University Selectivity and the Relative Returns to Higher Education: Evidence from the UK

by

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Abstract

We study the wage outcomes of university graduates by course (i.e. by subject and institution) using the UK Labour Force Surveys (LFS). We that the selectivity of undergraduate degree programmes plays an important role in explaining the variation in the relative graduate wages. In fact, we find that much of the variation in relative wages across courses is due to the quality of students selected. Once we allow for course selectivity in our analysis we find that our estimates of the effects of attending the most prestigious HEIs is around 10 percentage points lower than otherwise; the effects of attending the middle ranking HEIs is around 5 percentage points lower; and that of attending these lowest ranking HEIs is unaffected. We go on to consider selection (on observables) into subjects *and* institutions using the Inverse Probability Weighted Regression Adjusted (IPWRA) method to estimate multiple treatment effects.

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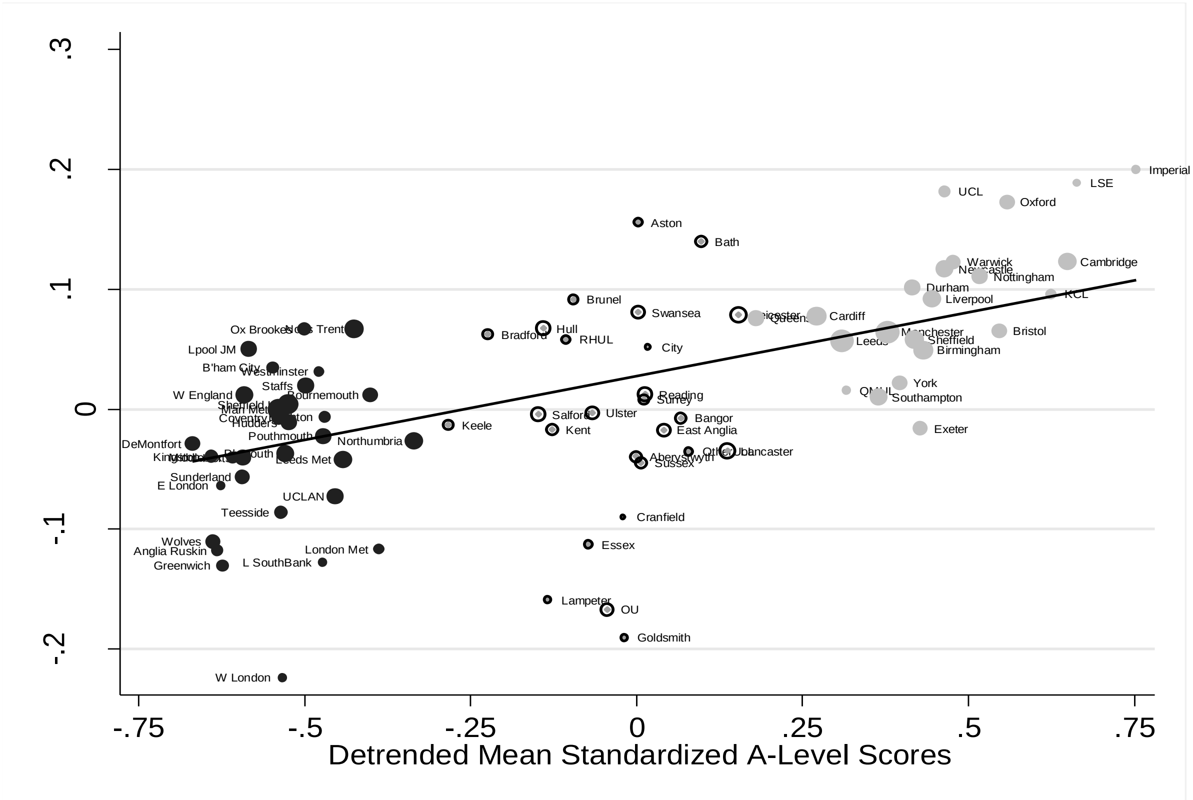
Graphical abstract

University selectivity and the estimated relative value added by universities



Dark grey is New universities; black hollow is Old universities; and light grey is the Russell Group of elite universities. The size of the bubble indicates institution size. Fitted line: slope=0.107 (robust se=0.013, R^2=0.394) shows the effect of institutional selectivity on wages – 1 SD change in selectivity raises wages by 11%. Omitted HEI is Manchester Metropolitan University.

Highlights

- Variation in graduate wages across institutions largely due to student selectivity
- Explores an inverse probability weighted regression adjustment (IPWRA) method
- OLS wage equation results hold when allow for selection on observables using IPWRA

1. Introduction

Higher Education (HE) selectivity, often called college selectivity in the US, refers to the quality of the students that attend each Higher Education Institution (HEI). HE selectivity is typically measured by the average ability of students on a course, as measured by their mean Standard Aptitude Test (SAT) score in the US, or "A(dvanced)-level" score in the UK. The strong correlation between HE selectivity and the labour market success of college graduates is firmly established in the literature (e.g. Dale and Krueger, 2002, 2014) – those students who attended more highly rated institutions, earn higher wages on average. Dale and Krueger (2002 and 2014) go on to examine the impact of self-selection on this correlation by controlling for the average SATs score of the colleges that students applied to. While they are cautious about the validity of this, their data shows convincingly that the correlation becomes weaker and statistically insignificant.

There are very few UK studies that focus on this important topic, mostly due to data limitations. This is disappointing since the UK is a good laboratory for addressing this topic because of the relatively homogeneous nature of the HE landscape, the low levels of non-completion and delayed completion, and the much more specialized nature of UK bachelor degrees where students apply for a specific major which they specialize in for the, typically, three-year duration of the course. Heterogeneity is largely confined to the (strong) differences in the degree of selectivity across courses. The contribution of this paper, which builds on previous Labour Force Survey (LFS) analysis by Walker and Zhu (2013), is to examine the labour market earnings of graduates in the UK using the LFS (broadly equivalent to the US CPS data) by both major and by type of HEI. Recent LFS data provides information on both major and HEI and can be matched to course "selectivity", as measured by the mean standardised scores at the institution-subject-cohort level in A-level national examinations at the end of High School using data provided by the Higher Education Statistics Agency (HESA).

This allows us to correct for the quality of the intake of students by course. We are able to obtain well determined estimates even when we disaggregate to as many as 16 subject groups.[1] Unlike earlier UK studies, we are able to consider the effect of differences across

[1] Economics in the same group as the rest of Social Studies and we are not able to separate them out. The results in our previous work where it was possible to distinguish between Economics and the rest of Social Studies) suggest that the annual average earnings of Economics graduates is substantially higher than the rest of the group. Britton et al (2016) show similar results using an alternative dataset. We omit nursing because there are very few males and because a bachelor degree has only been a requirement for the last two decades.

undergraduate (UG) bachelor degree subjects *and* institutions. To the best of knowledge, we are the first UK study combining a nationally representative survey dataset (LFS) that records both HEI attended and the subject, together with mean A-level scores by UG course and entry year.

Our new OLS results, that control for institutional selectivity, strongly suggest that the elite "Russell Group" (RG) institutions have substantially lower value-added once one controls for their selectivity (relative to Manchester Metropolitan University, MMU) which is the largest of the "New" universities - institutions that were created from the former Polytechnics from 1993 onwards. The difference in value added for Old universities (relative to MMU) is somewhat smaller; and the difference for all New institutions (relative to MMU) is very close to zero. It appears that the raw data exaggerates the differences in the financial returns to attending more selective institutions.

A further contribution of the paper is to allow for the effect of selection (on observables) into institutions and subjects. Since the problem then becomes one of evaluating multiple treatments (of type of subject studied and the type of HEI attended) we adopt an Inverse Probability Weighted Regression Adjusted (IPWRA) methodology. To implement this in our comparatively small sample we need to aggregate institutions into three broad, but natural, groups and we aggregate subjects into three groups. We find that OLS underestimates the effects of attending the most prestigious HEIs relative to the middle ranking HEIs, and of attending these relative to the lowest ranking HEIs, even when we include course selectivity, That is, the causal effect of institution type is biased downward in the OLS analysis. In addition, we provide the IPWRA estimates of the effects of subjects studied which reveals large effects of studying STEM subjects, smaller effects of Social Science, and no effect of studying Arts and Humanities courses. These estimates are somewhat larger than OLS estimates of the effects of subject.

The remainder of the paper is organized as follows. Section 2 briefly reviews the literature. Section 3 presents the institutional background while Section 4 introduces the data. Section 5 provides an explanation of the empirical methodology and its connection to OLS. Section 6 presents the OLS results and checks the robustness, while Section 7 explores the causal effect of broad HEI type and broad subjects of study using our IPWRA approach. Finally, Section 8 concludes.

2. Literature Review

The literature on college selectivity can be classified into two strands. The first is concerned with the relationship between college selectivity and students' college choice and performance; while the second is concerned with the estimation of returns to college selectivity. Davies and Guppy (1997), using the US NLSY data, find that socio-economic status (SES) predicts entry into selective colleges, but not subject studied directly - except for the most lucrative majors within selective colleges. Moreover, men were more likely to enter fields of study with higher economic returns than women. Hoxby (2009) reviews the trend in college selectivity in the US over the past four decades: she finds that US colleges are *not* getting more selective, except at the very top end; and changes in selectivity are mostly due to the falling costs of distance and information. Descriptive analysis by Chetty *et al* (2017) of US college students since 1999 suggest that, while students from high income backgrounds are much more likely to attend highly selective colleges, the earnings of low *and* high-income background students are similar, conditional on college attended. Smith (2013), using a large twins dataset with application and enrolment information from the US, finds that a student's probability of degree completion within four years increases by choosing a more selective institution - an increase of 5 percentage points from choosing an institution with a median SAT score 100 points higher than the alternative. However, one should be cautious in interpreting the twins fixed-effect estimates as causal because these twins, not all identical, are unlikely to be as good as randomly assigned to different institutions. Indeed, Goodman et al. (2015) find that college choice is affected by older sibling college choice. Nonetheless, Smith (2013) found that methodology made little difference to the results.

While earlier studies on returns to college selectivity are by and large descriptive in nature, the more recent literature pays closer attention to data quality and methodological issues in order to minimize bias in the estimates. Loury and Garman (1995) present a model where human capital depends on both performance at college (e.g. GPA) and college selectivity. Using the National Longitudinal Study (NLS) of the High School Class of 1972, they show that omitting college performance overstates the effect of college selectivity for Whites and understates it for Blacks. However, Black students with below median SAT scores of the college they attend have lower probability of graduation.

Causal estimation of the effect of college selectivity on earnings may also be biased by selection on unobservables, to the extent that more selective HEIs assess applicants on characteristics that are related to future earnings but would, in general, be unobservable to the econometrician. In order to moderate this bias, Dale and Krueger (2002) match students who applied, and were accepted by, with those rejected by the same set of colleges. Using the College and Beyond data set and the National Longitudinal Study (NLS) of the High School Class of 1972, they find little evidence of returns to attending more prestigious colleges for students with the same ability. Similarly, after partially adjusting for unobserved student ability by controlling for the average SAT score of the colleges that students applied to, Dale and Krueger (2014) conclude that estimates of the effects of college characteristics fall substantially and are generally indistinguishable from zero, except for students from disadvantaged backgrounds.[2]

It is usual to group UK HEIs into three main types in descending order of selectivity — the Russell Group (RG), which is the self-selected "elite" research intensive universities that include Oxford and Cambridge; "Old" universities founded pre-1992 but outside the RG; and the post-1992 "New" universities which were formerly polytechnics prior to the end of the "binary divide" that existed between universities and polytechnics.[3] There are very few studies on HE selectivity in the UK. Chevalier and Conlon (2003) is the first UK study on the subject. Using exit surveys of three UK graduate cohorts, known as the Destinations of Leavers from Higher Education (DLHE), they find that attending "Elite" (RG) universities leads to a 6% wage premium, compared to "New" universities; and no significant differences between "Old" and "New". But wages were observed soon after graduation when wages are typically very noisy and little other information will be available to the employer, and their Propensity Score Matching estimates are imprecise because of thin common support. Hussain et al. (2009) use four graduate cohort studies and five different measures of HEI quality including the total tariff score[4] at admission. They also find a positive return to attending a higher quality institution, of about 6% earnings difference for one standard deviation increase in the composite HEI quality index that they construct. Again, this study uses only recent graduates where employers may depend heavily on the quality signal associated with HEI reputation.

[2] Brewer et al. (1999) find significant returns to elite private HEIs in the US even after accounting for selection. Chen et al. (2012) also find substantial returns to MBA selectivity using the Dale and Krueger method.

[3] We use RG, Old and New as short-hand hereafter.

[4] We use the variable that HESA labels "Total Tariff (average pre-university test score – A-level or equivalent)". This is missing for a large minority of cells because there is a variety of alternative qualifications that are regarded as equivalent to a specific A-level score. This is unimportant provided the A-level score still provides a measure of the overall degree of selectivity of a course.

Finally, Broecke (2012) uses data that is collected from UK graduates three years after graduation who respond to the follow-up of the DLHE exit survey, for just the 2004/5 cohort of graduates. The response rate is low, and there is clear non-random non-response, and the earnings is still confined to a relatively early part of a graduate's career. It compares the earnings of graduates who satisfied the entry conditions for their preferred institution, with applicants that failed to satisfy the entry conditions at the same institution but went to their second-choice institution instead. This is effectively a difference in differences design. The analysis controls for subject of study, and for the overall A-level score achieved, and the parameter of interest is the coefficient on the institutional selectivity measure (the average tariff score requirement for admission). While the author is cautious in interpreting the results, it seems that the part that signalling plays in the estimates is likely to be quite large at this relatively early point in a graduate career, compared to later on when we might expect the value added to productivity by the course attended to have a greater weight.

Most recently, Britton *et al*. (2016) have examined the annual earnings of English domiciled graduates up to 10 years after graduation, allowing for HE selectivity using the HESA data in the same way as used here. Their data comes from the Her Majesty's Revenue & Customs (HMRC, the UK tax authority) merged with Student Loan Company (SLC) data on graduates. SLC debt repayments in the UK are linked to earnings and are administered by HMRC through the Pay-As-You-Earn (PAYE) system. They find substantial annual earnings premia for Medicine, Economics, Law, Maths and Business relative to the excluded category – which broadly reflects our results. Moreover, they find large differences associated with a (relatively crude) measure of family background on median graduate earnings– a raw gap of 25% in favour of students from higher income families: but this fails to be statistically significant (at the 5% level) in their analysis which accounted for HEI and subject.[5]

Their study differs from ours in four important ways. First, their outcome variable is tax year earnings, while we focus on hourly wages - which is likely to be a better measure of productivity. Secondly, the LFS interviews all cohorts each year (although we can only include those for which HESA data is available which limits our data to entry cohorts from 1992[6] so

[5] The Walker and Zhu (2013) analysis cannot distinguish between HEI types due to pre-2011 data limitations.

[6] In fact, we restrict the data to entry cohorts from 1992/93 because this is when the "binary divide" (between universities and "polytechnics") was abandoned. Moreover, HESA data only became available for entry cohorts from 2000/01. However, we believe that *relative* admission "tariffs" changed little over time and we use STATA's extensive missingness capabilities to allow us to retain data back to the end of the binary divide.

even our own sample contains few observations with more than 20 years of post-graduation work experience). In contrast, their HMRC data is restricted to individuals who are in the student loan system from 1998 and so will have no more than 10 years of work experience. Thirdly, our sample only covers graduates working as employees, whereas their data includes only those who choose to take out a loan.[7] Finally, the HMRC data is the universe of students and is much larger than our survey-based data, and this will adversely affect the precision of our estimates, relative to theirs.

3. Institutional Background

Higher education in the UK is almost universally provided by publicly funded universities, that are independent not-for-profit institutions and there is little direct control that the government exercises over any institution. Indeed, public subsidies have fallen dramatically in recent years with the introduction of large tuition fees supported by a sophisticated student loan scheme.[8] Over the past half century, the UK HE sector has experienced several rounds of expansion, the most recent of which took place in the early 1990s. The 1992 Higher Education Act granted university status and degree awarding power to all higher education institutions, including former polytechnics – who responded by changing their names to replace the title polytechnic, with university. Most universities offer a wide range of majors. UG majors in England, Wales and Northern Ireland are typically of three years' duration. Professional vocational subjects are offered as UG majors in the UK – for example Law, Architecture, Medicine and Dentistry are all subjects that can be studied at UG level although, amongst these, only Law is available as a three-year degree. Many of the less selective institutions do not offer these professional majors, although they do tend to offer a wider range of more vocational subjects that do not feed into the traditional "professions" such as Accountancy. Universities will have pre-requisites for entry into many majors - for example, Science A-levels are required for entry to Medicine; Maths is required for Science, Technology, Engineering, and Maths (STEM), as well as for most Economics majors; while A-levels in one or more modern languages is a requirement for most modern language majors. Students in England, Wales and Northern Ireland typically study just four, or even three, A-Level subjects during the two years

[7] While many law, accounting, and medical graduates become self-employed this is not typical early in the lifecycle and we find only 12% of the working graduates in our cohorts are self-employed. We know little about selection into loans, but the means-tested nature of the repayments mechanism suggests that those with higher rates of return will be less likely to participate in the programme because they would enjoy lower subsidies.

[8] See Walker and Zhu (2013) for the details of the history of UK student financial arrangements.

of post-compulsory schooling in senior high school.[9] The narrow nature of the high school curriculum and the use of pre-requisites for many subjects imply that students effectively constrain their university major by the high school subject choices at the age of 16.

There is a single portal that is used to apply to all HEIs and the same application information is circulated anonymously to all HEIs that a student applies to. Typically, students will apply for a range of HEIs at age 18 that offer the, sometimes narrow, range of major(s) that might be open to them. The range of HEIs that a student applies to will be driven, in part, by their expectations of their likely level of achievement at the end of high school. Most applicants apply to the same subject for all of the five HEIs that they are allowed to choose. Applicants are well aware of the likely grade requirements for admission since this is posted by institutions. They also have a good idea of their likely grades at the end of their A-level studies from predicted scores provided by their schools, so they tend to apply for several institutions that match their likely grades, and often to several slightly less selective institutions. University applicants are already heavily selected since the senior high school participation rate is still well below 100%. The HE participation rate is approaching 50% of the overall cohort size. Students apply ahead of their high school graduation examinations on the basis of grades predicted by teachers and are made offers of admission that are conditional on grades achieved. They are allowed to provisionally accept two such offers; one of which is nominated as the insurance offer - against missing the conditions of the most preferred offer. Those who satisfy their conditional offer are admitted to their most preferred HEI, and students who do not are passed to their insurance HEI. Students who fail to meet either conditional offers can apply through a "clearing" mechanism that matches such students to remaining vacant places in that entry year.[10]

The UK is small and yet students typically apply to HEIs that are some distance from the parental home – there is a tradition of college being a rite of passage associated with leaving home. Institutions usually provide accommodation in halls of residence (dorms) to ease the transition from home. Most institutions provide a full range of subjects, although there are some exceptions (Imperial College London is heavily focused on science and engineering,

[9] Scottish school qualifications are completely different and many students in Scottish HEIs study for a duration of 4 years instead of 3. Therefore, we drop Scottish HEIs from this study.

[10] Over 98% of applicants received at least one offer (in 2016). About 70% make the grades by their most preferred institution, and around 12% gain places via the clearing mechanism. Only the most selective institutions/subjects interview prospective students.

while the LSE is focused mostly on social science). However, there are large regional differentials in earnings, especially among graduates and we capture these using regional fixed effects for London, rest of the South East, Wales and Scotland - relative to the rest of England.

A large majority of students move straight from high school to university although many of the less selective institutions admit a large number of "mature" students and students with unconventional entry qualifications. HE completion rates are very high – typically over 90% and most drop-outs occur close to the start of their studies. UG courses in the UK are quite specialized and, nonetheless, the proportion attending graduate school to gain post-graduate (PG) qualifications is as common as in the US. Machin and Lindley (2013) find that just over one third of graduates in both the UK and the US have PG qualifications. Our default specification elects not to control for PG qualifications so that the interpretation of our results include the option value of being able to progress to PG.

4. Data

Our analysis is based on the Labour Force Survey which is broadly comparable to the US Current Population Survey (CPS). The LFS data is a short rotating panel and we first construct a sample of employees aged 20 to 60 years old, who hold at least a bachelor (UG) degree, using Waves 1 and 5 (the waves that contain earnings and hours of work data), in the Quarterly LFS 2012_{Q1}-2015_{Q2} inclusive, the years for which the information on HEI attended, and subject studied, was available.[11] We exclude Scottish HEIs because of their different secondary school qualifications and their distinctive four-year duration. We exclude all post-1992 universities that are not also ex-polytechnics, since these are very new HEIs with very few observations in our data. We include Medicine (with Dentistry) but exclude Subjects Allied to Medicine.[12] There are 20,597 observations in our resulting graduate sample. We merge the LFS data with collapsed data drawn from the Higher Education Statistics Agency (HESA) that provides data on the extent of selectivity by institution and course for all graduates since 2001.

[11] The data is readily available from the UK Data Service, subject to registering with them and undertaking some training. The data is potentially highly disclosive and can only be used via the Citrix server of the UK Data-Service's Secure Data Lab. The selectivity data is available from HESA and can, with permission, be merged into the LFS. Our own STATA code has been approved for release to other researchers who wish to explore the data.

[12] This group is dominated by nursing, a non-traditional graduate discipline. Note that many (of the higher earning) doctors have some self-employed income that is not recorded in our data. Indeed, a significant minority of doctors who are in General Practice (i.e. physicians who work in the community) are entirely self-employed and are dropped from the analysis altogether.

The HESA data we use is based on the individual student records of all A-level scores for UK domiciled, full-time, first degree (which excludes a relatively small minority of students who study a Foundation Year degree that combine academic and workplace skills) students studying at UK HE providers - but only for the entry years 2000/01-2013/14. We derive standardised A-level scores by UG entry cohort, HEI and subject, after normalization (with zero mean and unity standard deviation) within each cohort. Figures 1A and 1B show the average log wage for each subject and institution type, for males and females respectively.[13] On average, RG graduates, both men and women, earn 0.11-0.12 log points more than graduates from Old universities; who in turn earn 0.06-0.07 log points more than graduates from New universities. Across subjects, graduates in Medicine and Dentistry have the highest wages, followed by Business and Administrative Studies, Social Studies (including Economics), Law and then most of the Science, Technology, Engineering and Maths (STEM) subjects. The lowest wages subjects tend to be Arts and Humanities disciplines.

The HESA data contains the A-level scores for all students across all subjects in all universities in all cohorts[14]. We collapse this data to cohort*subject*institution*year cells which provides our measure of course selectivity. Figure 2 shows the variation in mean selectivity across institutions, on the horizontal axis, and the within institution standard deviation on the vertical axis, where the data has been standardized to have zero mean and unit variance within each cohort. The mean of the raw data is close to a score associated with BBB grades. The size of the institution is indicated by the size of the bubble (for the 79 institutions who have a minimum cell size of 15 individuals), and the colour indicates institution type (Dark Grey is New; Hollow is Old; and Light Grey is RG). The estimated slope of this population relationship is 0.129 (robust SE=0.009 and R^2=0.775) and, surprisingly, there is a clear increase in institutional variance as we move to more selective institutions. The data neatly divides between New, Old and RG, with little overlap. The New HEIs are surprisingly tightly clustered, while the RG HEIs have surprisingly large differences within the group. To allow for grade

[13] The omission of the self-employed is a major drawback of the data and may bias some of our estimated subject effects because of the large proportion of self-employed who studied Accountancy, Law, and Medicine/Dentistry.

[14] A-levels are graded with letter scores A*, A, B, C, D, E and then fail, and these are coded into a numerical scale by attributing a score to a grade and aggregating the best 3.

Figure 1A Log wages by subject and institution type: Men

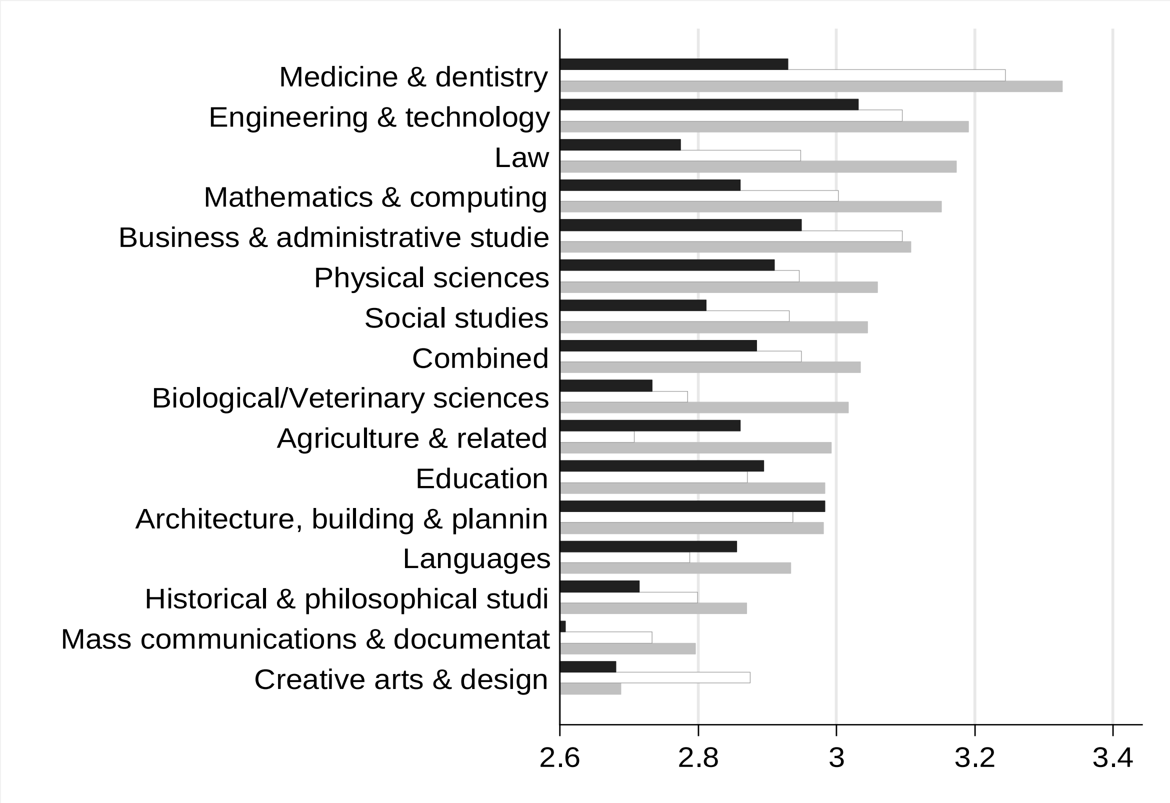
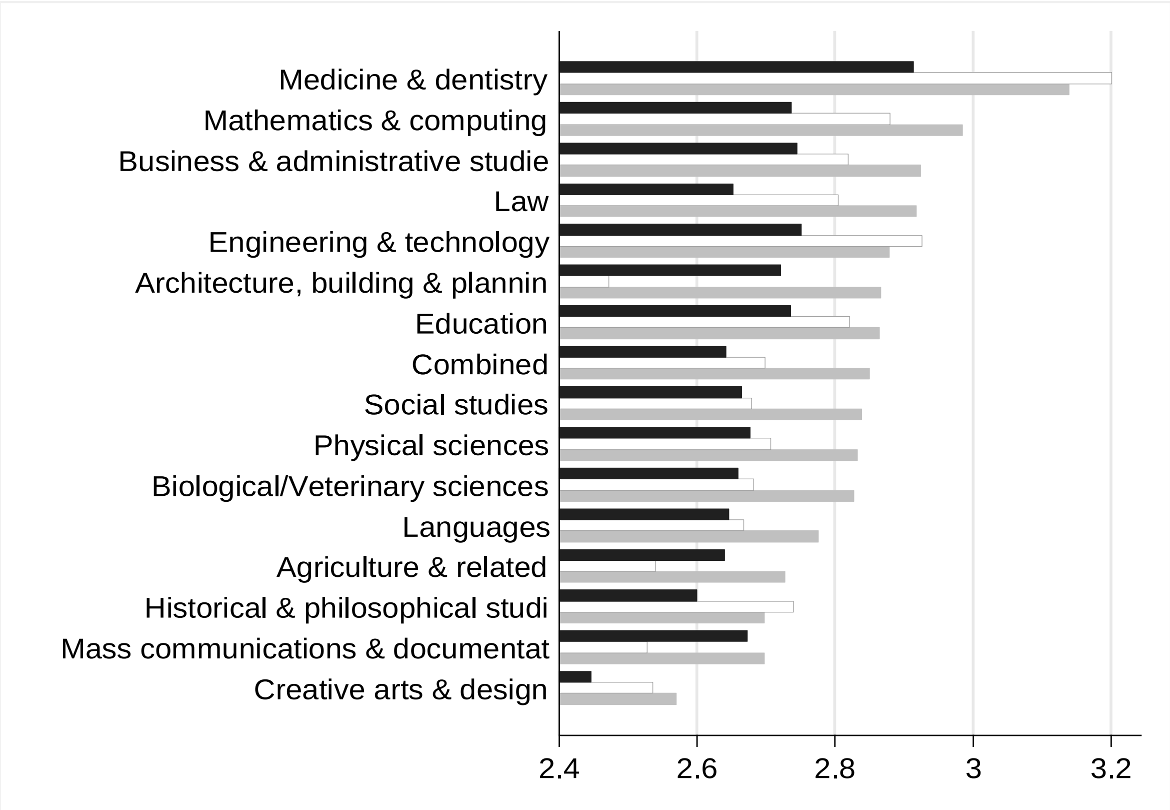
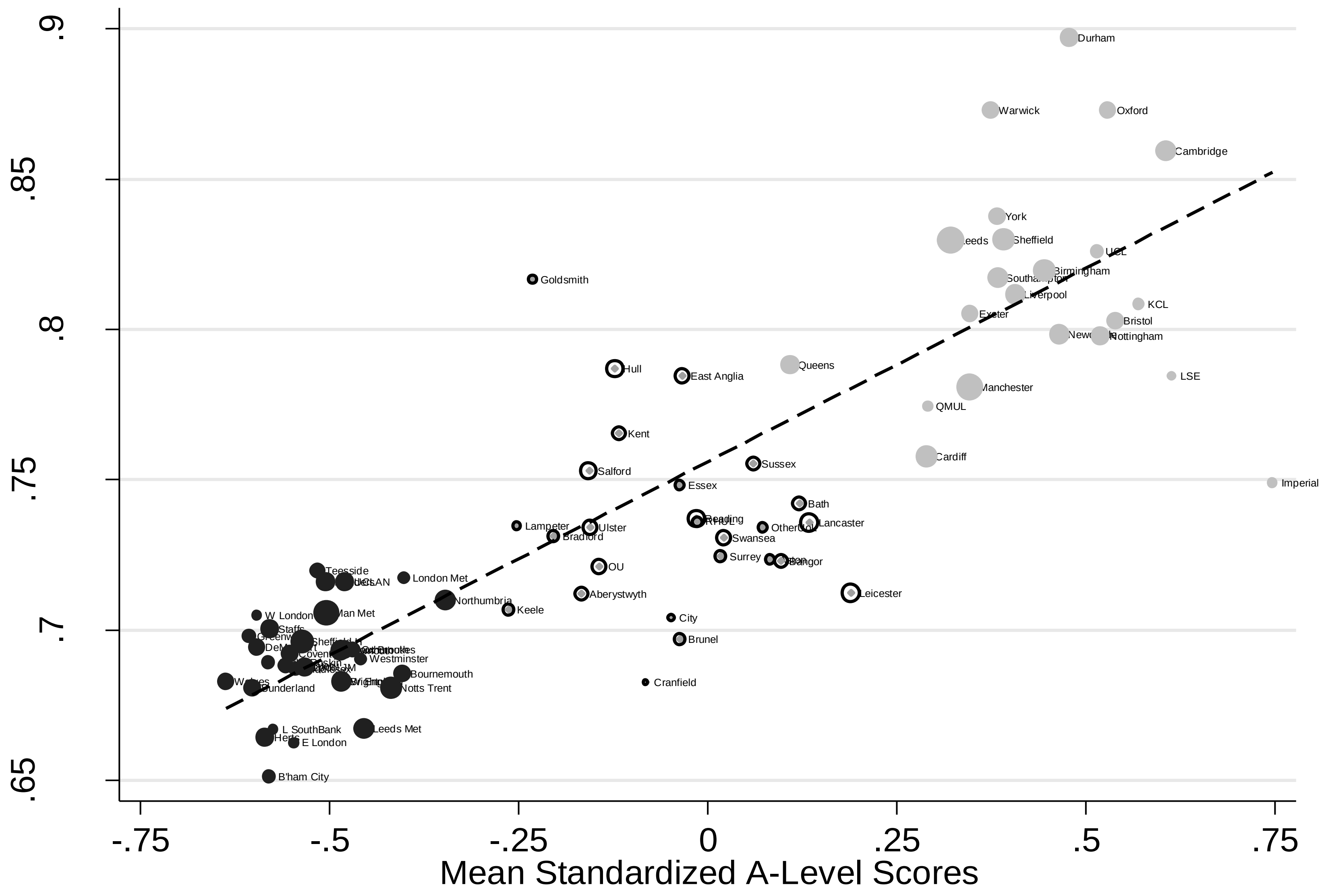


Figure 1B Log wages by subject and institution type: Women



Note: Full graduate sample (with cell size as in Table 1). Dark grey is New; hollow is Old; light grey is RG.

Figure 2: Standard Deviation and Mean A-Level scores by HEI, All subjects

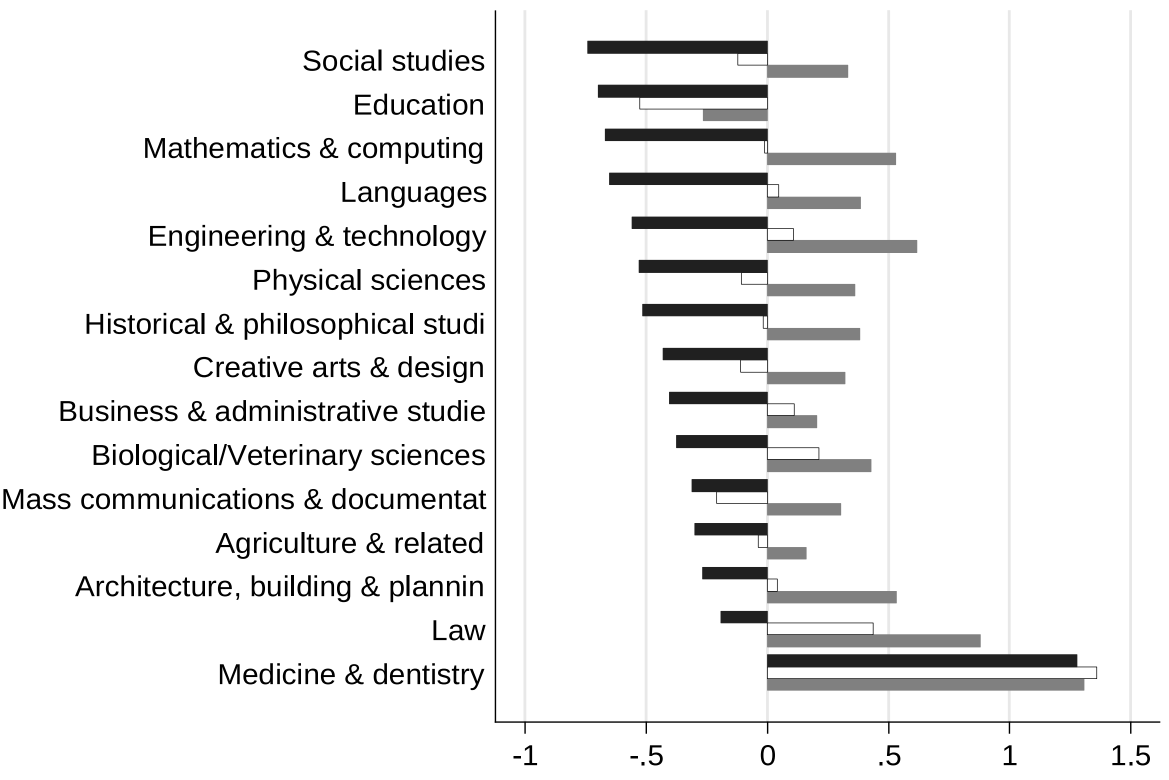


Note: Dark grey solid circle is New; Black hollow circle is Old; and Light grey solid circle is RG. Fitted line: slope=0.129 (Robust SE=0.009, R^2=0.775). Minimum cell size is 15.

inflation in admission scores, we use the detrended mean standardized A-Level scores from a subject-specific regression on a linear time trend, as our preferred measure of selectivity.[15]

Figure 3 shows the differences in the degree of selectivity by the three main subject groups, and by the three main institutional types. "New" universities are considerably less selective than the "Old" (by roughly half of a standard deviation) and, on average require modest grades in three A-level at around grades CCC. The "Old" are considerably less selective (by roughly another half of a standard deviation) than the "Russell Group" of 24 elite institutions that includes Oxford and Cambridge. Old institutions typically require 3 A-levels at around grades BBB; while RG institutions typically require three A-level subjects, at around grades AAA, and the very best of them demand three A* grades. Only in Medicine do all HEI types demand similar grades. Otherwise, RG and Old are usually much more selective than New.

Figure 3: Selectivity Scores by Subject and HEI Type



Note: Post-2002 cohorts with actual A-Level scores. Dark grey is New; hollow is Old; light grey is RG.

[15] To allow for A-level grade inflation across entry cohorts we detrend the standardized A-Level mean by running a regression on the (unadjusted) standardized A-Level mean with full interactions with subject dummies, where the interaction terms are jointly significant (p=0.000) as well as individually significant for many subjects. This suggests that overlooking subject-specific grade inflation will lead to biased estimates of the effects of selectivity.

Table 1 shows the cell sizes for each subject group broken down by HEI type and gender in the full sample of graduates. The merged LFS-HESA sample contains 10,602 graduates who entered HE in 1992 or later, with mean standardised A-Level entry scores matched at the HEI-subject (JACS) level for post-2002 UG entry cohorts.[16] For 1992-2001 UG entry cohorts, which is more than 70% of our data, we impute the missing A-Level scores and we subsequently examine the robustness of the results to including this extended sample.[17]

Table 1: Frequencies by subject, HEI type and gender, graduate sample

	Men				**Women**			
JACS Subject Area	**New**	**Old**	**RG**	**Total**	**New**	**Old**	**RG**	**Total**
Medicine & dentistry	10*	31	118	159	24	41	173	238
Biological/Veterinary	200	184	242	626	335	307	380	1,022
Agriculture & related	46	28	33	107	42	37	54	133
Physical sciences	189	248	431	868	135	133	211	479
Maths & computing	374	285	417	1,076	96	88	164	348
Engineering & tech	523	327	473	1,323	53	32	69	154
Architect/build/plan	247	55	87	389	69	13	51	133
Social studies	258	240	344	842	442	332	357	1,131
Law	117	61	131	309	207	118	136	461
Bus/admin studies	634	275	252	1,161	645	249	229	1,123
Mass comms & docs.	119	33	41	193	138	55	58	251
Languages	41	94	153	288	146	261	330	737
Historical/philosophic	83	168	226	477	102	140	272	514
Creative arts & design	260	76	67	403	338	103	102	543
Education	221	121	185	527	636	382	445	1,463
Combined	536	398	457	1,391	641	589	500	1,730
Total	3,858	2,624	3,657	10139	4,049	2,880	3,531	10460

Note: *: cell size rounded. The data is our full graduate sample (see sample note for details). New universities refer to ex-polytechnics which became universities post-1992. Old universities refer to universities founded pre-1992 which are not in the Russell Group (RG) of elite pre-92 institutions which form an association of 24 (as of 2012) public research-intensive universities, including Oxford and Cambridge.

[16] HESA data for 00/01-01/02 cohorts cannot be used because of inconsistencies in the tariff score calculations.

[17] We test for the robustness with respect to including of the 1992-2001 UG entry cohorts in Section 6. Pre-1991 cohorts are not used as they pre-date the major HE expansion which gave ex-polytechnics university status.

5. Empirical Methods

We begin with least squares estimation in Tables 4 and 5 of section 6 below, where the latter table controls for college selectivity. We conduct this exercise as a way of connecting ourselves to the existing literature that has depended heavily on least squares. The analysis that is implemented in Section 6 extends existing UK work by including the degree of college selectivity as a control variable in OLS. In addition, it conducts some robustness checks.

It seems likely that graduates' earnings will, to some extent, reflect their pre-university test scores – high scoring high school students will, on average, ultimately earn more. It also seems possible that students who attend different HEIs will ultimately earn different amounts and that this will, in part, reflect different admission requirements on HEIs as well as the individual student test scores. In particular, students with the same score but at different institutions might well earn different amounts. The regression analysis, even controlling for selectivity of institution and subject group, may still not be regarded as providing causal estimates. The OLS counterfactual depends on there being no unobservable confounders (i.e. selection only on observables) *and* a parametric functional form assumption (that the relationship between wages and A-level test scores was, for example, linear).

There may be unobserved confounders such as social background, non-cognitive skills, and personality traits. The usual approach to this problem is to: either search for instruments for the choices that individuals make; or to exploit discontinuities associated with admission requirements. In this application, the choice set is so large that it would be difficult to envisage a large number of instruments or discontinuities being available. Kirkeboen *et al* (2016) is the exception in the literature. Norway has just a handful of institutions – they are able to do precisely this because of the closely observed and rigid nature of the Norwegian HE system whereby admission to a course depends *only* on having at least the requisite high school score, and because their data records the next best alternative to the course each student was admitted to. In this UK analysis here, we have to rely on the assumption that there is no selection on unobservables – an assumption that is a necessary condition for OLS to yield unbiased estimates. However, OLS also requires that there is no other form of misspecification in the estimated relationship – for example, functional form. Matching methods circumvent the second requirement but still requires the former. Matching methods are, however, limited to binary treatments. Our application allows for multiple treatments, where we assume that selection into each treatment is driven only by observables. While not amenable to matching methods which is applicable only to a single binary treatment, this multiple treatment case can be addressed using weighted methods. Like matching methods, weighting can be used to ensure treatment groups are similar to the control group by weighting them accordingly. As with matching methods this weighting method can yield causal estimates of the Average Treatment Effects providing a conditional independence assumption is satisfied that implies that there is only selection on observables.

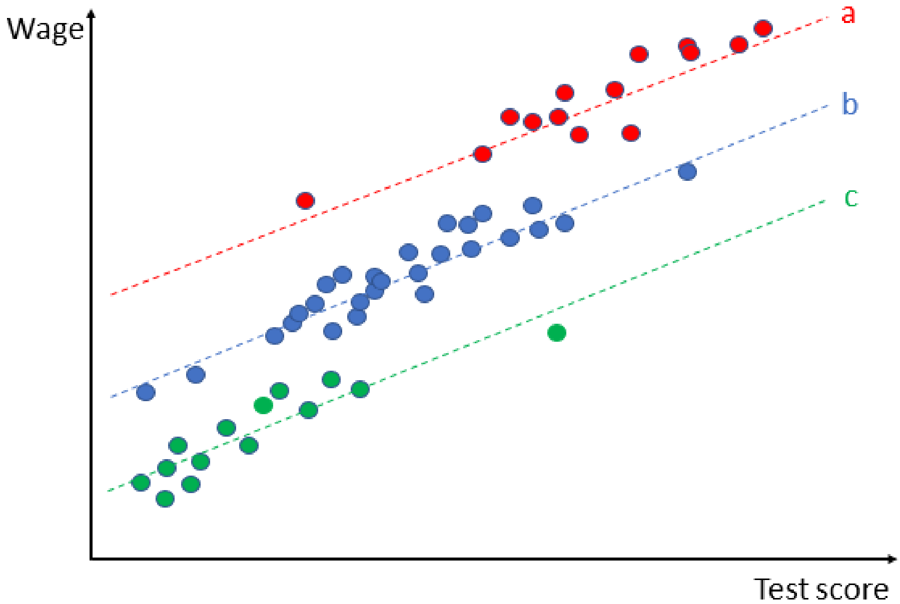
In section 7, we explore the treatment effects (assuming selection on observables alone) of HEI type and subject on log hourly wages, using the "doubly robust" inverse-probability weighted regression-adjustment (IPWRA) estimator (see Wooldridge 2007, Wooldridge 2010 chapter 13, and Imbens and Wooldridge 2009). In IPWRA the weights are the inverse of the predicted probabilities from a multinomial logit first stage that models the probabilities of each individual choosing each possible combination of subject and institution. In the second stage, this estimator then applies regression methods to the reweighted data. If students were randomly assigned to cells then the probabilities would be equal and the estimator is equivalent to unweighted least squares. On the other hand, if the researcher knows the true specification of the relationship between log wages and the X's, for example that it is linear in variables, then weighting observations arbitrarily will not affect the analysis. If the functional form is correct then the OLS estimates are unaffected by any weighting. However, since we seldom know the true functional form this is not very reassuring and relying on linearity to compare observations with others that fail to have overlap is unlikely to prove reliable. IPWRA weights observations in the sparse parts of the distribution *more* heavily. Imagine, for the sake of the exposition, that there are just three institutions (*a*, *b* and *c*) and just one subject (see Figure 4 below). Students apply to institutions before their test scores are known and suppose that the test score was a simple scalar figure. Institutions differ in terms of their "selectivity" (i.e. the test score level that their applicants require to gain admission). Individuals are made offers of a place at each of the institutions they apply to and these offers are conditional on test score achieved. Suppose that *a* is more selective than *b*, and *b* is more selective than *c*. Individuals are limited to apply to a small number of institutions (say just one for the purposes of this exposition). Some individuals who fail to make the critical test score to meet the condition of their offer from some institution might nonetheless gain a place at that institution – for example, this might be allowed if the institution finds itself with spare capacity, perhaps because many other students wanting this subject have also underperformed. If not, they might seek a place elsewhere, at a less selective institution, where spare capacity might exist. Thus, there may be a few students at highly selective institutions that have relatively low test scores. Equally, there might be some students who attend less selective institutions where they easily made the grade for admission but who had decided to attend this weaker institution for idiosyncratic reasons.

Therefore, there will be a distribution of test scores within institutions, as well as differences in the average scores across institutions. Importantly, the distributions of individual test scores at similarly selective institution are likely to overlap. For example, the lower tail of the distribution for institution *a* is likely to overlap with the upper tail of the distribution for *b*, and the upper tail of scores at *c* is likely to overlap with the bottom tail of the scores at *b*. Even thought there is no overlap between institutions *a* and *c,* the existence of *b* facilitates support for both a and *b.*

Had we omitted institution *b* the results would be highly dependent on the validity of the assumption that the relationship between wages and scores was additive and linear - i.e. that the relationships are parallel across institutions. If this assumption is true, as Figure 4 suggests, then the weight that we attach to each observation is irrelevant – OLS would provide unbiased estimates of slope and intercepts irrespective of how the observations were weighted. The additive linearity assumption is strong – but it allows us to extrapolate the effect of test scores on wages across the test score distribution, even to those parts of the distribution where data is sparse. If the functional form assumptions of linearity and additivity were correct we could rely on the differences in the estimated OLS intercepts in Figure 4 to provide measures of institutional relative value added. The essence of OLS is that all observations are equally weighted and that the counterfactual wage is obtained from a linear prediction.

One solution to the sparsity of data in the left and right tails for the top and bottom institutions is to attach greater weight to observations that do occur in the range where the data is sparse. This is what the Inverse Probability Weighting (IPW) method does. If we over-weighted the weakest students in the best institution then this would improve the overlap with all weaker institutions, and not just the next weakest one. Similarly, if we over-weighted the best students in the weakest institution we would also improve the overlap with all better institutions, not just the next worst. It is this property, that weighting improves overlap between all pairs of institutions, that gives IPWRA its advantage over estimation methods that rely on matching just pairs of institutions together. It is only by including intermediate institutions is it possible to estimate the effect of HEI and subject on wage outcomes across all cells. In

Figure 4: *IPWRA methodology*



Note: The dots indicate hypothetical students at institutions *a*, *b* and *c* respectively. The dashed lines fit the relationship between wages on test scores in a linear regression with additive institutional fixed effects.

particular, not including institution *b* would result in their being little common support between institutions *a* and *c*. This is the single robustness property – one that OLS also possesses. Figure 4 illustrates why this works. By increasing the weight of the weakest students in the top institution and the strongest in the bottom we can examine the validity of the assumption that one can linearly extrapolate from the dense part of the data. Figure 4 is drawn such that linearity is true and so the extreme individuals happen to lie close to the fitted linear line and therefore the weight given to them would make little difference to the estimates.

In practice, the weights are obtained by modelling the probability that an individual belongs to a particular cell – in our case defined by HE subject and institution. There are, in our data, very many cells and modelling such a large range of choices is impractical with the small survey data available since many cells will have very few observations within them. The natural way of modelling such choices is using a multinomial logit first stage. This provides predicted probabilities of being in a particular cell which can be used to change the contribution of each observation in the second stage where a weighted least squares regression is run.

Indeed, IPWRA is *doubly* robust in the sense that the estimates of the second step, the wage equation, are robust to misspecification in the weighting of the data conditional on the specification of the second step being correct; *and* the estimates of the second step are robust to misspecification of the second step provided the weighting step is correctly specified. That is, only one step needs to be specified correctly. IPWRA estimates the average treatment effect (ATE) of any HEI type and subject group relative to an omitted category allowing for selection into a particular HEI type and subject, relative to the omitted category, using multinomial logit model in the first step. Due to concerns of cell sizes and common support, we group the subjects into STEM, Social Sciences and Arts & Humanities.

6. Ordinary Least Squares Results

Before we turn to the effect of HE selectivity, we first present conventional wage equations as a benchmark in Table 2, for men and women separately, using the full graduate sample. In contrast to the results presented in Walker and Zhu (2013), where the LFS data did not include HEI, here we provide estimates that include HEI type differentials - which we could only provide in our earlier work from a separate survey.[18] In columns (1) and (3), we include HEI type, while in (2) and (4) we additionally control for degree subjects. We resist the temptation to control for PG qualifications, so these results should be interpreted as including the option value of the possibility of pursuing PG studies. Even though family circumstances such as partnership status and the number of children are correlated with wages and with HEI type and subject, we find that controlling for family circumstances made no significant difference to these estimates. The wage coefficients for attending Russell Group are robust at around 9% for men and 11% for women relative to New university graduates. Old university, relative to New, are similarly stable and are approximately 7% for men and 5% for women and these estimates are statistically significantly different to both RG and New. The subject differentials reflect those in Walker and Zhu (2013) with large positive effects for Medicine/Dentistry, Law, Business Studies, and Maths, relative to the Languages omitted subject; and large negative ones for other Arts subjects.

In order to assess the value-added of HEIs, we include a full set of HEI fixed effects in the results reported in Table 3 using the post-1992 entry sample with actual or imputed standardized A-Level scores,[19] as opposed to the HEI type indicators in Table 2. But we omit detailed subject controls for the moment because institutions would be dropped if they did not provide all

[18] In Appendix Table A1 we replicate the Walker and Zhu (2103) specification but we extend the sample and we are now able to add the selectivity variable. The results remain very similar to those in the 2013 report in terms of the effect of major, and these remain insensitive to the inclusion of additional controls.

[19] We exclude HEIs with fewer than 15 graduates in the post-1992 graduate sample. This resulted in a small reduction of the sample size to 10,602 instead of 10,627 as in Table 1. Table A2 compares results that use imputed selectivity for the graduates who predated our HESA selectivity data with results on the post-2002 sample only so imputation is not required.

subjects. We control for age, age squared, non-white, and regions but only report the coefficient for institutional selectivity. Controlling for HEI-fixed effects, a 1 SD increase in selectivity (roughly corresponding to the gap between RG and New universities on average) increases hourly wage by 0.08 log points for both men and women. This is only a little larger than previous estimates reflecting the young nature of graduates used in previous work[20].

Table 2: Wage equations without selectivity, PG or degree class controls

	MEN		WOMEN	
Old university	0.058***	0.065***	0.046***	0.046***
	(0.011)	(0.011)	(0.011)	(0.011)
RG university	0.099***	0.091***	0.119***	0.106***
	(0.011)	(0.011)	(0.011)	(0.011)
Medicine & dentistry		0.426***		0.448***
		(0.038)		(0.029)
Bio/Veterinary sciences		0.054*		0.081***
		(0.029)		(0.021)
Agriculture & related		0.012		0.009
		(0.051)		(0.040)
Physical sciences		0.102***		0.071***
		(0.029)		(0.025)
Mathematics & computing		0.172***		0.191***
		(0.028)		(0.029)
Engineering & technology		0.225***		0.149***
		(0.027)		(0.039)
Architecture, build & plan		0.142***		0.059
		(0.032)		(0.041)
Social studies		0.078***		0.065***
		(0.029)		(0.020)
Law		0.171***		0.145***
		(0.034)		(0.028)
Business & admin studies		0.208***		0.152***
		(0.028)		(0.021)
Mass comms & documents		-0.057		0.021
		(0.039)		(0.030)
History & philosophy		-0.050		0.002
		(0.032)		(0.026)
Creative arts & design		-0.045		-0.088***
		(0.033)		(0.024)
Education		0.052*		0.123***
		(0.030)		(0.019)
Combined		0.100***		0.060***
		(0.028)		(0.019)
Constant	-0.272***	-0.271***	-0.092	-0.084
	(0.099)	(0.101)	(0.089)	(0.088)
Observations	10137	10137	10460	10460
R^2	0.276	0.308	0.210	0.240

Note: Robust standard errors (RSE) in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Omitted category: New (post-1992); Languages; born in the 80's or 90's; survey year=2012. We do not report on variables not of direct interest: born in the 50's, 60's, 70's; year; age and age squared; non-white; and region effects.

[20] Experimenting with including higher moments of the selectivity scores we found them to be sometimes statistically significant, but they do not affect the coefficients of other key variables, or the R^2.

Table 3: Wage equations with HEI fixed effects and selectivity controls

	Male	Female	Pooled
Selectivity	0.078***	0.086***	0.084***
	(0.012)	(0.011)	(0.008)
Observations	4938	5664	10602
R^2	0.388	0.327	0.363

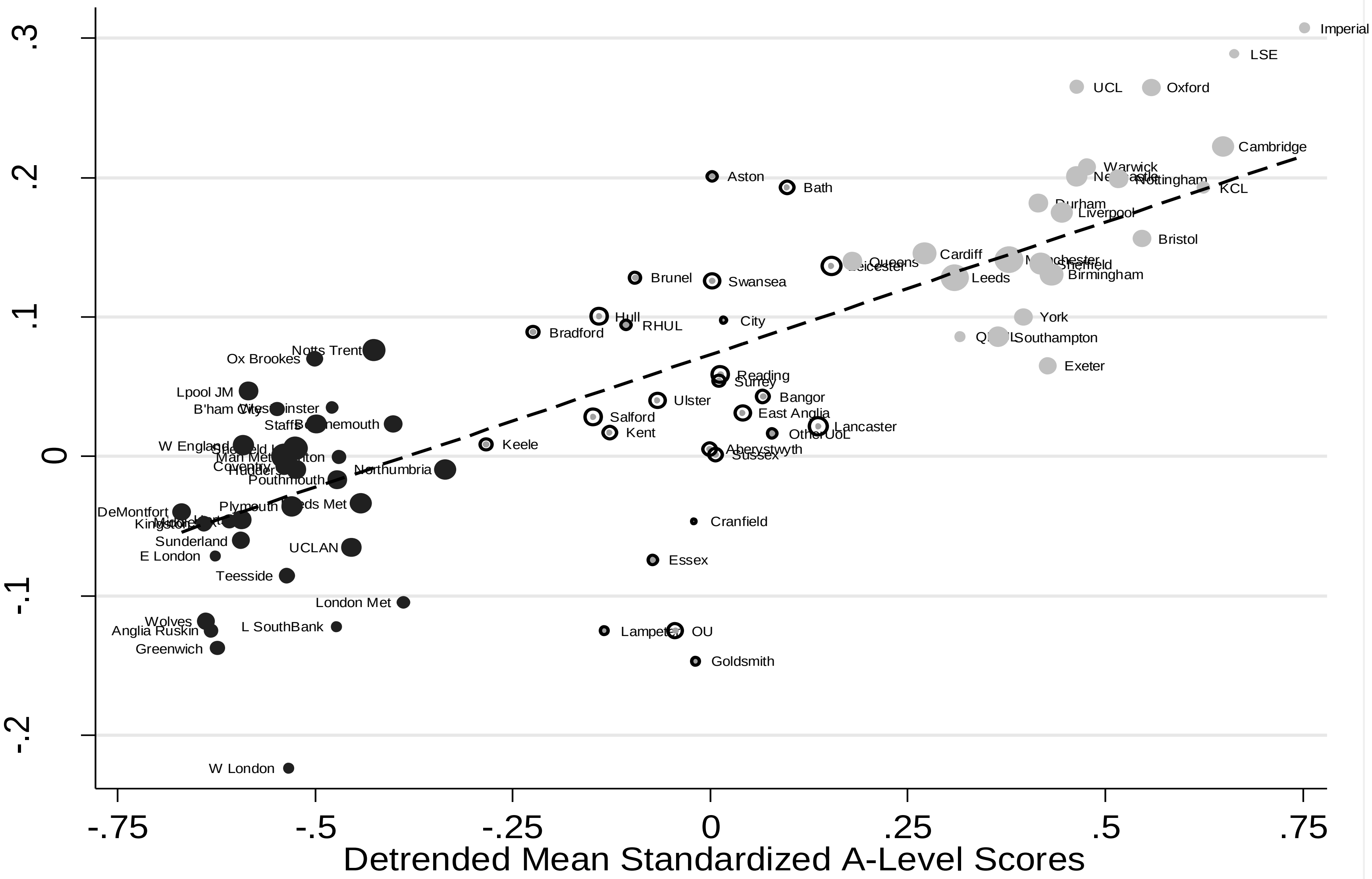
Note: SE in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Parameter estimates for age, age squared, non-white, regions, year, and cohort fixed effects are not reported. The HEI fixed-effect estimates are presented in Figure 7.

The estimated institutional fixed effects, for pooled men and women, from the estimation in Table 3 (using the pooled column estimates) are plotted in Figures 5 and 6. These present scatter plots of estimated HEI fixed effects (relative to MMU) unadjusted for selectivity (Figure 5) and adjusted for the degree of selectivity (Figure 6), for all subjects pooled together. The choice of MMU as the omitted category is entirely benign – any other choice would leave the relative effects unchanged. MMU is, nonetheless, a good numeraire because it is one of the largest HEIs, it offers a full range of subjects, and is close to the average level of selectivity.

Each dot represents a unique HEI fixed effect with a given mean A-Level admission score on the horizontal axis, and the mean unadjusted (for both HEI and subject) wage on the vertical axis; and the size of the bubble is proportional to the number of graduates from that HEI in the sample. In Figure 5, the A-Level scores on the x-axis, shows that graduates from more selective HEI's (that demand higher scores) earn significantly more than graduates of less selective HEI's that demand lower grades - the dashed black line, with a slope of 0.190 (robust standard error of 0.013), reflects a weighted least square regression of the unadjusted HEI fixed-effect estimates on standardized entry scores) – there would *appear* to be a large return to attending a more selective HEI.

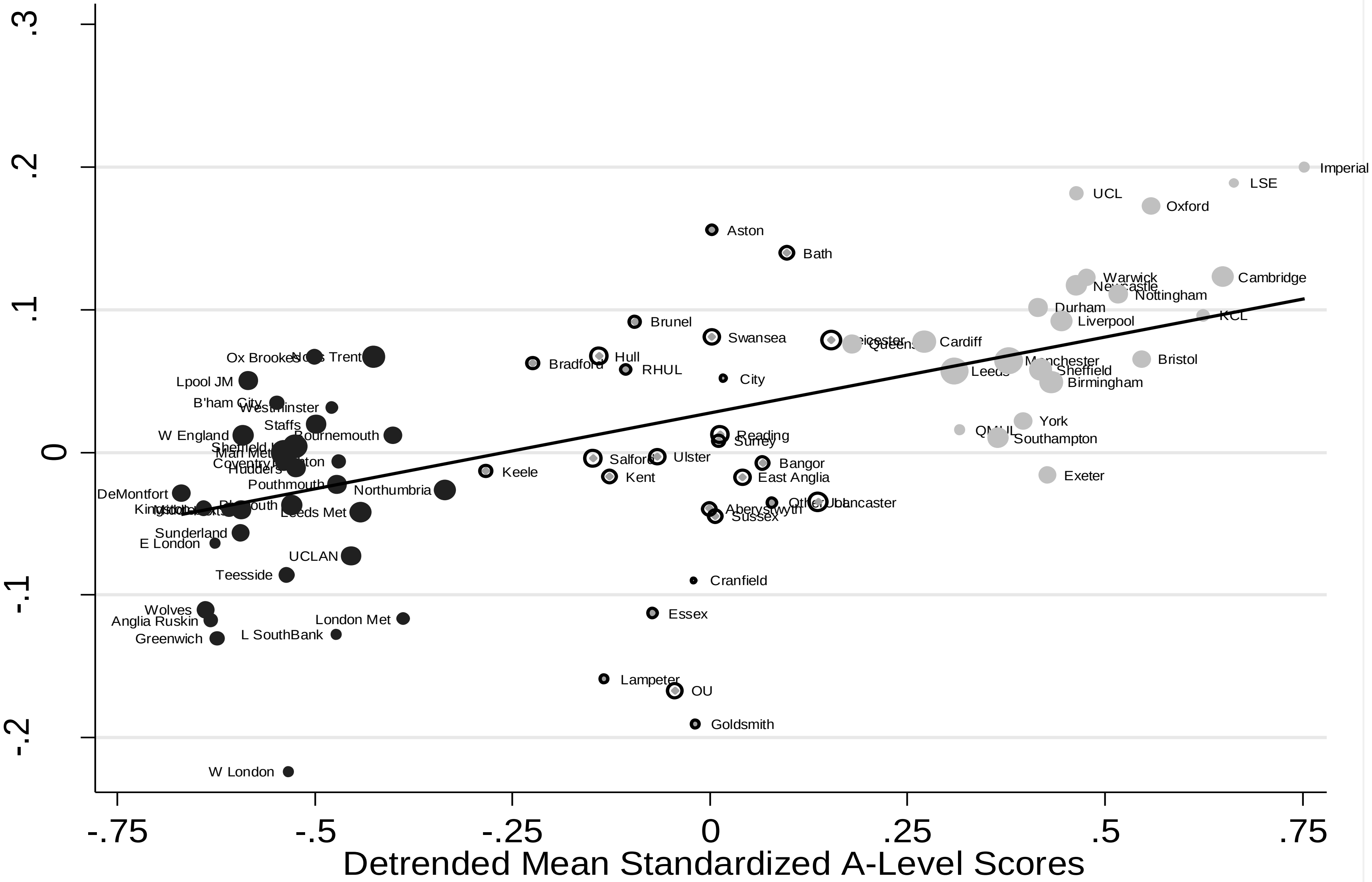
In contrast, when we control for course selectivity (using the pooled column estimates from Table 3) in Figure 6, we find much lower HEI fixed effects wage differentials, on average - the dashed fitted line is much flatter with a slope of 0.107 (Robust SE=0.013), reflecting a weighted least square regression of the effect of selectivity-adjusted HEI fixed-effect estimates on standardized entry scores. Failing to control for HEI selectivity gives the *mistaken* impression that more selective HEIs add considerably more value, when in fact approximately half of this is due to their greater selectivity.

Figure 5: ***Scatter plots of unadjusted HEI fixed effects***



Note: Dark grey is New; Black hollow is Old; and light grey is RG. The size of the bubble indicates institution size. Fitted line: slope=0.190 (rse=0.013), R^2=0.675. Omitted HEI: MMU.

Figure 6: Scatter plots of selectivity adjusted HEI fixed effects, all subjects



Note: Dark grey is New; black hollow is Old; and light grey is RG. The size of the bubble indicates institution size. Fitted line: slope=0.107 (rse=0.013), R^2=0.394. Omitted HEI: MMU.

The analysis above pools all subjects. Yet we know that there are considerable differences in selectivity by subject, as well as by institution, and there are some differences in subject mix across institutions that may confound the analysis. However, when we addressed both subject and HEI selectivity we found that including subject into the analysis seems not to affect the general message from contrasting Figures 5 and 6.

The further message of Figure 6 is that we estimate that there are large differences across institutions with similar levels of selectivity. For example, Exeter is 0.1 below the expected level of the fixed effect, while UCL has a fixed effect that is 0.1 higher than expected. A student that would expect to gain admission to Exeter would be considerably better off going to UCL because of its much higher value added (i.e. the estimated institutional fixed effect). Similarly, an average student at the very selective Cambridge would earn as much if she had attended the substantially less selective Aston.

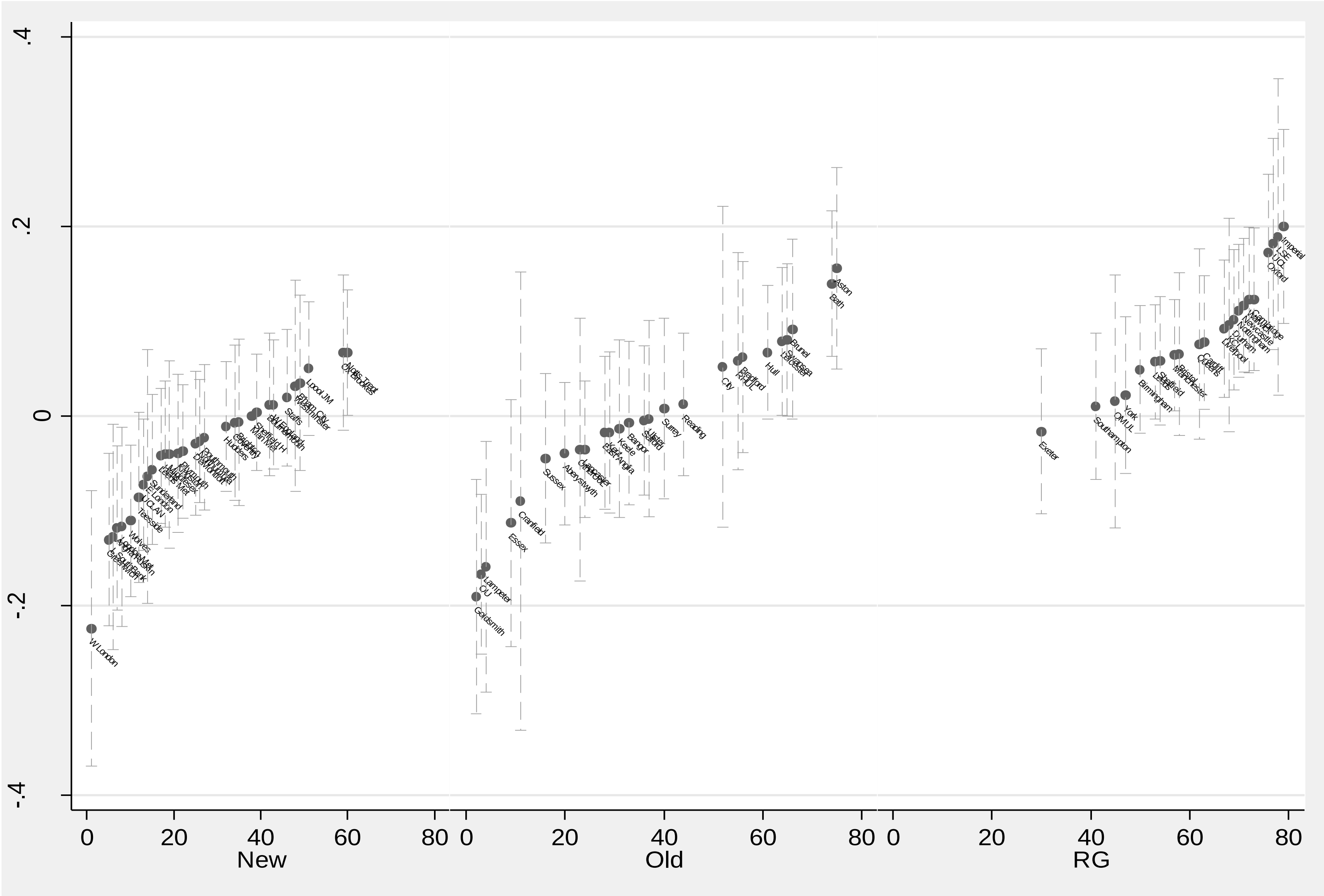
In Figure 7 we show the distribution of estimated relative (to MMU) value added **not** controlling for course selectivity, with the associated confidence intervals, ranked by value-added. The institutions are grouped from New, Old, RG across the figure.

The **overall** highest ten are Imperial College (London), London School of Economics, University College London, Oxford, Aston (a university in Birmingham whose main focus is management courses), and then Bath, Cambridge, Warwick, Newcastle, and Nottingham. Only Aston and Bath in this list are outside the RG elite institutions.

The **overall** lowest rated ten institutions include Essex which is a highly regarded "Old" institution.[21] Between these two extremes there are few institutions that are significantly different from zero (i.e. from MMU). The point estimates are relatively imprecise, but they suggest that more than 70% of the estimated institutional fixed effects lie within ± 10% of MMU and about 40% lie within ± 5% of MMU.

[21] The bottom 10 of the Old HEIs includes three other "Old" institutions that are unusual – Lampeter is a very small Welsh institution that offers a limited range of subjects; Goldsmith College is small and specializes in music; while the OU is large but caters mainly for atypical students who study part-time and almost entirely through distance learning.

Figure 7: Estimated Value Added Distributions, all subjects



Note: New institutions are portrayed in the block on the left; Old are in the middle block, and RG are in the rightmost block. The vertical axis is relative (to MMU) value added from the HEI fixed effects. The horizontal axis is the position in the overall rank of all HEIs – the gaps in each block demonstrate that the rankings overlap institution types. The vertical bars indicate 95% confidence intervals.

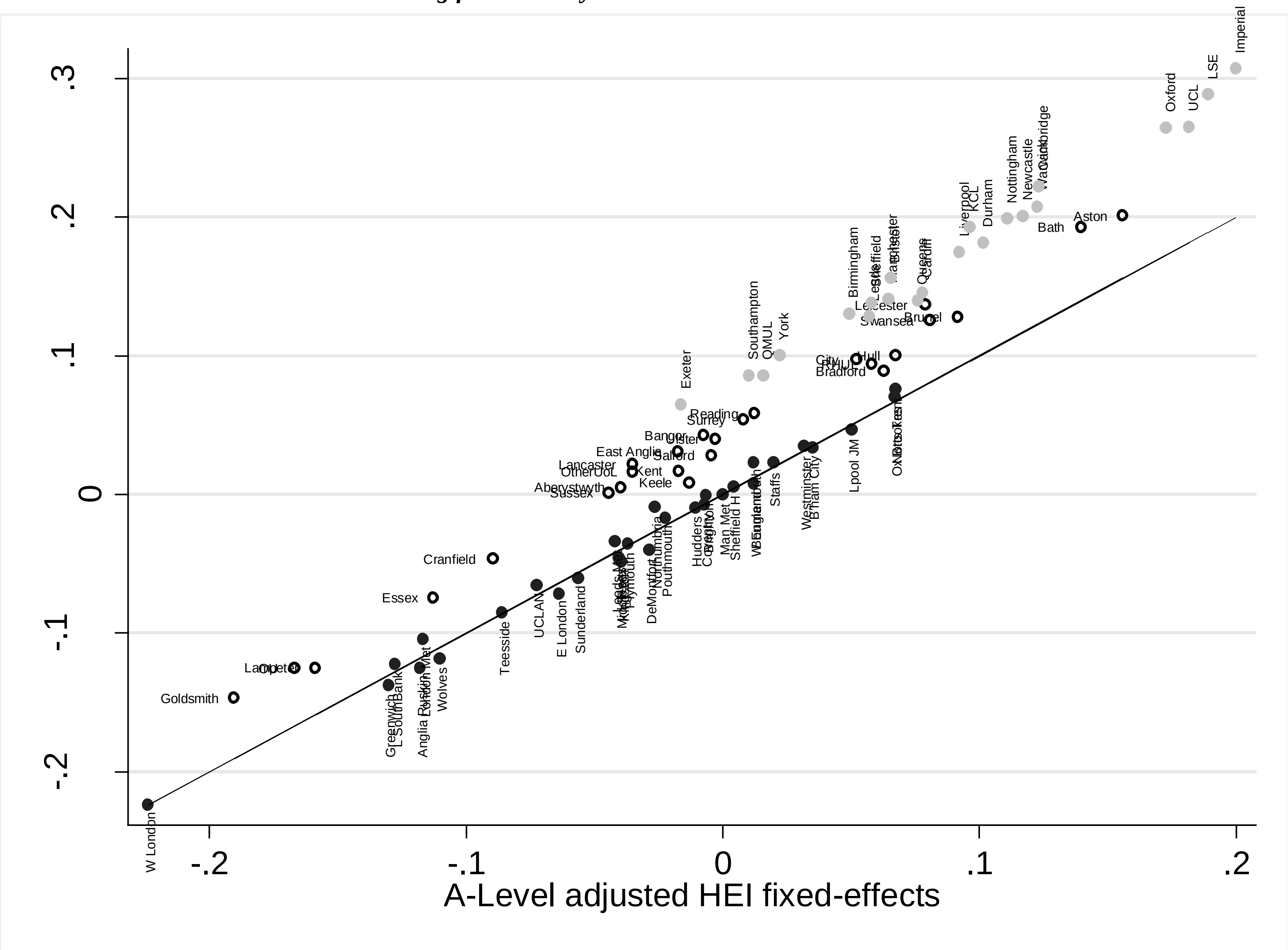
Finally, Figure 8 shows the scatter plot of the adjusted and unadjusted value-added figures (again all relative to MMU). It is very clear that the RG institutions have substantially lower value-added once one controls for their selectivity (i.e they lie above the 45^{o} line); while the difference between adjusted and unadjusted for Old universities is somewhat smaller; and the difference for New institutions is very close to zero. Note that the average wage differential between MMU graduates and non-graduates is of the order of 25% so the *absolute* graduate premium (i.e. the wage rate for marginal university graduates, who typically attend the New HEIs, vs high-school graduates who could, in principle, have attended a New HEI) is nonetheless positive, on average, for all New HEIs.

There are a number of robustness checks that we performed. 17.5% of graduates in our sample do not hold at least 2 A-Levels – many of whom have unconventional qualifications. So, we first check the robustness of estimated adjusted value-added distributions by excluding those without at least 2 A-Levels from the analysis. The pattern of results looks almost identical to those in Figures 5 and 6 reflecting the stability of the extent of institutional selectivity over time - in particular the effect of institutional selectivity remains similar whether we omit students who have unconventional qualifications or not.

Secondly, while Figures 5, 6, 7, and 8 are for all subjects, we find that decomposing each of these figures into three separate broad subject groups makes no important differences to the ordering, and the pattern is similar - with a small and similar group significantly better than MMU, another small and similar group significantly worse, and a large range of institutions that have value added insignificantly different from MMU in each subject. The implication is that subject selectivity affects all institutions more or less equally.

Thirdly, Table 4 assesses the sensitivity of returns to HEI types and subjects with respect to institutional selectivity, without and with controlling for post-graduate degrees (we group all such degrees together, and degree class (we code “First” and “Upper Second”, that around half of students achieve, as “good” compared to the lower classifications). This table corresponds to Table 3 for men, women and pooled respectively, but now with subject controls added and HEI fixed effects replaced by HEI-type controls. The wage premia for studying at Russell Group universities as opposed to New universities are substantial, for both men and women. Moreover, getting a good degree or PG qualification also carry significant wage premia. However, they do not appear to matter for the coefficients of A-Level scores or HEI types for both genders. This pattern implies that our key estimates of the HEI value added are insensitive to how we control for PG qualifications or UG degree class.

Figure 8: Value Added distributions controlling for selectivity and not.



Note: Dark grey is New, black hollow is Old, and light grey is RG. Minimum cell size (graduates per HEI) is 15

A final concern is that the effect of college selectivity might vary with the years of employment since graduation. This might arise if course selectivity generates some value as a signal – since employers know little about their employees' productivities soon after graduation, they may use college selectivity as a signal. We might expect the value of such a signal would diminish as new information about productivity is revealed by work experience. In the first 3 columns of Table 5 we replicate Table 4, but now adding years since graduation and its interaction to the selectivity measure. There seems to be no evidence of such an effect for men; and for women it even appears that a more selective degree has a value in the market that *increases*, albeit only slowly, with tenure. In the last 3 columns, we only allow the effect of selectivity to vary with age up to the median age in our sample (32 and 31 for men and women respectively). The primary finding is that the estimate of the effect of HEI selectivity using new graduates is much higher (0.08 compared to 0.045) for the younger group of men (but not women).

It is difficult to compare our results with those in the existing literature. The closest, and best, work comes from Britton *et al* (2016) which uses income tax records matched to student loan information that records institution and subject. Their data is effectively the population of loan recipients and the income data is annual taxable income. But the earnings history is for, at most, 10 years after graduation. Moreover, the names of institutions were regarded as confidential by the data providers and only a subset of RG institutions agreed to be revealed. They report median earnings (and self-employed income) and their Figures 9 and 11, for female and male respectively, show the same general shape as our Figure 7 with a long flat range with a small number of institutions in the top and bottom tails that are statistically significant. There is considerable overlap in their top 10 institutions and ours.

As far as subject is concerned, Britton *et al* (2016) report a hockey-stick of median earnings in which Medicine and Economics are considerably higher than a long shallow plateau of fields of study which is led by Engineering, Law, and Physical Sciences; while the left tail of this plateau is dominated by Arts and Humanities. Similar findings can be seen in US research, for example, by Altonji *et al* (2012) which uses the large US *American Community Survey* (ACS) that is similar to our LFS data. Course selectivity has been extensively studied in the US literature, but there are just a handful of estimates for the UK. Our estimates are approximately 10% for a 1 SD increase in selectivity – from Figure 6 and Table 3. This is slightly larger than other UK estimates although our analysis is for a much longer range on ages. Dale and Krueger (2019) provide an 6.8% estimate of the effect of what

Table 4: Wage equations without and with PG and degree class controls

	Men		Women		All	
	Subjects	PG & Good Degree	Subjects	PG & Good Degree	Subjects	PG & Good Degree
Course selectivity	0.078***	0.075***	0.071***	0.070***	0.075***	0.073***
	(0.013)	(0.013)	(0.012)	(0.012)	(0.009)	(0.009)
Old university	0.052***	0.049***	0.022	0.003	0.037***	0.026**
	(0.016)	(0.016)	(0.015)	(0.015)	(0.011)	(0.011)
RG university	0.052***	0.043***	0.082***	0.072***	0.069***	0.058***
	(0.015)	(0.015)	(0.015)	(0.015)	(0.011)	(0.010)
Female					-0.101***	-0.114***
					(0.008)	(0.008)
Good Degree (I/2I)		0.104***		0.087***		0.094***
		(0.011)		(0.011)		(0.008)
PG indicator		0.039***		0.114***		0.079***
		(0.014)		(0.013)		(0.009)
Constant	-0.819***	-0.942***	-0.991***	-0.978***	-0.814***	-0.858***
	(0.216)	(0.217)	(0.198)	(0.197)	(0.147)	(0.146)
Observations	4938	4938	5664	5664	10602	10602
R^2	0.390	0.402	0.334	0.351	0.372	0.386

Note: Robust SE in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Merged LFS/HESA graduate sample (see sample notes for details). Institutional fixed effects not reported. Omitted category: New (post-1992) universities; Lower Second Class or Below (2II) degree; Languages. We do not report the coefficients on subject which are similar to before.

Table 5: Wage equations with selectivity interacted with years since graduation

	Full age range			Below Median Age		
	Men	Women	All	Men	Women	All
Course Selectivity	0.045**	0.017	0.029**	0.080***	0.017	0.030**
	(0.021)	(0.019)	(0.014)	(0.015)	(0.019)	(0.014)
Years since graduation	0.031***	-0.006	0.010	-0.000	0.000	-0.000
	(0.011)	(0.010)	(0.007)	(0.002)	(0.002)	(0.001)
Years since graduation * selectivity	0.003	0.005***	0.004***	-0.001	0.005***	0.004***
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)
Old university	0.052***	0.049***	0.022	0.003	0.037***	0.026**
	(0.016)	(0.015)	(0.011)	(0.016)	(0.015)	(0.011)
RG university	0.044***	0.071***	0.058***	0.043***	0.071***	0.058***
	(0.015)	(0.015)	(0.010)	(0.015)	(0.015)	(0.010)
Female			-0.113***			-0.113***
			(0.008)			(0.008)
Good Degree (I/2I)	0.104***	0.088***	0.094***	0.103***	0.088***	0.094***
	(0.011)	(0.011)	(0.008)	(0.011)	(0.011)	(0.008)
PG indicator	0.037***	0.113***	0.078***	0.039***	0.113***	0.078***
	(0.014)	(0.013)	(0.009)	(0.014)	(0.013)	(0.009)
Constant	-0.278	-1.134***	-0.654***	-0.940***	-0.991***	-0.878***
	(0.319)	(0.291)	(0.216)	(0.249)	(0.211)	(0.160)
Observations	4938	5664	10602	4938	5664	10602
R^2	0.403	0.353	0.386	0.402	0.353	0.386

Note: Robust SE in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

is an approximately ½ SD of college entry SATs scores, that do not adjust for selection into college; while our unadjusted estimate implied by Figure 6 is close to 20% for a 1SD increase in selectivity. Thus, our estimate is higher. They also report the effects of college selectivity that control for average college entry scores and find statistically insignificant results, while our own equivalent estimates are close to 10%.

7. Inverse Probability Weighted Regression Adjustment estimates

It is clear from Table 1 that the data would not support the estimation of effects of a large number of cells defined by both subject and institution without relying on the additive linear functional form assumptions. Fortunately, there is a natural way of collapsing institutions into three distinct types: RG, Old and New. Similarly, there is a natural way of collapsing subjects into three types: STEM (Science, Technology, Engineering, and Mathematics), Social Sciences, and Arts & Humanities. The resulting nine cells (for males and females separately) have a minimum of 5% of their respective sample sizes of over 4000 graduates.

Table 6 presents the mean selectivity scores and frequencies for each of the 3 by 3 HEI type * subject group combinations, for men and women separately. It is clear that there is larger variation in A-Level scores across HEI types than across subjects, conditional on type. On the other hand, the variation in frequencies across gender reflects the differences in subject popularity, e.g. women appear to prefer Social Sciences and Arts & Humanities to STEM, relative to men. Indeed, the differences across gender in some cells suggests that men and women are choosing different subjects within those subject groups.

Table 6: Average selectivity across HEI type and subjects

	Men		Women	
	A-Level Mean	Obs	A-Level Mean	Obs
New – STEM	-0.474	801	-0.422	432
New - Social Science	-0.456	539	-0.476	722
New - Arts & Humanities	-0.434	313	-0.456	472
Old – STEM	0.074	485	0.175	321
Old - Social Science	0.015	286	0.043	362
Old - Arts & Humanities	-0.016	212	-0.016	280
RG – STEM	0.510	819	0.582	645
RG - Social Science	0.391	362	0.471	418
RG - Arts & Humanities	0.360	255	0.390	437
Total	-0.018	4,072	0.017	4,089

We proceed from the most restrictive case where we assume that the broad subject choice is effectively pre-determined at age 18, and students simply choose what HEI type to attend reflecting their view of what is appropriate for their ability and background. This seems like a natural restriction to place on the data since it reflects the reality that the subject choices are heavily restricted by decisions made at 16, while HEI type is selected at 18 according to one's perceptions of likely attainment in these school subjects. This, in Table 7 we report the effects of HEI type conditional on broad subject. The Average Treatment Effect from the IPWRA for men applying to STEM courses is close to 16% for both Old and RG compared to the omitted New HEI-type. For those men applying to Social Sciences, the ATE is about 8% for attending an Old HEI and 14% for a RG HEI relative to New. And for those men applying to Arts and Humanities courses the ATEs are not significantly different from New. These ATEs are somewhat larger when estimated by IPWRA than when estimated using OLS even though this allows for the effect of course selectivity. Surprisingly, we estimate negative effects for women of attending an Old HEI relative to New for STEM and for Arts and Humanities. It is not clear why these results should differ from those for men. We suspect that the subject mix within broad fields might be different between men and women and also between New and other HEIs. This will not become clear until we are able to acquire administrative data to boost cell sizes to allow more disaggregation.

Table 8 reports the ATE estimates of HEI type-subject combination (all relative to New-STEM) on wages as well as the corresponding OLS estimates. That is, it relaxes the restriction in Table 7 that subject is predetermined.

The full set of the IPWRA estimates for Table 8 for the control variables are reported in the Appendix Tables A3 and A4 for men and women respectively. Those tables include the estimates of both the outcome equations (wages) and the estimates of the determinants of the treatment effects, i.e. the multinomial logit estimates. The wage effects of institution and subject types in Table 8 suggest that OLS substantially *underestimates* the effect of attending the more prestigious HEIs for men. The effects of Old and RG STEM are not significantly different from each other, but they are approximately 15% greater than New-STEM. Using OLS for men we find no significant effect of Old-STEM relative to New-STEM and a somewhat smaller effect of RG-STEM. For women, Old and RG STEM are not significantly different than New-STEM. There appear to be no significant Arts and Humanities effects for men in RG or Old relative to New. Although, for men, RG-SocSci is large and significantly

Table 7: IPWRA of HEI type on wages, conditional on broad subjects

	Men			Women		
	STEM	Social	Arts &	STEM	Social	Arts &
OLS		Science	Humanities		Science	Humanities
Old	0.043*	0.069**	0.061	-0.060*	0.013	-0.024
	(0.023)	(0.031)	(0.039)	(0.031)	(0.029)	(0.031)
RG	0.120***	0.093***	0.043	0.052	0.040	0.047
	(0.025)	(0.035)	(0.044)	(0.032)	(0.033)	(0.034)
Selectivity	0.077***	0.063**	0.075**	0.145***	0.131***	0.049*
	(0.017)	(0.026)	(0.036)	(0.020)	(0.022)	(0.026)
Observations	2105	1187	780	1398	1502	1189
R^2	0.400	0.405	0.384	0.342	0.341	0.386
ATE IPWRA						
Old	0.156***	0.078**	0.018	-0.081**	0.028	-0.096***
	(0.033)	(0.031)	(0.034)	(0.041)	(0.028)	(0.033)
RG	0.162***	0.138***	0.058	-0.005	0.108***	-0.053
	(0.034)	(0.039)	(0.041)	(0.041)	(0.036)	(0.036)
Observations	2103	1187	765	1397	1502	1186

IPWRA stands for inverse probability weighted regression adjustment. Robust SE in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Omitted category: New Universities. Other controls include age, age squared, nonwhite, dummies for decade of birth, survey years, and regions. PG and good degree dummies only enter the outcome (wage) equations but not the treatment (selection) equations in the IPWRA.

Table 8: IPWRA and OLS estimates of HEI type and broad subjects on wages

	IPWRA Average Treatment Effects			OLS		
	Men	Women	Pooled	Men	Women	Pooled
	(1)	(2)	(3)	(4)	(5)	(6)
New	0.075***	-0.071**	-0.007	0.007	0.010	0.003
Social Science	(0.029)	(0.031)	(0.025)	(0.020)	(0.023)	(0.015)
New	-0.010	-0.050	-0.040	-0.155***	-0.065***	-0.106***
Arts & Humanities	(0.036)	(0.035)	(0.030)	(0.025)	(0.024)	(0.017)
Old	0.161***	-0.047	0.054*	0.038*	-0.053*	-0.001
STEM	(0.031)	(0.037)	(0.028)	(0.022)	(0.028)	(0.017)
Old	0.133***	-0.036	0.045	0.085***	0.032	0.055***
Social Science	(0.036)	(0.033)	(0.028)	(0.028)	(0.028)	(0.020)
Old	0.003	-0.156***	-0.077***	-0.090***	-0.121***	-0.110***
Arts & Humanities	(0.037)	(0.033)	(0.028)	(0.031)	(0.030)	(0.021)
RG	0.153***	0.039	0.102***	0.115***	0.071***	0.092***
STEM	(0.033)	(0.041)	(0.030)	(0.023)	(0.027)	(0.017)
RG	0.196***	0.049	0.108***	0.109***	0.070**	0.086***
Social Science	(0.041)	(0.038)	(0.031)	(0.027)	(0.029)	(0.020)
RG	0.027	-0.119***	-0.065**	-0.104***	-0.072***	-0.084***
Arts & Humanities	(0.039)	(0.038)	(0.032)	(0.030)	(0.028)	(0.020)
Female						-0.131***
						(0.009)
Selectivity				0.071***	0.123***	0.097***
				(0.014)	(0.014)	(0.010)
Observations	3950	4083	8138	4072	4089	8161
R^2				0.404	0.355	0.391

IPWRA stands for inverse probability weighted regression adjustment. Robust SE in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The observations which are off common support are excluded from the treatment effect models. Other controls include age, age squared, nonwhite, dummies for decade of birth, survey years, and regions. PG and good degree dummies only enter the outcome (wage) equations but not the treatment (selection) equations in the IPWRA specifications. We do not report the coefficients for selectivity in the IPWRA estimates because there is one coefficient for each potential outcome – too many to report. The same applies to the female dummy.

different to Old-SocSci, which in turn is significant greater than New-SocSci. The same is true for OLS estimates although these are again considerably underestimated relative to IPWRA. The OLS control for subject group and HEI type selectivity, but these findings are not sensitive to the exclusion of the selectivity control.

The results here are hard to compare with others in the literature. To date there is no other IPWRA work on either field of study effects or on institutional effects. The closest work is the Norwegian research by Kirkeboen *et al* (2016) which uses IVs based on multiple RDs and so control for selection on unobservables. Norway has the advantage of administrative data that covers the whole population together with a small number of institutions, and they consider a relatively small number of broad fields. The distribution of estimated institutional effects resembles our results earlier in that there is no significant difference across most institutions. Indeed, only NHH (in Bergen) is significantly better than the rest in Norway. Unlike the UK, which is small with many institutions within easy reach of the overwhelming majority of students, Norway is larger with relatively large distances between the major cities which limits choices somewhat. Thus, their work pays close attention to the subject studied relative to the next most preferred subject, whereas the vast majority of students will apply to the same subject at several places. Nonetheless, their estimated returns by subject look similar to our ranking.

8. Conclusions and Implications

We study the graduate *relative* wage premia in the UK – between subjects and between institutions. Our aspiration is to estimate value-added by institution and by subject. We do not directly consider the *absolute* returns – i.e. the wage differentials between college and no college[22]. Nonetheless the usual selection on unobservable problems pervade the analysis – although there is a good case for thinking that subject is predetermined by earlier choices in senior high school, there is no case for thinking that the same is true of institution. Unlike earlier UK studies, we are able to consider the effect of differences in UG degree subjects, in particular the selectivity of the subject at the institution attended. This is important, since earlier studies from the US (e.g. Loury and Garman 1995) have shown that omitting university

[22] See Blundell *et al* (2018) for the most recent work on the LFS data used here that focusses on the absolute return (i.e. the college premium) on average across all subjects/institutions, which shows that this has been remarkably stable across cohorts of students despite the massive expansion of the UK HE system that has dramatically increased the supply of graduates into the labour market.

performance might lead to biased estimates of the effects of college selectivity. Our results show that UG degree programme selectivity, as proxied by de-trended A-Level admission scores of the degree programme attended, plays an important role in explaining the variation in the graduate wage premium across HEI types and subjects. Moreover, the extent to which more selective institutions add value varies substantially by subject.

The primary finding is that there is a selectivity effect on wages in the UK. The corollary of this is that estimated value added for the elite institutions is exaggerated by least squares. This is an important finding because there is a strong policy emphasis in encouraging wider access to elite HEIs and these estimates suggest that the differences in value-added do not support such a strong emphasis because much of the observed wage differentials across graduates reflect their pre-HE ability. We subsequently estimate treatment effects (that are based on the assumption of selection on observables only) using the IPWRA method. Here we find strong differences by subject type as well as institution type for men. The returns to STEM exceed those to Social Sciences for both Old and RG institutions, but the effect of Arts and Humanities for both RG and Old are not significantly different from that for New.

While the research throws up new findings on the effects, and side-effects, of selectivity we cannot say what causes these findings. The fact that more selective institutions generate greater value-added on average might be due to a number of factors. A traditional explanation would point to signalling, but the large variance across similar institutions that we uncover would be difficult to fit into such an interpretation. An alternative explanation is that more research-intensive institutions add more value through turning students into higher productivity workers. Finally, it could be that it is the selection mechanism itself that generates strong peer effects.

Irrespective of the transmission mechanisms, the work is important for policy. The funding of HE in the UK has changed radically in the last two decades and the costs of study has been focussed increasingly on the students themselves through a combination of high tuition fees and a sophisticated income contingent loan scheme. Two notable aspects of the new system are that: there is very little variation in fees across subjects or institutions, and yet the costs of providing tuition varies greatly across subjects. UK HEIs are independent of government and are free to charge whatever they wish up to a maximum that has been set by the government. Almost all courses are priced at the same maximum fee: despite the fact that different courses have radically different financial returns on average; and despite the large differences in the costs of provision. The costs of providing some courses (Arts and Humanities and Social Sciences) are well below the tuition fee, while the costs of other courses (STEM and especially Medicine and Dentistry) exceed the tuition fee. Universities are engaging in extensive cross-subsidisation. There is other research which suggests that there is a strong social gradient in the subject and institution choices that students make. Our research suggests that these choices can have important long run consequences that would adversely affect social mobility. This adverse effect is exacerbated by the cross subsidies that are occurring. That is, low SES students are more likely to choose to attend institutions that offer lower value added, and they tend to choose courses that have lower value added. Perversely, these “bad” subject choices cross-subsidise the costs of tuition for high SES students who are more likely to choose higher value-added courses. Moreover, the means-tested nature of loan repayments implies that students who choose low return subjects/institutions are likely to repay a smaller proportion of their notional debt - which is ultimately covered by the taxpayer. That is, taxpayers are ultimately subsidising students to take low return subjects, whose tuition fees then cross-subsidise other students to take high return students. Such inequities and inefficiencies persist partly because students do not have good information about the consequences of their choices. This paper is a small contribution to alleviating that ignorance. Much more detailed work is required to quantify the consequences.

Of course, students choose their courses for many reasons and value-added might well be a small element of the drivers of their choices. Some subjects might be low value-added that make them a poor financial investment but generate large consumption benefits. The means-tested nature of the loan scheme provides important subsidies for high consumption / low investment courses and, to the extent to which students are aware of such subsidies, this might exacerbate this inefficiency.

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Appendix

Table A1: Weekly earnings equations without and with PG and degree class controls

	Men		Women		All	
	+ Subjects	+ PG & Good Degree	+ Subjects	+ PG & Good Degree	+ Subjects	+ PG & Good Degree
Selectivity	0.080***	0.076***	0.075***	0.074***	0.077***	0.075***
	(0.015)	(0.015)	(0.018)	(0.018)	(0.012)	(0.012)
Old university	0.041**	0.039**	0.015	-0.006	0.026*	0.015
	(0.018)	(0.018)	(0.022)	(0.022)	(0.015)	(0.015)
RG university	0.059***	0.052***	0.094***	0.082***	0.080***	0.069***
	(0.019)	(0.018)	(0.022)	(0.022)	(0.015)	(0.015)
Female					-0.265***	-0.278***
					(0.010)	(0.010)
Medicine & dentistry	0.434***	0.481***	0.457***	0.488***	0.456***	0.494***
	(0.065)	(0.066)	(0.048)	(0.050)	(0.039)	(0.039)
Biological/Veterinary sciences	0.015	0.035	0.063*	0.055	0.048	0.052*
	(0.053)	(0.053)	(0.036)	(0.036)	(0.030)	(0.030)
Agriculture & related	0.130	0.148*	0.022	0.034	0.065	0.076
	(0.082)	(0.081)	(0.065)	(0.066)	(0.052)	(0.051)
Physical sciences	0.122**	0.143***	0.081*	0.070	0.107***	0.109***
	(0.051)	(0.051)	(0.046)	(0.045)	(0.032)	(0.031)
Mathematics & computing	0.193***	0.216***	0.183***	0.180***	0.183***	0.196***
	(0.051)	(0.051)	(0.056)	(0.056)	(0.031)	(0.031)
Engineering & technology	0.231***	0.239***	0.208***	0.195***	0.238***	0.237***
	(0.051)	(0.050)	(0.065)	(0.065)	(0.031)	(0.031)
Architecture, building & planning	0.181***	0.180***	0.129**	0.104*	0.176***	0.162***
	(0.055)	(0.055)	(0.060)	(0.060)	(0.036)	(0.036)
Social studies	0.152***	0.165***	0.033	0.035	0.082***	0.088***
	(0.051)	(0.051)	(0.038)	(0.038)	(0.030)	(0.030)
Law	0.130**	0.147**	0.130***	0.126***	0.133***	0.134***
	(0.059)	(0.058)	(0.046)	(0.046)	(0.036)	(0.036)
Business & admin studies	0.211***	0.225***	0.199***	0.197***	0.204***	0.207***
	(0.051)	(0.051)	(0.037)	(0.037)	(0.029)	(0.029)
Mass communications & documentation	-0.057	-0.045	0.054	0.044	0.004	0.002
	(0.065)	(0.065)	(0.049)	(0.048)	(0.039)	(0.039)
Historical & philosophical studies	-0.039	-0.040	0.022	0.023	-0.009	-0.014
	(0.057)	(0.057)	(0.044)	(0.044)	(0.034)	(0.034)
Creative arts & design	-0.015	-0.005	-0.114***	-0.105***	-0.065**	-0.056*
	(0.056)	(0.056)	(0.041)	(0.041)	(0.033)	(0.032)
Education	0.141***	0.149***	0.136***	0.081**	0.142***	0.111***
	(0.053)	(0.054)	(0.036)	(0.037)	(0.030)	(0.031)
Combined	0.135***	0.156***	0.081**	0.106***	0.102***	0.123***
	(0.050)	(0.050)	(0.035)	(0.035)	(0.028)	(0.028)
Good Degree (I/2I)		0.113***		0.083***		0.096***
		(0.013)		(0.017)		(0.011)
PG indicator		0.019		0.130***		0.077***
		(0.016)		(0.018)		(0.013)
Constant	2.202***	2.051***	2.091***	2.124***	2.397***	2.349***
	(0.265)	(0.267)	(0.308)	(0.307)	(0.209)	(0.208)
Observations	4938	4938	5664	5664	10602	10602
R^2	0.362	0.372	0.178	0.188	0.290	0.298

Note: RSE in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Merged LFS/HESA graduate sample (see sample notes for details). Institutional fixed effects not reported. Omitted category: New universities; Lower 2nd Class; Languages.

Table A2: Robustness to the exclusion of imputed A-Levels, Pooled Men and Women

	Sample with actual A-Level scores only		Sample with imputed A-Level scores only	
	HEI FE & No A-Level	HEI Type & Mean A-Level	HEI FE & No A-Level	HEI Type & Mean A-Level
Age of respondent	0.193***	0.209***	0.190***	0.185***
	(0.049)	(0.048)	(0.014)	(0.013)
Age squared	-0.003***	-0.003***	-0.002***	-0.002***
	(0.001)	(0.001)	(0.000)	(0.000)
Non-white	-0.043*	-0.046**	-0.061***	-0.062***
	(0.022)	(0.022)	(0.019)	(0.018)
Born in 1970s			-0.022	-0.019
			(0.017)	(0.016)
Year 2013 = 1	0.048***	0.049***	0.039***	0.038***
	(0.017)	(0.017)	(0.013)	(0.013)
Year 2014 = 1	0.069***	0.066***	0.074***	0.073***
	(0.017)	(0.017)	(0.013)	(0.013)
Year 2015 = 1	0.102***	0.103***	0.071***	0.071***
	(0.019)	(0.019)	(0.016)	(0.016)
London	0.257***	0.259***	0.313***	0.305***
	(0.018)	(0.015)	(0.013)	(0.012)
Southeast	0.081***	0.093***	0.095***	0.087***
	(0.020)	(0.017)	(0.015)	(0.013)
Wales	-0.007	-0.027	-0.029	-0.033
	(0.037)	(0.034)	(0.028)	(0.025)
Scotland	0.033	0.055	0.035	-0.004
	(0.057)	(0.057)	(0.047)	(0.048)
Northern Ireland	-0.070	-0.122**	-0.153***	-0.145***
	(0.096)	(0.055)	(0.045)	(0.022)
Detrended Standardized A-Level mean score		0.063***		0.102***
		(0.013)		(0.010)
Female	-0.074***	-0.078***	-0.135***	-0.135***
	(0.012)	(0.012)	(0.010)	(0.009)
Old university		0.057***		0.011
		(0.018)		(0.013)
RG university		0.070***		0.068***
		(0.018)		(0.013)
Constant	-0.941	-1.159*	-0.959***	-0.809***
	(0.628)	(0.614)	(0.211)	(0.208)
HEI fixed effect	Yes	No	Yes	No
Observations	3087	3087	7515	7515
R^2	0.357	0.327	0.270	0.262

Note: Robust SE in parentheses, * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns (1) and (3) control for HEI fixed-effects, while columns (2) and (4) present parsimonious specification with HEI types dummies and selectivity measures only.

Table A3: Full set of IWPRA estimates of Table 8, Men

Outcome (wage) equations	**New-STEM**	**New-SocSc**	**New-Arts**	**Old-STEM**	**Old-SocSc**	**Old-Arts**	**RG-STEM**	**RG-SocSc**	**RG-Arts**
Age of respondent	0.213***	0.011	0.266***	0.103*	0.248***	0.128*	-0.012	0.404***	0.130*
	(0.065)	(0.065)	(0.051)	(0.057)	(0.087)	(0.076)	(0.056)	(0.071)	(0.069)
Age squared	-0.003**	0.001	-0.004***	-0.001	-0.003**	-0.001	0.001	-0.006***	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Non-white	-0.305***	-0.186***	-0.168*	0.165***	-0.038	-0.154	0.082*	0.125	0.021
	(0.081)	(0.055)	(0.096)	(0.060)	(0.108)	(0.149)	(0.047)	(0.087)	(0.078)
Born in 1970s	0.041	-0.182**	0.202**	-0.149*	-0.020	-0.052	0.099	0.203**	0.143
	(0.101)	(0.076)	(0.096)	(0.083)	(0.143)	(0.114)	(0.100)	(0.101)	(0.115)
Year=2013	0.113*	0.058	0.047	0.009	0.014	0.046	0.036	0.117	0.163**
	(0.060)	(0.048)	(0.076)	(0.045)	(0.088)	(0.076)	(0.060)	(0.099)	(0.073)
Year=2014	0.188**	0.144***	0.079	-0.041	-0.065	0.015	0.060	0.220***	0.118*
	(0.078)	(0.049)	(0.071)	(0.060)	(0.098)	(0.075)	(0.075)	(0.065)	(0.068)
Year=2015	0.098	0.081	0.206**	0.036	0.075	0.040	-0.001	0.279***	-0.007
	(0.082)	(0.056)	(0.086)	(0.060)	(0.104)	(0.095)	(0.092)	(0.070)	(0.079)
London	0.239***	0.544***	0.426***	0.274***	0.342***	0.351***	0.312***	0.256***	0.456***
	(0.070)	(0.049)	(0.055)	(0.065)	(0.061)	(0.059)	(0.061)	(0.068)	(0.060)
SE	0.058	0.291***	0.055	0.047	0.198***	0.101	0.036	0.191*	0.274***
	(0.045)	(0.065)	(0.069)	(0.054)	(0.061)	(0.085)	(0.068)	(0.110)	(0.104)
Wales	-0.120	0.141*	0.289***	-0.134	-0.144**	0.115	-0.088	0.119	-0.100*
	(0.137)	(0.072)	(0.067)	(0.086)	(0.071)	(0.101)	(0.076)	(0.084)	(0.052)
Scotland	0.167	0.432***	0.088	0.219	0.017	-0.188*	0.114	0.047	-0.076
	(0.156)	(0.076)	(0.129)	(0.154)	(0.248)	(0.102)	(0.116)	(0.406)	(0.157)
Northern Ireland	-	-	-	-	-	-	-	-	-
Selectivity	-0.086*	-0.002	0.243***	-0.016	0.044	-0.016	0.071**	-0.016	-0.076*
	(0.047)	(0.023)	(0.052)	(0.033)	(0.063)	(0.052)	(0.033)	(0.063)	(0.045)
Good Degree (I/2I)	0.168***	0.100***	0.110**	0.173***	0.140**	0.056	-0.006	0.095	0.047
	(0.050)	(0.038)	(0.053)	(0.041)	(0.063)	(0.057)	(0.066)	(0.061)	(0.063)
PG indicator	-0.049	-0.096*	0.004	0.032	0.061	0.087	0.156***	0.219***	0.017
	(0.041)	(0.058)	(0.064)	(0.054)	(0.067)	(0.059)	(0.060)	(0.053)	(0.061)
Constant	-1.577*	1.089	-2.030***	0.266	-1.877	-0.137	2.344***	-4.136***	-0.035
	(0.923)	(0.933)	(0.763)	(0.878)	(1.223)	(1.134)	(0.874)	(1.060)	(1.014)

Treatment equations	**New-STEM**	**New-SocSc**	**New-Arts**	**Old-STEM**	**Old-SocSc**	**Old-Arts**	**RG-STEM**	**RG-SocSc**	**RG-Arts**
Age of respondent		0.203	-0.164	-0.123	0.076	-0.202	0.219	0.178	0.064
		(0.156)	(0.179)	(0.170)	(0.195)	(0.216)	(0.179)	(0.205)	(0.223)
Age squared		-0.004	0.002	0.002	-0.002	0.003	-0.004	-0.004	-0.003
		(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)
Non-white		0.545***	-0.294	-0.043	-0.292	-0.691*	-0.225	-0.106	-1.882***
		(0.185)	(0.258)	(0.257)	(0.290)	(0.357)	(0.255)	(0.271)	(0.545)
Born in 1970s		0.575***	0.135	0.272	0.360	0.139	0.616**	0.256	1.155***
		(0.222)	(0.269)	(0.259)	(0.293)	(0.332)	(0.259)	(0.299)	(0.329)
Year=2013		0.021	0.023	-0.101	-0.169	-0.244	-0.153	-0.103	0.096
		(0.153)	(0.186)	(0.181)	(0.205)	(0.235)	(0.174)	(0.196)	(0.230)
Year=2014		-0.238	0.023	0.046	-0.028	-0.174	-0.215	-0.407**	0.253
		(0.160)	(0.187)	(0.180)	(0.200)	(0.224)	(0.179)	(0.204)	(0.228)
Year=2015		-0.121	-0.136	-0.044	-0.615**	0.063	0.044	-0.264	0.127
		(0.185)	(0.228)	(0.218)	(0.276)	(0.256)	(0.204)	(0.244)	(0.282)
London		0.261	0.618***	0.336*	1.205***	1.203***	0.408**	1.236***	1.079***
		(0.160)	(0.179)	(0.182)	(0.190)	(0.200)	(0.171)	(0.181)	(0.196)
SE		0.054	0.248	0.538***	0.655***	0.206	0.318*	0.329	0.218
		(0.157)	(0.179)	(0.167)	(0.198)	(0.239)	(0.164)	(0.203)	(0.223)
Wales		0.728*	-1.468	1.764***	2.049***	2.004***	1.299***	1.328***	0.654
		(0.388)	(1.032)	(0.389)	(0.398)	(0.425)	(0.392)	(0.468)	(0.599)
Scotland		0.586	0.659	0.231	1.360**	1.257**	0.525	0.160	1.011*
		(0.558)	(0.638)	(0.619)	(0.578)	(0.592)	(0.546)	(0.716)	(0.562)
Northern Ireland		-	-	-	-	-	-	-	-
Selectivity		0.192	0.302*	3.491***	3.158***	3.007***	5.296***	4.794***	4.722***
		(0.166)	(0.177)	(0.196)	(0.202)	(0.202)	(0.197)	(0.203)	(0.208)
Constant		-2.654	2.368	2.208	-0.979	2.725	-2.894	-2.481	-0.176
		(2.340)	(2.671)	(2.549)	(2.923)	(3.233)	(2.668)	(3.024)	(3.293)

Table A4: Full set of IWPRA estimates of Table 8, Women

Outcome (wage) equations	New-STEM	New-SocSc	New-Arts	Old-STEM	Old-SocSc	Old-Arts	RG-STEM	RG-SocSc	RG-Arts
Age of respondent	0.215***	0.249***	0.125**	0.241***	0.060	0.231***	0.249***	0.132	0.185***
	(0.050)	(0.046)	(0.051)	(0.058)	(0.053)	(0.058)	(0.088)	(0.084)	(0.060)
Age squared	-0.003***	-0.003***	-0.001	-0.003***	-0.000	-0.003***	-0.004**	-0.001	-0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
Non-white	-0.013	-0.061	-0.025	-0.025	-0.030	0.163**	-0.102	-0.305**	0.145
	(0.072)	(0.126)	(0.072)	(0.082)	(0.085)	(0.076)	(0.095)	(0.128)	(0.139)
Born in 1970s	-0.067	0.152*	0.101	-0.006	0.003	0.203**	0.148	-0.127	-0.084
	(0.080)	(0.087)	(0.077)	(0.104)	(0.110)	(0.087)	(0.151)	(0.106)	(0.114)
Year=2013	0.052	-0.035	0.034	0.097	-0.009	0.167***	-0.110	0.220**	-0.010
	(0.074)	(0.058)	(0.055)	(0.069)	(0.064)	(0.055)	(0.093)	(0.089)	(0.079)
Year=2014	0.115	-0.053	0.011	0.171**	0.161***	0.211***	-0.180**	0.170**	0.097
	(0.074)	(0.086)	(0.051)	(0.073)	(0.059)	(0.058)	(0.076)	(0.082)	(0.062)
Year=2015	-0.005	0.032	0.193***	0.049	0.052	0.279***	-0.045	0.225*	0.127
	(0.072)	(0.062)	(0.065)	(0.088)	(0.074)	(0.078)	(0.106)	(0.119)	(0.093)
London	0.223***	0.234***	0.350***	0.272***	0.240***	0.216***	0.206***	0.429***	0.382***
	(0.058)	(0.073)	(0.055)	(0.065)	(0.064)	(0.070)	(0.070)	(0.059)	(0.070)
SE	-0.012	0.140**	0.143***	0.077	0.070	0.128**	-0.087	0.142	0.130**
	(0.087)	(0.058)	(0.055)	(0.072)	(0.060)	(0.055)	(0.129)	(0.095)	(0.054)
Wales	-0.273	0.134***	0.022	0.070	0.032	0.110	-0.021	0.133	0.107
	(0.197)	(0.046)	(0.126)	(0.078)	(0.071)	(0.080)	(0.097)	(0.130)	(0.133)
Scotland	0.202	0.267	-0.030	-0.160	0.199	-0.393	-0.004	-0.027	0.073
	(0.221)	(0.340)	(0.072)	(0.227)	(0.182)	(0.243)	(0.123)	(0.080)	(0.122)
Northern Ireland	-0.534***	-0.028	0.063	-0.202*	-0.047	-0.169*	-0.183**	-0.148	-0.070
	(0.062)	(0.168)	(0.126)	(0.116)	(0.089)	(0.087)	(0.080)	(0.126)	(0.138)
Selectivity	0.302***	0.016	0.278***	0.003	0.122***	0.033	0.100	0.147***	0.038
	(0.038)	(0.022)	(0.041)	(0.043)	(0.039)	(0.045)	(0.071)	(0.032)	(0.045)
Good Degree (I/2I)	0.177***	0.080**	0.082**	0.030	0.233***	0.134***	-0.007	-0.013	0.328***
	(0.051)	(0.034)	(0.040)	(0.060)	(0.053)	(0.052)	(0.109)	(0.070)	(0.086)
PG indicator	0.215***	0.147***	0.108*	0.170***	0.203***	0.108**	0.027	0.033	0.047
	(0.050)	(0.051)	(0.062)	(0.054)	(0.055)	(0.048)	(0.082)	(0.056)	(0.058)
Constant	-1.400*	-1.822***	-0.187	-1.745**	0.777	-1.753**	-1.202	-0.267	-1.310
	(0.732)	(0.686)	(0.704)	(0.850)	(0.758)	(0.807)	(1.217)	(1.233)	(0.852)

Treatment equations	**New-STEM**	**New-SocSc**	**New-Arts**	**Old-STEM**	**Old-SocSc**	**Old-Arts**	**RG-STEM**	**RG-SocSc**	**RG-Arts**
Age of respondent		0.102	0.106	-0.086	0.022	0.281	0.461**	0.132	0.024
		(0.161)	(0.180)	(0.209)	(0.194)	(0.216)	(0.192)	(0.197)	(0.191)
Age squared		-0.002	-0.003	0.001	-0.001	-0.005	-0.008**	-0.002	-0.001
		(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)
Non-white		0.326	-0.578**	0.170	0.048	-0.557*	-0.443*	-0.325	-1.317***
		(0.199)	(0.259)	(0.267)	(0.247)	(0.311)	(0.261)	(0.266)	(0.325)
Born in 1970s		0.246	0.618**	0.277	0.362	0.438	0.181	0.145	0.319
		(0.244)	(0.278)	(0.334)	(0.303)	(0.327)	(0.301)	(0.312)	(0.313)
Year=2013		-0.216	-0.042	-0.169	-0.451**	-0.086	-0.175	-0.104	-0.209
		(0.174)	(0.188)	(0.219)	(0.210)	(0.223)	(0.202)	(0.218)	(0.211)
Year=2014		-0.317*	-0.236	-0.401*	-0.642***	-0.223	-0.515**	-0.188	-0.354*
		(0.175)	(0.192)	(0.217)	(0.211)	(0.224)	(0.204)	(0.216)	(0.210)
Year=2015		0.045	-0.187	-0.353	-0.293	-0.352	-0.319	-0.079	-0.174
		(0.205)	(0.231)	(0.269)	(0.248)	(0.280)	(0.241)	(0.259)	(0.249)
London		0.191	0.621***	0.411*	0.710***	0.656***	0.445**	0.877***	1.068***
		(0.185)	(0.198)	(0.240)	(0.216)	(0.230)	(0.207)	(0.211)	(0.204)
SE		0.140	0.018	0.862***	0.765***	0.842***	0.409**	0.370*	0.430**
		(0.169)	(0.191)	(0.202)	(0.201)	(0.209)	(0.196)	(0.215)	(0.203)
Wales		1.023	1.699**	3.408***	3.324***	3.263***	2.501***	2.693***	2.102***
		(0.785)	(0.778)	(0.768)	(0.764)	(0.775)	(0.779)	(0.787)	(0.808)
Scotland		-1.572***	-1.429**	0.051	0.150	-0.113	-0.804	-0.979	-0.114
		(0.577)	(0.660)	(0.554)	(0.488)	(0.557)	(0.563)	(0.761)	(0.587)
Northern		1.437	0.970	3.722***	4.421***	4.058***	3.804***	4.579***	3.820***
Ireland		(1.068)	(1.162)	(1.078)	(1.048)	(1.055)	(1.079)	(1.066)	(1.086)
Selectivity		-0.346**	-0.259	3.335***	2.769***	2.502***	4.865***	4.489***	4.150***
		(0.159)	(0.161)	(0.207)	(0.193)	(0.184)	(0.194)	(0.203)	(0.191)
Constant		-0.945	-0.513	1.393	0.359	-4.208	-6.499**	-2.254	0.129
		(2.377)	(2.630)	(3.041)	(2.824)	(3.193)	(2.811)	(2.903)	(2.812)