

# **A natural experiment in school accountability: the impact of school performance information on pupil progress and sorting**

**Simon Burgess<sup>1</sup>, Deborah Wilson<sup>2</sup> and Jack Worth<sup>2</sup>**

<sup>1</sup> CMPO and Department of Economics

University of Bristol

2 Priory Road

Bristol BS8 1TX

UK

[simon.burgess@bristol.ac.uk](mailto:simon.burgess@bristol.ac.uk)

<sup>2</sup> CMPO, University of Bristol (address as above)

[d.wilson@bristol.ac.uk](mailto:d.wilson@bristol.ac.uk)

**January 2011**

## **Abstract**

We test the hypothesis that the publication of school performance tables raises school effectiveness. Our data allow us to implement a classic difference-in-difference analysis comparing outcomes in England and Wales, before and after the abolition of the tables in Wales. We find significant and robust evidence that this reform markedly reduced school effectiveness in Wales. There is significant heterogeneity across schools: schools in the top quartile of the league tables show no effect. We also test whether the reform reduced school segregation in Wales, and find no systematic significant impact on either sorting by ability or by socioeconomic status.

**Keywords:** school accountability, school effectiveness, performance tables, segregation

**JEL Codes:** I20

## **Acknowledgements**

We gratefully acknowledge the DfE for providing the English PLASC/NPD data and DCELLS for providing the Welsh data, particularly Steve Hughes at the Education and Lifelong Learning Statistics Unit. We thank Rebecca Allen, Gwyn Bevan, Howard Glennerster, Ellen Greaves, Harvey Goldstein, Rich Harris, George Leckie, Alastair Muriel, Carol Propper, Helen Simpson, Sarah Smith and Chris Taylor for comments and discussion on previous drafts. We are grateful to the ESRC for funding this project via grant RES-343-28-0001 at CMPO. The usual disclaimer applies.

## 1. Introduction

A formal school accountability system is well-established in England, dating from the Education Reform Act of 1988. The system is a mix of market-based accountability to parents, publishing information on school performance as a basis of school choice, and administrative accountability, based on the outcome of standardised student testing. In Hanushek and Raymond's (2005) terminology it is "consequential accountability" as in England funding follows pupils and so poor performance can have serious consequences. In their survey of accountability systems and student performance, Figlio and Ladd (2008) argue that it is very difficult to estimate an exogenous causal effect for two reasons: lack of an adequate control group for the counter-factual, and the typical introduction of a multi-faceted performance management reform all at once, removing the possibility of evaluating an individual component. Two of the leading attempts to evaluate the overall effect of school accountability systems echo these points (Hanushek and Raymond, 2005; and Dee and Jacob, 2009). In this paper we are able to exploit a natural experiment to evaluate a core component of the accountability system in England which allows us to side-step these major data problems.

From 1992 to 2001 secondary school performance tables were published annually in both England and Wales. They quickly gained high public recognition, with league tables published annually in national and local media and all relevant stakeholder groups very conscious of the significance of individual schools' league table positions (Wilson et al 2006). Following devolution of power over education policy, the Welsh Assembly Government abolished the publication of these tables, announcing its decision in July 2001. Other parts of the system continued unchanged: for example, Wales continued national testing against a common core curriculum at the end of compulsory schooling (more detail is given below). Our data constitute a natural experiment that removed a key element from the accountability system of two otherwise-identical education systems, allowing us to circumvent the issues raised by Figlio and Ladd (2008). First, we have a ready-made control group in students in England. The education systems of the two countries were practically identical until devolution of power over education to the Welsh Assembly Government in 2001, certainly all the major system features were the same. Second, there were few other major changes to the education system in Wales at the same time. Much of the system

continued the same: the national curriculum, the school inspection system, the teaching, exam-setting and marking of the tests taken the end of compulsory schooling, which remained as high-stakes as ever for the students in Wales.<sup>1</sup>

We test the hypothesis that school effectiveness in Wales after league table abolition is lower than that of schools in England. A theoretical basis for the accountability system is a principal-agent model; the publication of school performance tables helps to monitor the output of the school. These tables are scrutinised by parents who may react by avoiding low-performing schools and by the education authorities who may impose sanctions, either explicit (as in the US in the No Child Left Behind (NCLB) Act of 2002) or implicit (in England). So stopping their publication will affect both market-based and administrative accountability systems. It may also be the case that stopping publication will shift schools' focus away from improving the reported test scores and towards broader educational outcomes. Our data allow us to implement a classic difference-in-difference analysis comparing outcomes in England and Wales, before and after the reform. Furthermore, we can also estimate a triple difference by exploiting the fact that we also have data for primary schools, for which there was no equivalent differential reform.

We find systematic, significant and robust evidence that abolishing school league tables markedly reduced school effectiveness in Wales. The impact is sizeable: a fall of 1.92 GCSE grades per student per year, equivalent to 0.23 of a (school-level) standard deviation. The key published performance measure, the percentage of students achieving at least five good GCSE passes, falls by 3.4 percentage points per school, or 0.24 of a standard deviation. Figlio and Ladd (2008) make the point that effect sizes are usually quoted in terms of pupil-level standard deviations, so these are equivalent to 0.086 and 0.068 of the respective pupil-level standard deviations. These are sizeable numbers: they are equivalent to optimistic estimates of the effect sizes for class size reductions of around 30%<sup>2</sup>. The fact that we test separately for effects on the published outcome measure and a non-published outcome measure, and find very similar and significant effects on both, suggests that the outcome is not simply about gaming the published measure. Furthermore, the national trends in the completely separate PISA tests (described below), which are unaffected by gaming, mirror our results very closely.

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<sup>1</sup> There were some changes to low-stakes testing at younger ages, discussed below.

<sup>2</sup> Calculations based on Angrist and Lavy (1999)

We find significant heterogeneity across schools: the effect is concentrated in the schools in the lower 75% of the distribution of ability and poverty. Schools in the top quartile of the league tables show no effect. This is in contrast to the findings of Dee and Jacob (2009) who find least effect of NCLB on disadvantaged students; Hanushek and Raymond (2005) find that Hispanic students gain the most and Black students the least from the introduction of accountability. We show that the effect is robust across levels of local competition between schools, and across rural and urban areas.

Closest to our analysis are the papers by Hanushek and Raymond (2005) and Dee and Jacob (2009). The former use a state-level fixed effects model to identify the effect of introducing state accountability and find a positive effect of around 0.2 of a (state-level) standard deviation on test scores. They also find that this effect is only significant for accountability systems where direct consequences, such as teacher financial incentives or private school vouchers, are attached to poor performance. Dee and Jacob (2009) similarly use the federal introduction of NCLB to estimate its effect on school performance. States that had implemented a system of school accountability before NCLB, and hence were unaffected by it, were employed as a control group. They found that NCLB had no impact on reading scores and a 0.15 pupil-level standard deviation impact on 4<sup>th</sup> grade maths scores. Figlio and Ladd (2008) provide a comprehensive summary of the recent literature; see also Wilson (2010). Much of the literature focuses on the ways in which publishing school rankings can induce gaming responses from schools (see, for example, Neal and Schanzenbach 2007; Reback 2008; Jacob and Levitt 2003; Burgess et al 2005).

A common critique of school league tables is that their publication encourages social segregation by indirectly informing parents which schools have high concentrations of more advantaged students. A more indirect argument in West et al (2006) is that the potential for cream skimming by over-subscribed schools may lead to considerable differences in composition between over- and under-subscribed schools. We test the implied hypothesis that league tables lead to greater sorting by ability, as those high-performing, over-subscribed schools admit more able pupils. With regard to socio-economic sorting, the model by Hoyle and Robinson (2003) predicts increased socioeconomic stratification where league tables are published as part of a system of school choice, as parents from higher socioeconomic backgrounds, most able to exercise choice through the housing market, are attracted to the best performing schools. An alternative view is that publishing performance tables

democratises the information that higher socio-economic status families are aware of through their social networks, thereby reducing sorting.

We are able to test these competing hypotheses in our data. We consider the standard segregation indices, and also the evolution of school-level poverty rates, and look for any changes in the student assignment function from neighbourhood poverty to school poverty. Whilst sorting is clearly not a short run phenomenon, we have six years of data for this analysis. We show that the policy change had no systematic significant impact on either sorting by ability or by socioeconomic status. There is a suggestion in one analysis of a slight polarisation in Wales relative to England but no evidence in any of the analyses of a relative fall in sorting in Wales.

The rest of the paper is structured as follows. In the next section we describe the policy environment in England and Wales, discussing in detail the policy change that we exploit in this analysis. Section 3 outlines the methodology we employ, and section 4 details our data. We present our results in section 5, and offer some conclusions in section 6.

## **2. The Policy Environment and the Policy Change**

### ***National testing and performance tables***

In England and Wales pupils in state-funded schools all follow the same National Curriculum and progress through a series of four Key Stages. Our focus is on secondary education, where pupils enter from primary schools at the age of 11, having just taken tests marking the end of Key Stage 2. Key Stage 3 covers the first three years of secondary schooling and is completed at age 14, and Key Stage 4 leads to the exams at the end of compulsory education, known as GCSEs and taken at age 16. During the time period covered by our analysis, tests in English, maths and science were compulsory at the end of Key Stage 3 (KS3)<sup>3</sup>. Key Stage 4 exams are taken by pupils in May and June each year with the results released privately to pupils at the end of August. These are high-stakes exams, crucial for progression into further education and valued by employers.

In the November of each year performance tables are published by the government detailing the achievement of each school's pupils in the most recent exams with a variety of measures.<sup>4</sup>

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<sup>3</sup> These tests have since been abolished.

<sup>4</sup> See <http://www.dcsf.gov.uk/performance/tables>.

National and local media publish the data in the form of ranking schools on the basis of one selected performance measure, which has served to substantially focus attention on this one aspect of school performance (Wilson et al 2006). This key indicator on which schools are ranked is proportion of pupils gaining 5 GCSEs at grades A\*-C (which is the minimum requirement to go on to post-16 education), though usually other performance indicators are reported alongside.<sup>5</sup>

### ***Policy change***

Following the election of the UK Labour Government in 1997 a referendum was held in Wales to determine whether the population thought powers should be devolved away from Westminster to an assembly in the capital, Cardiff. A majority favoured the motion and the National Assembly for Wales was created by the Government of Wales Act in 1998. The Welsh Assembly was responsible for spending the Welsh block grant from the UK, which included health and education spending, and for setting policy in those areas. In July 2001 the Welsh Assembly Government announced that it would no longer publish secondary school league tables at a national or local level, having published them since they were introduced in England and Wales in 1992.

The main motivation for the policy change was the Welsh Assembly Government's eagerness to implement a Welsh alternative to the perceived 'consumerist' approach to the provision of public services pursued by the New Labour government in England. League tables, according to the evidence in a Welsh Assembly Government consultation, "do not have the support of either the teaching profession or members of the public" (WAG 2001). Reynolds (2008) argues that the policy change was motivated by "the left wing political history of Wales and the use of government to ensure enhanced social justice for Welsh citizens, [which] created a climate of greater trust in producer determined solutions" (Reynolds, 2008: 754).

The policy was announced in the July of 2001, just after one cohort of pupils had taken their GCSE exams. This is cohort B on the timeline in Figure 1. Pupils from cohort B and the teachers teaching them would have expected their school's results to be published in the following November just as all prior cohorts had been. However, as a result of the immediate implementation of the policy in Wales, no league tables were published in November 2001. In Wales, it only became apparent that the exam results would not be published after the

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<sup>5</sup> See, for example, <http://news.bbc.co.uk/1/hi/education/8439617.stm> (accessed 14.7.10).

exams had been taken in May/June, therefore we can categorise cohort B as the last cohort before the policy was introduced. Cohorts taking GCSEs in Wales after July 2001 (cohort C onwards) would have been aware their school's results would not be published, so comprise our treatment group. In looking at school performance we will focus our attention on cohorts E onwards, as they selected which KS4 qualifications they were going to study after the policy was announced. So we investigate the impact of league tables on school effectiveness by comparing the outcomes of the wholly 'before' cohorts (A and B) with those of the wholly 'after' cohorts (E onwards) in England and Wales.

The timeline relevant to our sorting hypotheses is shown in Figure 2. Here we are interested in the ways pupils are allocated to secondary schools around the time of the policy change, and so the 'before' and 'after' cohorts are different to those defined above. Cohorts E – H all applied to secondary school in September with the most recent GCSE results from those schools having been published the previous November. Pupils in cohort H, for example, applied in September 2001 and were able to use the information in the November 2000 League Tables to inform their choice. Whereas in England up to date information was available for the cohorts after this, in Wales in November 2001 cohort B's GCSE results were not published, meaning cohort I had no more recent league tables to inform their choice. So we investigate the impact of league tables on sorting by comparing the outcomes of the 'before' cohorts (E – H) with those of the 'after' cohorts (I onwards) in England and Wales.

### ***Other potential off-setting information or policy changes***

To be sure that our natural experiment design is a good one for assessing the effect league tables have on school performance we need to be convinced there were no underlying differences or additional changes around the time of the policy change between England and Wales that might bias our estimate of the effect we are interested in. Our identification relies on the assumption that abolishing league tables in Wales reduced the amount of information available to parents choosing schools as well as reducing the potential for publicly identifying poor performers. We therefore need to investigate whether other sources of information became available in Wales after the policy announcement, potentially offsetting the loss of information and estimates of the policy effects.

One concern was that school inspections would be increased in frequency or intensity in Wales following the policy change, which would in turn increase the information available to

parents. England and Wales had separately run school inspection organisations – Ofsted (the Office for Standards in Education, Children’s Services and Skills) and Estyn<sup>6</sup>, respectively – after devolution, so this is a distinct possibility. However, this has not been the case. In both countries schools were inspected at least every six years and are more frequently inspected if they are performing poorly. During the period of our study there were no major differences between the England and Wales inspection regimes (Reynolds 2008).

Another concern is that while newspapers do not publish school performance data in Wales, schools themselves may publish the information on their websites or in their prospectuses. Indeed, when announcing the policy change the minister declared that “schools will continue to publish their own results in their prospectuses and Governors’ Annual Reports” (WAG 2001)<sup>7</sup>. We compared secondary schools in Cardiff with the schools in two English cities with similar pupil compositions – Newcastle and Plymouth – to see whether there were differences in what schools reported (findings are available on request). We found that a majority of schools (12 out of 17) in Cardiff reported the percent achieving 5 GCSE grades at A\*-C in the most recent exams either on their website or in their Annual Report, but some did not. The five that did not report their results had a significantly lower 5 A\*-C score than the local average. In Newcastle, all ten secondary schools reported their most recent 5 A\*-C results. In Plymouth, a majority of schools (10 out of 16) did not report their exam results; the six that did all performed above average.

A full investigation into the information schools publish in their prospectuses is beyond the scope of this paper, but our findings suggest a mixed picture across England and Wales. How much information schools publish themselves seems to depend on how well they have performed and on the level of competition in an area’s education market, which is something we control for in our analysis. Nonetheless, even if all schools publish their recent results, the policy change led to a reduction in the amount of information collated centrally and made publicly available as part of the systems of school accountability in England and Wales.

Another possibility is that private *ad hoc* websites sprang up to take the place of the official performance tables. While we cannot rule this out completely, a web search has not found

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<sup>6</sup> Estyn provides access to the individual school inspection reports by sector and by LEA, but does not provide any comparative information. See [http://www.estyn.gov.uk/inspection\\_reports.asp](http://www.estyn.gov.uk/inspection_reports.asp), accessed 10/7/2010.

<sup>7</sup> Performance information for each individual school is still available on the Welsh Assembly Government website: <http://www.statswales.wales.gov.uk/TableViewer/document.aspx?ReportId=18342>



any. Also, the website of the Times newspaper's ParentPower service<sup>8</sup>, which provides a long list of websites with school comparison tables, has none for Wales.

We have also investigated potential systematic changes to the broader accountability mechanisms within which schools in England and Wales have been operating through the time period of our study and which could potentially undermine our identification strategy. A system of school choice still operates in both England and Wales. Burgess et al (2009) describe the English system in some detail. In Wales, for example, Cardiff's admission guide states 'Parents have the right to express a preference which will be considered individually and complied with wherever possible' (Cardiff Council, 2009: 5). In both countries geographical proximity is used as the main criterion for allocation of places to over-subscribed schools. Straight line distances are generally used by English Local Authorities; in Wales there is more focus on pre-defined, explicit catchment areas, although places are not guaranteed to all children living in those areas. Details of the overall system of school choice differ both across local authorities and through time; we have not found any evidence of systematic change around the time of the policy change investigated in this analysis which could undermine our identification strategy.

This is also the case with regard to the more 'top-down' accountability mechanisms. After the abolition of league tables in Wales, school performance data continued to be collected and analysed by local government and used as "the basis for challenge and discussion of strengths and weaknesses" (WAG 2008: 21). What changed was the previously very public, comparative and 'high stakes' nature of the performance information (still in evidence in England). School closures are not directly or publicly related to a school 'failing'; rather there are various intervention strategies employed to help such schools improve. This runs alongside planned reorganisations of local school places arising due to demographic change. A full historical analysis of local authority education policy is outside the scope of the current analysis, but again we have found no evidence of a systematic policy change in this area that could bias our estimates.

### **3. Methodology**

We identify the effect of school league tables on outcomes by exploiting the natural experiment described above. We use a difference-in-difference methodology, employing

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<sup>8</sup> <http://www.timesonline.co.uk/parentpower/links.php>, accessed 10/7/2010

English schools as a control group for the policy change affecting Welsh schools. The principle throughout our analysis is to difference out common time effects and control for the fixed characteristics of the treatment group to identify the effect of the policy on outcome  $y$  in the treatment group. To investigate the impact of the policy change on school performance we estimate the following model for cohort  $t$  in school  $s$ :

$$(1) \quad y_{st} = \alpha w_{st} + \rho_t a_{st} + \eta_s + \delta_t + \beta X_{st} + u_{st}.$$

Our main variable of interest is the ‘policy off’ variable  $w_{st}$ , which is equal to one for Welsh schools after the policy change and zero otherwise. We include a cohort\*school-specific measure of prior attainment  $a_{st}$  to control for pupil ability. We also include school fixed effects  $\eta_s$ , cohort dummies  $\delta_t$  and time-varying school control variables  $X_{st}$ . We estimate the model using the within estimator, weighted by the size of the school and cluster standard errors at the Local Education Authority (LEA) level.

A key identifying assumption underlying the use of England as a counterfactual is that the time effects during the period are common to both countries. Institutional differences between the English and Welsh education systems were small before the league table policy change and did not change dramatically during our study period. The National Qualifications Framework ensured that qualifications attained by pupils across the countries were comparable during this period and none of our cohorts studied for a Welsh Baccalaureate or a Diploma, which has since led to a divergence of qualification regimes. Both countries also define Free School Meals eligibility identically. This suggests that any macro effects, for example idiosyncratic shocks to examination results or changes to the poverty rate, should affect each country similarly. We also check for common prior trends.

In our investigation of the effects of league table abolition on sorting we estimate the following model:

$$(2) \quad y_{jt} = \alpha w_{jt} + \eta_j + \delta_t + u_{jt},$$

where  $y_{jt}$  is the Dissimilarity Index for cohort  $t$  in LEA  $j$ . We weight observations by cohort size and include LEA fixed effects and cohort-dummies as above. We also follow two other approaches to investigate school sorting: first, we analyse the poverty dynamics after the policy change of schools with pupils from relatively poor and relatively wealthy families. Second, we analyse the changing capacity of pupils from different types of neighbourhood to

access schools with more affluent peer groups. We use the timing of the policy change to identify effects throughout.

## **4. Data**

### ***NPD/PLASC***

The primary source of data we use in this study is administrative data from school censuses in England and Wales. For England's schools we use data from the National Pupil Database (NPD), developed and made available by the Department for Education (DfE). Between 1999 and 2001 the data consisted of pupil-linked Key Stage (KS) 3 and 4 results and school characteristics such as school type, percent of pupils eligible for Free School Meals (FSM) and number of pupils. From 2002 onwards the Pupil Level Annual School Census (PLASC) is incorporated into the NPD, giving a number of linked pupil characteristics, such as FSM, ethnicity and age. For Welsh schools we use similar data, maintained and provided to us by the Welsh Assembly Government. The Welsh PLASC was first collected in 2004, with similar school- and pupil-linked variables to its English counterpart. However, prior to 2004 – the key 'before' time period in our natural experiment design – Welsh KS3 and KS4 data was only available at school level, which means our analysis must be done using school-cohort cells. Using school- rather than pupil-level data means that we cannot analyse differential effects of the policy across the within-school distribution of pupil ability.

### ***School performance***

#### **(a) Data**

The dependent variables in our analysis of school performance are school-mean GCSE points and the proportion of pupils achieving 5 GCSEs at grades C or above. We include controls for school\*cohort average KS3 performance in regressions as a proxy for prior ability. This is a good approximation for school value-added (VA), a measure of relative school achievement calculated using pupil-level data, which we are unable to use because of the Welsh data limitations described above. Conventionally VA is calculated by comparing a pupil's progression between KS3 to KS4 with the average progression of pupils of the same ability, and aggregated to school level. We can test how good our approximation is by constructing conventional VA variables in England and also in Wales from 2006 onwards and comparing them with our estimates. The correlations between 'real' VA and our estimates are shown in

Table A1 and are typically around 0.9. The abolition of KS3 testing in Wales in 2006 only affects the last cohort in our study. For these pupils we use teacher assessment scores, to which we apply a data-based adjustment.

We include a number of variables from other sources. School expenditure data comes from the Section 52 School Outturn statements available from DfE<sup>9</sup> and the Welsh Department for Children, Education, Lifelong Learning and Skills (DCELLS)<sup>10</sup> and is deflated by the CPI and weighted by the Department for Communities and Local Government's area cost adjustment factor<sup>11</sup>. Population density data, which we use as a proxy for the competitiveness of the local area, comes from the 2001 UK Census.<sup>12</sup> We construct a variable to capture local competition, counting the number of other secondary schools within five kilometres of each school. The variables we use are summarised in Table 1.

## (b) Sample

In our analysis of school performance we construct two samples. In both samples we exclude all schools in local authorities with more than 10% of pupils in selective (known as Grammar or Secondary Modern) schools. The first sample is a balanced panel of English and Welsh schools in the years 1999/2000 to 2000/01 and 2003/04 to 2007/08. To balance the panel we drop around 7% of school-year observations as some schools closed or opened during the period and some had missing data. The differences between the balanced panel and the subsample that was dropped is shown in Table A2. The balanced panel may be a biased sample of all schools as a result of these differences, so we construct a second sample to mitigate the potential bias.

The second sample is a subsample of English schools from the balanced panel, matched with Welsh schools on the basis of a number of observed characteristics. Our matching variables are measures of school performance and their prior trends, percent FSM, percent white students, population density, school expenditure and our measure of local school competition, and are averaged over the 'before' period. Welsh secondary schools are poorer and less ethnically diverse than English schools and also have more Community schools. Welsh

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<sup>9</sup> See [www.dcsf.gov.uk/everychildmatters/strategy/financeandfunding/informationforlocalauthorities/section52/schoolbalances/s52sb/](http://www.dcsf.gov.uk/everychildmatters/strategy/financeandfunding/informationforlocalauthorities/section52/schoolbalances/s52sb/)

<sup>10</sup> See [www.statswales.wales.gov.uk/ReportFolders/ReportFolders.aspx?IF\\_ActivePath=P,324,332,5780](http://www.statswales.wales.gov.uk/ReportFolders/ReportFolders.aspx?IF_ActivePath=P,324,332,5780).

<sup>11</sup> See [www.local.odpm.gov.uk/finance/0708/acameth.pdf](http://www.local.odpm.gov.uk/finance/0708/acameth.pdf)

<sup>12</sup> See [www.statistics.gov.uk/STATBASE/ssdataset.asp?vlnk=7662](http://www.statistics.gov.uk/STATBASE/ssdataset.asp?vlnk=7662)

schools are generally less well funded and are in areas that are less densely populated. A full list of matching variables is shown in Table A3. We match English schools by propensity score to one nearest neighbour in the Welsh sample without replacement. In the matched sample the difference in the means of the matching variables in England and Wales are all individually and jointly insignificant.

## **Sorting**

### **(a) Data**

We use a different set of cohorts for our sorting analyses, as explained above and shown in Figure 2. Our focus will be on sorting across schools by socioeconomic background, for which we use FSM as a proxy, and by ability, which we proxy with Key Stage 2 scores. The pupil characteristics are aggregated at secondary school level, taking pupils' KS2 results from the last year of primary school and their FSM status in the first year of secondary school.

There are some approximations we need to make to get equivalent data across the period of interest in England and Wales. As mentioned above, pupil-level data in Wales was first collected in 2004, so to have cohort-level data on FSM from before the policy change we need to predict the 'before' data using later observations of the same pupils. We do the same in England to ensure the data are comparable. Key Stage 2 data in Wales are available for the whole period in our study but assessment by external examination was stopped in 2005. Teacher Assessment (TA) continued to be collected however, so after 2005 in both England and Wales we use TA score<sup>13</sup>.

In England and Wales PLASC records contain pupils' home postcodes, which enables us to characterise the neighbourhood in which they live. We have matched pupils' postcodes to the socio-economic (Mosaic) classification of that address. Mosaic classification is a postcode-level dataset that categorises each postcode in the UK into one of 61 different types on the basis of demographics, socio-economics and consumption, financial measures, and property characteristics and value<sup>14</sup>. Over 400 variables are used to construct these classifications and so this provides a rich picture of pupil's neighbourhoods at a very local level.

### **(b) Sample**

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<sup>13</sup> We are aware that there may be some incomparability of teacher assessment processes due to the presence or not of external validation of teacher scores in England and Wales respectively.

<sup>14</sup> For more information see <http://www.experian.co.uk/business-services/business-services.html>.

To analyse changes in Dissimilarity Indices we construct a balanced panel of LEAs in England and Wales matched on proportion FSM and population density. The matching regression is shown in Table A4.

## **5. Results**

We first set out our results for the impact of the abolition of league tables on school performance, and then consider a number of robustness checks. Second, we consider the impact on the sorting of pupils.

### ***Impact of league tables on school performance***

We start with two simple aggregate charts for our two dependent variables, the country-year averages of the school mean GCSE points (Figure 3), and the percentage of the school's pupils gaining at least 5 grade Cs or above (Figure 4). In both figures, the vertical line indicates the timing of the policy change, and the two unconnected points are the in-between years, neither wholly before nor wholly after the policy change. Figure 4 provides the much longer history available on the 5 A\*-C measure (these earlier points cannot be included in our regression analysis for data reasons, discussed above).

Both figures indicate a clear pattern: after the reform, there was faster attainment growth in England than Wales. In both figures this gap increases continuously from the reform. In the longer 'before' period available in Figure 4, it is clear that the gap after the reform was many times larger than it had been at any time since at least the mid 1990s. A simple difference-in-difference confirms this result; this is presented in Table 2 for both variables and for both the full sample and the matched sample. The effect sizes are substantial; in the matched sample the estimate is 2.204 GCSE points, equivalent to just over a quarter (0.27) of a school-level standard deviation (SD). We deal with statistical significance below.

However, these are unconditional outcomes and we now move on to the full difference-in-difference model in Table 3. These models include school fixed effects, time-varying school level averages of their pupils' prior attainment (KS3), time-varying measures of school composition, school size, school expenditure, measures of competition, and year effects. The regressions are all weighted by school size, and the standard errors are clustered at Local Education Authority (LEA) level.

Table 3 confirms the results above. Our leading cases are the matched sample, including all our control variables, columns 4 and 8 in the table; this is also conservative, since the other estimates are all larger. The effect sizes are in fact very similar to the simple difference-in-difference and are strongly statistically significant. We are controlling here for time-invariant factors influencing school performance, and also time-varying school composition (gender, ability, and poverty status) and time-varying school expenditure. Use of the matched sample also captures school heterogeneity. The results suggest a negative annual impact of 1.92 GCSE points (grades) per student in Wales after the reform. This is equivalent to 0.23 of a school-level SD and 0.086 of a pupil-level SD. As might be expected in a less homogenous sample, the effects are slightly larger using the full sample than the matched sample. The inclusion of school fixed effects mops up most of the heterogeneity and the control variables are generally insignificant, particularly so in the matched sample.

It is worth noting that the effect sizes are about the same for both dependent variables. If schools' gaming of the performance measure in the league table (the school %5A\*-C) were a major factor, we would expect a considerably greater impact on the measured variable than the un-measured variable. The PISA evidence described below reinforces this argument.

In Table 4 we allow the treatment effect to vary across different years after the reform. The impact is highest in 2006 and is lower by the end of our sample in 2008, the temporal variation being significant for each specification shown. For the school mean GCSE score in the matched sample and including school controls, the effect peaks at -3.88 GCSE points (grades), and remains at -2.03 points in 2008. This apparent decline of the impact effect in fact simply derives from the fact that we are estimating a value-added model, modelling progress from age 14 exams to age 16 exams. For the early post-reform cohorts, their age 14 exams would have measured their learning under the old system, and the 14 to 16 progress captures the effect of the reform. But for the later post-reform cohorts, both their age 16 and their age 14 exams are under the new system, and so their progress may be much less affected. Additional regressions (available from the authors) support this interpretation: if we re-run the analysis in Table 4 without prior attainment we see no decline in the impact (for example, for column 4, the treatment effects are -1.789 (in 2005), -2.516 (2006), -2.054 (2007) and -2.399 (2008)). Secondly, we find strong and significant treatment effects on the age 14 test scores themselves.

In Tables 5 and 6 we explore different dimensions of effect heterogeneity by school characteristics. Table 5 shows significant heterogeneity by quartile of the school's ability profile (measured by KS3). The impact of the reform is most negative for the schools in the lowest quartile of student ability and insignificant for schools in the highest quartile. Again, the LR test shows these effects to be significant in each specification. Table 6 shows a very similar story by quartile of the poverty rate of the school. The reform has the greatest negative impact in the poorest quartile of schools, and has no statistically significant effect for the least poor schools. The pattern is repeated again by quartile of schools' position in the league tables (not reported). Taken together, these results show that the 'best' Welsh schools – measured by highest prior attainment, lowest poverty rate, and sitting at the top of the league tables – perform in a similar way to their matched English counterparts, and appear to be unaffected by the reform. The lower 75% of schools are affected negatively however, with the poorest and lowest ability schools falling behind the most.

Finally, we consider whether the treatment effect varies by our measure of local competition. Table 7 shows that the impact effect is robust across rural and urban areas (though very urban areas in England are not in the matched sample). We do not see any systematic differential effect by degree of competition.

### ***Robustness checks***

In the results above, we have ignored the time series properties of the data. Bertrand, Duflo and Mullainathan (2004) show that serial correlation can be a problem for difference-in-difference estimation, leading to downward bias in standard errors and to over-rejection in hypothesis tests. In a panel setting they suggest that collapsing the time dimension to 'before' and 'after' periods, that is, reducing the time series information performs well as a correction. We report the results of doing this in Table 8; the results remain significant after this transformation, suggesting that serial correlation is not a major concern.

One of the key assumptions in a difference-in-difference setting is that the composition of the two groups does not change. We consider two possible reasons why this assumption might not hold in this case. For some families, private schools are a margin of school choice that we need to consider. Some families in Wales might re-consider private schools once information on state schools was restricted. This would change the composition of some schools at the margin and might bias our estimates. In fact, data from the Independent Schools Council on



pupils attending private secondary schools does not show a sharp rise in private school attendance in either country around the time of the policy reform (see Table A5).

A second possibility is significant movements of families across the border between England and Wales in response to the policy reform. The data in Table A5 indicate that the numbers are small and in fact show no large change in pupils living in England attending Welsh schools or pupils living in Wales attending English schools.

The potential of schools to game the 5 A\*-C measure is a concern; it could be that the effect we estimate simply reflects schools in Wales no longer gaming. However, our estimate of an equivalent impact on the unpublished school-average measure is reassuring. Evidence from the PISA study reinforces that view. PISA conducts international tests of its own in maths, reading and science, and since these are completely irrelevant to schools ranking the scores are free from gaming and reflect the true state of children's aptitude. We display the national averages in Figure 5 across the three subjects, and they mirror the results we have here very closely<sup>15</sup>.

We consider a further experiment. Primary schools in England and Wales experienced no such differential reform over the same time period. In England, league tables of KS2 scores have been published continuously since 1996, but have never been published in Wales. All that changes over the policy date is devolution and a desire to foster a new ethos of collaboration between schools, so this allows us to separate this potential overall effect from the removal of the league tables. In order to “difference” primary and secondary school data, we collapse primary and secondary data to LEA-year cells and standardise the dependent variables<sup>16</sup>. We estimate a triple-difference  $(\text{after} - \text{before}) * (\text{Wales} - \text{England}) * (\text{primary} - \text{secondary})$  over LEA-year cells and report the results in Table A6. We find a large, statistically significant treatment effect of around half a school-level SD; this essentially comes from the substantial negative effect at secondary level discussed above, plus a positive effect at primary level.<sup>17</sup>

Finally we consider the possibility that other differential and coincident policy changes might have been responsible for our results. We have found two potential candidates. First, the

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<sup>15</sup> There was no independent data for Wales in 2000.

<sup>16</sup> The data do not allow a perfect equivalent model as we do not have KS1 scores to use as prior attainment control. So the primary school component has no prior attainment.

<sup>17</sup> We do not want to speculate too strongly on the cause of the positive effect we see at primary level, because of the potential incomparability of teacher assessments across England and Wales (see the previous footnote).

‘Literacy Hour’ was introduced in primary schools in England in 1998, and Machin and McNally (2008) find a positive effect of the policy on reading scores at the end of primary school. The timing means that our first ‘after’ cohort were the first to be exposed to the policy in England but not Wales; this therefore offers an alternative potential explanation of the relative acceleration in secondary schools in England. However, there are a number of reasons to be sceptical about the capacity of this to explain our results. First, we are controlling for prior attainment (KS3 tests at age 14) and it is hard to see an age 5–11 policy exerting a strong influence on progress at age 14–16, controlling for attainment at age 14. Second, the effect was only found in urban areas, while large parts of our matched sample are very rural areas. We think it unlikely therefore that our results are a literacy hour effect in disguise.

The second coincident policy change was the staggered introduction of replacement qualifications in England and Wales. In addition to the regular GCSE qualifications, schools in both England and Wales have used GCSE-equivalent qualifications, typically more vocational qualifications and more frequently used for less academically able children. They count towards the GCSE points total and the fraction of pupils gaining at least five ‘C’ grades or better, and are in our data as such. The new equivalent qualifications were introduced in England in 2005 and in Wales in 2007 as part of the restructuring of the curriculum between the ages of 14 and 19.<sup>18</sup> Because these are replacement equivalent qualifications and not additional qualifications, we would not expect any substantial impact on overall exam points scores. Nevertheless, minor differences in the GCSE points attached to the new qualifications relative to the previous ones might produce a temporary change in relative school scores over 2005 – 2007. It could be argued that this contributes to the time series pattern in Table 4. However, there are two reasons for believing this to have a negligible impact. First, these equivalent qualifications only account for a small proportion of the total GCSE points, and even then really only in the lowest 20% of students in the KS3 distribution<sup>19</sup>. For example, in England in 2006 they account for 9.5% of total points in the lowest decile, and only 4.3% at the median. So any differential points equivalence between new GCSE-equivalents (England) and old (Wales) only applies to a small fraction of total points, while we know from Table 5 that the treatment effect is statistically indistinguishable in the bottom quartile and the third

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<sup>18</sup> For more information see <http://www.qcda.gov.uk/phases-of-education-and-training/92.aspx> (accessed 29.9.10).

<sup>19</sup> We do not have access to the data disaggregated by qualification type in Wales, otherwise we could address this directly.

quartile. Second, given the differential take-up of the equivalent qualifications by the KS3 distribution, if their staggered introduction was to be an alternative explanation for our results, we would expect to see differential time trends over the KS3 distribution. However, as we show in Appendix Table A7, the treatment\*KS3 quartile and treatment\*year interactions explain almost all of the overall variation, and the treatment\*KS3 quartile\*year interactions are jointly insignificant. So while the staggered introduction of replacement vocational qualifications may make some contribution to the time pattern of the effect, it seems clear that this cannot be an alternative explanation for our results.

### ***Impact of league tables on school sorting***

The primary hypothesis is that the removal of league tables decreased sorting by removing the information basis for segregationist choices and/or decreased schools' incentive for cream skimming. It is also possible though that removal of league tables removed performance information only from less well networked families, allowing a greater concentration of better off families in the high-scoring schools. In fact, in the range of analyses we report below, the overall result is of little impact of the policy on sorting. We are confident that sorting has not decreased in Wales relative to England.

We start by plotting country-averages of simple Dissimilarity Indices for poverty status, top and bottom ability quartiles (see Duncan and Duncan, 1955; Massey & Denton 1988) at LEA level over the policy change. Figures 6 – 8 show that the Dissimilarity Indices are noisy, but there appears to be no consistent picture of change in sorting in Wales relative to England, with the possible exception of the top quartile<sup>20</sup>. In a regression framework, the difference-in-difference estimates in Table 9 Panels A–C confirm this, finding no statistical effect of the league table policy change.

However, there are different dimensions to sorting and we consider two other ways of quantifying sorting. First, we match all Welsh schools with English schools on FSM percentage in the 'before' period. We then split these into quartiles and trace out the evolution of the percentage FSM for each quartile separately for England and Wales (normalised by the national aggregate time series). The idea is to see whether we see any

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<sup>20</sup> Confidence intervals for D indices are not straightforward to calculate (see Allen, Burgess & Windmeijer 2009) so we rely on the difference-in-difference for statistical significance.

reduction in polarisation: whether poor schools become less poor in Wales and more affluent schools more poor. Figure 9 shows that this is not the case, and the regressions in Table 10 confirm very little significant impact of the policy change on FSM concentration.

Finally, we look at the likelihood of pupils from different neighbourhoods being able to access high-scoring schools. We assign pupils to Mosaic neighbourhood types based on their home postcode and calculate the mean poverty rate of the schools attended by pupils of each Mosaic type. We regress this mean school poverty rate on Mosaic code dummies before and after the policy change, in both England and Wales. We then examine whether the average proportion FSM of the schools pupils from the same Mosaic types go to changes after the policy. The figures are graphed in Figure 10; each observation represents a particular Mosaic neighbourhood type, the horizontal axis the pre-reform period and the vertical axis the post-reform period. The value plotted is the mean poverty rate of schools attended by students living in that Mosaic type in that period. If the data lie along a line steeper than  $45^\circ$ , this indicates polarisation, and we are obviously interested in a differential slope between England and Wales. The figure hints at a slightly steeper slope in Wales, and this is confirmed in the regressions in Table 11. The OLS coefficient suggests increasing stratification in Wales but not in England, the positive coefficient indicating poor schools getting poorer and rich schools getting richer. We also ran a median regression to reduce the effect of outliers with so few observations, and this yields the same answer.

## **6. Conclusion**

The publication of school performance tables based on a national testing programme is a common aspect of school accountability systems. It is a core element of the long-standing accountability system in England and of the NCLB Act in the US. In this paper we exploit a natural experiment which involved this component of an accountability system being dropped. The policy change arose when the devolved government of Wales ended the publication of the performance tables, thereby removing this source of comparative public information, whilst they carried on as before in England. Our research design allows us to circumvent the two main data problems that previous studies of accountability have had to cope with: lack of a credible control group, and the introduction of a whole performance management system at once (Figlio and Ladd, 2008). Using a difference-in-difference regression we compare changes in England and Wales before and after the policy change in

2001 in order to isolate the impact of the policy change on two outcomes: school effectiveness (value added) and pupil sorting across schools.

We find that the reform significantly and systematically reduces school effectiveness. The effect is substantial, amounting to 0.23 of a school-level standard deviation or 0.09 of a pupil-level SD. This is as effective (in a negative sense) as a class size increase of 8 students, out of a typical class size of 30 (Angrist and Lavy, 1999). The publication of performance tables given the pre-existence of the test score data is therefore extremely cost-effective; it is arguably still very cost-effective even including the cost of the national test system. We find significant heterogeneity in the effect of the policy reform: the performance of schools in the top quartile of schools, by intake ability, poverty status or league table position, is not affected. Our results show that the policy reform in Wales reduced average performance and raised educational inequality.

We also investigate whether the removal of public performance information changed the sorting of students to schools, testing the leading hypothesis that the league tables facilitate segregation. In fact, we find no evidence that the policy change has had a significant impact on either sorting by ability or by socioeconomic status.

Of course, teachers and schools may have broader educational aims for their pupils than GCSE exam results. While these are not measured in our data, the PISA results cited above tend to support the idea that attainment on much broader measures has fallen in Wales relative to England.

We can rule out a number of potential causes of our findings. They cannot be explained by different resource levels or funding regimes as we control for that in our analysis. Our estimation on a matched sample, the inclusion of school fixed effects and time-varying school composition and resource variables all give us reason to be confident that we have dealt with a lot of school heterogeneity. Nor does it seem to be the case that gaming of the league tables simply ceased in Wales and continued in England.

Finally we speculate on the most likely accountability channels through which this reduction of public information is having an impact. The effect may partly be driven through the reduction of centrally collated and published information for parental choice. The fact that the effect is absent at the top end of the school performance adds to this view. Schools at the top of the league tables are known to be so locally and are generally oversubscribed, and we

would not expect the removal of league table information to affect the incentives facing such schools in the short to medium term. Contrary to this view, however, we find that the impact of the reform does not vary significantly by the level of local competition, which suggests that it is not only choice-based accountability that is driving our results. Moreover, there is generally a relatively low level of potential choice in Wales, illustrated by the matched English schools being located in largely rural areas. It appears unlikely, therefore, that market-based accountability via parental choice is the main driver behind our results.

If we now consider the concurrent administrative accountability system as a potential channel for change, high-stakes testing at the end of compulsory schooling continued unchanged and Local Authorities and national governments of both countries continued to collect and monitor performance information on individual schools. This suggests that diminished government scrutiny of performance data is unlikely to be the major pathway for change. There is, however, less scope for ‘naming and shaming’ when results are no longer regularly reported by the media, which may mean that this accountability mechanism is perceived as less ‘high stakes’ in Wales compared to the very public school league tables in England. Such high stakes, public accountability systems have been shown to have an impact on provider behaviour in the UK health care context (Propper et al 2008; 2010) and it may be that such systems elicit a similar response across schools, resulting in higher average test scores.

Our results suggest that school accountability policies hold promise for raising school performance, particularly for students in disadvantaged schools and neighbourhoods. If uniform national test results exist, publishing these in a locally comparative format appears to be an extremely cost-effective policy for raising attainment and reducing inequalities in attainment.

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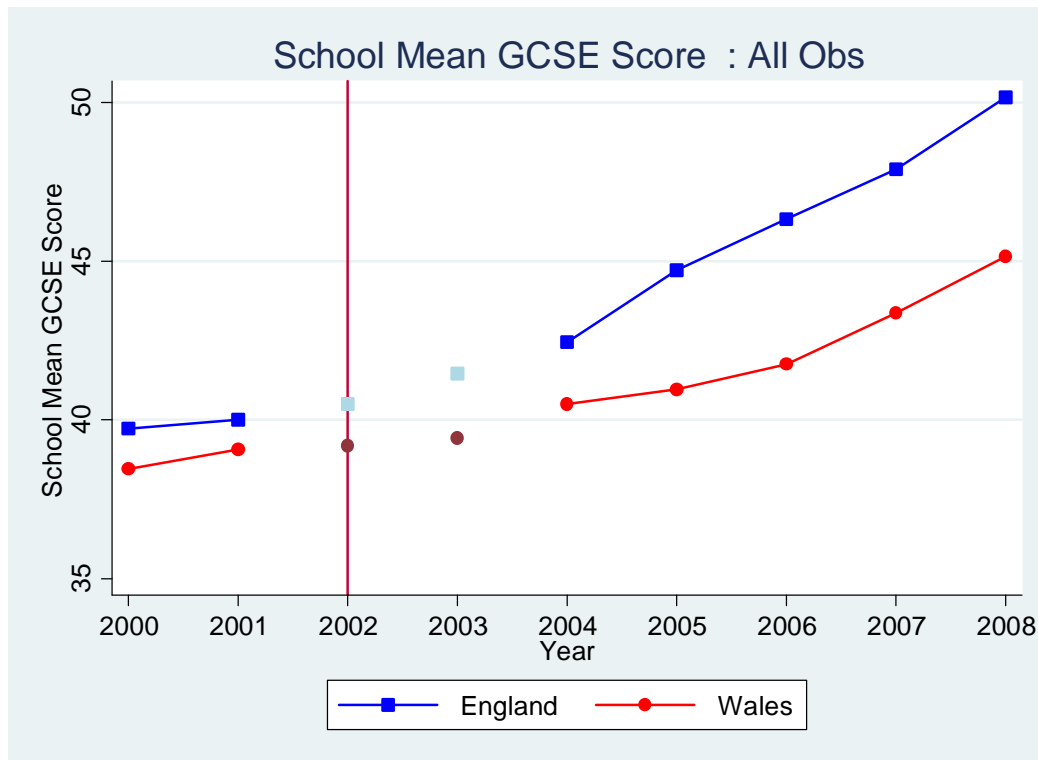
**Figure 1: Performance Timeline**

Cohort		1997/98	1998/99	1999/00	2000/01	2001/02	2002/03	2003/04	2004/05	2005/06	2006/07	2007/08
Before	A	KS3		KS4	LT Published							
	B		KS3		KS4	Expect LT Published						
	C			KS3		KS4	No LT (in Wales)					
	D	KS2			KS3		KS4	No LT (in Wales)				
After	E		KS2			KS3		KS4	No LT (in Wales)			
	F			KS2			KS3		KS4	No LT (in Wales)		
	G				KS2			KS3		KS4	No LT (in Wales)	
	H	KS1				KS2			KS3		KS4	No LT (in Wales)
	I		KS1				KS2			KS3		KS4

**Figure 2: Sorting Timeline**

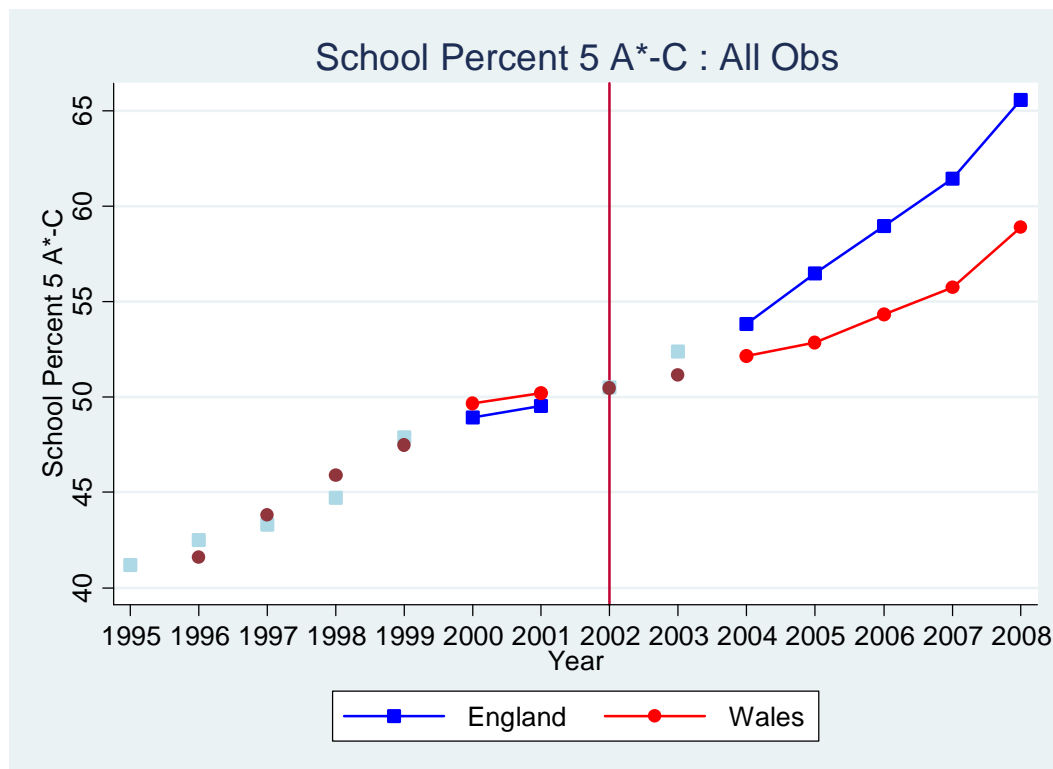
Cohort		1998/99	1999/00	2000/01	2001/02	2002/03	2003/04	2004/05	2005/06	2006/07
Before	E	KS2 Apply			KS3		KS4			
	F		KS2 Apply			KS3		KS4		
	G			KS2 Apply			KS3		KS4	
	H				KS2 Apply			KS3		KS4
After	I	KS1				KS2 Apply			KS3	
	J		KS1				KS2 Apply			KS3
	K			KS1				KS2 Apply		
	L				KS1				KS2 Apply	
	M					KS1				KS2 Apply

**Figure 3: School Mean GCSE Points score in England and Wales over time**

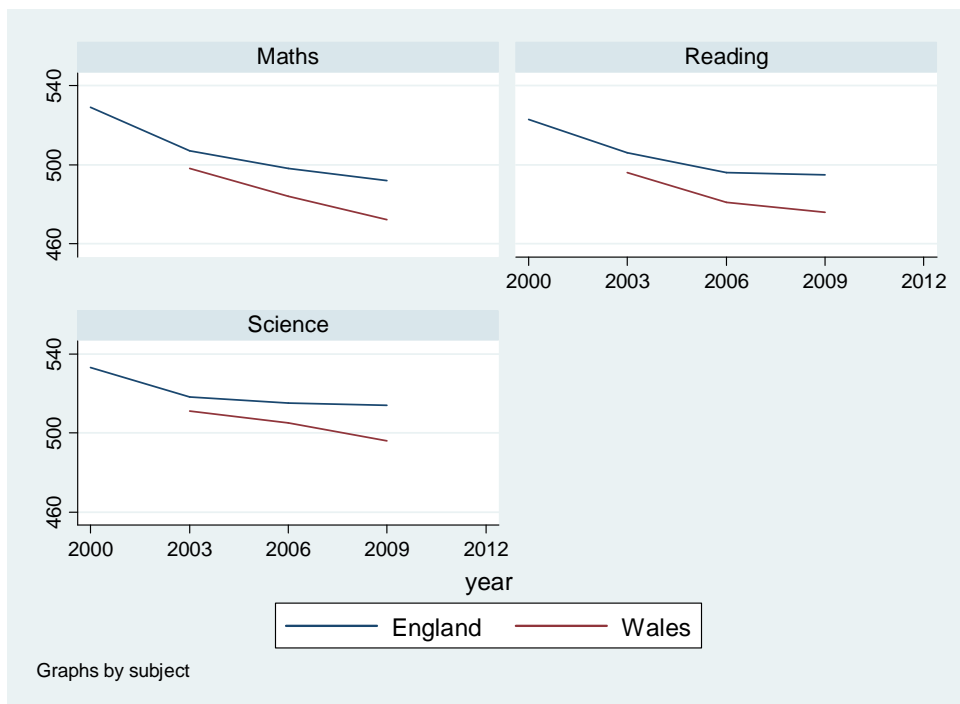


N.B. Connected points indicate the before and after periods in our analysis

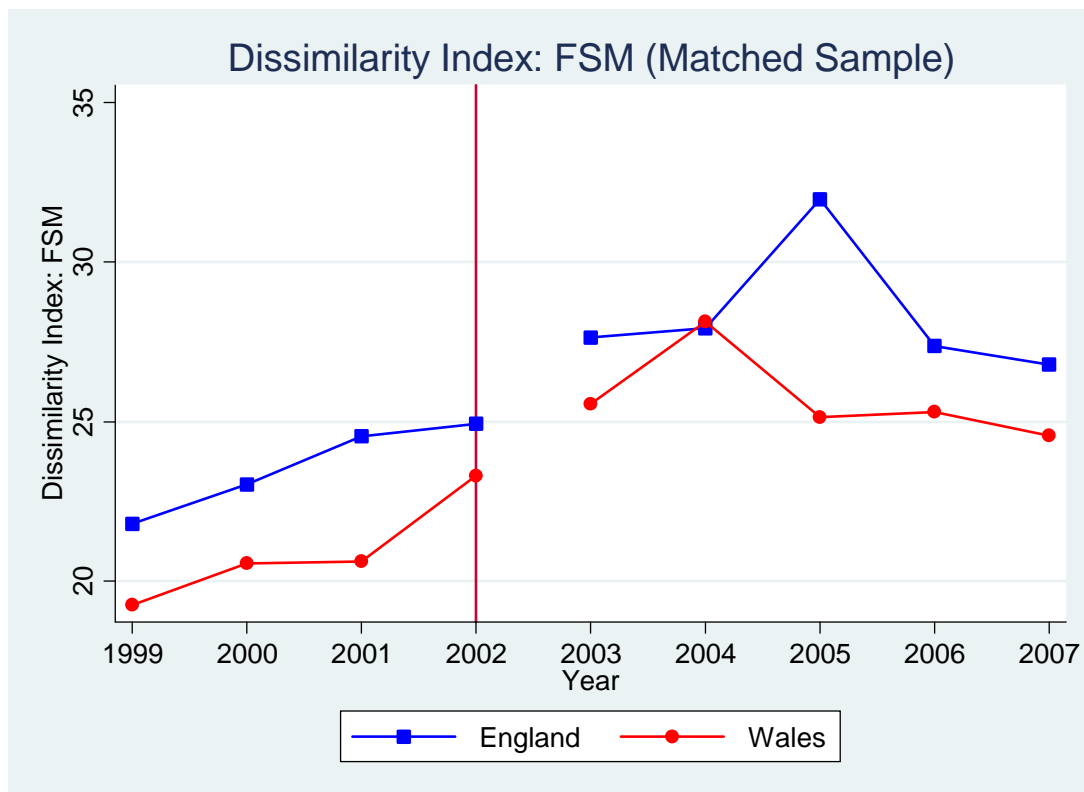
**Figure 4: School Percent 5 A\*-C in England and Wales over time**



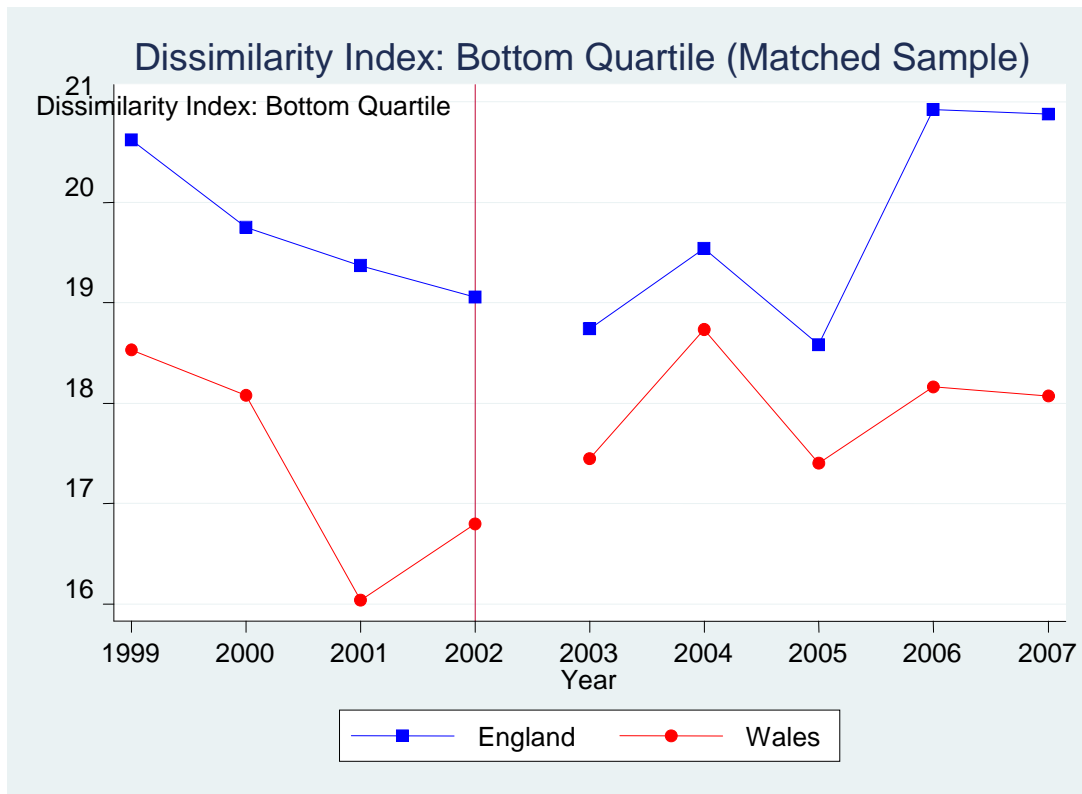
**Figure 5: PISA test scores for England and Wales over time**



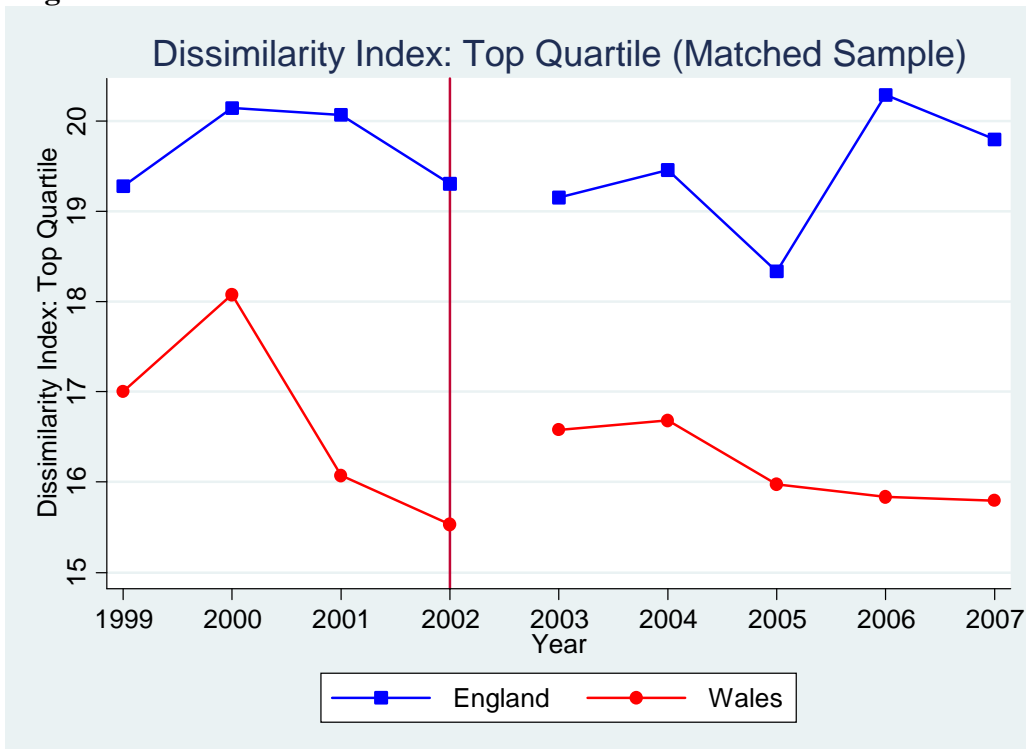
**Figure 6: LEA-level FSM Dissimilarity Index in England and Wales over time**



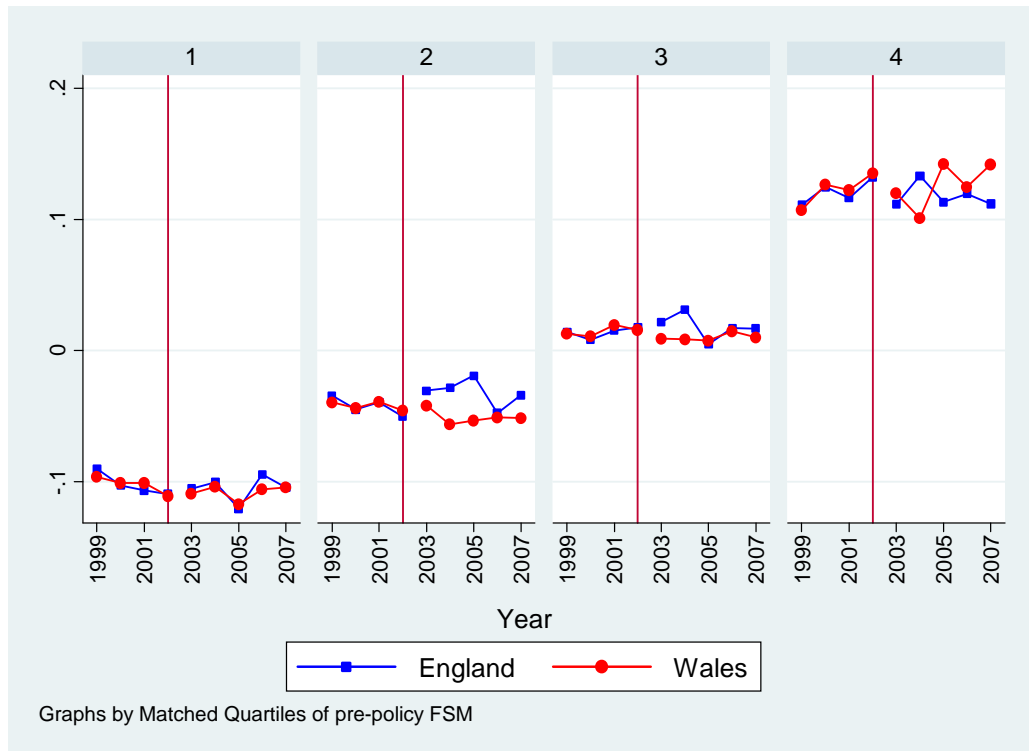
**Figure 7: LEA-level Dissimilarity Index of Lowest Quartile Key Stage 2 mean score in England and Wales over time**



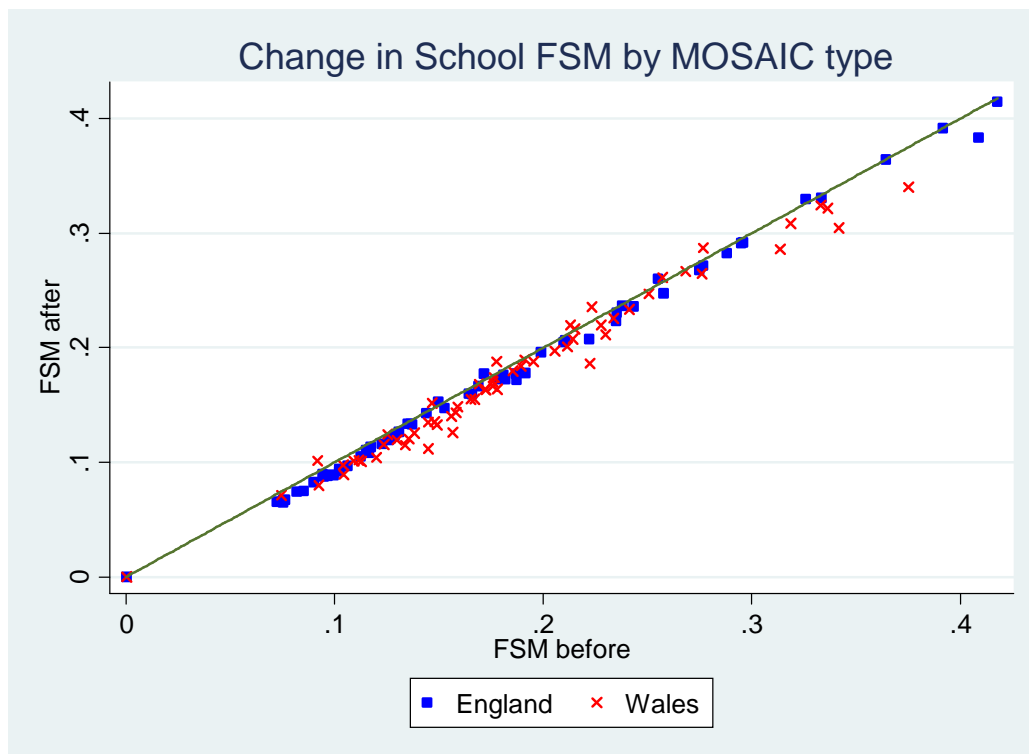
**Figure 8: LEA-level Dissimilarity Index of Highest Quartile Key Stage 2 mean score in England and Wales over time**



**Figure 9: Growth in FSM within matched quartiles of initial FSM, across England and Wales**



**Figure 10: Average %FSM of school attended, before and after in England and Wales, by Mosaic type**



**Table 1: Summary Statistics**

	All Observations		Matched Sample	
	England	Wales	England	Wales
Number of Schools	2376	207	193	193
Number of Observations (school*year)	16632	1449	1351	1351
Mean size of cohort	206.6 67.2	186.5 58.3	199.6 67.6	188.1 58.5
Mean size of school	1.143 0.346	1.077 0.367	1.077 0.360	1.084 0.369
Mean proportion female	0.497 0.146	0.492 0.095	0.493 0.078	0.492 0.098
Mean proportion white	0.825 0.236	0.951 0.095	0.945 0.084	0.950 0.097
Mean proportion FSM	0.146 0.215	0.155 0.085	0.154 0.200	0.159 0.085
Mean GCSE points	44.6 8.7	41.4 8.3	42.9 7.8	41.2 8.3
Mean Key Stage 3 points	34.1 2.7	33.8 2.0	33.6 2.2	33.8 2.0
Mean proportion achieving 5 A*-C at GCSE	56.6 16.4	53.5 14.3	52.5 14.5	53.3 14.3
School Outturn Expenditure per pupil (in £1000s)	4.111 0.951	3.702 0.640	4.148 0.940	3.717 0.645
Mean change in GCSE points	0.327 2.478	0.778 3.354	0.617 2.481	0.791 3.394
Mean change in Key Stage 3 points	-0.18 0.96	-0.19 0.82	-0.14 0.97	-0.18 0.81
Mean change in School Expenditure per pupil	0.304 0.378	0.196 0.122	0.174 0.422	0.200 0.121
Local Authority Population Density	1.65 2.14	0.54 0.60	0.59 0.68	0.55 0.62
Voluntary Aided	0.146 0.353	0.060 0.237	0.075 0.263	0.059 0.235
Voluntary Controlled	0.034 0.181	0.012 0.110	0.004 0.063	0.013 0.113

**Table 2: Simple Difference-in-differences**

		England			Wales			<b>D-in-D (Wales-England)</b>
		Before	After	<b>Difference (Before-After)</b>	Before	After	<b>Difference (Before-After)</b>	
All Observations	Five A*-C	49.249	59.286	<b>10.037</b>	49.946	54.794	<b>4.848</b>	<b>-5.189</b>
	Mean GCSE Points	39.872	46.332	<b>6.460</b>	38.770	42.347	<b>3.578</b>	<b>-2.882</b>
Matched Sample	Five A*-C	46.388	54.650	<b>8.262</b>	49.824	54.589	<b>4.765</b>	<b>-3.497</b>
	Mean GCSE Points	38.680	44.464	<b>5.784</b>	38.638	42.218	<b>3.581</b>	<b>-2.204</b>



**Table 3: Full Difference-in-Difference Model**

	School Mean GCSE Score				School Percent 5 A*-C			
	All Obs (1)	All Obs (2)	Matched .....(3)	Matched (4)	All Obs (5)	All Obs (6)	Matched (7)	Matched (8)
Treatment Effect	-2.741*** (0.404)	-2.328*** (0.388)	-2.329*** (0.656)	-1.923*** (0.546)	-4.968*** (0.586)	-4.577*** (0.567)	-3.663*** (1.069)	-3.432*** (0.950)
Proportion FSM		-0.138 (0.231)		-0.380 (0.809)		-0.594 (0.355)		-1.500 (1.340)
Proportion Female		8.490** (2.717)		12.13* (5.106)		15.31** (4.844)		20.81* (8.205)
Number of Schools within 5km		-0.150 (0.117)		0.00155 (0.369)		-0.222 (0.227)		0.810 (0.600)
Total Pupils in School		0.979 (1.091)		-2.235 (2.750)		-0.682 (1.691)		-5.509 (3.406)
Outturn Expenditure per Pupil		1.323*** (0.349)		-1.826 (1.366)		1.489* (0.692)		-1.703 (2.378)
Outturn Expenditure squared		-0.0591 (0.0316)		0.323* (0.154)		-0.0709 (0.0650)		0.340 (0.272)
Observations	18079	18040	2702	2702	18079	18040	2702	2702
R <sup>2</sup>	0.564	0.567	0.484	0.498	0.558	0.560	0.442	0.458
Number of Schools	2583	2583	386	386	2583	2583	386	386

Standard errors in parentheses.

\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .

School Fixed Effects, Controls for Prior Attainment and Year Effects are included. Standard Errors are clustered at LEA level. School control variables are %FSM, %Girls, School Size and Expenditure per Pupil and a measure of local competition.

**Table 4: Full D-in-D allowing for Heterogeneity through time**

	School Mean GCSE Score				School Percent 5 A*-C			
	All Obs (1)	All Obs (2)	Matched (3)	Matched (4)	All Obs (5)	All Obs (6)	Matched (7)	Matched (8)
Treatment Effect: Year 2004	-0.349 (0.307)	-0.0771 (0.298)	0.0738 (0.416)	0.114 (0.394)	-1.509*** (0.432)	-1.255** (0.412)	-0.391 (0.706)	-0.573 (0.671)
Treatment Effect: Year 2005	-2.625*** (0.372)	-2.266*** (0.367)	-2.296*** (0.568)	-2.122*** (0.527)	-4.305*** (0.688)	-3.965*** (0.672)	-3.045** (1.047)	-3.128** (1.020)
Treatment Effect: Year 2006	-4.102*** (0.453)	-3.776*** (0.442)	-4.026*** (0.772)	-3.881*** (0.714)	-6.778*** (0.739)	-6.525*** (0.722)	-6.085*** (1.389)	-6.256*** (1.336)
Treatment Effect: Year 2007	-3.434*** (0.544)	-3.034*** (0.527)	-2.922** (0.891)	-2.599** (0.789)	-6.367*** (0.712)	-6.026*** (0.688)	-5.059*** (1.377)	-4.970*** (1.237)
Treatment Effect: Year 2008	-3.181*** (0.595)	-2.619*** (0.587)	-2.569** (0.953)	-2.032* (0.888)	-5.866*** (0.849)	-5.370*** (0.849)	-3.874* (1.479)	-3.675* (1.460)
School Control Variables	No	Yes	No	Yes	No	Yes	No	Yes
Observations	18079	18040	2702	2702	18079	18040	2702	2702
R <sup>2</sup>	0.567	0.570	0.500	0.512	0.561	0.563	0.455	0.470
Number of Schools	2583	2583	386	386	2583	2583	386	386
LR P-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Standard errors in parentheses

\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ 

School Fixed Effects, Controls for Prior Attainment and Year Effects are included. Standard Errors are clustered at LEA level. School control variables are %FSM, %Girls, School Size and Expenditure per Pupil and a measure of local competition.

**Table 5: Full D-in-D allowing for Heterogeneity in Prior Attainment**

	School Mean GCSE Score				School Percent 5 A*-C			
	All Obs (1)	All Obs (2)	Matched (3)	Matched (4)	All Obs (5)	All Obs (6)	Matched (7)	Matched (8)
Treatment Effect: KS3 Q1 (Lowest KS3 quartile)	-3.795*** (0.763)	-3.263*** (0.759)	-3.750** (1.176)	-3.241** (0.974)	-8.532*** (1.133)	-8.083*** (1.132)	-7.449*** (1.801)	-7.301*** (1.566)
Treatment Effect: KS3 Q2	-2.895*** (0.580)	-2.513*** (0.559)	-2.312** (0.756)	-1.886** (0.631)	-5.224*** (1.021)	-4.904*** (1.002)	-4.322*** (1.237)	-4.152*** (1.093)
Treatment Effect: KS3 Q3	-3.145*** (0.513)	-2.745*** (0.518)	-2.451** (0.806)	-2.211** (0.739)	-5.135*** (0.911)	-4.793*** (0.928)	-2.788* (1.381)	-2.795* (1.358)
Treatment Effect: KS3 Q4 (Highest KS3 quartile)	-0.835 (0.609)	-0.515 (0.598)	-0.959 (0.773)	-0.650 (0.781)	-0.727 (0.767)	-0.330 (0.754)	-0.604 (1.161)	-0.472 (1.176)
School Control Variables	No	Yes	No	Yes	No	Yes	No	Yes
Observations	18079	18040	2702	2702	18079	18040	2702	2702
R <sup>2</sup>	0.565	0.567	0.487	0.500	0.559	0.561	0.449	0.465
Number of Schools	2583	2583	386	386	2583	2583	386	386
LR P-value	0.000	0.000	0.001	0.002	0.000	0.000	0.000	0.000

Standard errors in parentheses

\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ 

School Fixed Effects, Controls for Prior Attainment and Year Effects are included. Standard Errors are clustered at LEA level. School control variables are %FSM, %Girls, School Size and Expenditure per Pupil and a measure of local competition.

**Table 6: Full D-in-D allowing for Heterogeneity in Poverty Status**

	School Mean GCSE Score				School Percent 5 A*-C			
	All Obs (1)	All Obs (2)	Matched (3)	Matched (4)	All Obs (5)	All Obs (6)	Matched (7)	Matched (8)
Treatment Effect: FSM Q1 (Lowest FSM quartile)	-0.735 (0.546)	-0.431 (0.548)	-1.033 (0.807)	-0.705 (0.799)	-0.587 (0.986)	-0.232 (0.963)	-1.568 (1.380)	-1.406 (1.372)
Treatment Effect: FSM Q2	-2.079 <sup>***</sup> (0.443)	-1.668 <sup>***</sup> (0.427)	-2.571 <sup>**</sup> (0.774)	-2.222 <sup>**</sup> (0.732)	-3.871 <sup>***</sup> (0.831)	-3.438 <sup>***</sup> (0.824)	-3.739 <sup>**</sup> (1.128)	-3.504 <sup>**</sup> (1.134)
Treatment Effect: FSM Q3	-3.239 <sup>***</sup> (0.555)	-2.824 <sup>***</sup> (0.544)	-2.395 <sup>**</sup> (0.854)	-1.933 <sup>*</sup> (0.742)	-5.122 <sup>***</sup> (0.994)	-4.728 <sup>***</sup> (0.976)	-3.315 <sup>*</sup> (1.469)	-2.877 <sup>*</sup> (1.321)
Treatment Effect: FSM Q4 (Highest FSM quartile)	-3.633 <sup>***</sup> (0.611)	-3.201 <sup>***</sup> (0.594)	-3.344 <sup>**</sup> (1.039)	-3.070 <sup>***</sup> (0.842)	-7.969 <sup>***</sup> (0.921)	-7.681 <sup>***</sup> (0.842)	-6.182 <sup>***</sup> (1.647)	-6.582 <sup>***</sup> (1.361)
School Control Variables	No	Yes	No	Yes	No	Yes	No	Yes
Observations	18079	18040	2702	2702	18079	18040	2702	2702
R <sup>2</sup>	0.565	0.567	0.487	0.500	0.559	0.561	0.445	0.463
Number of Schools	2583	2583	386	386	2583	2583	386	386
LR P-value	0.000	0.000	0.005	0.004	0.000	0.000	0.001	0.000

Standard errors in parentheses

\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ 

School Fixed Effects, Controls for Prior Attainment and Year Effects are included. Standard Errors are clustered at LEA level. School control variables are %FSM, %Girls, School Size and Expenditure per Pupil and a measure of local competition.

**Table 7: Full D-in-D allowing for Heterogeneity in Local Competition**

	School Mean GCSE Score				School Percent 5 A*-C			
	All Obs (1)	All Obs (2)	Matched (3)	Matched (4)	All Obs (5)	All Obs (6)	Matched (7)	Matched (8)
Treatment Effect: Local Competition Q1 (Lowest Competition quartile)	-2.518*** (0.600)	-2.145*** (0.590)	-2.244** (0.792)	-1.945* (0.744)	-4.906*** (0.704)	-4.553*** (0.676)	-3.288** (1.120)	-3.045** (1.055)
Treatment Effect: Local Competition Q2	-3.120*** (0.503)	-2.645*** (0.489)	-1.915 (1.025)	-1.469 (0.931)	-5.242*** (0.909)	-4.793*** (0.918)	-4.070** (1.422)	-3.718** (1.284)
Treatment Effect: Local Competition Q3	-3.769*** (0.376)	-3.375*** (0.353)	-2.559** (0.786)	-2.085** (0.708)	-6.496*** (0.845)	-6.127*** (0.813)	-3.853** (1.366)	-3.653* (1.383)
Treatment Effect: Local Competition Q4 (Highest Competition quartile)	0.463* (0.203)	0.872*** (0.205)	-2.680** (0.889)	-2.299** (0.847)	1.167** (0.352)	1.549*** (0.350)	-3.393* (1.428)	-3.371** (1.245)
School Control Variables	No	Yes	No	Yes	No	Yes	No	Yes
Observations	18079	18040	2702	2702	18079	18040	2702	2702
R <sup>2</sup>	0.565	0.567	0.485	0.498	0.559	0.561	0.442	0.458
Number of Schools	2583	2583	386	386	2583	2583	386	386
LR P-value	0.002	0.003	0.554	0.532	0.002	0.002	0.808	0.867

Standard errors in parentheses

\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ 

School Fixed Effects, Controls for Prior Attainment and Year Effects are included. Standard Errors are clustered at LEA level. School control variables are %FSM, %Girls, School Size and Expenditure per Pupil and a measure of local competition.

**Table 8: Collapse to Pre- and Post-reform periods**

	School Mean GCSE Score		School Percent 5 A*-C	
	All Obs	Matched	All Obs	Matched
Treatment Effect	-1.823*** (0.364)	-1.227* (0.576)	-3.794*** (0.571)	-2.401* (1.162)
Proportion FSM	-3.324** (1.073)	-6.282 (3.499)	-6.265** (1.909)	-12.60* (6.166)
Outturn Expenditure per Pupil	3.202*** (0.598)	-0.255 (1.949)	4.039*** (1.192)	1.144 (3.444)
Outturn Expenditure squared	-0.158*** (0.0461)	0.291 (0.190)	-0.192* (0.0954)	0.223 (0.361)
Proportion Female	5.155 (3.409)	11.80 (6.565)	12.69* (6.339)	23.89* (11.25)
Number of Schools within 5km	-0.0541 (0.159)	-0.313 (0.475)	-0.124 (0.281)	0.154 (0.656)
Total Pupils in School	2.761* (1.155)	-1.435 (1.966)	2.293 (1.837)	-4.034 (2.843)
Observations	5157	772	5157	772
$R^2$	0.687	0.631	0.688	0.582
Number of Schools	2583	386	2583	386

Standard errors in parentheses

\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ 

School Fixed Effects and Controls for Prior Attainment are included. Standard Errors are clustered at LEA level.

**Table 9: Difference-in-Difference model**  
**Panel A: Dissimilarity Index: FSM**

	Matched OLS	Matched OLS	Matched FE	Matched FE
Treatment Effect	-0.115 (1.773)	-0.245 (1.665)	-0.0858 (0.976)	-0.330 (1.001)
Wales	-2.589** (1.208)	-4.354*** (1.093)		
Percent FSM		47.67*** (7.611)		70.08** (30.10)
Observations	396	396	396	396
$R^2$	0.159	0.244	0.393	0.441

**Panel B: Dissimilarity Index: KS2 Lowest Quartile**

	Matched OLS	Matched OLS	Matched FE	Matched FE
Treatment Effect	0.539 (1.393)	0.776 (1.425)	0.463 (0.766)	0.668 (1.396)
Wales	-2.330** (1.013)	-2.260** (1.017)		
KS2 Bottom Quartile		9.059 (11.19)		7.719 (46.11)
$R^2$	0.036	0.040	0.052	0.053

**Panel C: Dissimilarity Index: KS2 Highest Quartile**

	Matched OLS	Matched OLS	Matched FE	Matched FE
Treatment Effect	-0.263 (1.332)	-0.263 (1.329)	-0.317 (0.727)	-0.291 (0.700)
Wales	-3.027*** (0.977)	-3.026*** (0.972)		
KS2 Top Quartile		-0.0512 (9.835)		4.237 (15.93)
$R^2$	0.057	0.057	0.023	0.024

Standard errors in parentheses. Year Effects included.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table 10: Growth in FSM within matched quartiles of initial FSM, across England and Wales**

FSM Quartiles	Lowest Quartile	2 <sup>nd</sup> Quartile	3 <sup>rd</sup> Quartile	Highest Quartile
Treatment Effect	-0.00346 (0.00510)	-0.0191 <sup>**</sup> (0.00677)	-0.00951 (0.00580)	0.00624 (0.0122)
After	-0.00626 (0.00400)	0.00648 (0.00558)	0.000463 (0.00397)	-0.00749 (0.00829)
Wales	0.000488 (0.00279)	0.000374 (0.00271)	0.000963 (0.00322)	0.00143 (0.00763)
Constant	0.0873 <sup>***</sup> (0.00192)	0.148 <sup>***</sup> (0.00184)	0.204 <sup>***</sup> (0.00223)	0.312 <sup>***</sup> (0.00539)
Observations	983	976	981	974

Standard errors in parentheses

\* p<0.05, \*\* p<0.01, \*\*\* p<0.001

**Table 11: Average %FSM of school attended before and after, by Mosaic type**

	OLS FSM After	Median Regression FSM After
FSM Before	0.996 <sup>***</sup> (0.00537)	1.009 <sup>***</sup> (0.00638)
Wales	0.0598 (0.0321)	0.0512 <sup>***</sup> (0.0151)
FSM Before * Wales	-0.0138 <sup>*</sup> (0.00622)	-0.0125 <sup>***</sup> (0.00282)
Constant	-0.00456 <sup>***</sup> (0.00111)	-0.00700 <sup>***</sup> (0.00134)
Observations	123	123

Standard errors in parentheses

\* p<0.05, \*\* p<0.01, \*\*\* p<0.001



## APPENDIX: NOT FOR PUBLICATION

**Table A1: Estimated VA versus 'Real' VA**

	All Years	2000	2001	2004	2005	2006	2007	2008
England	0.968	0.966	0.970	0.997	0.986	0.930	0.980	0.972
Wales	0.946	.	.	.	.	0.945	0.941	0.953
Both	0.960	0.964	0.967	0.997	0.987	0.919	0.968	0.961

N.B. Each cell contains the correlation coefficient between school value added, as conventionally calculated, and our measure of value added (the residuals from a regression of mean KS3 score on mean KS4 score). Regressions and correlation coefficients are weighted by school size.

**Table A2: Balancing the Panel**

	Balanced Panel	Unbalanced Schools	Difference
Number of Schools (N)	2,583	416	
Number of Observations (N*T)	18,081	1,457	
Five A*-C	56.37%	46.17%	10.2%***
Free School Meals (FSM)	14.69%	25.09%	-10.4%***
Number of other schools within 5km	7.29	9.63	-2.34***
Academy school	0.00%	9.37%	-9.37%***
School in Wales	7.23%	5.98%	1.25%

\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

**Table A3: Propensity Score Matching (School - Performance)**

	<b>Matching Regression Coefficients</b>	<b>t-statistic (difference in means)</b>	<b>p-value</b>
School Mean GCSE Score	0.0413 <sup>**</sup> (0.0149)	0.65	0.517
School Mean KS3 Score	0.168 <sup>**</sup> (0.0513)	0.62	0.535
Change in School Mean GCSE Score	0.0479 <sup>*</sup> (0.0192)	0.39	0.699
Change in School Mean KS3 Score	-0.0414 (0.0567)	-0.83	0.407
Change in Outturn Expenditure per Pupil	-1.159 <sup>***</sup> (0.183)	0.41	0.679
Outturn Expenditure per Pupil	-0.746 <sup>***</sup> (0.194)	-0.55	0.581
LEA Population Density 2002	-0.196 <sup>*</sup> (0.0828)	-0.9	0.367
Proportion Female	-0.570 (0.507)	0.11	0.913
Total Pupils in School	-0.140 (0.156)	0.5	0.617
Proportion FSM	22.70 <sup>***</sup> (2.234)	-0.64	0.523
Proportion FSM Squared	-29.80 <sup>***</sup> (4.471)	-0.52	0.603
Proportion White	1.292 <sup>**</sup> (0.447)	0.47	0.635
Voluntary Aided	-0.546 <sup>**</sup> (0.181)	-1.22	0.224
Voluntary Controlled	-0.691 (0.380)	0.58	0.563
Foundation	-0.786 <sup>***</sup> (0.221)	-0.54	0.587
Number of Schools within 5km	-0.117 <sup>***</sup> (0.0201)	-1.17	0.244
Constant	-8.652 <sup>***</sup> (1.727)		
Observations	2552		
Pseudo $R^2$	0.312		
Likelihood Ratio (joint difference in means)		Before Matching	After Matching
P-value (chi-squared)		415.15	6.27
		0.000	0.985

Standard errors in parentheses

\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

**Table A4: Propensity Score Matching (LEA - Sorting)**

	Wales
LEA Population Density 2002	-0.00136*** (0.000363)
Proportion FSM	7.160* (2.997)
Constant	-0.945* (0.441)
Observations	127
Pseudo $R^2$	0.316

Standard errors in parentheses

\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ **Table A5: Difference-in-difference of Composition Variables**

	England			Wales			<b>D-in-D (Wal-Eng)</b>
	Before	After	<b>Difference (B-A)</b>	Before	After	<b>Difference (B-A)</b>	
Proportion of pupils attending private schools	7.53%	7.57%	<b>0.04%</b>	2.09%	2.24%	<b>0.14%</b>	<b>0.10%</b>
Number of pupils crossing border to go to school in...	179.0	191.0	<b>12</b>	140.5	141.4	<b>0.9</b>	<b>-11.1</b>

Sources: Independent Schools Council; PLASC

**Table A6: Primary and Secondary School Triple Difference**

	Standardised Key Stage (2 & 4) Point Scores			
	All Obs	All Obs	Matched	Matched
Treatment Effect	-0.536 <sup>***</sup> (0.0745)	-0.542 <sup>***</sup> (0.0709)	-0.532 <sup>***</sup> (0.0470)	-0.514 <sup>***</sup> (0.0460)
Proportion FSM	-0.451 <sup>**</sup> (0.138)	-0.227 (0.132)	0.225 (0.146)	0.251 (0.142)
Outturn per cap Expenditure	-0.105 (0.208)	0.0958 (0.198)	0.925 (1.330)	0.351 (1.300)
Outturn per cap Expenditure sq'd	0.000324 (0.0918)	-0.0407 (0.0871)	-0.674 (2.035)	0.0398 (1.989)
Total Pupils in School	-0.0167 <sup>***</sup> (0.00442)	-0.00874 <sup>*</sup> (0.00424)	-0.0106 (0.0129)	-0.0145 (0.0127)
Prior Attainment Controls	No	Yes	No	Yes
Observations	2024	2024	796	796
R <sup>2</sup>	0.708	0.739	0.828	0.839
Number of Groups	290	290	114	114

Primary and Secondary LEA-year cells. Standard errors in parentheses

\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

**Table A7: D-in-D allowing for Heterogeneity in Prior Attainment and Through Time**

	School Mean GCSE Score (Matched Sample)				School Percent 5 A*-C (Matched Sample)			
	Treatment Effect:				Treatment Effect:			
	KS3 Q1	KS3 Q2	KS3 Q3	KS3 Q4	KS3 Q1	KS3 Q2	KS3 Q3	KS3 Q4
Treatment Effect: Year 2004	-0.317 (0.782)	0.0719 (0.710)	0.158 (0.666)	0.441 (0.730)	-2.918* (1.298)	-1.322 (1.177)	0.477 (1.106)	0.835 (1.211)
Treatment Effect: Year 2005	-3.500*** (0.791)	-2.375*** (0.717)	-2.274*** (0.673)	-0.647 (0.743)	-6.626*** (1.313)	-4.348*** (1.189)	-2.965** (1.117)	0.464 (1.232)
Treatment Effect: Year 2006	-5.692*** (0.780)	-3.941*** (0.719)	-3.802*** (0.688)	-2.307** (0.752)	-10.50*** (1.293)	-6.903*** (1.192)	-5.288*** (1.141)	-3.296** (1.248)
Treatment Effect: Year 2007	-4.203*** (0.804)	-1.783* (0.725)	-3.547*** (0.680)	-1.133 (0.748)	-9.208*** (1.333)	-4.989*** (1.203)	-4.508*** (1.128)	-2.347 (1.241)
Treatment Effect: Year 2008	-3.547*** (0.842)	-2.288** (0.752)	-2.443*** (0.701)	-0.474 (0.739)	-9.145*** (1.396)	-4.900*** (1.247)	-3.341** (1.162)	0.392 (1.227)
School Control Variables	Yes				Yes			
Observations	2702				2702			
R <sup>2</sup>	0.517				0.479			
Number of Schools	386				386			
Interactions F-test (P-value )	0.397				0.454			

Standard errors in parentheses

\*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$

School Fixed Effects, Controls for Prior Attainment and Year Effects are included. Standard Errors are clustered at LEA level. School control variables are %FSM, %Girls, School Size and Expenditure per Pupil and a measure of local competition.