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A Question of Degree:
The Effects of Degree Class on Labor Market Outcomes

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Abstract
How does measured performance at university affect labor market outcomes? We show that degree class - a coarse measure of student performance used in the UK - causally affects graduates' industry and hence expected wages. To control for unobserved ability, we employ a regression discontinuity design that utilizes rules governing the award of degrees. A First Class (Upper Second) increases the probability of working in a high-wage industry by thirteen (eight) percentage points, and leads to three (seven) percent higher expected wages. The results point to the importance of statistical discrimination, heuristic decision making, and luck in the labor market.

Keywords: High skill wage inequality, regression discontinuity design, statistical discrimination
JEL Classifications: C26, I24, J24, J31

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1 Introduction

How does measured performance at university affect labor market outcomes? In this paper we estimate the causal effect of degree class on graduates' labor market outcomes. In the United Kingdom (UK) and other Commonwealth nations, degree class is used as a coarse measure of performance in university degrees, and its importance is highlighted by the sizeable fraction of employers who report using the classification in hiring decisions.[1] It is not obvious that the classification system is useful because degree transcripts provide more information about applicant quality. However, detailed transcripts may be difficult to interpret and to compare across students, leading employers to rely instead on the much simpler, five-step degree class measure when screening applicants. Given the coarseness of degree class, this may in turn lead to mismatch in the graduates labor market.

Identifying the effects of degree class is complicated by the fact that a naive comparison of, say, students who received a First Class with students who received an Upper Second could be biased by the differing ability composition of the two groups. To isolate the casual effect of degree class we need to approximate an ideal experiment and randomly assign degree class across students.

Using survey and administrative data from the London School of Economics and Political Science (LSE), we adopt a fuzzy regression discontinuity design (RD) which utilizes institutional rules governing the award of degree class. Undergraduates at the LSE typically take nine courses over three years. Every course is graded out of 100 marks and fixed thresholds are used to map the marks to degree class. A First (Upper Second) Class Honors degree requires at least four marks of 70 (60) or above.[2] We use the discontinuous relationship between degree class and marks received on the fourth highest mark in our RD. This amounts to comparing students who barely made and barely missed a degree class within a narrow window of the marks received. We argue that this generates quasi-experimental variation needed for clean identification of degree class effects.

We find that higher degree classes positively affect a graduate's probability to work in a high-wage industry six months after graduation.[3] A First Class increases the probability of working in a high-wage industry by thirteen percentage points relative to an Upper Second. The corresponding estimate for an Upper Second, relative to a Lower Second, is eight percentage points. These effects translate into sizeable differences in expected wages. As we do not observe graduates' wages directly, we use information from the Labour Force Survey (LFS) to assign wages by industry, gender, and year. A First Class is worth roughly three percent in expected starting wages. An Upper Second is worth more on the margin—seven percent in expected starting wages. These results are robust to a battery of specification checks.

Our results suggest that employers rely on degree class when forming beliefs about graduates' abilities. This is despite the fact that a graduate's detailed exam grades are typically available to the employer as well—it appears that it would be too costly for employers to process and exploit this richer source of information.

In further results, we find heterogenous effects by gender and by mathematical content of degree

[1] Degrees are classified as First Class, Upper Second Class, Lower Second Class, Third Class, and Pass. This coarse measure of performance stands in contrast to the much more detailed GPA measure used in the US. Degree class is also used by universities to screen applicants to postgraduate programmes.

[2] In terms of letter grades, a mark of 70 or higher would correspond to an A, while a mark between 60 and 69 would be a B.

[3] High-wage industries are the top 20 percent (un-weighted) industries in our sample by log wages averaged across genders and years.

programmes. A higher degree class increases the probability of working in a high-wage industry and expected wages by more for males and for graduates of mathematical degree programmes.

We argue that a standard model of statistical discrimination can explain our findings of heterogenous effects. In this model, employers cannot observe an applicant's productivity directly. However, they can use signals such as degree class to form beliefs about productivity. Employers attach more weight to the degree class signal when the variance in ability is larger. We provide suggestive evidence that ability variance is higher among males and students of mathematical programmes in our sample.

The paper is related to the literature on the effects of performance in degrees on labor market outcomes. Using a differences-in-differences strategy, Freier, Schumann, and Siedler (2014) estimate a 14 premium for graduating with honors among German law graduates. In papers most closely related to this paper, Di Pietro (2010), Ireland, Naylor, Smith, and Telhaj (2009) and McKnight, Naylor, and Smith (2007) examine the effects of degree classification for students in the UK. Notably Di Pietro (2010) adopts a regression discontinuity design using final year marks and finds no effect on employment. We get similar results on employment but extend the analysis by looking at wage differences. Ireland, Naylor, Smith, and Telhaj (2009) use OLS regressions and find 4 and 5 percent returns to First Class and Upper Second degrees respectively. Their sample consists of a much larger dataset of UK students across many universities and years but does not have the course history information we have to construct finer comparison groups.

Although concerned with university rather than high school graduation, our paper is similar to Clark and Martorell (2014) in using the discontinuous relationship between exam scores and the probability of receiving an educational credential to implement a regression discontinuity design. Clark and Martorell (2014) estimate a zero effect of high school diploma receipt by comparing workers who barely passed to those who barley failed high school exit exams in the US.

The aforementioned literature interprets earnings differences associated with degree class or high school diplomas as pure signaling effects as in Spence (1973). We hesitate to follow this interpretation. We believe that our empirical strategy and similar ones in the literature are capable of establishing the existence of information frictions in the labor market, which give rise to statistical discrimination. But these strategies do not necessarily distinguish between pure signalling and human capital theories of education. Evidence in favour of statistical discrimination is not necessarily evidence in favour of signalling. Statistical discrimination simply says that employers expect higher productivity if an applicant has obtained some credential. The theory does not speak to the sources of these average productivity differences. In our context, it is conceivable that graduates with higher degree classes accumulated more human capital on average, since doing well in exams typically requires studying more.[4]

Our findings contribute to a literature that documents the importance of simple heuristics for decision making in real world settings. Anderson and Magruder (2012) find substantial effects of Yelp.com ratings on restaurant reservation availability. The ratings are rounded to the nearest half-star. While the true average score is not shown, consumers could in principle calculate the score based on the individual reviews. Busse, Lacetera, Pope, Silva-Risso, and Sydnor (2013) find that prices for used cars drop discontinuously at 10,000-mile odometer thresholds, implying that consumers pay significantly more

[4] If an RD design estimates a precise zero effect of some credential (as in Clark and Martorell (2014)), then this is evidence against the credential in question being a signal in the labor market.

attention to the first digit than to subsequent ones. In our setting, employers observe the same detailed exam results that we use in our regression discontinuity design. However, our findings show that they do not regularly make use of this information.

Our paper also contributes to the literature that documents how sheer luck can affect labor market outcomes. Indeed, our identification strategy relies on the fact that a single mark out of one hundred can make the difference between say a Lower and an Upper Second degree, which we find greatly affects the probability of starting one's career in a high-wage industry. The effect would be exacerbated if initial earnings differences persist due to path dependence in graduates' careers. Rather than focussing on idiosyncratic risk, existing literature has investigated the role of aggregate risk. Oreopoulos, von Wachter, and Heisz (2012) document substantial earnings losses associated with graduating during a recession for university graduates in Canada. Oyer (2008) shows that stock market conditions while MBAs students are still in school affect their decision whether to work in the finance industry. Oyer (2006) finds that macroeconomic conditions at the time of graduation affect job characteristics in the short and long run for economists.

The rest of the paper is organized as follows. In Section 2 we discuss the institutional setting, in Section 3 we explore the data sources and empirical strategy, in Section 4 we present our results and provide an explanation. Section 5 concludes.

2 Institutional Setting

Our data come from the London School of Economics and Political Science (LSE). LSE is a highly ranked public research university located in London, UK, specializing in the social sciences. Admission to LSE is highly competitive and it offers a range of degree programmes. In 2012, LSE students came top for employability in the UK in the Sunday Times University Guide. Thus, our results speak to the high end of the skills market.

The degree classification system in the UK is a grading scheme for degrees. The highest distinction for an undergraduate is the First Class honors followed by the Upper Second, Lower Second, Third Class, Pass and Fail degrees. While all universities in the UK follow this classification scheme, each university applies its own standards and rules to determine the distribution of degrees. A similar system exists in other Commonwealth countries including Australia, Canada, India and many others. In the US, a system of Latin Honors performs the similar purpose of classifying degrees. In principle, this implies that our results apply to a broad range of countries.[5] Anecdotal evidence points to the importance of degree class in hiring decisions. One report found that 75 percent of employers in 2012 required at least an Upper Second degree as minimum entry requirement.[6]

[5] In the US, the grade point average (GPA) system is also used. This is usually a scale from 0 to 4 with one decimal accuracy and is a finer measure of performance than the UK system. There have been calls to scrap the UK system in favor of a GPA system, see "Degree classifications: time for a change?", the Guardian, July 9th 2012, available at `http://www.guardian.co.uk/education/2012/jul/09/degree-classsifications-change`. More recently, a group of UK universities have decided to experiment with a more detailed letter grading scheme, see "Universities testing more detailed degree grades", BBC News, September 25th, 2013, available at `http://www.bbc.co.uk/news/education-24224617`.

[6] See "Top jobs 'restricted to graduates with first-class degrees' ", the Daily Telegraph, July 4th 2012, available at `http://www.telegraph.co.uk/education/educationnews/9373058/Top-jobs-restricted-to-graduates-with-first-class-degrees.html` and "Most graduate recruiters now looking for at least a 2:1", the Guardian, July 4th 2012, available at `http://www.guardian.co.uk/money/2012/jul/04/`

In our identification strategy, we use a unique feature of the rules governing the award of degree class. Undergraduates in the LSE typically take nine courses over three years. Every course is graded out of 100 marks and fixed thresholds are used to map the marks to degree class. As shown in Appendix Table A.1, a First Class Honors degree requires 5 marks of 70 or above or 4 marks of 70 or above with aggregate marks of at least 590. This mapping from course marks to final degree class applies to all departments and years.[7]

We use the discontinuous relationship between degree class and marks received on the fourth highest mark in a fuzzy regression discontinuity design (RD). We employ a fuzzy, as opposed to a sharp, regression discontinuity because the receipt of the degree class also depends on aggregate marks, as shown in Appendix Table A.1. Our strategy amounts to comparing otherwise similar students who differ only in a critical course mark that determines their final degree class.

To be specific, let us consider the award of a First Class degree that depends on the receipt of at least four first class marks. This suggests that the fourth highest mark for any student plays a critical role in determining the degree class. A student whose fourth highest mark is higher than 70 is about twice as likely to obtain a First Class degree as a student whose mark just missed 70, everything else equal. This is seen clearly in Figure 1 which plots the fraction of students who receive a First Class degree against their fourth highest mark received. There is a jump in the probability of receiving a First Class after the 70-mark threshold. A similar story is seen in the award of an Upper Second degree at the 60-mark threshold. To summarize, the fourth highest mark plays the role of the assignment variable in our RD strategy.[8]

By the rules for awarding degree class as summarized in Appendix Table A.1, necessary conditions for a First and Upper Second are that the fourth best mark be no less than 70 and 60, respectively. However, Figure 1 reveals that non-negligible fractions of students who miss these necessary conditions by a few marks do receive Firsts or Upper Seconds, likely due to discretion exercised by departments in borderline cases. While we do not view this as a threat to the validity of our RD design, we check that our results are robust to excluding marks around the discontinuity.

Our claim that students close to either side of the relevant mark threshold are comparable in terms of observed and unobserved characteristics relies on some additional institutional features of grading at LSE. Exams are graded *anonymously*. Undergraduate courses at LSE are large enough so that it is very unlikely for graders who taught the course in question to be able to identify students by their handwriting.[9] This means that graders are not influenced by any knowledge of a student's ability other than what is written on the exam script.

We can also rule out the possibility that the fourth best exam is graded in a different way than other

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`graduate-recruiters-look-for-21-degree`.

[7]Four courses are taken each year, however only the average of the best three courses in the first year counts towards final classification. Undergraduate law students are an exception and follow a different set of rules. We exclude them from all analyses. Full details of the classification system is available online at the LSE website, `http://www.lse.ac.uk/resources/calendar/academicRegulations/BA-BScDegrees.htm`.

[8]Appendix Table A.1 suggests another possible RD design that uses the sum of marks as the assignment variable, with the probability of receiving Firsts and Upper Seconds increasing discontinuously at 515 and 590, respectively. We found that this yields a weaker first stage than when using the fourth highest mark. We could have employed a combination of these running variables to make better use of the institutional information, but this would have made our empirical strategy more complicated, with little added benefit given the already strong first stage.

[9]Exams are graded by two internal examiners. Having graded each script separately, graders convene to deliberate on the final mark. External examiners grade scripts for which no agreement could be reached.

exams. This is a worry given the fourth best exam's importance in determining degree class. However, the ranking of exams is only known after all results in a given year have been released, which happens on the same day for all undergraduate programmes.

3 Data and Empirical Strategy

3.1 Student Characteristics and University Performance

From student records we obtain age, gender, nationality and country of domicile information. Course history includes information on degree programme, courses taken and grades awarded, and eventual degree classification. Table 1 reports the descriptive statistics of the variables used in our analysis. We have 5,912 students in the population from 2005-2010 of which 2,649 are included in the Destination of Leavers from Higher Education (DLHE) survey (described in detail below). Columns (1) and (4) report the mean and standard deviations of variables for surveyed and non-surveyed students, respectively, while column (5) reports whether the difference is significant. Surveyed students are less likely to be female, more likely to be UK nationals, more likely to receive an Upper Second and less likely to receive a Lower Second.

To implement our empirical strategy, we create two samples. In column (2), the "First Class sample" consists of students who received either a First Class or an Upper Second and whose fourth highest mark is within five marks of 70. The "Upper Second sample" in column (3) consists of students who received either an Upper Second or Lower Second and whose fourth highest mark is within five marks of 60.[10] This provides two discontinuities that we examine separately and narrows our comparisons to students who are on either side of each threshold. In Table 1, *First Class*, *Upper Second* and *Lower Second* are dummy variables for the degree classes. Among all surveyed students, the majority of 60 percent received an Upper Second with the remaining 40 percent roughly evenly split between First Class and Lower Second. $\mathbb{1}[\text{4th MARK} \geq 70]$ and $\mathbb{1}[\text{4th MARK} \geq 60]$ are dummy variables equal to one if the fourth highest mark is no less than 70 or 60 respectively.

One shortcoming of this database is that we do not have measures of a student's pre-university ability. For a typical UK student this might include her GCSE and A-level results. Although admissions to LSE programmes require A-level or equivalent results to be reported, these data are not collected centrally but are received by each department separately. To partly address this shortcoming, in all our regressions we control for department $\times$ year fixed effects.[11] Furthermore, the validity of our RD strategy does not rely on controlling for ability. As noted in Lee and Lemieux (2010) an RD design mimics a natural experiment close to the discontinuity. If our RD design is valid, there should be no need for additional controls except to improve precision of estimates.

[10] We dropped Third Class and below because they constituted less than 5 percent of the population. Including them among the Lower Second population did not change results.

[11] Results in McKnight, Naylor, and Smith (2007) suggest that controlling for degree programme reduces the importance of pre-university academic results.

3.2 Labor Market Outcomes

Data on labor market outcomes come from the DLHE survey which is a national survey of students who have recently graduated from a university in the UK. This survey is conducted twice a year to find out employment circumstances of students six months after graduation.[12] Due to the frequency of the survey and its statutory nature, LSE oversees the survey and reports the results to HESA (Higher Education Statistics Authority). The survey is sent by email and responded to online and includes all students including non-domiciled and non-UK nationals. Typically response rates are higher for domiciled and UK nationals.[13] Appendix Figure A.1 shows an instance of an invitation to take part in the survey sent by email to a recent graduate.

The survey provides us with data from 2005-2010. Our key variables of interest are industry and employment status. Industry is coded in four digit SIC codes, although we aggregate to two digits for merging with LFS data (see Section 3.3). In Table 1, "employed" is a dummy variable equal to one if a graduate is employed in full-time work.[14]

Table 1 shows that 85 percent of students who responded are employed within six months of graduation. More than one-third are employed in the finance industry although this varies slightly across the degree classes (see Appendix Table A.5). Given the importance of the finance industry, we construct a dummy variable for employment in finance as a separate outcome variable and look at results excluding the finance industry.

Because the survey is conducted six months after graduation, we interpret our analysis as applying to first jobs. Although we do not observe previous job experience and cannot control for this in our analysis, 98 percent of our students were younger than 21 years of age when they started their degrees. Thus, any work experience is unlikely to have been in permanent employment. Also, we cannot follow students over longer periods of employment to examine the dynamic effects of degrees.

A further concern is that employment six months after graduation may have been secured before the final degree class is known. Anecdotes suggest that students start Summer internships, work experience and job applications prior to graduation. The more common it is that students sign job contracts before graduating, the less likely we are to pick up any effects of degree class. However, anecdotal evidence suggests that many early job offers are conditional on achieving a specified degree class. If students then narrowly miss the requirement of their job offer and are forced to work in jobs with lower pay, then this would be picked up by our identification strategy.

3.3 Labor Force Survey

We merge wage data from the LFS into the DLHE survey at the industry $\times$ year $\times$ gender level.[15] We calculate mean log hourly wages for each industry $\times$ year $\times$ gender cell unconditional on skills or experience. One concern with this approach is that mean wages are not representative of the earnings facing undergraduates. To address this concern we also calculate mean log wages conditional on university

[12] The surveys are conducted from November to March for the "January" survey, and from April to June for the "April" survey.

[13] Formally, LSE is required to reach a response rate of 80 percent for UK nationals and 50 percent for others. Students who do not respond by email are followed up by phone.

[14] Self-employed, freelance and voluntary work is coded as zero along with the unemployed or unable to work.

[15] There are potentially $67 \times 6 \times 2 = 804$ cells, however the actual number of cells is 338 since not all industries are present in each year-gender cell.

and three experience levels. To match the labor market prospects of undergraduates we chose 1 and 3 years of potential experience.

This gives us four different measures of industry wages—overall mean, university with 1 and 3 years of experience and overall mean for the sub-sample of students in non-finance industries. Our preferred measure is the overall mean because it provides a clean measure of the industry's "rank" compared to other industries. In any case the four measures are highly correlated with pairwise correlations never less than 0.8. Table 1 shows that the mean log wage is 2.45 which is roughly GBP11.60 per hour in 2005GBP. As expected, industry wages increase in years of experience.

Using industry wages implies that we do not have within-industry variation in outcomes. The lack of a more direct wage measure is an issue for other studies in the literature as well (Di Pietro, 2010; McKnight, Naylor, and Smith, 2007). Appendix Table A.5 shows the top 15 industries ranked by total share of employment. Even accounting for the large share in finance, there is substantial distribution in employment across industries—of the 84 two-digit SIC codes, 66 are represented in our data.

As a further important outcome variable we create an indicator for working in one of the 20 percent (unweighted) highest-paying industries by averaging our preferred mean log hourly wage variable across genders and years. Table 1 shows that two thirds of graduates work in these high-wage industries, which are listed in Table A.6.

While the DLHE survey includes a question about annual salary, we prefer using expected industry wages in our analysis for several reasons.[16] First, response to the annual salary question is voluntary and less than half of respondents report their salary. Second, we do not observe hours worked, so the salary variable conflates productivity and the labor supply decision. Third, actual salary contains a transitory component, whereas this is unlikely to be the case for industry mean wages. Since industry of work is persistent, industry mean wages may be more informative about expected lifetime income than actual salary.

3.4 Empirical Strategy

Our unit of observation is a student. For each student we observe her degree classification and her course grades. In particular, we observe her fourth highest mark taken over three years of the degree. As described in Section 2, institutional rules imply that the fourth highest mark is critical in determining her degree class. When the fourth highest mark crosses the 70-mark or 60-mark cutoff, there is a discontinuous jump in the probability of receiving a First Class and Upper Second respectively. We use a dummy variable for the fourth highest mark crossing these thresholds as an instrument for the degree class "treatment".

Identification in a fuzzy RD setup requires the continuity assumption (Lee and Lemieux, 2010).[17] Apart from the treatment– in this case degree class– all other observables and unobservables vary continuously across the threshold. This also means that the assignment variable should not be precisely manipulated by agents. We cannot test the continuity of the unobservables directly. Instead we test the continuity of observables. Second, we employ the McCrary (2008) test to see if there is a discontinuity

[16] The correlation between the log of reported salary and industry mean log wages is 0.33.

[17] Regression discontinuity was introduced by Thistlethwaite and Campbell (1960) and formalized in the language of treatment effects by Hahn, Todd, and Van der Klaauw (2001). The close connection between fuzzy RD and instrumental variables is noted in Lee and Lemieux (2010), Imbens and Lemieux (2008) and Imbens and Wooldridge (2009). Instead of the usual exclusion restrictions, however, we require the continuity assumption and non-manipulation of the assignment variable.

in the probability density of the treatment which may suggest manipulation of the assignment variable. These are discussed in Section 4.2.

In our benchmark specification we use a non-parametric local linear regression with a rectangular bandwidth of 5 marks above and below the cutoff (Imbens and Wooldridge, 2009). This means we include the fourth mark linearly and interacted with the dummy variable as additional controls. A non-parametric approach observes that a regression discontinuity is a kernel regression at a boundary point (Imbens and Lemieux, 2008). This motivates the use of local regressions with various kernels and bandwidths (Fan and Gijbels, 1996; Li and Racine, 2007). Although a parametric function such as a high order polynomial is parsimonious it is found to be quite sensitive to polynomial order (Angrist and Pischke, 2009). In specification checks we vary the bandwidth and try polynomial functions to flexibly control for the fourth mark. As discussed in Section 4.4 these specification checks produce qualitatively similar results.

In theory, identification in an RD setup comes in the limit as we approach the discontinuity asymptotically (Hahn, Todd, and Van der Klaauw, 2001). In practice, this requires sufficient data around the boundary points– as we get closer to the discontinuity estimates tend to get less precise because we have fewer data. Furthermore, when the assignment variable is discrete by construction, there is the additional complication that we cannot approach the boundary infinitesimally.[18] In this paper, we choose the 5 mark bandwidth as a reasonable starting point and accept that some of the identification necessarily comes from marks away from the boundary. We follow Lee and Card (2008) in correcting standard errors for the discrete structure of our assignment variable by clustering on marks throughout.[19]

We write the first-stage equation as:

$$\begin{aligned} \text{CLASS}_i &= \delta_0 + \delta_1 \mathbb{1}[\text{4th MARK} \geq \text{cutoff}]_i + \delta_2(\text{4th MARK}_i - \text{cutoff}) \\ &\quad + \delta_3(\text{4th MARK}_i - \text{cutoff}) \times \mathbb{1}[\text{4th MARK} \geq \text{cutoff}]_i + X_i\delta_4 + u_i \end{aligned} \tag{1}$$

where CLASS is either First Class or Upper Second and the cutoff is 70 or 60 respectively. $\mathbb{1}[\text{4th MARK} \geq \text{cutoff}]$ is a dummy variable for the fourth mark crossing the cutoff and our instrument for the potentially endogenous degree class. X is a vector of covariates including female dummies, age and age squared, dummies for being a UK national, dummies for having resat or failed any course, 15 dummies for department, 5 year of graduation dummies and 75 dummies for department $\times$ year of graduation interactions.

We use the predicted degree class from our first-stage regression in our second-stage equation:

$$\begin{aligned} Y_i &= \beta_0 + \beta_1 \text{CLASS}_i + \beta_2(\text{4th MARK}_i - \text{cutoff}) \\ &\quad + \beta_3(\text{4th MARK}_i - \text{cutoff}) \times \mathbb{1}[\text{4th MARK} \geq \text{cutoff}]_i + X_i\beta_4 + \varepsilon_i \end{aligned} \tag{2}$$

where Y are various labor market outcomes including employment status, employment in high-wage or finance industry, and four measures of industry wages.

[18] This is also a problem facing designs where age in years or months is the assignment variable, e.g. Carpenter and Dobkin (2009).

[19] In our preferred specification the number of clusters is eleven, and therefore we may be concerned about small-sample bias in estimating standard errors. We employ STATA's "vce(cluster *clustervar*)" option, which performs a small-sample bias correction by default (Brewer, Crossley, and Joyce, 2013). In a robustness check, the results of which are available upon request, we estimate the effects of an Upper Second and a First jointly in a pooled specification with 31 or 47 clusters, depending on the bandwidth. We obtain very similar and statistically significant results as in our benchmark specification.

4 Results

4.1 First-Stage and Reduced Form Regressions

In this section we report results from the first-stage (1) and the reduced form regressions:

$$\begin{aligned} Y_i &= \gamma_0 + \gamma_1 \mathbb{1}[\text{4th MARK} \geq \text{cutoff}]_i + \gamma_2(\text{4th MARK}_i - \text{cutoff}) \\ &\quad + \gamma_3(\text{4th MARK}_i - \text{cutoff}) \times \mathbb{1}[\text{4th MARK} \geq \text{cutoff}]_i + X_i\gamma_4 + \nu_i \end{aligned} \tag{3}$$

where Y are the various labor market outcomes.

Table 2, column (1), reports the first-stage results for the First Class discontinuity (panel A) and Upper Second discontinuity (panel B). Both first-stage F-statistics are above the rule-of-thumb threshold of 10 and mitigate any concerns about weak instruments (Staiger and Stock, 1997; Stock, Wright, and Yogo, 2002).[20] In order to better interpret the first-stage, we perform a simple count of the complier population at LSE (Angrist, Imbens, and Rubin, 1996; Imbens and Angrist, 1994), based on the relationship between fourth highest mark and degree class when not controlling for covariates. In Figure 2 the schematic shows the breakdown of students into compliers, always takers and never takers around the discontinuity. For instance, always takers are students who receive a First Class regardless of their fourth highest mark, while compliers are students who receiver a First Class *because* their fourth highest mark crosses the threshold. The breakdown suggests that the complier population is sizeable at 87 percent. This is expected because the institutional rules are strictly followed and supports the validity of our results to the rest of the LSE population.

Columns (2) to (4) report the reduced form regressions for the extensive margin of employment, for working in a high-wage industry, and for working in the finance industry. Both First Class and Upper Second discontinuities show statistically insignificant results for employment and finance, but significant effects of crossing the mark thresholds on working in a high-wage industry. Columns (5) to (8) report the reduced form results for industry wages. In panel A, the results for the First Class discontinuity are positive but insignificant. In panel B, we find stronger and significant results for the Upper Second discontinuity.

The reduced form evidence for industry mean log wages is presented graphically in Figure 4. The plots are suggestive of overall positive effects of degree class on wages, which are likely driven by males. Figure A.2 shows a similar pattern for the effect of degree class on working in a high-wage industry.

4.2 Randomization Checks and McCrary Test

As discussed in Section 3.4, identification in an RD setup requires continuity in the observables (and unobservables) across the threshold as well as non-manipulation of the assignment variable. To test for continuity in the observables, we regress each covariate on the treatment dummy in Table 3. Apart from age in the First Class sample and gender in the Upper Second sample, the results are consistent with the lack of discontinuity in the observables. The apparent discontinuity in the distribution of age is in fact due to outliers, as it disappears when excluding the top percentile of age (the coefficient becomes -0.006 with a standard error of 0.071). The apparent discontinuity in gender in the Upper Second Class sample is

[20] The sample size varies over outcome variables but we confirmed that the first-stage and other results are not sensitive to these sample differences.

most likely due to chance, especially given that it is absent in the First Class sample.

To test for the manipulation of the assignment variable, McCrary (2008) suggests using the frequency count as the dependent variable in the RD setup. The idea is that manipulation of the assignment variable should result in bunching of individuals at the cutoff. In the education literature, this was shown to be an important invalidation of the RD approach (see for e.g. Urquiola and Verhoogen (2009)). In our case, we should see a jump in the number of students at the threshold of 70 or 60 marks. In column (1) of Table 4 we perform the McCrary test and find large and (in the case of the Upper Second threshold) significant jumps in the number of students. *Prima facie*, this might suggest that students are manipulating their marks in order to receive better degrees.

We argue that this bunching is not the result of manipulation but is a consequence of institutional features. Figure 3 plots the histogram of the highest to the sixth highest marks. In every case there is a clear bunching of marks at 60 and 70 even for the highest mark which is not critical for eventual degree class. This is because exam graders actively avoid giving borderline marks (i.e. 59 or 69) and either round up or down. Columns (2) and (3) of Table 4 show that there are similar jumps in the number of students at the thresholds for the 3rd and 5th highest marks. Columns (4) and (5) pool the best three marks, with column (5) allowing for the jump at the thresholds to be different for our running variable, the 4th best mark. The running variable does not feature jumps that are statistically significantly different from the other two marks, thus our RD design passes this augmented version of the McCrary test.

One may still worry that students who received 58 or 68 may appeal to have their script re-graded. From discussions with staff, the appeals process is arduous and rarely successful. Nonetheless we follow the literature in dealing with the potential manipulation of marks by excluding the threshold in specification checks reported in Section 4.4 (see for e.g. Almond and Doyle (2011), Almond, Joseph J. Doyle, Kowalski, and Williams (2010) and Barreca, Guldi, Lindo, and Waddell (2011)). Doing so does not change our results.[21]

4.3 Effects of Degree Class on Labor Market Outcomes

Table 5 reports the results for the effects of receiving a First Class degree compared with an Upper Second. In panel *A*, we compare average differences in outcomes without controlling for any covariates. There are no differences in employment in general or in the finance industry specifically. However, a higher degree class is associated with a 13 percentage point increase in the probability of working in a high-wage industry (column (2)). Consequently, there are significant differences in industry wages. Using our preferred measure of mean industry log wages in column (4), a First Class receives seven percent higher wages. Conditional wage measures in columns (5) to (7) paint a similar picture. Panel *B* includes covariates to allow for closer comparisons of students. This corresponds to estimating (2) using OLS (but without controlling for the fourth best mark). Coefficients for being employed and working in the finance industry remain insignificant, while coefficients for working in a high-wage industry as well as

[21] An alternative identification strategy would be to restrict the sample to students whose fourth mark is the average of the best three first-year exams, which are aggregated to count as one mark in determining degree class. This would produce a more balanced frequency count. It would also address the concern that examiners may sometimes determine a ten-mark bin based on a general impression, and only later decide the precise mark (conversations with staff suggest that this marking strategy is common in essay-based exams such as in philosophy or history). Unfortunately, the sample we obtain from this restriction is much too small to yield precise results.

the wage estimates decrease but remain significant. In panel C we report our benchmark RD model. We instrument for the First Class treatment using a dummy variable for the fourth highest mark crossing the 70 mark threshold. Although the difference in industry mean wages remains significant at 5 percent, the conditional experience measures show lower coefficients that are imprecisely estimated.

Table 6 reports the same specifications for the Upper Second degree. There are no significant differences in average outcomes across students without controlling for covariates in panel *A*. This is because of inter-departmental comparisons we are making in the absence of department fixed effects. Once we control for covariates including department-by-year fixed effects in panel *B* we observe that an Upper Second is associated with a 10 percentage point increase in the probability of working in a high-wage industry (column (2)), and receives 4 percent higher wages than a Lower Second (column (4)). Conditional wage measures in columns (5) to (7) are smaller in magnitude but show similar positive estimates. An Upper Second also has a 7 percentage point higher probability of working in finance. Using the dummy variable $\mathbb{1}[\text{4th MARK} \geq 60]$ as an instrument for Upper Second, panel C reveals that the returns are significant and sizeable at 7 percent for mean wages, 8 percent for high-wage industry employment, and 12 percentage points for finance industry employment. Conditional wage measures in columns (5) to (7) offer a qualitatively similar picture of positive wage effects.

To interpret these results we translate the percentage differences to pounds. Using our preferred measure of wages in the specification in column (3) we find that a First Class and Upper Second are worth around GBP1,000 and GBP2,040 per annum respectively in current money.[22]

4.4 Specification Checks

We conduct a battery of specification tests of our RD results. In Table A.7 we report checks for the First Class degree while Table A.8 reports the same for Upper Second. Each row is a different specification check and the columns are the different dependent variables. We report the coefficient and standard error on the degree class dummy, and the number of observations below each set of regressions with common sample size. We report the benchmark results for comparison.

First, we report results from the benchmark specification but excluding covariates. The second sets of results in each table are from varying the bandwidth (our benchmark is a 5-mark bandwidth). The third group of results are due to specifications using parametric polynomial controls. The fourth set of results include controls for the sum of marks and all other marks separately to show that our results are not driven by omission of other course grades. A fifth type of specification check deals with the worry that bunching of marks around the threshold reflects manipulation. Finally, we address the concern that our results misrepresent students who are not domiciled in the UK by looking only at domiciled students.[23]

[22] Assuming a 40 hour week for 52 weeks for a full time worker using 23 percent CPI inflation from 2005-2012. First Class: $\exp(2.473) \times 40 \times 52 \times 1.23 \times 0.033$. Upper Second: $\exp(2.418) \times 40 \times 52 \times 1.23 \times 0.071$.

[23] We have carried out two additional robustness checks, the results of which are available upon request. First, we pool the Upper Second and First discontinuities and estimate the effects of an Upper Second and a First jointly in a single regression. As we cluster standard errors by mark, this specification has the advantage of increasing the number of clusters to 31 or 47, depending on the bandwidth. We find statistically significant effects of Upper Seconds and Firsts that are quantitatively similar to our benchmark results. Second, we stack the two discontinuities, that is, we overlay them, as follows. For students with a fourth-highest mark between 55 and 64 (65 and 75), the treatment of a higher degree class is switched on if they obtained an Upper Second (First), and switched off otherwise. We find a positive and statistically significant effect of higher degree class if we average across the two discontinuities in this way.

The estimated effects on employment appear to be sensitive to bandwidth choice. For the First Class some specifications even suggest a negative effect on employment, e.g. rows (3) and (4). Likewise for the Upper Second degree, employment outcomes do not display a consistent pattern across specifications. To be conservative we interpret this as suggesting that the extensive margin is not affected by degree class. This is similar to Di Pietro (2010) who did not find significant effects on employment. This may be due to the limited variation we have in employment and requires further investigation in future work. In the following section we focus on the industry wage outcomes.

The estimated effect of degree class on working in a high wage industry remains generally statistically significant across the various specifications, although its size varies. We find consistent results when we look at industry mean wages. Looking at industry means for First Class degrees, we find effects significant at 5 percent ranging from 2.5 to 6.8 percent with the benchmark result of 3.3 percent. For Upper Second, the range is 5.7 to 13 percent with the benchmark of 7.1 percent.[24] Not including any covariates in the regressions reduces precision, as expected. The estimates for an Upper Second are almost unchanged when leaving out covariates, while for a First appear reduced but imprecisely estimated.

4.5 Heterogenous Effects by Gender and Degree Programmes

In this section we investigate heterogeneity of the effects of degree class. We define two groups in the data. First, we define groups by gender. Second, we group degree programmes by their math admissions requirements. Math admissions requirements are a measure of how mathematical the degree is, as opposed to an emphasis on essay writing. Appendix Table A.2 lists the degree programmes in our sample. Using information on the math entry requirements, we distinguish between programmes which required at least A-level in maths and those which do not. The former group includes programmes related to economics, management, mathematics and statistics. These programmes represent just over half of students.

Table 7 presents our estimates by gender. We estimate our benchmark RD specification for each group separately. For males, a First raises the probability of working in a high-wage industry by 23 percentage points and increases expected wages by 6 percent (panel *A1*, columns (2) and (4)). The effects of a Firs appear smaller for females and we cannot reject that they are zero (panel *A2*, columns (2) and (4)). Similarly, Upper Second effects are larger in magnitude for males. The effect on working in a high-wage industry is precisely estimated at 29 percent for males, but wage effects are imprecisely estimated for both groups (columns (2) and (4) in panels *B1* and *B2*).

Table 8 shows results by degree programmes. For both First Class and Upper Second, mathematical programmes display larger and significant effects. The probability of working in a high-wage industry increases by 21 and 27 percentage points, respectively; expected wages increase by 6 and 14 percentage points, respectively (columns (2) and (4) in panels *A1* and *B1*). Effects for non-mathematical programmes appear to be no different from zero in a statistical sense (columns (2) and (4) in panels *A2* and *B2*).

[24] In Table A.7, columns (5)-(7) show insignificant effects of a First on industry wages conditional on experience and when excluding the finance industry. Gelman and Imbens (2014) caution against using polynomials of third or higher order in RD designs.

4.6 Explaining the Results: Degree Class and Statistical Discrimination

Why does degree class raise the probability of working in a high-wage industry and expected wages? After all, there should be no effect of degree class if employers take into account all marks when forming beliefs about applicants' productivity. Employers in the UK do routinely request full transcripts, so it seems puzzling at first that they do not use course marks as finer signals of ability instead of using the cruder degree class.

However, if the computational costs of understanding diverse transcripts are too high, employers could rely on degree class to form rules of thumb, or heuristics, in making hiring and salary decisions. As a rough gauge of the potential computational costs, Appendix Table A.3 counts the number of modules taken by students across departments. In the department of government, for example, students took a total of 167 different modules. This will lead to much diversity among transcripts in terms of courses shown, and it may be difficult for employers to use course level marks to differentiate between candidates.

These insights suggest a simple theory of statistical discrimination as an explanation for our results. According to this theory, employers believe that applicants who obtained a higher degree class are on average more productive, because the skills needed to do well in exams are positively correlated with productivity in the job. Hence, students with higher degree classes work on average in higher paid industries and are expected to earn higher wages.

However, the weight that employers attach to degree class may vary between groups. For example, if true skill features a higher variance in group A than in group B, then a higher degree class should lead to a larger increase in wages for group A than group B. This is because students in group A are more likely to obtain higher degree classes as a result of true skill, rather than factors unrelated to productivity ('noise'). This argument is made precise in the appendix, where we present a standard model of statistical discrimination with a comparative statics exercise.

Our findings of heterogenous effects by gender and the mathematical content of programmes can be rationalized using the theory of statistical discrimination. Appendix Table A.4 presents the means and standard deviations of the fourth highest mark by the different groups. The fourth highest mark is used indirectly as a signal of productivity by employers, since it strongly predicts degree class. Males tend to have higher marks on average than females, and they tend to have higher variance in their marks. Furthermore, mathematical degrees have higher average and variance in marks. Thus, the higher variance among males and students in mathematical programmes is consistent with larger effects of degree class for these groups.[25]

An alternative explanation for the absence of an effect of degree class among females could be that females care more about non-wage attributes of jobs, and hence are not as likely to work in high-wage industries even if given the opportunity.[26] This explanation would imply a difference between males and females even in the raw correlations between degree class and working in high-wage industries.

[25]For this interpretation, it is critical that the higher variance in the fourth highest mark is due to a higher variance of true skill rather than higher variance in noise. Unfortunately, it is not possible to empirically distinguish between the two. It is conceivable that exam results in mathematical degrees are more informative about ability than in non-mathematical degrees, in the sense of featuring less variance in noise. The model would in this case also predict larger effects for mathematical degrees. However, the fact that mathematical degrees feature higher overall variance suggests that both forces—higher variance in true skill and lower variance in noise—would have to be at play at the same time.

[26]This could be for example because females may be more averse to risk and to working in highly competitive work environments (Bertrand, 2011).

Comparing Upper Second degrees with lower degree classes, we find an increase of 6 percentage points in the fraction of males working in high-wage industries, but no difference for females. However, when comparing First Class to Upper Second degrees, the numbers are 17 and 20 percentage points for males and females, respectively. Therefore, differences in the importance of non-wage attributes of jobs between genders can explain only our findings for Upper Second degrees.

Non-wage attributes may also affect job choice differently depending on mathematical content of programmes. An outstanding economics graduate may aspire to a risky job in the finance industry, while an outstanding history graduate may prefer a safer but less lucrative civil service job. But again, the raw correlations are not consistent with this being the only explanation. The fraction of graduates of non-mathematical programmes working in high-wage industries declines by 7 percentage points with Upper Second degrees, while the figure is plus 14 percentage points for mathematical programmes. However, the corresponding figures for the comparison between First and Upper Second are 22 and 8 percentage points, respectively.

Our preferred explanation of statistical discrimination is capable of explaining our findings of heterogenous effects for both dimensions of heterogeneity, and both degree class comparisons.

5 Conclusion

In this paper we estimate the causal effects of university degree class on initial labor market outcomes using a regression discontinuity design that utilizes university rules governing the award of degrees. We find sizeable and significant effects for Upper Second degrees and positive but smaller effects for First Class degrees on the probability of working in a high-wage industry and on expected wages. A First Class (Upper Second) increases the probability of working in a high-wage industry by thirteen (eight) percentage points, and leads to three (seven) percent higher expected wages. Our results are robust to a battery of specification checks. Our results are consistent with the existence of information frictions and statistical discrimination in the labor market. The findings also point to the importance of heuristic decision making and luck for graduates' career outcomes.

The paper informs a policy debate about the adequacy of degree class as a measure of degree performance. Recently, several universities in the UK have decided to experiment with a more detailed letter grading scale. Our results suggests that a more detailed grading scheme may lead to a better match of graduates' pay and ability (at least as measured by performance in university exams).

An important question that we cannot answer with our data is whether the initial differences in earnings due to degree class persist over time. Since students close to the threshold on either side have similar productivity, the effects of degree class may attenuate over time as employers learn about workers' productivities.[27] However, if initial industry placement persists, we may observe earnings differences over the experience profile.

[27] The literature on employer learning argues that any signal used in initial labor market outcomes attenuates over time as employers discover more about ability (Farber and Gibbons, 1996; Altonji and Pierret, 2001; Lange, 2007; Arcidiacono, Bayer, and Hizmo, 2010). Empirically, this means that the effects of schooling attenuate over time while coefficients on hard-to-observe variables like test scores increase over time (Altonji and Pierret, 2001).

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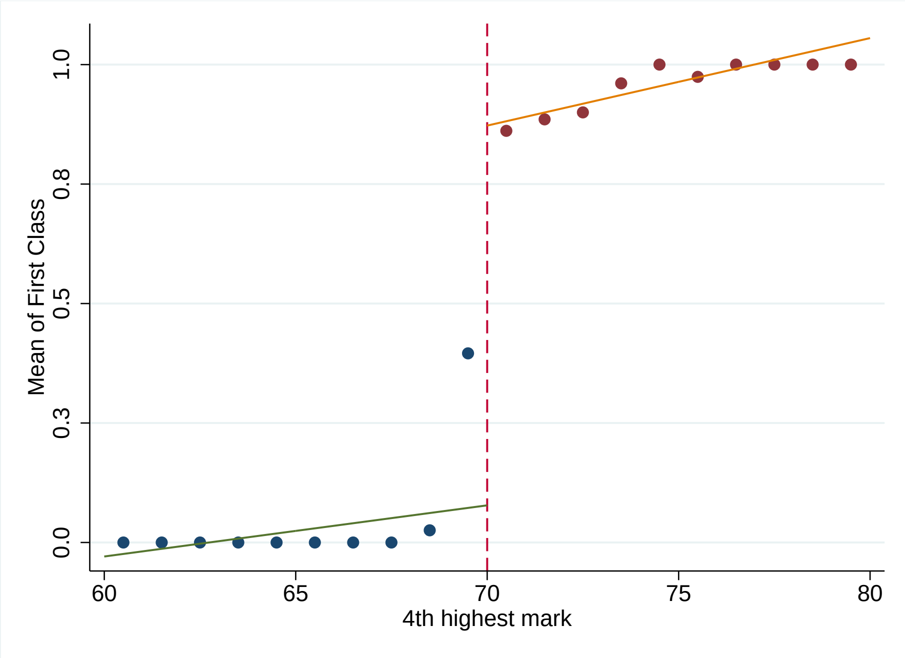
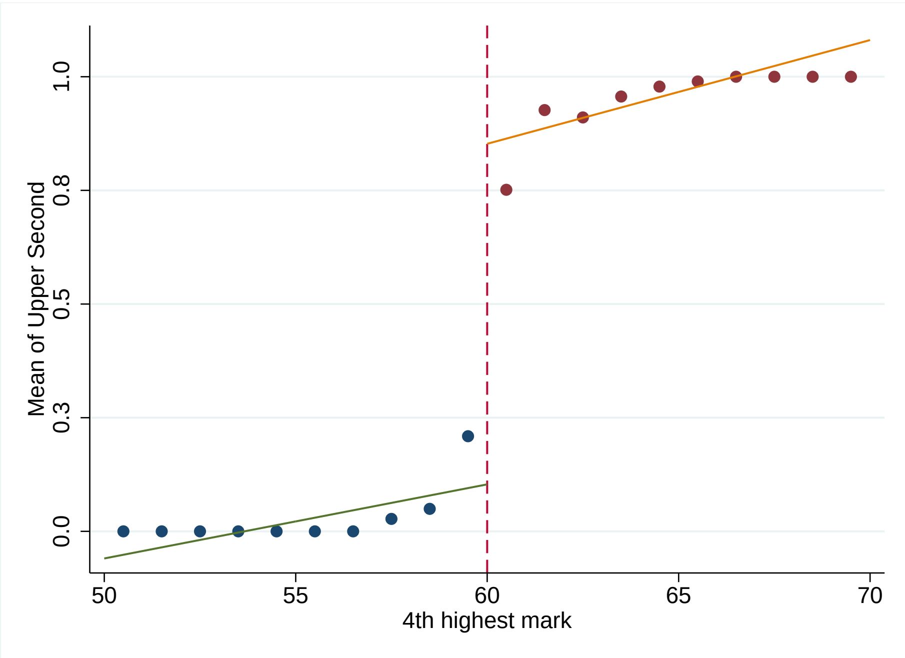
Figure 1: Expected Degree Classification and Fourth Highest Mark

(a) Expected First Class degree



(b) Expected Upper Second degree



The figures plot the fraction of students receiving the indicated degree class against the fourth highest mark. Lines are from OLS regressions estimated separately on each side of the cutoffs.

Figure 2: Counting Compliers

(a) Schematic

		Assignment variable is above threshold	
		0	**1**
Degree Class	**0**	Never takers + Compliers	Never takers
	1	Always takers	Always takers + Compliers

(b) First Class sample (N = 1,136)

		4th highest mark is above 70	
		0	**1**
First Class	**0**	652	44
	1	23	417

Always Takers = 3% = 23/(23+652)
Never Takers = 10% = 44/(44+417)
Compliers = 87%

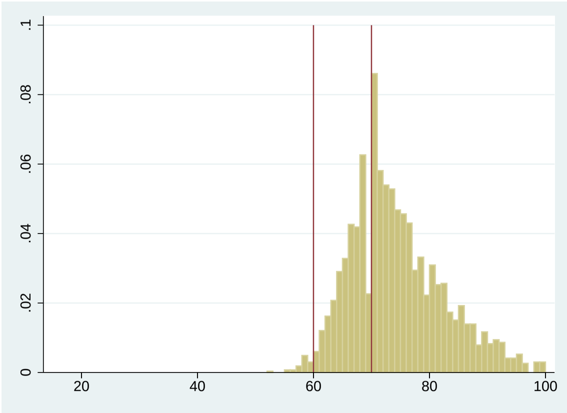
(c) Upper Second sample (N = 1,406)

		4th highest mark is above 60	
		0	**1**
Upper Second	**0**	307	87
	1	16	996

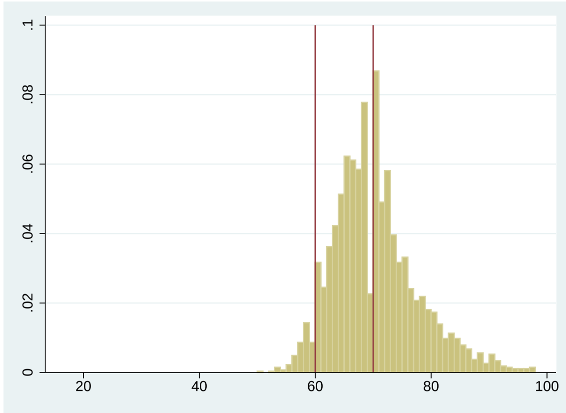
Always Takers = 5% = 16/(16+307)
Never Takers = 8% = 87/(87+996)
Compliers = 87%

Figure 3: Density of Marks

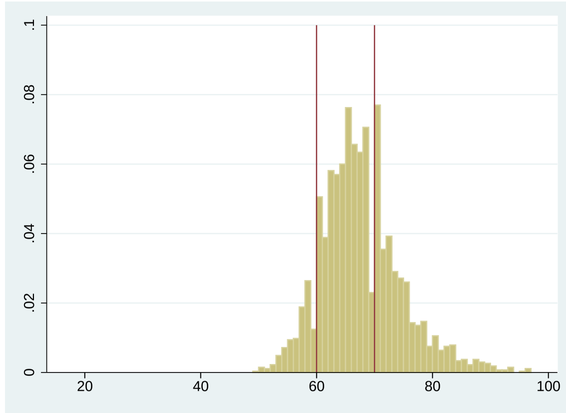
(a) Highest mark



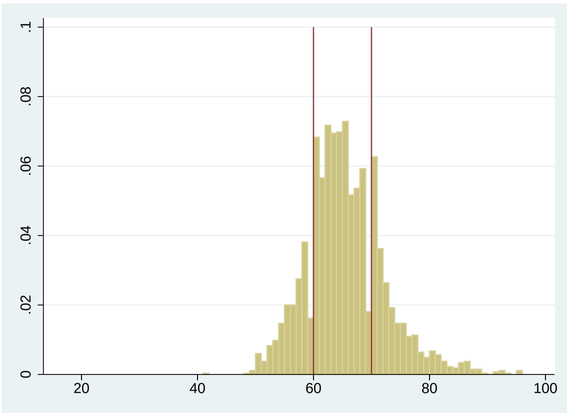
(b) Second highest mark



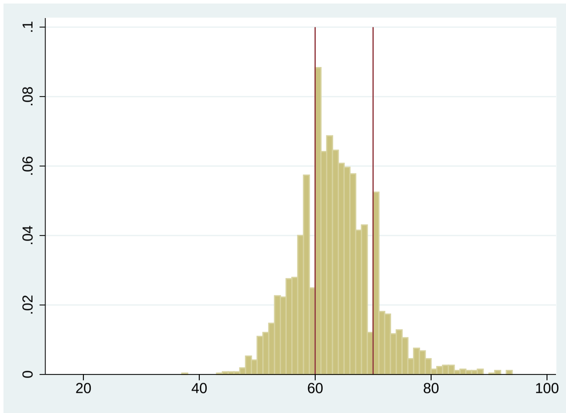
(c) Third highest mark



(d) Fourth highest mark



(b) Fifth highest mark



(c) Sixth highest mark

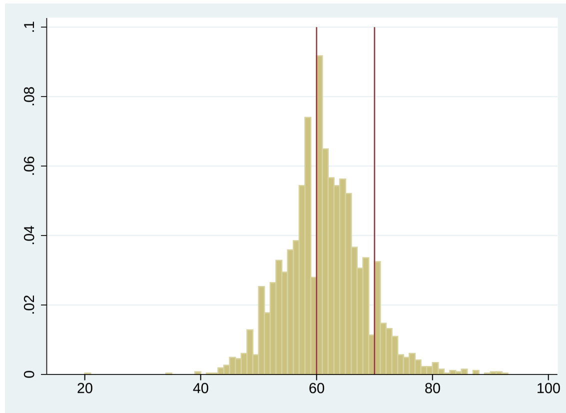
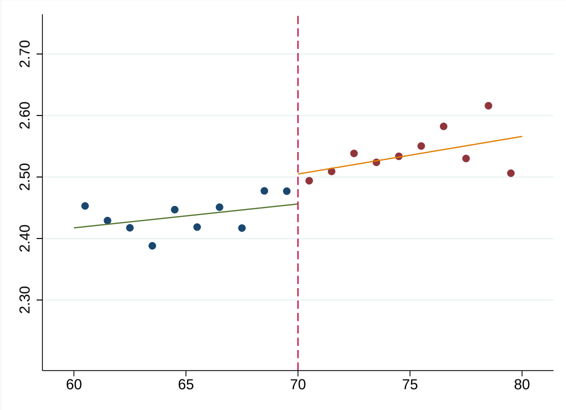
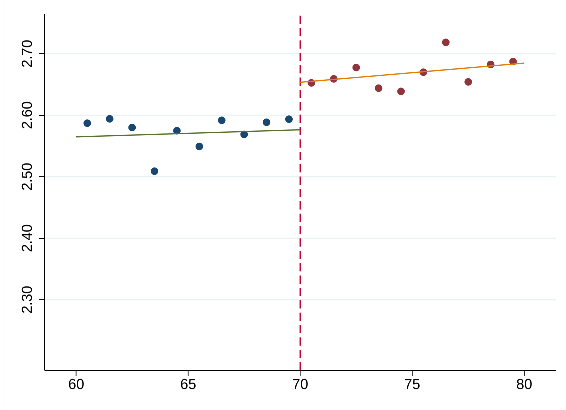


Figure 4: Expected Industry Average Log Wages on Fourth Highest Mark

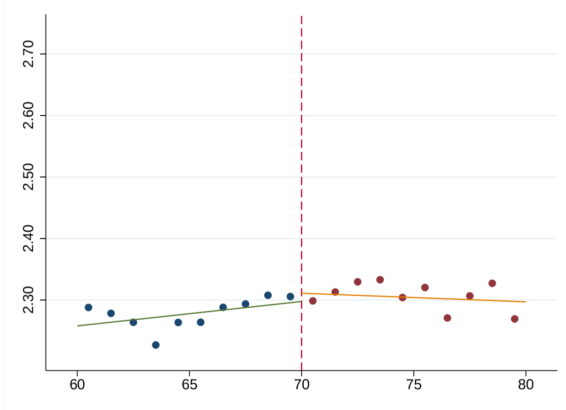
(a) All, First Class threshold



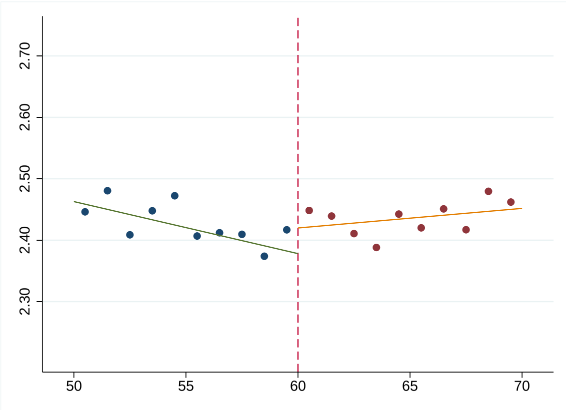
(b) Males, First Class threshold



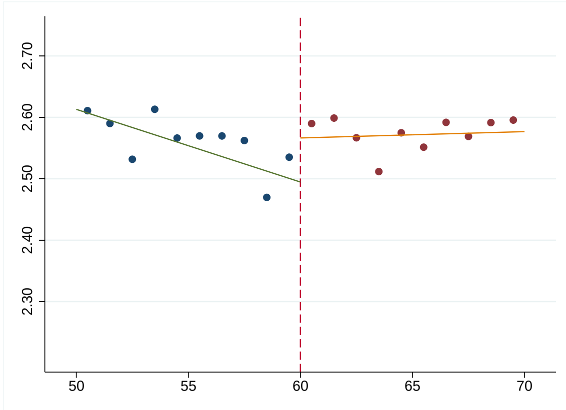
(c) Females, First Class threshold



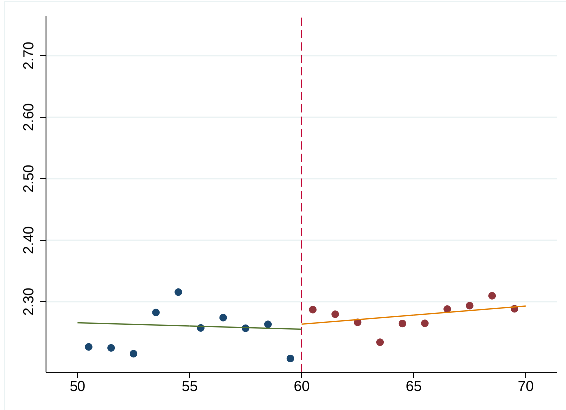
(d) All, Upper Second threshold



(b) Males, Upper Second threshold



(c) Females, Upper Second threshold



The figures plot the mean of industry average log wages against the fourth highest mark. Lines are from OLS regressions estimated separately on each side of the cutoffs.

Table 1: Descriptive Statistics

	Surveyed			Not surveyed	Difference svd./n. svd.
	(1) All	(2) First Class sample	(3) Upper Second sample	(4)	(5)
A. Full sample					
Female	0.45	0.45	0.48	0.51	-0.06***
Age	22.06	22.03	22.06	22.10	-0.04
UK national	0.60	0.59	0.66	0.42	0.19***
Resat any module	0.10	0.03	0.13	0.11	-0.00
Failed any module	0.06	0.02	0.08	0.06	-0.00
First Class	0.23	0.39	0.00	0.25	-0.02
Upper Second	0.57	0.61	0.72	0.53	0.04***
Lower Second	0.19	0.00	0.28	0.22	-0.03***
4th highest mark	65.10	68.63	61.31	65.08	0.01
$\mathbb{1}(\text{4th mark} \geq 70)$	0.24	0.41	0.00	0.25	-0.01
$\mathbb{1}(\text{4th mark} \geq 60)$	0.83	1.00	0.77	0.81	0.03***
Observations	2649	1136	1406	3263	
B. Survey sample					
Employed	0.85	0.86	0.83		
Observations	2649	1136	1406		
High-wage (top-quintile) industry	0.67	0.72	0.60		
Finance industry	0.38	0.42	0.32		
Industry mean log wages					
Industry mean	2.45	2.47	2.42		
College with 1 year experience	2.14	2.15	2.11		
College with 3 years experience	2.34	2.35	2.31		
Observations	2244	978	1168		
Industry mean, top quintile industries	2.55	2.55	2.54		
Observations	1512	700	702		
Industry mean excl. finance	2.38	2.40	2.35		
Observations	1389	567	796		

Means of indicated variables are shown. Surveyed students are respondents to the Destination of Leavers from Higher Education (DLHE) survey conducted six months after a student graduates. The First Class sample includes surveyed students who received either a First Class or Upper Second degree and whose fourth highest mark is within five marks of 70. The Upper Second sample includes surveyed students who received either an Upper Second or Lower Second degree and whose fourth highest mark is within five marks of 60. First Class, Upper Second and Lower Second are dummy variables for degree class. 4th highest mark is the fourth highest mark received by the student among all full-unit equivalent courses taken. $\mathbb{1}(\text{4th mark} \geq 70)$ and $\mathbb{1}(\text{4th mark} \geq 60)$ are dummy variables for the fourth highest mark being at least 70 or 60, respectively. Employed is an indicator for whether a student is in employment six months after graduation. Self-employment, voluntary work and further studies are not considered employment. High-wage is an indicator for working in one of the (un-weighted) top-quintile industries in terms of mean log wages taken across gender and years. Finance industry is an indicator for working in the finance industry. Industry mean log wages are measures of hourly wages in two-digit SIC industry $\times$ year $\times$ gender cells. Two-digit SIC industry wage data is taken from the Labor Force Survey and deflated to 2005GBP. ***, **, * significant at the 1, 5 and 10 percent level.

Table 2: First Stage and Reduced Form Regressions for First Class and Upper Second Degrees

					Industry mean log wages			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Treated	Employed	High-wage	Finance	All	Col 1yr	Col 3yrs	No finance
A. First Class discontinuity								
$\mathbb{1}$(4th mark $\geq$ 70)	0.673***	0.007	0.089**	0.007	0.022	0.014	0.009	0.035
	(0.124)	(0.034)	(0.039)	(0.054)	(0.014)	(0.013)	(0.012)	(0.023)
4th mark $-$ 70	0.046	-0.006	0.002	0.017	0.007*	0.008**	0.009**	0.006
	(0.030)	(0.012)	(0.004)	(0.016)	(0.004)	(0.003)	(0.003)	(0.005)
(4th mark $-$ 70) $\times$ $\mathbb{1}$(4th mark $\geq$ 70)	-0.016	0.006	-0.010	-0.050**	-0.011**	-0.013***	-0.013***	-0.005
	(0.031)	(0.015)	(0.009)	(0.017)	(0.005)	(0.004)	(0.004)	(0.006)
Observations	1136	1136	978	978	978	978	978	567
R squared	0.80	0.20	0.32	0.26	0.61	0.44	0.40	0.50
First-stage F-statistic	29.2							
B. Upper Second discontinuity								
$\mathbb{1}$(4th mark $\geq$ 60)	0.670***	-0.024	0.057*	0.080	0.048**	0.036**	0.046**	0.042*
	(0.078)	(0.030)	(0.029)	(0.050)	(0.020)	(0.015)	(0.016)	(0.019)
4th mark $-$ 60	0.031	0.004	-0.008	-0.013	-0.002	-0.004	-0.005	-0.004
	(0.018)	(0.006)	(0.010)	(0.010)	(0.005)	(0.004)	(0.004)	(0.005)
(4th mark $-$ 60) $\times$ $\mathbb{1}$(4th mark $\geq$ 60)	0.006	0.006	0.019	0.015	-0.000	0.001	0.002	0.002
	(0.022)	(0.007)	(0.011)	(0.015)	(0.006)	(0.005)	(0.005)	(0.006)
Observations	1406	1406	1168	1168	1168	1168	1168	796
R squared	0.72	0.10	0.27	0.20	0.48	0.35	0.32	0.40
First-stage F-statistic	74.8							

***, **, * significant at the 1, 5 and 10 percent level. Standard errors are clustered by marks. All regressions include controls for sex, age and age squared, being a UK national, having resat or failed any course, as well as fully interacted department and year fixed effects. Column (1) reports the first-stage regression of degree class on an indicator for marks crossing the relevant cutoff. "Treated" is a dummy indicating receipt of a First Class (panel *A*) and an Upper Second (panel *B*), respectively. The first stage F-stat for excluded instruments is reported in the last row of each panel. Columns (2) to (8) report reduced form regressions of labor market outcomes on the cutoff instrument. Outcomes related to industry mean log wages include the industry mean log wage conditional on gender and year ("All"), the mean log wage by gender and year for university graduates with one and three years experience ("Col 1yr" and "Col 3yrs"), and again the mean log wage by gender and year but excluding the finance industry ("No finance").

Table 3: Testing the Randomization of Instruments Around the First Class and Upper Second Discontinuities

	(1) Female	(2) Age	(3) from UK	(4) resat any	(5) failed any
A. First Class discontinuity					
$\mathbb{1}(\text{4th mark} \geq 70)$	-0.001	-0.158*	0.012	-0.001	-0.009
	(0.055)	(0.071)	(0.060)	(0.022)	(0.011)
4th mark $-$ 70	-0.002	0.024	-0.007	-0.002	-0.005
	(0.012)	(0.025)	(0.009)	(0.006)	(0.005)
$(\text{4th mark} - 70) \times \mathbb{1}(\text{4th mark} \geq 70)$	-0.016	0.013	-0.011	-0.004	0.004
	(0.012)	(0.036)	(0.016)	(0.007)	(0.005)
Observations	1136	1136	1136	1136	1136
B. Upper Second discontinuity					
$\mathbb{1}(\text{4th mark} \geq 60)$	0.103**	0.119	-0.031	0.041	0.002
	(0.036)	(0.383)	(0.066)	(0.054)	(0.064)
4th mark $-$ 60	-0.033**	-0.093	0.014	-0.036**	-0.014
	(0.014)	(0.088)	(0.019)	(0.015)	(0.017)
$(\text{4th mark} - 60) \times \mathbb{1}(\text{4th mark} \geq 60)$	0.025*	0.084	-0.011	0.017	0.006
	(0.012)	(0.092)	(0.020)	(0.015)	(0.017)
Observations	1406	1406	1406	1406	1406

***, **, * significant at the 1, 5 and 10 percent level. Standard errors are clustered by marks. All regressions include the controls listed in the note to Table 2 except the variable that appears on the left-hand side, as well as fully interacted department and year fixed effects.

Table 4: Testing for Manipulation of the Running Variable Around the Threshold

	Number of students in each mark				
	(1)	(2)	(3)	(4)	(5)
	4th best	3rd best	5th best	pooled	pooled
A. First Class discontinuity					
$\mathbb{1}$(4th mark $\geq$ 70)	62.90				-14.47
	(39.92)				(8.07)
4th mark – 70	-20.02**				-4.66***
	(8.87)				(0.85)
(4th mark – 70) $\times$ $\mathbb{1}$(4th mark $\geq$ 70)	-8.37				-0.30
	(10.69)				(1.14)
$\mathbb{1}$(mark $\geq$ 70)		59.24	67.97**	61.18	70.20
		(50.38)	(28.67)	(42.00)	(42.17)
mark – 70		-17.15	-22.53***	-18.54*	-14.15
		(10.29)	(5.46)	(8.65)	(8.79)
(mark – 70) $\times$ $\mathbb{1}$(mark $\geq$ 70)		-10.74	-1.68	-7.23	-6.98
		(12.54)	(8.02)	(10.57)	(10.49)
Observations	1138	1412	893	3443	3443
B. Upper Second discontinuity					
$\mathbb{1}$(4th mark $\geq$ 60)	80.80**				15.05
	(31.87)				(8.90)
4th mark – 60	6.32				6.82***
	(7.52)				(1.58)
(4th mark – 60) $\times$ $\mathbb{1}$(4th mark $\geq$ 60)	-2.09				-5.35
	(7.94)				(3.17)
$\mathbb{1}$(mark $\geq$ 60)		46.75	75.63	67.55	62.27
		(26.62)	(48.92)	(39.29)	(39.21)
mark – 60		7.57	11.57	7.81	1.57
		(6.03)	(11.61)	(9.19)	(8.93)
(mark – 60) $\times$ $\mathbb{1}$(mark $\geq$ 60)		6.93	-24.85*	-7.60	-3.31
		(7.20)	(12.35)	(10.02)	(10.39)
Observations	1406	1107	1547	4060	4060

***, **, * significant at the 1, 5 and 10 percent level. Standard errors are clustered by marks. The dependent variable is the frequency count of students in each mark for the relevant exams. Results in columns (4) and (5) are based on a sample that pools the 3rd, 4th, and 5th best exams. All regressions include controls for sex, age and age squared, being a UK national, having resat or failed any course, as well as fully interacted department and year fixed effects.

Table 5: The Effects of Obtaining a First Class Degree Compared to an Upper Second Degree on Labor Market Outcomes

				Industry mean log wages			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Employed	High-wage	Finance	All	Col 1yr	Col 3yrs	No finance
A. OLS without covariates							
First Class	0.019	0.128^{***}	0.069	0.070^{***}	0.062^{***}	0.061^{***}	0.077^{***}
	(0.023)	(0.030)	(0.042)	(0.015)	(0.013)	(0.012)	(0.020)
B. OLS with covariates							
First Class	-0.022	0.075^{**}	0.013	0.037^{***}	0.033^{***}	0.035^{***}	0.052^{***}
	(0.019)	(0.026)	(0.035)	(0.007)	(0.007)	(0.008)	(0.013)
C. RD							
First Class	0.011	0.134^{**}	0.010	0.033^{**}	0.021	0.014	0.054^{**}
	(0.045)	(0.060)	(0.074)	(0.016)	(0.015)	(0.015)	(0.024)
4th mark $-$ 70	-0.006	-0.005	0.016	0.005	0.007^{**}	0.008^{***}	0.003
	(0.012)	(0.008)	(0.017)	(0.003)	(0.003)	(0.003)	(0.004)
$(\text{4th mark} - 70) \times \mathbb{1}(\text{4th mark} \geq 70)$	0.006	-0.008	-0.050^{***}	-0.011^{**}	-0.013^{***}	-0.013^{***}	-0.004
	(0.014)	(0.010)	(0.017)	(0.004)	(0.004)	(0.004)	(0.005)
Observations	1136	978	978	978	978	978	567

***, **, * significant at the 1, 5 and 10 percent level. Standard errors are clustered by marks. All regressions include controls for sex, age and age squared, being a UK national, having resat or failed any course, as well as fully interacted department and year fixed effects. Outcomes related to industry mean log wages include the industry mean log wage conditional on gender and year ("All"), the mean log wage by gender and year for university graduates with one and three years experience ("Col 1yr" and "Col 3yrs"), and again the mean log wage by gender and year but excluding the finance industry ("No finance").

Table 6: The Effects of Obtaining an Upper Second Degree Compared to a Lower Second Degree on Labor Market Outcomes

				Industry mean log wages			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Employed	High-wage	Finance	All	Col 1yr	Col 3yrs	No finance
A. OLS without covariates							
Upper Second	-0.004	0.020	0.029	0.020	0.001	0.001	-0.007
	(0.015)	(0.035)	(0.022)	(0.011)	(0.013)	(0.015)	(0.015)
B. OLS with covariates							
Upper Second	0.027	0.099***	0.069**	0.040***	0.025**	0.027**	0.028**
	(0.015)	(0.022)	(0.030)	(0.008)	(0.010)	(0.010)	(0.010)
C. RD							
Upper Second	-0.035	0.084**	0.118**	0.071***	0.052***	0.067***	0.063**
	(0.043)	(0.038)	(0.058)	(0.024)	(0.019)	(0.019)	(0.026)
4th mark $-$ 60	0.005	-0.010	-0.016*	-0.004	-0.005	-0.007*	-0.006
	(0.006)	(0.011)	(0.009)	(0.005)	(0.004)	(0.004)	(0.005)
$(\text{4th mark} - 60) \times \mathbb{1}(\text{4th mark} \geq 60)$	0.006	0.018*	0.014	-0.001	0.001	0.001	0.001
	(0.006)	(0.010)	(0.012)	(0.005)	(0.004)	(0.004)	(0.006)
Observations	1406	1168	1168	1168	1168	1168	796

***, **, * significant at the 1, 5 and 10 percent level. Standard errors are clustered by marks. All regressions include controls for sex, age and age squared, being a UK national, having resat or failed any course, as well as fully interacted department and year fixed effects. Outcomes related to industry mean log wages include the industry mean log wage conditional on gender and year ("All"), the mean log wage by gender and year for university graduates with one and three years experience ("Col 1yr" and "Col 3yrs"), and again the mean log wage by gender and year but excluding the finance industry ("No finance").

Table 7: RD Estimates by Gender

				Industry mean log wages			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Employed	High-wage	Finance	All	Col 1yr	Col 3yrs	No finance
A1: First Class, males							
First Class	0.012	0.229^{***}	0.086	0.059^{***}	0.048^{***}	0.048^{***}	0.054
	(0.041)	(0.080)	(0.076)	(0.013)	(0.013)	(0.013)	(0.050)
Observations	627	549	549	549	549	549	290
A2: First Class, females							
First Class	0.038	0.057	0.007	-0.022	-0.032	-0.032	-0.034
	(0.082)	(0.081)	(0.142)	(0.029)	(0.024)	(0.023)	(0.057)
Observations	509	429	429	429	429	429	277
B1: Upper Second, males							
Upper Second	-0.068	0.285^{***}	0.148^{*}	0.084	0.081	0.089^{*}	0.082
	(0.100)	(0.059)	(0.083)	(0.059)	(0.050)	(0.049)	(0.060)
Observations	737	618	618	618	618	618	397
B2: Upper Second, females							
Upper Second	-0.038	-0.149	0.045	0.052	0.034	0.036	0.062
	(0.078)	(0.098)	(0.116)	(0.042)	(0.041)	(0.037)	(0.075)
Observations	669	550	550	550	550	550	399

***, **, * significant at the 1, 5 and 10 percent level. Standard errors are clustered by marks. All regressions include controls for age and age squared, being a UK national, having resat or failed any course, as well as fully interacted department and year fixed effects. Outcomes related to industry mean log wages include the industry mean log wage conditional on gender and year ("All"), the mean log wage by gender and year for university graduates with one and three years experience ("Col 1yr" and "Col 3yrs"), and again the mean log wage by gender and year but excluding the finance industry ("No finance").

Table 8: RD Estimates by Program Admissions Math Requirement

				Industry mean log wages			
	(1) Employed	(2) High-wage	(3) Finance	(4) All	(5) Col 1yr	(6) Col 3yrs	(7) No finance
A1: First Class, math required							
First Class	0.086^{*}	0.207^{**}	0.017	0.063^{***}	0.045^{**}	0.039^{**}	0.124^{***}
	(0.044)	(0.084)	(0.103)	(0.015)	(0.021)	(0.019)	(0.047)
Observations	621	576	576	576	576	576	259
A2: First Class, math not required							
First Class	-0.072	0.087	0.058	0.038	0.002	-0.002	0.034
	(0.091)	(0.077)	(0.090)	(0.036)	(0.038)	(0.041)	(0.031)
Observations	515	402	402	402	402	402	308
B1: Upper Second, math required							
Upper Second	0.014	0.272^{***}	0.181^{***}	0.146^{***}	0.107^{***}	0.118^{***}	0.171^{*}
	(0.073)	(0.065)	(0.063)	(0.051)	(0.030)	(0.031)	(0.100)
Observations	625	550	550	550	550	550	304
B2: Upper Second, math not required							
Upper Second	-0.048	-0.039	0.051	-0.004	-0.011	0.005	-0.007
	(0.137)	(0.086)	(0.088)	(0.042)	(0.032)	(0.031)	(0.031)
Observations	781	618	618	618	618	618	492

***, **, * significant at the 1, 5 and 10 percent level. Standard errors are clustered by marks. All regressions include controls for age and age squared, being a UK national, having resat or failed any course, as well as fully interacted department and year fixed effects. Outcomes related to industry mean log wages include the industry mean log wage conditional on gender and year ("All"), the mean log wage by gender and year for university graduates with one and three years experience ("Col 1yr" and "Col 3yrs"), and again the mean log wage by gender and year but excluding the finance industry ("No finance").

Appendix

A Simple Model of Statistical Discrimination

In statistical discrimination, employers differentiate across otherwise identical workers on the basis of observable group membership, for example race or gender. More recent versions of these models introduce the dynamics of employer learning (Farber and Gibbons, 1996; Lange, 2007; Altonji and Pierret, 2001; Arcidiacono, Bayer, and Hizmo, 2010). Our exposition follows Aigner and Cain (1977) and Belman and Heywood (1991) (see also Hungerford and Solon (1987) and Jaeger and Page (1996)).

Suppose employers observe a noisy signal of student ability:

$$y = q + u$$

where y is the signal, q is unobserved ability and u is a normally distributed mean zero random variable uncorrelated with q. Note that on average the signal is unbiased, $E[y] = E[q]$. Students know their own ability but employers only see y and know that q is distributed with mean $\bar{q}$ and some variance σ_q. Therefore, employers pay wages that are equal to the expected ability of students conditional on their signal. That is, employers solve a signal extraction problem:

$$wages = E[q|y] = (1-\gamma)\bar{q} + \gamma y$$

which is a regression of q on y where linearity follows from the normality assumption. The regression coefficient is written as:

$$\gamma = \frac{\sigma_q}{\sigma_q + \sigma_u}$$

where σ_u is the variance of the noise term.

Additionally, employers observe a student's group. Suppose there are two groups, A and B, with means and variances $\bar{q}^A$, $\bar{q}^B$, σ^A and σ^B. For any observed signal y, the difference in predicted ability between groups is:

$$\begin{aligned} E[q|y,A] - E[q|y,B] =& (1-\gamma^A)\bar{q}^A + \gamma^A y - (1-\gamma^B)\bar{q}^B - \gamma^B y \\ =& (\bar{q}^A - \bar{q}^B)(1-\gamma^B) + (y - \bar{q}^A)(\gamma^A - \gamma^B) \end{aligned}$$

This formula gives us three predictions from a comparative statics exercise in which one parameter at a time is set to be different between the two groups. Given some signal y, the wages to group A are higher than group B, $E[q|y,A] - E[q|y,B] > 0$, if

1. $\bar{q}^A - \bar{q}^B > 0$, average signal is higher in group A than B
2. $\sigma_q^A - \sigma_q^B > 0$ and $y > \bar{q}$, ability variance is higher in group A than B for a "good" signal
3. $\sigma_u^A - \sigma_u^B < 0$ and $y > \bar{q}$, noise variance is lower in group A than B for a "good" signal.

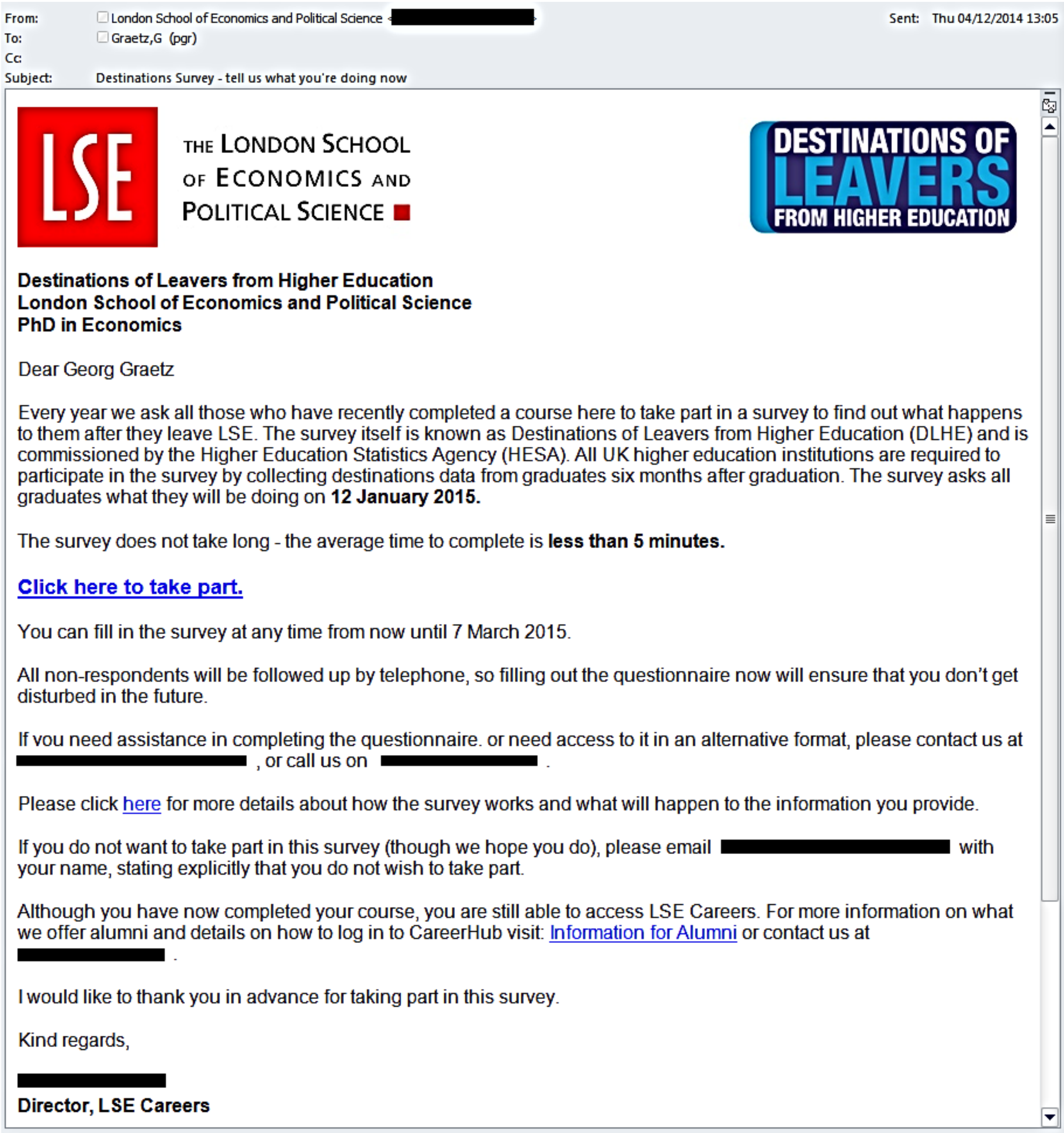
We bring this theory to the data by interpreting y as the fourth highest mark. Fourth highest marks determine degree class and are a noisy signal of students' abilities. The total variance in marks, σ_y, is the sum of the variance in ability, σ_q, and the noise variance, σ_u. We can now re-state our theoretical predictions. At any given mark and resulting degree class, a student from group A has a higher predicted wage than an otherwise identical student from group B if:

1. group A has higher average marks than group B;
2. group A has higher variance in marks than group B;
3. group A has lower variance in the noise term than group B.

In our context, a positive signal is receipt of the higher degree class. Both First Class and Upper Second are positive signals because we are always comparing to the next lower class. Note that we do not actually observe the noise term or its variance, so we cannot exactly decompose the differences in average wages.

Appendix Figures and Tables

Figure A.1: Example for Invitation to Participate in DLHE Survey



From: London School of Economics and Political Science <[redacted]>
To: Graetz,G (pgr)
Cc:
Subject: Destinations Survey - tell us what you're doing now
Sent: Thu 04/12/2014 13:05

THE LONDON SCHOOL OF ECONOMICS AND POLITICAL SCIENCE

DESTINATIONS OF LEAVERS FROM HIGHER EDUCATION

Destinations of Leavers from Higher Education
London School of Economics and Political Science
PhD in Economics

Dear Georg Graetz

Every year we ask all those who have recently completed a course here to take part in a survey to find out what happens to them after they leave LSE. The survey itself is known as Destinations of Leavers from Higher Education (DLHE) and is commissioned by the Higher Education Statistics Agency (HESA). All UK higher education institutions are required to participate in the survey by collecting destinations data from graduates six months after graduation. The survey asks all graduates what they will be doing on **12 January 2015.**

The survey does not take long - the average time to complete is **less than 5 minutes.**

Click here to take part.

You can fill in the survey at any time from now until 7 March 2015.

All non-respondents will be followed up by telephone, so filling out the questionnaire now will ensure that you don't get disturbed in the future.

If you need assistance in completing the questionnaire, or need access to it in an alternative format, please contact us at [redacted] , or call us on [redacted] .

Please click here for more details about how the survey works and what will happen to the information you provide.

If you do not want to take part in this survey (though we hope you do), please email [redacted] with your name, stating explicitly that you do not wish to take part.

Although you have now completed your course, you are still able to access LSE Careers. For more information on what we offer alumni and details on how to log in to CareerHub visit: Information for Alumni or contact us at [redacted] .

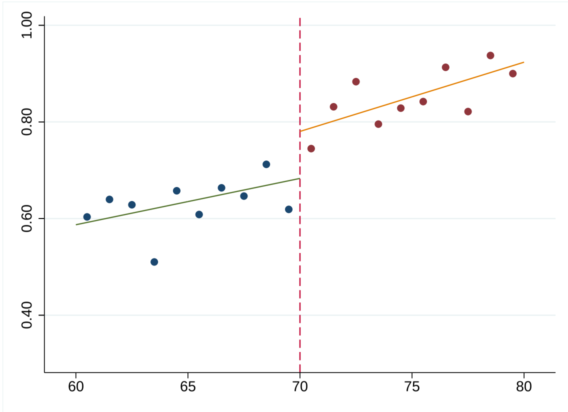
I would like to thank you in advance for taking part in this survey.

Kind regards,

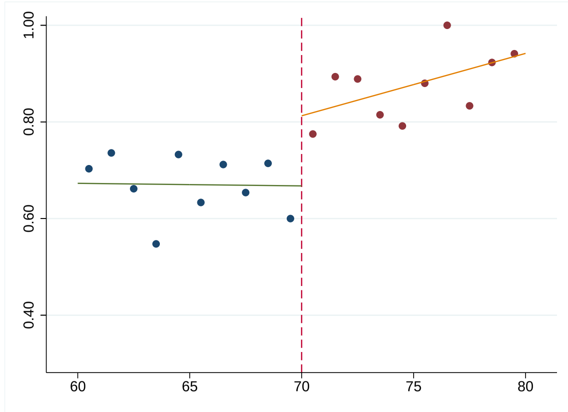
[redacted]
Director, LSE Careers

Figure A.2: Probability of Working in High-Wage Industry on Fourth Highest Mark

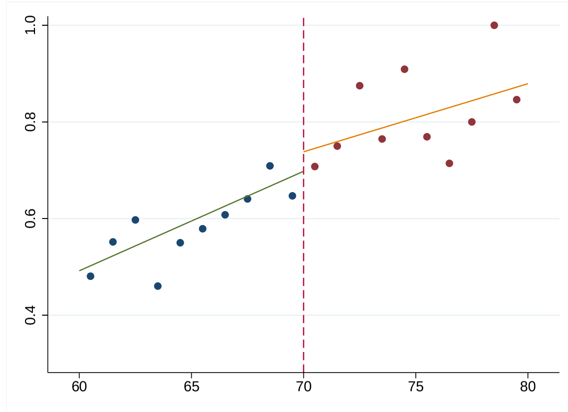
(a) All, First Class threshold



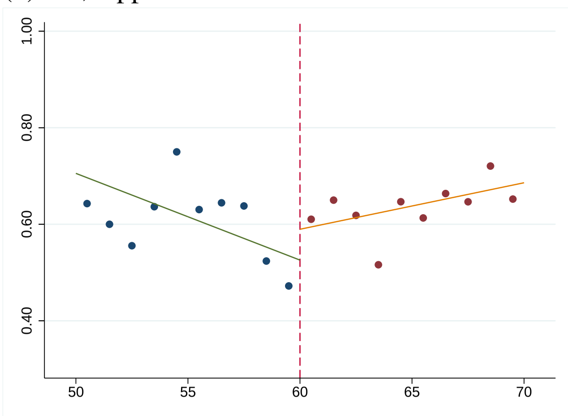
(b) Males, First Class threshold



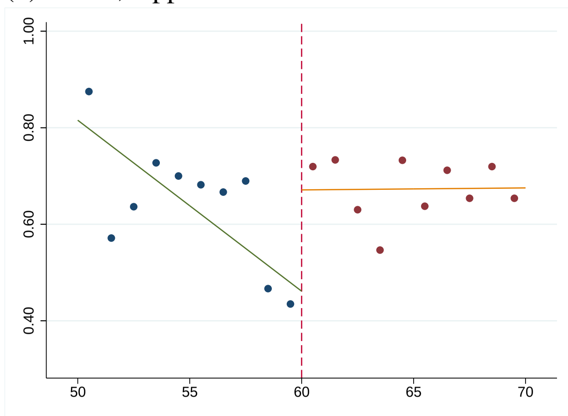
(c) Females, First Class threshold



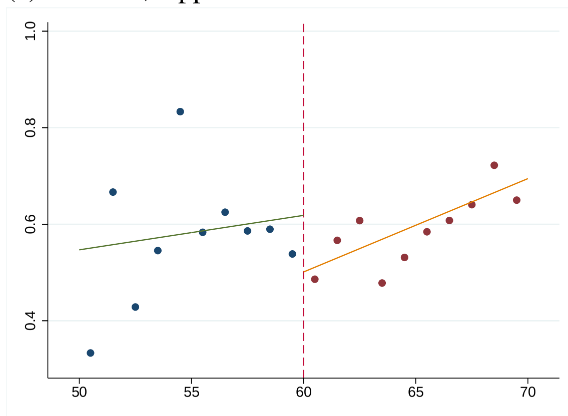
(d) All, Upper Second threshold



(b) Males, Upper Second threshold



(c) Females, Upper Second threshold



The figures plot the fraction of students working in a high-wage industry against the fourth highest mark. Lines are from OLS regressions estimated separately on each side of the cutoffs.

Table A.1: Mapping from Course Marks to Final Degree Class

Final degree class	Course grade requirements
First Class Honors	5 marks of 70 or above or 4 marks of 70 or above and aggregate marks of at least 590
Upper Second Class	5 marks of 60 or above or 4 marks of 60 or above and aggregate marks of at least 515
Lower Second Class	5 marks of 50 or above or 4 marks of 50 or above and aggregate marks of at least 440

Institutional rules governing award of degree class taken from `http://www.lse.ac.uk/resources/calendar/academicRegulations/BA-BScDegrees.htm`, accessed on January 5, 2015.

Table A.2: Degree Programmes

Department	Programme	No. of students	Math required
Accounting	BSc in Accounting and Finance	711	0
Anthropology	BA in Anthropology and Law	82	0
	BA in Social Anthropology	66	0
	BSc in Social Anthropology	125	0
Economic History	BSc in Economic History	136	0
	BSc in Economic History with Economics	23	1
	BSc in Economics and Economic History	67	1
Economics	BSc in Econometrics and Mathematical Economics	55	1
	BSc in Economics	1178	1
	BSc in Economics with Economic History	23	1
Employment Relations	BSc in Human Resource Management	74	0
	BSc in Industrial Relations	15	0
Geography & Environment	BA in Geography	133	0
	BSc in Environmental Policy	36	0
	BSc in Environmental Policy with Economics	35	1
	BSc in Geography and Population Studies	2	0
	BSc in Geography with Economics	116	1
Government	BSc in Government	164	0
	BSc in Government and Economics	227	1
	BSc in Government and History	104	0
International History	BA in History	243	0
	BSc in International Relations and History	155	0
International Relations	BSc in International Relations	308	0
Management Science Group	BSc in Management Sciences	189	1
Managerial Economics	BSc in Management	291	1
Mathematics	BSc in Mathematics and Economics	275	1
Philosophy	BA in Philosophy	7	0
	BSc in Philosophy	7	0
	BSc in Philosophy and Economics	137	1
	BSc in Philosophy, Logic and Scientific Method	69	0
Social Policy	BSc in Population Studies	2	0
	BSc in Social Policy	34	0
	BSc in Social Policy and Administration	10	0
	BSc in Social Policy and Criminology	19	0
	BSc in Social Policy and Economics	14	1
	BSc in Social Policy and Government	7	0
	BSc in Social Policy and Sociology	29	0
	BSc in Social Policy with Government	35	0
	BSc in Social Policy with Social Psychology	4	0
	BSc in Social Policy, Criminal Justice and Psychology	18	0
Sociology	BSc in Sociology	177	0
Statistics	BSc in Actuarial Science	319	1
	BSc in Business Mathematics and Statistics	191	1

The total number of students is 2,649. *Math required* is a dummy variable for whether the programme requires A-level maths for admissions. This information was taken from the LSE website. For information on current admission requirements, see `http://www.lse.ac.uk/study/undergraduate/degreeProgrammes2015/degreeProgrammes2015.aspx`.

Table A.3: Number of Modules Taken by Students in Department

Department	No. of modules
Accounting	100
Anthropology	90
Economic History	99
Economics	143
Employment Relations and Organisational Behaviour	76
Geography & Environment	84
Government	167
International History	125
International Relations	104
Management Science Group	46
Managerial Economics and Strategy Group	72
Mathematics	54
Philosophy	104
Social Policy	98
Sociology	86
Statistics	77

Number of different modules taken by students based in the department. Students can take modules offered by other departments.

Table A.4: Summary Statistics by Groups

	First Class		Upp. Second	
	Mean	S.D.	Mean	S.D.
By gender				
Male	67.56	6.00	62.33	4.47
Female	66.60	5.40	62.32	4.32
By math requirements				
Math required	68.74	6.57	62.33	4.75
No math required	65.39	4.07	62.32	4.06

The columns titled "First Class" and "Upp. Second" refer to the First class and Upper Second samples, respectively.

Table A.5: Top 15 Industries Ranked by Total Share of Employment

Industry (SIC two-digit)	Industry mean log wages	Share of employment Total	First Class	Upper Second	Lower Second and below
financial ex insurance and pension	2.53	38.10	47.90	36.28	31.00
legal and accounting activities	2.50	16.22	21.21	14.43	15.15
public admin, defence, social sec	2.35	7.44	5.85	8.52	6.29
head offices; management consultancy	2.49	6.51	8.04	6.23	5.36
insurance, reinsurance and pension	2.38	4.55	4.75	3.79	6.53
education	2.41	3.88	2.01	4.97	3.03
advertising and market research	2.51	2.01	1.10	2.37	2.10
security & investigation activities	1.95	1.74	0.37	2.05	2.56
office admin, support and other	2.14	1.52	0.18	1.58	3.03
retail trade, except vehicles	1.91	1.47	0.73	1.58	2.10
auxiliary to financial and insurance	2.50	1.34	1.46	1.50	0.70
other prof, scientific and technical	2.22	1.07	0.73	1.26	0.93
publishing activities	2.40	0.85	0.37	0.87	1.40
employment activities	2.26	0.80	0.18	1.18	0.47
human health activities	2.25	0.80	0.18	0.87	1.40

Industry mean log wages (deflated to 2005GBP) are simple averages across genders and years of our basic industry log wage variable, which is conditional on gender and year.

Table A.6: Top Fifth of Industries Ranked by Mean Log Wages

Industry (SIC two-digit)	Industry mean log wages	Share of employment Total	First Class	Upper Second	Lower Second and below
computer programming and consultancy	2.68	0.27	0.18	0.24	0.47
manufacture of tobacco products	2.66	0.00	0.00	0.00	0.23
architectural and engineering	2.60	0.71	0.91	0.55	0.93
extraction of crude petroleum and gas	2.57	0.40	0.55	0.24	0.70
programming and broadcasting	2.55	0.67	0.55	0.39	1.63
financial ex insurance and pension	2.53	38.10	47.90	36.28	31.00
scientific research and development	2.51	0.31	0.00	0.39	0.47
extraterritorial organisations	2.51	0.76	0.18	0.79	1.40
advertising and market research	2.51	2.01	1.10	2.37	2.10
auxiliary to financial and insurance	2.50	1.34	1.46	1.50	0.70
legal and accounting activities	2.50	16.22	21.21	14.43	15.15
head offices; management consultancy	2.49	6.51	8.04	6.23	5.36
water collectn, treatment & supply	2.47	0.00	0.00	0.00	0.23

Industry mean log wages (deflated to 2005GBP) are simple averages across genders and years of our basic industry log wage variable, which is conditional on gender and year.

Table A.7: Specification Checks: First Class Degree

				Industry mean log wages			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Employed	High-wage	Finance	All	Col 1yr	Col 3yrs	No finance
Benchmark	0.011	0.134**	0.010	0.033**	0.021	0.014	0.054**
	(0.045)	(0.060)	(0.074)	(0.016)	(0.015)	(0.015)	(0.024)
Benchmark, no covariates	-0.024	0.088	-0.047	0.006	0.006	-0.010	0.030**
	(0.056)	(0.093)	(0.068)	(0.026)	(0.015)	(0.013)	(0.014)
	1136	978	978	978	978	978	567
3 marks around 70	-0.164**	0.287*	0.251*	0.042**	0.010	0.014	0.009
	(0.065)	(0.153)	(0.139)	(0.019)	(0.017)	(0.017)	(0.071)
	730	629	629	629	629	629	345
4 marks around 70	-0.117***	0.210**	0.210***	0.068***	0.050***	0.038**	0.046*
	(0.026)	(0.093)	(0.057)	(0.017)	(0.015)	(0.015)	(0.027)
	906	774	774	774	774	774	426
6 marks around 70	-0.017	0.145***	0.009	0.044***	0.031***	0.031**	0.074***
	(0.030)	(0.050)	(0.053)	(0.011)	(0.011)	(0.013)	(0.021)
	1346	1147	1147	1147	1147	1147	671
7 marks around 70	-0.012	0.088*	-0.010	0.025*	0.015	0.015	0.054***
	(0.028)	(0.046)	(0.037)	(0.013)	(0.012)	(0.012)	(0.018)
	1552	1322	1322	1322	1322	1322	790
8 marks around 70	-0.022	0.098***	0.005	0.038***	0.032**	0.032**	0.061***
	(0.024)	(0.036)	(0.037)	(0.013)	(0.013)	(0.013)	(0.017)
	1742	1478	1478	1478	1478	1478	884
9 marks around 70	-0.025	0.103***	0.038	0.051***	0.045***	0.046***	0.071***
	(0.024)	(0.030)	(0.043)	(0.009)	(0.010)	(0.010)	(0.013)
	1894	1602	1602	1602	1602	1602	953
10 marks around 70	-0.018	0.093***	0.011	0.056***	0.049***	0.050***	0.080***
	(0.025)	(0.030)	(0.043)	(0.007)	(0.008)	(0.009)	(0.015)
	2048	1735	1735	1735	1735	1735	1045
2nd order polynomial	0.009	0.139**	0.054	0.043***	0.033***	0.026**	0.058**
	(0.037)	(0.060)	(0.055)	(0.013)	(0.013)	(0.013)	(0.024)
3rd order polynomial	-0.006	0.118	0.108	0.049*	0.032	0.016	0.010
	(0.063)	(0.124)	(0.127)	(0.026)	(0.030)	(0.029)	(0.033)
4th order polynomial	-0.133***	0.116	0.205**	0.051*	0.029	0.015	0.011
	(0.029)	(0.133)	(0.093)	(0.029)	(0.034)	(0.033)	(0.037)
	1136	978	978	978	978	978	567
Sum of marks on RHS	0.010	0.132**	0.010	0.032**	0.020	0.013	0.052**
	(0.044)	(0.060)	(0.073)	(0.015)	(0.015)	(0.015)	(0.022)
Other marks on RHS	0.011	0.134**	0.021	0.034**	0.024	0.017	0.051**
	(0.045)	(0.059)	(0.073)	(0.015)	(0.015)	(0.015)	(0.023)
	1136	978	978	978	978	978	567
Excluding marks 69, 70	-0.002	0.183***	0.008	0.048***	0.035**	0.036***	0.078***
	(0.062)	(0.038)	(0.094)	(0.011)	(0.014)	(0.012)	(0.017)
	922	791	791	791	791	791	462
UK domicile sample	-0.015	0.132**	0.138	0.031	0.047**	0.035*	-0.007
	(0.063)	(0.066)	(0.094)	(0.025)	(0.021)	(0.020)	(0.040)
	701	585	585	585	585	585	367

***, **, * significant at the 1, 5 and 10 percent level. Standard errors are clustered by marks. The table reports specification checks for the benchmark model in Table 5, Panel *C*. Each cell reports a different regression where the coefficients on the First Class indicator are reported in the first lines, standard errors in brackets in the second lines, and number of observations in the third lines. All regressions include the same controls as the benchmark model. Outcomes related to industry mean log wages include the industry mean log wage conditional on gender and year ("All"), the mean log wage by gender and year for university graduates with one and three years experience ("Col 1yr" and "Col 3yrs"), and again the mean log wage by gender and year but excluding the finance industry ("No finance").

Table A.8: Specification Checks: Upper Second Degree

	(1) Employed	(2) High-wage	(3) Finance	Industry mean log wages (4) All	(5) Col 1yr	(6) Col 3yrs	(7) No finance
Benchmark	-0.035	0.084**	0.118**	0.071***	0.052***	0.067***	0.063**
	(0.043)	(0.038)	(0.058)	(0.024)	(0.019)	(0.019)	(0.026)
Benchmark, no covariates	0.016	0.245***	0.157*	0.075**	0.091***	0.119***	0.062
	(0.041)	(0.057)	(0.085)	(0.035)	(0.018)	(0.020)	(0.055)
	1406	1168	1168	1168	1168	1168	796
3 marks around 60	-0.113*	0.111*	-0.014	0.082***	0.043	0.064**	0.107**
	(0.063)	(0.065)	(0.079)	(0.031)	(0.028)	(0.028)	(0.048)
	922	759	759	759	759	759	517
4 marks around 60	-0.029	0.153***	0.068	0.093***	0.061**	0.075**	0.100***
	(0.060)	(0.050)	(0.074)	(0.035)	(0.031)	(0.030)	(0.030)
	1160	954	954	954	954	954	648
6 marks around 60	-0.018	0.134***	0.133**	0.080***	0.059**	0.072***	0.067**
	(0.038)	(0.050)	(0.064)	(0.030)	(0.025)	(0.025)	(0.028)
	1582	1310	1310	1310	1310	1310	877
7 marks around 60	-0.002	0.134***	0.086	0.084***	0.056***	0.066***	0.072***
	(0.032)	(0.036)	(0.060)	(0.026)	(0.021)	(0.021)	(0.023)
	1750	1448	1448	1448	1448	1448	962
8 marks around 60	-0.030	0.093*	0.114**	0.064**	0.042*	0.051**	0.035
	(0.035)	(0.049)	(0.056)	(0.028)	(0.022)	(0.023)	(0.039)
	1925	1602	1602	1602	1602	1602	1047
9 marks around 60	-0.011	0.068	0.095*	0.057**	0.033	0.045**	0.033
	(0.037)	(0.045)	(0.054)	(0.026)	(0.021)	(0.021)	(0.032)
	1964	1637	1637	1637	1637	1637	1069
10 marks around 60	-0.014	0.044	0.055	0.047*	0.021	0.030	0.024
	(0.032)	(0.043)	(0.058)	(0.024)	(0.021)	(0.022)	(0.027)
	2003	1672	1672	1672	1672	1672	1092
2nd order polynomial	-0.024	0.069**	0.081	0.084***	0.061***	0.076***	0.078***
	(0.041)	(0.033)	(0.075)	(0.026)	(0.018)	(0.019)	(0.025)
3rd order polynomial	0.006	0.079	-0.040	0.125***	0.090***	0.106***	0.138***
	(0.053)	(0.060)	(0.076)	(0.033)	(0.023)	(0.026)	(0.028)
4th order polynomial	-0.036	0.146*	-0.113	0.121***	0.071**	0.095***	0.158***
	(0.066)	(0.084)	(0.104)	(0.046)	(0.033)	(0.033)	(0.042)
	1406	1168	1168	1168	1168	1168	796
Sum of marks on RHS	-0.037	0.070*	0.105*	0.065**	0.047**	0.063***	0.060**
	(0.042)	(0.040)	(0.059)	(0.026)	(0.020)	(0.020)	(0.027)
Other marks on RHS	-0.043	0.071*	0.117*	0.071***	0.052***	0.067***	0.062**
	(0.051)	(0.041)	(0.060)	(0.026)	(0.020)	(0.020)	(0.027)
	1406	1168	1168	1168	1168	1168	796
Excluding marks 59, 60	-0.036	0.076*	0.214***	0.077***	0.055***	0.068***	0.055*
	(0.040)	(0.045)	(0.033)	(0.022)	(0.015)	(0.014)	(0.029)
	1182	978	978	978	978	978	654
UK domicile sample	-0.083*	0.127*	0.033	0.091***	0.076***	0.087***	0.102***
	(0.042)	(0.076)	(0.059)	(0.023)	(0.021)	(0.023)	(0.032)
	974	792	792	792	792	792	574

***, **, * significant at the 1, 5 and 10 percent level. Standard errors are clustered by marks. The table reports specification checks for the benchmark model in Table 6, Panel *C*. Each cell reports a different regression where the coefficients on the First Class indicator are reported in the first lines, standard errors in brackets in the second lines, and number of observations in the third lines. All regressions include the same controls as the benchmark model. Outcomes related to industry mean log wages include the industry mean log wage conditional on gender and year ("All"), the mean log wage by gender and year for university graduates with one and three years experience ("Col 1yr" and "Col 3yrs"), and again the mean log wage by gender and year but excluding the finance industry ("No finance").

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