Information and choice of A-level subjects

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Labour market knowledge and choice of school subject: A Single Blind Cluster Randomized Controlled Trial with linked administrative data

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Abstract
We estimated the effects of an intervention providing labour market information about the consequences of educational choices to 5,593 students in England, using a double blind cluster randomised controlled trial in 50 schools (registration:AEARCTR-0000468). Our primary outcome was students' actual choices of subjects at age 16. We also recorded the students' expectations of future wages and future intentions before and after the intervention, and linked their data into national administrative records. We found evidence students in the intervention arm were more likely to study Mathematics. This suggests providing accessible and credible information on labour market consequences of school choices may influence students' decisions.

JEL Classifications

A00, A21, A22, I00, I21, D80, D83, C93.

Keywords

Educational economics, human capital, wage differentials

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INTRODUCTION

Policymakers have expressed concern that recruitment to undergraduate subjects has inadequately responded to employers' demand or national interests (Roberts 2002, European Commission 2003, Browne 2010, HEFCE 2010). These policy statements have argued that relatively high graduate premia for science, technology, engineering and mathematics (STEM) subjects reflect excess demand for STEM graduates and that government interventions are required to boost recruitment to these subjects. Three types of government assistance have been used or actively considered in England: (i) subsidising the cost of undergraduate tuition in these subjects; (ii) requiring students to study these subjects in the final years of secondary schooling; and (iii) providing students with information about the labour market benefits of studying these subjects.

In this paper we concentrate on the 'information' strategy. In particular we examine whether the kind of information provided to school students in the context of formal lessons makes a difference to the subjects they choose in their final two years of schooling. Broad messages about the labour market rewards awaiting STEM graduates are passed on to school students in England through officially sanctioned web sites. In the first part of the paper we review this strategy in the light of evidence on heterogeneity in graduate premium in the UK and evidence from other countries about the effectiveness of providing information in changing students' higher education decisions. In the second part of the paper we report the effect of a randomised intervention which provided information about variation in graduate earnings by subject to school students. We focus on fifteen and sixteen year-old students in England. At this age, students make decisions about the 'advanced' (A-level) subjects to study between the ages of 16 and 18. The opportunity to enrol on a STEM degree at university is greatly affected by their choice of advanced subjects. We report effects of the intervention on students' expectations of graduate salaries, intentions towards and actual choices of subjects to study in their final years of schooling.

THE POLICY CONTEXT

Many OECD countries have implemented policies aimed to increase the proportion of students studying STEM subjects (e.g. Roberts 2002, European Commission 2003, DEST 2005). Government support in England has been provided through capital and recurrent grants towards teaching costs and the funding for a national higher education STEM programme to encourage students to choose STEM subjects[i]. Interventions to change the proportion of students studying different subjects have been justified on the basis of two kinds of market failure[ii]. First, it is suggested (e.g. Browne 2010, p.47) that graduates in 'strategically important' subjects generate greater social benefits than other subjects. Whilst an extensive literature (e.g. Moretti 2004, McMahon 2009) suggests substantial social benefits of education, there is limited and conflicting evidence for differential levels of social benefit according to degree subject (e.g. Bourne and Dass 2003, Winters 2013). In this paper we focus on a second argument that students are inadequately informed about the labour market rewards for graduates of STEM and modern foreign languages.

Policy makers have argued (e.g. Roberts 2002, HEFCE 2010) that STEM and language graduates earn high wages because supply has been unresponsive to employers' demands for these graduate skills. We might, therefore, expect to find that, compared with other graduates, STEM and language graduates experience lower levels of over-education and higher graduate premia.

Using employment data on individuals seven years after their graduation in 1995, Chevalier and Lindley (2009) found that STEM graduates were less likely to be over-educated than humanities graduates. However, they also found that graduates in languages were more likely than other graduates to be over-educated. There is now a growing body of evidence about the *associations* between subjects studied and future earnings[iii]. Mathematics is the only school subject which has been shown to be consistently associated with higher earnings in the UK (Dolton and Vignoles 2002) or in the US (Levine and Zimmerman 1995, Arcidiacono 2004, Rose and Betts 2004). Levine and Zimmerman (1995) found no effect on future wages for studying science at school. However, Johnes (2005) found that the association of a particular school subject with future earnings in the England depended on the other subjects with which it was combined.

In contrast, studies of variation in graduate premia in the UK have reported strong and persistent differences between subjects (Chevalier 2011, O'Leary and Sloane 2011, Walker and Zhu 2011). Graduates in mathematics, computing and engineering have relatively high wage premia whilst the premia for pure science graduates tend to be modest. Social science graduates, particularly in business, economics and management tend to have higher graduate premia than pure science graduates, whilst graduates in humanities and arts tend to have relatively lower wage premia. These differences are stronger for males than for females.

In summary, studies which controlled for students' characteristics suggest that claims about labour market demand and wages for STEM graduates found in UK policy reviews present too simple a picture. The graduate premia for pure science and modern foreign languages are modest compared to other subjects whilst the premia to mathematics and engineering are high. This evidence is consistent with 'skills match' data. Labour market evidence may indicate a case for the desirability of more mathematics and engineering graduates. It does not provide a case for more graduates in pure science or modern foreign languages. It is possible that total social benefits from higher education would be increased by a shift from non-STEM to STEM graduates, but this is more likely to be achieved through policies to increase the demand rather than the supply of STEM graduates.

PREDICTING THE EFFECT OF LABOUR MARKET INFORMATION ON SUBJECT CHOICE

A simple human capital model presents subject choice as an optimization problem given endowments, individual preferences and the dependence of future earnings on choice of subject (e.g. Boudarbat & Montmarquette 2009, Arcidiacono *et al.* 2012). This model has been extended to (i) include effects of subject difficulty on expected effort; (ii) separate out consumption preference effects (enjoyment from learning a subject) from investment effects; and (iii) take account of completeness of information and learning from experience (Zietz & Joshi 2005, Beffy *et al.* 2012, Altonji *et al.* 2013). Altonji *et al.*'s model predicts that additional labour market information will reduce recruitment to a subject if students discover that they have over-estimated future earnings relative to earnings for other subjects they could choose.

Despite extensions to address bounded rationality, the predictions of optimizing models may turn out to be inaccurate if students rely on heuristics to overcome the complexities of choosing subjects to study (Diamond & Vartiainen 2007). Students' expectations of future earnings need to account for the distributions of earnings around the mean graduate wage for each subject and also the probability that an individual will find themselves at any particular point on that distribution (Manski 2004). Two biases have been observed the expectations literature.

First, students in England tend to over-estimate their own future graduate earnings (Jerrim 2011). One possible explanation for this phenomenon is the 'big-fish-little pond effect' which suggests that students' academic self-concept is inversely related to the average achievement of their school peers (Marsh and Hau 2003). A consequence is that students are more likely to choose to study a subject if they believe they have a relative advantage in that subject. Stinebrickner and Stinebrickner (2013) provided evidence of the effect of subjective judgement of relative ability on dropout rates in the US. They found that as students realised that they were initially over-optimistic about their relative ability in the subject in science they switched to other subjects. A study of students' choice of A-level subjects in England (Davies *et al.* 2009a) found that choice of subject was more strongly associated with relative advantage than with gender or social background. Therefore, we would expect to find that the average student studying a subject at a higher level has a substantially stronger ability in that subject than the average student studying that subject at a level below. If students' expectations of success are based on their academic self-concept established in relation to their current peers then they will, on average, over-estimate their relative success at the higher level. This implies that, at the point they are choosing a subject, they will tend to believe they will earn above the average wage of graduates in that subject.

Second, students tend to believe that variation around the mean is the same for all subjects (Davies et al. 2013b). This simplifies the imagined world on which students base their choices. However, in reality O'Leary and Sloane (2011) found that in the period 2004-2006 the interquartile range for wages of graduates in social science and in business and finance was small, whereas for graduates in engineering and education the interquartile range was large. A combination of overestimation of (i) the variance in wages and (ii) their own likely position in the distribution of graduates' wages in their chosen subject are likely to weaken students' responsiveness to information about average and variability of graduate earnings in different subjects.

Behavioural economics has also drawn attention to 'lack of self-interest' in decision-making and this may matter if some subjects generate larger social benefits than others. Whilst optimization models (e.g. Beffy *et al.* 2012, D'Haultfœuille & Maurel 2013) have included non-pecuniary motives, they do not make a distinction between self-interest and altruistic motivation. Students who internalise social benefits through altruistic motivation are more likely to choose subjects with relatively high social benefits (Davies et al. 2013a). Alternatively, social benefits may be unintended outcomes of choosing subjects on the basis of expected earnings and non-pecuniary private benefits. In either case, students who are only weakly motivated by future earnings are less likely to change their choice of subject in the light of new information about graduate wages.

A small number of studies have begun to examine the effects on educational choices of providing students with labour market information through a randomized controlled trial. Jensen (2010) found that 14 year-old students in the Dominican Republic who were given information about graduate earnings were more likely to intend to go to university. McGuigan et al. (2012) found that 14-15 year-old students in London who accessed a web site providing information about graduate premia and employment improved the accuracy of their knowledge, but with little effect on their intentions to further study. Kerr et al. (2012) found that school students in Finland who were given information about variation in graduate earnings were less likely to apply for humanities courses in polytechnics and more likely to apply for social science or business and finance in polytechnics. Differences between the effects of these trials may be attributable to the form of the intervention (in class or online in students' own time) or the context (e.g. country) in which the intervention took place. One consideration here is the way in which students' choices are framed by guidance from the school (Adnett & Davies 2005, Davies et al. 2009b). Mediation by schools may affect the likelihood that information is brought to students' attention and schools may also choose to frame information in ways they believe are either in the interests of the students or the school.

This study differs from previous trials in several ways. First, we gathered data on: (i) students' expectations of average graduate wages by subject as well as their expectations for their own earnings; (ii) students' beliefs about the distribution of wages around the average; and (iii) the strength of different motivations in their choice of subject. Second, our intervention took place within normal lesson time and was explained using a structured and pre-planned lesson, in contrast to the web-based intervention reported by McGuigan *et al.* (2012). We hypothesised that providing students with information about graduate salaries would affect their choice of A-level subjects to study between the ages of 16 and 18.

METHODS

We registered the trial with the social science registry (AEARCTR-0000468) and the trial protocol is available on the project web site. In this section we explain our trial design before providing more specific information about the data collection and analysis.

Trial design

RCT designs vary in the way they address each of the following alternatives (Torgerson & Torgerson 2001, Lindsay 2004, Treweek & Zwarenstein 2009):

[1] Form of research question: 'Does the intervention work? Does the intervention work in normal practice? What difference does the intervention make compared with alternative *X*?

[2] Participants aware or unaware (single-blind) of whether they are in the control or intervention.

[3] Comparison between intervention and 'normal practice' or comparison between alternative interventions.

[4] Homogeneous or heterogeneous group of participants (in intervention and control).

[5] Intervention in controlled or normal context (controlled context requires strict adherence to form of intervention, compared with variation allowed in normal contexts)

[6] Outcome measure is an immediate proxy or outcome measure is a longer term outcome valued by practitioners and/or policymakers.

Some of these design issues are closely linked. If participants are kept blind to whether they are in the control or intervention group the trial cannot be a comparison of an intervention with 'normal practice'. Torgerson & Torgerson (2001) suggest that issues [2], [3] & [4] are related. They refer to single-blind trials with homogeneous groups as 'explanatory' and unblinded trials with heterogeneous groups comparing the intervention with 'normal practice' as 'pragmatic'. Treweek & Zwarenstein (2009) distinguish between 'explanatory attitude' and 'pragmatic attitude' in test design referring to issues [1], [3], [4], [5] & [6]. This variation in use of the terms 'pragmatic' and 'explanatory' limits their usefulness as labels for types of trial. We explain our trial design by referring to the six issues in two groups: 'Causal process' [1], [2], [3] and 'Applicability' [4], [5] & [6].

Our intervention was designed to test whether 15-16 year-olds in English schools changed their choice of subjects to study (when aged 17 and 18) if they were given information about the differences by subject in the average salaries of graduates. The intervention was a single lesson[iv] . We prepared an alternative lesson in which students received different information which is freely available and widely used in schools[v]. Therefore, our trial tests whether the kind of information that students receive in schools makes a difference to their subsequent choices [1] & [3]. This overcomes two limitations with comparisons between an intervention and 'normal practice'. First, it facilitates interpretation of the effect size since we are able to be specific about what the intervention is different *from*. Educational interventions are designed to activate a causal mechanism. For example, one widely cited review of educational interventions (Hattie & Timperley 2007) estimated the effects on attainment of providing different types of feedback to students. A comparison of an educational intervention with 'normal' practice conflates two questions: (i) to what extent does normal practice activate this (or other relevant) causal mechanisms? And (ii) to what extent does this intervention activate this causal mechanism more strongly than normal practice? Second, 'normal' practice may generate other valuable outcomes which are lost by the intervention.

We also used a single-blind design [2] so that teachers and schools did not know which arm of the study they were in. This avoided the risk that participants could change their behaviour simply as a consequence of knowing they are in the intervention or control arms of a trial (Wood et al. 2008, Guyatt et al. 2011). This design feature is not feasible in a comparison with 'normal' practice and reinforced our responses to issues [1] and [3].

Turning to issues of applicability, our focus on 15-16 year-olds reflected the design of the education system in England. Students choose at age 16 whether to continue in full-time education and which subjects to study for the following two years. Most students between the ages of 16 and 18 study a small group (3-5) of subjects which are designed as preparation for undergraduate study. Most undergraduate programmes in English universities concentrate on a single subject (the major) from the first year. Therefore, students choose their major at age 17-18 whilst still in their final year at school. We focused on students at this age because we wanted to find evidence of their expectations and decision-making at a key transition point [4]. Whilst the intervention and alternative lessons were designed by the researchers, they were taught by usual class teachers during the regular, timetabled, curriculum [5]. Our process evaluation noted that teachers introduced variations into the lessons through the way they framed the activities, through the way they highlighted particular features of the data (e.g. gender differences) and through the ways in which they summed up the lesson (or left the end of the lesson open). We included a broad range of schools and students [4] and our trial outcome directly addresses policymakers' concerns (OfSTED 2001, 2010) with the advice that schools give to students when choosing subjects [6].

The Intervention

Students allocated to the intervention were given information on graduate premia for ten subjects : Business, Education, Engineering, History, Languages, Law, Mathematics, Politics, Psychology, and Science. The data were drawn from O'Leary & Sloane (2011) and the relative average salaries are presented in Table 1. The data were used in three activities within a lesson lasting roughly one hour[vi]. The activities compared absolute average wages for males and females. If we had used other studies (e.g. Walker & Zhu 2011 or Chevalier 2011) we would have presented slightly different relative wages. But the message about graduates' wages from different studies is broadly the same for each study: mathematics, engineering and computing graduates earn, on average, substantially more than graduates in pure science, languages graduates' earnings are below pure science, whilst humanities graduates earn the least.

Table 1 about here

Students in our control schools were given a different lesson using sources in the public domain to inform students about subject choice. We selected information to align with current practice in schools from (i) a web site on subject choice[vii]; (ii) a publication by the Russell Group of Universities (2011) and (iii) a publication (Coe et al. 2008) on the relative difficulty of different A-level subjects. None of these publications or the lesson activities contained any information about relative wages of graduates from different subjects.

Outcomes

Students' intended subjects of study

We gathered data through questionnaires before and after the intervention and control lessons. The questionnaires asked students to state their intentions towards studying each of several subjects: Art, Biology, Business Studies, Chemistry, Computing, Design and Technology, Economics, English, Geography, History, Languages, Mathematics, Media Studies, Music, Physical Education, Physics, Psychology, Travel and Tourism. Students were also asked to indicate what they believed graduates would earn at age 30 if they studied any of: Art, Business Studies, Education, Engineering, History, Languages, Law, Politics or Sociology, Mathematics or Computing, Physics or Chemistry or Medicine. For each subject area, students were asked to express their expectations of (i) average graduate earnings; (ii) the earnings of a graduate just in the top quarter of earners for that subject; and (iii) the earnings of a graduate just in the bottom quarter of earners for that subject. They were also asked to indicate what they thought they would earn if they studied that subject at degree level. Our data enable us to investigate intervention effects on the probability distribution of students' expectations (Manski 2004) as well as their expectations of average graduate earnings. As far as we are aware this is the first study to provide evidence of students' expectations of average graduate earnings *and* their expectations of their own earnings. This enables our analysis to take account of rigidity in personal wage expectations when beliefs about average relative salaries change. We also asked students to indicate the relative strength of different motivations (including future salary) in their choice of subject to study[viii].

Students actual choices of subjects

We linked our questionnaire data with data from the National Pupil Database which includes information on students' characteristics, backgrounds and academic achievement from ages 4 to 16. We also linked to information provided by the schools about the actual subjects the students subsequently chose at A-level.

Sample size calculation

Based on our previous study (Davies et al. 2009a) we estimated that we would have 80% power to detect a 0.3 percentage point difference in outcomes using a two-tailed test at $\alpha = 0.05$, assuming an intraclass correlation of 0.1 and an average of 83 A-level students per year and a sample of 48 schools. This is a large effects of the intervention, however, we expected to substantially increase our power by adjusting for baseline covariates which associate with student choice, particularly their stated preferences for subjects prior to receiving the intervention lesson. Furthermore, our power was increased because on average the schools in our study had more students than we expected in our original design.

Sample

We generated a list of all schools within a large and diverse geographical area within England which satisfied our criteria: serving the age range 13-18 and having at least 100 students in their 'sixth forms' (students in the academic year between 16 and 18) (details provided in Appendix 1). We also stratified our sample to include 20 private schools and 30 state schools so we would have power to detect heterogeneity in the effect of the intervention across school types. These criteria meant that the average achievement of students in our sample was higher than average for all 15-16 year-old students in England. The majority of the students in our sample achieved grades at age 16 which are regarded as a minimum for entry into higher education. Only 9% and 10% of our sample failed to achieve at least GCSE grade C in mathematics and English respectively. Eleven per cent of our sample were 'marginal students' in terms of GCSE grades (Davies *et al.* 2009c) in that their English and mathematics GCSE grades summed to 8 points (equivalent to grade C in both subjects). Further details about our sample are available in Davies *et al.* (2013b).

Randomisation

The randomisation was carried out in Stata by a statistician in a university medical trials unit who was independent from the project team. The randomization was stratified by three variables: state or private school, single or mixed sex school, average pupil achievement above or below the median for the whole sample to create 8 blocks. Participating schools were blinded to the allocation between the intervention and control arms.

Primary and secondary outcomes

The primary outcome was pre-specified as actual choice of A-level subject as indicated by schools after the students had started their A-level courses. We included all subjects chosen by more than ten percent of the students. This means if the student left the school we do not know which subjects they took at other schools. In our primary analyses we addressed this problem using multiple imputation, we present a complete case results as a sensitivity analysis in the appendix. The secondary outcomes were intentions to study subject as measured on a Likert scale and salary expectations were recorded in thousands of pounds per year as measured in the follow-up lesson given in the same week as the intervention and control lessons.

Deviations from the study protocol

The total project sample included 50 schools and 5,593 students. Students were eligible to be included in the analysis if they gave consent for their data to used, see Figure 1 for actual responses to each round of data collection. Ten schools withdrew part way through the project. Six schools (571 students) allocated to treatment arm did not take part in the intervention, and four schools (487 students) allocated to control did not participate in the second round of questions. Six schools cited practical problems: staff illness or workload or an impending school inspection. Four schools stated that they were uncomfortable with the data in the lessons on the grounds that the information might influence students' choices in ways that was not acceptable to the teachers in these schools. Two of these schools were in the intervention arm and two of the schools were in the control arm of the trial[ix]. There were missing values for some participants due to student absence for the lesson or for one of the questionnaires and students not giving permission for their answers to be used. The sample sizes for our key dependent variables in the study are shown in Table 2. However, 46 of the schools returned information on the actual A-level choices of their students. As per recommendations for reporting randomised trials (Moher *et al.* 2001), we report results using an intention to treat analysis. Hence we report differences in outcomes on the basis of allocation to treatment and control rather than per protocol.

Figure 1 here

Imputation of missing data

We imputed missing values using the multiple imputation routines in Stata. We used the following variables in multiple imputation: wage expectations, family background, expected exam results, Key Stage 2 and 3 exam results, whether the students intended to study each subject at A-level, and the students' actual A-level choices. We imputed 20 datasets. We have a rich set of background and post-intervention data, including data on intentions, socioeconomic status, and academic attainment precisely measured from linked administrative data. Multiple-imputation allows for missingness under the missing at random assumption. We also report a complete case analysis which depends on the stronger assumption that individuals with any missing values are a random sample of the experimental sample. Therefore, as per established guidelines (Wood et al. 2004) for reporting randomised trials, we report the complete case analysis restricted to individuals with no missing data as a sensitivity analysis.

Table 2 here

Statistical analysis

We report the balance of characteristics of the students between the two arms of the trial, and test for the intention to treat effects of the intervention on the students' their choices using logistic or linear regressions. This treats each student's choices of subjects as independent. We relax this assumption below in a sensitivity analysis using a multivariate logistic regression regression. All analyses we present allow for clustering of choices across students within each school, this accounts for peer-group effects.

Whilst students studied up to five different A-level subjects our primary outcome for the trial was defined in terms of change in the probability of studying each of twelve subjects. We used logistic regressions to estimate the odds-ratio of taking a subject between the arms of the trial. We also report the results adjusted for a range of baseline characteristics, gender, achievement at age 14 in English, Maths and Science, the students' expected grades in English and Maths at age 16 (GCSE) and the students' intentions towards studying the subject prior to the intervention as shown in equation 1.

$$P(p_k = 1) = \Lambda\left(\alpha_{k0} + \alpha_{k1}x_1 + \alpha_{k2}x_2\right) \tag{1}$$

where subject choice for each of *k* subjects is indicated by the binary variable p_k, equal to one if the student took the subject. The intervention arm is indicated by x_1, equal to one if the student attended a school allocated to the intervention. The other covariates, such as prior intentions on taking a subject, are indicated by the vector x_2. Therefore the parameter of interest is $\exp(\alpha_1)$. This parameter has a simple interpretation as the ratio of odds of taking the subject in the intervention and control arms. The adjusted analysis absorbs some of the heterogeneity in the outcome, so has greater power to detect effects of the intervention.

We estimated the effects of the intervention on the students' expectations of graduates' wages and their own wages across a range of subjects using linear regression, shown in equation 2.

$$w_k = \beta_{k0} + \beta_{k1}x_1 + \beta_{k2}x_2 \ . \tag{2}$$

For these results, the parameter of interest is the mean difference in wage expectations, w_k for degree subject *k*, between the intervention and control arms, indicated by β_{k1}. To increase power, we also report results adjusted for the participants' prior wage expectations.

For subjects in which we measured both the students' intentions to study the subject and their wages expectations (Art, Business, History, Languages, Maths and Physics) we investigated whether there were any differences in the effects of the intervention in students who initially stated they were likely or definitely going to take a subject.

We investigated whether there was any effect of the intervention on students' stated intentions, indicated by s_{ki}, of taking each subject measured on a Likert scale using multinomial logistic regression, as shown in equation 3.

$$P(s_{ki} = j|x_1, x_2\ ) = \frac{\exp(\gamma_0+\gamma_{1kj}x_{1k}+\gamma_{2j}x_{2k})}{1+\sum_{h=1}^{J}\exp(\gamma_0+\gamma_{1kh}x_1+\gamma_{2kh}x_2)}\ , \tag{3}$$

The parameters of interest $\exp(\gamma_{1kj})$ is the ratio of odds of a student stating they would definitely, likely, possibly, or unlikely to take a subject compared to the odds of stating definitely not between the intervention and control arms. Again to increase the precision of our results, we also report odds-ratios adjusted for the students' initial intentions of taking each subject. Please see the online appendix for further sensitivity analysis, including a permutation analysis which accounts for multiple hypothesis testing.

Permutation tests

We investigated the effect of the intervention on students' choices of multiple subjects, therefore we tested more than one hypothesis. This means that conventional thresholds for testing hypotheses were likely to be too low. However, the students' choice of subjects are correlated, therefore the Bonferroni correction would be overly conservative and underpowered. We overcome this issue using permutation tests (Welch 1990). We resampled the data and randomly reassigned individuals A-level choices to a set of covariates and their allocation to treatment or control. We resampled the data 1,000 times and estimated our main results using each resampled dataset. Because we know that there is no relationship between exposure and outcome in the resampled data (due to the randomisation), the distribution of the p-values from the permutation datasets is a valid estimate of the null-distribution. We inverse ranked the subjects by p-value, and calculated the number of permutation datasets in which each rank choice had a lower p-value. For example, for the subject with the lowest p-value, maths, we calculated the number of permutation datasets in the lowest p-value was smaller than seen in the actual data from the project. For the second ranked subject, we calculated the number of permutations in which the second lowest p-value was smaller than observed in the data. We repeated this process for all the subjects.

Sensitivity analyses

We carried out sensitivity analyses via a complete case analysis and our modelling of students' choice. In Davies et al. (2009) we estimated a multivariate choice models with multiple outcomes which allowed for correlations between the choices. However, we were not able to jointly estimate these models using data for all the subjects reported in this paper. We attempted to fit a multivariate choice model for all subjects, however it did not converge. In a simplified analysis we investigated whether allowing for bivariate correlations between subjects meaningfully affected the results. We used the imputed data and for every possible pair of subjects we estimated a bivariate probit model, which allows for correlations between the students' choices of two subjects. We included the same covariates as described in our primary analysis above. Our results were substantively unchanged, we report the p-values on the effect of the intervention in the appendix. As a sensitivity analysis we investigated whether our adjusted results were sensitive to adjusting for Key Stage 2 English, Maths and Science results at age 11, rather than Key Stage 3 at age 14. This made little difference to our results. In an exploratory analysis, we investigated whether there was any evidence for heterogeneity in the treatment effect by gender and between private and state schools. We report standard errors clustered by school for all statistical tests and a sensitivity analyses restricted to individuals with complete data in the online appendix.

The appendix contains a sensitivity analysis of the results reported in Tables 5, 7-11 in the main paper, but restricting the sample to participants with no missing data. In the paper we refer to this as the complete case analysis. All other details of the analysis remain identical to those described in the paper, standard errors are clustered by school. Sample sizes are reported in the tables. The code used to produce the results reported in this study can be accessed here (https://github.com/nmdavies/subject-choice-rct-nuffield/).

RESULTS

Of the initial 5,593 students eligible to take part in the study, 4,539 took part in the initial survey, 4,435 took part in the second survey and 46 schools provided information on the actual choices of 3,594 students. Thus in our multiple imputed results we have 5,593 students, 3,334 allocated to the intervention and 2,259 allocated to the control lesson. We found no evidence of systematic differences between the two arms of the trial for 16 characteristics listed in **Table 5**.

Table 6,7 and Figure 2 here

The raw numbers of students taking each subject is shown in **Table 6**. Of those students who remained in the same school, students in the intervention were more likely to take Maths (52% vs. 42%) and chemistry (33% vs. 25%) than those in the control arm. The effects of the intervention on the likelihood of taking each subject are shown in **Table 7** and **Figure 2**. In the unadjusted results we found weak evidence that students in the intervention arm were more likely to take maths (odds-ratio= 1.42 95 confidence interval (95%CI): 0.94, 2.14). There were differences for the other subjects although the results were imprecise. Adjustment for demographics, prior exam results and prior intentions increased the precision of the results. In the adjusted results (right hand column) more students in the intervention arm took Maths (odds-ratio=1.39, 95%CI: 1.06, 1.82). Fewer students took biology (odds-ratio=0.73, 95%CI: 0.54,1.00), and computing (odds-ratio=0.61, 95%CI: 0.38, 0.99). The results for other subjects were imprecise and consistent with relatively large increases and decreases in enrolment. The permutation p-values suggest that the differences we observed for Maths, computing, biology and English were greater than observed in the permutation datasets.

Table 8, 9 here

We investigated possible mediating factors by estimating the effects of the intervention on students' beliefs about graduate wages (**Table 8**). Students believed that Law and Medical graduates had the highest average salaries. They also believed their own earnings would be highest in these subjects. We tested whether the intervention affected students' beliefs using linear regression (**Table 9**). The intervention caused students' beliefs about average politics graduates' salaries to fall by £2,991 (95%CI: £2051, £3931) and law graduates to fall by £2,682 (95%CI: £1,868, £3,496). Students' reduced their expectations of history, languages, medicine and physics graduates, whilst their beliefs of education graduates salaries increased. There was little effect on their beliefs about average maths or computing graduates salaries. There were fewer changes in students' beliefs about their own future salaries in each subject. The students increased their expectations of their own salaries if they took education and engineering degrees, but reduced their expectations of law, medicine and politics. We found little evidence that the effects of the intervention differed depending on the how likely the student was to take the subject prior to the intervention (**Table 10**).

Table 10 here

Finally, we investigated whether the intervention had any effects on the students' stated preferences using multinomial logistic regression (**Table 11**). After adjustment for prior intentions on taking each subject, fewer students intended to take Biology, Chemistry, Economics, English, Geography, and Languages. There was little change in intentions to take Business, History, Maths, Physics and Psychology. More students stated they intended to study computing. When we restricted the analysis to students who actually studied maths we found that whilst the control students did not change their intentions to study maths, there was a 16 percentage point increase in the proportion of students in the intervention group declaring that they would probably or definitely study mathematics. In a sensitivity analysis allowing for bivariate correlations between subjects we found few differences with the main results. This suggests that correlated choices are unlikely to affect our results (**eTable 7**). The results were substantially unchanged when we adjusted for Key Stage 2 results in English, Maths, and Science (**eTable 8**). The effects of the intervention were similar in state and private schools (**eTable 9**). The intraclass correlations for subject choices across schools can be seen in **eTable 10**. There was little difference in probability of missing outcome data between the arms of the trial (risk difference=-2.2% 95%CI:-13.0%, 8.7%).

Table 11 here

DISCUSSION

We found that students changed their subject choices after they received an hour long lesson on information about graduate wages. They were more likely to take maths and less likely to take biology and computing (See **Figure 2**). We found evidence that mediating factors such as their beliefs about average graduate salaries and their own likely salary in each subject were affected by the intervention.

Figure 2 here

Higher education policy in England increasingly relies on market forces to allocate students to courses and resources to universities. However, undergraduate recruitment to STEM courses has remained an area of government concern and intervention. Earlier policy (Roberts 2002) asserted that university applicants were not responding adequately to high salaries being offered in the labour market for science graduates. This assertion is consistent with evidence of salaries for UK engineering graduates but it is not consistent with evidence of salaries for graduates in pure science. More recent policy (Browne 2010) has argued that applications to science courses in higher education will fall below the socially desirable equilibrium because students will not take account of positive externalities. The implementation of the reforms to higher education funding proposed in the Brown Review have seen universities accepting subsidies for science courses whilst charging identical tuition fees to STEM and non-STEM students. The policy has resulted in little additional incentive to study STEM subjects.

The impact of government interventions in higher education depend on students' knowledge as well as on the choices of providers. Recent evidence (Jerrim 2011) has suggested that applicants to undergraduate courses in the UK tend to overestimate their own future earnings. More specifically, they tend to believe that the graduate premium for pure science is the same as the graduate premium for mathematics and applied science. Our results support this conclusion.

The key addition from our study concerns the effect of providing school students with information about graduate salaries. Human capital theory predicts that students will change their choices if they better informed about monetary benefits of different options for study. Students' beliefs about graduate premia for mathematics and computing were confirmed by the information in the intervention. However, students' beliefs about graduate premia for pure science, sociology, politics and law were challenged. A significant number of students reduced their wage expectations and there was a small but significant reduction in intentions to study these subjects in their final years of schooling.

Generalizability

Our sample comprises a high proportion of schools with high achieving students, however our sample selection process should have reduced the risk of unobserved differences between our sample schools and the whole population. Furthermore, because we evaluated the intervention using a randomised design the estimates of the causal effect of the intervention are likely to be internally valid. Nevertheless, our results raise further questions, such as whether similar interventions can be used to affect educational choices of other groups of students, for example disadvantaged students eligible for free school meals, or under-represented groups, such as women in engineering. Further randomised trials are needed to determine how generalizable these results are to all schools and possible educational choices.

Strengths and limitations of the study

Each student had a range of possible subject to choose. This means that our results may be affected by multiple hypothesis testing. However, we found evidence of effects on three of twelve subjects at $p<0.05$. This suggest these results are unlikely to be due to chance. Furthermore, we found further evidence that the intervention affected mediating beliefs about wages and intentions to study each subject. Finally, the differences observed in the actual data were greater than we found in the permutation analysis. This suggests that our findings are unlikely to be due to chance.

As with many randomised controlled trials, our study suffered from missing data. This could introduce bias into the results if it is not properly accounted for. We addressed this issue using multiple imputation. This approach will work well in our study because we have outcome data (the actual A-level choices) for a large proportion of our sample. We have the students' actual A-level choices for most of the schools regardless of whether they took part in the second wave of surveys. Multiple imputation depends on the assumption that the data are missing at random, and our rich set of background characteristics, including exam results from five years before the intervention took place, means that the assumption is plausible. Additionally, a complete case analysis (reported in the appendix as a robustness check) leaves our results substantively unchanged.

This is the first randomised controlled trial in the UK to demonstrate that students' educational choices can be affected by providing information on returns to schooling in a structured lesson. These results may be due to chance, to increase the certainty of the effects of this intervention this experiment must be replicated (Ioannidis 2014).

A strength of our study is that we measured students' intentions for studying each subject and their expectations of wages prior to the intervention and then measured their intentions and expectations after they received the intervention or control lessons. This has two benefits, first we can tell if the intervention has an effect on these intermediate outcomes, second we can use the baseline intentions to absorb the baseline heterogeneity in choices. This increased the power of our experiment to detect difference in choices between the two arms of the study.

Policy implications

One possible policy response is to provide students with labour market information. There is some variation in estimates of the size of differences in graduate premia, but the message is the same across all of the studies of UK data: premia for mathematics and applied science are high but premia for pure science are modest and premia for humanities relatively low. This study suggests that an information strategy focusing on maths could change the pattern of subject choice. Providing information to schools is a cheap and possibly powerful policy intervention.

Future research

Future RCTs could examine students' choices at alternative margins of educational choice. For example, do students who choose to leave school at 17 have different beliefs about the returns to education than those who stay on? Is it possible to affect these beliefs by providing credible information? Similarly, do disengaged students have different beliefs about the financial returns to education? The students involved in this study will have chosen their university courses, so one avenue for future study is to examine whether our intervention affected the students' university choices. The intervention encouraged more students to take Maths, therefore it would be interesting to find out if there are differences in the average Maths A-level results between the arms of the trial. We might expect the results to be lower in the intervention arm because less able students took Maths. Finally, in future it may be possible to link earnings data to the participants of the trial to see if the intervention affected the students' earnings in the labour market.

CONCLUSIONS

We conducted a cluster randomised trial of providing information to school students about graduate salaries. We found that the intervention encouraged more students to take Maths. Policymakers are frequently concerned with encouraging sufficient students to take STEM subjects. The results of our trial suggest simply telling students what is in it for them could be sufficient to affect their choices.

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Appendix 1 Steps in Sample Design

1. Starting from the 2010 Sixth form performance tables for England made available by the Department for Education.
2. Following Institution Types eliminated: Further Education Colleges and Sixth Form Colleges. Institutions with no name. This left 2617 records.
3. All institutions with less than 100 students in ‘Key Stage 5’ (students aged 16-18) removed leaving 1981 records.
4. Restricted to a geographical area comprising roughly a quadrilateral bounded by the cities of London, Bristol, Liverpool and Sheffield and in detail comprising the postcodes starting: AL, B, BA. BR, BS, CH, CR, CV, CW, DE, E, EN, GL, HA, HP, IG, KT, L, LE, LU, M, MK, N, NG, NN, NW, OL, OX, RG, RH, RM, SE, SG, SK, SL, SM, ST, SW, TW, UB, W, WA, WD WR, WS, WV. 1156 records. (957 state schools, 199 private schools).
5. Randomisation stratified by state and private schools.
6. Schools approached in order following each randomized list until quota for state schools and private schools achieved.

Figure 1: Flowchart of sampling, allocation and attrition

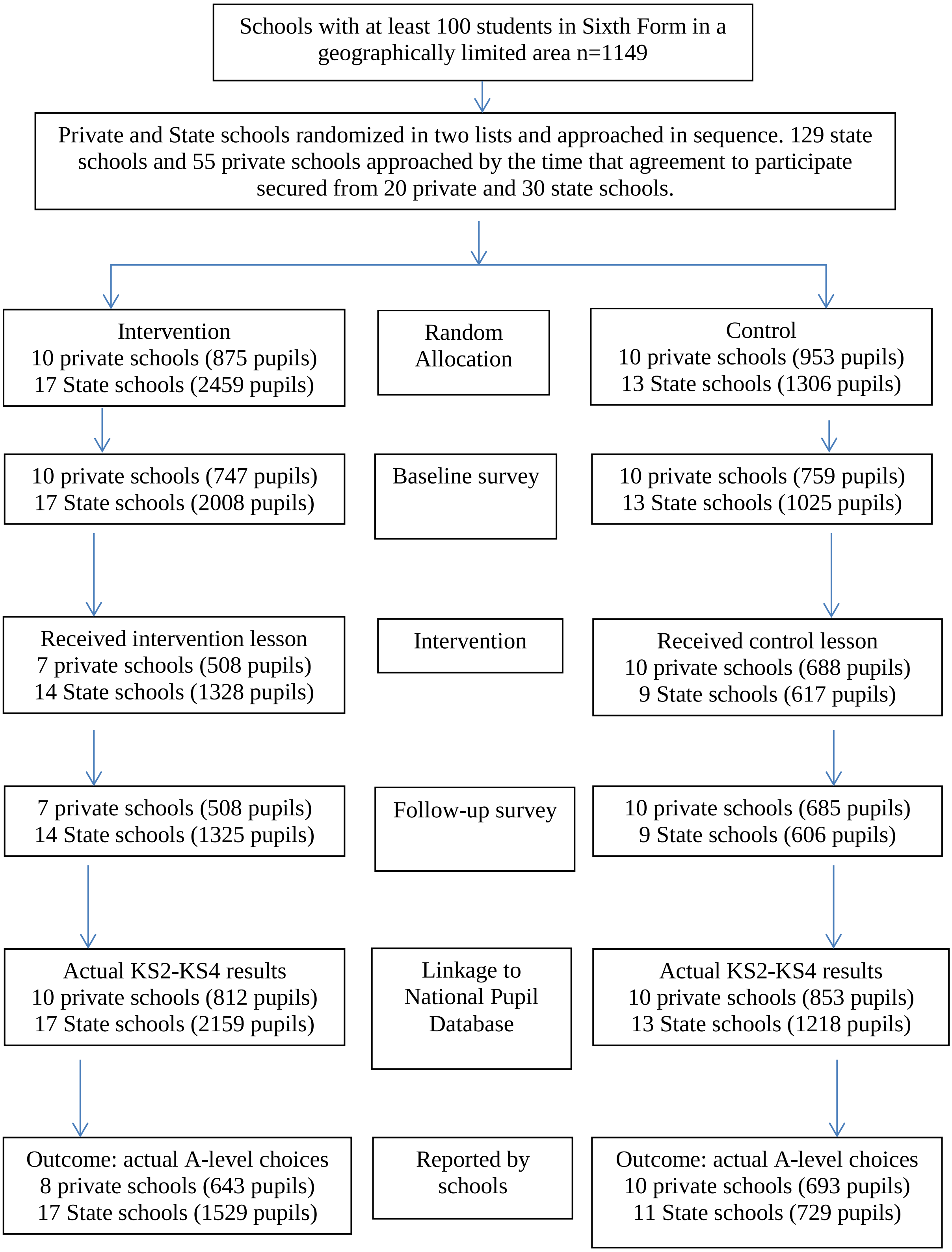

Figure 2: Proportion of intervention and control arms choosing to study each subject by allocation group.

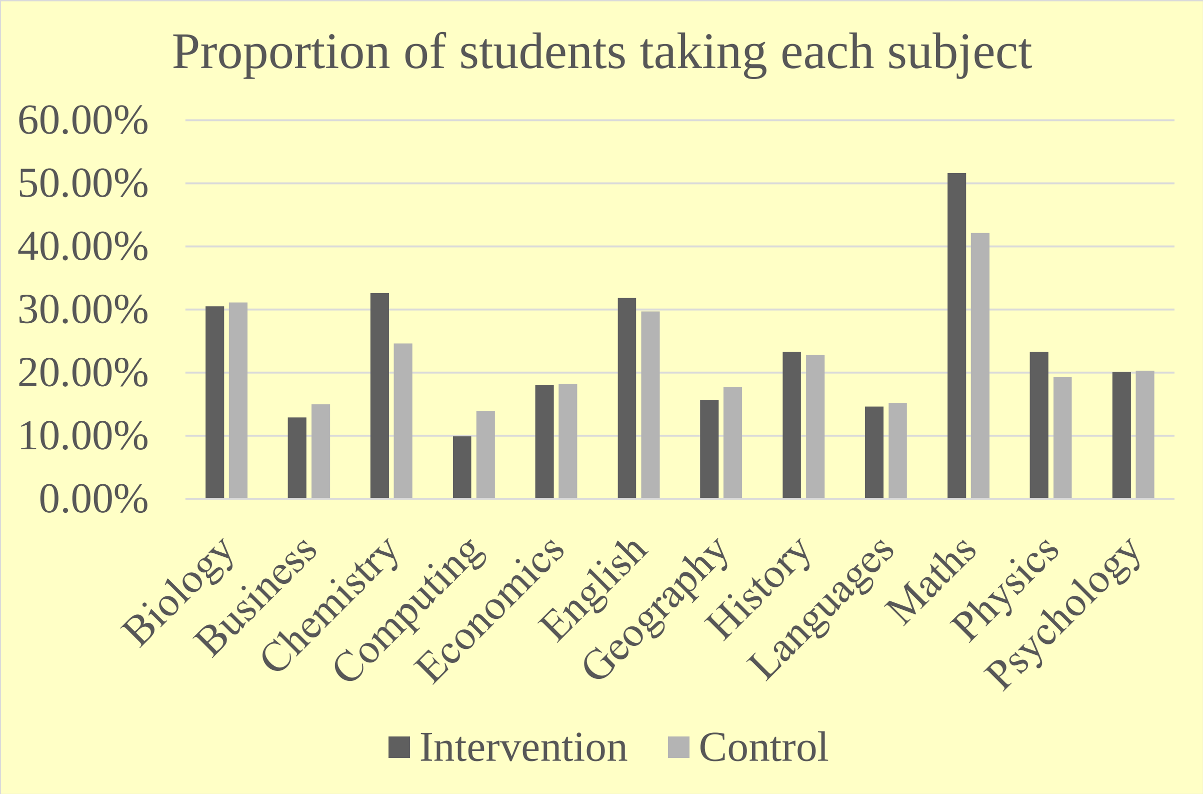

Table 1 Data used in the intervention

Degree subject	Female average graduate salary	Male average graduate Salary	Female % different from A levels	Male % different from A levels
Art	£26,000	£26,500	18	-4
Business or Financial	£30,000	£33,500	36	22
Education	£32,250	£33,500	47	22
Engineering	£30,000	£36,750	36	34
History	£26,000	£30,000	18	9
Languages	£27,000	£31,500	23	15
Law	£32,750	£34,000	49	24
Maths or Computing	£31,250	£36,500	42	33
Politics	£26,000	£30,500	18	11
Psychology	£27,000	£31,500	23	15
Science	£28,000	£33,000	27	20

Table 2: Descriptive statistics of the sample imputed data (N=5,593)

	Intervention (%)	Control (%)	
Male	48.8	43.3	0.56
Expected			
Maths GCSE grade*	6.57	6.54	0.89
English GCSE grade*	6.47	6.50	0.91
State school	73.8	57.9	0.26
Ethnic group			
White	71.8	78.1	0.22
Other ethnic group	9.3	8.1	
Indian	8.8	5.3	
Pakistani	2.2	1.9	
Black African	2.3	1.4	
Black Caribbean	1.9	1.6	
Chinese	1.9	1.4	
Black Other	0.6	0.7	
Bangladeshi	0.8	0.3	
Father professional or managerial	59.0	56.6	0.69
Mother professional or managerial	43.6	41.5	0.64
Student aspires for professional or managerial job	81.8	78.4	0.40
Graduate father	50.0	46.4	0.57
Graduate mother	46.2	44.6	0.78
Eligible for free school meals	6.0	8.3	0.42
Salary very important for choice of subject	53.9	50.9	0.22
Intending to attend university			
Definitely	58.8	58.4	0.95
Probably	26.5	26.4	0.97
Unlikely	11.2	11.2	1.00
Definitely not	3.5	4.0	0.73

Notes: * mean points score. P-values allow for clustering between schools and are estimated using ordinary least squares.

Table 6: Number of students taking each subject by allocation to intervention or control arm

	Intervention		Control	
	N	(%)	N	(%)
Biology	662	30.5%	442	31.1%
Business	281	12.9%	214	15.0%
Chemistry	708	32.6%	350	24.6%
Computing	215	9.9%	197	13.9%
Economics	391	18.0%	262	18.2%
English	690	31.8%	423	29.7%
Geography	341	15.7%	252	17.7%
History	505	23.3%	324	22.8%
Languages	318	14.6%	216	15.2%
Maths	1,121	51.6%	599	42.1%
Physics	505	23.3%	275	19.3%
Psychology	437	20.1%	289	20.3%
Total	2,172		1,422	

Notes: we do not have data on the subject choices of 1,999 students who left their school.

Table 7: Effect of intervention on student's actual choice of A-levels (N=5,593)

	Unadjusted				Adjusted				
		Confidence interval				Confidence interval			Permutation
	Odds-ratio	lower	upper	p-value	Odds-ratio	lower	upper	p-value	p-value
Biology	0.96	0.62	1.48	0.85	0.73	0.54	1.00	0.048	0.004
Business	0.89	0.54	1.44	0.63	1.00	0.69	1.45	0.990	0.873
Chemistry	1.36	0.89	2.09	0.16	1.08	0.82	1.43	0.568	0.359
Computing	0.68	0.39	1.19	0.18	0.61	0.38	0.99	0.045	0.025
Economics	1.06	0.61	1.84	0.84	1.05	0.73	1.52	0.785	0.621
English	1.07	0.85	1.34	0.57	1.18	0.94	1.48	0.154	0.020
Geography	0.89	0.60	1.33	0.58	0.88	0.66	1.19	0.419	0.358
History	1.06	0.75	1.50	0.74	1.09	0.83	1.43	0.540	0.505
Languages	0.97	0.58	1.63	0.91	0.95	0.68	1.33	0.774	0.796
Maths	1.42	0.94	2.14	0.10	1.39	1.06	1.82	0.016	0.050
Physics	1.19	0.77	1.84	0.42	0.98	0.74	1.29	0.884	0.506
Psychology	0.98	0.63	1.51	0.92	0.96	0.66	1.39	0.828	0.551

Notes: standard errors clustered by school, imputed data, adjusted analysis adjusts for KS3 science English and maths levels and expected English and maths grades, gender and prior intentions to study subject. Permutation p-value equal to the proportion of permutation samples in which the ranked p-value was lower than in the observed data. For example, in 5% of permutation samples the lowest p-value was smaller than 0.016 found for Maths.

Table 8: Students' Expectations of Average, Lower and Upper Limit and Own Earnings Prior and Post Intervention (£k) (N=5,593).

	Prior intervention beliefs				Post intervention beliefs			
	Average	Lower limit	Higher limit	Me	Average	Lower limit	Higher limit	Me
Art	27.5	17.6	42.7	23.5	28.1	18.3	42.5	23.9
Business or Financial	36.4	25.1	50.3	34.9	35.8	25.2	49.1	33.8
Education	31.0	21.4	43.3	30.6	32.7	22.8	44.9	31.5
Engineering	35.6	24.7	47.7	32.9	37.6	26.7	50.1	35.0
History	31.6	21.7	42.5	28.4	32.3	22.6	43.4	29.3
Languages	32.7	22.5	43.8	28.8	32.9	22.8	44.1	29.3
Law	45.0	32.6	58.6	44.3	43.2	31.3	56.8	42.7
Maths or Computing	38.5	26.9	51.2	37.1	39.0	27.8	51.6	37.5
Medicine	44.5	32.0	57.9	43.0	43.8	31.8	57.4	42.7
Physics	38.8	27.6	51.0	36.0	38.3	27.4	50.7	35.9
Politics	38.9	27.5	51.7	36.5	37.4	26.6	49.8	35.1

Notes: Imputed sample.

Table 9: Change in Expectations of Graduate Salaries (£k, N=5,593)

	Average salaries				My salary			
	Mean difference	Lower CI	Upper CI	P-value	Mean difference	Lower CI	Upper CI	P-value
Art	-0.23	-1.04	0.58	0.58	1.70	0.57	2.84	0.004
Business or Financial	-0.81	-1.65	0.02	0.06	0.46	-1.05	1.96	0.55
Education	1.32	0.54	2.10	0.001	2.55	1.47	3.63	<0.001
Engineering	0.25	-0.70	1.20	0.60	1.85	0.11	3.58	0.04
History	-1.43	-2.21	-0.65	<0.001	0.64	-0.58	1.85	0.30
Languages	-1.94	-2.89	-0.99	<0.001	-0.78	-2.06	0.50	0.23
Law	-2.69	-3.50	-1.88	<0.001	-2.23	-4.04	-0.41	0.02
Maths or Computing	-0.01	-0.77	0.75	0.99	0.71	-1.05	2.47	0.42
Medicine	-1.85	-2.69	-1.01	<0.001	-2.93	-4.56	-1.30	<0.001
Physics	-2.34	-3.17	-1.51	<0.001	-1.08	-2.80	0.65	0.21
Politics	-3.00	-3.94	-2.06	<0.001	-1.71	-3.22	-0.21	0.03

Notes: CI=95% confidence intervals. Imputed sample, standard errors clustered by school.

Table 10: Change in Expectations of Graduate Salaries Interacted by Intention to Study Subject (£k, N=5,593)

Subject		Mean difference	Lower CI	Upper CI	P-value
Art	Intervention	1.93	0.72	3.14	0.002
	*likely to take subject	-1.55	-4.12	1.02	0.23
Business	Intervention	0.70	-0.86	2.27	0.37
	*likely to take subject	-1.05	-3.94	1.85	0.47
History	Intervention	0.96	-0.13	2.06	0.08
	*likely to take subject	-1.44	-3.34	0.46	0.13
Languages	Intervention	-0.48	-1.77	0.81	0.46
	*likely to take subject	-1.35	-3.77	1.06	0.26
Maths	Intervention	0.99	-0.90	2.89	0.30
	*likely to take subject	-0.78	-2.97	1.40	0.47
Physics	Intervention	-0.91	-2.62	0.80	0.29
	*likely to take subject	-1.05	-3.49	1.38	0.39

7Note: Only subjects in which both salary expectations and intentions to study are included in this table. Each pair of rows for each subject is taken from a separate ordinary least squares regression with standard errors clustered by school. Intervention is a dummy variable equal to one if the student is in the intervention arm of the trial. The interaction is equal to one for individuals who were in the intervention arm of the trial and before the intervention stated they were either likely or definitely going to take the subject. The regressions for also adjusted for prior intention of taking subject and prior expectations of wages. The dependent variable was expected wages in thousands of pounds. Uses imputed data.

Table 11: Effect of intervention on students' intentions to study Biology, Chemistry, Computing, Math and Physics, (N=5,593).

	Unadjusted				Adjusted			
	Odds-ratio	Lower CI	Upper CI	P-value	Odds-ratio	Lower CI	Upper CI	P-value
Biology								
Definitely not	1	1	1		1	1	1	
Unlikely	0.90	1.09	0.73	0.28	0.86	1.05	0.71	0.14
Possible	0.72	0.96	0.54	0.02	0.68	0.92	0.50	0.01
Likely	0.67	0.99	0.46	0.05	0.60	0.89	0.40	0.01
Definitely	0.57	0.90	0.36	0.02	0.51	0.80	0.32	0.003
Business								
Definitely not	1	1	1		1	1	1	
Unlikely	1.01	1.25	0.81	0.92	1.03	1.28	0.83	0.79
Possible	1.16	1.53	0.88	0.29	1.17	1.51	0.91	0.21
Likely	1.19	1.67	0.85	0.32	1.21	1.63	0.89	0.22
Definitely	1.04	1.73	0.62	0.88	1.09	1.66	0.71	0.69
Chemistry								
Definitely not	1	1	1		1	1	1	
Unlikely	0.99	1.21	0.80	0.91	0.95	1.16	0.77	0.60
Possible	0.75	1.07	0.52	0.11	0.67	0.96	0.47	0.03
Likely	0.71	1.10	0.46	0.13	0.63	0.98	0.41	0.04
Definitely	0.75	1.22	0.47	0.25	0.62	0.98	0.40	0.04
Computing								
Definitely not	1	1	1		1	1	1	
Unlikely	1.27	1.57	1.03	0.02	1.23	1.52	0.99	0.06
Possible	1.67	2.19	1.27	<0.001	1.56	2.01	1.21	<0.001
Likely	1.54	2.24	1.05	0.03	1.40	1.97	0.99	0.05
Definitely	1.57	2.55	0.96	0.07	1.39	2.23	0.87	0.17
Economics								
Definitely not	1	1	1		1	1	1	
Unlikely	0.87	1.08	0.69	0.20	0.85	1.05	0.68	0.14
Possible	0.78	1.01	0.59	0.06	0.75	0.93	0.60	0.008
Likely	0.79	1.11	0.57	0.17	0.76	1.00	0.57	0.05
Definitely	0.69	1.12	0.43	0.13	0.67	1.03	0.44	0.07
English								
Definitely not	1	1	1		1	1	1	
Unlikely	0.80	1.00	0.64	0.05	0.80	1.00	0.64	0.05
Possible	0.69	0.91	0.52	0.010	0.69	0.93	0.51	0.02
Likely	0.65	0.88	0.48	0.005	0.65	0.90	0.47	0.009
Definitely	0.73	1.04	0.51	0.08	0.70	1.00	0.49	0.05
Geography								
Definitely not	1	1	1		1	1	1	
Unlikely	0.78	0.97	0.64	0.02	0.77	0.94	0.63	0.01

Possible	0.82	1.08	0.62	0.15	0.80	1.04	0.61	0.10
Likely	0.61	0.85	0.43	0.004	0.63	0.87	0.45	0.005
Definitely	0.58	0.92	0.36	0.02	0.59	0.94	0.38	0.03
History								
Definitely not	1	1	1		1	1	1	
Unlikely	1.02	1.22	0.86	0.81	1.02	1.22	0.85	0.85
Possible	0.96	1.24	0.74	0.74	0.96	1.25	0.73	0.75
Likely	0.97	1.35	0.70	0.87	1.01	1.36	0.75	0.95
Definitely	0.83	1.24	0.55	0.36	0.84	1.23	0.57	0.37
Languages								
Definitely not	1	1	1		1	1	1	
Unlikely	0.83	1.01	0.68	0.06	0.83	1.01	0.68	0.07
Possible	0.73	1.00	0.53	0.05	0.76	1.05	0.55	0.09
Likely	0.67	1.00	0.46	0.05	0.71	1.03	0.50	0.07
Definitely	0.58	0.93	0.36	0.02	0.64	0.98	0.41	0.04
Maths								
Definitely not	1	1	1		1	1	1	
Unlikely	0.99	1.24	0.78	0.90	0.97	1.23	0.76	0.79
Possible	1.01	1.33	0.77	0.94	0.94	1.24	0.71	0.66
Likely	0.98	1.36	0.71	0.92	0.88	1.22	0.63	0.44
Definitely	0.97	1.53	0.61	0.89	0.86	1.26	0.58	0.43
Physics								
Definitely not	1	1	1		1	1	1	
Unlikely	0.97	1.20	0.79	0.78	0.93	1.14	0.76	0.47
Possible	0.91	1.21	0.68	0.50	0.79	1.06	0.59	0.11
Likely	0.92	1.27	0.66	0.60	0.82	1.12	0.60	0.21
Definitely	0.89	1.38	0.57	0.61	0.76	1.18	0.49	0.22
Psychology								
Definitely not	1.10	1.35	0.89	0.38	1.13	1.40	0.92	0.25
Unlikely	1.09	1.36	0.87	0.45	1.13	1.42	0.90	0.31
Possible	1.06	1.52	0.74	0.75	1.08	1.55	0.76	0.67
Likely	1.10	1.63	0.74	0.64	1.18	1.70	0.82	0.37
Definitely	1.10	1.35	0.89	0.38	1.13	1.40	0.92	0.25

Note: Multi-nominal logistic regression, standard errors clustered by school. Adjusted for all variables listed in Table 4.

An earlier version of a paper subsequently accepted for publication in the *British Educational Research Journal*.

eTable 1: Descriptive statistics of the non-imputed sample

Male	5,056	46.5%		
Expected				
Maths GCSE grade	4,406		6.59	1.22
English GCSE grade	4,390		6.51	1.06
State school	5,593	67.3%		
Ethnic group	4,504			
White		75.1%		
Other ethnic group		8.8%		
Indian		7.4%		
Pakistani		2.0%		
Black African		1.9%		
Black Caribbean		1.8%		
Chinese		1.7%		
Black Other		0.6%		
Bangladeshi		0.6%		
Father professional or managerial	4,126	59.0%		
Mother professional or managerial	4,108	44.0%		
Student aspires for professional or managerial job	4,111	83.5%		
Graduate father	3,839	51.2%		
Graduate mother	3,916	47.4%		
Eligible for free school meals	4,561	4.8%		
Salary very important for choice of subject	4,367	52.8%		
Intending to attend university				
Definitely	4,443	59.3%		
Probably	4,443	26.0%		
Unlikely	4,443	11.0%		
Definitely not	4,443	3.8%		

eTable 2: Effect of intervention on student's actual choice of A-levels non-imputed sample (N=3,594)

	Unadjusted					Adjusted			
	Odds-ratio	Confidence interval lower	Confidence interval upper	p-value	N	Odds-ratio	Confidence interval lower	Confidence interval upper	p-value
Biology	0.97	0.61	1.56	0.91	1604	0.59	0.35	1.00	0.05
Business	0.84	0.43	1.62	0.60	1603	1.16	0.75	1.80	0.49
Chemistry	1.48	0.96	2.30	0.08	1601	0.91	0.59	1.42	0.69
Computing	0.68	0.31	1.52	0.35	1596	0.46	0.26	0.80	0.006
Economics	0.99	0.54	1.80	0.96	1603	1.76	0.59	5.26	0.31
English	1.10	0.84	1.44	0.49	1602	1.18	0.79	1.78	0.42
Geography	0.86	0.57	1.30	0.49	1594	0.74	0.43	1.25	0.26
History	1.03	0.72	1.47	0.89	1594	1.48	0.89	2.47	0.13
Languages	0.96	0.55	1.67	0.88	1604	1.07	0.62	1.86	0.80
Maths	1.47	0.90	2.40	0.13	1611	1.49	0.96	2.33	0.08
Physics	1.26	0.81	1.97	0.30	1598	1.10	0.71	1.71	0.66
Psychology	0.99	0.56	1.75	0.97	1602	0.88	0.58	1.32	0.52

Notes: standard errors clustered by school, imputed data, adjusted analysis adjusts for KS3 science English and maths levels and expected English and maths grades and gender.

eTable 3: Students' Earnings Expectations Prior to Intervention (£k) Complete Case

	Average		Lower limit		Higher limit		Me	
	N	Mean	N	Mean	N	Mean	N	Mean
Art	3930	27.0	3825	17.4	3814	42.5	3086	22.5
Business	3875	36.2	3763	25.1	3770	50.3	3086	35.3
Education	3840	30.7	3732	21.2	3720	43.1	3046	30.6
Engineering	3822	35.8	3726	25.0	3716	48.2	3044	33.8
History	3797	31.7	3713	21.8	3690	42.7	3010	29.0
Languages	3808	32.6	3712	22.4	3694	43.9	2997	29.0
Law	3813	45.0	3726	32.4	3762	58.5	3037	45.0
Maths	3838	38.6	3730	26.9	3741	51.4	2993	37.7
Medicine	3813	44.5	3734	32.0	3745	58.0	3015	44.1
Physics	3808	38.9	3728	27.6	3732	51.3	2974	36.8
Politics	3792	38.6	3709	27.1	3712	51.5	3005	36.5

eTable 4: Students' Earnings Expectations Post the Intervention (£k) Complete Case

	Average		Lower limit		Higher limit		Me	
	N	Mean	N	Mean		N	Mean	N
Art	2902	27.8	2867	18.2	2840	42.4	2068	23.9
Business	2875	35.7	2817	25.2	2815	49.1	2154	34.5
Education	2856	32.5	2795	22.7	2778	44.8	2145	32.0
Engineering	2866	37.9	2818	27.0	2797	50.6	2137	36.6
History	2848	32.3	2803	22.4	2769	43.2	2122	30.0
Languages	2849	32.8	2794	22.7	2782	43.9	2097	29.5
Law	2855	43.1	2806	31.2	2816	56.8	2137	43.5
Maths	2868	39.1	2804	27.8	2791	51.8	2121	38.6
Medicine	2850	43.9	2804	31.7	2804	57.4	2113	43.9
Physics	2844	38.4	2806	27.4	2773	50.7	2116	37.1
Politics	2827	37.1	2776	26.2	2771	49.4	2110	35.0

eTable 5: Change in Expectations of Graduate Salaries (£k, complete case sample)

	Average salaries					My salary				
	N	Mean difference	Lower CI	Upper CI	P-value	N	Mean difference	Lower CI	Upper CI	P-value
Art	2,007	-0.41	-1.47	0.65	0.44	1,365	1.56	0.33	2.79	0.01
Business	1,970	-0.68	-2.03	0.68	0.32	1,420	0.01	-2.01	2.03	0.99
Education	1,948	1.55	0.60	2.49	0.002	1,403	2.41	1.23	3.58	<0.001
Engineering	1,952	-0.05	-1.21	1.11	0.94	1,406	1.02	-1.42	3.46	0.40
History	1,939	-1.60	-2.73	-0.48	0.006	1,382	0.01	-1.26	1.27	0.99
Languages	1,936	-2.30	-3.80	-0.80	0.004	1,359	-1.57	-3.41	0.28	0.09
Law	1,948	-3.14	-4.54	-1.75	<0.001	1,399	-2.92	-5.57	-0.27	0.03
Maths	1,961	-0.34	-1.53	0.85	0.57	1,367	0.94	-1.36	3.23	0.41
Medicine	1,944	-2.27	-3.55	-0.99	<0.001	1,377	-2.49	-4.16	-0.82	0.004
Physics	1,949	-2.90	-4.13	-1.67	<0.001	1,360	-1.43	-3.83	0.98	0.24
Politics	1,921	-3.18	-4.50	-1.86	<0.001	1,372	-2.08	-3.74	-0.42	0.02

eTable 6: Effect of intervention on students' intentions to study Biology, Chemistry, Computing, Math and Physics, non-imputed sample.

		Unadjusted				Adjusted				
	N	Odds-ratio	Lower CI	Upper CI	P-value	N	Odds-ratio	Lower CI	Upper CI	P-value
Biology										
Definitely not	2,234	1	1	1		779	1	1	1	
Unlikely	2,234	1.08	1.35	0.87	0.47	779	0.92	0.58	1.44	0.71
Possible	2,234	0.80	1.10	0.59	0.18	779	0.59	0.37	0.95	0.03
Likely	2,234	0.72	1.17	0.45	0.18	779	0.80	0.34	1.87	0.61
Definitely	2,234	0.65	1.14	0.37	0.13	779	0.60	0.28	1.30	0.20
Business										
Definitely not	2,224	1	1	1		773	1	1	1	
Unlikely	2,224	0.97	1.32	0.71	0.83	773	0.85	0.54	1.36	0.51
Possible	2,224	1.32	1.89	0.92	0.14	773	1.63	1.04	2.54	0.03
Likely	2,224	1.25	1.86	0.84	0.27	773	1.27	0.67	2.43	0.47
Definitely	2,224	0.96	1.89	0.49	0.90	773	0.79	0.25	2.49	0.69
Chemistry										
Definitely not	2,230	1	1	1		779	1	1	1	
Unlikely	2,230	1.34	1.73	1.04	0.02	779	1.27	0.86	1.89	0.23
Possible	2,230	0.83	1.26	0.55	0.38	779	0.62	0.35	1.11	0.11
Likely	2,230	0.74	1.26	0.43	0.27	779	0.72	0.29	1.82	0.49
Definitely	2,230	0.90	1.47	0.55	0.67	779	0.64	0.31	1.32	0.22
Computing										
Definitely not	2,200	1	1	1		772	1	1	1	
Unlikely	2,200	1.26	1.63	0.98	0.07	772	0.84	0.47	1.51	0.57
Possible	2,200	1.89	2.68	1.34	<0.001	772	1.43	0.90	2.29	0.13
Likely	2,200	1.95	3.14	1.22	0.006	772	1.18	0.53	2.64	0.68
Definitely	2,200	1.38	2.36	0.81	0.23	772	0.19	0.08	0.43	<0.001
Economics										
Definitely not	2,207	1	1	1		773	1	1	1	
Unlikely	2,207	0.84	1.15	0.61	0.28	773	0.84	0.53	1.33	0.46
Possible	2,207	0.76	1.06	0.54	0.11	773	1.01	0.62	1.64	0.97
Likely	2,207	0.87	1.34	0.56	0.52	773	0.93	0.40	2.15	0.86
Definitely	2,207	0.77	1.46	0.41	0.42	773	1.57	0.63	3.91	0.33
English										
Definitely not	2,225	1	1	1		775	1	1	1	
Unlikely	2,225	0.66	0.91	0.48	0.01	775	0.66	0.42	1.04	0.07
Possible	2,225	0.62	0.94	0.41	0.02	775	0.45	0.20	1.03	0.06
Likely	2,225	0.57	0.87	0.37	0.009	775	0.56	0.27	1.17	0.12
Definitely	2,225	0.66	1.10	0.40	0.11	775	0.50	0.20	1.24	0.13
Geography										
Definitely not	2,210	1	1	1		764	1	1	1	
Unlikely	2,210	0.75	1.03	0.55	0.07	764	0.76	0.46	1.25	0.29
Possible	2,210	0.88	1.28	0.60	0.50	764	0.82	0.47	1.40	0.46

	Likely	2,210	0.60	0.90	0.41	0.01	764	0.54	0.32	0.92	0.02
	Definitely	2,210	0.46	0.78	0.27	0.004	764	0.33	0.14	0.74	0.008
History											
	Definitely not	2,207	1	1	1		771	1	1	1	
	Unlikely	2,207	1.20	1.57	0.91	0.19	771	1.70	0.92	3.13	0.09
	Possible	2,207	0.92	1.30	0.66	0.65	771	0.65	0.34	1.26	0.20
	Likely	2,207	1.22	1.78	0.84	0.29	771	1.21	0.43	3.40	0.72
	Definitely	2,207	0.72	1.19	0.44	0.20	771	0.46	0.22	0.97	0.04
Languages											
	Definitely not	2,229	1	1	1		776	1	1	1	
	Unlikely	2,229	0.77	0.99	0.59	0.04	776	0.88	0.63	1.24	0.47
	Possible	2,229	0.66	1.00	0.44	0.05	776	0.85	0.39	1.88	0.69
	Likely	2,229	0.56	0.87	0.36	0.01	776	1.10	0.42	2.86	0.85
	Definitely	2,229	0.38	0.68	0.21	0.001	776	0.87	0.33	2.34	0.79
Maths											
	Definitely not	2,241	1	1	1		783	1	1	1	
	Unlikely	2,241	1.11	1.46	0.85	0.44	783	0.92	0.59	1.42	0.69
	Possible	2,241	1.00	1.44	0.70	1.00	783	0.85	0.42	1.73	0.65
	Likely	2,241	0.95	1.37	0.66	0.78	783	0.77	0.42	1.42	0.40
	Definitely	2,241	0.99	1.61	0.61	0.97	783	0.67	0.33	1.37	0.27
Physics											
	Definitely not	2,207	1	1	1		767	1	1	1	
	Unlikely	2,207	1.04	1.38	0.78	0.81	767	1.06	0.75	1.50	0.74
	Possible	2,207	1.02	1.48	0.70	0.94	767	0.78	0.42	1.46	0.43
	Likely	2,207	1.01	1.43	0.72	0.94	767	0.73	0.46	1.16	0.18
	Definitely	2,207	0.97	1.44	0.65	0.87	767	0.52	0.21	1.28	0.15
Psychology											
	Definitely not	2,205	1	1	1		766	1	1	1	
	Unlikely	2,205	1.13	1.49	0.86	0.37	766	1.19	0.72	1.95	0.50
	Possible	2,205	1.13	1.45	0.88	0.35	766	1.21	0.61	2.39	0.59
	Likely	2,205	1.27	2.11	0.77	0.35	766	1.52	0.70	3.33	0.29
	Definitely	2,205	1.16	1.91	0.70	0.57	766	1.00	0.46	2.16	1.00

Note: Multi-nominal logistic regression, standard errors clustered by school. Adjusted for all variables listed in Table 4.

eTable 7: P-values of effect of intervention on actual subject choice allowing for bivariate correlations between subjects (N=5,593).

	P-value on effect of intervention:											
Allowing for bivariate correlation with:	Biology	Business	Chemistry	Computing	Economics	English	Geography	History	Languages	Maths	Physics	Psychology
Biology		1.00	0.59	0.06	0.69	0.15	0.41	0.54	0.87	0.01	0.91	0.88
Business	0.05		0.58	0.06	0.70	0.17	0.43	0.55	0.86	0.01	0.89	0.88
Chemistry	0.04	0.98		0.06	0.71	0.16	0.45	0.54	0.85	0.01	0.90	0.87
Computing	0.05	0.99	0.59		0.71	0.16	0.43	0.56	0.84	0.01	0.90	0.87
Economics	0.05	0.99	0.60	0.06		0.16	0.44	0.54	0.86	0.01	0.89	0.88
English	0.05	0.99	0.64	0.05	0.72		0.43	0.52	0.87	0.01	0.83	0.88
Geography	0.05	0.99	0.59	0.06	0.69	0.17		0.55	0.87	0.01	0.90	0.88
History	0.05	0.99	0.59	0.06	0.69	0.16	0.42		0.86	0.01	0.89	0.88
Languages	0.05	0.99	0.59	0.06	0.69	0.16	0.43	0.54		0.01	0.89	0.89
Maths	0.05	0.99	0.50	0.06	0.70	0.16	0.41	0.59	0.85		0.95	0.87
Physics	0.05	1.00	0.55	0.06	0.71	0.17	0.41	0.57	0.84	0.01		0.87
Psychology	0.05	0.99	0.58	0.06	0.70	0.16	0.43	0.54	0.86	0.01	0.89	

eTable 8: Effect of intervention on A-level choice adjusted for Key Stage 2 results (N=5,593).

	Adjusted for Key Stage 2 results			
A-level chosen	Odds-ratio	Lower CI	Upper CI	P-value
Biology	0.78	0.58	1.06	0.12
Business	0.93	0.63	1.36	0.71
Chemistry	1.18	0.90	1.54	0.22
Computing	0.59	0.37	0.96	0.03
Economics	1.03	0.70	1.50	0.89
English	1.21	0.96	1.54	0.11
Geography	0.89	0.66	1.20	0.46
History	1.12	0.86	1.46	0.39
Languages	0.99	0.70	1.40	0.97
Maths	1.51	1.15	1.98	0.003
Physics	1.04	0.80	1.36	0.75
Psychology	0.99	0.68	1.43	0.95

eTable 9: Heterogeneity in the effect of the intervention between state and private schools (N=5,593).

		Unadjusted				Adjusted			
		Odds-ratio	Lower CI	Upper CI	P-value	Odds-ratio	Lower CI	Upper CI	P-value
Biology	Main effect	0.89	0.57	1.40	0.61	0.87	0.08	8.95	0.91
	State school interaction	1.26	0.59	2.71	0.55	0.91	0.47	1.76	0.79
Business	Main effect	1.19	0.42	3.40	0.74	1.74	0.11	26.97	0.69
	State school interaction	0.60	0.19	1.89	0.38	0.65	0.23	1.84	0.42
Chemistry	Main effect	1.36	1.01	1.84	0.04	0.53	0.04	7.80	0.64
	State school interaction	1.22	0.61	2.43	0.58	0.92	0.51	1.65	0.78
Computing	Main effect	0.53	0.15	1.88	0.33	0.83	0.07	9.28	0.88
	State school interaction	1.13	0.29	4.35	0.86	1.24	0.26	5.78	0.79
Economics	Main effect	0.94	0.62	1.42	0.76	2.04	0.18	22.84	0.56
	State school interaction	2.08	0.92	4.70	0.08	1.56	0.77	3.16	0.22
English	Main effect	0.95	0.69	1.30	0.74	2.41	0.39	15.04	0.35
	State school interaction	1.09	0.72	1.65	0.69	0.92	0.59	1.45	0.73
Geography	Main effect	1.00	0.67	1.48	0.99	0.79	0.06	10.65	0.86
	State school interaction	1.07	0.58	1.98	0.83	0.85	0.45	1.58	0.60
History	Main effect	0.92	0.63	1.35	0.67	1.53	0.18	13.24	0.70
	State school interaction	1.49	0.87	2.56	0.15	1.17	0.67	2.04	0.58
Languages	Main effect	1.06	0.74	1.51	0.76	1.15	0.07	19.57	0.92
	State school interaction	1.39	0.68	2.86	0.37	1.09	0.56	2.13	0.80
Maths	Main effect	1.17	0.74	1.86	0.50	4.16	0.31	55.39	0.28
	State school interaction	1.60	0.81	3.17	0.17	0.97	0.54	1.74	0.93
Physics	Main effect	1.12	0.73	1.71	0.62	0.92	0.04	21.31	0.96
	State school interaction	1.50	0.79	2.86	0.21	1.10	0.61	2.00	0.74
Psychology	Main effect	1.06	0.42	2.71	0.90	2.25	0.24	20.92	0.48
	State school interaction	0.75	0.28	2.03	0.57	1.00	0.37	2.66	1.00

Notes: Imputed data. Adjusted regressions control for the same set of baseline confounders as the main results presented in Table 6. Unadjusted regressions additionally control for a state/private school dummy. Adjusted regressions include interaction terms for all covariates.

An earlier version of a paper subsequently accepted for publication in the *British Educational Research Journal*.

eTable 10: Intraclass correlation of subject choice (N=3,594).

A-level chosen	Intraclass correlation
Biology	0.089
Business	0.119
Chemistry	0.097
Computing	0.240
Economics	0.113
English	0.043
Geography	0.045
History	0.048
Languages	0.067
Maths	0.125
Physics	0.056
Psychology	0.118

An earlier version of a paper subsequently accepted for publication in the *British Educational Research Journal*.

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[i] A summary of these interventions is provided by HEFCE (2013b). Science, technology, engineering and mathematics (STEM). http://www.hefce.ac.uk/whatwedo/crosscutting/sivs/stem/

[ii] Justifications may also be found in correcting distortions arising from government intervention, for example, in the form of caps on fees and student numbers. However, these lie outside the scope of this paper.

[iii] No opportunities for a regression discontinuity design are available in recent decades in England and it is difficult to conceive how a true causal test of choices in an option system could be engineered.

[iv] Available on the project web site at http://www.birmingham.ac.uk/research/activity/education/projects/subject-choice.aspx

[v] Available on the project web site at http://www.birmingham.ac.uk/research/activity/education/projects/subject-choice.aspx

[vi] The project activities and baseline questionnaire are available at http://www.birmingham.ac.uk/research/activity/education/projects/subject-choice.aspx

[vii] ‘Studential” http://www.studential.com/further_education/alevels/choosingyouralevels

[viii] Reflecting the arguments of Expectancy Value Theory () as well as the economics of non-pecuniary incentives.

[ix] Lesson materials were sent to the schools after the random allocation into the two arms of the project. If schools had seen both lesson materials before the allocation then we would not have been able to rule out leakage from one arm of the trial to the other. As the number of schools withdrawing due to unhappiness with the materials was the same for each arm this should not introduce bias into our results.