schools other than that closest to them.

The majority of secondary schools in England are community schools (66%); they are owned and maintained by the LEA, who decide on the admissions structure and set catchment areas for schools based on geography. A smaller proportion of schools have foundation status (16%) or are voluntary aided (14%). These schools are administered by a governing body, and act as their own admissions authority[4]. A small minority of LEAs retain some selection on ability in some schools. Grammar schools select on ability tests taken at the end of primary education, and pupils failing the tests are assigned to secondary modern schools. We therefore exclude grammar and secondary modern schools, as any estimate of the effect of a change in competition on school performance may be confounded by these admission policies.

b) Model of Education Production

We want to gauge the impact of a school's teaching effort on pupil progress, and how that responds to changes in the degree of competitive pressure the school feels. We focus on pupil progress from KS3 at age 14 to GCSE at 16, referred to as value-added. In modelling the test score at GCSE of pupil i in school s, y_{is}, we assume all

[4] Most voluntary aided schools are connected to a particular religious denomination. Admissions criteria can vary from selection on ability, to preference given to children of former pupils (see West & Hind, 2003).

prior influences on human capital are summarised in the KS3 score (k_i). Progress during the year depends on pupil characteristics (X), plus the impact of the school (m_s), and we assume some testing noise e:

$$y_{is} = a\, k_i + bX_i + m_s + e_{is} \qquad (1)$$

The school impact in turn depends on the effort put into teaching (v_s), which may depend on competition, measured structural school factors (Z_s) such as size, plus unmeasured influences (u_s):

$$m_s = v_s + p.Z_s + u_s \qquad (2)$$

We have omitted time subscripts from (1) and (2) for clarity.

It is the response of v_s to the competitive environment that we are interested in modelling here. This is chosen by the school to maximise an objective function, given assumptions about strategies that other schools may pursue. The standard view is that competitive pressure leads agents to work harder to retain or win new clients. State schools in England are not profit-maximising firms so this view cannot simply be read across. Nonetheless, there are incentives in the system outlined above that mean schools would be concerned about losing pupils. Furthermore, it may be that school size or the average ability of pupils[5] enters the school headteacher's utility function directly.

One useful analysis of this issue is McMillan (2003). In his model, schools maximise total net revenue by choosing teaching effort. This influences both cost and student enrolment. In our context that would work through greater effort raising results, producing a higher league table position and attracting more pupils. Optimal effort is sensitive to the level of competition. McMillan sets out the conditions under which an increase in competition will lead to an increase or a decrease in effort. This essentially relates to the income profile of the neighbourhood given that parents may also have access to private schools. Given that our data allow a difference-in-difference analysis, we do not need to adopt a structural approach to the issue. Thus we do not set out a preferred theoretical model, but simply note that it is possible that optimal behaviour will imply state schools reducing effort levels and hence productivity when competition increases.

[5] A high-performing school would expect more applications and perhaps the chance to (implicitly) select the more able ones.

c) *Econometric model – Value Added*

Using data from before and after the boundary change, we compare changes in pupil outcomes for schools that experience falls in competition, and schools that did not. The advantages of using this methodology are well known: any constant factors, observable or unobservable, that may affect the dependent variable or are correlated with our treated group are eliminated. We do require the assumption that any unmeasured time-varying factors affect both treated and untreated groups in the same way. A suitable choice of control group should minimise any heterogeneity between the control and treated populations. As noted, we can exploit two levels of controls – pupils in other LEAs, and pupils in schools in Berkshire that saw no change in competition. The former are completely ‘clean’ controls. The latter group will not be perfect controls if either the choice of a ten minute drive time zone is too narrow, or there are important spill-over effects between schools within Berkshire. Conversely, they will control for any Berkshire-specific effects.

We now introduce a time subscript to differentiate our two data periods. Recall that any one pupil only appears in one period – it is a panel of schools, and it is the change in the school effect that we are interested in. Expanding (2) to allow the teaching effort v_s to depend on the change in competition:

$$m_{st} = \bar{v}_s + l I(s \in T).I(t = 2002) + pZ_{st} + d.t + u_{st} \qquad (3)$$

T is the group of treated schools, and 2002 is the post-change year. We also allow a common time factor in school effects. Substituting (3) into (1) yields the regression we estimate at pupil-level:

$$y_{ist} = ak_{it} + bX_{it} + pZ_{st} + l I(s \in T).I(t = 2002) + d.t + \bar{v}_s + (u_{st} + e_{ist}) \qquad (4)$$

We include school fixed effects, which pick up the permanent unobserved differences in value-added, $\bar{v}_s$. The parameter of interest is l, the conditional difference-in-difference, which measures any shift in outcome within the treated schools relative to the untreated schools after the Berkshire LEA boundary change, after controlling for pupil and school factors. Given the error structure in (4), we cluster errors at school level.

Our hypothesis concerns the effect of a change in competition on school performance, and so the unit of analysis is properly the school. It is useful, therefore, to extract a measure of the school effect to directly examine. The semi-parametric approach we use is to control for pupil characteristics on pupil outcomes as in the basic model of pupil performance (1), but allow for freely varying school effects, m_{st}:

$$y_{ist} = a\, k_{it} + bX_{it} + m_{st} + e_{ist} \tag{5}$$

Having extracted the school effects, we provide a graphical analysis below and a school level regression:

$$\hat{m}_{st} = l\, I(s \in T).I(t = 2002) + pZ_{st} + d.t + (\bar{v}_s + u_{st}) \tag{6}$$

This may be biased by correlation of $\bar{v}_s$ with membership of the treatment group. So finally, we difference the school effects to remove the unobserved permanent component:

$$\Delta\hat{m}_s = d + l\, I(s \in T) + p\Delta Z_s + \Delta u_s \tag{7}$$

In the results section, we report estimates of (4), (6), and (7).

d) Bias correction

The pupil level data sets for both cohorts in our sample include information on gender, age and KS3 scores, see below for details. However, the 1997 dataset does not include pupil-level data on free school meals (FSM), ethnicity or special educational needs. Omitting such controls is likely to lead to bias in our estimates of school effects. For example, schools with higher than average numbers of pupils eligible for FSM will be wrongly assigned low value added. Given the absence of this individual data, we parameterise the bias and include this in the regression. To take a stripped down version of (1), suppose the true model is:

$$y_{is} = f_s I_{is}(i \in s) + f_2 f_i + u_{is} \tag{8}$$

where $I_{is}(i \in s)$ is a dummy for pupil i attending school s, and f_i is a dummy for eligibility for FSM. The lack of individual pupil FSM data forces us to estimate:

$$y_{is} = d_s I_{is}(i \in s) + w_{is} \tag{9}$$

The estimated school effect in (6) is:

$$E(d_s) = f_s + f_2\left(\frac{\text{cov}(f_i, I_{is})}{\text{var}(I_{is})}\right)$$

which simplifies to:

$$E(d_s) = f_s + f_2\left(\frac{\overline{f}_s - \overline{f}}{1 - s_s}\right) \qquad (10)$$

where $\overline{f_s}$ is the fraction of pupils in school *s* eligible for free meals, $\overline{f}$ is the sample mean fraction of eligible pupils, and s_s is the proportion of pupils in the sample that are in school *s*. Therefore, the expected value of our estimated fixed effect is a function of the true fixed effect f_1 and the bias expression $\left(\left(\overline{f}_s - \overline{f}\right)/\left(1 - s_s\right)\right)$. Since we have school level data on FSM and school size, we calculate this bias term for both time periods and include it in the estimation.

Furthermore, we are able to test the validity of our bias correction method. We use the richer 2002 data set, which includes pupil-level FSM eligibility information. We estimate regression (1) without an FSM dummy and obtain the fixed effects. We regress these fixed effects on the bias term to produce a purged school effect. We compare these with school effects estimated including individual pupil-level FSM data. Correlating the school effects estimated both ways gives a correlation coefficient between the two of 0.98. That is, $\hat{f}_s \approx \hat{d}_s - coeff.\left(\left(\overline{f}_s - \overline{f}\right)/\left(1 - s_s\right)\right)$. So we are reasonably confident that the bias correction deals adequately with the problem.

e) Econometric Model – School Percentage 5 A-C grades*

We also have a longer run of data on a school's output for three areas which saw changes in boundaries, but without data on prior ability. Using the model of a pupil's test score from (5) and (6):

$$y_{ist} = ak_i + bX_i + lI(s \in T).I(t > t') + pZ_{st} + d.t + \left(\bar{v}_s + u_{st} + e_{ist}\right)$$

where t' is the date of the boundary change, we assume that the probability that a pupil achieves at least 5 A* to C grades is some non-linear function of y: $g_{ist} = f\left(y_{ist}\right)$. It is not a straightforward threshold function: for example if pupil 1 has

8 ‘D’ grades and pupil 2 has 5 ‘C’ grades and 3 fails, then pupil 1 has more points, but only pupil 2 gets over the line. Nevertheless, on average, the higher the pupil’s score, the greater the probability of getting 5 C grades or better. The school average is then $\overline{g}_{st} = \sum_{i \in s} f(y_{ist})$. We first control for the effects of a school’s gender composition by estimating, $\overline{g}_{st} = d_0 + d_1 \overline{B}_{st} + e_{st}$, where $\overline{B}_{st}$ is the fraction of male pupils. Having extracted the residuals from this regression, we take a simple linear approximation to the function $f(y_{ist})$ and estimate:

$$e_{st} = g_0 + g_1 I(s \in T).I(t > t') + g_2 Z_{st} + g_3 t + \left(g_4 \overline{k}_{st} + g_5 \overline{X}_{st} + \overline{v}_s + u_{st}\right) \qquad (11)$$

Note that in the data we use for this, $\overline{k}_{st}$ and $\overline{X}_{st}$ are both unobserved, so are in the error term. This means that the coefficient of interest here, g_1, estimates the true effect of the fall in competition on teaching effort (l above), plus any effect on the intake composition of the school consequent on the fall in competition, depending on $\operatorname{cov}\left(\left[g_4 \overline{k}_{st} + g_5 \overline{X}_{st}\right], \left[I(s \in T).I(t > t')\right]\right)$. We would expect this covariance to be either zero or negative – the treatment effect reduces average pupil quality. In which case, the estimated g_1 would be more negative than the true effect on teaching effort, isolated above using the VA approach.

3. Data

We first discuss the nature of the boundary changes we use for identification, then the individual pupil data, and then our measure of competition.

a) Boundaries

The identification strategy is based on a difference-in-difference approach. We argue that boundary redefinitions produce exogenous changes in the degree of competition, producing a policy treatment. We select other, similar, areas with no boundary redefinitions as controls. The use of pupil prior attainment data in our main analysis means that we isolate the impact of the treatment on pupil progress, arising from changes in teaching effort.

During the 1990’s the structure of local government in England changed considerably. It is clear that these changes are exogenous for a study of education markets. The main driver for the policy was political[6], unrelated to education policy. A Conservative politician, Michael Heseltine, led a major local government reorganisation. This was intended as a general efficiency drive, reducing layers of governance and bureaucracy. The process in England was run by a Commission, which was given an initial steer to favour unitary authority solutions, although it did consult widely on options. Big cities in rural shire areas aimed to get back their old unitary status, and most of these succeeded (e.g. Nottingham). There was, however, a counter-attack from other rural shire areas, which built up a strong groundswell of support for the existing two-tier structure, particularly in more 'traditional' shires. So most of the later decisions went in favour of the *status quo*.

Some Local Education Authority (LEA) boundaries were affected as a result of this policy, mainly between 1996 and 1999. LEA boundaries are important since there is a strong presumption that pupils will attend a school within the LEA in which they live, and cross-LEA flows are rare, particularly so outside London[7]. This policy change meant several schools in England faced an exogenous change in competition during this period.

However, unfortunately most of these boundary changes are not useful for the purposes of this paper. Most of the changes produced no usable changes in competition. For example, with the separation of Plymouth and Torbay from the rest of Devon, the positioning of almost all schools in these LEAs meant they did not experience a change in measured competition. The majority of other LEA changes resulted in no competition changes for the schools as the city area became distinct (as an LEA) from the surrounding rural area. Others had one or two schools changing competition, but not enough to warrant investigation. We drew the line to exclude Humberside (which also changed in 1996), where 9 schools out of 60 saw a competition change.

Secondly, some of the changes happened before pupil level data is available, so that we cannot isolate the impact of the change on the teaching effort put in by the school. For example, the change from the single LEA of Avon to four separate LEAs occurred in 1996 and there are no available data on individual outcomes before 1997.

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[6] We are grateful to Glen Bramley at Heriot-Watt University for this information.
[7] Using our PLASC dataset (see below) we can show that only 6.2% of pupils outside London attend schools outside their own LEA.

In particular, there are no data on prior attainment of pupils. However, we can utilise this data for these LEAs to perform analysis on the gross output.

In terms of an analysis is of value-added, we are unfortunately left with only one boundary change that we can exploit: Berkshire. In 1998, Berkshire was split into six unitary authorities[8], and consequently the LEA was split into six separate LEAs: Bracknell Forest, Windsor and Maidenhead, West Berkshire, Reading, Slough and Wokingham. It is not clear why Berkshire chose the unitary authority solution. Politics may have been important - it may or may not be significant that both Heseltine, leading the push for unitary authorities, and John Redwood a highly placed supporter, were both Berkshire MPs (Heseltine for Henley and Redwood for Wokingham). But since the reform covered all aspects of local government, it seems very unlikely that the quality of local schools was the clinching factor. This is reinforced by the fact that schools in Berkshire were slightly above average among England's schools at the time (with 50% of its pupils achieving at least 5 A*-C grades, compared to a national average of 43%). This splitting up of the LEA means that a number of schools that were in the same jurisdiction now fell into separate LEAs. This is what drives the change in competition – some schools that once provided alternatives for a student are no longer available after the change. The degree of competition falls for some schools after the boundary change. We describe the pattern of change below after defining the competition measure.

We can, however, examine a longer run of data and two more boundary change episodes if we use data on school output without controls for pupil prior attainment. These are the split of Avon into City of Bristol, Bath and North East Somerset, South Gloucestershire, and North Somerset in 1996; and the split of Cleveland into Middlesbrough, Stockton on Tees, Hartlepool, and Redcar and Cleveland in the same year.

We select a set of other LEAs as the control group; we use data from all pupils and schools in those LEAs in the analysis. To find our control group, we match LEAs with similar characteristics to our treated LEA, Berkshire. Our initial cut excluded LEAs in central London and LEAs that underwent a substantial LEA boundary change

[8] It may or may not be significant that both Heseltine, leading the push for unitary authorities, and John Redwood a highly placed supporter, were both Berkshire MPs (Heseltine for Henley Redwood for Wokingham).

during the period of our analysis. We then ranked the remaining LEAs using a variety of variables: area, population, a measure of poverty (income support claimants as a percentage of the population), the number of wards, a political variable (the percentage of council seats under Conservative control), and education-related characteristics at LEA level (percentage of students eligible for free school meals, the percentage with English as a second language, the percentage of schools that were grammar schools, and the percentage of schools that were independent. The 10 selected LEAs that were consistently comparable to Berkshire in all measures are: Leeds, Bedfordshire, East Sussex, Cambridgeshire, Devon, Hertfordshire, Peterborough, Plymouth, Staffordshire, Surrey, Torbay, Warwickshire, and West Sussex. Similar procedures were followed for Avon and Cleveland.

b) Pupil data

To use a difference in difference evaluation methodology we require data for a suitable time period before and after the boundary change, and for both treated and control schools. For the before period, we use pupil level data for students who took their KS3 exams in 1995 and GCSEs in 1997, so all their school time was under the single LEA of Berkshire. Our after period uses pupil level data for KS3 exams taken in 2000 and GCSEs in 2002, four years after the boundary change. Note that these pupils chose their schools in 1996/7, under the old boundaries, and so the results we show below are due to changes in the teaching of these pupils, not different pupil choice.

We employ four datasets in our analysis: the Pupil Level Annual School Census which forms part of the National Pupil Database (PLASC/NPD), national matched datasets released by the Department for Education and Skill (DfES), the Annual School Census (ASC) and School Performance Tables. PLASC/NPD contains data on all pupils in both primary and secondary state schools in England, with approximately half a million pupils in each cohort. At pupil level, it provides linked histories of scores in national tests, plus some individual characteristics: gender and within-year age, ethnicity, eligibility for free school meals (FSM) (an indicator of low household income) and special educational needs. We use pupils who took GCSE exams in 2002. At school level, there is data on a range of school characteristics, including performance measures, geographical co-ordinates, school size, age range, religious denomination, funding status, gender mix and admissions policy.

This data is unavailable for our 1997 cohort, so we have to use the first national matched exam dataset released by the DfES. The dataset contains linked test scores for KS3 and GCSE, plus pupils' gender and within-year age. This data set does not include pupil-level information on FSM eligibility, ethnicity and SEN. We discuss how we deal with this below. We augment this dataset with data from two other school level datasets. Information at school level on pupil eligibility for free school meals (FSM), ethnicity, special educational needs and school size is obtained from the Annual School Census (ASC). School Performance Table data further supplements the matched dataset with information on schools' admission policy, funding status, religious denomination, performance measures, age range and gender mix.

In the longer run of data for Avon, Berkshire and Cleveland, we have school-level data on the percentage of students achieving at least 5 C grades or better, as well as the additional controls mentioned above. The key point is that we do not have school level prior attainment measures for all of these time periods.

c) Measuring Competition

Our measure of the degree of competition a particular school faces is a spatial one. It is based on the number of alternative schools that parents could reach rather than go to the focus school. In this paper, we chose a 10-minute drive time zone (DTZ) around every state secondary school and count the number of schools within this area. This number of 'nearby' schools is our measure of the extent of competition in a local market. We use geographical co-ordinates for each school to construct our measure of choice. Whilst this measure is clearly going to be correlated with population density, we are able to include a number of school level variables to pick up the main differences between urban and rural schools.

To construct our measure of competition change, we count the number of schools within a 10 min DTZ in the Berkshire LEA for each school in the before-change period. We repeat the process for the later period, but restrict our count of schools in the 10 min DTZ to within the new smaller LEA for each school. Comparing the two gives us the change in competition across the two time periods.

Figure 1 illustrates the location of schools within Berkshire, with the change in competition for each school due to the boundary change. A number of points can be noted here. For many schools, the degree of competition did not change. These schools were not located near the new boundaries and for them, the number of alternatives available nearby did not decrease. For a number of schools near the newly imposed boundaries, there was a change with a range from a fall of one school, two, four and six. This within-LEA variation of competition change, including zero change, is useful to us as it provides an additional level of control.

Table 1 provides the means and other descriptive statistics of all the variables used in the main analysis. It can be seen that the characteristics of the pupils between the treated and control areas are very similar: the mean test scores for example are (41.2 (control) and 41.6 (treated)). School factors are also similar in general, though there are some differences. Slightly more of the treated schools are community schools and fewer are foundation or voluntary aided.

4. Results

We first present the results on value-added in Berkshire, describing the distribution of estimated value-added levels and changes, and then reporting regression results. Second, we use the longer run of data on gross output (percentage of students with at least 5 C or better grades) for the three areas with useable boundary changes.

a) *Value Added Results for Berkshire*

We begin by looking at the distribution of school value added. Figure 2 shows the distribution of value added[9] by treated group (those schools in Berkshire) and year. The width of each bar in a histogram is constrained to equal 1 so that the height of each bar represents the proportion of schools in that value added band.

The VA distribution for the treated schools is less concentrated than the untreated schools due to the relatively smaller number of schools. The mean VA for the treated schools slightly decreases over the 2 time periods, due to a reduction in high school VA levels in 2002. Figure 3 presents the distribution of the change in VA by treatment group. The distribution is slightly skewed to the left for the treated schools, but this is due to one school that had a particularly large fall in VA.

[9] Value added are estimates from equation (5)

As discussed above, Foundation and Voluntary Aided schools in England have more freedom over their own admissions criteria (as decided by the governing body) under the code of practice. Figure 3 shows that these schools saw a decrease in VA between the two time periods. This is examined further in our regression analysis below.

Turning to the regressions, we report both pupil-level and school-level results. The advantage of the pupil-level regression is that it involves a single estimation, and clustering standard errors at the school level ensure that we are relying on the right degree of variation in the data. The advantages of the school-level analysis are that it uses an unrestricted estimate of the change in school effect, and allows us to look directly at that change.

We present estimates of the effect of the boundary change using our pupil level regression, equation (4), in Table 2 and consider four different characterisations of the treatment effect. The first definition defines the treatment group as all schools in the Berkshire LEA that experienced a change in competition; this is in column 1. Second, in column 2, we separately distinguish schools in Berkshire that did not experience any change in competition, giving a treatment group, an untreated Berkshire group and a control group. The geography of Berkshire's schools meant that schools faced varying degrees of competition change when the boundaries changed. The more densely populated school areas saw some of the largest decreases in competition because they were near the new LEA borders. However, some schools experienced no change in competition, particularly those situated in the middle of a newly created LEA. Given this variability in competition change within our treated LEA, we consider different measures of treatment intensity alongside a simple dichotomous measure of change or no change. Turning to treatment intensity, there is no general presumption whether it is absolute or proportional changes in competition that might matter, so we examine both. In column 3 we split schools in Berkshire into one of three groups: no change, a small change in competition (less than a 50% fall) or a large change (more than a 50% decrease). Finally in column 4, we account for the competition level of each school in Berkshire before the boundary change. We control for schools that started with either a high or low level of competition (defined as above or below the median competition level within Berkshire) and then saw either a decrease or no change in competition.

From columns 1 and 2, we find that a decrease in competition for our treated schools has a negative effect on pupil outcomes. The coefficient on the treated dummy interacted with time remains negative when we also control for schools in Berkshire that didn't have a change in competition in column 2. However, neither of these effects is statistically different from zero. These schools with no change in competition had almost no change in pupil outcomes. Column 3 shows the varying effect of the boundary change within those schools that saw a decrease in competition as a result. Again, the overall effect is negative, but schools that had the smallest percentage decrease in competition had the largest drop in student test scores. Our final treatment breakdown reveals that the change in test scores does not vary according to whether the initial level of competition was high or low. Similar results are found if we use simple changes in the school variables rather than in the bias control form of equation (4). All the results in Table 2 are presented with clustered standard errors at school level.

We present the results of our school level analysis, equations 6 and 7, in Tables 3 and 4. Table 3 shows the treatment effect on VA levels. We present the same four specifications again in columns 2 to 5. In column 1, no controls are included except for a treated dummy, a time dummy and an interaction between the two; this is the estimate of the difference in difference. The results are very similar to above: the point estimates are all negative but insignificant. Table 4 presents results where the dependent variable is the change in value added. Column 1 is the difference in difference estimate again, as in Table 3, reported here for reference. Column 3 shows a bigger fall for treated Berkshire schools than untreated Berkshire schools, and bigger again than schools in control LEAs. Column 4 shows a greater point effect for schools experiencing a smaller decline in competition, but again not a significant difference. In column 5, we see that schools that had a high level of competition performed badly even if they saw no change in competition. This may indicate that our measure of competition is not broad enough, and that the high competition schools did in fact perceive a fall in the number of alternative schools. If this is the case, it suggests that the column 2 results comparing all Berkshire schools to non-Berkshire controls are to be preferred to those distinguishing within Berkshire schools.

In Table 5, we look specifically at foundation and voluntary aided schools within our treated groups. What can be clearly seen is a significantly large negative treatment effect for these schools. Again, we suffer from a lack of data, as only 4 foundation or voluntary aided schools had a decrease in competition. We present the results for the breakdown of intensity of treatment in column 3, showing that it is the Foundation and VA schools with smaller changes in measured competition that drive the results. We do not repeat column 4 as it makes no sense to further sub-divide the small number of such schools.

b) GCSE Score Results for Avon, Cleveland and Berkshire

We use data on the percentage of a school's students who achieve at least 5 C grade or better GCSE grades. This has been the standard benchmark of school performance, but does not take into account the quality of the school's pupil intake. We again divide schools into those within the original LEA with a change in competition, those with no change, and a control group from other matched LEAs. The unweighted mean scores for these groups of schools for the three areas are plotted in Figure 4. The figures all show the general upward trend in standards over this period. In Berkshire and Cleveland, there are no obvious differences in trend between the groups of school experiencing a change in competition and those not. However, for Avon, the treated schools clearly show a decline in performance against the un-treated after the change in boundaries.

More formally, we carry out the difference-in-difference regressions on a balanced panel of school, estimating equation (11). The dependent variable is the percent 5 A* to C grades, purged of gender effects. We include school fixed effects, and school level controls. The results are in Tables 6 (Avon), 7 (Berkshire) and 8 (Cleveland). In all the three areas, we see a negative effect of the fall in competition. However, only in Avon is this of any quantitative or statistical significance. Here, the treatment effect is of the order of 3 percentage points, slightly larger for the schools that saw a big fall in the degree of competition. Note that this is a different dependent variable to the VA regressions (the school % 5A*-C grades rather than mean GCSE points), so the size of the coefficient is not directly comparable to that above.

For Berkshire, unsurprisingly the results in Table 7 mirror those reported above for VA. There is no significant effect of the treatment on GCSE scores. The table does show a significant negative effect on schools with no measured change in competition. Since this did not show up in the VA regressions, this must be due either to the longer run of data, or to changes in the characteristics of pupils attending the schools. In particular, if these schools saw no change in VA but a decrease in GCSE score, this suggests a lower level of prior attainment in these schools. This may reflect the outcome of a complex re- sorting of pupils to schools given the new boundaries.

The results for Cleveland in Table 8 show no effect of the treatment at all. Almost all of the schools seeing a fall in competition were in Middlesbrough, a poor area with low scoring schools, but the change appeared to have no effect on their performance. In some ways, the Cleveland context is not unlike that of Avon, but in this case, there is no significant change in the relative performance of the treated schools.

5. Discussion and Conclusions

An important part of the argument in favour of greater choice and competition between schools is that it will help to raise standards across the board. In a system where money follows pupils, schools will want to perform well in order to attract pupils. There is a very limited amount of good evidence available on this mechanism in the UK, rather more for other countries. As always, identification is a key problem. In this paper, we use an exogenous change in the boundaries of an education market to identify the impact of a change in the degree of competition between schools. We find only very weak support at best for the view that school competition will raise standards.

We show that the boundary change was politically driven, and divorced from the educational context. The nature of the change was the division of a large education market (LEA) into a number of smaller ones. Since the number of alternative schools available to parents fell, we would expect less pressure on schools to perform well, and consequently test score results to fall.

We use data that can identify this effect. We look at the value-added that the treated schools add to two cohorts of pupils, before and after the boundary change, and compare to the change in value-added in a set of control schools. Thus we control for pupils' prior attainment, other pupil characteristics, school effects, and general time effects. In fact, we exploit two levels of control schools – schools in other LEAs with no boundary changes, and schools in the affected LEA but sufficiently far from the new LEA boundaries to experience no effective change in the degree of competition.

Our difference-in-difference approach shows no significant change in value-added. Whilst the point estimate is of the expected sign – the fall in competition reduced test scores – the effect is not statistically significant. We do find a significant effect for a group of schools with more control over their own admissions, but this is based on only a small number of such schools. It may be that schools which are more active in pupil intake management, are also more active in responding to market conditions. Using our longer-run data and three boundary change areas but without prior attainment controls produces mixed results. We find evidence in one area, Avon LEA, of a significant effect, and insignificant effects in the other two.

It is clear from these results that any impact of competition on outcomes is heterogeneous and complex. It could perhaps be argued that these schools were not really in a competitive environment. In fact, the key conditions for it to be so are met (see Hoxby, 2003): money followed pupils, parents were informed about school quality, and the outcome was a high-stakes test. And whilst schools' ability to expand rapidly was limited (thus blunting the incentive to attract more pupils), there remained the incentive of attracting better quality pupils. Alternatively, it may be that a different driver for efficiency replaced the fall in the degree of competition: "voice". The idea is that parents, keen to protect the value of their houses, exerted pressure on Headteachers to maintain standards and league table position[10]. It might conversely be argued that voice is only effective given choice, and the fall in the latter meant parents had no incentive to engage in the former. It seems unlikely that choice and voice should be equally effective in influencing school practice, and the effects seen here can be thought of as the net outcome of the change in competition and any consequent change in voice.

The apparent heterogeneity of outcomes across our three boundary change cases for gross output is intriguing. Taking the point estimates at face value, it may be that competition only works in certain circumstances[11]. For example, the unavailability of

[10] Thanks to Tim Besley for this comment; see Bayer and McMillan (2005).
[11] Equally, it could be that these are three noisy estimates of the same number.

some schools only matters if those schools would have been chosen by significant numbers of parents. That is likely to depend on the perceived difference in quality between them and nearby schools. In modelling terms, that quality gradient is an outcome of the full general equilibrium model of residential choice, school choice and peer effects, which is beyond this paper[12]. It is interesting to note that in the case of Avon, there was a steep quality gradient between the schools in the city of Bristol, and those in some of the areas surrounding the city, suggesting that the removal of the opportunity to go easily to those schools was a real change in the competitive environment. But the same is true in the case of Cleveland for schools in Middlesbrough relative to its surrounding area. Bristol is a richer city than Middlesbrough, and it may be that the ability to finance the longer school commutes was lower in the latter than the former, implying that their unavailability after the boundary change was largely irrelevant to the practical choices of most parents. This would explain the greater estimated effect of the boundaries changes in Avon than in Cleveland.

To conclude, this study uses a robust research design to identify the effects of school competition on test score outcomes. At best, the impact is quantitatively small and contingent on particular market characteristics.

[12] See Bayer and McMillan (2005) for such a model for the (very different) US context. Such a model is unlikely to be useful for England as it relies on very tight local catchment areas around schools.

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Figure 1

Map of schools in Berkshire and their corresponding change in competition after the LEA boundary changes

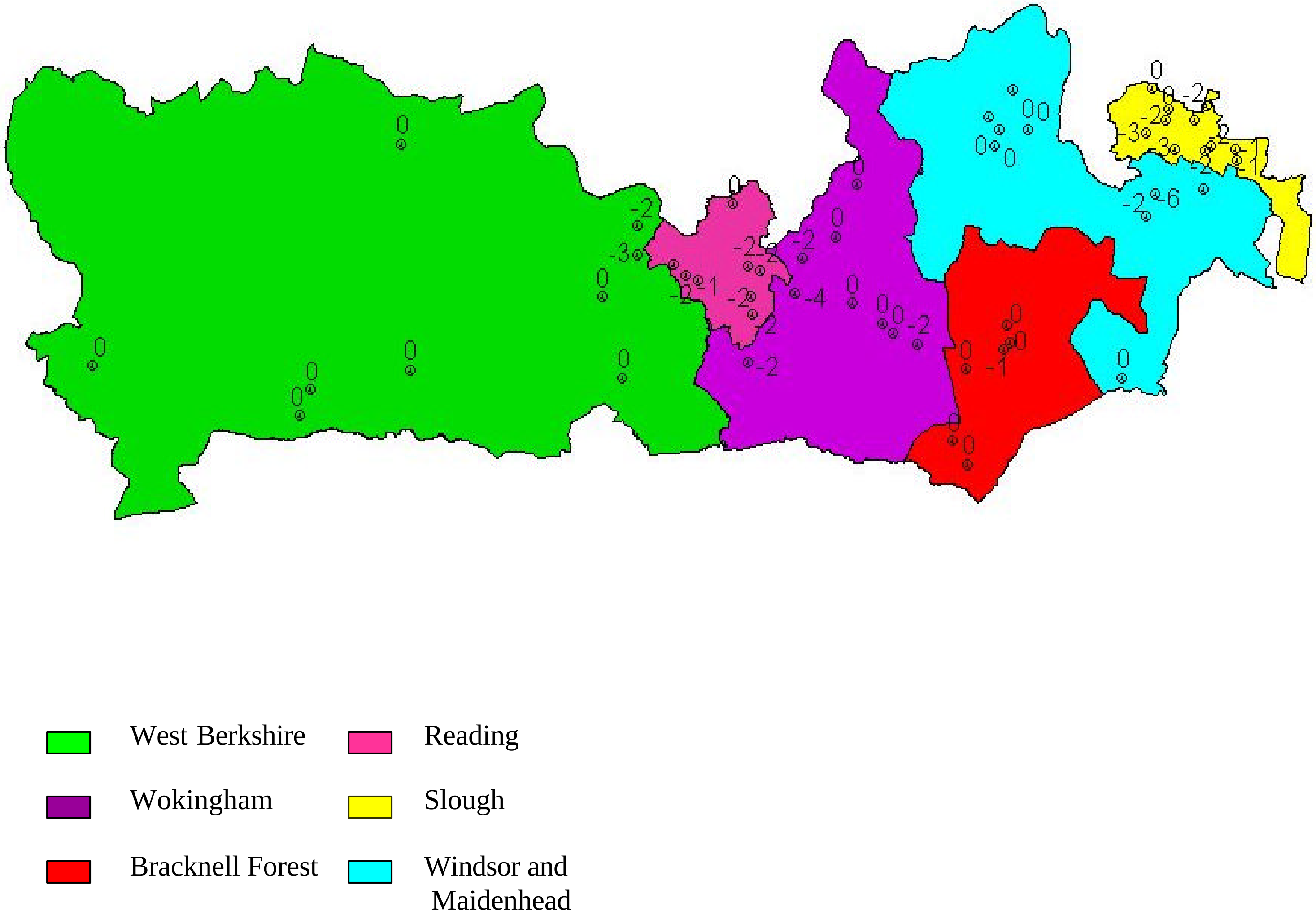


Figure 2: Distribution of School Value-added

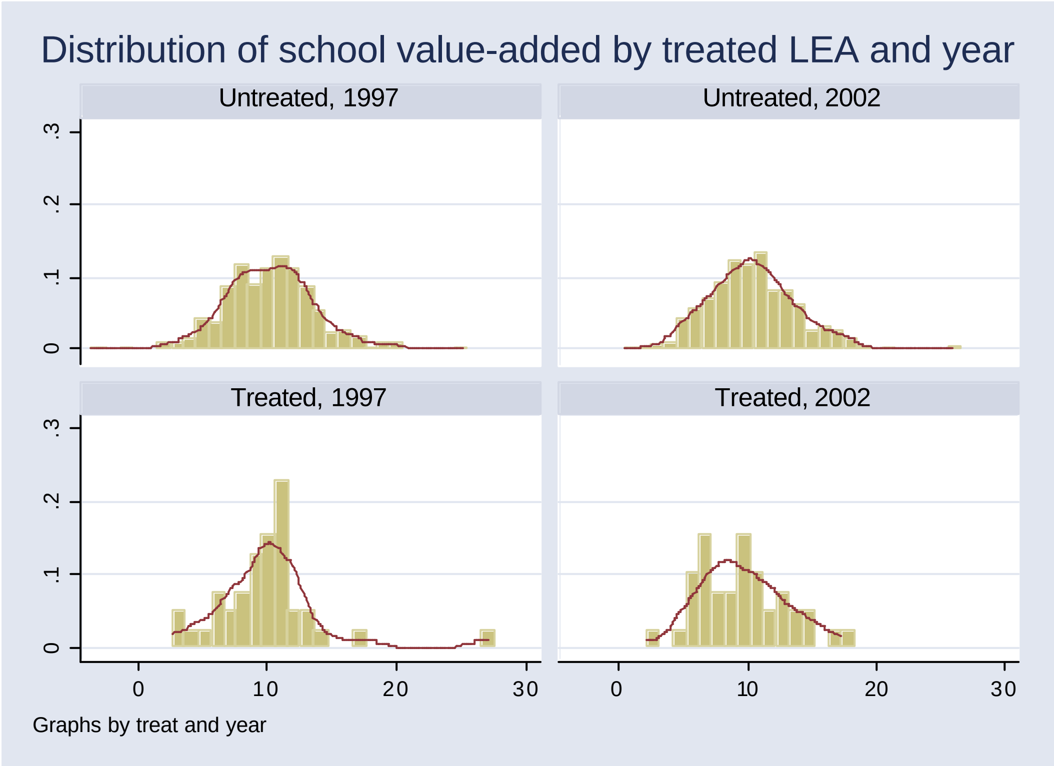


Figure 3: Distribution of Changes in School Value-added

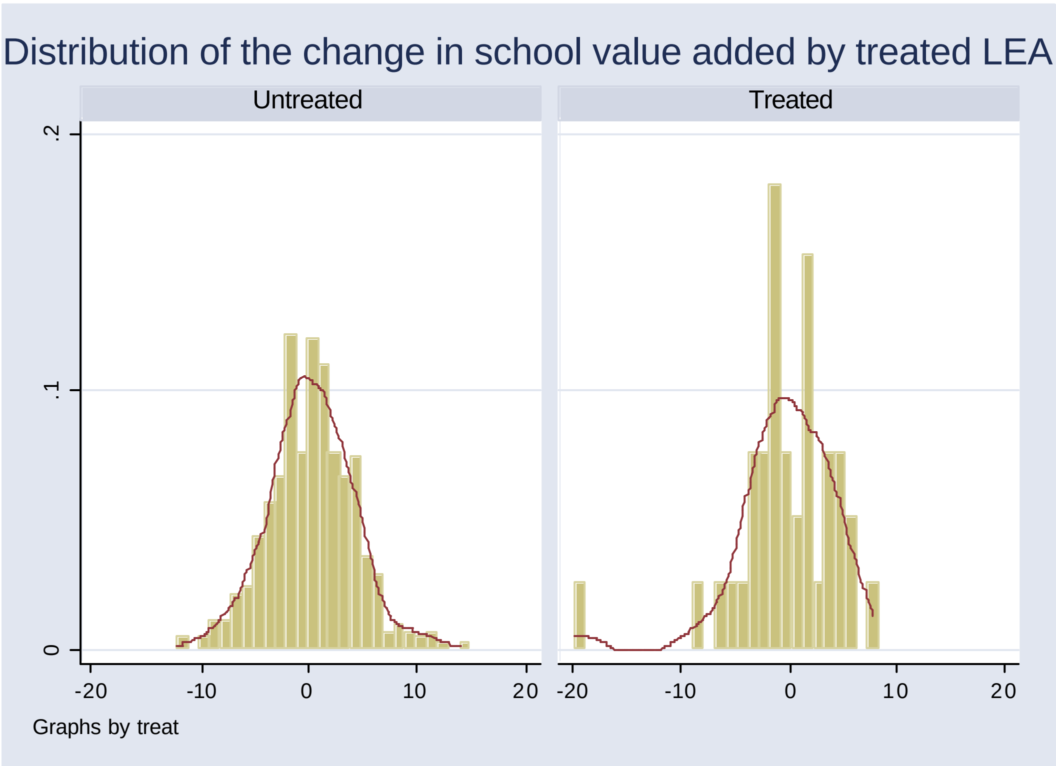
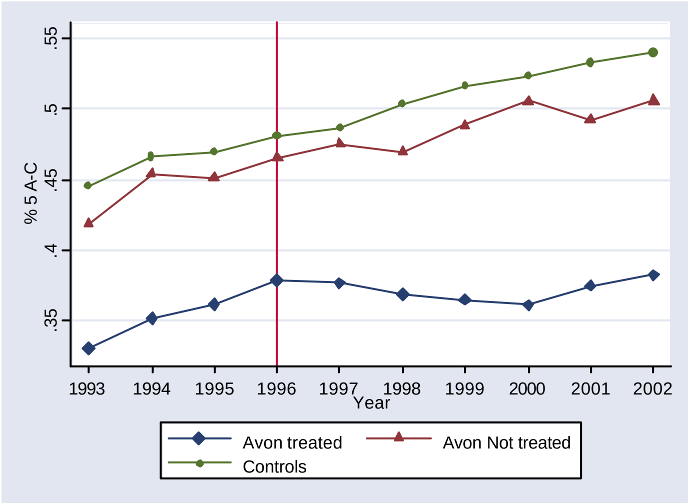
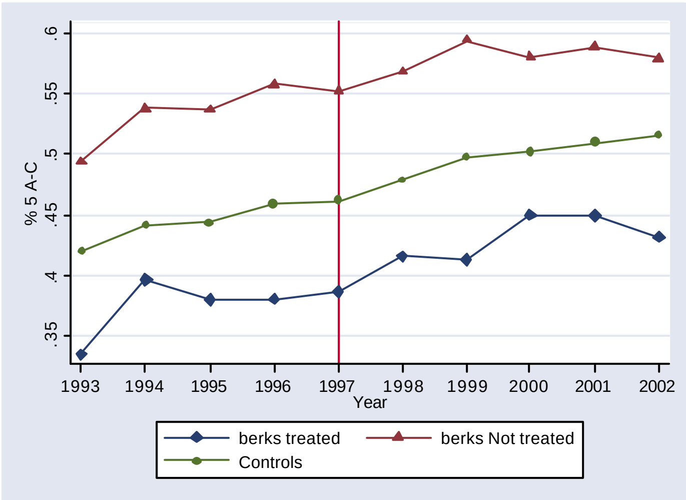


Figure 4: GCSE Scores over time by Groups of Schools.

Avon:



Berkshire:



Cleveland:

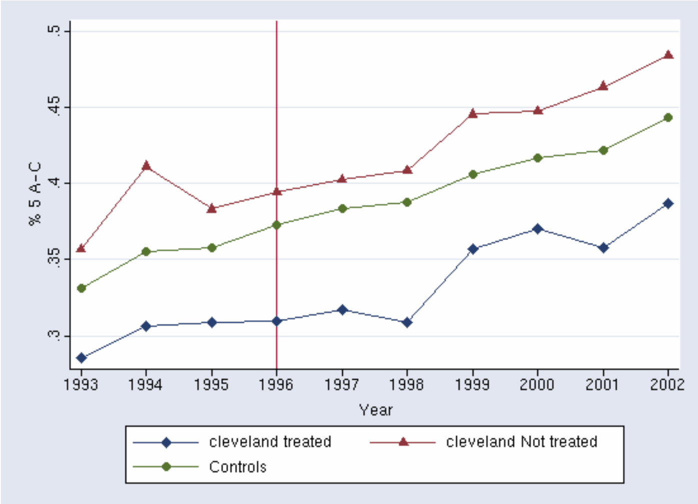


Table 1: Means and standard deviations of variables, Berkshire VA

Variable	Overall		Untreated		Treated	
	Mean	Standard deviation	Mean	Standard deviation	Mean	Standard deviation
Pupil level controls, n=#	175716		161079		14637	
Total GCSE point score	40.60659	17.83615	40.50314	17.85752	41.74256	17.56041
KS3mn	32.95334	6.255073	32.89721	6.238468	33.57124	6.4028
MALE dummy	.5043422	.4999826	.5042557	.4999834	.5052948	.499989
Born in January	.0784049	.2688085	.0777755	.2678188	.0853317	.2793842
Born in February	.0712229	.2571975	.071406	.2575025	.0692082	.2538165
Born in March	.082935	.2757847	.0821895	.274654	.0911389	.2878164
Born in April	.0786439	.269183	.078446	.2688729	.0808226	.2725718
Born in May	.0833163	.2763604	.0833069	.2763465	.0834187	.2765235
Born in June	.0813529	.273377	.0810782	.2729561	.0843752	.2779592
Born in July	.0851431	.2790953	.0850887	.2790145	.0737856	.2799917
Born in August	.082326	.2748616	.0831021	.2760373	.0857416	.2614306
Born in September	.0799813	.2712651	.0803208	.2717901	.0762451	.265399
Born in October	.0787179	.2692988	.0786757	.2692327	.0791829	.2700332
Born in November	.0728562	.2599011	.0727717	.2597623	.0737856	.2614306
Born in December	.0721619	.2587565	.0728649	.2599154	.0644258	.2455182
Month of birth missing	.0529377	.2239098	.052974	.2239823	.0525381	.2231171
School level controls, n=456	456		417		39	
Foundation	.1447368	.3522217	.146283	.3538142	.1282051	.3386884
Voluntary Aided	.1337719	.340781	.1414868	.3489417	.0512821	.2234559
Voluntary controlled	.0307018	.1726979	.028777	.16738	.0512821	.2234559
Community	.6907895	.4626757	.6834532	.4656876	.7692308	.4268328
All boys school	.0328947	.178557	.028777	.16738	.0769231	.2699528
All girls school	.0394737	.1949329	.0335731	.1803441	.1025641	.3073547
Mixed school	.9276316	.2593815	.9376499	.2420808	.8205128	.3887764
Cohort size	190.4167	60.65798	190.9089	61.39356	185.1538	52.55751
Urban	.6951754	.4608387	.6810552	.4666275	.8461538	.3655178
% pupils with Free School Meals	.0899933	.0813487	.0920636	.0820165	.0678568	.0710461
% pupils with English as a second language	.049403	.0913743	.0472034	.0913555	.0729216	.0893631
% pupils with special education needs	.1755078	.0972233	.1757676	.0967499	.1727306	.1034297
% pupils with ethnicity unknown	.0815543	.1374896	.0817198	.1376454	.0797847	.1375794
% Black Caribbean pupils	.0049088	.0113683	.004327	.0097562	.01113	.0214946
% Black African pupils	.0031114	.0075585	.0027111	.005951	.0073919	.0166075
% Black other pupils	.0042187	.0085213	.0043922	.0087654	.0023635	.0049461
% Indian pupils	.0120145	.0236467	.0114034	.0232954	.0185483	.0265804
% Pakistani pupils	.0158984	.0487506	.0145682	.0478832	.0301209	.0559446
% Bangladeshi pupils	.0036183	.0145474	.0038224	.0151745	.001436	.0027979
% Chinese pupils	.0031534	.0055822	.0032447	.0057366	.002177	.0034327
% other ethnicity pupils	.0129188	.0190384	.0118931	.0175432	.0238867	.0289102
Church of England/Roman Catholic	.0032895	.0572909	.0035971	.059904	0	0
No Religious affiliation	.0328947	.178557	.0311751	.1739994	.0512821	.2234559
Roman Catholic	.0767544	.2664937	.0815348	.2739833	.025641	.1601282
Religion does not apply	.8245614	.3807595	.822542	.3825148	.8461538	.3655178
Christian	.002193	.0468293	.0023981	.0489702	0	0
Church of England	.0570175	.2321308	.0551559	.2285585	.0769231	.2699528

Table 2: Pupil level VA regressions - Berkshire

Unit of observation is a pupil; Dependent variable – total pupil point score at GCSE

	(1) FE	(2) FE	(3) FE	(4) FE
Schools in the treated LEA that had a decrease in competition after the LEA boundary changes * time dummy	-1.482 (1.924)	-1.503 (1.928)		
Schools in the treated LEA that saw no change in competition after the LEA boundary changes * time dummy		-0.375 (0.692)	-0.366 (0.691)	
Schools that saw between a 0 and 50 % decrease in competition in the treated LEA * time dummy			-1.955 (3.484)	
Schools that saw more than a 50 % decrease in competition in the treated LEA * time dummy			-1.023 (1.182)	
Schools that had a high level of competition in the first time period and saw no change in competition * time dummy				-2.182 (1.062)**
Schools that had a low level of competition in the first time period and saw no change in competition * time dummy				0.615 (0.758)
Schools that had a low level of competition in the first time period and saw a decrease in competition * time dummy				-1.508 (0.700)**
Schools that had a high level of competition in the first time period and saw a decrease in competition * time dummy				-1.466 (2.232)
Time dummy	0.794 (0.278)***	0.812 (0.281)***	0.815 (0.281)***	0.803 (0.282)***
Pupil level controls	Yes	Yes	Yes	Yes
School level Bias controls	Yes	Yes	Yes	Yes
Observations	162513	162513	162513	162513
R-squared	0.73	0.73	0.73	0.73

Notes:
1) Standard errors in parenthesis
2) All regressions include pupil level controls as in Table 1, school level mean KS3mn, cohort size and school level bias controls (due to missing pupil level data) for: % fsm, % esl, % SEN, % ethnicity groups
3) All regressions include school fixed effects and standard errors are clustered at school level.

Table 3: School-year level VA regressions - Berkshire

Unit of observation is a school-year; Dependent variable – school value added

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Schools in the treated LEA that had a decrease in competition after the LEA boundary changes * time dummy	-1.294 (1.328)	-1.304 (1.286)	-1.318 (1.288)		
Schools in the treated LEA that saw no change in competition after the LEA boundary changes * time dummy			-0.058 (1.024)	-0.055 (1.025)	
Schools that saw between a 0 and 50 % decrease in competition in the treated LEA * time dummy				-1.827 (1.734)	
Schools that saw more than a 50 % decrease in competition in the treated LEA * time dummy				-0.697 (1.887)	
Schools that had a high level of competition in the first time period and saw no change in competition * time dummy					-1.934 (1.691)
Schools that had a low level of competition in the first time period and saw no change in competition * time dummy					0.976 (1.265)
Schools that had a low level of competition in the first time period and saw a decrease in competition * time dummy					-1.304 (1.374)
Schools that had a high level of competition in the first time period and saw a decrease in competition * time dummy					-1.400 (3.554)
School level Bias controls	No	Yes	Yes	Yes	Yes
Observations	912	912	912	912	912
R-squared	0.00	0.11	0.11	0.11	0.11

Notes:
1) Standard errors in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%
2) All regressions include cohort size, religious denomination of school dummies, single sex school dummies, an urban dummy, funding type of school dummies and school level bias controls (due to missing pupil level data) for: % fsm, % esl, % SEN, % ethnicity groups
3) All regressions are weighted by the average cohort size across the 2 time periods
4) School value added taken as a time varying fixed effect residual in a pupil level education production function

Table 4: School level VA regressions - Berkshire

Unit of observation is a school; Dependent variable – change in school level value added over the 2 time periods

	(1)	(2)	(3)	(4)	(5)
Schools in the treated LEA that had a decrease in competition after the LEA boundary changes * time dummy	-1.294 (1.076)	-1.100 (1.108)	-1.122 (1.111)		
Schools in the treated LEA that saw no change in competition after the LEA boundary changes * time dummy			-0.343 (0.876)	-0.340 (0.877)	
Schools that saw between a 0 and 50 % decrease in competition in the treated LEA * time dummy				-1.375 (1.525)	
Schools that saw more than a 50 % decrease in competition in the treated LEA * time dummy				-0.831 (1.636)	
Schools that had a high level of competition in the first time period and saw no change in competition * time dummy					-2.190 (1.448)
Schools that had a low level of competition in the first time period and saw no change in competition * time dummy					0.671 (1.082)
Schools that had a low level of competition in the first time period and saw a decrease in competition * time dummy					-1.026 (1.181)
Schools that had a high level of competition in the first time period and saw a decrease in competition * time dummy					-1.562 (3.059)
School level Bias controls	No	Yes	Yes	Yes	Yes
Observations	456	456	456	456	456
R-squared	0.00	0.03	0.03	0.03	0.03

Notes:
1) Standard errors in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%
2) All regressions include change in cohort size and change in the school level bias controls (due to missing pupil level data) for: % fsm, % esl, % SEN, % ethnicity groups
3) All regressions are weighted by the average cohort size across the 2 time periods
4) School value added taken as a time varying fixed effect residual in a pupil level education production function

Table 5: School level VA regressions - Berkshire

Unit of observation is a school; Dependent variable – change in school level value added over the 2 time periods

	(1)	(2)	(3)
Schools in the treated LEA that had a decrease in competition after the LEA boundary changes	1.470 (1.252)	1.447 (1.256)	
Schools in the treated LEA that had a decrease in competition after the LEA boundary changes and are foundation or Voluntary Aided schools	-10.018 (2.409)***	-10.007 (2.414)***	
Schools in the treated LEA that saw no change in competition after the LEA boundary changes		-0.417 (0.919)	-0.422 (0.915)
Schools in the treated LEA that saw no change in competition after the LEA boundary changes and are foundation or Voluntary Aided schools		0.954 (2.509)	0.972 (2.497)
Schools that saw between a 0 and 50 % decrease in competition in the treated LEA			3.792 (1.832)**
Schools that saw between a 0 and 50 % decrease in competition in the treated LEA and are foundation or Voluntary Aided schools			-14.552 (2.997)***
Schools that saw more than a 50 % decrease in competition in the treated LEA			-0.557 (1.717)
Schools that saw more than a 50 % decrease in competition in the treated LEA and are foundation or Voluntary Aided schools			-1.475 (4.418)
School level Bias controls	Yes	Yes	Yes
Observations	456	456	456
R-squared	0.06	0.06	0.08

Notes:
1) Standard errors in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%
2) All regressions include change in cohort size and school level bias controls (due to missing pupil level data) for: % fsm, % esl, % SEN, % ethnicity groups
3) All regressions are weighted by the average cohort size across the 2 time periods
4) School value added taken as a time varying fixed effect residual in a pupil level education production function

Table 6: School Level %5A*-C Regressions – Avon

Unit of observation is a school-year; Dependent variable –school % pupils achieving at least 5 C grades or better

	(1)	(2)	(3)	(4)	(5)
Schools in the treated LEA that had a decrease in competition after the LEA boundary changes * time dummy	-0.041 (0.011)***	-0.029 (0.011)***	-0.030 (0.011)***		
Schools in the treated LEA that saw no change in competition after the LEA boundary changes * time dummy			-0.012 (0.008)	-0.012 (0.008)	
Schools that saw between a 0 and 50 % decrease in competition in the treated LEA * time dummy				-0.027 (0.014)**	
Schools that saw more than a 50 % decrease in competition in the treated LEA * time dummy				-0.034 (0.016)**	
Schools that had a high level of competition in the first time period and saw no change in competition * time dummy					-0.002 (0.015)
Schools that had a low level of comp etition in the first time period and saw no change in competition * time dummy					-0.016 (0.010)
Schools that had a low level of competition in the first time period and saw a decrease in competition * time dummy					0.081 (0.011)***
Schools that had a high level of competition in the first time period and saw a decrease in competition * time dummy					-0.032 (0.020)***
School level controls	No	Yes	Yes	Yes	Yes
Observations	2712	2712	2712	2712	2712
R-squared	0.90	0.90	0.90	0.90	0.90

Notes:

1) Standard errors in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%
2) All regressions have school fixed effects and robust standard errors

Table 7: School Level %5A*-C Regressions – Berkshire

Unit of observation is a school-year; Dependent variable –school % pupils achieving at least 5 C grades or better

	(1)	(2)	(3)	(4)	(5)
Schools in the treated LEA that had a decrease in competition after the LEA boundary changes * time dummy	-0.001 (0.010)	-0.004 (0.010)	-0.005 (0.010)		
Schools in the treated LEA that saw no change in competition after the LEA boundary changes * time dummy			-0.022 (0.007)***	-0.022 (0.007)***	
Schools that saw between a 0 and 50 % decrease in competition in the treated LEA * time dummy				0.001 (0.016)	
Schools that saw more than a 50 % decrease in competition in the treated LEA * time dummy				-0.011 (0.013)	
Schools that had a high level of competition in the first time period and saw no change in competition * time dummy					-0.032 (0.011)***
Schools that had a low level of competition in the first time period and saw no change in competition * time dummy					-0.015 (0.009)*
Schools that had a low level of competition in the first time period and saw a decrease in competition * time dummy					0.012 (0.019)
Schools that had a high level of competition in the first time period and saw a decrease in competition * time dummy					-0.008 (0.011)
School level controls	No	Yes	Yes	Yes	Yes
Observations	3584	3584	3584	3584	3584
R-squared	0.91	0.92	0.92	0.92	0.92

Notes:

1) Standard errors in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%
2) All regressions have school fixed effects and robust standard errors

Table 8: School Level %5A*-C Regressions – Cleveland

Unit of observation is a school-year; Dependent variable –school % pupils achieving at least 5 C grades or better

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Schools in the treated LEA that had a decrease in competition after the LEA boundary changes * time dummy	-0.007 (0.011)	-0.015 (0.012)	-0.014 (0.012)		
Schools in the treated LEA that saw no change in competition after the LEA boundary changes * time dummy			0.005 (0.008)	0.005 (0.008)	
Schools that saw between a 0 and 50 % decrease in competition in the treated LEA * time dummy				-0.020 (0.013)	
Schools that saw more than a 50 % decrease in competition in the treated LEA * time dummy				-0.008 (0.020)	
Schools that had a high level of competition in the first time period and saw no change in competition * time dummy					0.004 (0.014)
Schools that had a low level of competition in the first time period and saw no change in competition * time dummy					0.006 (0.010)
Schools that had a low level of competition in the first time period and saw a decrease in competition * time dummy					0.000 (0.000)
Schools that had a high level of competition in the first time period and saw a decrease in competition * time dummy					-0.014 (0.012)
School level controls	No	Yes	Yes	Yes	Yes
Observations	2160	2160	2160	2160	2160
R-squared	0.91	0.91	0.91	0.91	0.91

Notes:

1) Standard errors in parenthesis. * significant at 10%; ** significant at 5%; *** significant at 1%
2) All regressions have school fixed effects and robust standard errors