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Evaluating the Impact of Performance-related Pay for Teachers in England

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Abstract

This paper evaluates the impact of a performance-related pay scheme for teachers in England. Using teacher level data, matched with test scores and value-added, we test whether the introduction of a payment scheme based on pupil attainment increased teacher effort. Our evaluation design controls for pupil effects, school effects and teacher effects, and adopts a difference-in-difference methodology. We find that the scheme did improve test scores and value added, on average by about half a grade per pupil. We also find heterogeneity across subjects, with maths teachers showing no improvement.

Keywords: Incentives, teachers pay, education reform, pupil attainment

JEL Classification: J33, J45, D23, I28

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1 Introduction

Improving education outcomes is a key priority for governments around the world. The accumulating evidence suggests poor returns from simply raising school resources[1], so attention has turned to other mechanisms such as school choice, and incentives for teachers. Hanushek (2003) highlights these: "The alternative set of potential policies emphasizes performance incentives." (p. F93), but goes on to note that there is very little robust evidence yet on the impact of such incentives (p. F94). In this paper we start to plug that gap. In 1999, the UK government introduced a performance-related pay policy for teachers, with pupil progress (value-added) as one of its key criteria. Using longitudinal teacher-level data and a difference-in-difference research design, we provide a quantitative evaluation of the policy's impact on test score gains.

The incentive scheme was explicitly teacher-based (rather than school-based) and so equivalent data is required to properly evaluate it. We have collected longitudinal data following teachers over two complete two-year teaching cycles, before and after the policy was introduced. By dealing with schools directly we were able to link pupils to the teachers who taught them for specific subjects, and not rely on school level averages. This is a crucial attribute of our data: school averages would not allow us to directly compare the performance of eligible and ineligible teachers. For the pupils linked to the sample teachers, we collected prior attainment data, so we can control for pupil characteristics and measure the target of the scheme – pupil progress. Thus in the analysis we can control for pupil and teacher fixed effects.

[1] See Hanushek (2003).

We find important effects of the incentive scheme, that are both statistically and economically significant. We find that teachers eligible for the incentive payment increased their value-added by almost half a GCSE grade per pupil relative to ineligible teachers, equal to 73% of a standard deviation. GCSE exams, taken at age 16, are the key qualifications for entry into higher education, so these are high stakes tests and this increase is not trivial. We find significant differences between subjects, with eligible maths teachers showing no effect of the scheme.

Our difference-in-difference research design means that differences in general between eligible and ineligible teachers drop out once comparisons are made over time. We compute the change in each teacher's average test score gain between two complete teaching cycles, and compare the results for eligible and ineligible teachers. In the scheme, the chance to earn the initial performance bonus was offered to teachers who had been in the profession for about 8 years. Thus eligible and ineligible teachers differed systematically in experience, and such differences in experience will not drop out of the difference-in-difference if there is a non-linear experience-effectiveness relationship. Specifically, if teachers improve in their capacity to generate value-added but at a decreasing rate, then all else equal we would see greater improvements in progress between the two teaching cycles for the less experienced (ineligible) group. Therefore we also perform regression analysis, controlling for teacher experience, detailed in section 6.

The rest of the paper is organized as follows. The next section reviews the literature on incentive schemes for teachers. Section 3 discusses the UK scheme and section 4 our evaluation methodology. Section 5 describes the data and section 6 presents the results. Finally, section 7 offers some conclusions.

2 Previous literature

There is a large literature on the effects of incentives in the private sector (see Prendergast (1999) and Murphy (1999) for surveys). There is a smaller but growing body of evidence about incentives in the public sector (see surveys by Dixit (2002) and Burgess and Ratto (2003)). Some recent empirical work has produced convincing results suggesting that incentives do affect the behaviour of employees in particular parts of the public sector (see, for example, Courty and Marschke (2001) and Kahn, Silva and Ziliak (2001)).

There is some quantitative evidence on the effects of teacher incentive schemes on pupil attainment. The literature has been hampered by difficulties in finding measurable proxies for key variables, in obtaining individual-level data, and in designing evaluations which include adequate controls when incentive schemes are not introduced as part of an experimental design. Lazear (2003) sets out a theoretical framework for thinking about performance-related pay (PRP) for teachers. He discusses the pros and cons of PRP in general, and then as applied to the case of teachers. PRP plays two roles – an incentive mechanism to elicit greater effort, and a recruitment and retention device to improve average teacher quality. Our study focuses on the former, and we briefly review previous findings on this issue.

Early studies by Ladd (1999), by Cooper and Cohn (1997) and by Boozer (1999) found a positive relationship between incentive schemes and pupil attainment, however the results of these studies are not conclusive. Ladd (1999) compared gains in school-level test scores in Dallas with gains in other cities, to evaluate the impact of a school-level bonus scheme introduced in Dallas. The study is limited by the lack of data enabling "before/after" comparisons, or controls for pupil, teacher or school fixed effects, however Ladd is able to control for a number of school characteristics, such as racial mix and relative deprivation. The results are generally positive, in that pass rates appeared to increase faster in Dallas than in other cities. Effects differ by ethnic sub-groups, being most positive for Hispanics and whites, and insignificant for blacks. The results are, however, muddied by the fact that a positive Dallas effect is also found for the year before the scheme was introduced.

Cooper and Cohn (1997) and Boozer (1999) evaluate a South Carolina scheme, which included both school-level rewards and rewards to individual teachers. Pupil effects are controlled for, since the dependent variables are gain in median class test scores, and the studies also control for teacher characteristics and for class-level pupil characteristics. The incentive plan variables were positive and significant. However this variable confounds both incentive effects and selection effects, since teachers could choose whether or not to apply for an award. As Cooper and Cohn put it, "It is possible, even likely, that only the most productive teachers choose to apply for an award" (pp.320-1).

Studies by Eberts et al.(2000), by Figlio and Kenny (2003) and by Dee and Keys (2004) were able to use rich data sets to control for many potentially confounding variables. It is, however, notable that although these papers assess the relationship between incentive schemes and pupil attainment, they evaluate schemes which did not directly link rewards to pupil attainment. Promotion was based on overall good performance, which included time spent in the classroom, evidence of skills and classroom evaluation; test results were not the major deciding factor in measuring 'success'. Eberts et al (2000) used difference-in-difference techniques to assess the impact on pupil attainment of a Michigan merit pay scheme which rewarded individual teachers according to student retention rates and their performance on pupil evaluation questionnaires. The scheme did not directly target pupil attainment although it was hoped that this would be an indirect benefit of the scheme. The scheme had a positive and significant impact on student retention. However, pass rates decreased, and attendance rates and grade point averages were unchanged. The authors conclude "that incentive systems within complex organisations such as schools,…may produce results that are unintended and at times misdirected." (p.19).

Figlio and Kenny (2003) combine panel data from the US National Education Longitudinal Survey to estimate the effects of teacher incentives on an education production function. They define incentive schemes as any merit raise or bonus awarded to any proportion of teachers in a school. The variables do not identify whether schemes intended rewards to be tied directly to pupil attainment. They control for various student, teacher, school and family characteristics. The results are positive, particularly in public and poor schools. Test scores are higher in schemes that are more high powered, in the sense of being given to only small numbers of teachers within a school and offering higher rewards. However, the results may not be generalisable since the schools that responded to the survey are not representative of schools in the US.

Dee and Keys (2004) use a fixed effects model to estimate incentive effects on student SAT scores in the Tennessee STAR and Career Ladder Evaluation schemes. The contemporaneous data from STAR reduces the problem of unobserved heterogeneity in student propensity for achievement. There are controls for pupil, teacher and class fixed effects and characteristics. The results show a positive effect, which varies across subjects and with teacher seniority. The authors investigated whether the results were biased by the high rate of student attrition from the schools participating in STAR by imputing test scores for those students who left, but the results remained similar.

Studies from Kenya and from Israel have been able to capitalize on the experimental design of incentive schemes and use difference-in-difference techniques to control for confounding variables. Glewwe, Ilias and Kremer (2003) investigate a Kenyan school-based incentive scheme, by comparing differences in test scores between treatment and comparison schools. Differences in test scores are positive and significant in the scheme's second year in both the random effects and difference-in-difference models.

In a series of papers, Lavy analyses the impact of incentive schemes introduced in Israel: two school-level schemes targeted on a variety of outcomes (Lavy 2001); and a tournament scheme which rewarded individual teachers according to their pupils' attainment (Lavy 2003, 2004). In the first paper, evaluating the school-level scheme, the quasi-experimental design of the scheme allowed Lavy to employ difference in difference techniques and control for school fixed effects. Results on all student outcomes are positive. The schemes include both financial and non-financial rewards: Lavy compares their cost effectiveness and concludes that money incentives are the more effective, but that the schools included in the scheme are probably not representative of all Israeli schools. Lavy (2003, 2004) uses panel data to evaluate the effects of a teacher-based tournament scheme on pupil attainment. A rich dataset allowed for propensity score matching and OLS estimates to be calculated, controlling for various student (including lagged) characteristics and school covariates. The results are positive and significantly different from zero, but are complicated by non random assignment to the program. However, identification strategies based on a regression discontinuity design and particularly matching, produce similar results, suggesting that there was indeed a positive relationship between the incentive scheme and pupil test scores. The results of a follow up survey suggest that the results were achieved by reducing class size and increased teacher effort. Again, however, Lavy suggests that the schools may not be a representative sample of Israeli schools.

In summary, the results of the earlier literature in this area are confounded by the problems of distinguishing the effects of teacher quality from the effects of increased teacher effort, and by the problems of controlling for other fixed effects such as pupil ability. Later studies which have been able to use rich data sets to manage identifications problems have found a positive relationship between financial incentives and pupil attainment. Most studies, including our own, work on an un-representative sample of schools.

3 The English education sector and the PRP scheme

(a) The structure of the English education market

The English education system has been choice-based since the Education Reform Act of 1988, which introduced a 'quasi-market' in education (Glennerster 1991). The following key features of the education quasi-market remain to the present date: some degree of parental choice on schools, with money following the pupils; funding and management of schools devolved to a more local level, but with funding provided by central government, out of general taxation revenue. The intention was that per capita funding and parental choice would bring about competition between schools for pupils, which would raise educational outcomes. While the quasi-market increased the autonomy of individual schools, each school still operates within a fairly centralised system. Central government regulates the way in which the quasi-market can operate, as well as controlling the contents of the national curriculum and the accompanying set of national exams (detailed more below). Pay scales for teachers are also centrally determined.

Parental choice is informed by a range of indicators for each school. The "league tables" report the results of exams taken by all pupils at the end of each Key Stage of the national curriculum; at ages 7, 11 (taken in primary school), 14 and 16 (taken in secondary school). These tests are known respectively as Key Stage 1 (KS1) to Key Stage 4 (KS4) exams. Pupils take KS1, KS2 and KS3 tests in English, maths and science. KS4 exams, taken at the end of compulsory schooling, cover a much broader mix of subjects, and comprise two different types of exam known as GCSE and GNVQ, the latter historically being associated with more vocational subjects. English, maths and science are compulsory for all students at KS4 as GCSE exams, in addition to which they are able to choose from a broad range of options. For secondary schools (our focus in this paper) the primary focus of both schools and parents has been the raw output indicator of the proportion of pupils gaining five or more KS4 passes between grades A * and C. In this paper, we consider teacher impact on both raw output – GCSE grades – and on value added, i.e. test score gains between KS3 and GCSE. In all our analyses, we focus on the compulsory subjects of English, maths and science to eliminate selection effects.

(b) The PRP scheme

Interest in PRP grew during the 1980s, stimulated by a perception that teaching standards were poor and contributing to low educational attainment and, perhaps, to poor economic performance (Tomlinson 1992, 2000)[2]. During the 1990s, successive administrations attempted to introduce some form of PRP into state schools, but succeeded only in introducing PRP for headteachers and their deputies (Marsden and French 1998).

The 1997 Labour administration signalled a range of reforms to education in a consultative Government paper published in 1998: "Teachers: meeting the challenge of change". The reforms included the introduction of a performance-related system, the Performance Threshold plus the Upper Pay Scale, designed to affect teacher effort as well as recruitment into and retention within the profession. The Green Paper argued that teacher motivation was adversely affected by a culture which did not recognise and reward outstanding performance. The scheme was introduced in the academic year 1999/2000, with the first applications submitted by teachers in July 2000.

Prior to the introduction of the PRP scheme, all teachers were paid on a unified basic salary scale, which had nine full points, ranging from £14,658 – £23,193 per annum (2000 prices). An individual's position on the scale depended on his/her qualifications

[2] Performance related pay for teachers is not a new idea. During the second half of the nineteenth century teachers in English state secondary schools were paid according to students' exam results, but this was abandoned because it was believed to reward teachers who concentrated on more able pupils (Hood et al 1999). Formal systems of performance related pay (PRP) were not used in state schools during most of the twentieth century.

and experience, and teachers usually progressed up the scale in annual increments. In addition to the basic salary, there were management, excellence, or recruitment and retention points (School Teachers' Review Body 2000). In 1999/2000, about 75% of teachers were at the top of this scale, at spine point 9.

After the reforms, teachers at spine point 9 could apply to pass the Performance Threshold. Passing the Threshold has two effects. First, it gives teachers an annual bonus of £2,000, payable without revision until the end of their career and included in calculations of pensionable salary. It is therefore of significant lifetime value. Second, once over the Threshold, teachers move onto a new Upper Pay Scale (UPS), which comprises additional increments, each of which are also related to performance[3].

To pass the Threshold, individual teachers had to demonstrate that they had reached acceptable standards in five areas: knowledge and understanding of teaching; teaching management and assessment; wider professional effectiveness; professional characteristics; and pupil progress (DfEE 2000). In the area of pupil progress, the focus of our evaluation, the Threshold application form gave teachers the following instruction:

> *"Please summarise evidence that as a result of your teaching your pupils achieve well relative to their prior attainment, making progress as good or better than similar pupils nationally. This should be shown in marks or grades in any relevant national tests or examinations, or school based assessment for pupils where national tests and examinations are not taken"* (DfEE 2000).

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[3] In 2000 the UPS comprised five spine points; this has since been reduced to three (www.teachernet.gov.uk provides more information).

Teachers were required to complete their application forms by July 2000, demonstrating their performance in each of the five areas. The information provided did not necessarily just cover the year 1999/2000 but could be based on the teacher's career to that point. Passing the Threshold was largely about rewarding historical performance, and so cannot be considered true performance pay. However, passing did give access to the Upper Pay Scale which does offer conventional pay performance pay.

80 per cent of eligible teachers applied. Headteachers then assessed each application and recommended whether or not individual teachers should pass the Threshold. Each school was then audited by an external assessor. Teachers who didn't pass had limited rights of appeal. Performance Threshold payments were funded out of a separate budget, administered by central government, with no limit or quota on the number of teachers allowed to obtain these payments[4].

Progressing up the UPS was similarly focussed on "sustained and substantial performance", and Headteachers were required to conduct a performance review before awarding a pay increase.

Wragg et al (2001) conducted a survey of a random sample of 1000 schools in order to investigate this process. They found that in these schools, 88 percent of the eligible teachers applied, and of these, 97 percent were awarded the bonus. This very high figure influences the interpretation of the scheme. It suggests that, *ex post*, the initial Threshold operated more as a general pay increase for (almost) all teachers at the eligible point of the scale. Clearly, an unconditional pay increase will have little impact on

[4] See Croxson and Atkinson (2001a, b) for an analysis of headteachers' views on both the implementation and the impact of the Performance Threshold.

teacher effort, though it may help staff retention rates. In fact, evidence from Marsden (2000) suggests that *ex ante* it was seen by a majority of teachers as a real incentive scheme, and not as a general pay rise. Marsden (2000) reports on his survey of teachers taken after the Performance Threshold was announced, but before it was implemented. Two particular questions on the questionnaire are relevant to us:

	Disagree (%)	Neutral (%)	Agree (%)
"The Green Paper pay system is a device to avoid paying more money to all teachers"	9	18	68
"In practice, many excellent teachers will not pass the Threshold because there is certain to be a quota on places available"	3	8	82

Source, Marsden. D (2000), p. 4
Sample size is c. 3000.

This suggests that a substantial majority of teachers were expecting the scheme to be 'real' – that is, for only some teachers to be awarded the bonus. Given this, we should expect to see an impact on effort for eligible teachers. This impact should be reinforced by the forward-looking element of the scheme, the Upper Pay Scale, progress up which is dependent on additional assessments of individual teacher performance.

4 Evaluation Methodology

In this section we set out the model and evaluation methodology. Pupils are indexed i, teachers j, and teaching cycle (data tranche) t. Denote the pupil's test score by g, value added by v, pupil ability by Z and pupil effort by e. Teacher effectiveness is X, performance pay eligibility is I, and experience is W. School effects on test scores are denoted S, a common time effect is T (for example, due to changes in testing) and test score noise is u. Teacher effectiveness combines the effects of ability, effort and experience:

$$X_{jt} = b_j + bI_{jt} + b_1 f(W_{jt}) \qquad (1)$$

The term b_j captures individual teacher heterogeneity in ability and core effort, and *f(.)* is an increasing function. The pupil's test score outcome is determined as follows:

$$g_{i(j)t} = g_1 Z_i + g_2 e_i + g_3 X_{j(i)} + S + T + u_{i(j)t} \qquad (2)$$

It seems likely that there are strong interactions between pupil effort and teacher effectiveness – better teachers eliciting more pupil effort, and hard working pupils encouraging more teacher preparation. These multiplier effects are included in the parameters g_2 and g_3. Substituting from (1) into (2) and computing the teacher mean score, $\overline{g}_{jt}$:

$$\overline{g}_{jt} = g_1 \overline{Z}_j + g_2 \overline{e}_j + g_3 b_j + g_3 bI_{jt} + g_3 b_1 f(W_{jt}) + S + T \qquad (3)$$

where the upper bar denotes averaging over pupils taught by teacher j, and we assume that mean testing noise for a teacher is zero, $E(\overline{u}_{jt}) = 0$. Taking the difference between the two tranches for teacher j yields:

$$\Delta\overline{g}_j = (g_1 \Delta\overline{Z}_j + g_2 \Delta\overline{e}_j) + g_3 b\Delta I_j + g_3 b_1 \Delta f(W_j) + \Delta T \qquad (4)$$

where D denotes differencing between tranches. The change in the teacher's mean performance depends on differences in the pupils assigned to her, whether she increased her effort due to the incentive scheme, her gain in experience and any common time shocks. Note that the fixed teacher ability term has dropped out, as has the school effect. Finally, we compare the expected value of (4) for eligible teachers (*DI = 1)* and ineligible teachers (*DI = 0).* With D denoting differencing expected values across eligibility status,

$$D\Delta\overline{g} = \left(g_1 D\Delta\overline{Z} + g_2 D\Delta\overline{e}\right) + g_3 \boldsymbol{b} + g_3 b_1 D\Delta f(W) \qquad (5)$$

This is the difference-in-difference. Assuming that the experimental design allows us to set $E\left(g_1 D\Delta\overline{Z} + g_2 D\Delta\overline{e}\right) = 0$ (which we discuss below), this yields $D\Delta\overline{g} = g_3 \boldsymbol{b} + g_3 b_1 D\Delta f(W)$. The parameter of interest is $g_3\boldsymbol{b}$, the impact of eligibility for the scheme on teacher effort and hence on pupil test scores. We can recover this directly from the difference-in-difference estimates if *f(.)* is linear, and the second term therefore disappears. If *f(.)* is concave, then $D\Delta\overline{g}$ underestimates $g_3\boldsymbol{b}$. We discuss this further below.

The other main outcome variable we use is value-added, *v*. Pupil value added depends on their effort, their teacher's effectiveness, school and time effects and testing noise:

$$v_{i(j)t} = m_1 e_i + m_2 X_{j(i)} + S + T + v_{i(j)t} \qquad (6)$$

Following the same procedure as above yields the value-added difference-in-difference:

$$D\Delta\overline{v} = \left(m_1 D\Delta\overline{e}\right) + m_2 \boldsymbol{b} + m_2 b_1 D\Delta f(W) \qquad (7)$$

We can now address the plausibility of our assumptions that $E\left(g_1 D\Delta\overline{Z} + g_2 D\Delta\overline{e}\right) = 0$ for test scores and $E\left(m_1 D\Delta\overline{e}\right) = 0$ for value-added. There is a strong reason for believing that the latter holds. Pupils in England are either grouped into classes on test outcomes, or they are not systematically grouped at all. They are not grouped on effort, so no classes are created based on effort. Also, the timing of the incentive scheme makes it highly unlikely that schools would be able to differentially assign classes to eligible teachers, given that successful applications for the initial Threshold were largely based on performance data from classes allocated prior to the PRP scheme being introduced. So we argue that the value added results are free from any assignment bias, and that the test score results are highly likely to be. In fact, any difference between the results for the two outcome measures is an indication of potential ability-based differential class assignment between eligible and ineligible teachers.

We present results for the difference-in-difference estimates in (5) and (7). As noted, these include the effect of differential experience gain. The nature of the incentive scheme means that ineligible teachers were necessarily less experienced than their eligible colleagues. If *f(.)* is concave, then *DDf(W)* will not drop out of the difference-in-difference. We also therefore estimate regression equations for (4) and the value-added equivalent, controlling for measures of experience.

In fact, our data allow us to differentiate outcomes by subject – English, maths and science – and so we compute (5) and (7) separately by subject. This is useful since it addresses spillover effects between teachers. For example, an incentivised teacher might set a lot of homework, thus cutting students' time for other subjects and potentially reducing those other scores. Within subject such a spillover is not possible – if a student has two maths teachers for example, the single maths score is attributed to both. We do not have enough data to run the regressions controlling for experience by subject, but pool over subjects and include subject dummies.

5 Data

The performance pay system introduced in 1999 combined the Performance Threshold (PT) and the Upper Pay Scale (UPS). For each cohort of teachers, the former offers a substantial pay rise following a successful application, and a gateway to the UPS, which offers a series of performance-related pay increments. We analyse the performance of a sample of teachers who were eligible for the performance pay system when it was introduced in September 1999. By focussing on eligibility as opposed to application, we sidestep the problem faced by researchers using the South Carolina scheme of likely differential application rates (Cooper and Cohn, 1997, and Boozer, 1999). As noted above, in this scheme almost all eligible applied, and almost all who applied were successful, so almost all the teachers in our eligible group faced both the PT and the UPS. Thus we compare the ineligible and those passing the PT and placed on the UPS.

We use data on pupils' performance in the two-year GCSE teaching cycle before the system was introduced – our first (control) tranche runs from September 1997 to June 1999. For the treatment period, we take the first teaching cycle that excludes the PT monitoring year for our sample of teachers (September 1999 to June 2000), since success at the PT was based on historical data, and the UPS provides a conventional performance incentive. Thus our second tranche runs September 2000 to June 2002. The timeline in Appendix Figure 1 sets this out in detail.

(a) Data requirements

The first key feature of the data is to control for pupil prior attainment and measure value-added. The UK education assessment and information system provides a number of opportunities to measure this. In this paper we examine value added between the Key Stage 3 (KS3) exams at age 14 and the GCSE exams at age 16. Key Stage exams are taken in English, Maths and Science; pupils also have to take GCSE exams in these subjects (among others). The choice of the GSCE – KS3 gain is for several reasons. First, students are mainly taught by the same teacher(s) for a particular subject throughout that period. Second, the gap between exams is shorter than the five-year cycle between KS2 exams at 11 and GCSE exams at 16. As measuring value added requires school records for all years in which exams are taken, a shorter focus period is easier to undertake. Third, the GCSE exams are important for students as they are the crucial gateway qualifications for higher education and for the employment prospects of those who leave school at 16. Fourth, the GCSE exams are the headline component of published school performance tables. They are thus nationally set and marked high-stakes exams for both pupils and for schools.

The second key feature is the longitudinal element, following the same teachers through complete KS3 – GCSE teaching cycles, one before and one during the scheme. A detailed timeline is given in Appendix Figure 1. The scheme was introduced in the academic year 1999/2000, with eligibility defined in September 1999, and the first forms submitted in July 2000. The 'before' (tranche 1) data relate to the teaching cycle from September 1997 through June 1999, when the GCSE exams are taken; the prior attainment measure is the KS3 score from June 1997. The 'after' (tranche 2) data are three years later, starting September 2000 to June 2002, with KS3 scores from June 2000. Teachers are tagged as eligible if they were on spine point 9 in September 1999. The evaluation design requires data that links teachers to individual pupils, before and during the scheme. As students may be taught by a number of teachers in the two class years between KS3 and GCSE, to create the teacher average requires that pupils be matched to teachers for both these years, in each of the two teaching cycles.[5]

The data linking pupils to teachers are class lists, which are held only by schools. Schools therefore were approached directly initially in 2000, and invited to participate in the study. They were told at this point that they would have to provide data for two cohorts of pupils: those who followed the GCSE syllabus between 1997 and 1999 (the 'before' data) and those who followed the GCSE syllabus between 2000 and 2002 (the 'after' data). The data we requested from each participating school are summarised in Table 1. The information covers 2 tranches of students: those who took their GCSEs in 1998/9 and those who took them in 2001/02. For each tranche, we requested the student's GCSE and KS3 scores in English, maths and science plus other pupil characteristics including date of birth, gender and home postcode (zip code). From files on teachers we requested information on teacher characteristics, including pay spine point, salary and threshold eligibility, and gender. The pupil and teacher data were matched at teacher level.

To measure pupil progress and account for prior attainment, we compute value-added as the residual from regressing pupil GCSE score against KS3 score and gender

[5] Students do not generally repeat years between KS3 and GCSE.

within our dataset, combining both tranches of data. We did this subject by subject, and also the pupil's average GCSE score against average KS3 score. We have experimented with other functional forms, but this straightforward linear model typically works well[6].

(b) Sample representativeness

These data demands on schools were quite onerous. In fact, relatively few schools keep class list data from one year to the next (the software system used by many schools overwrites the class lists each new academic year). Thus provision of data on class lists back to 1996/7 in 2000 was not possible for many schools. In addition, teacher turnover is high, meaning that much of the data provided could not be used to provide estimates of value added. Our final sample uses data from 18 schools, covering 182 teachers and almost 23,000 pupils. This is a low acceptance rate from schools, but is a function of the high data demands of the evaluation. Details of our approach to schools are given in Appendix 1.

There can be no presumption that this sample is representative of all secondary schools. In terms of unobservables, the schools that did participate were more likely to have good performance management and IT systems as schools without good IT systems could not typically provide the required class lists.

We also undertook in-depth qualitative interviews with participating school to establish, *inter alia*, the view of the head-teacher on the scheme and its operation within their school. It is not the case that all the participating schools were keen proponents of

[6] The measure of value added used by the Department for Education and Skills normalises value-added to have mean zero at each KS3 level, but we did not want to impose this feature on our data.

the reform. There was a range of views, illustrated by the following quotes (see Atkinson and Croxson (2001a, b) for further details and analysis):

> *"So I'm not opposed to performance-related pay, per se … the notion of reward for good behaviour, that's how you motivate children I think. And I don't think adults are any different."*

> *"I personally am a supporter of performance-related pay. But I do have some major misgivings about the Threshold Assessment component of it. Because my personal view is that isn't performance pay …the system that exists at the moment I feel doesn't discriminate adequately enough. It's discriminatory measure is: is the teacher competent or is the teacher incompetent? For example, you know, if the outcomes of your last evaluation from OFSTED are that your teaching is unsatisfactory then clearly you wouldn't go for a threshold. If you're under current competency procedures then you wouldn't go for the threshold. But other than that it's hard to see who isn't going beyond the threshold. Because everybody seems to be."*

> *"I don't think the Performance Threshold is anything other, if I'm being crudely honest, to sum it as being an interesting political way of giving teachers 2000 quid on the basis they're probably Labour voters."*

Data on the performance of pupils in the sample, presented in Table 3, show a general increase in KS3, GCSE and value added scores across all subjects from tranche 1 (1997/9) to tranche 2 (2000/02). The exception is the maths value added score, which decreased. Table 4 uses national data from the Department for Education and Skills (DfES) to compare the exam performance of the sample with performance at national level. Generally, the over time changes for each pupil output measure for the sample do not match the nationwide picture closely. Between the 2 tranches, there was a nationwide increase of 0.12 average KS3 points, a slight decrease in average GCSE score and no change in value added. The sample data shows an increase of 0.05 KS3 and 0.14 GCSE points, as well as an increase in value added over the period. As noted above, the sample contains schools with better data archives than average, which may be used for performance management. The bigger test score gains in the sample may reflect this, or may indicate that schools interested in raising school achievement chose to participate.

The sample of schools is not, therefore, very representative of the national picture in terms of value added and GCSE scores. However, the implications for the sample of teachers is less clear, and teachers are the unit of observation. There are 182 teachers in our estimation sample. Of these, 1 has missing eligibility data, 36 were not eligible for performance related pay and 145 were eligible. Thus in our core analysis when we split by eligibility status we have 181 observations, but in the summary tables on overall scores, we report all 182. A summary of the teacher characteristics for the two groups is presented in Table 2 (the number of teachers with the relevant data are in brackets). Eligible teachers are on average, older and more experienced than the not eligible teachers, which we would expect given the nature of the program. Average KS3 scores across the 2 tranches are higher for eligible teachers, indicating that these teachers teach more able pupils at age 14 (KS3).

6 Results

We present results in three parts: first, overall teacher level outcomes; second, the difference-in-difference analysis; third, regression analysis, and finally we provide an interpretation. We briefly consider the potential for gaming of the scheme. Throughout, we report weighted results, where the weights adjust for the number of pupils taught by a teacher (unweighted results are available from the authors).

(a) Teacher level outcomes

We begin by describing the overall pattern of exam results at teacher-tranche level, across the two two-year teaching cycles, split by the three subjects: English, maths and science. Denote g_i as pupil i's GCSE score in a particular subject. Denote *J1* as the set of pupils taught by teacher j in tranche 1(numbering n_{j1}), and equivalently *J2* and n_{j2} in tranche 2. We compute teacher-tranche level summary statistics as the mean over all pupils taught by a given teacher in a given two-year cycle[7]. That is, teacher-tranche mean GCSE performance is given by $\bar{g}_{j1}$ and $\bar{g}_{j2}$:

$$\bar{g}_{j1} = \frac{\sum_{i \in J1} g_i}{n_{j1}} \qquad \bar{g}_{j2} = \frac{\sum_{i \in J2} g_i}{n_{j2}} \tag{8}$$

The teacher level change in GCSE performance is given by:

$$\Delta \bar{g}_j = \bar{g}_{j2} - \bar{g}_{j1} \tag{9}$$

the empirical counter-part to (4). Similarly, we define $\bar{v}_{j2}, \bar{v}_{j1}$ and $\Delta \bar{v}_j$ based on pupil i's value added, v_i.

[7] In all but 1 case, teachers just taught one subject, so teacher level is also implicitly subject level.

The simple GCSE results in table 5 show an overall increase in mean GCSE scores for our teachers. The quantiles show that this was not universal and that some teachers had considerable falls in GCSE scores between the two cycles. These patterns are clear in the subject specific results as well.

Table 6 analyses value-added. There is an overall increase in value-added between the two dates, and in English and science, but not in maths. Again there is considerable dispersion of the levels of teacher mean VA, and also of teacher level changes in mean VA. For example, adding over all subjects, the biggest fall in VA is 1.58, equivalent to moving from the median VA (-0.01 in tranche 2) to the minimum (-1.57 tranche 2). The biggest improvement is of a similar scale. As these are value-added outcomes, they do not depend on the particular set of pupils assigned to a teacher. This suggests considerable cycle-to-cycle variability in teacher production of value-added. Indeed, remarkably the change in VA for a given teacher over time is as variable (standard deviation of 0.58) as the difference between teachers in VA within a tranche (standard deviation of 0.55).

(b) Difference in difference analysis

Figures 1 and 2 present the distribution of teacher-level GCSE and VA changes, split by subject and by teacher eligibility status. Each panel shows a kernel estimate of the density of the outcome variable (GCSE or VA) separately for each tranche. Some of the plots are based on relatively small numbers of teachers.

Focussing on the GCSE plots across all subjects (the top row of Figure 1), we see differences across eligibility status: for eligible teachers, there is some evidence of a rightward shift of the whole distribution in tranche 2, whereas the increase in mean GCSE for ineligibles arises from a large reduction in low GCSE scores. These patterns are not universal across subjects. The weighted VA plots across all subjects (the top row of Figure 2) reflect a fairly similar pattern – a general rightward shift for eligibles, but a change only in the lower half of the distribution for ineligibles.

The mean increases in scores across teaching cycles and eligibility status can be seen in Table 7, along with some details of the distribution. Comparing the distribution of GCSE changes across eligible and ineligible teachers, we see that the former raised their GCSE scores by 0.21 on average and the latter by 0.13. Regressing the change in GCSE between the two tranches against the eligibility dummy shows the difference-in-difference of 0.08 to be insignificantly different from zero. Looking at other parts of the distribution, we see that there are significant positive differences in the differences at the lower quartile and the median, while the upper quartiles of the distributions show a bigger gain for ineligibles[8]. These are suggestive of a rightward shift in the lower and middle parts of the distribution of GCSE differences for the eligible teachers. By subject, the difference-in-difference results reveal a larger increase in mean GCSE scores for ineligible teachers in maths and English; this is reversed in science. None of the subject difference-in-difference estimates are significantly different from zero.

The pattern across the distribution shows considerable variation over time in performance for a lot of teachers, illustrated in Figure 3. This plots for each teacher, her/his mean GCSE score in tranche 1 on the horizontal axis against the equivalent in tranche 2 on the vertical axis. Changes in performance therefore appear as deviations from the 45^{o} line, and eligibles and ineligibles are separately identified. Overall, there is a

[8] Standard errors were derived from quantile regression.

greater concentration of ineligible teachers near or above the 45^0 line relative to those who are eligible. We also break down the performance of teachers over the two tranches by subject, with the largest variation appearing in maths.

However, the GCSE results depend in part on the pupils assigned to teachers in schools. Our main focus is on VA. Figure 4 plots the comparison across the two tranches for eligible and ineligible teachers. Again we see the considerable variation in teacher performance between teaching cycles. The graph also shows no obviously greater improvement for eligible teachers. Table 8 confirms this impression: VA increases slightly more for eligible than ineligible teachers, though not significantly so. Again considering other parts of the distribution, the lower quartile of the distribution of differences is significantly higher for eligibles, with no significant difference at the median or upper quartile. As with GCSE, this suggests greater impact of the scheme in the lower half of the distribution. Looking at individual subjects, the difference-in-difference is positive in both English and science, but much larger and negative in maths. The overall difference-in-difference estimate is small and positive, but insignificantly different from zero (note that the estimates for science (positive) and maths (negative) are significantly different from zero).

The results so far suggest no clear difference between eligible and ineligible teachers. In terms of overall value-added there is a very small and insignificantly positive effect, arising from positive effects in science and English, and negative in maths. However, as noted above, the difference-in-difference estimates under-estimate the impact of the incentive scheme if the experience-effectiveness profile $f(W)$ is concave. We now turn to parameterising teacher experience.

(c) Regression Analysis

The difference-in-difference estimates control for teacher effects, (implicitly school effects) and pupils' prior attainment. What they do not control for, however, is the systematic difference in experience between eligible and non-eligible teachers arising from the nature of the performance pay scheme. If there is a positive and concave experience-effectiveness relationship, we can only identify the impact of the incentive scheme by controlling for differences in experience.

There is insufficient data to characterise a continuous experience-effectiveness schedule. We do know an individual's spine point, age and years spent at the current school, but not their total teaching experience. The pay spine point is the best summary of experience as teachers are rewarded for experience by movement up the pay scale. Figures 5 and 6 plots this against teacher mean GCSE and VA outcomes. It is clear that the mass of data at spine point 9 – the eligible set – makes it difficult to describe a clear relationship of outcomes with experience. It also makes it difficult to separately identify eligibility and experience.

An alternative to a smooth profile is to isolate new teachers and separate out any substantial gains they may achieve by moving up a learning curve. We define a novice teacher dummy, equal to 1 if the teacher is at spine point 5 or below in the first tranche. We also define a leadership dummy, equal to 1 if the teacher is a deputy head or head teacher.

Regression results for a variety of specifications of experience are shown in Table 9 for GCSE and Table 10 for VA[9]. The unit of observation is a teacher, and the dependent variable is the teacher's change in GCSE score (respectively VA) between the two tranches. A full set of school dummies is included, as are two subject dummies. Taking Table 9 first, the first three columns illustrate the difficulty of trying to separately identify eligibility and spine point as a general measure of experience. Comparing columns 1 and 2, we see that the inclusion of the eligibility dummy makes the spine point variable much more negative – it almost doubles in size. The eligibility dummy itself is reasonably big (and positive) but not significant. Dropping the spine point variable pushes the eligibility dummy close to zero. In other words, high collinearity between eligibility and spine point makes it impossible to isolate any experience effect through the spine point variable. Our preferred specification is in column 4, in which we pick up the effect of experience through the "novice teacher" dummy. This is large and significantly positive. Its inclusion also yields a positive and sizeable eligibility effect, which is significant at 5%. Of the other variables, "years in school" is always zero (conditional on the other variables), and "leadership" role is positive but not significant. As expected, given that we have differenced out teacher effects, the gender of the teacher appears not to matter a great deal. We discuss the quantitative significance of the eligibility coefficient below.

The regression results for VA are shown in Table 10. These tell a very similar story. The change in coefficients between columns 1 to 3 again reflects the high correlation between eligibility and spine point. In column 4, we see that the novice dummy is significantly positive and that eligibility is large and positive with a t-statistic

[9] Note that in the specifications involving the spine point variable we only have 180 observations.

of 2.66. Years in school, leadership role and teacher gender again have no effect. The consistent drop in VA in maths is clear.

In Table 11 we allow for the effect of eligibility to vary between subjects, reporting specifications equivalent to column 4 in Tables 9 and 10. This is suggested by the difference-in-difference findings above. We see that for GCSE, the eligibility dummy is positive and well-defined. The interaction with the maths subject dummy is negative and of similar size, but not significant. In the case of VA, the eligibility coefficient is positive and now well-defined[10]. Again, the maths interaction is equal and opposite in sign, and this time is significant. The sample of maths teachers is not different in terms of observables to the other subject teachers – very similar age and experience patterns and a gender ratio between English and science.

We are interested to see whether the incentive scheme had a constant impact across the ability range, or had differential impacts. To get at this, we compute for each teacher the difference between the k^{th} percentile of the outcome distribution for tranche 2 and subtract from that the k^{th} percentile of the outcome distribution for tranche 1. So this is not the difference in mean performance for each teacher, but the difference in performance across the distribution. We do this for k = 10, 25, 50, 75 and 90. We regress these outcome differences against eligibility and the other variables as in the previous Table. The results in Table 12 simply report the eligibility coefficient. There is no clear overall pattern in this for GCSE, but for VA the coefficient is considerably higher (by about a third) at the bottom two points than higher up. This suggests that the incentive

[10] If we allow the errors to be clustered at school level, the results carry through – the standard error for eligibility status in the GCSE regression rises from 0.437 to 0.440 with clustering against a coefficient of 1.339, and in the VA regression it rises from 0.180 to 0.205 with a coefficient of 0.653.

scheme had greater impact on raising scores among low achievers, possibly because teachers concentrated their efforts where they thought easier gains were to be made. While this may be related to ceiling effects (see next section), the fact that it appears in the bottom quarter suggests there is a real effect too.

(d) Robustness checks

We use the novice teacher dummy to capture the experience profile and identify the effect of the scheme. To check our degree of reliance on the novice dummy, we instead drop all novices from the estimation and re-estimate. In this case, identification comes from a comparison of eligible and non-eligible teachers with a similar level of experience. The first two columns of Table 13 show that this makes a marginal difference to our results: the estimated impacts of eligibility are in fact marginally higher at 1.389 for GCSE and is 0.786 for VA, compared to 1.339 and 0.653 in Table 11[11]. This is reassuring that the use of the novice teacher dummy is not too restrictive.

A second problem may be the presence of ceiling effects in test scores. Both in GCSE and more particularly VA, there is an upper limit to the grade a student can be awarded, and therefore to the progress that they can be measured to make in our measure of VA. This problem will differ between classes depending on ability distribution, and so might affect the score gain that we attribute to the scheme. One simple way to deal with this is to consider the bottom 75% of the pupil distribution only (the bottom 75% of the initial KS3 distribution), and we report the results in columns 3 and 4 of Table 13. They show similar coefficients on eligibility status, slightly lower than in Table 11.

[11] Note that the interaction with English is now significantly negative.

(e) Gaming

The obvious way that a school could try to help its teachers succeed would be to assign them classes in which they could more easily demonstrate above-average pupil progress. In fact, the timing of the start of the school year and the deadline for the submission of applications mean that this is very unlikely. Applications for the initial Threshold, submitted in July 2000, were largely based on performance data from classes allocated prior to the announcement of the PRP scheme, so differential allocation of classes between eligible and non-eligible teachers in our 'before' data does is not a credible gaming strategy. Also, our data suggest little change in class assignment between the two tranches – the mean KS3 average of pupils taught by eligible teachers rose from 5.167 in tranche 1 to 5.290 in tranche (a difference of 0.123), and from 4.989 to 5.191 for ineligibles (0.202). These are both small changes, and the difference between them is only around 1.5% of the tranche 1 scores. There is no support here for systematic changes in class assignment. There is some evidence that Headteachers did help with filling in the paperwork (Wragg et al 2001), but this will have no effect on pupil test scores in national exams.

(f) Evaluation

We evaluate the size of the coefficient on eligibility status in two ways. Both GCSE and VA changes are in the same metric of GCSE points (the latter is just a residual of the former), and one point is the difference between a grade (an 'A' and a 'B' for example). As we have seen above (Tables 5 and 6), one standard deviation in the teacher-mean change in GCSE is 1.29 and for VA is 0.58. We can compare these to the coefficients on eligibility of 0.890 for GCSE change (Table 9, column 4) and 0.422 for

VA change (Table 10, column 4); as percentages of a standard deviation these are 69% and 73% respectively. In terms of straightforward grades, the estimates suggest that the scheme added on average almost half a grade of VA per child for eligible teachers. These are substantial effects in high-stakes exams: GCSEs are the gateway exams into higher education. It makes no sense to incorporate these results into a cost-benefit analysis of the scheme, since we only capture the incentivisation aspect here and not the recruitment and retention aspect.

An alternative way of thinking about the impact of the scheme is to note that the eligibility dummy is about 67% of the size of the novice teacher dummy for GCSE change, and 78% for VA change. Thus the incentive scheme can be thought of as eliciting extra effort equivalent to about three-quarters of the effect of young teachers moving up the learning curve.

It would be of interest to distinguish between those among the eligible who passed the Threshold, and so reached the UPS, and those who did not. In practice, this is not possible. As we noted above, nationally 97% of the eligible passed the Threshold, so a distinction between the groups is largely impossible. In our dataset, information on whether a teacher passed is missing for some teachers, so we are unable to confirm this figure for our sample. It seems very likely that almost all of these in fact passed the Threshold, and hence we cannot distinguish between those passing and those not.

7 Conclusion

This paper evaluates the impact of a performance-related pay system for teachers in England. Using teacher level data, matched with pupil test scores and value-added, we test whether the introduction of a payment based on pupil attainment increased teacher effort. Our evaluation design controls for pupil effects, school effects and teacher effects, and adopts a difference-in-difference methodology.

We find that the scheme did improve test score gains, on average by about half a grade per pupil. We also found heterogeneity across subject teachers, with maths teachers showing no improvement. A caveat is the necessity, given our data, to define the experience-effectiveness profile in quite a parametric way. Nevertheless, our results add to the very small literature on individual teacher-based performance pays schemes, evaluated in the context of a robust research design. The results show that teachers do respond to direct financial incentives. In an incentive scheme strongly based on pupil progress, test scores improved. Whether this represented extra effort or effort diverted from other professional activities is not something we can determine in our dataset. But our results suggest that teacher-based performance pay is a policy tool that education authorities should consider as part of their drive to raise educational performance.

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Table 1: Data Requested

Information	Level
Class lists for year 10 in 1997/8 and year 11 in 1998/9, with pupil identifiers and teacher identifiers	pupil
Class lists for year 10 in 2000/1 and year 11 in 2001/2, with pupil identifiers and teacher identifiers	pupil
Pupil test/exam scores for Key Stage 3 in 1996/7 and GCSE 1998/9, for all English, maths and science subjects, with pupil identifiers	pupil
Pupil test/exam scores for Key Stage 3 in 1999/00 and GCSE 2001/02, for all English, maths and science subjects, with pupil identifiers	pupil
Supplementary information for each pupil: date of birth, gender, postcode. With pupil identifier	pupil
Teachers characteristics at 1 September 1999: age, gender, salary, experience, spine point, whether applied for PT. With teacher identifier	teacher
Information about school policy: exam boards used, streaming/setting policy, pre-existing performance management system	school

Table 2: Summary teacher statistics

	Means			Missing data	Missing Eligibility Info
	All	Eligible	Ineligible		
Teacher Characteristics					
Age	41.9 (80)	44.8 (61)	32.1 (18)	102	1
Female dummy	0.57 (182)	0.59 (145)	0.47 (36)	0	1
Years at School	10.6 (182)	13.2 (145)	5.44 (36)	0	1
Spine point	8.59 (181)	9 (145)	7.26 (35)	1	1
Leadership Scale	0.02 (182)	0 (145)	0.11 (36)	0	1
Eligibility	0.8 (181)	1 (145)	0 (36)	0	1
Mean KS3	5.15 (182)	5.24 (145)	5.08 (36)	0	1
Mean GCSE	4.65 (182)	4.81 (145)	4.65 (36)	0	1
Mean Value added	-0.09 (182)	-0.03 (145)	-0.02 (36)	0	1

1. Number of teachers with the information in brackets
2. Unweighted

Table 3: Summary pupil statistics

	Tranche 1	Tranche 2
Pupil Characteristics		
Gender	0.53 (10777)	0.50 (12170)
KS3	5.29 (10611)	5.34 (11902)
English	5.12 (2706)	5.13 (2865)
Science	5.34 (5515)	5.53 (6418)
Maths	5.39 (2386)	5.53 (2619)
GCSE	4.88 (10717)	5.02 (11559)
English	5.04 (2805)	5.16 (2879)
Science	4.92 (5493)	5.11 (6044)
Maths	4.62 (2415)	4.65 (2636)
Value added	-0.04 (9830)	0.03 (11140)
English	0.33 (2593)	0.39 (2786)
Science	-0.07 (4970)	0.09 (5809)
Maths	-0.38 (2263)	-0.49 (2545)

1. Number of pupils with the information in brackets
2. Unweighted
3. VA is the residual from the pupil level regression of GCSE against KS3 and a female dummy

Table 4: Comparative Summary statistics for National and Estimation data sets

	National			Estimation		
	Tranche 1	Tranche 2	Difference	Tranche 1	Tranche 2	Difference
KS3						
English	4.66 (0.0022)	4.82 (0.0021)	0.16 (0.0031)	5.12	5.13	0.01
Science	4.93 (0.0017)	4.88 (0.0018)	-0.05 (0.0024)	5.39	5.53	0.14
Maths	5.02 (0.0019)	5.24 (0.0019)	0.22 (0.0027)	5.33	5.36	0.03
Overall	4.87 (0.0013)	4.99 (0.0011)	0.12 (0.0016)	5.29	5.34	0.05
GCSE						
English	4.82 (0.0024)	4.81 (0.0022)	-0.01 (0.0003)	5.04	5.16	0.12
Science	4.32 (0.0026)	4.21 (0.0024)	-0.11 (0.0035)	4.92	5.11	0.19
Maths	4.13 (0.0027)	4.21 (0.0025)	0.08 (0.0037)	4.62	4.65	0.03
Overall	4.42 (0.0015)	4.41 (0.0014)	-0.01 (0.0020)	4.88	5.02	0.14
Value added						
English	0.553 (0.0017)	0.504 (0.0016)	-0.049 (0.002)	0.33	0.39	0.06
Science	-0.132 (0.0017)	-0.074 (0.0015)	0.058 (0.002)	-0.07	0.09	0.16
Maths	-0.425 (0.0016)	-0.427 (0.0014)	-0.002 (0.002)	-0.38	-0.49	-0.11
Overall	-0.002 (0.0010)	0.002 (0.0009)	0.004 (0.0014)	-0.04	0.03	0.07

Source: Department for Education and Skills
Authors' calculations using Pupil-level Annual Schools Census
Standard deviations for national data in parentheses

Table 5: GCSE scores: overall and by subject

Overall

Tranche	Sample Size	Mean	Standard Deviation	Min	Percentiles 25th	50th	75th	Max	Output Measure
1	182	4.82	1.33	1.08	3.92	4.90	5.62	7.74	GCSE
2	182	5.02	1.23	1.62	4.18	5.04	5.85	7.79	GCSE
Difference	182	**0.19**	1.29	-3.62	-0.57	0.10	0.77	4.05	GCSE
English									
1	58	4.93	1.26	1.42	4.10	4.95	5.86	7.74	GCSE
2	58	5.13	1.28	2.00	4.50	5.22	5.89	7.65	GCSE
Difference	58	**0.20**	1.25	-2.54	-0.64	0.07	0.77	4.05	GCSE
Science									
1	74	4.91	1.26	1.08	4.31	4.96	5.58	7.39	GCSE
2	74	5.11	1.11	2.89	4.48	4.97	5.90	7.33	GCSE
Difference	74	**0.20**	1.18	-2.37	-0.33	0.10	0.36	3.68	GCSE
Maths									
1	50	4.51	1.53	1.33	3.41	4.59	5.88	7.63	GCSE
2	50	4.68	1.40	1.62	3.47	4.73	5.73	7.79	GCSE
Difference	50	**0.17**	1.58	-3.62	-0.84	0.10	1.36	3.70	GCSE

Unit is a teacher
Means are weighted by number of pupils per teacher, averaged over two tranches

Table 6: Value Added Scores: overall and by subject

Tranche	Sample Size	Mean	Standard Deviation	Min	25th	50th	75th	Max	Output Measure
Overall									
1	182	-0.07	0.55	-1.93	-0.45	-0.10	0.34	1.38	Value added
2	182	0.05	0.56	-1.57	-0.35	-0.01	0.51	1.50	Value added
Difference	182	**0.12**	0.58	-1.58	-0.18	0.09	0.42	2.03	Value added
English									
1	58	0.29	0.54	-1.93	0.02	0.34	0.69	1.38	Value added
2	58	0.40	0.51	-0.67	0.04	0.49	0.72	1.50	Value added
Difference	58	**0.11**	0.71	-1.13	-0.49	0.15	0.56	2.03	Value added
Science									
1	74	-0.10	0.45	-1.29	-0.43	-0.01	0.10	0.87	Value added
2	74	0.11	0.45	-0.83	-0.29	0.06	0.51	0.90	Value added
Difference	74	**0.21**	0.44	-0.88	-0.05	0.16	0.38	1.43	Value added
Maths									
1	50	-0.41	0.53	-1.42	-0.82	-0.43	-0.07	0.75	Value added
2	50	-0.49	0.44	-1.57	-0.73	-0.52	-0.23	0.59	Value added
Difference	50	**-0.08**	0.64	-1.58	-0.50	0.02	0.32	1.92	Value added

Percentiles: 25th, 50th, 75th.

Unit is a teacher

Means are weighted by number of pupils per teacher, averaged over two tranches

Table 7: Overall Weighted GCSE Means for Eligible and Not Eligible Teachers

Overall

Tranche	Eligible	Sample Size	Mean	Standard Deviation	Min	Percentiles 25th	50th	75th	Max	Output Measure
1	Yes	145	4.84	1.30	1.33	3.99	4.90	5.58	7.74	GCSE
2	Yes	145	5.05	1.28	1.62	4.12	5.12	5.90	7.79	
Difference			**0.21**	1.24	-3.62	-0.29	0.16	0.75	4.05	
1	No	36	4.78	1.50	1.08	3.81	5.26	5.71	6.90	GCSE
2	No	36	4.91	1.05	2.33	4.30	4.71	5.73	7.08	
Difference			**0.13**	1.53	-1.91	-0.88	-0.64	1.23	3.70	
Difference in Difference			**0.08 (0.17)**			**0.59 (0.10)**	**0.81 (0.11)**	**-0.47 (0.38)**		

1. standard error in brackets

Table 7a: Weighted GCSE Means for Eligible and Not Eligible English Teachers

English

Tranche	Eligible	Sample Size	Mean	Standard Deviation	Min	Percentiles 25th	50th	75th	Max	Output Measure
1	Yes	47	4.91	1.28	1.42	4.10	4.95	5.54	7.74	GCSE
2	Yes	47	5.10	1.34	2.00	4.12	5.22	5.93	7.65	
Difference			**0.19**	1.22	-2.54	-0.34	0.12	0.77	4.05	
1	No	11	5.01	1.25	3.00	4.58	5.52	6.04	6.39	GCSE
2	No	11	5.25	1.00	3.21	4.71	5.38	5.88	7.08	
Difference			**0.24**	1.39	-1.21	-0.64	0.06	1.30	2.80	
Difference in Difference			**-0.05 (0.30)**							

1. standard error in brackets

Table 7b: Weighted GCSE Means for Eligible and Not Eligible Science Teachers

Science

Tranche	Eligible	Sample Size	Mean	Standard Deviation	Percentiles Min	25th	50th	75th	Max	Output Measure
1	Yes	59	4.91	1.22	1.92	4.31	4.96	5.43	7.39	GCSE
2	Yes	59	5.19	1.11	2.89	4.64	5.15	5.90	7.33	
Difference			**0.28**	1.09	-2.37	-0.17	0.18	0.36	3.28	
1	No	14	4.91	1.51	1.08	4.36	5.26	5.62	6.90	GCSE
2	No	14	4.77	1.20	3.14	3.90	4.48	4.92	6.95	
Difference			**-0.14**	1.54	-1.91	-0.88	-0.78	0.05	3.68	

Difference in Difference	**0.42** **(0.25)**

1. standard error in brackets

Table 7c: Weighted GCSE Means for Eligible and Not Eligible Maths Teachers

Maths

Tranche	Eligible	Sample Size	Mean	Standard Deviation	Percentiles Min	25th	50th	75th	Max	Output Measure
1	Yes	39	4.57	1.49	1.33	3.45	4.59	5.88	7.63	GCSE
2	Yes	39	4.63	1.50	1.62	3.40	4.73	5.96	7.79	
Difference			**0.06**	1.57	-3.62	-0.84	0.15	1.36	2.69	
1	No	11	4.27	1.73	2.12	2.33	3.88	5.71	6.79	GCSE
2	No	11	4.85	0.99	2.33	4.20	4.67	5.73	6.04	
Difference			**0.58**	1.65	-1.72	-0.65	0.00	1.85	3.70	

Difference in Difference	**-0.52** **(0.39)**

1. standard error in brackets

Table 8: Overall weighted Value Added Means for Eligible and Not Eligible Teachers

Overall

Tranche	Eligible	Sample Size	Mean	Standard Deviation	Min	Percentiles 25th	50th	75th	Max	Output Measure
1	Yes	145	-0.08	0.54	-1.93	-0.46	-0.11	0.30	1.38	Value added
2	Yes	145	0.05	0.57	-1.57	-0.35	0.05	0.51	1.50	
Difference			**0.13**	0.56	-1.58	-0.13	0.16	0.42	2.03	
1	No	36	-0.04	0.62	-1.42	-0.39	-0.07	0.49	1.29	Value added
2	No	36	0.06	0.53	-0.89	-0.31	-0.07	0.59	1.25	
Difference			**0.10**	0.66	-1.12	-0.40	0.05	0.43	1.92	
Difference in Difference			**0.03 (0.08)**			**0.28 (0.10)**	**0.11 (0.11)**	**-0.01 (0.12)**		

1. standard error in brackets

Table 8a : Weighted Value Added Means for Eligible and Not Eligible English Teachers

English

Tranche	Eligible	Sample Size	Mean	Standard Deviation	Min	Percentiles 25th	50th	75th	Max	Output Measure
1	Yes	47	0.27	0.53	-1.93	-0.02	0.34	0.57	1.38	Value added
2	Yes	47	0.39	0.51	-0.67	0.04	0.46	0.70	1.50	
Difference			**0.12**	0.69	-1.13	-0.31	0.16	0.55	2.03	
1	No	11	0.39	0.59	-0.64	-0.01	0.54	0.78	1.29	Value added
2	No	11	0.48	0.54	-0.31	-0.21	0.71	0.89	1.25	
Difference			**0.09**	0.83	-1.12	-0.57	-0.16	1.13	1.18	
Difference in Difference			**0.03 (0.17)**							

1. standard error in brackets

Table 8b : Weighted Value Added Means for Eligible and Not Eligible Science Teachers

Science

Tranche	Eligible	Sample Size	Mean	Standard Deviation	Min	Percentiles 25th	50th	75th	Max	Output Measure
1	Yes	59	-0.11	0.46	-1.29	-0.52	-0.13	0.10	0.87	Value added
2	Yes	59	0.14	0.45	-0.83	-0.26	0.18	0.51	0.90	
Difference			**0.25**	0.40	-0.82	-0.03	0.19	0.38	1.12	
1	No	14	-0.04	0.45	-1.03	-0.16	-0.09	0.10	0.82	Value added
2	No	14	0.01	0.44	-0.38	-0.38	-0.07	0.40	0.87	
Difference			**0.05**	0.60	-0.88	-0.32	-0.18	0.25	1.43	

Difference in Difference	**0.20 (0.09)***

1. standard error in brackets
2. * significant at the 5% level.

Table 8c : Weighted Value Added Means for Eligible and Not Eligible Maths Teachers

Maths

Tranche	Eligible	Sample Size	Mean	Standard Deviation	Min	Percentiles 25th	50th	75th	Max	Output Measure
1	Yes	39	-0.39	0.50	-1.39	-0.75	-0.34	-0.07	0.75	Value added
2	Yes	39	-0.54	0.43	-1.57	-0.76	-0.61	-0.30	0.38	
Difference			**-0.15**	0.62	-1.58	-0.64	-0.02	0.32	0.89	
1	No	11	-0.48	0.67	-1.42	-0.92	-0.61	-0.07	0.62	Value added
2	No	11	-0.28	0.43	-0.89	-0.64	-0.26	-0.04	0.59	
Difference			**0.20**	0.63	-0.70	0.03	0.14	0.23	1.92	

Difference in Difference	**-0.35 (0.15)***

1. standard error in brackets
2. * significant at the 5% level.

Table 9: GCSE Analysis

Unit of observation is a teacher
Dependent variable is teacher change in GCSE score

	(1)	(2)	(3)	(4)
Eligible		0.706	0.286	0.890
		(0.371)	(0.298)	(0.366)*
Novice				1.333
				(0.489)**
Spine point	-0.092	-0.181		
	(0.065)	(0.080)*		
Years in School	-0.001	-0.114	-0.120	-0.008
	(0.015)	(0.251)	(0.254)	(0.015)
Leadership	0.831	2.144	0.277	0.873
	(0.927)	(1.150)	(0.789)	(0.803)
Female teacher	0.062	-0.020	0.096	-0.003
	(0.220)	(0.223)	(0.222)	(0.220)
Maths dummy	-0.174	-0.191	-0.132	-0.135
	(0.250)	(0.248)	(0.251)	(0.246)
English dummy	-0.129	-0.114	-0.120	-0.121
	(0.253)	(0.251)	(0.254)	(0.249)
Constant	0.433	0.685	-0.532	-1.096
	(0.659)	(0.667)	(0.451)	(0.488)*
Observations	180	180	181	181
R-squared	0.15	0.17	0.14	0.18

1. Standard errors in parentheses
2. * significant at 5%; ** significant at 1%
3. Note that in the specifications involving the spine point variable we only have 180 observations, as one teacher has no spine point data.
4. Weighted by number of pupils per teacher, averaged over two tranches
5. All specifications have school dummies

Table 10: Value Added Analysis

Unit of observation is a teacher
Dependent variable is teacher change in VA score

	(1)	(2)	(3)	(4)
Eligible		0.193	0.063	0.422
		(0.173)	(0.128)	(0.152)**
Novice				0.539
				(0.203)**
Spine point	-0.022	-0.057		
	(0.027)	(0.033)		
Years in School	-0.003	-0.005	-0.006	-0.005
	(0.006)	(0.006)	(0.006)	(0.006)
Leadership	0.045	0.556	-0.005	0.236
	(0.381)	(0.473)	(0.327)	(0.333)
Female teacher	-0.014	-0.046	-0.001	-0.040
	(0.091)	(0.092)	(0.092)	(0.091)
English dummy	-0.145	-0.139	-0.134	-0.134
	(0.104)	(0.103)	(0.105)	(0.103)
Maths dummy	-0.337	-0.344	-0.315	-0.317
	(0.103)**	(0.102)**	(0.104)**	(0.102)**
Constant	0.308	0.407	-0.021	-0.249
	(0.271)	(0.275)	(0.187)	(0.202)
Observations	180	180	181	181
R-squared	0.27	0.29	0.26	0.29

1. Standard errors in parentheses
2. * significant at 5%; ** significant at 1%
3. Note that in the specifications involving the spine point variable we only have 180 observations, as one teacher has no spine point data.
4. Weighted by number of pupils per teacher, averaged over two tranches
5. All specifications have school dummies

Table 11: Subject Differences

Unit of observation is a teacher

Dependent variable is teacher change in output score

	GCSE	Value added
Eligible	1.339	0.653
	(0.437)**	(0.180)**
Novice	1.313	0.511
	(0.480)**	(0.202)*
Interaction: Eligible* English	-0.771	-0.394
	(0.667)	(0.275)
Interaction: Eligible* Maths	-1.138	-0.588
	(0.625)	(0.257)*
Years in School	-0.010	-0.006
	(0.015)	(0.006)
Leadership	0.531	0.062
	(0.862)	(0.355)
Female Teacher	-0.059	-0.070
	(0.223)	(0.092)
English dummy	0.554	0.211
	(0.612)	(0.252)
Maths dummy	0.791	0.162
	(0.563)	(0.232)
Constant	-1.468	-0.441
	(0.524)**	(0.216)*
Observations	181	181
R-squared	0.20	0.31

1. Standard errors in parentheses
2. * significant at 5%; ** significant at 1%
3. Note that in the specifications involving the spine point variable we only have 180 observations, as one teacher has no spine point data.
4. Weighted by number of pupils per teacher, averaged over two tranches
5. All specifications have school dummies

Table 12: Distributional Impacts

Unit of observation is a teacher

Dependent variable is: (kth percentile of outcome distribution for tranche 2 for teacher j) – (kth percentile of outcome distribution for tranche 1 for teacher j)

Coefficient on Eligibility:

Percentile	GCSE	Value added
10th	1.145	0.849
	(0.516)*	(0.246)**
25th	1.394	0.856
	(0.465)**	(0.220)**
50th	1.394	0.689
	(0.465)**	(0.224)**
75th	1.753	0.562
	(0.499)**	(0.216)*
90th	1.308	0.637
	(0.522)*	(0.294)*

1. Standard errors in parentheses
2. * significant at 5%; ** significant at 1%
3. Weighted by number of pupils per teacher, averaged over 2 tranches
4. All specifications have school dummies
5. Each cell reports coefficient from a different regression
6. Other variables included as in Table 11
7. All regressions have 181 observations

Table 13: Robustness checks

Unit of observation is a teacher

Dependent variable is teacher change in output score

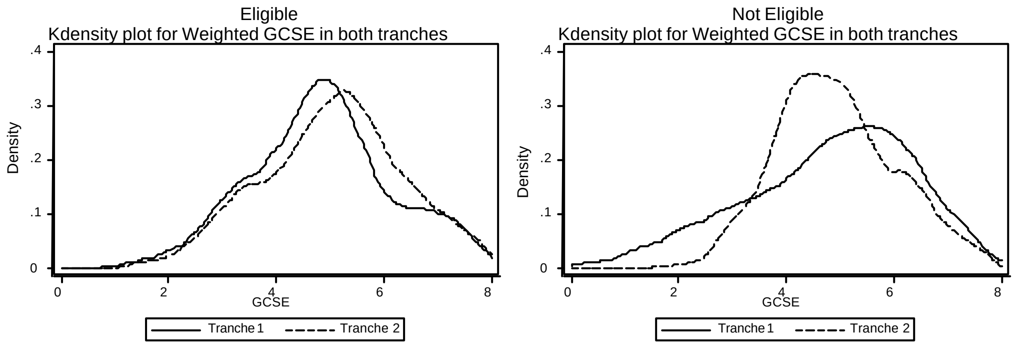
	No Novices		Lower 75% KS3 only	
	GCSE	VA	GCSE	VA
Eligible	1.389	0.786	0.996	0.605
	(0.487)**	(0.204)**	(0.421)*	(0.191)**
Novice			1.099	0.511
			(0.447)*	(0.203)*
Interaction: Eligible* English	-1.980	-0.962	-0.560	-0.328
	(0.922)*	(0.386)*	(0.606)	(0.276)
Interaction: Eligible* Maths	-0.877	-0.772	-0.955	-0.559
	(0.824)	(0.345)*	(0.579)	(0.264)*
Years in School	-0.017	-0.007	-0.018	-0.010
	(0.014)	(0.006)	(0.015)	(0.007)
Leadership	-0.329	-0.267	0.381	0.139
	(0.927)	(0.388)	(0.813)	(0.370)
Female teacher	0.166	-0.059	0.041	-0.002
	(0.233)	(0.097)	(0.209)	(0.095)
English dummy	1.691	0.791	0.274	0.100
	(0.888)	(0.372)*	(0.553)	(0.251)
Maths dummy	0.568	0.365	0.560	0.062
	(0.778)	(0.325)	(0.517)	(0.235)
Constant	-1.497	-0.569	-0.924	-0.301
	(0.560)**	(0.234)*	(0.507)	(0.231)
Observations	164	164	178	178
R-squared	0.20	0.32	0.17	0.32

1. Standard errors in parentheses
2. * significant at 5%; ** significant at 1%
3. Weighted by number of pupils per teacher, averaged over 2 tranches
4. All specifications have school dummies

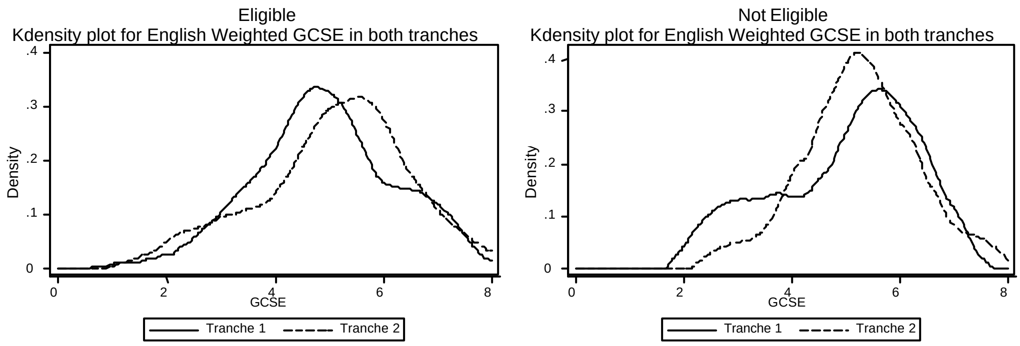
Figure 1: Distribution of teacher outcomes: GCSE

Kernel de nsity estimates over teacher mean scores

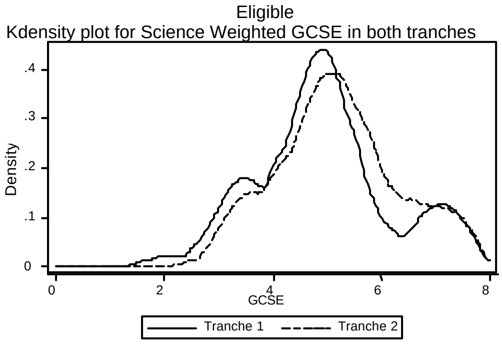
Overall

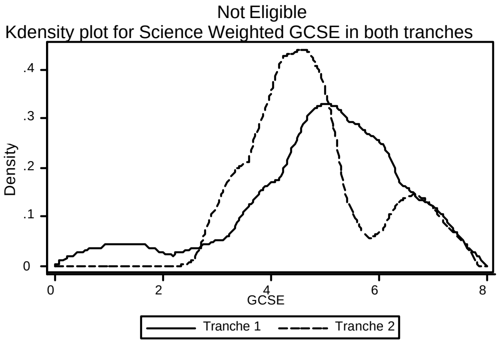


English

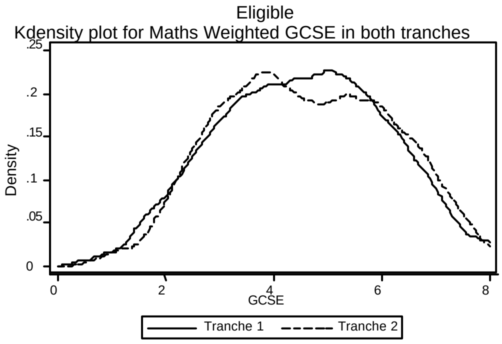


Science





Maths



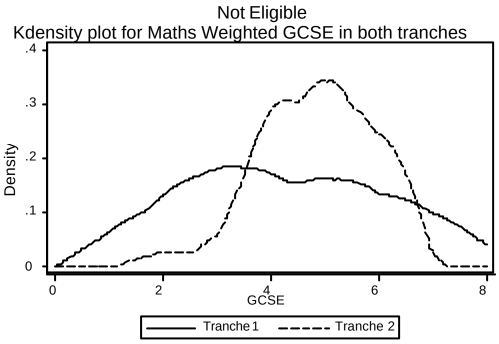
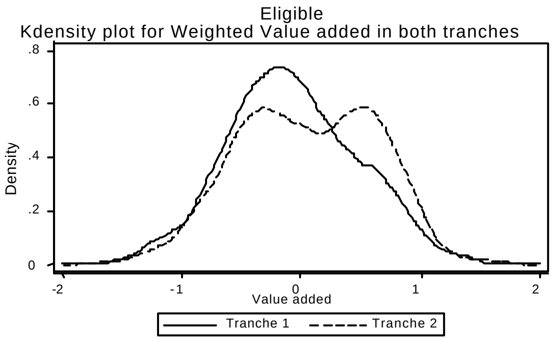
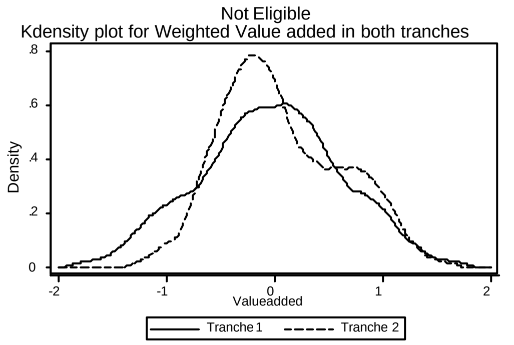


Figure 2: Distribution of teacher outcomes: Value Added

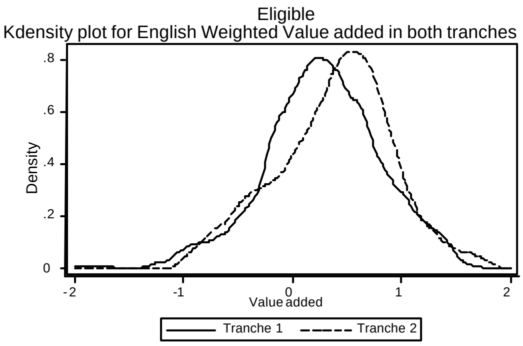
Kernel density estimates over teacher mean scores

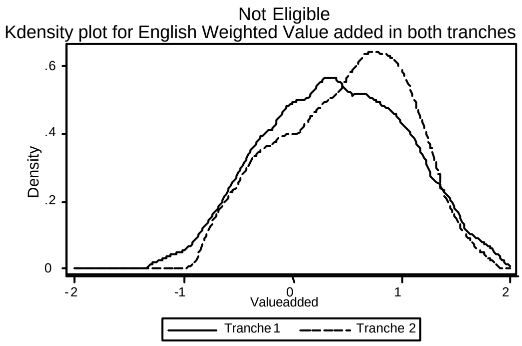
Overall



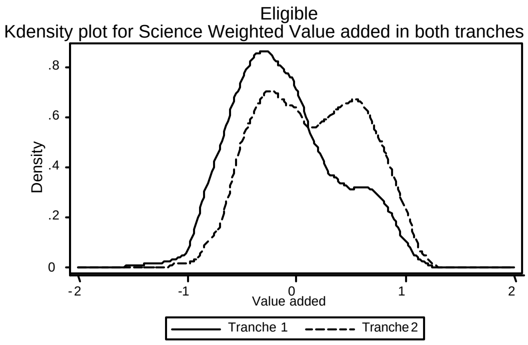


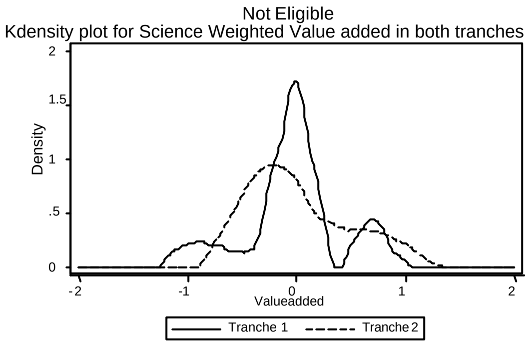
English





Science





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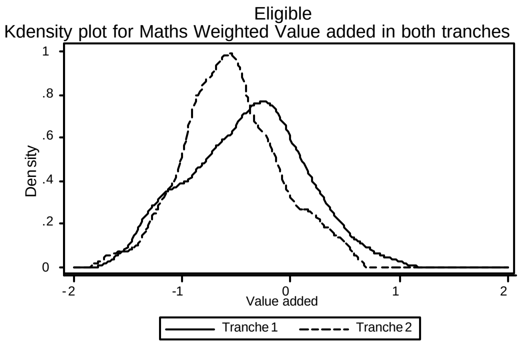

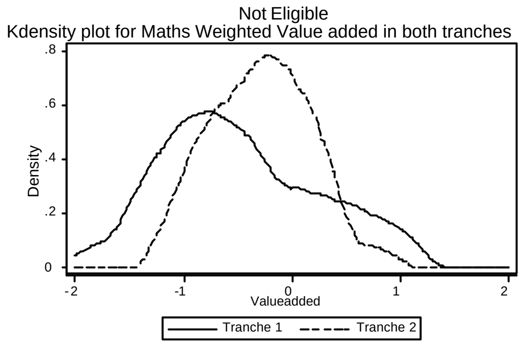

Figure 3: Comparing tranche 1 and tranche 2

Unit is a teacher. The line is 45°

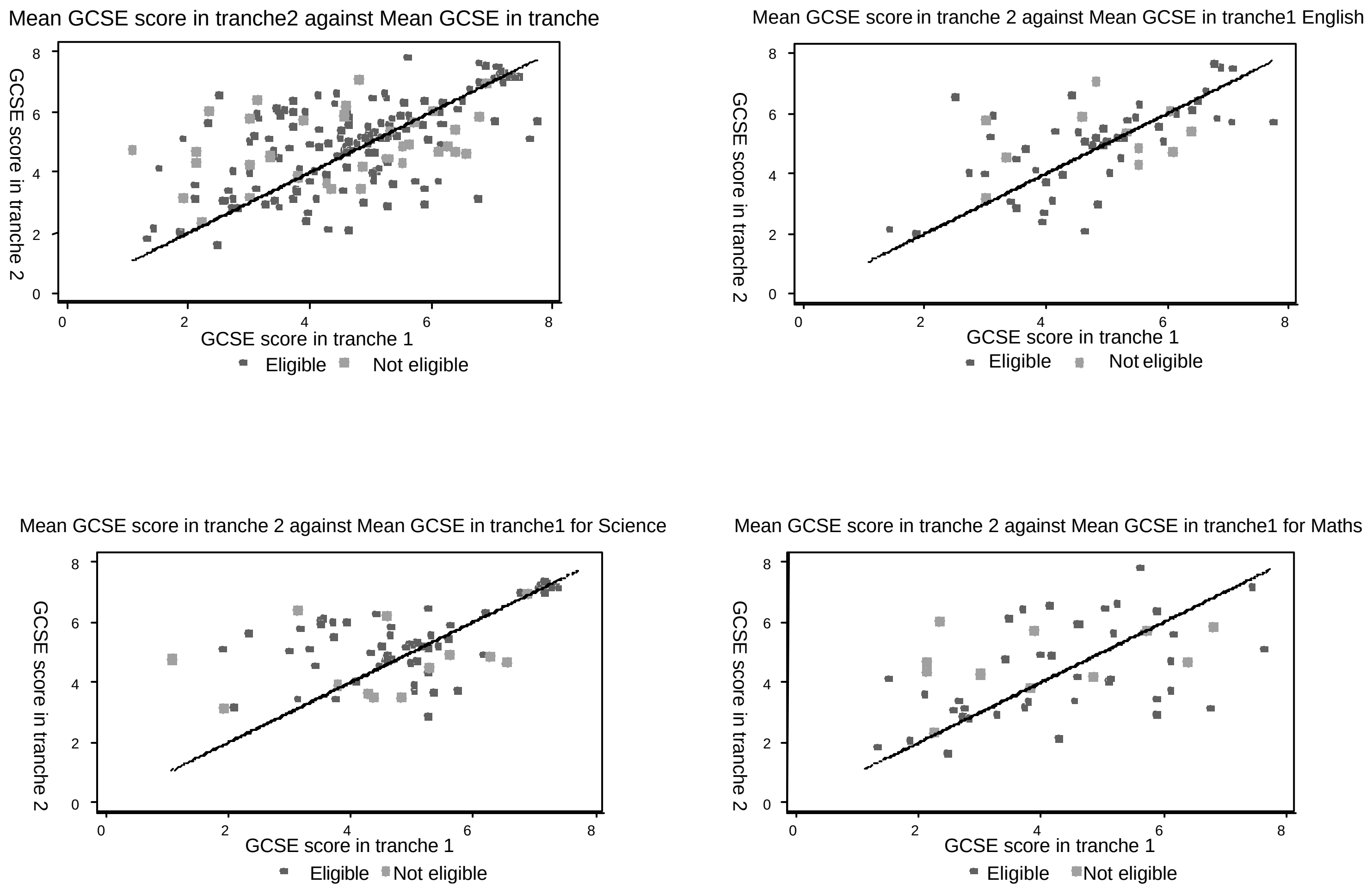

Figure 4: Comparing tranche 1 and tranche 2

Unit is a teacher. The line is 45°

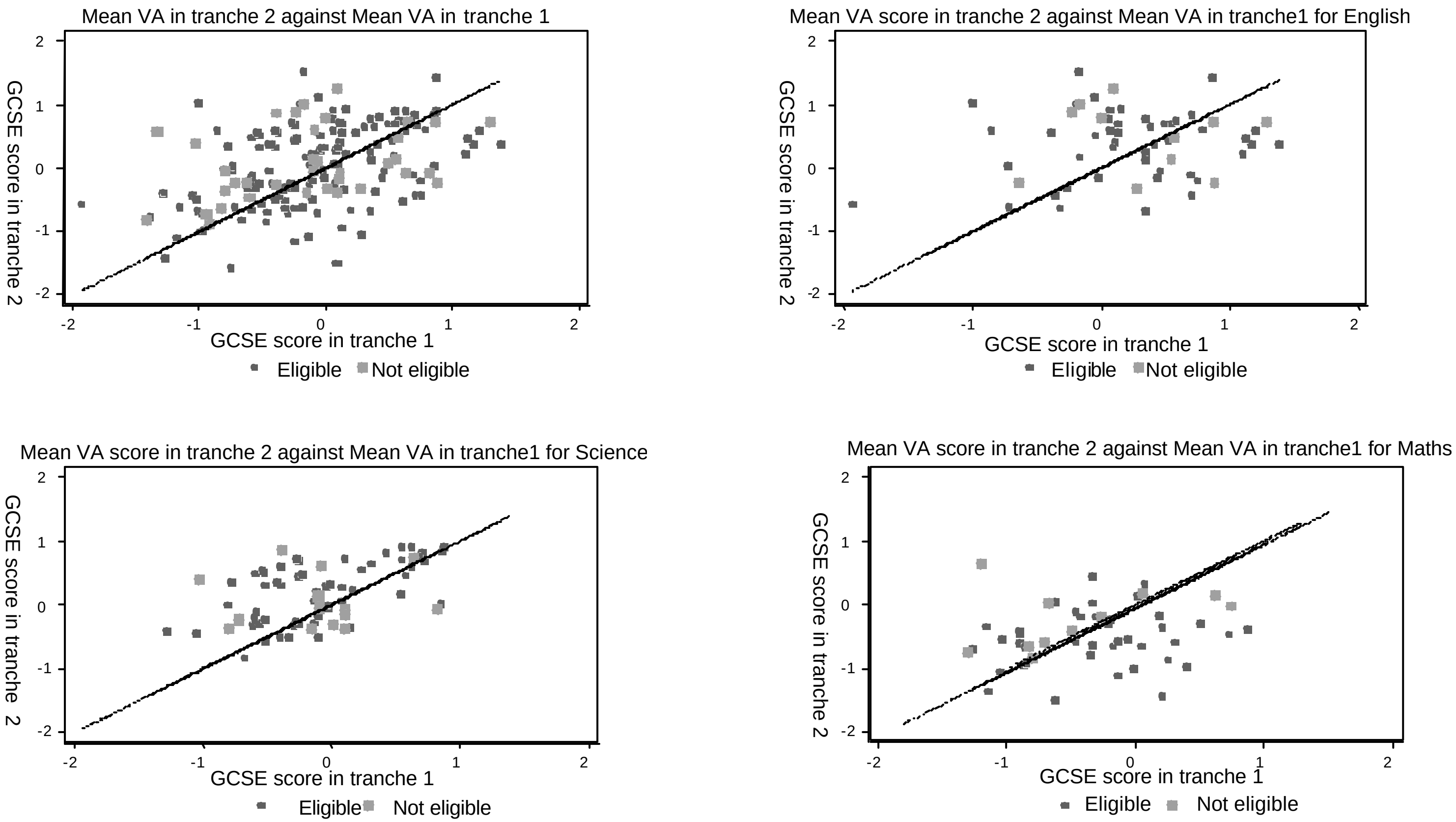

Figure 5: Teacher outcome experience profile: GCSE
Unit is teacher, data from tranche 1

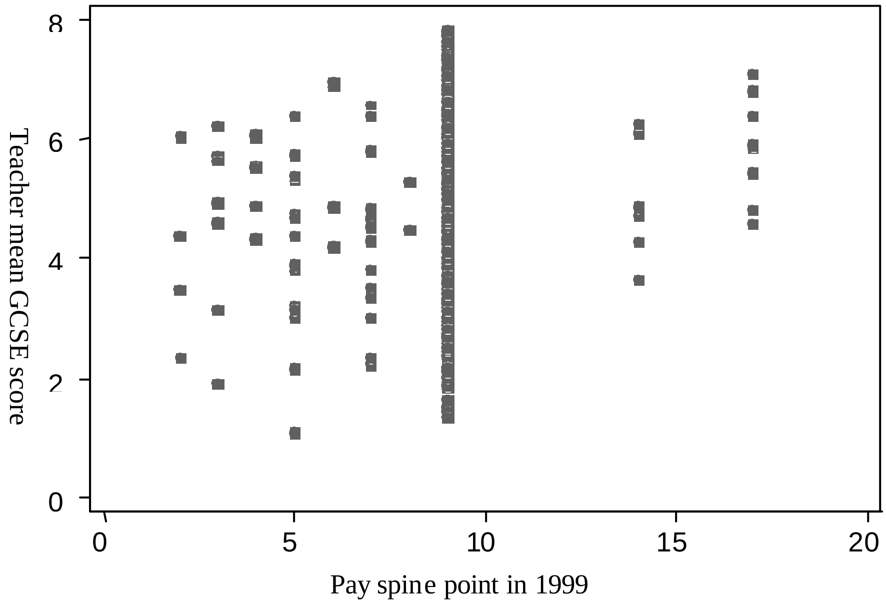

Figure 6: Teacher outcome experience profile: Value added
Unit is teacher, data from tranche 1

Appendix 1: Data collection

Schools were approached in several waves, between June 2000 and January 2002. The first wave comprised a pilot group of 40 schools, chosen from a group of about 200 schools who had previously participated in a Department for Education and Skills (DfES, then DfEE) pilot value-added exercise (O'Donogue et al 1997). These schools were therefore known to be research aware and to have good information systems. We selected the 40 schools as a convenience sample of the schools closest to Bristol. The second wave comprised the remaining schools from the DfEE pilot group. Third and subsequent waves comprised schools in particular groups of LEAs, chosen to reflect a balance of school types and urban/rural features.

Schools were approached by being sent a letter inviting them to participate, as well as a project outline and a stamped-addressed postcard to return indicating whether they were / were not willing to participate, or wanted more information before taking a decision. Their addresses were obtained from the DfES Performance Tables.

Schools in the first wave were all telephoned after the initial, letter-based approach. Schools in the first and second waves who did not respond to the initial approach were sent a follow-up letter and subsequently telephoned. The phone calls were extremely time consuming (up to eight calls had to be made to each school to speak to the head, or to draw the issue to the head's attention), and only a small number of schools agreed to participate as a result. Intensive follow-up was therefore dropped from subsequent waves, in favour of more extensive sampling. Given the poor response rate from our initial waves and the expected high drop-out rate, we invited all state maintained secondary schools in England to take part.

After agreeing to participate, schools were sent a letter guaranteeing the confidentiality and security of the data and a schedule of the data required for Tranche 1 (1997 – 1999). To obtain additional supplementary information the headteachers of participating schools were interviewed or were asked to fill in a questionnaire (see Croxson and Atkinson (2001a, b) for more on these interviews).

Participating schools were then sent a second data schedule in the Autumn of 2002, which specified the data required for Tranche 2 (2000 - 2002). A copy of this schedule is shown in Table A1. 41 schools provided Tranche 1 data, of whom 24 also provided data for Tranche 2. Out of this number, the data supplied by 18 schools was sufficiently complete for our analyses.

Table A1: General schedule of data requirements for pupils taking GCSEs in Summer 2002

		Examples
Class lists for *Year 10: 2000/2001,* *Year 11: 2001/2002* *All Maths, English and Science subjects*	Names of pupils **linked to the appropriate teachers** for each of the 2 years (if classes remain the same across years do not duplicate the info) Subject **Setting** information (ie which group is highest ability, etc. if relevant)	**Class**: 10A (00/01) Maths; **Class**: 11G **Teachers**: Mr Smith & Ms Jones **Teachers:** A Rig **Pupils:** Jo Dean, Bob Aasive, **Pupils:** ***and so on for each class*** **Maths 10A High Achievers - 10G Strugglers** English, no setting Science S1 Double Science, S2 – 5 Single Science
Information about these pupils	• date of birth • gender; • postcode.	***for each pupil***
Test and exam information for these pupils	KS3 test scores for these pupils, in each of **Maths, English** and **Science** GCSE Results *and Exam Board* in each of **Maths, English** and **Science**	**Pupil** / **KS3 Science** / **GCSE Science (*AQA*)** Jo Dean 4 D Bob Aasive 4 C Tane Mujab 7 A* … ***and so on for each pupil***
Characteristics of the teachers who taught them	• date of birth; • gender; • subject(s) taught; • **length of time** at school at 1/9/01; • **total salary** at 1/9/00; **total salary** at 1/9/01; • **spine point** at 1/9/00; **spine point** at 1/9/01; • whether they were (a) **eligible** for; (b) **applied** for; (c) **awarded** Threshold Payment in 2001; • whether they were (a) **eligible** for; (b) **applied** for; (c) **awarded** Threshold Payment in 2002.	**Teacher** / **Date of birth** / **Gender** Ms Jones 09/07/66 F Mr Smith 08/04/61 M *and so on for each teacher*
Information about the school	• total number of teachers in the school; **full time and part time** at 1/9/00 AND 1/9/01; • number of teachers that were (a) **eligible** for; (b) **applied** for; (c) **awarded** a Threshold payment in 2001; • number of teachers that were (a) **eligible** for; (b) **applied** for; (c) **awarded** Threshold payment in 2002.	

Appendix Figure 1