# Friends in Higher Places: Social Fit and University Choice

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**Abstract**

Low-income students are less likely to attend elite universities than equally qualified high-income peers, in large part because they apply at lower rates. We study whether this reflects lack of exposure to students who have attended top universities, and how exposure affects students' perceptions. Using UK administrative data, we exploit "breakthrough" events when a school first sends a student to a top university. Applications from that school to that university subsequently rise by 30%. This access promotes upward mobility: marginal entrants graduate at typical rates and earn £4,000 more annually than matched control students, despite coming from relatively poor backgrounds. To understand why students who lack exposure might not apply, we turn to a field experiment in British schools. We find that a primary barrier is students' beliefs about their social fit. At baseline, low-income students are more pessimistic about their social fit at elite universities, but not their chances of receiving an offer or graduating. Students randomly assigned to view short videos of undergraduates discussing their experiences are 6 percentage points more likely to apply to the speaker's university. This treatment makes students more positive about their social fit at that university, with no effect on other beliefs. Finally, when matched with mentors, students primarily seek out information about social life. Our findings highlight perceptions of the social environment at elite universities as a central barrier to applications and illustrate scalable treatments to promote access and social mobility.

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# 1 Introduction

Elite universities can be a crucial vehicle for social mobility, but only if enough students from low-income backgrounds attend them. In the UK, students attending one of the four most selective universities earn 30% more than those attending mid-ranking universities by age 30 (Britton et al. 2022). Many elite professions are disproportionately comprised of graduates of just two universities, Oxford and Cambridge: 75% of senior judges, 66% of senior civil servants, 43% of newspaper columnists, and 20% of Members of Parliament attended one of these two universities, though they educate less than 1% of the population (Sutton Trust 2025). Low-income students who graduate from the most selective universities often see similar labour market outcomes to their high-income counterparts, so this can be an effective path to social mobility for these students (Chetty et al. 2020; Van Der Erve, Drayton, and Britton 2021). But universities' role in promoting social mobility is limited by the number of low-income students who actually attend these universities. British students from low-income neighbourhoods are around 10–15% less likely to apply to one of the 10 most selective UK universities than those from high-income neighbourhoods with the same standardised test scores. These gaps persist despite a context with uniform tuition fees across universities, generous government loans for tuition and living costs, high geographical density of universities, and extensive university outreach programmes. Disparities in applications explain a substantial share—around 40%, as reported in section 3.2—of overall disparities in attendance at top universities between low-income and high-income students, so it is crucial to understand *why* such application gaps persist and *how* they can be addressed.

In this paper, we examine how exposure to students who have previously attended top universities affects university application decisions, and which student beliefs about universities respond to this exposure. We use a combination of national administrative data and a field experiment with university applicants in the UK to document the extent to which students are more likely to apply to a university following exposure to past attendees at that university, measure beliefs about universities, and study which beliefs respond to exposure to past attendees. In the administrative data analysis, we study how application decisions respond to the choices of past cohorts at a student's school, and the consequences of these application choices for students' labour market outcomes. In the field experiment, we randomly connect university applicants with past students at selective universities and measure the effects on beliefs about and applications to that university. Taking the analyses together, we find that interactions with past attendees at top universities improve university applicants' expectations of fitting in socially at selective universities, encourage applications to these universities, and improve their labour market outcomes.

We motivate our questions by describing the variation in exposure to elite universities, attendance at elite universities, and consequences of graduation from elite universities in the UK. First, we illustrate the importance of attending an elite university for labour market outcomes by estimating the returns to each university following the methodology in Dale and Krueger (2002, 2014). We find an average age-27 earnings premium of £4,614 (US$5,907) for graduating from one of the top 10 ranked universities in the UK, but find that below this ranking the earnings-selectivity gradient is largely flat. Second, we document substantial gaps in enrolment at top 10 universities between low-income and high-income students. Decomposing these gaps, we find that differences in students' application rates (conditional on test scores) explain around 40% of them, so enrolment gaps cannot be fully closed without closing application gaps. Third, we show that low-income students are less likely to be exposed to attendees of top universities through schoolmates and document the disparities in exposure across the geography of England. Thus we show that university choice matters in the labour market, that there are socioeconomic disparities in application choices that explain a large fraction of overall enrolment gaps at top universities, and that these are correlated with lack of exposure as low-income students have less exposure to students who attend top universities—that is, less opportunity to meet someone who has attended one of these universities. This motivates our focus on whether increasing this exposure can encourage applications.

Our first main result is that students' application behaviour responds to the choices of past cohorts at their school. Specifically, we study *breakthroughs* to a university—cases where a student attends a particular university after several years where no student from their school had attended it—taking these as discrete changes in exposure to a university through a student's school. In an event study framework we find that breakthroughs persistently encourage applications to the specific university to which there is a breakthrough at their school, raising application rates by around 30–50%. However, breakthroughs have no effect on applications to other, similarly ranked universities, so the effect of breakthroughs seems to be to provide university-specific exposure rather than raising the overall ambition of applications. We also do not see a decrease in applications to similarly ranked universities, ruling out the possibility that breakthroughs simply move applications horizontally between similarly-ranked universities. Instead, we see a concomitant decline in application portfolios that only include lower-ranked universities, so breakthroughs to top universities draw applications away from lower-ranked universities. Breakthroughs thus encourage students to apply more ambitiously, but with effects concentrated at the breakthrough university, rather than reflecting a general increase in the ambition of applications.

The welfare implications of these application effects depend on longer-run outcomes. We provide novel evidence that the students who attend top universities following a breakthrough are well-matched to these universities. Induced students are likely to receive offers, enroll at, and graduate successfully from the universities in question; enrollment increases by 60–85% of the increase in applications. We find little evidence that would point towards overmatch or mismatch in the sense of e.g. Arcidiacono et al. (2011), where marginal disadvantaged students are made worse off by a policy change inducing more ambitious university enrollment. We see some increase in the number of students who do not place at university in the application cycle—indicating that the more risky application portfolio has costs for some students—but the students who do go on to attend the top universities are successful. Students who enroll following a breakthrough are at least as likely to graduate, or graduate with a passing grade, than typical students on their university course. Students induced to apply to a top 10 university by a breakthrough earn around £4,000 (US$5,120) more per year than matched controls by age 27. These earnings effects are robust to two different methods of matching on observables: matching to students in earlier cohorts at the school and matching to students at *other* schools applying in the same year. This effect is comparable in magnitude to the estimated earnings premia for top universities described above, and corresponds to roughly 15% of median earnings at this age for university graduates. Under conservative assumptions, the results imply discounted lifetime earnings effects of £90,000–£100,000 (US$115,000–$130,000).

Treating breakthroughs as an instrument for applications, we find that compliers who apply in response to a breakthrough are high-ability relative to their school but no less likely to be economically disadvantaged than the typical student at their school. Compared to typical students at the university they enrol at following a breakthrough, they are slightly lower-ability and substantially more disadvantaged, meaning that breakthroughs tend to diversify the economic background of the intake at top universities. We also find evidence that students who are more similar to the breakthrough student at their school, or shared a school with the breakthrough student for longer, see a stronger effect on their likelihood of applying to the breakthrough university. Since these variables are correlated with social connections between students, they suggest that interactions with the breakthrough student or students in their social network at least partly explain the effects of breakthroughs on applications.

Taken together, these results imply that *lack of exposure* to students who have attended top universities discourages applications from low-income students who would see substantial benefits, particularly in the labour market, from attending these universities. Back-of-the-envelope calculations suggest that 15–30% of the gap in applications between low-income and high-income students would be closed by equalising student exposure to top universities across income groups. The treatment effects of breakthroughs thus imply that inequality in exposure to top universities plays a substantial role in overall application disparities.

These results demonstrate that breakthroughs affect application decisions by other students, and that this in turn can serve to promote social mobility. What are the mechanisms that drive this response to past cohorts' decisions? To help clarify belief mechanisms that may be at play, we first provide a conceptual framework in which observation of the outcomes of a past student change application behaviour by shifting beliefs. In the framework, students care about both their academic and non-academic experience at university, and choose between a safe university with known payoffs and a risky university with unknown payoffs. Students assume a linear relationship between their expected outcome and their academic ability and social type. They can then observe a signal of the payoffs of another student, as well as that student's ability and social type, and update their beliefs accordingly. As signals reduce posterior variance and have a mean-zero effect on the expected payoff (assuming rational priors), exposure to another student will tend to raise the expected utility of the risky university and encourage applications. Students will update more about the payoff that they are more uncertain about. We also use the framework to describe how characteristics of the student and the mentor matter for belief updating.

Based on this framework, we focus on two mechanisms that might explain the effects of breakthroughs on university application choices and are consistent with the lack of spillovers in applications to to similarly-ranked universities. First, there is a high degree of complexity in university application decisions, and students who attended a particular university might provide information about the application process or academics at the university that makes students more confident about their academic prospects at that particular university (Dynarski et al. 2023). Second, hearing from students who attended the university might make students more likely to expect to fit in with the social environment at that university; Walton and Cohen (2011) find similar effects of past students' experiences on student belonging.

To disentangle these mechanisms, we conduct a field experiment with university applicants at over 20 schools in England and Wales. Our experiment randomly provides students with some exposure to students who went to a particular university in different forms; these treatments are more scalable and more comparable to the strategies used by university outreach programmes than the exposure provided by past cohorts at a student's school. Students in an active control arm receive an informational in-school workshop about university applications and life at university, which helps to calibrate students' prior beliefs. The remaining students receive this workshop and are also assigned to cross-randomised treatment arms. All students in a treatment arm are shown two videos of past university attendees talking about their experiences at university, and are then connected with 1–2 mentors with whom they can have video calls to discuss their university applications and experiences at university. We cross-randomise two dimensions of variation among these treated students. First, we vary whether the mentors assigned are demographically matched with the student. Second, we vary whether students are additionally offered a £75 (US$100) financial subsidy for travel costs associated with visiting a university in person to encourage them to attend. We conduct surveys of participants to elicit beliefs about university, creating a novel dataset that allows us to measure how beliefs respond to exposure, as well as intended applications. The randomised nature and design of the experiment allows us to shut down several alternative mechanisms that might explain results in the administrative data, isolating belief mechanisms.

We first find that students are more likely to apply to a university after watching a video from a past attendee. Students randomized into watching these videos have a 30% increase in their stated intent to apply to that university—around 5 percentage points—in a survey completed a median of four days after watching the video. Videos are presented as informative about the application process and life at university in general, and we do not encourage students to list the university or remind them about the video in the follow-up survey. This magnitude is comparable in percentage terms to the effects of breakthroughs in the administrative data.

What beliefs, if any, do these videos shift? First, we find at baseline that low-income students are more pessimistic about their probability of fitting in and making friends at elite universities, even after controlling for their academic ability, but are not systematically more pessimistic about their chances of admission or graduation. Turning to treatment effects of videos, we find precise null effects on beliefs about admissions probability or successful graduation successfully from the video universities. This null applies to both mean belief updating and the distribution of updates.

The result is consistent with lack of income disparity in these beliefs at baseline and indicates that information affecting beliefs about academic performance is not a key mechanism for exposure effects.

However, we find that video exposure induces a positive shift in the distribution of belief updates about the probability of fitting in and making friends. Students are 6 percentage points more likely to update positively and 10 percentage points less likely to update negatively about these beliefs between the two surveys conducted, and treatment effects on applications are somewhat higher for students who update positively about social beliefs. Furthermore, in one-on-one conversations with mentors, participants are more likely to seek out information about student life and fitting in at university than to discuss careers, university academics, or advice about how to choose courses to apply to. Mentors are more likely to report that life at university is the most important factor in students' decisions than any other factor. Taken together, this evidence indicates that students' perceptions of the social environment at a university is an important mechanism for the effects of exposure on university applications.

Overall, we consistently find across both evidence from breakthroughs and exposure treatments that exposure to students who have attended a university encourages applications to that university. The effect sizes in administrative data imply that disparities in exposure explain 15–30% of overall disparities in applications. Our analysis of breakthroughs suggests that marginal students who are encouraged to apply to a university by exposure graduate at typical rates for their university, despite tending to be more socioeconomically disadvantaged, and receive higher earnings by age 27 than similar students not exposed to a breakthrough. Finally, evidence from our surveys and randomised exposure treatments point to students' beliefs about the probability of fitting in and making friends at particular universities as an important mechanism for these effects. Disadvantaged students have more pessimistic priors about social fit at these universities. Exposure, in the form of videos and mentor interactions, makes student beliefs about social fit more optimistic, and students primarily seek out information about life at university and social fit when speaking with mentors. We conclude with a brief discussion of implications for higher education policy and the effects of exposure on decision-making in other contexts.

**Related literature.** This paper relates most closely to a strand of literature that documents the influence of different peers' university enrollments on students' higher education decisions. Altmejd et al. (2021) illustrate that younger siblings are strongly influenced by their older students' college destinations, while Bechichi and Kenedi (2024) find effects of past cohorts at a school on subsequent cohorts' applications, similar to the breakthrough effects that we study; other studies finding similar effects for siblings and neighbors include Barrios-Fernández (2022) and Avdeev et al. (2024). We are able to build on this literature in two directions: first, by using our linked administrative data to study consequences for graduation and labour market outcomes; second, by using our RCT to isolate the belief mechanisms at play and highlight the role of social fit.

More broadly, we speak to the extensive literature on undermatching of disadvantaged students in university enrollment, primarily in US and UK contexts (Hoxby and Avery 2012; Black, Cortes, and Lincove 2015; Dillon and Smith 2020; Campbell et al. 2022; Chetty, Deming, and Friedman 2023; Wyness 2023). We highlight a potential mechanism for these effects – students' perception of their social fit at elite universities – and explore how students' exposure to universities can address this barrier. Literature explaining these disparities has frequently focused on financial and informational frictions in the US context (e.g. Dynarski et al. 2021); we provide evidence on particular non-financial frictions in a context where finance is less of a barrier than in the United States, building on evidence reviewed in Dynarski et al. (2023) on non-financial barriers to university access. We also contribute to literature evaluating interventions aimed at increasing college access and enrollment (C. Hoxby and Turner 2013; C. M. Hoxby and Turner 2015; Andrews, Imberman, and Lovenheim 2020; Dynarski et al. 2021; Sanders, Chande, and Selley 2017; Cohodes, Ho, and Robles 2022); our interventions are targeted at providing role models and exploiting the effects of exposure and beliefs about social fit.

There is an extensive literature focusing on peer effects (Sacerdote 2011; Barrios-Fernandez 2023) and role models (Rask and Bailey 2002; Porter and Serra 2020) in educational choices. We contribute to this literature by comparing effects from different types of peers and different aspects of exposure, and unpacking the interactions and mechanisms that underlie such effects. In addition, we relate to a broader literature on the effects of exposure on high-stakes decisions. Malmendier and Veldkamp (2022) provide a framework that explains experience and exposure effects in terms of differential 'resonance' of information from different sources. Dean, Kreindler, and Mbonu (2025) show experimentally that exposure affects neighbourhood choices. Our work provides both experimental and quasi-experimental evidence in support of these effects in the higher education context, and develops evidence on the mechanisms underlying these effects in this context.

Finally, our work relates to multiple strands of literature outside economics. Walton and Cohen (2007, 2011) and related papers in the psychological literature discuss social fit and 'belonging', largely in the context of how social fit within a college affects students' performance and outcomes; we build on this by studying how students' *expectations* of belonging affect their choice of college application and enrolment, and ways to affect these perceptions. A literature in the sociology of education (Ball and Vincent 1998; Slack et al. 2014) discusses the sources of information that students use in making decisions about higher education, highlighting the role of 'hot information' from first- or second-hand sources of information within a social network; we provide further insight into why this matters, and quantify consequences for college choice and labour market outcomes.

# 2 Background and data

## 2.1 The UK educational system

We provide a brief overview of key features of the educational system in England and Wales, reserving further details for Appendix A.

Students in England and Wales take compulsory General Certificate of Secondary Education

(GCSE) exams at age 16 in mathematics, English, science, and other optional subjects; we use GCSE grades in the 'core' subjects of maths, English and science, converted to a percentile, throughout the paper as a measure of students' academic ability. Students then spend the last two years of secondary education completing academic or vocational qualifications; students aiming for selective universities will generally take 3 A-levels (the more academic qualification) in specific subjects that they choose. Some students complete these qualifications at the same school they completed their GCSEs at if the school offers both 14–16 and 16–18 provision, while others switch to a different institution, often to a dedicated 'sixth form college' or 'further education college' that provides only age 16–18 education.

If students choose to apply to university, they do so through the Universities and Colleges Application Service (UCAS). UCAS centralises applications and imposes deadlines and regulations for the entire application system; virtually all British universities accept applications only through UCAS. Students apply to 'courses', which are specific majors at a particular university (e.g. Economics at University College London, or Politics, Philosophy and Economics at the University of Oxford). Importantly, students can only apply to 5 courses in a given cycle, and this cap typically binds: 80% of students in our administrative data applied to 5 courses. Applications are independent, and students are not required to rank their preferences. Students are free to apply to courses in the same subject at different universities or different courses at the same university, but typically students will apply to the same or related courses at different universities. Universities observe a short 'personal statement' that is common to each of the courses a student applies to, as well as students' GCSE grades and predicted grades[1] in each of their A-Level or vocational subjects.

Universities choose whether to make an offer to a student or reject them outright. If they accept the student, they can choose to make the student an offer that is *conditional* on receiving certain grades in their A-level or vocational qualifications – which are completed after students receive offers – or can choose to make an unconditional offer. Most offers are conditional and are frequently based on a students' top 3 A-level grades, particularly at higher-ranked universities.[2] After receiving offers, students choose an offer to accept. Once they receive their grades, they can attend the university they received an offer from if they meet the grade conditions; if they fail to meet them, they can instead attend an offer that they had previously designated as an 'insurance' choice, enter 'Clearing' (an after-market scramble coordinated by UCAS), or choose not to attend a university in that cycle. They are free to reapply in future years based on their final grades.

The typical length of an undergraduate degree in the UK is three years, although a substantial

1. Students typically apply to university and receive decisions before they complete their A-Level exams or vocational qualifications, so a standard component of the application system is that teachers assign their students predicted grades in each of these subjects that are reported to universities as part of students' application. These grades are noisy and generally upward-biased (Murphy and Wyness 2020). In parallel work (Tadjfar and Vira 2025), we study a change to the A-Level exam system that eliminated an intermediate standardised test taken before applications, reducing the accuracy of predicted grades and drawing new students into university.

2. Over the 2010s, there was a sharp increase in the number of courses making unconditional offers, followed by a sharp decrease after the practice was discouraged by the university regulator; in forthcoming work along with Phi Adajar, we study the effects of this on student-university matching and the implications for our understanding of inter-university competition.

minority of courses last four years – particularly those that embed a requirement for a year abroad or a year in industry as part of the course – and medical courses last six years. When students graduate, they receive a degree with an honours class based on some weighted average of the marks they receive over the course of their degree, which can be thought of as a coarse GPA: the available classifications are first-class honours, upper second-class honours (2:1), lower second-class honours (2:2) and third-class honours. At most universities around 20–30% of students are awarded first-class honours and the next 40–50% awarded a 2:1; receiving a 2:1 or higher is generally considered to be a 'good degree', and is often required of new graduates by employers.[3]

University tuition is uniform across universities in England and Wales.[4] Tuition for domestic students was capped at £3000 from 2006–2011 and increased to £9000 in 2012, with irregular increases thereafter, generally below the rate of inflation. Essentially all courses charge tuition fees exactly at the cap, meaning that there is no variation in tuition between universities; financial considerations thus only enter into the choice *between* universities to the extent that a student's cost of living differs between different universities.

## 2.2 Administrative data and breakthroughs

Our administrative data is drawn from the Longitudinal Education Outcomes (LEO) dataset (Office for National Statistics 2023), which is produced by the UK Department for Education (DfE). This programme provides researchers with access to several administrative datasets taken from different data providers, along with consistent anonymised individual identifiers so that these datasets can be linked. We use four components of LEO for this project. The National Pupil Database (NPD) provides data on students attending English schools, including demographics, test scores at various ages, school type, and subjects taken. UCAS provides data on applications to undergraduate university, offers, student responses to offers, and final offers accepted. The Higher Education Statistics Agency (HESA) collates data provided on a mandatory basis by universities on student enrollment, graduation, degree class, course studied, and various other details at university. Finally, HM Revenue and Customs (HMRC), the UK tax authority, provides data on employment spells, employer ID and industry, and annual earnings, drawn from tax records.

We focus on the sample of students completing secondary education and applying to university between 2007 and 2021, which is the timespan for which we observe all four of these datasets, and exclude students who do not apply to any university. Since our data on longer-run outcomes (university graduation and earnings) extends only until 2021, we lack data on long-run outcomes for later application cohorts – for instance, we can only observe earnings at age 27 for cohorts from 2012 and earlier. Appendix table B1 indicates the cohorts (indexed by the year at which they complete their high school education and apply to university) for which each main outcome variable is available.

3. Walker et al. (2022) discuss the labour market consequences of degree class, finding the largest marginal earnings premium for students who receive a 2:1.

4. Scottish universities have no tuition associated for Scottish students, but students from England who attend these universities pay the same tuition fees that they would pay at English universities.

Within these cohorts, we apply the following further sample restrictions. First, we restrict to students who apply to university as part of the UCAS 'main scheme' and are aged 18 as of 31st August of the year of their application cycle; this is the typical UCAS application process described above. Students who apply only through an alternative route or who do not apply at all are excluded, as are any applications at different ages. Second, we exclude students who do not apply to at least one university that can be linked with HESA data. UCAS courses that do not link with HESA data are frequently more specialised courses, such as arts academies or music conservatoires, rather than traditional university courses.

One limitation of the administrative data is that it does not include detail on parental background for all participating students. We observe parents' education in HESA data, but this is only available for students who enroll at a university, and we do not observe parental income for any students.[5] We therefore use the Index of Multiple Deprivation, a government-provided measure of neighbourhood deprivation based on income and other indicators, as our primary measure of socioeconomic status; we focus on a binary low-income indicator defined by being in the bottom two quintiles of this index. See Appendix section B.1 for details.

Another limitation to note is that LEO pseudonymises all institutional identifiers, so we are not able to identify particular schools or universities by name. For analysis that requires this, we use a different administrative dataset provided directly by UCAS, which does not have these restrictions. The standalone UCAS extract is essentially the same as the UCAS data provided in LEO, but it includes all university applicants regardless of domicile (whereas LEO includes only students from England) and applicants of all ages (whereas LEO includes only applications at age 18). The limitation of this data is that it does not include the linkages to any other datasets, but the standalone UCAS extract provides data on GCSE and A-level grades, demographics, and university applications, offers and acceptances, which is sufficient to use it for some analysis where identifying institutions is necessary.

### 2.3 Randomised controlled trial setting

In addition to our analysis of administrative data, we conducted a randomised controlled trial working with university applicants across the UK. We provide detail on the design of this RCT in Section 7, but outline the setting here.

We conducted the RCT in two waves in fall 2024 and spring 2025. Fall 2024 participants were in their final year of high school (Year 13) at the time of our first interaction with them; their cohort applied to university from October 2024 – January 2025, and received final admissions decisions after results were released in August 2025. Spring 2025 participants were in their penultimate year (Year 12); their cohort will apply to university between October 2025 – January 2026, and will receive final admissions decisions in August 2026.

Schools were recruited to participate in the programme through various channels, including

5. The Department for Education intends to link comprehensive data on parental background, including income, into future versions of LEO, but has not done so in the version currently available to researchers.

school networks from our partner organisation, WISE, as well as direct outreach to schools. WISE (Women Into Science and Engineering) is a UK social enterprise that aims to support women's access to STEM university courses and careers. During the study period, it became a subsidiary of the Institute of Engineering and Technology, a technology-focused non-profit. Within each school recruited, we then worked with teachers to encourage all of their students in the relevant year group to participate in the programme. We collected data through Qualtrics surveys administered to students at home and in school, as well as data on student applications and enrollment directly from participating schools.

Our sample in the RCT does not appear to be heavily selected on socioeconomic status. Table 1 presents summary statistics for students participating in our RCT, as well as comparable statistics drawn from the administrative data for comparison. Differences in composition may result from both non-random selection of schools into participation and from changes in the composition of the student population over time (as stated above, we observe administrative data from 2007–2021, while RCT data is drawn from 2024–25). Compared to the national administrative data, we find there are substantial differences on ethnicity (56% of students in the RCT sample are white, compared with 80% in the general population) and gender (45% of students in the RCT are female, compared to 50% in the general population and 56% among university applicants). In terms of disadvantage, RCT participants are comparable to the overall KS5 population and somewhat more disadvantaged than the typical university applicant, based on the shares of students from low-income neighbourhoods. However, they are slightly more likely to be taking 3 A-Levels (the typical academic qualification for students) than even the typical university applicant. In terms of the geographical distribution, students in our RCT are more likely to be from Northern England or London, less likely to be from the Midlands, and roughly as likely to be from Southern England, reflecting the geographical distribution and size of the participating schools.

## 3 Patterns of university access and returns

### 3.1 Earnings effects of graduating from selective universities

Why do we care about equality of access to elite universities? From an economic perspective, the primary motivation is that the university a student attends is consequential for their labour market outcomes. Since our administrative data links university applications and attendance to earnings and rich test score data and demographics for a very large sample of students, we are able to precisely estimate earnings returns to universities across the distribution of university selectivity. We use this to illustrate the potential returns to students going to a higher-ranked university. Throughout this analysis, as well as in the rest of the paper, we rank universities by the mean A-level tariff points of students who are enrolled at the university.

We first plot the age profile of earnings for graduates from different universities between 18–30, finding that the earnings of graduates overtake those of non-attendees or dropouts by around age 23. Our primary outcome in this analysis is annual earnings from an individual's primary employer, conditional on receiving positive earnings in the tax year.[6] Figure 1a illustrates how mean annual earnings change over time for students with different university outcomes. University attendees earn less than non-attendees until age 22, as we would expect, but substantial gaps open up after this age. We also clearly see that graduating from a higher-ranked university is associated with higher earnings, and that the gaps between higher- and lower-ranked universities are larger than those between lower-ranked universities and non-attendees. By age 30, non-attendees at university have annual earnings around £20,000, graduates from an average university (below the top 30) have earnings around £26,000, and graduates from a top 10 university have earnings around £44,000. We also see from this figure that the relative earnings differentials have largely stabilised by age 27, suggesting that earnings at this age are a reasonable proxy for future earnings differences. This motivates our focus on earnings at age 27 as our primary measure of earnings in the remainder of this paper. [7]

While there is substantial dispersion around these effects, the entire earnings distribution is shifted right for graduates of the highest-ranking universities, not just mean earnings. Appendix Figure B2 reports percentiles of the income distribution for the same set of university outcomes, reported at age 27. There is substantial dispersion within each group: for instance, the 25th percentile of earnings for graduates of top 10 universities (£21,989) is slightly lower than median earnings for graduates of universities below the top 30 (£22,704), or the 75th percentile of earnings for non-graduates (£23,788). However, the distribution of earnings for top 10 graduates first-order stochastically dominates earnings for graduates of lower-ranked universities.

We next attempt to adjust these outcomes for selectivity, now looking university-by-university at earning effects. For student $i$ who graduates from university $u$ and is aged 27 in year $t$, we estimate the regression

$$Y_{itu} = \alpha + \beta X_i + \theta_u + \delta_t + \varepsilon_{itu} \tag{1}$$

where $X_i$ is individual-level observables, $\theta_u$ is a university fixed effect, and $\delta_t$ a year fixed effect. The omitted university dummy is the modal university in the sample.

We plot the fixed effect on each university $\theta_u$ after applying empirical Bayes shrinkage to point estimates of the university effects. Specifically, let $s_u$ be the estimated standard error of the coefficient $\theta_u$ and $\mu_\theta, \sigma_\theta^2$ be the mean and variance of the estimated $\theta_u$ coefficients across universities; then

$$\theta_u^* = \frac{\sigma_\theta^2}{\sigma_\theta^2 + s_u^2}\theta_u + \frac{s_u^2}{\sigma_\theta^2 + s_u^2}\mu_\theta \tag{2}$$

$$s_u^* = \sqrt{\frac{\sigma_\theta^2}{\sigma_\theta^2 + s_u^2}} s_u \tag{3}$$

6. See appendix B.1 for an explanation of the precise earnings outcome used. Appendix figure B1 presents earnings age profiles including individuals with 0 earnings.

7. More precisely, since tax years and academic years are misaligned – tax years start in April while academic years start in September – we use earnings in the tax year starting 9 years after the student completed their high school education, meaning students born between April and August would be 26 at the start of the tax year while all other students would be 27.

While we cannot identify universities by name,[8] we plot earnings against each university's rank by the mean of the top 3 A-level grades, converted to UCAS tariff points (see Appendix B.1), achieved by students who enroll at the university. The modal (omitted) university is ranked 56, close to the median.

Figure 1b plots the results of this analysis: we find that a large fraction of raw earnings effects can be explained by sorting of high-ability students into elite universities, but that the top-ranked universities retain an earnings premium of around £3000–6000 at age 27 after controlling for ability and selection. Specification 1 includes no controls in $X_i$, and thus just reflects raw differences in earnings across universities after applying empirical Bayes shrinkage. These raw gaps are large even at the early stage in workers' careers that we study; the average earnings coefficient for the top 10 universities is £12,761. Specification 2 controls for demographics (gender, ethnicity, neighbourhood income, free school meal status, and home region of the UK), school test scores (GCSE and A-Level grades), and fixed effects for the major that a student studies. It also introduces fixed effects for the exact portfolio of universities that the student receives an offer from (a subset of those that they apply to).

Offer portfolio controls are included in the spirit of Dale and Krueger (2002, 2014), and more recently Mountjoy and Hickman (2021): controlling for offer portfolios may capture earnings-relevant ability that is not captured by the available demographics and test scores, driven by the combination of student self-selection into applications based on private information about their ability and university offer decisions that take into account unobservable-to-the-researcher signals such as a students' personal statement. We match on the exact set of universities that a student receives an offer from. Thus, earnings comparisons used to estimate university fixed effects are made among students who received offers from the same set of universities but ultimately attended and graduated from different universities. We can compare the resulting coefficients between two universities even if there is no pair of students with the same set of offers who attended each of the different universities, as long as there is a connected set of students across the offer set fixed effects that includes both of these universities. The large sample size of our data, as well as the dimensionality limitation provided by the cap of 5 applications in the UK context, means that all universities appear in the largest connected set, so comparisons between universities across the entire range of the distribution are valid under the assumption that treatment effects do not themselves vary by offer set (Mountjoy and Hickman 2021).

While including these controls substantially shrinks earnings effects, we still find substantial earnings effects for the top-ranked universities after controlling for selection. Figure 1c plots the coefficients on each university from just this latter specification, to better illustrate the range of estimated earnings effects, and adds confidence intervals to the estimates. Points in black have estimates that are significantly different from the mean university fixed effect (relative to the omitted

8. See Britton et al. (2022) for analysis the earnings returns to different universities in the UK that is able to name universities. Their analysis is conducted without data on applications and offers, so they are not able to include offer set controls in their specifications, but they find a similar pattern of high returns for the most selective universities and a flat selectivity-earnings gradient beyond these elite universities.

university) after shrinkage.

We see that most of the top 10–20 universities have distinctly higher returns – the average coefficient for the top 10 universities, relative to the omitted university, is £4,614. This is much lower than the descriptive gaps in earnings, but still a substantial increase, corresponding to 24% of mean earnings for all individuals at age 27 and 18% of mean earnings for all university graduates. If there were no growth in the real value of earnings differentials for these individuals over time, projecting a £4,614 gap from age 27 until retirement age (68 for the cohorts used in this analysis) and discounting at 3% per year yields a lower-bound lifetime earnings effect of £111,000 relative to if these students had attended the modal university.

Outside top institutions, there is a flat gradient between selectivity and earnings effects, and most coefficients are not significantly different from the mean effect of £1,220. Access to the most selective universities is thus particularly important for students' earning potential, and socioeconomic disparities in students' access to these universities is likely to perpetuate income inequality.

### 3.2 Income disparities in applications and enrollment at top universities

Campbell et al. (2022) document socioeconomic disparities in enrollment at UK universities: their main result is that low-SES students in the top quintile of the ability distribution attend universities that are 8 percentiles lower-ranked than their high-achieving peers, conditional on test scores and major. They also find that school fixed effects explain around 80% of these differences.

Adapting a decomposition described by Chetty, Deming, and Friedman (2023) to our context, we observe that the probability of enrollment at a given university through the main UCAS application scheme[9] can be decomposed as

$$\begin{aligned} P(\text{enroll}) = & P(\text{apply}) \\ & \times P(\text{receive offer} \mid \text{apply}) \\ & \times P(\text{accept offer} \mid \text{receive offer}) \\ & \times P(\text{enroll} \mid \text{accept offer}) \end{aligned} \tag{4}$$

Focusing on students who enroll at university in the main scheme, we can thus decompose overall enrollment gaps of the kind described in Campbell et al. (2022) (i.e. $P(\text{enroll} \mid \text{high income}) - P(\text{enroll} \mid \text{low income})$) into components explained by differences in application rates, offer rates, offer acceptance rates, and conditional enrollment rates. Taking logs of (4) produces an additive decomposition in terms of log points; we can also predict the enrollment rate for low-income

9. i.e. accepting it as their firm or insurance choice, not through Clearing or other application routes.

students if they applied at the same rate as high-income students by taking

$$\begin{aligned}
&P(\text{enroll} \mid \text{low income, high income application rate}) \\
=&P(\text{apply} \mid \text{high income}) \\
&\times P(\text{receive offer} \mid \text{apply, low income}) \\
&\times P(\text{accept offer} \mid \text{receive offer, low income}) \\
&\times P(\text{enroll} \mid \text{accept offer, low income})
\end{aligned}$$

Estimating this decomposition reveals that disparities in applications explain a large fraction of overall disparities in applications. Figure 2 presents this decomposition conditional on students' ability, as measured by their percentile in the national GCSE distribution. We focus on the probability of enrollment to one of the top 10 universities. In the 90th–100th percentile of the GCSE distribution, where application and enrollment rates are highest, we see that differences in application rates explain the largest share of differences in enrollment rates out of the four components of the decomposition; the share of enrollment differences explained by application differences averages around 40–50% and is consistently higher than that explained by offer differences, as shown in panel (b). This is a higher share than found by Chetty, Deming, and Friedman in the context of Ivy-Plus enrollment gaps in the US, where only 30% of the differences in the excess enrollment of the top 1% could be attributed to applications gaps, compared with 57% that could be explained by admissions. Future research could focus more explicitly on these contrasting findings, but a likely explanation is the differing structure of applications in the UK and US: the cap of 5 applications may more strongly discourage applications to ambitious but risky colleges than other frictions (e.g. frictions surrounding financial information as in Dynarski et al. 2021) do in the US.

So, low-income students attend top universities at lower rates than higher-income students with similar test scores, and a large fraction of the disparity results from differing application rates. As we saw above, these universities have higher earnings returns, so these disparities are likely to perpetuate inequality, with low-income students lacking access to the top universities.

### 3.3 Income disparities in university destinations of past school cohorts

Disparities in application and enrollment rates at top universities across income levels naturally result in differences in whether students are likely to attend schools that send students to top universities. Figure 3a illustrates that low-income students are substantially more likely to attend a school that sends no-one to top universities than their higher-income peers. Specifically, students from the poorest decile of neighbourhood deprivation are 20 percentage points more likely to attend a school that has sent no-one to one of the top 10 universities in the preceding three years, compared to students from the richest decile. Figure 3b plots the same differences conditional on test scores and other demographics,[10] showing that a 10 percentage point discrepancy remains after adjusting

10. Specifically, we control for GCSE grades, ethnicity, gender, and free school meal status at the individual level.

for these differences. Figure 3c plots variation in exposure across England, indicating the share of students in each region attending schools where no one has attended one of the top 10 universities in the preceding three years; we see that areas of low exposure are most common in Northern England, but there are areas with low exposure across the country.

So, we find overall that access to top universities can substantially affect earnings, that low-income students are less likely to apply to and enrol at these universities, and that they are less likely to be exposed to past enrollees at these universities in their school. Taken together, the latter two findings raise the question of whether low-income students' lack of of exposure to students attending top universities explains their relative reluctance to apply. Does their lack of exposure to students who have attended top universities discourage them from applying to these universities? The cross-sectional correlation may simply reflect persistence in application patterns, but *changes* in exposure to students attending top universities at a school will provide more insight into the effects of exposure. This is the core motivation for the focus on breakthroughs in sections 4.1 and 5, which we proceed to next.

# 4 Administrative design

## 4.1 Breakthrough event studies

To understand how variation in exposure to universities across may affect application behaviour, we study the effects of breakthroughs to universities. Consider two schools, School A and School B, that have both had none of their graduates attend University X for several years. If a student from School A is then admitted to and attends University X, we refer to this as a breakthrough to University X at school A, and refer to the student who is first admitted as a breakthrough student. This is a discrete change in the exposure to students attending University X at School A; students at the school in the next year will now know that someone from their school attended University X, providing them with information about the university. We ask how applications at school A change relative to application patterns at school B following this breakthrough, and how this affects longer-run outcomes.

We pool our analysis in an event study design to exploit this variation across the large number of schools and universities observed in our data. To implement the strategy outlined above, we identify for each university schools where no student enrolled at the university between 2007 and 2009, the first three years of our data. We then define the first year when a student attends that university from that school as the breakthrough year for that school; if no student attends the university during the period covered in our data – from 2010 to 2021 – we assign that school to the control group for that university. By construction, no students from any school in the sample are enrolled at that university before their school's event year, and no students from control schools are enrolled at that university at any point in the sample. Our estimating equation is then:

$$Y_{ist} = \alpha_s + \gamma_t + \delta X_{ist} + \sum_{\tau \neq -1} \beta_\tau \mathbb{I}(t - T_s = \tau) D_s + \varepsilon_{ist} \tag{5}$$

where $X_{ist}$ is a vector of individual-specific covariates, $T_s$ is the year in which school $s$ had a breakthrough and $D_s$ is the treatment indicator. Our primary specification omits individual-level covariates in order to transparently show trends in applications at the affected universities, but in robustness checks included in appendix figures, we include in $X_{ist}$ students' GCSE percentile (see Appendix B.1) and indicators for the number of A-levels they took, as well as the number of A-levels in facilitating subjects.[11]. We exclude the breakthrough student themselves from the sample (or if there are multiple breakthroughs to the same university in the same year, we drop one of the breakthrough students), in order to isolate the effect of a breakthrough on the rest of the breakthrough student's cohort in period $\tau = 0$. To allow for heterogeneous treatment effects by treatment year, we use the Sun and Abraham (2021) estimator for event studies.

In addition to university-by-university analysis, we stack breakthroughs across universities with similar academic rankings to provide a more aggregated picture of the effects of breakthroughs. To do this, we construct a dataset as above for each university. We then stack these datasets, indexing data from each by the breakthrough university $u$, and then run the following stacked event study regression:

$$Y_{istu} = \alpha_{su} + \gamma_{tu} + \delta_u X_{istu} + \sum_{\tau \neq -1} \beta_\tau \mathbb{I}(t - T_{su} = \tau) D_{su} + \varepsilon_{istu} \tag{6}$$

Note that all coefficients are interacted with the university except for the relative time indicators themselves. Standard errors are clustered at the school level in all specifications. A given student may appear multiple times in the stacked dataset if their school sees breakthroughs to, or is in the control group for, multiple universities; clustering at the school level, rather than the school-by-university level, accounts for correlation within a school across the breakthrough university samples (Wing, Freedman, and Hollingsworth 2024). Universities are ranked in order of the mean A-level tariff points of students enrolled at the university, a measure of the university's selectivity.

We also report results for certain outcomes using the analogous difference-in-differences specification of these effects, which pools effects across the post-treatment periods:

$$Y_{ist} = \alpha_s + \gamma_t + \delta X_{ist} + \beta \mathbb{I}(t - T_s \geq 0) D_s + \varepsilon_{ist} \tag{7}$$

$$Y_{istu} = \alpha_{su} + \gamma_{tu} + \delta_u X_{istu} + \beta \mathbb{I}(t - T_{su} \geq 0) D_{su} + \varepsilon_{istu} \tag{8}$$

Our primary outcome for this analysis is an indicator for applying to the breakthrough university.

11. In 2011, the Russell Group of universities published a list of 'facilitating subjects' that they indicated were most supportive for selective university applications: these were biology, chemistry, English literature, geography, history, maths, further maths, modern and classical languages, and physics. Conditional on the number of A-Levels taken, students with more facilitating A-levels are likely to be better prepared for applications to selective universities.

To understand where applications are drawn from, we also construct a set of mutually exclusive and exhaustive outcomes based on students' portfolios of five applications. These are portfolios including the breakthrough university; portfolios that exclude the breakthrough university, but include a different university in the same 5-university tier; portfolios that include no universities from the breakthrough tier but include at least one university from a higher tier; and portfolios including only applications from lower tiers than the breakthrough university. For each university tier, the four difference-in-difference coefficients from regressions with each of these outcomes sum to zero, thus decomposing where breakthrough applications are drawn from.

### 4.2 Matching for earnings effects

To understand the effects of breakthroughs on student welfare, we can go on to look at the effects on students' earnings, making use of the linkage with tax data in LEO. We identify students who applied to the breakthrough university in one of the years following the breakthrough at their school. We refer to these students as 'induced applicants', although for interpretation, note that this set of students potentially includes both compliers (who only apply to the breakthrough university because of the breakthrough at their school) and always-takers (who would have applied even without the breakthrough). For each such student, we then identify one matched control student from the sample for the same breakthrough university, and go on to regress earnings at age 27 on pair fixed effects and an indicator for applications.

We conduct two different matching procedures. In the first, induced applicants are matched with students from control schools applying to university for the same major in the same year; in the second, induced applicants are matched with students from the same school as themselves in a pre-treatment year. (We obviously cannot match exactly on year when matching pre-treatment and post-treatment students, and we do not match on major within the school because there are often no available matches for a specific school-major pair.) Within each of these sets, we then match exactly on students' quintile of neighbourhood income, ventile of GCSE grades, and an indicator for taking at least three A-Levels. Finally, we select one nearest neighbour from within the exactly matched set for each student, based on the Mahalanobis distance over gender, ethnicity, region of the UK (for the analysis matching students at different schools only), and the continuous GCSE grade variable. Induced applicants who have no available exact matches on the relevant variables are discarded from the estimation.

### 4.3 Complier characteristics

To understand who responds to breakthroughs, we can think of breakthroughs as an instrument for applications to the breakthrough university that holds conditional on school and year fixed effects, and then use standard IV methods to estimate mean characteristics for compliers – that is, students who apply to the breakthrough university if and only if they respond to a breakthrough. In particular, let $a_{ist}$ indicate whether a student $i$ applies to the breakthrough university. For any observable characteristic $X_{istu}$, we regress

$$X_{istu} a_{istu} = \alpha_{su} + \gamma_{tu} + \beta a_{istu} + \varepsilon_{istu} \tag{9}$$

instrumenting for $a_{istu}$ with the post-treatment dummy, $I(t - T_{su} \geq 0) D_{su}$; the resulting coefficient $\beta$ then estimates the mean of $X$ for compliers. See for instance Angrist, Hull, and Walters (2023).

# 5 Effects of university breakthroughs

## 5.1 Effects of breakthroughs to Cambridge and Oxford on applications

First, we present an illustrative example using breakthroughs to the universities of Oxford and Cambridge. We use these two universities as an example of the university-level event study design for two reasons: first, they are generally considered to be particularly elite universities; second, they are generally seen as *equally* selective and elite, to the extent that they are often just referred to by the portmanteau 'Oxbridge'. Note that this example uses the UCAS-only data extract, rather than the main LEO extract used for the rest of the analysis (see Section 2.2).

We see from this analysis that a breakthrough to Cambridge encourages applications to Cambridge, but not to Oxford, and vice versa. Figure 4a plots coefficients on the relative time indicators from (5), where the treated schools are those that experience a breakthrough to Cambridge and the outcomes are application to Oxford and Cambridge as indicated; Figure 4b does the same, but for schools that experience a breakthrough to Oxford. We see an increase in applications to Cambridge in panel (a) at the year of the breakthrough to Cambridge, rising by 0.6–0.8 percentage points, while the application rate to Oxford does not significantly increase. Similarly, in panel (b), there is an increase of 0.4–0.8pp in applications to Oxford following the breakthrough to Oxford while the application rate to Cambridge stays largely constant. The effects persist at least four years after the breakthrough. This persistence may reflect the creation of a pipeline, in which students apply to and attend the university at higher rates in the years immediately after the breakthrough, and subsequent students respond to these students' enrolment at the university.

This pattern illustrates our key finding, which we will show generalises across universities: following a breakthrough to a particular university at a school, applications to that university persistently increase, but applications to comparable, similarly-ranked universities do not. If breakthroughs were simply the result of a general increase in student ability (beyond that which is absorbed by our test score controls), or a change in school application guidance or policies that encourages students to apply to more ambitious universities, then we would expect applications to increase at top universities in general, rather than just the particular university that experiences a breakthrough.

This result is particularly surprising in the specific case of Cambridge and Oxford: both universities have similar application procedures that differ from the vast majority of other UK courses (for instance, both universities have an application deadline three months earlier than most other courses, bespoke admissions tests in addition to A-Levels, and interviews with faculty for all applications). *Ex ante*, many plausible explanations for the effect of breakthroughs to Cambridge on applications to Cambridge would centre on information about these procedures that would also be informative about and encourage applications to Oxford. Yet instead we find effects concentrated at Cambridge, and vice versa for breakthroughs to Oxford; any mechanism that explains the effects of breakthroughs must therefore be highly *university-specific*.

### 5.2 Where breakthroughs draw applications away from

Figure 5 generalises these results to a broader range of universities, pooling across universities with similar ranks as described. We focus on the top 30 ranked universities; while we cannot name the universities in this analysis, this is roughly equivalent in size to the Russell Group of 24 selective universities, and comprises around the top quarter of the overall distribution of universities. The first bar in each panel of Figure 5 indicates the effect of breakthroughs in a difference-in-differences framework (replacing the relative time indicators from the event study above with a single post-treatment indicator), as in equation (8). We see an increase in application rates of 0.5–1 percentage point across the range of universities. The pattern that breakthroughs increase applications thus generalises beyond the most elite universities, suggesting that the mechanism involved is not unique to these universities.

If applications to breakthrough universities increase following a breakthrough, this must draw applications away from other universities, given that the cap of five applications is typically binding in the UK. As described in section 4.1, we construct a mutually exclusive and exhaustive set of outcomes based on students' application portfolios: whether they applied to the breakthrough university, whether they applied to a different university in the same selectivity tier, whether they applied to a higher-ranked university (but none in the breakthrough tier), and whether they applied to a lower-ranked university (but none in the breakthrough tier or above).

The results about breakthroughs to Oxford and Cambridge suggest that, while breakthroughs are not associated with *increased* applications to similarly-ranked universities, they do not decrease them, and this pattern also generalises to other universities. Across the selectivity spectrum, we see that applications to universities ranked similarly to the breakthrough university see virtually no change following a breakthrough. There is similarly no effect on applications to higher-ranked universities, except for breakthroughs in the lowest of the six tiers we consider (ranks 26–30), where these applications do decline by 0.4 percentage points. Even at this tier of university, however, there is a larger negative effect on applications to lower-ranked universities, and among universities ranked 1–25, virtually all of the increase in applications to the breakthrough university is explained by a decrease in lower-ranked portfolios. Dividing the effect size by the pre-treatment mean of each portfolio outcome yields a percentage increase of 30–40% in applications to the breakthrough university, while the percentage increases or decreases in the other portfolio outcomes are less than 2%,[12] reinforcing the point that the influence of breakthroughs is highly concentrated at the university in question.

The effect of breakthroughs, at least to the top 25 or so universities, is thus to increase the ambition of some students' application portfolios by encouraging students to apply to the breakthrough university when they would otherwise have applied only to lower-ranked universities. This raises the importance of these breakthrough effects and the underlying exposure effects for welfare: if breakthroughs simply moved applications around similarly ranked universities, this would be unlikely to have major effects on students' long-run outcomes, but as they substantially increase the rank of the university that students apply to, they have the potential to substantially increase a student's earnings. We next evaluate the extent to which students seem to realise these benefits, as well as other longer-run outcomes.

### 5.3 Graduation and early-career outcomes for induced students

So far, we've seen that applications to universities increase following a breakthrough and that this draws applications away from lower-ranked universities. But merely applying does not necessarily mean students attend these universities at a higher rate, or that they graduate successfully or go on to benefit in the labour market from attending. Our administrative data lets us extend the analysis to these longer-term outcomes.

Figure 6 pools across the top 5 universities, and plots the event study first for applications to the breakthrough university, and then for the outcomes of receiving an admission offer, accepting the offer, enrolling at the university, and graduating successfully from the university. Enrolment and graduation from the university are mechanically 0 in the pre-period, but the magnitude of the increase in enrolment and graduation is still informative. While enrolment increases by 0.3–0.4 percentage points compared to a 0.4–0.6 percentage point increase in applications, the increase in enrolment is persistent, and constitutes 60–85% of the increase in applications. Furthermore, of those induced to enrol by the breakthrough, almost all students graduate, given that the coefficients in the enrolment and graduation event studies are nearly identical. Taking this together, we can at least say there is no clear evidence that the students who are induced to enrol by breakthroughs are mismatched at these universities; most students who are induced to enroll go on to graduate successfully.

More generally, Appendix Table B2 illustrates the effects of breakthroughs on the university that students enrol at, as opposed to their application portfolios. Broadly, we see similar patterns to applications: an increase in attendance at the breakthrough university and a decrease in the number of people attending lower-ranked universities. However, we do see increases in the number of students who are unplaced in that university cycle. This is an important caveat to the broadly positive results on the effects of breakthroughs for student welfare: the more ambitious application portfolios induced by breakthroughs are more risky, and some students lose out by failing to attend

12. This is because the baseline rate of applications to the one breakthrough university is substantially lower to the share of students applying to the lower portfolio tier.

university in that cycle. On the other hand, students are free to apply again in the next application cycle, so this result may overstate effects on final university attendance. The results below indicate that students who do attend the breakthrough university benefit, and that the average earnings effect for induced applicants is positive, but there are potentially costs for some students.

We can also compare outcomes for students who enrol at a university from a breakthrough school to typical enrollees at the university. To do this, we can simply regress an indicator for enrolling at a university following a breakthrough on student characteristics within each university. We control for university-by-major-by-year fixed effects, so that breakthrough students are being directly compared to the other students on their course. Table 2 provides the results of this comparison. Despite breakthrough students being lower income and having somewhat lower GCSE grades, they are, if anything, slightly more likely to graduate successfully from their course than typical enrollees: they are 5 percentage points more likely to have completed a degree within 4 years of finishing high school. They are 2 percentage points less likely to receive a first-class degree (roughly corresponding to the top 30% of university performance) than typical enrollees, but 2 percentage points more to receive a 2:1 (ranking between around the 30th and 80th percentiles at their university, though these differences are not statistically significant), and their odds of receiving *either* a 2:1 are the same as typical enrollees. Thus this analysis also produces no evidence of direct mismatch; the degree classification results suggest that breakthrough students are around the middle of the performance distribution for their university rather than the top, but there is no evidence that they are failing at high rates.

Given the relatively small magnitude of breakthrough effects in absolute terms—breakthroughs tend to increase applications by around 0.5–1 percentage points—event studies using earnings as the outcome are underpowered.[13] As an alternative, we identify students who apply to the breakthrough university following a breakthrough at their school and compare them with untreated students who are matched on observables, as described in section 4.2. Note that this includes all induced applicants, so we are not restricting to students who successfully place at the breakthrough university. Figure 7 illustrates that across both matching schemes (matching with students at control group schools applying in the same year for the same major, and matching with students at the same school in years before the treatment), induced applicants to the top 10 universities have earnings that are several thousand pounds higher than those of their matched controls. By age 27, induced applicants have earnings £4,414 higher than matched controls from control group schools, and £4,003 higher than matched controls from pre-treatment years at their own school (adjusted for inflation). The magnitudes of these effects are large, and broadly consistent with the effect sizes for the top 10 universities presented in Figure 1c, where the average graduate of a top 10 university earns £4,600 more than a student attending a university near the median of the quality ranking. Again, there is no evidence of the students who apply following a breakthrough being made worse

13. The reduced form effect of breakthroughs on earnings, pooling across the top 10 universities in a difference-in-differences setup, is not significantly different from 0 after controlling for GCSE grades; a 2SLS regression instrumenting for applying to the breakthrough university with the post-breakthrough indicator yields a 95% confidence interval of around (-£12000, +£92000), which includes implausibly high positive and negative effects.

off, at least on average.

We can conduct a simple exercise to extrapolate the lifetime effects of these gains. We take the point estimates of earnings premia using each approach at ages 22, 25, and 27, and linearly interpolate earnings premia between these ages. After age 27, we assume that there is no real growth in the earnings premium for induced applicants, so this remains constant until retirement at age 68; this is a conservative assumption given the substantial increase in the earnings premium between even ages 25 and 27. We discount the earnings premium at 3% per year back to age 22, following e.g. Angrist, Autor, and Pallais (2022). We assume no differences in earnings or costs between 18 and 22. Under these assumptions, the control schools matching scheme yields a discounted lifetime earnings effect of £100,073 (US$128,118) and the pre-treatment years matching scheme yields a discounted lifetime earnings effect of £92,362 (US$118,246). These are substantial private returns, and are greater even than the *total* tuition paid by domestic students for a four-year course. Given uniform tuition costs across universities, the *marginal* cost of attending a higher-ranked university is purely a function of increases in cost of living and is likely to be substantially less than this.

### 5.4 Heterogeneity in responses to breakthroughs

Who are the students who respond to breakthroughs? First, following the method described in section 4.3, we estimate characteristics for compliers—that is, students who apply to the breakthrough university as a result of the breakthrough at their school—and compare them to other populations of students. Table 3 provides the results of this analysis, with comparisons to the mean of each variable in the event study sample, at treated schools before treatment, and among the full set of enrollees at the relevant universities. Compliers who respond to a breakthrough by applying are about as likely to be economically disadvantaged as the typical student at their school, but have substantially higher academic ability. Compared to the typical enrollee at their university, they have slightly lower academic ability but are much more likely to be from low-income neighbourhoods or eligible for free school meals, and are less likely to be white. So the marginal students induced to apply to these universities by breakthroughs are disproportionately high-ability in the national distribution and low-income relative to the typical attendee at elite universities, which is exactly the population that policymakers would like to encourage to apply to these universities in order to promote social mobility and reduce undermatching. Breakthroughs serve to diversify the socioeconomic status of the intake at top universities.

These results summarise characteristics of students who respond to breakthroughs. If breakthroughs affect applications by creating exposure to students who have attended top universities, we would also expect stronger effects for students who are more closely connected with the breakthrough students. We cannot observe social connections directly in administrative data, but students who have demographic variables in common with the student are more likely to be connected with them, given homophily in social networks. We can verify this for certain characteristics in data from our RCT, where we ask students to name three friends, and find clear evidence that gender and ethnicity predict friendship (see Appendix Table D1). Students who have been at the same school for longer are also more likely to be connected; a breakthrough student who came to the school just a year before applying to university has less opportunity for interactions with other students and teachers than one who has been at the school for 7 years. We thus consider five dimensions of similarity that may predict social connectedness: low-income status, free school meal (FSM) eligibility, gender, ethnicity, and school at age 16. We focus on school at age 16 because, as discussed in section 2.1, it is common for students in the UK to change school after they complete their GCSEs at age 16, while at the same time many students stay at the same school to complete their A-levels.

Table 4 illustrates how breakthrough effects vary by this heterogeneity, pooling across breakthroughs to all of the top 30 universities. In panel (a), we report the difference-in-difference coefficients interacting with the number of shared characteristics, illustrating how shared characteristics in general affect breakthrough effects. We see that there are essentially no effects on applications if a student shares no characteristics with the breakthrough student, and substantially larger effects for students who share more characteristics. In panel (b), we break this out by specific characteristic, recording the difference-in-difference coefficients and the interaction with an indicator for sharing the specified characteristic. Sharing gender, ethnicity, neighbourhood income or FSM eligibility each raises the effect of a breakthrough by around 0.1–0.25 percentage points (around 30–50% of the total—main plus interaction—effect), while sharing a school at age 16 raises the effect of a breakthrough by 0.5 percentage points (64% of the total effect).

These effects suggest that exposure to breakthrough students and interactions with them at least partly explain breakthrough effects. The heterogeneity by whether students share a school at age 16 is particularly striking: while demographic similarity could partly reflect correlations in preferences or ability, the effects of sharing a school are substantially larger than these, and are plausibly a strong proxy for the strength of social connections at the school and interactions with the breakthrough student while being less likely to predict preferences or ability. The results are at least consistent with an explanation of breakthrough effects in terms of exposure to top universities through the breakthrough student at a school.

### 5.5 Implications for overall university undermatching

We have now established that applications to a top university increase by around 0.5–1 percentage points following a breakthrough, which induces a discrete change in exposure at a school. We also know that low-income students are less likely to be exposed to one of the top 10 universities at their school, as indicated in section 3.3, so they are more likely to be at schools that have not had a breakthrough to top universities and to have commensurately lower application rates. Finally, we saw in section 3.2 that around 40% of the differences in application rates between low-income and high-income students can be explained by lower-income students applying at lower rates.

How much of this application gap can in turn be explained by differences in exposure? We can combine estimates of differences in exposure to students attending top universities between high-income and low-income students with our estimates of the treatment effects of breakthroughs. Taking the latter as the causal effect of inducing exposure to a university on applications, we can now conduct a simple back-of-the-envelope exercise to quantify the effect that equalising exposure across income groups would have on application rates. We do not conduct a full counterfactual exercise, but this back-of-the-envelope exercise serves to provide a benchmark for the magnitude of the estimated effects on applications relative to overall application disparities.

Specifically, let the exposure rates for low-income and high-income students to university $u$ – specifically, the probability of low or high-income students attending a school where at least one student has attended that university in the last three years – be $e_u^l, e_u^h \in [0, 1]$ respectively, and let the treatment effect of exposure at university $u$, as estimated from the difference-in-difference coefficient in the breakthrough event studies, be $\Delta_u$. Then we can predict

$$Pr(\text{apply}_u \mid l, e_u^h) = Pr(\text{apply}_u \mid l, e_u^l) + \Delta_u(e_u^h - e_u^l) \tag{10}$$

To align more closely with the analysis in section 3.2, we can condition these calculations on GCSEs. We pool GCSE grades into ventiles (since we are not powered to estimate treatment effects effects conditional on exact percentiles), and then estimate the difference-in-differences regression separately within each ventile of GCSE grades for university $u$ to get a grade-dependent treatment effect, $\Delta_u(g)$. We then combine this with exposure rates similarly calculated by GCSE ventile, $e_u^h(g), e_u^l(g)$, to get

$$Pr(\text{apply}_u \mid g, l, e_u^h) = Pr(\text{apply}_u \mid g, l, e_u^l) + \Delta_u(g)\left(e_u^h(g) - e_u^l(g)\right) \tag{11}$$

Finally, we can sum these effects over each of the top 10 universities to get

$$Pr(\text{apply top 10} \mid g, l, e^h) = Pr(\text{apply top 10} \mid g, l, e^l) + \sum_{u \in \{1,\ldots,10\}} \Delta_u(g)\left(e_u^h(g) - e_u^l(g)\right) \tag{12}$$

making use of the empirical result that breakthroughs to university $u$ do not affect applications to any similarly ranked university $u'$ to simplify the calculation.

Figure 8 plots top 10 application rates for low-income and high-income students by GCSE grades, and the counterfactual application rate $Pr(\text{apply top 10} \mid g, l, e^h)$ calculated as in (12); panel (b) plots the fraction of the overall application gap explained by exposure for each GCSE level. At the top end of the GCSE distribution, around 30% of the difference in applications can be explained by differences in exposure. This falls as we move down the GCSE distribution to around 10–15%. In absolute terms, application rates are predicted to increase by 2.5 percentage points for low-income students in the top ventile of the GCSE distribution. Combining the estimated effects of breakthroughs to universities with the observed inequality across schools in the enrollment decisions of past cohorts thus suggests that differences in exposure at a school explain a substantial fraction of the differences in application rates to top universities.

### 5.6 Interpretation of breakthrough effects

What could explain the patterns that we see above? The key result that we see is that breakthroughs induce an increase in applications to the specific university that sees a breakthrough, but not to other equally selective universities. The lack of evidence of effects on similar universities rules out interpretations where the increase in applications to the breakthrough university is driven by a sudden increase in the ability or academic performance of students, as this should lead to a broader increase in the selectivity of applications. For further evidence that an increase in student ability is not the key mechanism, Appendix Table B3 illustrates that different ability controls, as well as a matched event study specification,[14] make little difference to the overall patterns of application effects. The university-specific nature of the effects also rules out an interpretation in terms of the school changing application advice and encouraging its students to apply more ambitiously, which would also be expected to affect peer universities.

One remaining explanation is that this is associated with the university in question conducting outreach to the school to encourage students at that school to apply. While we do not observe outreach activities directly,[15] outreach activities are generally targeted within the local area of the university. University outreach to schools is often coordinated through the 'Uni Connect' Programme, which is organised around 29 regional hubs across England. This programme connects universities and schools within geographical regions of the UK but does less to coordinate outreach across regions (see Burtonshaw et al. 2024 for an evaluation of Uni Connect). If breakthrough effects were driven by outreach, we would therefore expect effects to be concentrated in the geographical area of the university.

As Appendix Table B4 indicates, this does not seem to be the case. Column (1) restricts the application event study to students in a different region of the England from the breakthrough university; column (4) restricts to students above the median distance from the breakthrough university; and column (6) restricts to students over 30 miles away from the breakthrough university. In each case, we see a significant increase in applications following the breakthrough; indeed, effects are generally larger for students in different regions to the university or above 30 miles away than nearer students, although breakthrough effects on applications for schools above the median distance are generally smaller. Overall, there is no evidence that application effects are solely driven by the effects of university application decisions.

An explanation that is consistent with university-specific effects is the arrival of a new teacher that has a connection to a specific university, and encourages applications to this university. This is unlikely to operate through the teacher providing specific information about applications to the university, as there is a common application process for most universities, but it is possible that the

14. In the matched specification, we conduct 1:1 matching of treatment and control schools based on students' university application behaviour and ability in the pre-breakthrough window, 2007–2009, and include match-pair by year fixed effects in the specification, so effects are identified from changes in outcomes for treated schools relative to a matched control school. This reduces pre-treatment disparities in outcomes between treatment and control schools.

15. Data on university outreach activities is collected for many universities by the Higher Education Access Tracker (HEAT). These data are not currently available to be linked with LEO, but we are working with HEAT and with the Department for Education to implement this linkage.

teacher could convey other information that encourages students to apply. In the UK system, the teachers that students spend most time with are subject teachers, as students spend most of their time in school studying towards their A-level subjects. Two students who share no A-level subjects are thus unlikely to spend a significant amount of time interacting with the same teacher at their school. If there are still breakthrough effects for students who share no A-level subjects with the breakthrough student, this is evidence that the arrival of a new teacher cannot fully explain the effects of breakthroughs.

Appendix Table B5 reports difference-in-difference coefficients for students who share no subjects with the breakthrough student at their school, and the interaction of the post-treatment dummy with an indicator for sharing at least one subject. The main effect is an increase of 0.2 percentage points, indicating that breakthroughs affect the application behaviour even of students with no subjects in common with the breakthrough student. This component of breakthrough effects is less likely to be driven by teacher effects. There is a large interaction effect of 0.9 percentage points, meaning there is some component that may be driven by teacher effects, but this positive interaction effect may also reflect stronger social interactions (as students who share an A-level subject are more likely to be socially connected) and differences in preparation for university (as certain A-level subjects may be preferred by the university in admissions). As evidence for the latter channel, sharing a subject with the breakthrough student is associated with an 0.8 percentage point higher application rate even prior to the breakthrough, which is likely to reflect these students choosing subjects that make them better suited to an application. Nevertheless, the fact that there is a clear and substantial increase in the application rate for students who do not share any subjects indicates that teacher effects cannot fully explain the effects that we see.

The patterns of heterogeneity in breakthrough effects described in section 5.4 and Table 4 suggest that there are stronger effects for students who are more likely to be socially connected. There are stronger effects for students in the same income group, of the same ethnicity, of the same gender, who share FSM status, and who attended the same school at age 16. The interaction effect is substantially larger for sharing an age-16 school than for any other characteristic, and sharing a school is a property that is (a) highly likely to affect the strength of social connections between students, and (b) is less likely to explain other aspects of preparation for university applications.[16] Gender, neighbourhood income, ethnicity, and free-school meal eligibility are also likely to affect social connections: using social network data collected as part of the RCT described in later sections, we can verify that gender strongly predicts social connections, while ethnicity has a smaller but still significantly positive effect (see Appendix Table D1). The heterogeneity in effects within schools is evidence against interpretations of the breakthrough effects in terms of interventions that affect all students, and suggests that similarity and social proximity to the breakthrough student mediate the effects, so that connections to the breakthrough student matter.

16. One other interpretation of the age-16 school effect is that breakthrough effects are stronger at the types of schools that offer age-16 provision – where naturally more students would have gone to the same school at age 16 – but Appendix Table B6 shows that we see similar effects even when excluding school types that do not offer age-16 provision, such as sixth form colleges and further education colleges.

One other pattern of effects is that the increase in applications is persistent for at least four years after the effect. Social connections are likely to decay over time – the original breakthrough student is less likely to have any interaction or social connection with students at their own school four years later – but, as we see persistent increases in enrolment as well as applications, this may reflect a pipeline effect where the students attend encourage applications and attendance in the next year, and the students who respond encourage applications and attendance in the following year, and so on.

Ultimately, however, fundamental limitations of the administrative data mean that it is challenging to entirely rule out alternative mechanisms. Our RCT addresses this limitation by inducing precisely defined exposure to past university attendees—thus ruling out any of the alternative interpretations of breakthroughs discussed in this section—and measuring beliefs and application intentions. We proceed to describe the design and results of the RCT in sections 7 and 8. Before moving to this portion of the paper, we lay out a conceptual framework to illustrate how students' beliefs about university and resulting application choices might respond to exposure to past students, highlighting the class of mechanism that we focus on.

# 6 Conceptual Framework

## 6.1 A framework for belief updating about a risky university

Why would a students' application decisions react to students they encounter? To fix ideas, we introduce a straightforward model of Bayesian belief updating from peers which informs a university application decision. This framework highlights the role of information provided by other students in informing beliefs about payoffs at particular universities.

Individual $i$ is deciding between actions $y_i \in \{0, 1\}$ where $y_i = 0$ denotes attending a safe university with known payoff 0 and $y_i = 1$ a risky university. If the individual attends the risky university, they get a payoff

$$U_i = \kappa W(u_i) + (1 - \kappa) W(v_i) \tag{13}$$

where $u_i$ denotes their payoff from their experience at university, $v_i$ their payoff from their academic returns to university, $W(u) = \frac{1}{\gamma}\left(1 - e^{-\gamma u}\right)$ is a CARA utility function with risk aversion parameter $\gamma$, and $\kappa \in [0, 1]$ weights the two components of utility.

A student has two parameters: a social type $\theta_i \in [0, 1]$ and an ability type $a_i \in [0, 1]$. Ability $a_i$ represents academic ability, as proxied by student's test scores in the data. Social type should be interpreted as a composite concept that captures all of the demographic and social factors that may affect students' ability to succeed at an elite university, where higher values correspond to fitting in better.

Students know their own $\theta_i$ and $a_i$, and they know that $u_i, v_i$ depend on $\theta_i$ and $a_i$, but they are uncertain how much each of these components matter for payoffs. They assume that payoffs take the linear parametric forms

$$u_i = \beta_0^u + \beta_1^u \theta_i + \beta_2^u a_i + \varepsilon_i^u = x_i' \beta_u + \varepsilon_i^u \tag{14}$$
$$v_i = \beta_0^v + \beta_1^v \theta_i + \beta_2^v a_i + \varepsilon_i^v = x_i' \beta_v + \varepsilon_i^v \tag{15}$$

where:

$$\beta^u \sim N(b^u, V^u) \tag{16}$$
$$\beta^v \sim N(b^v, V^v) \tag{17}$$
$$\varepsilon_i^u \sim N(0, \sigma_u^2) \tag{18}$$
$$\varepsilon_i^v \sim N(0, \sigma_v^2) \tag{19}$$
$$Cov(\beta^u, \beta^v) = 0 \tag{20}$$
$$Cov(\varepsilon_i^u, \varepsilon_i^v) = 0. \tag{21}$$

Then $u_i$ is normally distributed conditional on $x_i$, with

$$E[u_i \mid x_i] = x_i' b^u \tag{22}$$
$$Var[u_i \mid x_i] = x_i' V^u x_i + \sigma_u^2 \tag{23}$$

and the same is true for $v_i$. It follows that the certainty equivalent of the payoff from the risky university for student $i$ is

$$CE_i = \kappa \left( x_i' b^u - \frac{1}{2}\gamma \left( x_i' V^u x_i + \sigma_u^2 \right) \right) + (1-\kappa) \left( x_i' b^v - \frac{1}{2}\gamma \left( x_i' V^v x_i + \sigma_v^2 \right) \right) \tag{24}$$

and $i$ will choose the risky university if this is positive.

Students then observe the characteristics and a noisy signal of another student: specifically, they observe for some other student $j$

$$(\tilde{u}_j = u_j + \eta_j^u, \tilde{v}_j = v_j + \eta_j^v, x_j) \tag{25}$$

where $\eta_j^u \sim N(0, s_u^2), \eta_j^v \sim N(0, s_v^2)$ They then update their beliefs about the parameters $\beta^u, \beta^v$, and then update the resulting expected utility of applying to the risky university as summarised by $CE_i$. We assume that the variances of the noise terms $\sigma_u^2, \sigma_v^2, s_u^2, s_v^2$ are known. This setup imposes structure on how students respond to seeing other students at the university. Error terms are independent, and students already know their own ability and social type, so they do not update about either of these terms. What they learn about from observing another student is how much their ability and social type matters for their payoffs at the risky university, as represented by the $\beta^u, \beta^v$ coefficients.

Updating $\beta^u$ and $\beta^v$ is simply a Bayesian linear regression problem. Focusing on the update for $u$ (the analogous formulas naturally hold for $v$), the posterior distribution for $\beta^u$ is $\beta^u \sim N(\tilde{b}^u, \tilde{V}^u)$, where

$$\tilde{V}^u = \left((V^u)^{-1} + \frac{1}{\sigma_u^2 + s_u^2} x_j x_j'\right)^{-1} \tag{26}$$

$$\tilde{b}^u = \tilde{V}^u \left(\frac{1}{\sigma_u^2 + s_u^2} x_j' u_j + (V^u)^{-1} b_0\right) \tag{27}$$

We can rewrite $\tilde{b}^u, \tilde{V}^u$ in forms that highlight updating as follows:

$$\tilde{V}^u = V^u - \frac{V^u x_j x_j' V^u}{\sigma_u^2 + s_u^2 + x_j' V^u x_j} \tag{28}$$

$$\tilde{b}^u = b^u + \frac{1}{\sigma_u^2 + s_u^2 + x_j' V^u x_j} (\tilde{u}_j - x_j' b^u) V^u x_j \tag{29}$$

Posterior variance is, intuitively, lower than prior variance, in the sense that the posterior variance is equal to prior variance minus a positive semidefinite matrix. Equation (29) implies the following expression for student $i$'s update to their expected payoff:

$$E[u_i \mid x_i, x_j, \tilde{u}_j] - E[u_i \mid x_i] = (\tilde{b}^u - b^u)' x_i \tag{30}$$

$$= \left(\frac{1}{\sigma_u^2 + s_u^2 + x_j' V^u x_j}\right) \left(\tilde{u}_j - x_j' b^u\right) \left(x_i' V^u x_j\right) \tag{31}$$

These three components each have clear interpretations. $\left(\frac{1}{\sigma_u^2 + s_u^2 + x_j' V^u x_j}\right)$ is a term reflecting idiosyncratic noise in the signal that student $i$ observes of student $j$'s payoff: this term is always weakly positive, but $i$ updates less when noise is higher. $\left(\tilde{u}_j - x_j' b^u\right)$ is effectively the residual in student $j$'s payoff given the prior coefficients. This term may be positive or negative. $\left(x_i' V^u x_j\right)$ is the covariance of the predictable component of student $i$ and student $j$'s payoffs, given the prior covariance matrix for all of the regression coefficients: $x_i' V^u x_j' = Cov(x_i' \beta^u, x_j' \beta^u)$. Intuitively, it is likely that this term is positive, but it could be negative if there is high negative covariance between certain coefficients. To make interpretation more straightforward, we impose the condition that

$$x_i' V^u x_j = Cov(x_i' \beta^u, x_j' \beta^u) \geq 0 \tag{32}$$

Finally, the posterior distribution of $u_i$ is normal, with:

$$E[u_i \mid x_i, x_j, u_j] = \tilde{b}_0^u + \tilde{b}_1^u \theta_i + \tilde{b}_2^u a_i = x_i' \tilde{b}^u \tag{33}$$

$$Var[u_i \mid x_i, x_j, u_j] = x_i' \tilde{V}^u x_i + \sigma_u^2 \tag{34}$$

So, in summary, after exposure, the certainty equivalent of the risky university becomes

$$CE_i = \kappa \left(x_i' \tilde{b}^u - \frac{1}{2}\gamma \left(x_i' \tilde{V}^u x_i + \sigma_u^2\right)\right) + (1 - \kappa) \left(x_i' \tilde{b}^v - \frac{1}{2}\gamma \left(x_i' \tilde{V}^v x_i + \sigma_v^2\right)\right) \tag{35}$$

and the change in the certainty equivalent is equal to

$$\Delta CE_i = \kappa \left[ x_i'(\tilde{b}^u - b^u) + \frac{1}{2}\gamma \left( x_i'(V^u - \tilde{V}^u)x_i \right) \right] \tag{36}$$

$$+ (1-\kappa) \left[ x_i'(\tilde{b}^v - b^v) + \frac{1}{2}\gamma \left( x_i'(V^v - \tilde{V}^v)x_i \right) \right] \tag{37}$$

$$= \kappa \left[ \left( \frac{1}{\sigma_u^2 + s_u^2 + x_j'V^u x_j} \right) \left( \tilde{u}_j - x_j'b^u \right) \left( x_i'V^u x_j \right) + \frac{1}{2}\gamma \left( \frac{x_i'V^u x_j x_j'V^u x_i}{\sigma_u^2 + s_u^2 + x_j'V^u x_j} \right) \right] \tag{38}$$

$$+ (1-\kappa) \left[ \left( \frac{1}{\sigma_v^2 + s_v^2 + x_j'V^v x_j} \right) \left( \tilde{v}_j - x_j'b^v \right) \left( x_i'V^v x_j \right) + \frac{1}{2}\gamma \left( \frac{x_i'V^v x_j x_j'V^v x_i}{\sigma_v^2 + s_v^2 + x_j'V^v x_j} \right) \right] \tag{39}$$

These formulæ characterise updating in response to a single observation; we can apply them repeatedly to capture exposure to multiple students.

### 6.2 Interpretation

Given this framework, how does exposure affect applications? The mechanism in this framework is that exposure affects students' beliefs about either $u_i$—interpreted as the non-academic experience at university—or $v_i$—interpreted as the academic experience. Specifically, exposure causes students to update their beliefs about how social type $\theta$ and ability $a$ affects payoffs $\kappa W(u) + (1-\kappa)W(v)$ at university, as represented by the coefficient vectors $b^u, b^v$; given their own social type $\theta_i$ and ability $a_i$, they then update their beliefs about their own payoffs $W(u_i), W(v_i)$.

If student knew exactly how their social type and ability would affect their payoffs at university, the covariance matrix of coefficients $V^u$ would be the zero matrix, and exposure would have no effect on beliefs or applications. This could be the case even if students are still uncertain about their final payoff at the risky university; if they know the value of the $\beta^u, \beta^v$ coefficients exactly, there is still variance in payoffs from the idiosyncratic noise terms $\varepsilon_i^u, \varepsilon_i^v$, but since these terms are fully idiosyncratic, a student does not learn anything about them from exposure to another student $j$. The effects of exposure on applications are wholly driven by students changing their beliefs about the *relationships* between social type or ability and university payoffs and the expected level of payoffs.

In an environment without full information about the coefficients, exposure affects applications through two channels. First, additional information from the other student reduces the variance in the coefficient estimates and thus the variance in payoffs. For risk-averse students, this effect always makes attendance at the risky university more attractive. Second, exposure also affects mean beliefs: in particular, if student $i$ is positively surprised by the signal of student $j$'s payoff ($\tilde{u}_j - x_j'b^u > 0$), they will update beliefs about their own payoff positively under the maintained assumption that $x_i'V^u x_j = Cov(x_i'\beta^u, x_j'\beta^u) > 0$. This component of updating has a mean-zero effect on expected utility for the risky university if beliefs are correct, so taken together, exposure will tend to encourage applications on average even with rational priors. If students have systematically pessimistic priors, this effect will be stronger. Exposure can affect both beliefs about life at university and academic performance at university through both of these channels.

It follows from this framework that the mechanisms by which exposure can affect applications depend on students' prior information and beliefs. *Ceteris paribus*, students' beliefs will update more about the payoff that they are least certain about to begin with, and thus the effects of exposure will operate primarily through this payoff. This is both because the posterior variance will be reduced less by exposure when prior variance is low, and because posterior beliefs about the coefficients will change less when prior variance is low. In particular, suppose that students receive more information about their likely academic payoff at university from other sources than their social payoff and so have more precise prior estimates of $\beta^u$ than of $\beta^v$. Low uncertainty about academic payoffs will mean that even exposure to someone with an unexpectedly high academic payoff will not do much to alter beliefs, while beliefs about social payoffs will be much more sensitive to exposure.

The effects of exposure also depend on the precision of the signal that students receive, as captured by the variance terms $s_u^2, s_v^2$. If students receive detailed information about another student's experience at university (i.e. $s_u^2$ is low), this will both decrease posterior variance more and increase the responsiveness to the signal $\tilde{u}_j$ observed.

Finally, this framework also provides insight into when students will respond more to students who are more similar to them. We can show that

$$\frac{d\Delta CE_i}{d\theta_i d\theta_j} = \kappa \left[ \left( \frac{1}{\sigma_u^2 + s_u^2 + x_j' V^u x_j} \right) \left( \tilde{u}_j - x_j' b^u \right) Var(\beta_2^u) \right] \tag{40}$$

$$+ (1-\kappa) \left[ \left( \frac{1}{\sigma_v^2 + s_v^2 + x_j' V^v x_j} \right) \left( \tilde{v}_j - x_j' b^v \right) Var(\beta_2^v) \right] \tag{41}$$

so that higher-social type students always respond more in absolute terms to exposure to other higher-social type students (since $Var(\beta_1^u), Var(\beta_1^v) \geq 0$), and they are more sensitive to the social type of the student they are exposed to when they are more uncertain about how social type predicts outcomes. The same is true for the cross-partial derivative with respect to ability. Intuitively, if you know how social type affects payoffs, you can interpret what the payoffs of someone with a different type mean for your own payoff, whereas if you are unsure then you will learn more from observing someone similar as you do not have to extrapolate as far.

The framework we provide here illustrates how beliefs about universities and application decisions respond to exposure to past students. We show that exposure will tend to encourage applications to a risky university by increasing the precision of students' beliefs about the university, and describe when beliefs and payoffs will respond to exposure. This framework provides context for the design of the RCT, which we turn to next.

# 7 RCT design

## 7.1 Mechanisms for the effects of breakthroughs

Our analysis of the quasi-experiments provided by breakthroughs indicates that changes does encourage applications, and that students who respond to breakthroughs to top universities tend to succeed at these universities, graduating at typical rates and seeing higher earnings. Evidence that effects are stronger for more similar students – and particularly that there are substantially stronger effects for students who have attended the same school for longer – suggests that these effects in part reflect exposure: the opportunity to interact with a student who has attended a particular university affects where students choose to apply.

But why should students respond to exposure? University choices are made in an information-rich environment: there is detailed information available about universities and courses available from university websites, from online forums and social media, from events run by universities, from school programmes supporting university application, and from university prospectuses and promotional materials. Universities provide relatively clear information about the grades required to be admitted to specific courses. In an environment where students have full information about their own ability and their payoffs at different universities, idiosyncratic exposure to a student who has attended a particular university should not affect applications, as illustrated in the framework set out above.

Thus, any effect of exposure on applications implies that students are not certain about their payoffs. As in that framework, we can divide the uncertainty into two components: uncertainty about their academic success at the university, and uncertainty about the non-academic experience that they would have—particularly their ability to fit in and make friends at the university. Our motivating hypothesis about these two channels is that most information about universities made available to students focuses on academic preparation and experience, so students are likely to have lower expected utility about social experience at unfamiliar, risky, or elite universities. If this is the case, students would tend to seek out information about the social environment from peers, and exposure would predominantly affect applications by improving students' beliefs about their social fit at these universities, while having little effect on academic beliefs.

We cannot distinguish between effects on these two components using choice data alone; patterns of heterogeneity in the administrative data suggest that social fit may matter, but are not conclusive. The distinction is important for policy. If students are concerned about their ability to succeed academically at university, interventions to address this should focus on making students feel more confident about their academic preparation, while if they are more concerned about social fit then students talking about fitting in at elite universities may be more helpful.

The framework also predicts that students may respond more to students who are more similar to them, particularly when they are uncertain about how much that characteristic affects payoffs. The patterns of heterogeneity in section 5.4 suggest this, but that interpretation is complicated by the fact that sharing demographic characteristics may increase the *probability* of an interaction as well as the effect of a *given* interaction. Furthermore, by definition breakthrough students attended the same school at least in the year of university application, which is likely to be a strong proxy for similarity on a variety of observable and unobservable characteristics. Understanding whether more similar students have stronger effects is also relevant to understand whether university outreach efforts should focus on matching students' characteristics or simply maximising the breadth of outreach.

Finally, the framework highlights the importance of the precision of a signal from another student—i.e. how much information is conveyed in an interaction. Again, we cannot observe this directly for breakthroughs, and the nature of interactions that are effective determines the likely cost-effectiveness of different interventions. Briefer, light-touch interactions that convey less information are cheaper and easier to scale, so it is relevant to understand whether these interactions affect beliefs and applications or whether longer interactions are required.

To address these questions, we designed, pre-registered, and conducted a field experiment at schools across the UK. The experiment targets the open questions remaining from the administrative data analysis. All students, including those in an active control arm, receive an informational workshop, bringing students up to a comparable baseline level of information about universities in general. We then randomly provide exposure to students who went to different universities by offering participating students video clips of students talking about university, connections with mentors for one-on-one conversations, and subsidised visits to universities. We additionally randomly vary whether students receive a mentor with whom they do or do not share demographic characteristics with. We measure baseline beliefs about several universities and how these beliefs update in response to treatments, and collect data on student demographics.

We use the experiment to distinguish between the mechanisms at play in the conceptual framework. If the different interactions with university attendees that we induce in the experiment provide information that affects students' academic prospects at universities, then these treatments should affect student beliefs about their probability of admission to the university and / or successful graduation from the university. If these treatments instead affect beliefs about students' ability to fit in and make friends at university, this is evidence in favour of the mechanism that interactions with past attendees helps students learn about the social environment at the university. If exposure in more direct forms has stronger effects on applications, this indicates how precise signals need to be in order to provide additional information over the baseline. And if students respond more to mentorship from more similar students, this directly provides evidence on whether similarity mediates the effects of each of these mechanisms.

The framework in section 6 highlights a particular set of mechanisms relating to students' beliefs about the university; it does not seek to provide an exhaustive model of all the ways in which a breakthrough could encourage applications. Our experiment shuts down some additional channels that might at least in part explain effects of breakthroughs in the administrative data. First, knowing someone from your school has attended a university might directly connect you to social networks at the university—you might join a social group or club with that older student. But the treatments that we provide do not provide lasting enough social connections for this to be likely: students have no way to contact speakers in the videos, and generally do not form lasting enough relationships with mentors to stay in contact when they reach university. Second, breakthroughs are by definition students from the same school, and students might infer something either about their own ability or the perception of students from their school by universities from the enrolment decisions of students at the same school; in the RCT, we provide exposure to students from different schools, shutting down this channel. Third, the alternative interpretations of breakthrough effects discussed in section 5.6—such as the arrival of a new teacher or university outreach activities—cannot explain any experimental results, as we provide clearly specified, randomised exposure treatments.

The RCT thus allows us to isolate the effects of interactions with past university attendees on current students' application decisions, to understand how different types of interactions affect choices, to understand what components of beliefs are responsive to exposure, and to understand how much similarity between students matters for the strength of the effects. We now explain the experimental design and implementation in more detail.

### 7.2 RCT treatments

Our treatments provide participating students with different forms of exposure to potentially less familiar universities. We explain each of these forms of exposure below. Students in different treatment arms received different combinations of these treatments, as outlined in Table 5 and in the text below. Figure 9 provides an overview of the design.

#### Active control: workshop about university applications

We invited all students, regardless of treatment arm, who were participating in the programme to attend a workshop about university applications that we organised in their school. The workshop was delivered by a current university student or recent graduate, generally drawn from our pool of mentors (see below for more details on the recruitment and composition of this pool), but in some cases recruited by the school from their alumni. In each case, the presenter or presenters talked through a slide deck that the research team created that provided students with information about the university application process, as well as adding their own commentary based on their experience of university applications and life at university. The slide deck is included in Appendix F.1. Information in the workshop was publicly available online, so an interested student could find the same information themselves.[17]

We designed the workshop to provide key information about the application process. Specifically, our materials discussed how universities make admissions decisions; statistics on grades and

17. The statistics we presented in the workshop were drawn from public data, not our secure administrative data, but in most cases these statistics had not been published in a user-friendly format (a report or press release), so it would be theoretically possible but highly unlikely for students to find these statistics without our workshop. The other information in the workshop was largely drawn from student-oriented advice pages that we collated and would be relatively easy for students to find.

qualifications at different universities; statistics on earnings and students' perspectives on different universities; the application process and timeline; 'degree apprenticeships' and other hybrid courses including work components; student loans, cost of living and student finance; advice on students' personal statements; suggestions about where to find more information; and a description of the mentorship and visit components of the programme. The statistics provided were the most novel component of the workshop, as other information largely mirrored easily accessible information about the application process. Appendix Figures F3–F10 show the statistics presented. We inform students about how universities make admission decisions—including information about how they may have more lenient offer conditions for low-income students—and provide statistics about the qualifications and grades required for admission at different universities, resulting earnings, and statistics from student surveys about their sense of belonging on their course, regret about course choice, and overall satisfaction.

Conceptually, this active control workshop ensured that all participating students had a baseline level of information about the university application process *in general*, allowing our treatments to then shock beliefs and preferences about *specific universities*. It helped to calibrate students' prior beliefs about how ability and social type might affect academic and non-academic payoffs at university by providing statistics about these outcomes, reducing variation in these priors across students. The workshops also served several logistical functions. Having a component of the programme that was available to all students rather than just students in the treatment group was important for school recruitment, as schools would have been less enthusiastic about a programme that a large fraction of their students would get no benefit from. Having a highly salient in-school component of the programme—students were taken out of their lessons to attend the workshop—substantially raised the salience of the programme and engagement with the other treatments, particularly as we instructed workshop presenters to describe and promote the mentorship and visit treatments during the workshop (see Appendix Figure F19).

**Videos**

Students in the relevant treatment groups were shown two videos towards the end of the baseline survey. Appendix Figure E15 illustrates how the videos were displayed to students in the survey. The videos were largely recorded by from current university students or recent graduates in our mentor pool. We provided people recording videos with a list of topics to discuss, covering the university application process and life at university. Speakers were asked to discuss: the school and city the student attended; their A-Levels / other qualifications; how they made their decisions about which universities to apply to; where they got offers from and ultimately attended; student life in their area at university; fitting in and making friends; cost of living and expenses; and teaching quality on their course. Videos generally lasted 3–4 minutes.

Videos provide exposure to a particular university in a manner that is more detailed and personal than generic online information, but more scalable than direct conversations online or in-person. The design of the video replicates what a student might learn in a conversation about university, but without the interactivity and potential for follow-ups of a full conversation. As illustrated in Appendix Figure E14, we introduced these videos as students describing their application process and university experience in a way that might be helpful for students; we did not imply that we were encouraging students to apply to these particular universities.

We selected videos to show students relating to universities that were aspirational but realistic given their predicted grades. Earlier in the baseline survey, we ask students to enter their predicted grades, and converted them into UCAS tariff points, a metric provided by UCAS to compare grades across different qualifications (see Appendix B.1). We then grouped universities into three tiers based on the distribution of students' grades, as recorded in the public statistics on the discoveruni.gov.uk website. Specifically, universities were assigned to one of four tiers based on the 25th percentile of UCAS tariff points of enrolled students at the university. Appendix Table C1 provides the cutoffs for each tier and their A-level letter grade equivalents, as well as some examples of universities in each tier. We then assigned *students* to tiers based on their predicted tariff points, using the 25th percentile groups described in Appendix Table C1 as cutoffs. We assigned all students to at least tier 3 even if their predicted grades fell below the tier 3 cutoff, on the grounds that lower-ranked universities would have the potential to discourage ambitious applications, and only assigned students to tier 1 if they were taking A-levels and had predicted grades exceeding the cutoff, on the grounds that the most selective courses generally do not accept vocational alternatives to A-levels.

Given these assignments, we then showed the students one video recorded by a male student and one by a female student at universities from within their tier. Students who were assigned to the control group were still notionally assigned videos using the same procedure, allowing us to identify the assigned video universities for all students and construct outcomes based on these. Students were not informed about this selection procedure for the videos they saw, so would be unlikely to infer any information about their own relative ability based on the video they were assigned.

**Mentors**

We recruited a set of current university students and recent graduates to act as volunteer mentors for students in the programme. The majority of these mentors signed up through STEM Ambassadors, a STEM-focused volunteering platform. Some were recruited through other channels, such as direct outreach via university partners or via AtkinsRealis, an engineering organisation that supported the programme. Table C3 describes the characteristics of the mentors taking part in the programme. The majority are current undergraduate students, but a substantial minority were older. The sample of mentors is disproportionately female, whereas our student sample is disproportionately male, but the mentor sample has similar levels of economic disadvantage compared to the RCT sample. As our RCT sample, there is a much lower share of white students in the sample of mentors compared with the broader student population.

Students assigned to the mentorship treatment were connected with 1–2 mentors from this pool.

We sent students and mentors an email to connect them, and encouraged them to get in touch to arrange a call to talk about university applications and life at university. We suggested that mentors and mentees could discuss "[mentor's] course, life at [mentor's university], uni life in general, and the application process", but did not provide a script for mentors or prescribe topics, as we wanted to allow for organic conversations and to treat the topics that students chose to discuss as an outcome of interest. Mentors were asked to have at least one 15-minute call with their students, to answer any further questions over email, and to arrange follow-up calls if the student was interested and the mentor was available. Students were also able to ask questions of their mentors over email if they preferred not to have a call.

During the midline survey, and before students were informed whether they would be connected with a mentor, students were asked (a) whether they would like a mentor, and (b) to name three universities that they would be interested in receiving a mentor from. We connected each student in the relevant treatment arm with a mentor from one of these three universities, subject to availability of a mentor in our mentor pool.[18] We also identified a second mentor from a less familiar university to connect students with, based on their university tier and the subject that they intended to apply for. In both cases, students in treatment arms 1a and 1b were matched with mentors with whom they did *not* share a gender, ethnicity, or home region of the UK, while students in arms 2a and 2b were matched with mentors with whom they shared at least one of these characteristics. We describe the matching algorithm in full in Appendix Section C.1.

The mentorship treatment aims to replicate the exposure provided by direct conversation. In contrast to the video treatment, mentors are able to answer the specific questions that the student is most interested in, and to respond to follow-up questions that the student asks. This exposure provides more detailed and relevant information about the mentor's university.

**Subsidised visits**

Finally, for some students, we provide a travel subsidy for visits to a university, motivated by discussions with students in which they discussed how visiting a university before applying was important, but that cost was a barrier. We subsidised costs of up to £75 ($\approx$ \$100); this cap bound for only 35% of submitted reimbursement requests, indicating that this cap covered a substantial share of typical travel costs to universities. For visits, we asked students to nominate a university that they would like to visit in the midline survey (prior to students being informed whether they would be paid), and then offered a visit subsidy to that university to students in treatment arms 1b or 2b. We did not algorithmically assign a visit university to students because of concerns that this would lead to low takeup: even if travel costs are covered, the time costs of a visit to a university are high—generally requiring a full day—and students are unlikely to be willing to do this for a university that they do not have some pre-existing interest in. However, we encouraged students to select a university that they would not otherwise be able to visit in our communications and during

18. Students who responded 'definitely not' to the question of whether they wanted a mentor were not connected with a mentor.

the workshop. The universities targeted for visits were therefore ones that students were considering applying to, but wanted the opportunity to visit in person to decide whether to apply. Students were sent a form where they could submit receipts for their travel to us and claim reimbursement in the form of an Amazon gift card or PayPal payment. Generally, students used these visits to attend organised Open Days, where universities invite prospective applicant to sign up to attend sessions providing details on the university's environment and specific courses.

Visiting a university provides more in-depth exposure than video or mentorship treatments. Students are able to talk to current students during visits, as these students are usually available on university Open Days, but can also experience the campus and the university's city in person, providing precise experiential information that is not available without a physical visit to the university. Furthermore, students are also able to talk to students in their own application cohort who are interested in that university, and can potentially form connections with students who will be in their cohort if they attend the university.

**Treatment arms**

The treatment arms combined assignments of these treatments. The assignments worked differently in each wave (see below for more details on the timing of each wave). In wave 1, we had a control arm C and a treatment arm T: students in the control arm received only the active control workshop, while students in the treatment arm received videos and mentor connections. In wave 2, we introduced two additional dimensions of treatment variation. First, for students assigned to receive videos and mentors, we varied whether these mentors would be demographically matched (on at least one dimension of gender, ethnicity and region of the UK) or unmatched with the student. Second, a subset of students who were assigned to receive videos and mentors were also offered subsidised visits. This yields the active control arm C and 4 treatment arms: T1a, with videos and dissimilar mentors; T1b, with videos, dissimilar mentors, and visit subsidies; T2a, with videos and similar mentors; T2b, with videos, similar mentors, and visit subsidies. Table 5 provides an overview of the treatment arms in each wave.

Students were randomly assigned to one of the treatment arms at the time they completed their first survey, either the baseline or the midline survey. In some cases we were not able to get students to complete the baseline survey before the workshop, but wanted to allow them to participate in the workshop and to complete the remaining components of the programme. Since students were shown videos during the baseline survey, student assigned to treatment arms when they completed the midline survey would not receive the videos, but would be offered mentor and visit treatments as relevant for that treatment arm.

We pre-registered relevant pooled comparisons between these arms as well as the comparisons of individual treatment arms. Following the discussion in Muralidharan, Romero, and Wüthrich (2025), pooled comparisons should be interpreted as a weighted average of the effects of one treatment averaging over assignment to other treatments – so the pooled comparison of (T2a, T2b) vs. (T1a, T1b) can be interpreted as the effects of demographic match pooling across whether or not the student was assigned a visit. Since many of our outcomes are university-specific and the universities assigned for videos, mentors and visits frequently differ, interaction effects between the different treatments are likely to be small, at least for these outcomes. Table 7 presents balance checks among our primary sample of students who completed both the baseline and midline surveys, pooling across the treatment arms; all covariates that we test are balanced across treatment and control groups. Appendix Table C4 provides counts of the numbers of students who completed different surveys and different treatments, and Appendix Table C5 presents an analysis of differential takeup of the mentorship treatment.

### 7.3 Outcomes

Our primary outcomes are beliefs about the universities that students are assigned exposure treatments for, intended and actual applications to these universities, and final university enrollments.

Beliefs are elicited in our surveys on Qualtrics; Appendix Figures E9–E11 provide screenshots of the belief elicitation portion of the survey. We ask students the following belief questions (presenting them with emphasis as below):

1. What do you think is the **percent chance that you'd get an offer** from each of the universities below, if you applied?

2. What do you think is the **percent chance that you'd make friends and fit in** at each of the universities below, if you attended?

3. What do you think is the **percent chance that you'd graduate successfully** from each of the universities below, if you attended?

We ask these questions about different universities, and have students select their belief on a 0–100 slider, restricting inputs to multiples of 10. At baseline, we ask students about their beliefs for one of the universities that they were assigned a video for (we randomly selected which of the two universities to elicit beliefs for), their top choice university, and Oxford University. We ask about Oxford to have a benchmark for students' beliefs about a particular elite university that all students are asked about; none of our participating schools are in or near Oxford, so no schools would have a particular local connection to this university. We elicit beliefs before the video in the baseline survey. For students in the treatment arms that receive videos, we repeat the elicitation after students watch the video to measure short-run updating; we do not do so in the control arm as students receive no information that would inform their beliefs after the first elicitation, and could have been confused about why they were being asked to report beliefs again.

In the midline survey, we repeat the elicitation for these three universities, but also ask about beliefs at the universities that the students were assigned mentors for (based on the algorithm described above) and the university that they requested a visit from. We continue to elicit beliefs about the initial top choice even if the student reports a different top choice in the midline survey. This elicitation happens after we ask students to report their preferred mentor / visit universities, but before we inform them of their assignment to this treatment arm. In the endline survey, we repeat the elicitation for all of these universities.

We also collect application outcomes (see Appendix Figures E5–E7). In each of our surveys, we ask students to name the subject that they would most like to apply for, and then to list five universities that they plan to apply to for it; this mirrors the typical UCAS application pattern of applying to one subject across five different universities, exhausting the application cap. Stated applications in these surveys are used to construct intermediate application outcomes. We are able to validate these outcomes by also collecting data on actual applications from schools. Schools play a role in the administration of UCAS applications, generally needing to approve the application before it is finally submitted, and routinely collect data on where their students apply to, receive offers from, and finally enroll. We collect this data directly from the school for participating students, providing outcomes collected from school administrative data that are based on actual decisions – as opposed to stated beliefs and preferences – and not subject to attrition.

A final intermediate application outcome is the universities that students choose for their mentors and / or visits. These outcomes are incentivised, as participants were told these choices would be used to determine the actual mentors and visits they were assigned, and made these choices before being informed of their assignment to these treatments.

In addition to these outcomes collected from student surveys, we conduct surveys of mentors who have contacted students, and ask them to provide information about their discussions with each of their mentees. These outcomes are only available in the treatment groups where students were matched with mentors, so we cannot observe treatment effects on these outcomes, but we use them to provide descriptive evidence of the topics that students are interested in discussing when interacting with past university attendees.

### 7.4 Recruitment and selection of schools for the RCT

We work within a sample of schools in the UK who were recruited for the experiment via our partner organisations, as well as contacts at local authorities. Vertical selection into taking part likely took place on two countervailing dimensions. First, teachers at schools that opted in would need to have been engaged with our promotional materials distributed via WISE, and then be open to putting in additional work to support the programme with the aim of supporting their students' university applications. This would likely select for schools with teachers who are particularly engaged and interested in supporting their students' applications, which will typically be more successful schools. On the other hand, the interventions we provide would be redundant at schools that already provide extensive support for university applications, or where students already apply ambitiously with the support they receive, which would tend to rule out the most successful and most economically advantaged schools.[19]

19. One school that we spoke to about the programme chose not to participate on the basis that they already provided many of the forms of exposure that the programme provided, such as workshops with recent students and alumni. This school was an independent (fee-paying) school, with an intake that was substantially more economically advantaged

Figure 10 illustrates the geographic distribution of schools in our sample, overlaid on the map from Figure 3c illustrating the local probability of not being exposed to a top 10 university.[20] We have a cluster of participating schools near Liverpool (in the north-west of England), thanks to a connection with the Liverpool City Region Combined Authority, as well as several participating schools in the Greater London area and others from across England.

Table 1 included summary statistics for the RCT sample alongside summary statistics from the administrative data, but these comparisons are potentially hard to interpret; the RCT took place four years after the last year observed in the administrative data, so differences between the samples also reflect time trends in average outcomes. To better understand how characteristics of schools participating in the RCT compare to the general population of schools, Appendix Table C2 presents statistics drawn from the administrative data for the schools in the RCT sample as well as the full sample of schools. We use the standalone UCAS data for this exercise since it requires identifying specific schools, meaning that the sample in Table C2 is restricted to university applicants, but this restriction holds consistently in all columns of the table. Results from this analysis largely corroborate the results from the summary statistics in Table 1, though differences are generally less stark: schools participating in our RCT are generally more heavily male than average, are equally likely to come from low-income neighbourhoods, are more heavily Asian and less white, are disproportionately in London and Northern England, and are academically somewhat stronger than the typical university applicant.

### 7.5 Study timing and waves

We conducted the study in two waves, working with different schools and sets of students in each. Table 6 outlines the timing of different components of these waves. The first wave of interventions took place in Fall 2024 with Year 13 students (those in their final year of high school), and the second wave in Spring – Summer 2025 with Year 12 students (those in their penultimate year). In each wave, after schools opted in to the study, all students in the relevant cohort at the school were sent a baseline survey to complete online via Qualtrics, and encouraged to complete the survey by the teacher we liased with at their school. We used Qualtrics randomisation to assign treatments in this survey, and students in the relevant treatment groups were shown videos embedded into this baseline survey. After students completed their baseline surveys, we conducted an in-school workshop that we invited all students to participate in, regardless of treatment assignment. The workshop was generally led by one of the volunteers from our pool of mentors, though in some cases we worked with the school to find alumni of the school who were able to deliver the workshop.

Students then completed a midline survey immediately after the workshop. Following this, we connected students in the relevant treatment groups with mentors and informed them about how to claim a subsidised visit. Students who completed the baseline survey had their treatment

than that of other participating schools.

20. We have one participating school in Wales which is omitted from this map, since our LEO data on university access only covers England.

assignment carried over to the midline survey, while those who did not complete it were assigned to a treatment arm using Qualtrics randomisation when conducting the midline survey, as described above. Note that treated students who did not complete the baseline survey received only the mentor (and possibly visit) treatments, not the video treatment, since videos were embedded in the baseline surveya . Mentor assignments were conducted using a custom web service that allowed the mentors to be assigned as students completed the midline survey, meaning that we could match students to mentors and elicit beliefs about the mentor universities in the same survey.

We followed up with matched students and mentors over the weeks after being matched by text and email, and in cases where a matched mentor was non-responsive we set up a new match with an active mentor, re-running the same algorithm after removing inactive mentors. We also reminded students about their opportunities to visit universities. In November 2025, we will follow up with participating students in the Spring 2025 wave to have them complete an endline survey, with the support of their school to encourage takeup. Our final outcomes of realised applications to each university will be collected between October 2025 and January 2026, and realised enrollments will be collected by August 2026.

# 8 RCT results

## 8.1 Effects of video exposure on intended applications

We focus here on the effects of the video treatment on relevant outcomes, as we do not yet observe outcomes after mentor calls and university visits.

Our first result is that students who were exposed to a university video become more likely to list that university as one of the five universities they plan to apply to in their midline survey. We pool across the different arms in both the fall and spring waves that provided video treatments. We regress an indicator for listing to either of the two assigned video universities at midline on an indicator for the student being in the video treatment arm (and thus actually being shown the video). Table 8 illustrates that there is a 5 percentage point increase in the probability of listing one of the video universities on the midline survey. Relative to a baseline mean application rate (to either university) of 18.3%, this constitutes around a 30% increase in the probability of applying to one of the video universities, which is very similar to the percentage effect of breakthroughs that we estimate in the administrative data. The much higher baseline application rate compared with the application rate to a given university in the administrative data reflects the facts that videos are tailored to students' ability, and that the application rate is the share of students who apply to either one of the two assigned video universities.

Is this just driven by a short-run salience effect? Students completed the midline survey a median of 4 days later than the baseline survey, so the students in question were listing this university in our survey multiple days after seeing the video for the university. Furthermore, if we condition on taking the midline survey 4 or more days after the baseline, and similarly regress applications at midline on applications at baseline and the video treatment (as in specification (1) of table 8), we find a nearly identical point estimate of 0.052, although the estimate is no longer statistically significant ($p = 0.121$)—see Appendix Table D2. So exposure to videos has effects that seem to persist at least for several days. We will be able to validate whether they persist over a longer period when we collect endline survey data and final application outcomes. However, video exposure does not seem to raise the probability of students requesting mentors from either exposed university ($\beta = 0.014, p = 0.507$) or requesting a subsidised visit to either of these universities ($\beta = 0.017, p = 0.309$).

Effects of videos on applications indicate that durable connections that allow students to stay in touch with past university attendees when they attend themselves are, at least, not the only mechanism that affects the effects of exposure. No contact details are provided for video speakers, so students have no way to follow up with the video speakers and connect with them, and some of the speakers had already graduated from the university. If students react to videos, it must be because of either information conveyed in the video itself or the impression of the university that the video creates, which potentially shifts beliefs about components of payoffs at the university.

### 8.2 Beliefs at baseline about elite universities

In the baseline survey, we ask students for the probability that they will 'make friends and fit in at', 'get an offer from', or 'graduate successfully from', different universities: the university that they are assigned a video about (which is an academically aspirational university given the student's predicted grades), the University of Oxford (a well-known university with an elite and selective reputation), and the student's top choice at baseline. We also elicit students' postcodes, enabling us to link to the neighbourhood income measures that are also available in the administrative data. Focusing on beliefs about Oxford as a proxy for beliefs about elite universities in general, we find that first-generation university attendees (students whose parents did not attend university), low-income students, and female students all have more pessimistic beliefs about their probability of fitting in and making friends at Oxford than their counterparts with similar grades (we control for A-level tariff points in the regressions). On average, low-income students have a 5 percentage point ($p = 0.030$) lower expected probability of fitting in and making friends at Oxford than other students. However, we do *not* see similar patterns for the probability of receiving an offer or graduating successfully from Oxford, suggesting that it is the social reputation of the university, more than the academic reputation, that discourages applications for qualified students. We do not see the same pattern for ethnicity, where non-white students are somewhat more optimistic about receiving an offer than their white counterparts; this is consistent with other results from the British context that indicate ethnicity is not as strongly associated with economic disadvantage and poor university outcomes as it is in the United States. These results suggest that lower-income students' uncertainty about whether they can fit in and make friends at different universities may be an important component of gaps in application to these universities.

We next find evidence that exposure to a video about a university shifts students' beliefs about their social fit, but not about offer or graduation probability. Figure 12 illustrates the distributions of belief updates between the baseline and midline survey for each of the three belief questions about the video university, separately for treatment and control groups. The modal update is 0 in each case, indicating that there is reasonable reliability between the belief measures, but there is a distribution of positive and negative updates around 0. For the probability of receiving an offer (in panel (a)) or graduating (in panel (c)) the distributions of updates for students who were and were not assigned to treatment largely overlap, but there is a distinct rightward shift in the distribution of belief updates about fitting in and making friends following the video treatment, shown in panel (b). A Kolmogorov-Smirnov test of equality in distributions between the treatment arms rejects the null for social belief updates ($p = 0.022$), while failing to reject the null for offer ($p = 0.980$) or graduation ($p = 0.993$) beliefs.

Table 9 illustrates that we do not find *average* treatment effects on any of the belief variables, controlling for baseline beliefs. However, students exposed to the video are directionally 6 percentage points more likely to update positively, and 10 percentage points less likely to update negatively, about their social beliefs, while we see no such effects for beliefs about offer or graduation probability. So we find precise null effects of videos on beliefs about admission or graduation probability, while social beliefs tend to update positively in response to a video.

We also find some suggestive evidence that effects on applications are stronger for students whose beliefs update positively, indicating a relationship between changes in beliefs and changes in applications. In Table , we regress applications at midline on baseline applications, baseline beliefs, video treatment, and an interaction of video treatment with belief updating, to evaluate whether increases in application propensity are correlated with positive updating. We find that video exposure increases applications by 7.1 percentage points ($p = 0.043$) among students who update positively about social beliefs. While the interaction term is not statistically significant, it is substantial in magnitude: treatment effects are estimated to be 2.2 percentage points lower ($p = 0.624$ for students who do not update positively about social beliefs. These results suggest that the effects of videos on applications are strongest for students who update their social beliefs positively.

In combination with the result that low-income students' baseline pessimism about elite universities also relates to social beliefs not admission or graduation beliefs, these results strongly suggest that exposure to universities can encourage applications by improving students' perception of their social fit at particular universities.

### 8.3 Gender homophily and effects of videos

Who is most affected by videos? Since students were shown a video from one student of each gender, we can evaluate whether the treatment effect of the video was stronger for the gender-matched video. Surprisingly, we find the opposite pattern: students consistently respond *more* to videos recorded by students with a *different* gender. Table 11 illustrates this, looking at both application intentions and beliefs. For application intentions in panel (a), column 1 has as its outcome whether the student listed the university that the video featuring a student of the same gender as them recorded, and column (2) the university in the opposite-gender student video. Video exposure made students 5.3 percentage points more likely to apply to the opposite-gender university ($p = 0.017$), but did not have a statistically significant effect on applications to the same-gender university, with a point estimate of 1.4 percentage points ($p = 0.402$). Using seemingly unrelated regression to test for equality of the treatment effects on the two outcomes, we marginally reject the null of equal treatment effects ($p = 0.075$).

In panel (b), since we only elicit beliefs about one of the two video universities, we interact the video treatment indicator with an indicator for whether we elicited beliefs about the gender-matched video. (The video that beliefs were elicited for is randomly selected from the two assigned universities.) The point estimate on this interaction effect is negative across all three beliefs, and statistically significant for beliefs about graduation probability, while the base coefficient on video treatment (corresponding to the treatment effect on beliefs about the opposite-gender video) is positive and significant for social beliefs.

Thus both our estimates of video treatment effects on applications and on beliefs indicate more positive effects for opposite-gender videos. This result is surprising as in section 5.4 we find a positive interaction effect of gender match between the breakthrough student and the induced student. Given that videos have most effects on social beliefs, however, it is plausible that students would react more to interactions with the opposite gender; college plays an important role in marriage markets (Kirkebøen et al. 2021), which may mean that (heterosexual) students infer more about aspects of the social environment that they care about when they hear about the experience of the opposite gender.

One way to reconcile these results that is that *conditional on interaction* students react more to the opposite gender (or at least do not react less), but that the *probability* of interaction between students is higher when they share the same gender. In our RCT we induce interactions directly, whereas in the administrative data, and it is possible that the latter effect dominates there. Supporting this hypothesis, social network data collected in our surveys indicates that students at this age are much more likely to socially interact with other students of the same gender, particularly about university access: we asked students to name three other students at their school who they discuss their university applications with, and students were 4.1 times more likely to name a student of the same gender as one of their friends as they were a student of the opposite gender (see Appendix Table D1).

### 8.4 Information sought by students from mentors

In our video treatments, we prescribed a fixed set of topics for students watching the videos to include in their discussion, so that video content was relatively standardised. By contrast, we did not prescribe topics for calls with mentors. As described in section 7.2, in our emails connecting students with mentors, we told them that they could discuss "[mentor's] course, life at [mentor's university], uni life in general, and the application process". We did not prescribe topics to discuss beyond this. When recruiting mentors, we described the programme in terms of evaluating ways to support access to university, but did not discuss specific mechanisms that we were interested in testing. The topics that students discuss with their mentors thus reflect what students are most interested in learning from past university attendees and choose to discuss with them. The topics of mentor conversations are incentivised in the sense that students are not able to learn information about a particular topic from mentors without asking them about that topic. This avoids potential demand effects that would arise if we directly ask students what they are considering in their university applications, as students may think they are 'supposed' to decide based on factors like course content and teaching quality that are usually reported in university guides, and respond with these answers.

In Table 12, we report the topics that students discuss with mentors, as reported by mentors in a survey. We ask mentors to select the topics from a multiple-choice list that they discussed with each of their mentees; the topics selected are reported in panel (a). The two most commonly discussed topics are student life at the mentor's university (70% of conversations) and life at university and fitting in (68% of conversations). Thus in these organic conversations students are most likely to want to discuss aspects of social fit, substantially more than advice about choosing applications (53%), careers after university (36%), or how to succeed academically at university (30%).

Panel (b) reports the results of asking mentors to report what they perceived as the importance of different factors to their mentee's university application decisions. For each of the factors listed in panel (b), mentors reported importance on a 5-point scale from 'Not at all important' to 'extremely important'. The factor with the highest average importance reported was whether students would fit in and enjoy their time at university, with a mean importance of 3.8, and in a majority of conversations (56%) mentors reported a weakly higher importance score for this factor than any other factor. Prospects of academic success on the course, careers, prospects of getting an offer and course content were all reported as less important.

Taken together, these results indicate not only that students were likely to talk about social life and fitting in with mentors relative to other topics, but that mentors thought what they learned from this would affect their application choices. This is consistent with the results from section 8.2 illustrating that video exposure affects social beliefs more than offer or graduation beliefs; both sets of results indicate that the social environment at university is the primary topic that students seek to learn about from interactions with past students.

Overall, we find evidence from this RCT that exposure to students attending top universities can encourage applications and that a primary channel for this effect is students' beliefs about their social fit at the university becoming more optimistic. We do not find evidence that effects are stronger for more similar students, at least focusing on gender (if anything, videos from the opposite gender have a stronger effect). In the context of our framework, this indicates that students are unsure about their social fit at unfamiliar universities, meaning that there is substantial scope for exposure to affect beliefs and encourage applications. But they are better informed about their likely prospects of successfully receiving an offer or graduating, so exposure has less effect on applications through this channel. We draw these results together with our earlier results from administrative data in the conclusion.

# 9 Conclusion

The university that a student attends can matter substantially for their earnings and career, but low-income students are less likely to apply to and attend top universities, which may perpetuate inequality across generations. Using both evidence from administrative data – where we look at the effects of a 'breakthrough' to a university from a particular school on applications from that school in subsequent cohorts – and an RCT where we provide treatments that connect students with enrollees at different universities, we show that students are more likely to apply to a university when their exposure to students who have attended that university increases. Low-income students are less likely to be exposed to top universities, so these effects contribute to overall discrepancies in applications; our back-of-envelope calculation suggests that 15–30% of the gap in applications to top universities between low-income and high-income student can be explained by differences in exposure given the effects of exposure that we estimate in our analysis of breakthroughs.

From analysis of breakthroughs, we learn that exposure to top universities tends to draw applications away from lower-ranked universities, that the marginal students who respond to exposure seem to be well-suited to the university and tend to graduate successfully, and that they earn around £4,000 more per year than observably similar students who were not affected by a breakthrough. Students who are induced to enrol at a top university by a breakthrough tend to benefit, and there is little evidence of mismatch.

From the RCT, we learn that the largest discrepancies in beliefs between low-income and high-income students at baseline are about students' probabilities of making friends and fitting in at university, and that exposure to students attending a university, in the form of a video about the university, shifts these social beliefs more than beliefs about the probability of receiving an offer or graduating successfully from the university. Evidence from students' calls with mentors also indicates the importance of social interactions, as students choose to discuss the social environment and life at university more than any other topics, and mentors perceive this as being important for students' decision-making. At least in this context, information and beliefs about the social environment, an aspect of choice that is harder to communicate in formal information, therefore seem to be the primary mechanism for the effects of exposure on applications.

We draw two sets of implications from these results. First, the results have implications for how to improve equity in access to top universities. Our evidence suggests that knowing someone who has gone to the university matters. From the university's perspective, these results indicate the potential for personalised outreach efforts targeted towards addressing students' concerns are likely to have positive effects on applications. Video and mentorship treatments are more intensive than broader marketing, but are likely to be effective based on the results from this analysis; an important avenue for future research is to develop a fuller understanding of how intensive and personalised feedback needs to be in order to have an effect.

From the government's perspective, findings like those in this paper indicate that policies like the Texas Top 10% policy, wherein the top 10% of graduating students at any school are guaranteed admission to state universities, may have important indirect effects. Admitting a high-achieving student from a school with little history of sending students to that university will encourage applications from future students at that school, and so policies to encourage universities to preferentially admit such students – either imposed centrally, as in the case of top percent policies, or voluntarily by the university – will have indirect effects on applications. In addition, the findings support government initiatives to coordinate outreach efforts by universities and have these target a sense of belonging. The UK government is currently scaling up an initiative to send letters to disadvantaged students encouraging them to apply to university (Weale 2025), building on the findings in Sanders, Chande, and Selley (2017); our interventions test comparable but more intensive outreach programmes that could also be scaled with government support.

Second, they have broader implications for our understanding of decision-making in similar contexts. The choice of where to go to university is a high-stakes, one-off decision; there are many aspects of the decision that might be payoff-relevant to the decision-maker, and they have access to abundant information aiming to help with their decision. We have presented evidence that exposure can have substantial effects on decisions in this environment, and that beliefs about less tangible features – in this context, beliefs about fitting in and making friends – are an important mechanism. Many other decisions, such as choice of major, occupational choice, and industry choice, have similar features; understanding the extent to which these decisions are affected by the forces we document in this paper is a potentially fruitful avenue for further research.

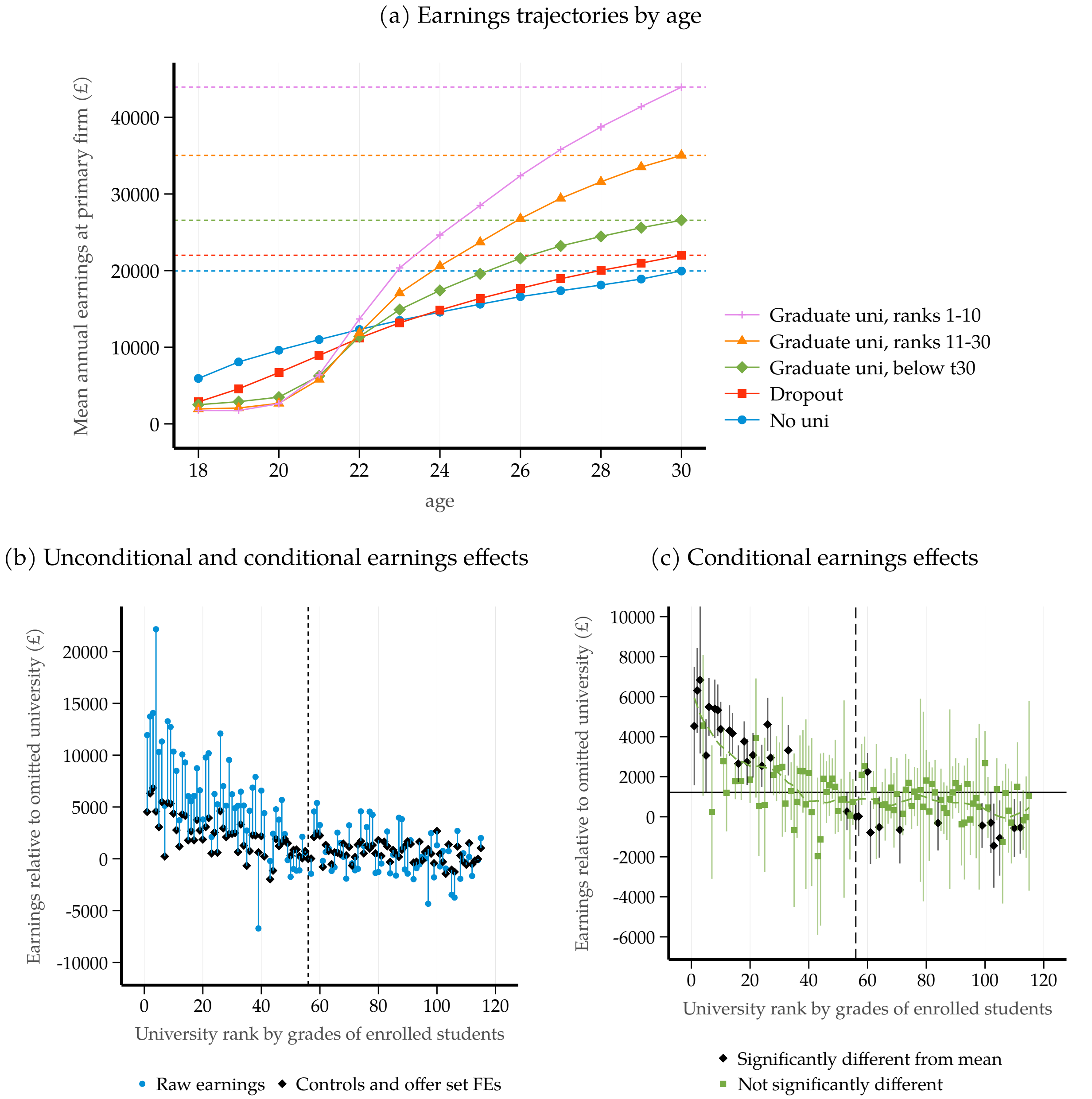
# 10 Exhibits

Table 1: Summary statistics

| | Age-18 school leavers | University applicants | Event study sample | RCT sample |
|---|---|---|---|---|
| Female | 51.2 | 56.0 | 57.3 | 45.2 |
| Low-income neighbourhood | 37.2 | 28.2 | 26.3 | 36.5 |
| Free school meal eligible | 21.6 | 12.5 | 11.6 | — |
| Parents attended university | — | 56.0† | 57.7† | 53.1 |
| White | 80.0 | 80.6 | 81.1 | 55.8 |
| Black | 4.6 | 4.8 | 4.5 | 8.1 |
| Asian | 9.2 | 10.2 | 10.2 | 24.4 |
| Other | 6.3 | 4.4 | 4.2 | 11.7 |
| Northern England | 28.2 | 28.7 | 28.9 | 37.7 |
| The Midlands | 19.6 | 19.3 | 19.2 | 2.7 |
| Southern England | 38.0 | 36.9 | 36.8 | 34.2 |
| London | 14.1 | 15.1 | 15.1 | 23.3 |
| Taking $\geq 3$ A-levels | 35.2 | 66.6 | 68.9 | 80.7 |
| Achieved A-level tariff points (med.) | 104 | 112 | 112 | — |
| Predicted A-level tariff points (med.) | — | — | — | 128 |
| $N$ | 7,164,386 | 2,920,445 | 2,290,950 | 806 |

*Notes:* Summary statistics comparing outcomes in different subsets of the LEO data and the RCT.
*Data:* LEO (columns 1–3), RCT (column 4).
*Samples:* KS5 school leavers are all students who attend school to age 18 – we exclude anyone who leaves full-time education before this date. University applicants are students who have a link to UCAS and apply to at least one university in HESA. The event study sample is students who are at a school that either experiences a breakthrough to, or is in the control group for, at least one of the breakthroughs to different universities. The RCT sample is drawn from students who completed both the baseline and midline survey, our primary sample for most analysis in the RCT data.
*Variables:* 'Low-income neighbourhood' is defined as a student's home postcode being in the bottom 40% of neighbourhoods as ranked by the Index of Multiple Deprivation. A-Level tariff points are a standard conversion of letter grades into a 0–56 numerical metric; we take the top 3 grades for each student, so the maximum possible tariff points is 168. Free school meal eligibility is not collected for students in the RCT.
† Data on parental university attendance in LEO is only available for students who themselves attend university.

Figure 1: Earnings returns to universities at age 27 across the distribution of university academic selectivity

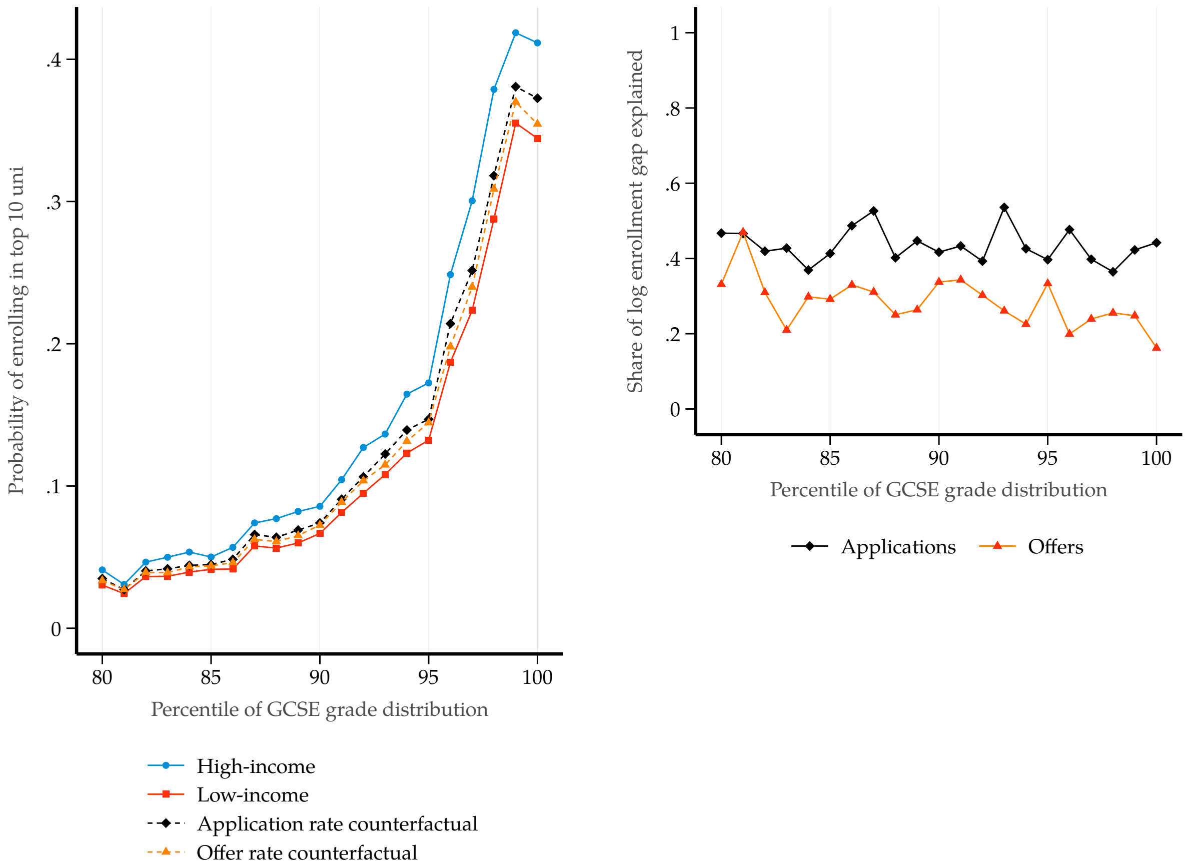



*Notes:* Estimates of the earnings return to different universities. Data: LEO. All earnings are in GBP and adjusted to inflation in 2018. Panel (a) plots mean earnings conditional on age and university outcome, and on positive earnings. We restrict to students starting a degree at age 18 for dropouts and university graduates; dropouts are those who do not earn a degree within 8 years of completing high school, and graduates are those who earn a degree within 4 years of completing high school. Students completing a degree in 5–8 years are excluded. Panel (b) plots unconditional earnings gaps (in blue) and conditional earnings gaps (in green) between graduates of different universities. The unconditional gap records the coefficient on each university in a regression of earnings at age 27 on university fixed effects, omitting the university at rank 56, after applying empirical Bayes shrinkage to university effect estimates. Universities are ranked on the X-axis by the mean A-level tariff points of their enrolled students (see Appendix B.1). The conditional specification adds controls for gender, ethnicity, neighbourhood income decile, GCSE grades, A-level grades, major, and offer set to the regression. Panel (c) simply plots the conditional estimates shown in panel (b), rescaling the Y-axis and reporting 95% confidence intervals (based on standard errors clustered at the high school level after applying empirical Bayes shrinkage). Estimates that are significantly different from the mean at the 5% level are highlighted in black.

Figure 2: Decomposition of gaps in attendance at top 10 universities

(a) Actual and counterfactual enrollment rates

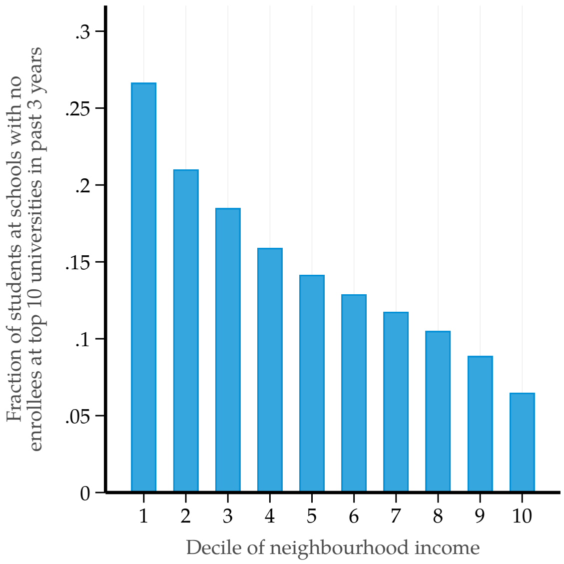
(b) Share of enrollment gap explained



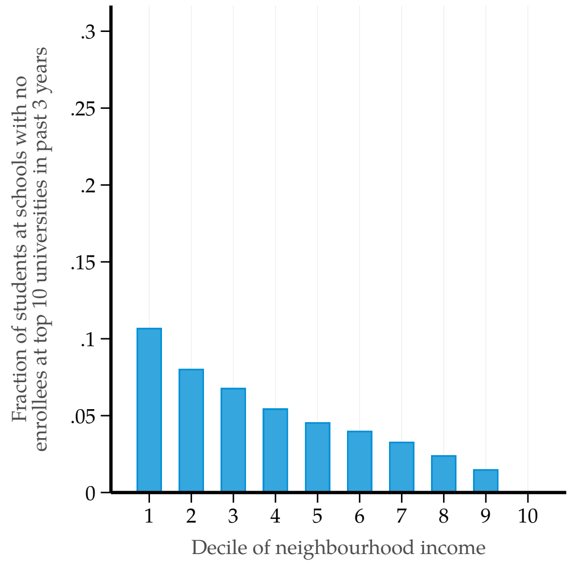

*Notes:* Data: LEO. In panel (a), solid lines plot probability of enrolling at one of the top 10 universities conditional on percentile in the national GCSE grade distribution (truncating at 80 since applications to top universities are very low below this rate), and conditional on enrolling through the main UCAS scheme. Application rate counterfactual calculated mechanically by multiplying application rate for high income students by offer rate, offer acceptance rate, and conditional enrollment rate for low-income students. Offer rate counterfactual similarly calculated by multiplying offer rate for high income students by application rate, offer acceptance rate, and conditional enrollment rate for low-income students. In panel (b), we take logs of the four components of the enrollment rate to get an additive decomposition, and plot the share attributable to applications and offers (i.e. the log difference in application rate and offer rate as a fraction of the log difference in enrollment rate).

Figure 3: Share of students attending schools that sent no-one to a top 10 university in the preceding three years, by neighbourhood income

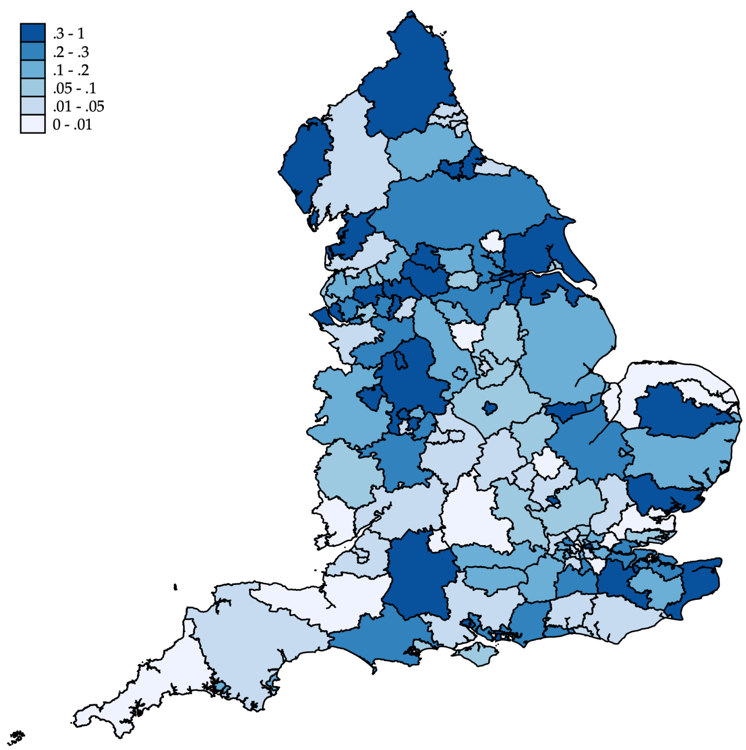
(a) Unconditional non-exposure rate




(b) Conditional non-exposure rate



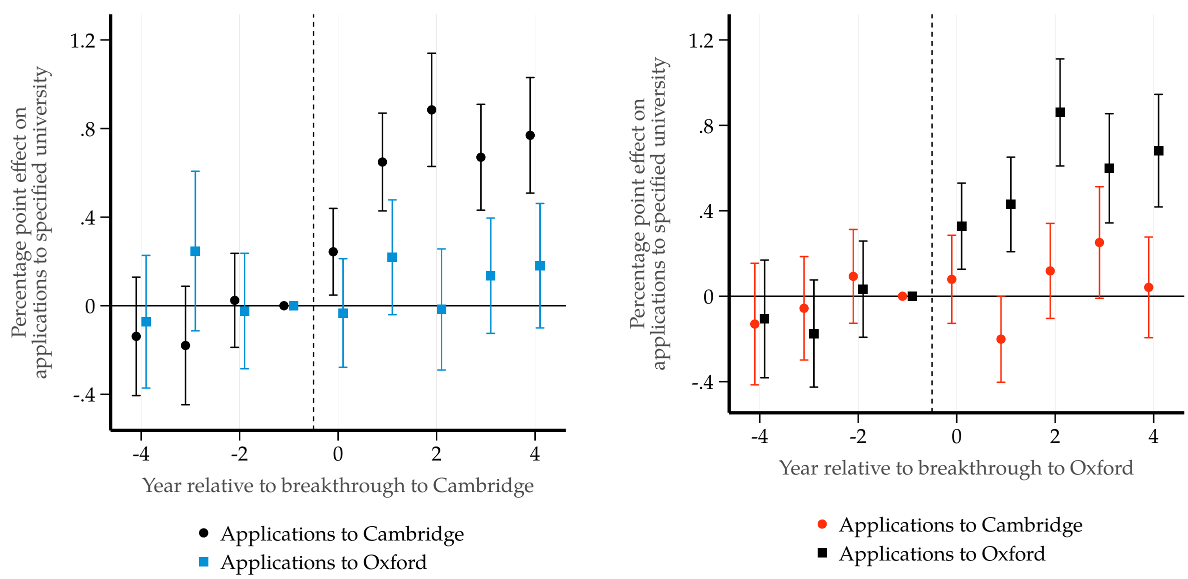

(c) Unconditional non-exposure rate by region of England




*Notes:* Data: LEO. Unconditional specification in panel (a) reports the share of students attending high schools that sent no-one to a top 10 university in the preceding three years, by decile of the student's neighbourhood income; lower deciles are more deprived. Conditional specification in panel (b) reports regression coefficients from a regression of the same outcome on IMD decile and controls for gender, ethnicity, free school meal eligibility, and GCSE grades, with the coefficient on decile 10 normalised to 0. Panel (c) plots the share of students in each ITL 3 region of England who attended high schools that sent no-one to a top 10 university in the preceding three years, as of 2018. All figures are at the individual level, as there is variation in neighbourhood income decile by school.

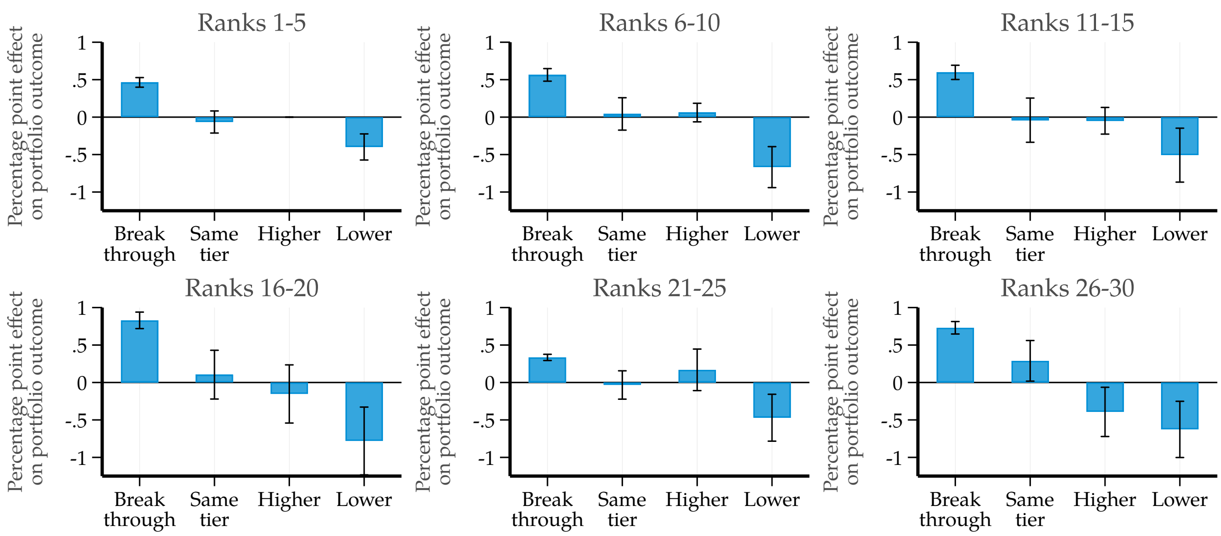
Figure 4: Event study of applications to Cambridge and Oxford following a breakthrough to each university

(a) Breakthrough to Cambridge (b) Breakthrough to Oxford



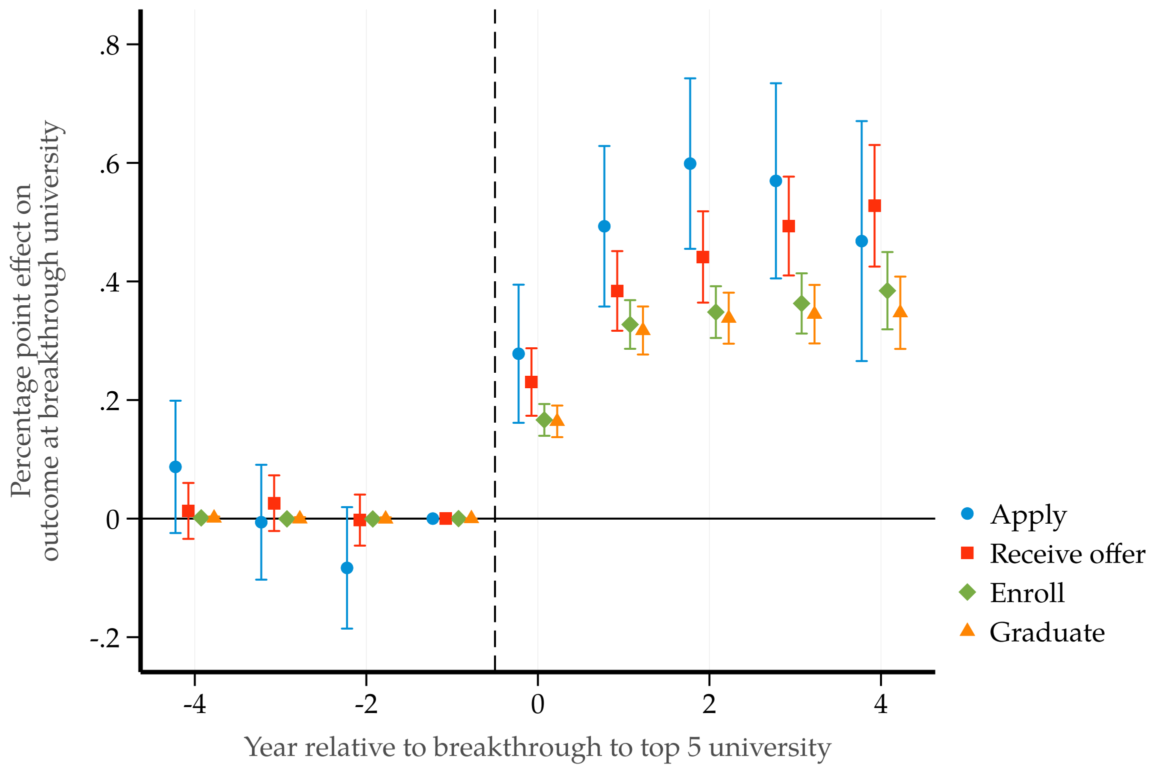

*Notes:* Data: UCAS. Coefficients from an event study of breakthroughs to specified university using the Sun and Abraham estimator, where the outcome is applications to Cambridge or to Oxford as specified. Coefficients are multiplied by 100 so they can be interpreted in percentage point terms. Regressions include school and year fixed effects, and we plot coefficients on the relative time indicators. 95% confidence intervals reported based on standard errors clustered at the school level.

Figure 5: Difference-in-difference coefficients for applications to universities of different ranks, following breakthroughs to universities of different ranks.




*Notes:* Data: LEO. Coefficients from difference-in-difference regressions estimating the effect of breakthroughs on portfolio outcomes. Outcomes are indicators for: (1) application portfolio including breakthrough university; (2) portfolio including a university ranked in the breakthrough tier but not the breakthrough university itself; (3) portfolio including a university ranked above the breakthrough tier but none in the breakthrough tier; (4) portfolio including a university ranked below the breakthrough tier but none in or above the breakthrough tier. As in figure 1, universities are ranked by the mean A-level tariff points of students enrolled at the university. The four outcomes are mutually exclusive and exhaustive, so coefficients mechanically sum to zero. Coefficients are multiplied by 100. Difference-in-difference regressions are pooled within a tier. Regressions include school-by-breakthrough-university and year-by-breakthrough-university fixed effects, and we plot coefficients on the indicator for Treated × Post-treatment. 95% confidence intervals reported based on standard errors clustered at the school level.

Figure 6: Event study for outcomes of applying to, receiving an offer from, enrolling at, and graduating from the breakthrough university, following breakthroughs to top 5 universities.




*Notes:* Data: LEO, 2007–2016 cohorts. Coefficients from an event study of the effects of breakthroughs on the specified outcome at the breakthrough university, using the Sun and Abraham estimator. Outcomes are indicators for applying to the university as one of the main five applications, receiving an offer from the university, enrolling at the university, and graduating from the university. Cohorts after 2016 excluded as graduation is not observed for these cohorts. Note that enrollment and graduation are mechanically 0 in the pre-period. Regressions include school-by-breakthrough-university and year-by-breakthrough-university fixed effects, and we plot coefficients on the relative time indicators. 95% confidence intervals reported based on standard errors clustered at the school level.

Table 2: Degree outcomes and characteristics for breakthrough students to top 5 universities relative to their course

| Variable | (1)<br>No ability controls | (2)<br>Ability controls |
|---|---|---|
| Graduate within 3 years | 0.075*** (0.009) | 0.066*** (0.009) |
| Graduate within 4 years | 0.053*** (0.008) | 0.043*** (0.008) |
| First-class degree | -0.022 (0.011) | -0.014 (0.012) |
| 2:1 degree | 0.022 (0.012) | 0.017 (0.012) |
| 2:1 or first | 0.000 (0.006) | 0.003 (0.006) |
| GCSE percentile | -0.624*** (0.132) | |
| $\geq$ 3 A-Levels | 0.034*** (0.003) | |
| Low-income neighbourhood | 0.021** (0.008) | 0.015 (0.008) |
| FSM-eligible | 0.008 (0.006) | 0.007 (0.006) |
| Female | 0.032*** (0.009) | 0.025** (0.009) |
| White | 0.093*** (0.008) | 0.084*** (0.008) |
| Northern England | 0.050*** (0.009) | 0.046*** (0.009) |
| Southern England | 0.023* (0.011) | 0.020 (0.011) |
| The Midlands | 0.028*** (0.007) | 0.027*** (0.008) |
| London | -0.101*** (0.008) | -0.092*** (0.009) |

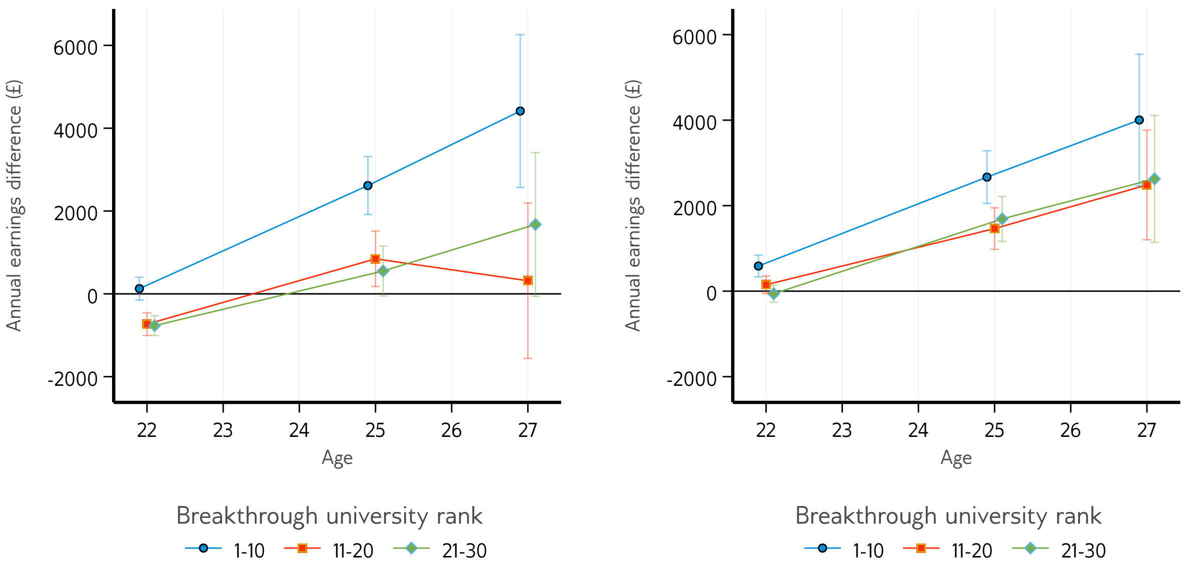
Standard errors in parentheses

$^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$

*Notes:* Data: LEO. Coefficients from regressions of the specified outcome variable on an indicator for having applied to the university following a school breakthrough, controlling for university-by-major-by-year fixed effects. Pooled across breakthroughs to top 5 universities. Column (2) additionally controls for an indicator for taking 3 A-Levels and the student's core GCSE percentile.

Figure 7: Earnings of breakthrough students relative to matched control students

(a) Matched controls at control group schools (b) Matched controls at same school before treatment




*Notes:* Regression of earnings at specified age on indicator for applying to the breakthrough university following a breakthrough, controlling for matched pair effects. Treated students are students who apply to the breakthrough university after the school experiences a breakthrough. Matched control students are drawn from a control set consisting of the set of students applying for the the same major in the same year at control high schools in panel (a), and from a control set consisting of students at the same school prior to the treatment in panel (b). Within the control set for each student, we also exactly match on the student's ventile in the sample GCSE grade distribution and an indicator for whether the student is taking 3 or more A-levels, and then select one nearest neighbour by Mahalanobis distance, matching on gender, neighbourhood income decile, and GCSE grades. Treated students who do not have a valid match (if there are no students in the control set who share the exact matching variables) are discarded. We then compare earnings for treated and matched control students at ages 22, 25, and 27, regressing the outcome on matched pair fixed effects and the treatment indicator. 95% confidence intervals reported based on standard errors clustered at the school level.

Table 3: Mean characteristics for compliers (students who apply in response to a breakthrough) and broader samples

| Variable | Complier mean | Mean for enrollees at breakthrough universities | Event study sample mean | Event study treated schools mean |
|---|---|---|---|---|
| *Panel A: University ranks 1–10* | | | | |
| Female | 0.460 | 0.482 | 0.576 | 0.571 |
| White | 0.707 | 0.803 | 0.809 | 0.81 |
| Low-income | 0.297 | 0.147 | 0.331 | 0.304 |
| FSM eligible | 0.188 | 0.057 | 0.147 | 0.135 |
| Northern England | 0.203 | 0.181 | 0.311 | 0.309 |
| The Midlands | 0.205 | 0.139 | 0.212 | 0.197 |
| Southern England | 0.318 | 0.472 | 0.332 | 0.337 |
| GCSE percentile | 88.4 | 92.3 | 68.4 | 70.8 |
| $\geq$ 3 A-levels | 0.921 | 0.954 | 0.620 | 0.688 |
| *Panel B: University ranks 11–20* | | | | |
| Female | 0.577 | 0.559 | 0.569 | 0.566 |
| White | 0.778 | 0.835 | 0.787 | 0.787 |
| Low-income | 0.271 | 0.192 | 0.346 | 0.328 |
| FSM eligible | 0.156 | 0.069 | 0.161 | 0.154 |
| Northern England | 0.171 | 0.324 | 0.263 | 0.256 |
| The Midlands | 0.158 | 0.206 | 0.187 | 0.186 |
| Southern England | 0.372 | 0.336 | 0.370 | 0.370 |
| GCSE percentile | 82.6 | 85.6 | 67.7 | 69.1 |
| $\geq$ 3 A-levels | 0.849 | 0.929 | 0.588 | 0.625 |
| *Panel C: University ranks 21–30* | | | | |
| Female | 0.582 | 0.532 | 0.566 | 0.565 |
| White | 0.763 | 0.816 | 0.810 | 0.803 |
| Low-income | 0.223 | 0.204 | 0.300 | 0.283 |
| FSM eligible | 0.150 | 0.084 | 0.134 | 0.128 |
| Northern England | 0.192 | 0.265 | 0.330 | 0.302 |
| The Midlands | 0.162 | 0.136 | 0.209 | 0.197 |
| Southern England | 0.390 | 0.424 | 0.323 | 0.345 |
| GCSE percentile | 82.3 | 82.2 | 71.3 | 72.8 |
| $\geq$ 3 A-levels | 0.850 | 0.907 | 0.678 | 0.711 |

*Notes:* Comparison of mean characteristics for compliers – treating breakthroughs as an instrument for applications to the breakthrough university – with the population of all students who enroll at the breakthrough university (in column 2), for all students in the analysis sample for event studies (in column 3) and for all students at treated schools in the analysis sample (in column 4). Characteristics for compliers are estimated as in equation (9); other characteristics are raw means within the specified sample. 'FSM eligible' denotes eligibility for Free School Meals, a proxy for disadvantage. 'Low-income' denotes students in the poorest two quintiles of our neighbourhood deprivation measure.

Table 4: Heterogeneity in effects of breakthroughs on applications by similarity between student in sample and breakthrough student

(a) Difference-in-difference coefficient interacted with similarity index (number of shared characteristics)

| | Apply breakthrough |
|---|---|
| Post × Treated | -0.000345 |
| | (0.000291) |
| Post × Treated × Num shared chars. | 0.00184*** |
| | (0.0000885) |
| Observations | 22,440,030 |
| Mean of outcome | 0.0187 |

Standard errors in parentheses

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

(b) Difference-in-difference coefficients for each component of similarity index

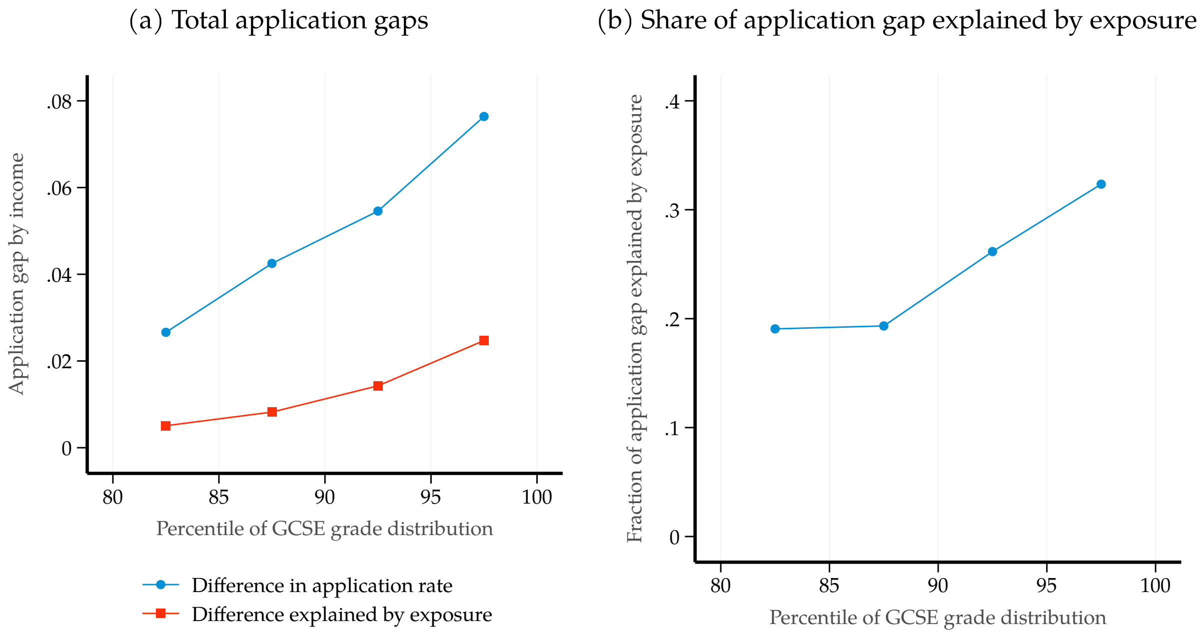
| | Income | Ethnicity | FSM | Gender | Age-16 school |
|---|---|---|---|---|---|
| Post × Treated | 0.00403*** | 0.00455*** | 0.00403*** | 0.00411*** | 0.00284*** |
| | (0.000219) | (0.000268) | (0.000232) | (0.000201) | (0.000197) |
| Post × Treated × Shared | 0.00212*** | 0.00134*** | 0.00215*** | 0.00243*** | 0.00507*** |
| | (0.000196) | (0.000304) | (0.000243) | (0.000181) | (0.000266) |
| Observations | 23,135,930 | 23,135,930 | 23,135,930 | 23,135,930 | 23,135,930 |
| Mean of outcome | 0.0196 | 0.0196 | 0.0196 | 0.0196 | 0.0196 |

Standard errors in parentheses

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

*Notes:* Data: LEO. Coefficients from difference-in-difference regressions estimating the effect of breakthroughs on applications to the breakthrough university, interacting the Treated × Post indicator with measures of similarity between each student in the sample and the breakthrough student at their school. Panel (a) interacts this indicator with a continuous measure of the number of characteristics shared with the breakthrough student (neighbourhood income, ethnicity, free school meal eligibility, gender, and age-16 school), between 0 to 5. Panel (b) interacts the indicator with indicators for sharing each individual characteristic with the breakthrough students. Difference-in-difference regressions are pooled across the top 30 universities. Regressions include school-by-breakthrough-university and year-by-breakthrough-university fixed effects, and we plot coefficients on the indicator for Treated *times* Post-treatment. 95% confidence intervals reported based on standard errors clustered at the school level.

Figure 8: Application gaps between low-income students and other students that are explained by differences in exposure given treatment effects



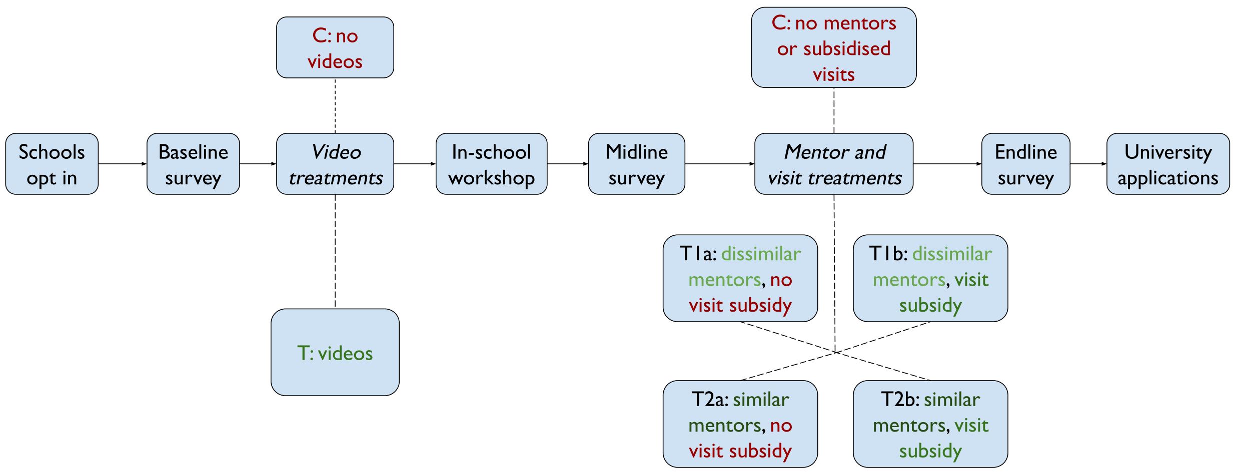

*Notes:* Difference in application rate to top 10 universities between low-income and higher-income students, and the amount of this that can be explained by exposure. We calculate treatment effects of breakthroughs on applications conditional on GCSE ventile for each university, $\Delta_u(g)$, and then calculate the gap in exposure by income also conditional on GCSE ventile (measured by the share of students at schools where no-one has attended that university in the last 3 years), $e_u^h(g) - e_u^l(g)$. Then equalising exposure rates would imply that $\Delta_u(g)\left(e_u^h(g) - e_u^l(g)\right)$ more students in gcse ventile $g$ apply to university $u$. Finally, we sum this over each of the 10 universities to get the overall effect of equalising exposure to each of the top 10 universities. (To simplify the calculation, we use the empirical finding that a breakthrough to university $u$ has no impact on applications to a similarly ranked university $u'$.) Panel (b) simply divides the difference explained by exposure by the total difference at each GCSE ventile to get a percentage effect.

Table 5: Treatment arms in each wave

| Wave | 1 (Fall 2024) | | 2 (Spring 2025) | | | | |
|---|---|---|---|---|---|---|---|
| Treatment arm | C | T | C | T1a | T1b | T2a | T2b |
| Workshop | Y | Y | Y | Y | Y | Y | Y |
| Videos | | Y | | Y | Y | Y | Y |
| Mentors | | Y | | Y | Y | Y | Y |
| Demo. matched mentors | | | | | | Y | Y |
| Visit subsidies | | | | | Y | | Y |

*Notes:* Table indicating which treatment components are offered to students by treatment arm. 'Demo. matched mentors' refer to mentors that are guaranteed to share at least one characteristic from gender, ethnicity, and UK region with the student they are matched with.

Figure 9: Overview of RCT design



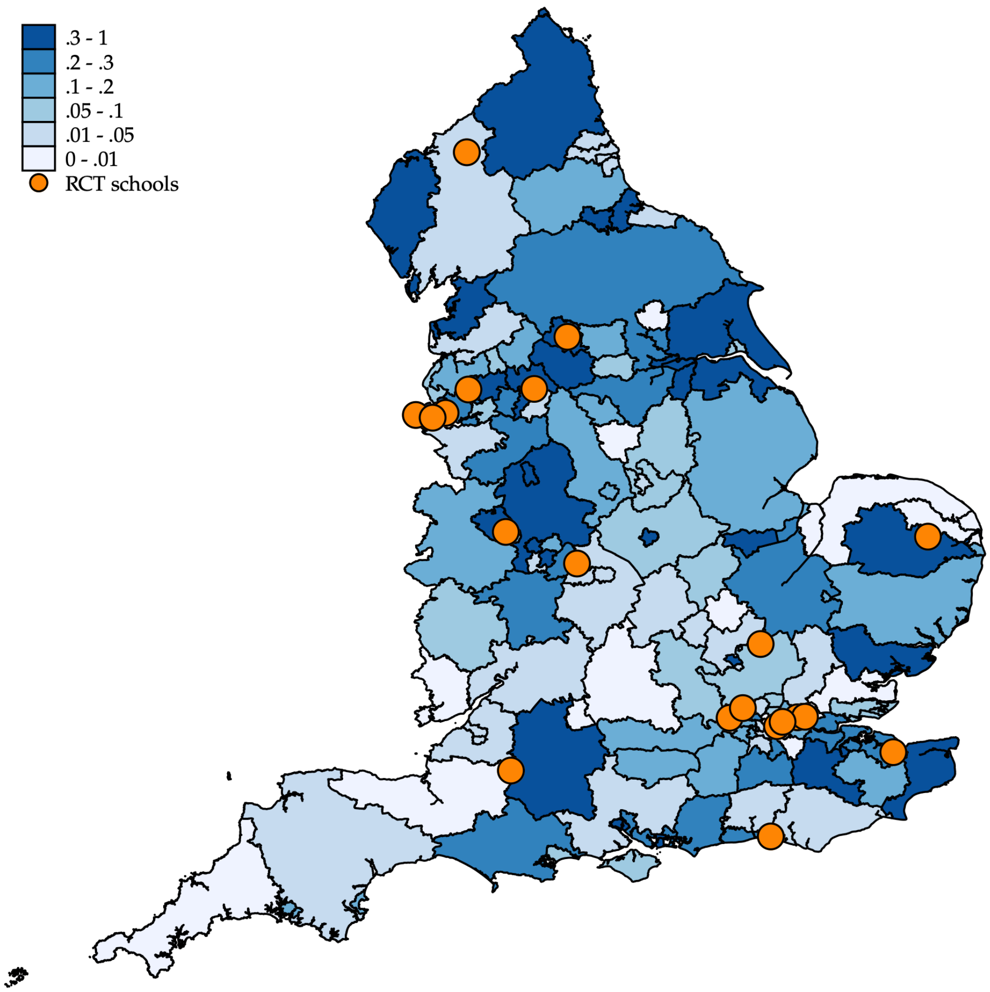

*Notes:* Diagram outlining stages of the RCT and treatment arms, as implemented in the Spring 2025 wave. The Fall 2024 wave does not implement visits, and does not vary mentor similarity.

Table 6: Timelines for each of the experimental waves

| | **Fall 2024** | **Spring 2025** |
|---|---|---|
| School recruitment | Jul – Sep 2024 | Sep 2024 – Apr 2025 |
| Baseline surveys | Sep – Nov 2024 | Jan – June 2025 |
| In-school workshops / midline surveys | Sep – Nov 2024 | Apr – June 2025 |
| Mentorship | Oct 2024 – Jan 2025 | May – Oct 2025 |
| Visits | N/A | Jun – Oct 2025 |
| Endline survey | N/A | Sep – Oct 2025 |
| University applications | Oct 2024 – Jan 2025 | Oct 2025 – Jan 2026 |
| University enrollment | Aug 2025 | Aug 2026 |

*Notes:* Timing of experiment and outcomes in Fall 2024 and Spring 2025 waves.

Figure 10: Locations within England of schools participating in our RCT, superimposed on map of share of students attending schools that sent no-one to a a top university from 2015–17




*Notes:* Map of the locations of schools participating in our RCT within England. Each orange dot represents a participating school. The underlying map of England is the map provided in Figure 3c, which plots the share of students in each ITL 3 region of England who attended high schools that sent no-one to a top 10 university in the preceding three years, as of 2018.

Table 7: RCT balance table

| Variable | Control mean | Treatment | N |
|---|---|---|---|
| Female | 0.463 | -0.041 | 805 |
| | (0.025) | (0.035) | |
| Low-income neighbourhood | 0.338 | 0.054 | 788 |
| | (0.024) | (0.034) | |
| Parents attended university | 0.520 | 0.021 | 805 |
| | (0.025) | (0.035) | |
| White | 0.562 | -0.006 | 805 |
| | (0.025) | (0.035) | |
| Black | 0.094 | -0.027 | 805 |
| | (0.015) | (0.019) | |
| Asian | 0.233 | 0.024 | 805 |
| | (0.021) | (0.030) | |
| Northern England | 0.374 | 0.005 | 805 |
| | (0.024) | (0.034) | |
| The Midlands | 0.032 | -0.010 | 805 |
| | (0.009) | (0.011) | |
| Southern England | 0.347 | -0.007 | 805 |
| | (0.024) | (0.033) | |
| London | 0.223 | 0.022 | 805 |
| | (0.021) | (0.030) | |
| Taking $\geq 3$ A-Levels | 0.804 | 0.004 | 805 |
| | (0.020) | (0.028) | |
| Predicted A-Level tariff points | 120.125 | -0.820 | 764 |
| | (2.896) | (4.075) | |

Standard errors in parentheses

$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

*Notes:* Balance table for demographics, restricted to students in our primary sample who completed the baseline and midline surveys. We report the control group mean and the coefficient on an indicator for being assigned to a mentor treatment arm for each specified variable. Standard errors are robust to heteroskedasticity. We omit significance stars for the control means, and find no differences for the mentor treatment assignment that are statistically significant at the 10% level.

Table 8: Treatment effects of video on intended applications to video university

| | (1)<br>Apply midline | (2)<br>Apply midline | (3)<br>Apply midline |
|---|---|---|---|
| Video treatment | 0.0542** | 0.0552** | 0.0556** |
| | (0.0217) | (0.0215) | (0.0218) |
| Apply at baseline | 0.727*** | 0.714*** | 0.710*** |
| | (0.0368) | (0.0378) | (0.0381) |
| Prior social belief | | 0.000616 | 0.000668 |
| | | (0.000437) | (0.000439) |
| Prior offer belief | | 0.000768* | 0.000786* |
| | | (0.000450) | (0.000453) |
| A-level percentile | | | 0.0628 |
| | | | (0.0408) |
| Parent att. uni | | | -0.0143 |
| | | | (0.0245) |
| Low-income | | | 0.0315 |
| | | | (0.0231) |
| Female | | | 0.00922 |
| | | | (0.0231) |
| Constant | 0.0439*** | -0.0231 | -0.0747** |
| | (0.0139) | (0.0229) | (0.0350) |
| Ethnicity | No | No | Yes |
| *N* | 701 | 701 | 701 |
| baseline_mean | 0.183 | 0.183 | 0.183 |

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

*Notes:* Data: RCT. Estimates of treatment effect of videos on intended applications. Students are assigned two videos about different universities regardless of their treatment status; outcome in the regression is an indicator for listing either of these universities as one of the five they intend to apply to in the midline survey, and we regress this on an indicator for being in a treatment arm where videos were displayed to the student in their baseline survey (pooling arms T1a, T1b, T2a and T2b), along with the specified controls. Standard errors are robust to heteroskedasticity.

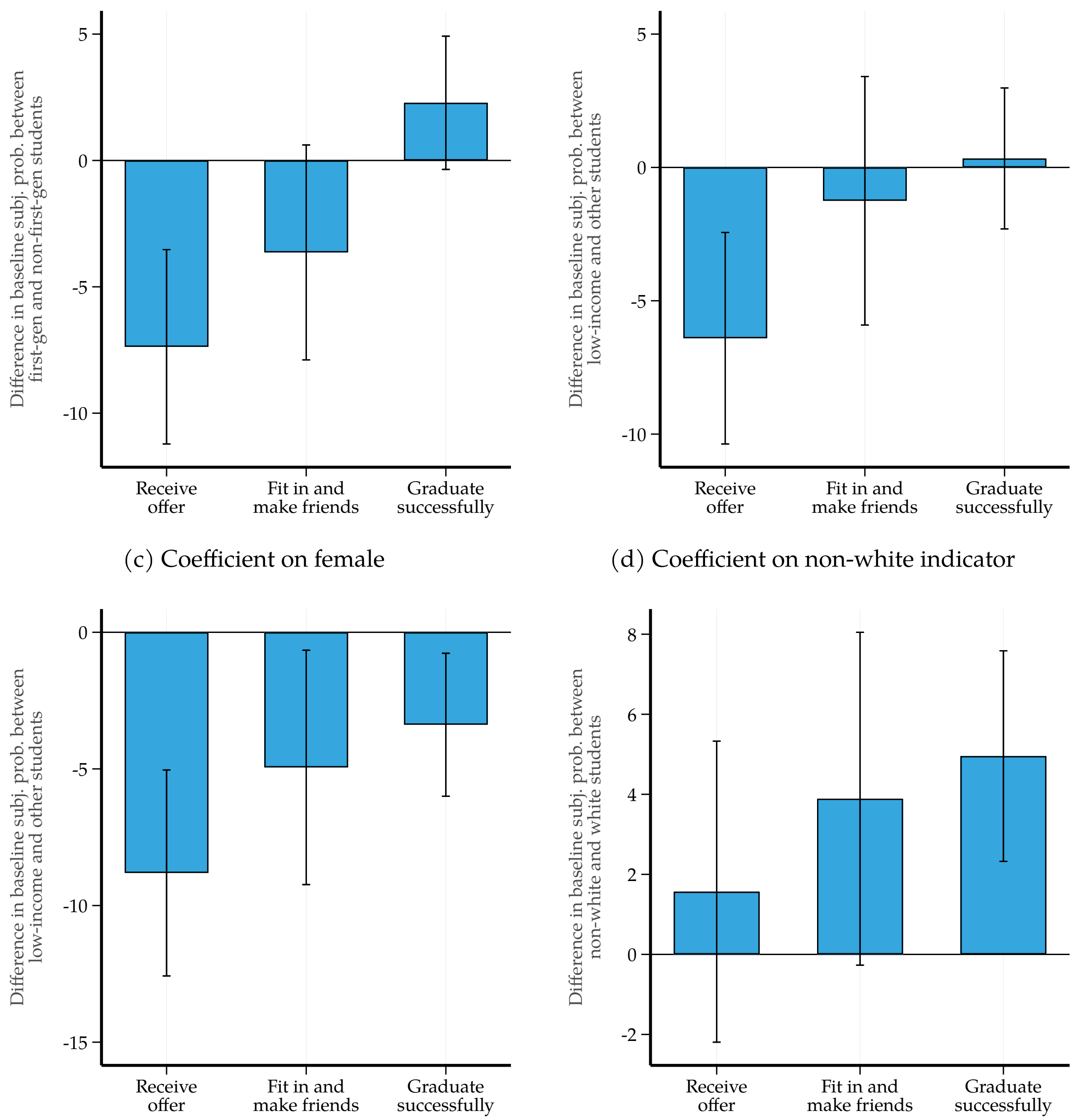
Figure 11: Beliefs about outcomes at Oxford University, by social disadvantage indicators

(a) Coefficient on first-generation indicator

(b) Coefficient on low-income indicator

(c) Coefficient on female

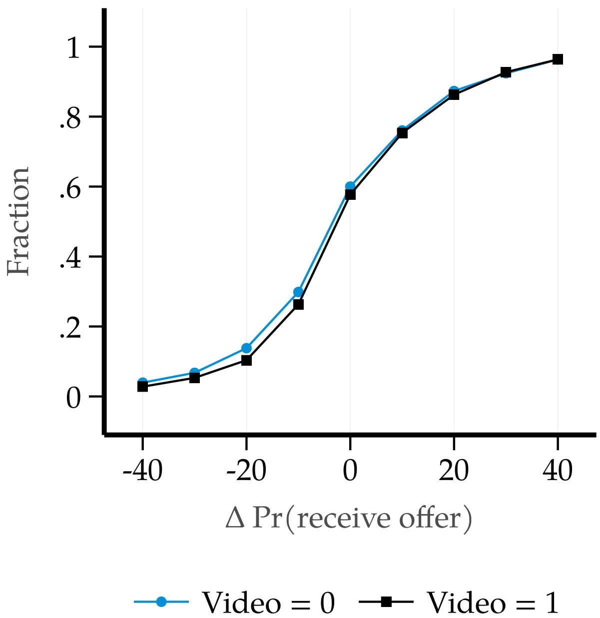
(d) Coefficient on non-white indicator




*Notes:* Data: RCT. Outcomes are subjective probabilities, between 0 and 100, of 'receiving an offer from', 'fitting in and making friends at', or 'graduating successfully from' Oxford University. We regress each outcome on the specified indicator and a control for the student's predicted A-level tariff points, and report the coefficient on the indicator. Low-income students are those whose home postcode is in the bottom two quintiles of IMD. 90% confidence intervals are reported based on heteroskedasticity-robust standard errors.

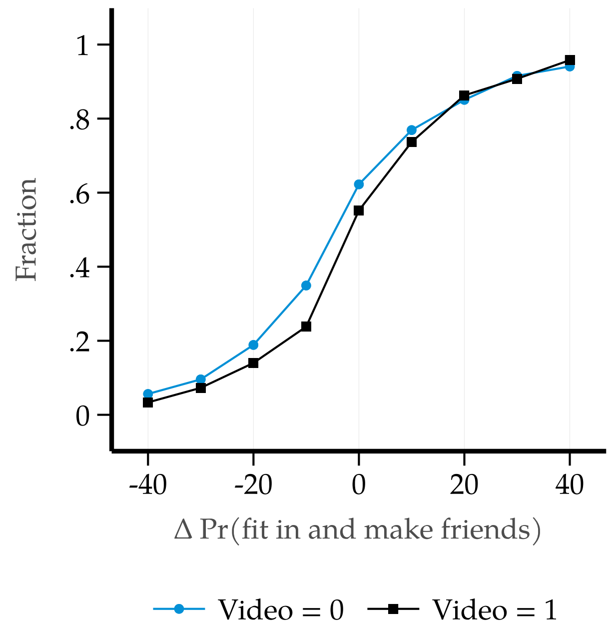
Figure 12: Distribution of belief updates about video university between baseline and midline survey by video treatment status

(a) Probability of receiving an offer



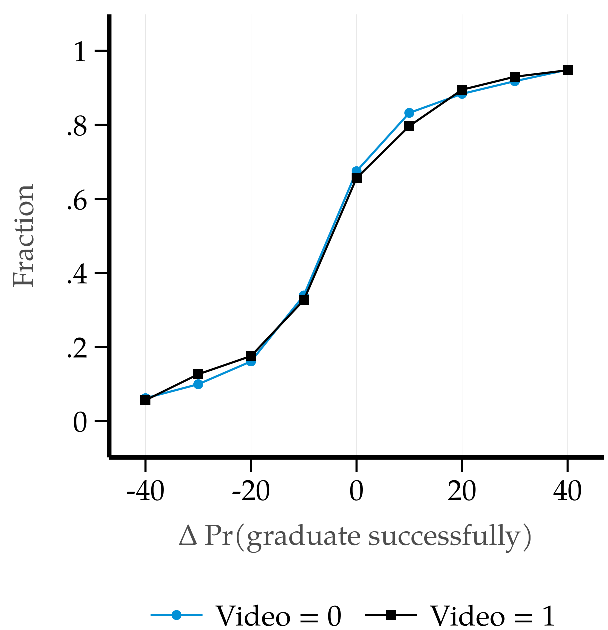

Mean update: b=1.80 (p=0.266)
Kolmogorov-Smirnov test: p=0.971

(b) Probability of fitting in and making friends




Mean update: b=2.76 (p=0.135)
Kolmogorov-Smirnov test: p=0.022

(c) Probability of graduating successfully




Mean update: b=0.19 (p=0.927)
Kolmogorov-Smirnov test: p=0.988

*Notes:* Distribution of belief updates between baseline and midline survey by video treatment assignment. We calculate the percentage point difference between beliefs reported in the midline survey and the baseline survey about student's assigned video university, and plot the distribution separately for students who were and were not assigned to the video treatment arms. Elicited beliefs were restricted to be multiples of 10, so distributions are discrete. Truncated to updates between -40 and +40.

Table 9: Treatment effects on belief updating and direction of belief updating between baseline and midline surveys

(a) Beliefs about offer probability

| | (1) Mean update | (2) I(update > 0) | (3) I(update < 0) | (4) I(update = 0) |
|---|---|---|---|---|
| Video treatment | 1.331 | 0.0157 | -0.0280 | 0.0124 |
| | (1.496) | (0.0357) | (0.0324) | (0.0347) |
| Prior offer belief | -0.319*** | -0.00495*** | 0.00491*** | 0.0000447 |
| | (0.0304) | (0.000633) | (0.000592) | (0.000698) |
| *N* | 712 | 712 | 712 | 712 |

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

(b) Probability of fitting in and making friends

| | (1) Mean update | (2) I(update > 0) | (3) I(update < 0) | (4) I(update = 0) |
|---|---|---|---|---|
| Video treatment | 2.065 | 0.0582* | -0.105*** | 0.0472 |
| | (1.685) | (0.0343) | (0.0334) | (0.0334) |
| Prior social belief | -0.350*** | -0.00630*** | 0.00292*** | 0.00337*** |
| | (0.0313) | (0.000520) | (0.000517) | (0.000590) |
| *N* | 712 | 712 | 712 | 712 |

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

(c) Probability of graduating successfully

| | (1) Mean update | (2) I(update > 0) | (3) I(update < 0) | (4) I(update = 0) |
|---|---|---|---|---|
| Video treatment | -1.534 | -0.0106 | -0.000843 | 0.0115 |
| | (1.832) | (0.0356) | (0.0390) | (0.0380) |
| Prior graduation belief | -0.434*** | -0.00733*** | 0.00299*** | 0.00434*** |
| | (0.0413) | (0.000616) | (0.000613) | (0.000704) |
| *N* | 577 | 577 | 577 | 577 |

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

*Notes:* Data: RCT. Presents treatment effects on mean belief updates and on the direction of belief updating between the baseline and midline survey, controlling for baseline beliefs. We pool across treatment arms in which students were shown videos. Standard errors are robust to heteroskedasticity.

Table 10: Interaction of video treatment effects with belief updating

| | (1) Apply midline | (2) Apply midline |
|---|---|---|
| Video treatment | 0.0708** | 0.0789** |
| | (0.0350) | (0.0354) |
| Social update $\leq 0$ | 0.00139 | |
| | (0.0311) | |
| Video treatment × social update $\leq 0$ | -0.0219 | |
| | (0.0446) | |
| Offer update $\leq 0$ | | -0.0135 |
| | | (0.0326) |
| Video treatment × offer update $\leq 0$ | | -0.0354 |
| | | (0.0446) |
| Apply at baseline | 0.712*** | 0.710*** |
| | (0.0384) | (0.0384) |
| Prior social belief | 0.000678 | 0.000549 |
| | (0.000439) | (0.000451) |
| Prior offer belief | 0.000769* | 0.000962* |
| | (0.000451) | (0.000499) |
| Observations | 697 | 697 |
| Baseline mean | 0.181 | 0.181 |

Standard errors in parentheses

$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

*Notes:* Data: RCT. Estimates of treatment effect of videos on intended applications, interacting treatment effects with an indicator for having a zero or negative belief update. Students are assigned two videos about different universities regardless of their treatment status; the outcome in the regression is an indicator for listing either of these universities as one of the five they intend to apply to in the midline survey, and we regress this on an indicator for being in a treatment arm where videos were displayed to the student in their baseline survey (pooling arms T1a, T1b, T2a and T2b from the Spring 2025 wave and arm T from the Fall 2024 wave), along with the specified controls. We interact this indicator with an indicator for whether the update in probability of fitting in and making friends (in column 1) or receiving an offer (in column 2) updated weakly negatively. Standard errors are robust to heteroskedasticity.

Table 11: Responses to videos by gender

(a) Gender heterogeneity in video effects on applications

| | Apply to video uni. at midline | |
|---|---|---|
| | (1) | (2) |
| | Same gender | Opposite gender |
| Video treatment | 0.0140 | 0.0531*** |
| | (0.0168) | (0.0174) |
| $N$ | 748 | 748 |
| Baseline mean | 0.104 | 0.120 |
| Treatment effect difference (p-val) | 0.0755 | 0.0755 |

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

(b) Gender homophily in video effects on beliefs

| | (1) | (2) | (3) |
|---|---|---|---|
| | Offer update | Social update | Graduation update |
| Video treatment | 2.758 | 5.188** | 2.578 |
| | (2.159) | (2.557) | (2.664) |
| Video treatment × video gender matches student | -2.374 | -5.432 | -6.553* |
| | (2.970) | (3.417) | (3.647) |
| $N$ | 700 | 700 | 567 |

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

*Notes:* Treatment effects of videos on applications and beliefs, separately by match between the student's gender and the gender of the individual in the video. In panel (a), we regress an indicator for applications to either the university in the matched-gender video (in column 1), or the university in the unmatched-gender video (in column 2), in the midline survey on the treatment indicator and applications at baseline. In panel (b), we regress the update in beliefs between baseline and midline on the video treatment indicator and baseline beliefs, and interact the video treatment indicator with an indicator for whether the video we elicited beliefs about was from the same gender as the student. Coefficients on the relevant prior belief (measured at baseline) are included in the regression but omitted from the regression table. We pool across treatment arms in which students were shown videos. Standard errors are robust to heteroskedasticity.

Table 12: Mentor reports of conversations with students

(a) Topics discussed with mentors

| Topic | Share of conversations where topic was discussed (%) |
|---|---|
| Student life around the mentor's university | 70 |
| Life at university and fitting in | 68 |
| The course(s) the mentor studied | 66 |
| How to put in a good application (e.g. personal statement advice) | 64 |
| How to choose which courses to apply to | 53 |
| Careers after university | 36 |
| How to succeed academically at university | 30 |
| Number of conversations reported | 103 |

(b) Factors affecting students' decision-making

| Factor | Mean importance (1–5 scale) | Share of conversations where topic was most important (%, includes ties) |
|---|---|---|
| Whether they would fit in and enjoy their time there | 3.81 | 56 |
| Whether they would be able to succeed academically on that course | 3.67 | 36 |
| Jobs that the course could help them to get | 3.59 | 38 |
| Whether they could get an offer from the course | 3.58 | 44 |
| Course content and teaching quality | 3.48 | 31 |
| Number of conversations with factors reported | 94 | 94 |

*Notes:* Descriptive statistics on topics discussed in calls and emails with mentors, based on post-mentorship survey. Panel (a) is based on a multiple choice question where mentors were asked to select all topics that they have discussed with each of their mentees, from the topics listed in the table; we tabulate the share of conversations for which the specified topic was selected. Panel (b) is based on a question where mentors were asked to report what they perceived as the importance of each topic for their mentee's application choices on a discrete scale (1 = not at all important, 2 = slightly important, 3 = moderately important, 4 = very important, 5 = extremely important). We report mean importance of each factor and the share of conversations for which that factor was the most important reported (including cases where factors were tied), excluding conversations where the mentor responded 'don't know' about the importance of every factor.

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# A Institutional details

Each of the four nations of the UK – England, Scotland, Wales, and Northern Ireland – has a somewhat different educational system. Our administrative data is drawn from English students, and all but one of the schools taking part in the RCT are in England (one school is in Wales), so we focus on institutional details for England below.

Students in both England and Wales are required to remain in education until age 18. At age 16, students complete General Certificate of Secondary Education (GCSE) qualifications in specific subjects. Students are required to complete GCSEs in English, Mathematics, and Science, and can take additional optional GCSEs depending on their preferences and the GCSEs offered by their school; the median student completes 8 GCSEs. Full-time education at ages 15–16 is focused on preparing students for GCSEs. After completing GCSEs, students must remain in education, but need not stay in full-time education. Academically oriented students typically remain in full-time education to complete GCE Advanced Level (A-Level) qualifications in specific subjects, but alternative routes include vocational qualifications (most commonly Business and Technology Education Council – BTEC – certificates), full-time apprenticeships, or part-time employment alongside part-time training. A-Level students pick a small number of subjects – typically 3-4 – to study, again based on both their preferences and the subjects offered by their school. It is reasonably common to change schools after completing GCSEs to attend a dedicated 'sixth form college' or 'further education college' that offers only 16–18 education, but many schools also offer 11–18 education. We refer to all educational establishments that provide 16–18 education as 'schools', and refer to higher education institutions as 'universities' throughout.

Courses receive applications from students without observing the other universities that students apply to. Admissions decisions are generally made at the course level, rather than holistically by the university.

After students receive offers, they choose up to two offers to accept, one as a 'firm choice' (a student's top choice that they plan to attend if they meet any offer conditions), and one as an 'insurance choice' (a course that will accept them if they fail to meet the conditions for their firm choice but achieve that course's conditions; students thus generally select an insurance choice with more lenient conditions than the firm choice). Students then complete exams and coursework required for their A-Levels or other qualifications, and receive grades later in the year. Depending on the grades they may attend their firm choice or their insurance choice, or if they fail to meet the conditions for either offer they can enter the 'Clearing' process – an after-market 'scramble' facilitated by UCAS in which universities list remaining open course slots and students can put in new direct applications to these courses given their realised (rather than predicted) grades. If they choose not to enter this process or fail to find an available course as part of it, they are free to reapply in the following year or not to attend university.

The government provides universal income-contingent loans covering all tuition costs to all students: under the current policy regime, students pay no tuition upfront and repay 9% of their annual income in excess of £25,000 after graduating. Interest rates are linked to inflation. Any debt remaining unpaid after 40 years is cancelled. The government also provides maintenance loans to cover living expenses: the amount offered depends on parental income and whether students live with their parents, and there is a supplement for students living in London. These are paid back in the same way as tuition loans.

# B Administrative data

## B.1 Definitions of constructed variables

**Region:** We generally 'region' to refer to the nine ITL 1 regions of England, unless otherwise specified. These are the largest statistical subdivisions of England: the North East, the North West, Yorkshire and the Humber, the West Midlands, the East Midlands, East of England, London, the South East, and the South West. For some analysis, we aggregate the regions into the North (including the North East, the North West, and Yorkshire and the Humber), the Midlands (West and East Midlands), the South (including the South East, the South West, and the East of England), and London.

**Neighbourhood income:** Our main measure of socioeconomic background is students' decile of the Index of Multiple Deprivation, which is a composite measure of deprivation constructed by the UK government based on incomes, unemployment, education, health, housing, and environment. This measure is defined based on the Lower-Level Super Output Area (LSOA) of a student's home residence; LSOAs are neighbourhoods with an average population of 1500, roughly equivalent to a US Census Block Group. As shorthand, we refer to students in the bottom 4 (most deprived) deciles of IMD as 'low-income' or 'from low-income neighbourhoods' throughout.

**GCSE grade percentile:** As described in section 2.1, all English students complete GCSEs in Maths, English, and Science, as well as some optional subjects. For our primary measure of ability, we focus on the core subjects since these are not affected by selection into who takes the subject. Students receive letter grades (A* – G) in each GCSE subject they complete until 2017, after which the grading switched to a 9–1 numerical scale. The mapping between letter grades and number grades was not one-to-one. To create a comparable measure across time, we therefore convert grades in each core subject into a percentile within each year based on the distribution of grades in that subject across all students who complete GCSEs. We then take the mean of these percentiles for each student across their core subjects. In some cases, we use the subject-specific percentiles.

**A-Level tariff points:** We convert letter A-Level grades to a numerical score using the UCAS tariff points scheme: A* = 56, A = 48, B = 40, C = 32, D = 24, E = 16. We then take the total of these tariff points across the student's subjects, restricting to the top 3 subjects for students who take more than 3 A-levels, for a score out of 168 (A*A*A*).

**Distance to university:** We observe the LSOA (see the neighbourhood income definition above) that each student lived in in each year of school, which provides a reasonably precise location measure; LSOAs have a median area of 0.5km$^2$ and a mean area of 4.3km$^2$. For universities, we directly observe the university's region but do not observe any more precise location. As a proxy for the university's location, we consider all students who attend that university and report that they live with their parents during term time (which generally means that they live near enough to the campus to commute regularly to the university, particularly since our sample period is pre-COVID so that remote lectures were less common). We then take the midpoint of these student locations, based on students' last observed LSOA while at school, and infer that this is the approximate location of the university. This is not an exact measure, but acts as a reasonable proxy; spot-checks produce roughly correct university locations.

**Earnings:** Our primary measure of earnings is the total earnings in pounds received in a tax year from an individual's primary employer, conditioned on receiving positive earnings. We exclude earnings from self-employment because these are only included for tax years after 2013, meaning that it would be impossible to consistently include self-employment earnings for the whole sample. We restrict to individuals with positive earnings recorded because the data does not generally let us distinguish between individuals with 0 earnings in a year, individuals who have no earnings from employment but positive self-employment earnings, and individuals who have positive earnings but did not work in the UK in the given tax year or do not appear in the tax data for some other reason. In Appendix Figure B1, we assume that anyone who is observed in the educational data in an appropriate cohort, but is not observed in the tax data in the year corresponding to that age, had earnings of 0. We do not observed hours worked, so annual earnings may reflect part-time work or employment spells lasting less than a full year, which is why mean earnings can be well below the annual equivalent of full-time minimum wage. Earnings are deflated to 2018 levels using CPIH (Consumer Prices Index including owner occupiers' housing costs). Conversions into US dollars are at the 2018 PPP exchange rate for household final consumption expenditures as reported by the OECD 'Annual Purchasing Power Parities and exchange rates' dataset, which is £0.7811 = $1.0000.

### B.2 Supplemental exhibits

Table B1: Variable availability by cohort

| Variable | Last cohort |
|---|---|
| University applications | 2021 |
| University enrollment | 2021 |
| Graduation within 4 years | 2016 |
| Earnings at age 27 | 2012 |

*Notes:* Indication of the last cohort for which each variable is available, where cohorts are indexed by the year in which students in that cohort graduated high school.

Table B2: Difference-in-differences estimates of breakthrough effects on university destination outcomes

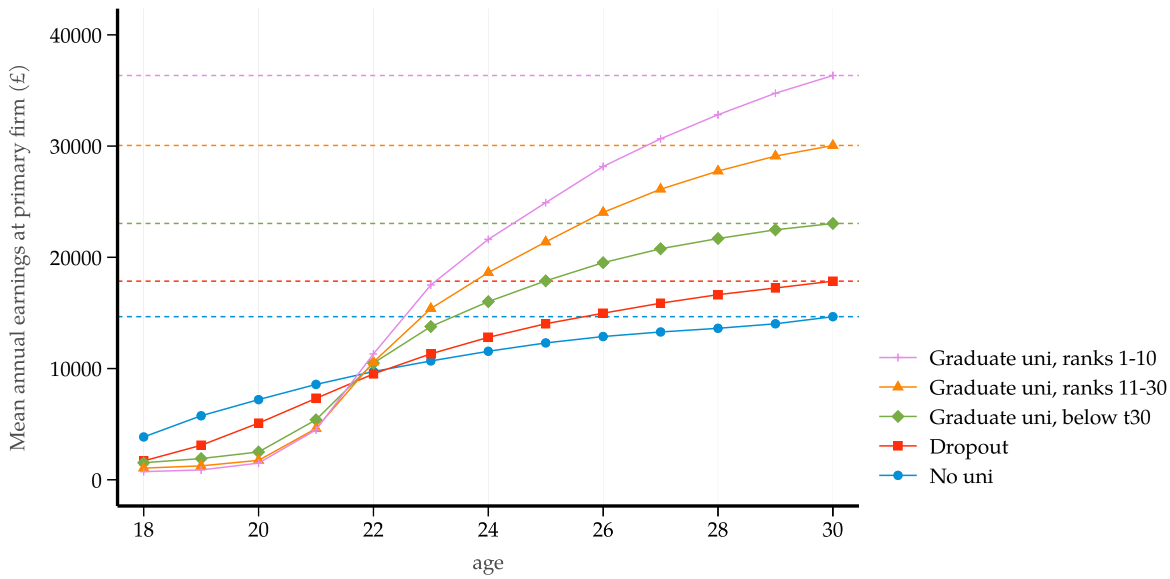
| | University destination | | | | | |
|---|---|---|---|---|---|---|
| | Breakthrough university | Same tier | Higher tier | Lower tier | Unranked institution | Unplaced |
| | *Panel A: University ranks 1–10* | | | | | |
| Treated × Post | 0.00265*** | 0.00117*** | -0.000248 | −0.00338** | −0.00359*** | 0.00339*** |
| | (0.0000925) | (0.000265) | (0.000145) | (0.00121) | (0.000929) | (0.00100) |
| $N$ | 8,133,835 | 8,133,835 | 8,133,835 | 8,133,835 | 8,133,835 | 8,133,835 |
| Sample mean | 0.00141 | 0.0185 | 0.00824 | 0.747 | 0.0723 | 0.153 |
| | *Panel B: University ranks 11–20* | | | | | |
| Treated × Post | 0.00386*** | 0.000926* | 0.00283*** | −0.00615*** | −0.00310** | 0.00163 |
| | (0.000117) | (0.000398) | (0.000688) | (0.00142) | (0.000976) | (0.00112) |
| $N$ | 5,469,250 | 5,469,250 | 5,469,250 | 5,469,250 | 5,469,250 | 5,469,250 |
| Sample mean | 0.00261 | 0.0385 | 0.0729 | 0.654 | 0.0761 | 0.156 |
| | *Panel C: University ranks 21–30* | | | | | |
| Treated × Post | 0.00228*** | 0.000607* | 0.00158 | −0.00679*** | −0.00186** | 0.00419*** |
| | (0.0000707) | (0.000299) | (0.000951) | (0.00115) | (00.000679) | (0.000837) |
| $N$ | 9,770,750 | 9,770,750 | 9,770,750 | 9,770,750 | 9,770,750 | 9,770,750 |
| Sample mean | 0.00138 | 0.0318 | 0.213 | 0.544 | 0.0629 | 0.147 |

Standard errors in parentheses

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

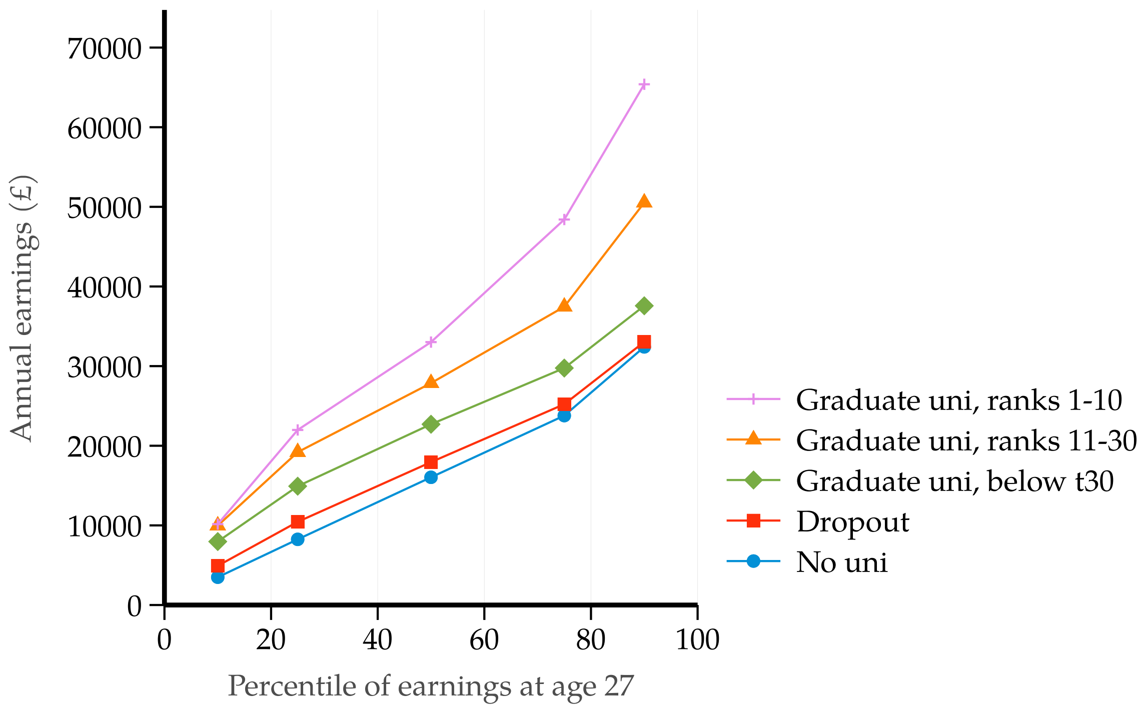
*Notes:* Data: LEO. Difference-in-differences regressions of the effects of breakthroughs on the specified university destination outcome. Outcomes are indicators for enrolling at the breakthrough university, enrolling in a different university in the same tier, enrolling in a higher-tier university, enrolling in a lower-tier university, enrolling in an unranked institution (an institution appearing in UCAS that could not be linked to an institution in HESA), and going unplaced in the cycle. Regressions include school-by-breakthrough-university and year-by-breakthrough-university fixed effects. Standard errors are clustered at the school level.

Figure B1: Earnings trajectories by age, including 0 earnings




*Notes:* Estimates of the earnings return to different universities. Data: LEO. All earnings are in GBP and adjusted to inflation in 2018. The figure plots mean earnings conditional on age and university outcome; we include individuals who do not appear in the tax data in a given year but are part of an educational cohort where students did appear, assigning these individuals 0 earnings. We restrict to students starting a degree at age 18 for dropouts and university graduates; dropouts are those who do not earn a degree within 8 years of completing high school, and graduates are those who earn a degree within 4 years of completing high school.

Figure B2: Percentiles of earnings distribution at age 27




*Notes:* Data: LEO. 10th, 25th, 50th, 75th, and 90th percentiles of the distribution of earnings at age 27 among each specified group, conditional on positive earnings. All earnings are in GBP and adjusted to inflation in 2018. We restrict to students starting a degree at age 18 for dropouts and university graduates; dropouts are those who do not earn a degree within 8 years of completing high school, and graduates are those who earn a degree within 4 years of completing high school.

Table B3: Application event studies for top 10 universities with ability controls

| | (1) Apply | (2) Apply | (3) Apply | (4) Apply | (5) Apply | (6) Apply |
|---|---|---|---|---|---|---|
| $1\{t=-4\} \times 1\{\text{Treated}\}$ | 0.000708 | 0.000831* | 0.000545 | -0.000914 | -0.000391 | -0.000656 |
| | (0.000378) | (0.000376) | (0.000398) | (0.000717) | (0.000711) | (0.000740) |
| $1\{t=-3\} \times 1\{\text{Treated}\}$ | -0.000348 | -0.000344 | -0.000344 | -0.000903 | -0.000712 | -0.000921 |
| | (0.000353) | (0.000358) | (0.000379) | (0.000700) | (0.000708) | (0.000739) |
| $1\{t=-2\} \times 1\{\text{Treated}\}$ | -0.0000626 | -0.0000712 | -0.000253 | -0.000752 | -0.000844 | -0.00115 |
| | (0.000365) | (0.000365) | (0.000383) | (0.000702) | (0.000696) | (0.000731) |
| $1\{t=-1\} \times 1\{\text{Treated}\}$ | 0 | 0 | 0 | 0 | 0 | 0 |
| | (0) | (0) | (0) | (0) | (0) | (0) |
| $1\{t=0\} \times 1\{\text{Treated}\}$ | 0.00337*** | 0.00284*** | 0.00304*** | 0.00361*** | 0.00290*** | 0.00277*** |
| | (0.000377) | (0.000371) | (0.000392) | (0.000710) | (0.000700) | (0.000726) |
| $1\{t=1\} \times 1\{\text{Treated}\}$ | 0.00489*** | 0.00440*** | 0.00431*** | 0.00591*** | 0.00504*** | 0.00501*** |
| | (0.000442) | (0.000427) | (0.000437) | (0.000783) | (0.000791) | (0.000807) |
| $1\{t=2\} \times 1\{\text{Treated}\}$ | 0.00590*** | 0.00554*** | 0.00539*** | 0.00661*** | 0.00566*** | 0.00567*** |
| | (0.000473) | (0.000458) | (0.000465) | (0.000800) | (0.000819) | (0.000847) |
| $1\{t=3\} \times 1\{\text{Treated}\}$ | 0.00505*** | 0.00480*** | 0.00479*** | 0.00494*** | 0.00375*** | 0.00364*** |
| | (0.000482) | (0.000474) | (0.000489) | (0.000913) | (0.000900) | (0.000929) |
| $1\{t=4\} \times 1\{\text{Treated}\}$ | 0.00543*** | 0.00541*** | 0.00545*** | 0.00510*** | 0.00399*** | 0.00363*** |
| | (0.000523) | (0.000516) | (0.000526) | (0.000804) | (0.000812) | (0.000842) |
| *N* | 8,133,835 | 8,080,870 | 7,284,345 | 1,969,650 | 1,962,180 | 1,812,640 |
| Sample mean | 0.0180 | 0.0178 | 0.0191 | 0.0126 | 0.0125 | 0.0131 |
| Pre-treatment mean | 0.0184 | 0.0184 | 0.0196 | 0.0108 | 0.0108 | 0.0114 |
| GCSE controls | N | Y | Y | N | Y | Y |
| A-level controls | N | N | Y | N | N | Y |
| School-level matching | N | N | N | Y | Y | Y |

Standard errors in parentheses.

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

*Notes:* Data: LEO. Coefficients from event studies of the effects of breakthroughs on applications to the breakthrough university using different sets of controls, pooled across top 10 universities. All regressions include school-by-breakthrough-university and year-by-breakthrough-university fixed effects. Column (2) includes GCSE percentile by breakthrough university effects; column (3) adds controls for the number of A-levels and the number of facilitating (more academic) A-levels completed; column (4) has no ability controls, but restricts to schools that are matched on 2007–2009 observables and includes match pair-by-year-by-breakthrough-university fixed effects; column (5) adds GCSE percentile by breakthrough university effects to the matched specification; column (6) adds controls for the number of A-levels and the number of facilitating (more academic) A-levels completed to this specification. 95% confidence intervals reported based on standard errors clustered at the school level.

Table B4: Application event studies for top 10 universities by distance to the breakthrough university

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Region | | Median dist. | | Dist. (miles) | |
| | Different | Same | Below | Above | $\leq 30$ | $> 30$ |
| $1\{t=-4\} \times 1\{\text{Treated}\}$ | -0.000353 | -0.00369 | 0.000630 | -0.000818 | -0.000740 | -0.000390 |
| | (0.000754) | (0.00354) | (0.00118) | (0.000930) | (0.00189) | (0.000794) |
| $1\{t=-3\} \times 1\{\text{Treated}\}$ | -0.000622 | -0.00306 | 0.0000477 | 0.0000154 | 0.000449 | -0.000479 |
| | (0.000738) | (0.00394) | (0.00113) | (0.00107) | (0.00201) | (0.000840) |
| $1\{t=-2\} \times 1\{\text{Treated}\}$ | -0.000901 | -0.0000121 | 0.00147 | -0.00168 | 0.000176 | -0.00110 |
| | (0.000730) | (0.00474) | (0.00126) | (0.00101) | (0.00242) | (0.000814) |
| $1\{t=-1\} \times 1\{\text{Treated}\}$ | 0 | 0 | 0 | 0 | 0 | 0 |
| | (0) | (0) | (0) | (0) | (0) | (0) |
| $1\{t=0\} \times 1\{\text{Treated}\}$ | 0.00276*** | 0.00626 | 0.00459*** | 0.00229* | 0.00283 | 0.00251** |
| | (0.000741) | (0.00405) | (0.00123) | (0.00104) | (0.00239) | (0.000802) |
| $1\{t=1\} \times 1\{\text{Treated}\}$ | 0.00496*** | 0.00265 | 0.00495*** | 0.00520*** | 0.00167 | 0.00538*** |
| | (0.000821) | (0.00410) | (0.00130) | (0.00116) | (0.00234) | (0.000948) |
| $1\{t=2\} \times 1\{\text{Treated}\}$ | 0.00506*** | 0.0153*** | 0.00789*** | 0.00297* | 0.00790** | 0.00390*** |
| | (0.000856) | (0.00450) | (0.00135) | (0.00124) | (0.00251) | (0.000980) |
| $1\{t=3\} \times 1\{\text{Treated}\}$ | 0.00331*** | 0.00901 | 0.00478*** | 0.00167 | 0.00290 | 0.00306** |
| | (0.000932) | (0.00523) | (0.00142) | (0.00135) | (0.00284) | (0.00108) |
| $1\{t=4\} \times 1\{\text{Treated}\}$ | 0.00315*** | 0.0116* | 0.00461*** | 0.00346** | 0.00521 | 0.00381*** |
| | (0.000828) | (0.00569) | (0.00135) | (0.00132) | (0.00273) | (0.000983) |
| $N$ | 1,703,905 | 107,585 | 881,675 | 857,295 | 346,370 | 1,392,910 |
| Sample mean | 0.0125 | 0.0237 | 0.0148 | 0.0103 | 0.0164 | 0.0116 |
| Pre-treat mean | 0.0108 | 0.0203 | 0.0125 | 0.00883 | 0.0137 | 0.00998 |

Standard errors in parentheses.

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

*Notes:* Data: LEO. Coefficients from event studies of the effects of breakthroughs on applications to the breakthrough university for students with different distances to the breakthrough university, pooled across top 10 universities. All regressions include school-by-breakthrough-university and year-by-breakthrough-university fixed effects. Column (1) includes only students in a different (ITL 1) region from the breakthrough university, while column (2) includes only students in the same region as the breakthrough university. Column (3) restricts to students below the median distance from the breakthrough university (which is calculated separately for each breakthrough university), and column (4) to students above the median distance. Column (5) restricts to students within 30 miles of the breakthrough university, and column (6) to students located more than 30 miles from the breakthrough university. 95% confidence intervals reported based on standard errors clustered at the school level.

Table B5: Application difference-in-difference heterogeneity in breakthrough effects: by sharing subject with breakthrough student, top 30 universities

| | (1) Apply to breakthrough |
|---|---|
| Shares subject with breakthrough student | 0.00805*** |
| | (0.000599) |
| Treated × Post | 0.00206*** |
| | (0.000273) |
| Treated × Post × shares subject | 0.00859*** |
| | (0.000677) |
| $N$ | 5,551,090 |
| Sample mean | 0.0131 |

Standard errors in parentheses.

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

*Notes:* Data: LEO. Coefficients from difference-in-difference regression estimating the effect of breakthroughs on applications to the breakthrough university, interacting the Treated × Post indicator with an indicator for whether the student shares at least one A-level subject with a breakthrough student at their school. Regressions include school-by-breakthrough-university and year-by-breakthrough-university fixed effects. 95% confidence intervals reported based on standard errors clustered at the school level.

Table B6: Application difference-in-difference heterogeneity in breakthrough effects: by sharing school with breakthrough student, among schools with KS4 provision

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Post × Treated × does not share KS4 school | 0.00254*** | 0.00128 | 0.00549*** | 0.00639*** | 0.00331*** | 0.00674*** |
| | (0.000559) | (0.000731) | (0.000853) | (0.000833) | (0.000348) | (0.000712) |
| Post × Treated × shares KS4 school | 0.00675*** | 0.00968*** | 0.00856*** | 0.0119*** | 0.00456*** | 0.00946*** |
| | (0.000416) | (0.000557) | (0.000675) | (0.000738) | (0.000296) | (0.000541) |
| University rank | 1–5 | 6–10 | 11–15 | 16–20 | 21–25 | 26–30 |
| $N$ | 2,867,260 | 2,534,520 | 2,062,815 | 1,605,145 | 4,230,575 | 2,611,840 |
| Sample mean | 0.0179 | 0.0304 | 0.0349 | 0.0376 | 0.0119 | 0.0295 |

Standard errors in parentheses

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

*Notes:* Data: LEO. Coefficients from difference-in-difference regressions estimating the effect of breakthroughs on applications to the breakthrough university, interacting the Treated × Post indicator with indicators for the student in the sample sharing or not sharing a school with the breakthrough student. Sample restricted to schools that offer both KS4 (age 14–16) and KS5 (age 16–18) education, so that students have the option to stay on at the same school after completing GCSEs. Regressions include school-by-breakthrough-university and year-by-breakthrough-university fixed effects. 95% confidence intervals reported based on standard errors clustered at the school level.

# C RCT design

## C.1 Algorithm for mentor assignment

Students are first assigned to a treatment arm T1a, T1b, T2a, or T2b. We assign a latent treatment arm for students in the control group, so that these students can be notionally assigned mentors according to the same procedure as treated students.

We seek to identify a tier-based and / or a preference-based mentor for each student—we target assigning each treated student one mentor of each type, but will assign only one mentor if there are no eligible mentors with remaining capacity. For preference-based mentors, we take the set of mentors from one of the three universities that the student requested. We then restrict to mentors with the appropriate demographics (different gender, ethnicity and region for students in T1a and T1b; same gender, ethnicity or region for students in T2a and T2b). Ethnicities are categorised into white, black, Asian, and other; regions are the 9 ITL 1 statistical regions of England. We then select the mentor with the highest capacity to take on new mentors remaining, and assign this mentor to the student. The mentor's capacity is decremented by 1 after being assigned, starting from the number of students that they initially told us they could take on.

For tier-based mentors, we similarly restrict on demographics based on the treatment arm. We exclude any mentors from universities that the student requested a mentor from, or that they reported a parent or sibling as having attended, so that the tier-based mentor is unfamiliar. We allow for overlap with the video treatment university. We then restrict to universities in the same tier that the student was assigned to (see Table C1). Next, we try to match on students' majors. We start by looking for a match on the exact major; if there are no available mentors within the exact major, we then use a more aggregated definition of major, and if there are still none then we take all mentors within the remaining universities. We then assign this mentor as the tier-based mentor; if there are multiple available mentors suitable to be matched, we take the mentor with the highest remaining capacity.

## C.2 Supplemental tables

Table C1: University tiers for video / mentorship treatments

| Tier | UCAS tariff cutoff (25th percentile) | Letter grade equivalent | Restricted to A-level students? | Example universities |
|---|---|---|---|---|
| 1 | 144 | AAA | Y | Cambridge, Imperial, LSE |
| 2 | 128 | ABB | N | Bath, Warwick, Durham |
| 3 | 96 | CCC | N | Nottingham, Sheffield, QMUL |
| 4 | 0 | — | N | All others |

*Notes:* Table indicating the university tiers used in the RCT. We report the UCAS tariff point cutoff for the tier – we include all universities whose 25th percentile tariff points for enrolled students is equal to or above this threshold in the tier – the equivalent in terms of letter grades, whether we restrict students assigned to this tier to be those taking A-levels (this is the case for tier 1), and some examples of universities in each tier.

Table C2: Summary statistics from UCAS data for schools in the RCT sample and for the full population of schools

| | All English uni. applicants | | Applicants at RCT schools | |
|---|---|---|---|---|
| | 2007–21 | 2017–21 | 2007–21 | 2017–21 |
| Female | 55.3 | 56.1 | 49.4 | 49.3 |
| Low-income neighbourhood | 32.6 | 31.2 | 34.6 | 32.2 |
| White | 73.1 | 68.7 | 69.9 | 66.3 |
| Black | 5.4 | 6.3 | 7.2 | 6.6 |
| Asian | 11.6 | 13.8 | 16.1 | 17.6 |
| Northern England | 26.7 | 25.8 | 38.8 | 33.3 |
| The Midlands | 18.2 | 18.2 | 2.2 | 2.0 |
| Southern England | 36.3 | 36.1 | 32.4 | 37.6 |
| London | 18.8 | 20.0 | 26.7 | 27.1 |
| Taking $\geq$ 3 A-Levels | 77.8 | 70.5 | 80.2 | 73.6 |
| Achieved A-Level tariff points (med.) | 104 | 104 | 112 | 112 |
| Predicted A-Level tariff points (med.) | 120 | 120 | 128 | 128 |
| Attend Oxford / Cambridge | 1.4 | 1.4 | 1.5 | 1.8 |
| Attend top 10 uni | 6.5 | 7.2 | 6.9 | 8.4 |
| Attend Russell Group uni | 21.9 | 24.3 | 25.5 | 28.0 |
| $N$ | 5,374,041 | 1,788,598 | 55,648 | 20,244 |

*Notes:* Data: UCAS. Summary statistics comparing the full population of English university applicants with students at schools that take part in our RCT.

Table C3: Characteristics of mentors

| Characteristic | Percentage of mentors |
|---|---|
| *Demographics* | |
| Female | 62.8 |
| Current university student | 59.0 |
| First-gen uni attendee | 42.2 |
| Low-income neighbourhood | 28.9 |
| *Age* | |
| 18–21 | 48.2 |
| 22–25 | 28.2 |
| 26+ | 23.7 |
| *Ethnicity* | |
| White | 60.0 |
| Black | 7.8 |
| Asian | 22.5 |
| Mixed / other | 9.8 |
| *Recruitment source* | |
| STEM Ambassadors | 65.7 |
| Own university | 20.0 |
| AtkinsRealis | 6.9 |
| Other | 7.3 |

*Notes:* Characteristics of participating mentors, as reported in our mentor recruiting survey.

Table C4: Student counts in experiment

| | Fall 2024 | Spring 2025 | Total |
|---|---|---|---|
| Baseline survey | 176 | 1275 | 1451 |
| Midline survey | 106 | 841 | 947 |
| Baseline and midline survey | 92 | 710 | 805 |
| Shown video | 85 | 621 | 706 |
| Matched with mentor | 44 | 332 | 376 |
| Had call / email with mentor | 10 | 93 | 103 |
| Assigned visit | 0 | 200 | 200 |
| Used visit subsidy | 0 | 26 | 26 |

*Notes:* Numbers of students who took part in different components of the RCT.

Table C5: Takeup of mentorship treatments by demographics

| | (1) Called mentor | (2) Called mentor | (3) Any contact with mentor | (4) Any contact with mentor |
|---|---|---|---|---|
| Female | 0.0392 | 0.0251 | 0.0172 | -0.00731 |
| | (0.0448) | (0.0444) | (0.0505) | (0.0496) |
| Parent attended uni. | 0.118*** | 0.113** | 0.113** | 0.108** |
| | (0.0446) | (0.0441) | (0.0512) | (0.0501) |
| Low-income | 0.0280 | 0.0268 | 0.0460 | 0.0409 |
| | (0.0499) | (0.0491) | (0.0565) | (0.0554) |
| Definitely not | | -0.264*** | | -0.187 |
| | | (0.0439) | | (0.146) |
| Probably not | | -0.295*** | | -0.398*** |
| | | (0.0407) | | (0.0600) |
| Maybe | | -0.103* | | -0.198*** |
| | | (0.0582) | | (0.0633) |
| Yes, probably | | -0.0693 | | -0.108* |
| | | (0.0581) | | (0.0649) |
| Yes, definitely | | 0 | | 0 |
| | | (.) | | (.) |
| Observations | 353 | 353 | 353 | 353 |

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

*Notes:* Regressions of indicators for mentor and visit takeup on demographics. In columns 1–2 we report effects on an indicator for whether the students or mentors have reported having a call; in columns 3–4 we report effects on an indicator for whether any interaction between mentors and mentees has been recorded. We restrict to students who were assigned to a treatment arm where they received a mentor. In columns 5–6 we report effects on an indicator for whether the student has requested a visit reimbursement, restricting to students assigned a visit subsidy. Standard errors are robust to heteroskedasticity.

# D RCT results

Table D1: Effects of shared characteristics on probability of two individuals being friends

| | (1)<br>Student pair are friends (OLS) | (2)<br>Student pair are friends (logit) |
|---|---|---|
| main | | |
| Same low-income status | -0.00317 | -0.148 |
| | (0.00204) | (0.0992) |
| Same gender | 0.0246*** | 1.437*** |
| | (0.00186) | (0.115) |
| Same ethnicity | 0.00423** | 0.200** |
| | (0.00188) | (0.0923) |
| *N* | 28152 | 28152 |
| Baseline mean | 0.0213 | 0.0213 |

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

*Notes:* Data: RCT. OLS and logit regressions of the probability of a given pair of individuals in the survey being friends, regressed on indicators for the pair sharing the same low-income neighbourhood status, gender, and ethnicity. Students are listed as friends if either one of the students names the other as one of three friends that they are likely to talk to about university.

Table D2: Heterogeneity in video treatment effects by time between surveys

| | (1) Apply midline | (2) Apply midline |
|---|---|---|
| Video treatment | 0.0540* | 0.0520 |
| | (0.0275) | (0.0335) |
| Apply at baseline | 0.768*** | 0.693*** |
| | (0.0548) | (0.0497) |
| *N* | 346 | 355 |
| Baseline mean | 0.147 | 0.217 |
| Days since survey | <4 | >=4 |

Standard errors in parentheses

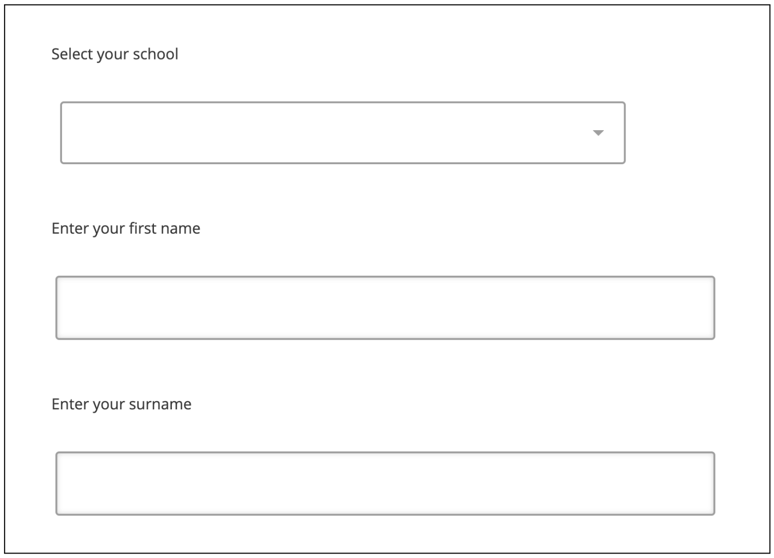
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

*Notes:* Data: RCT. Video treatment effects on applications at midline, reported separately by whether students completed the midline survey in a below-median number of days after watching the video (3 or less) or an above-median number of days (4 or more).

# E Survey materials

## E.1 Baseline survey

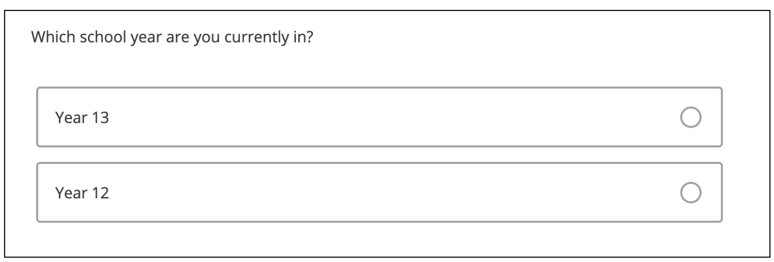
Figure E1: Student name and school


Select your school

Enter your first name

Enter your surname

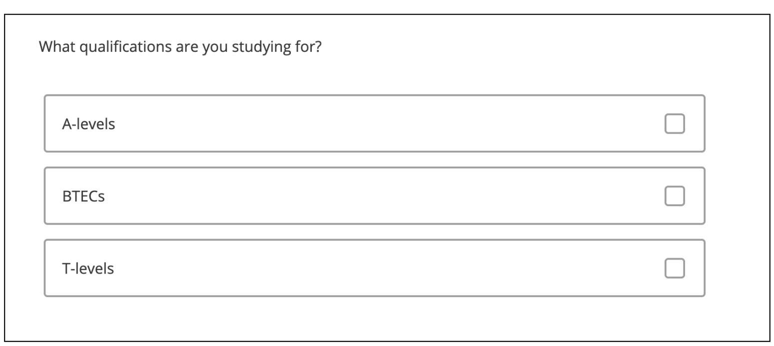
Figure E2: Student academic year


Which school year are you currently in?

Year 13

Year 12

Figure E3: Student qualifications

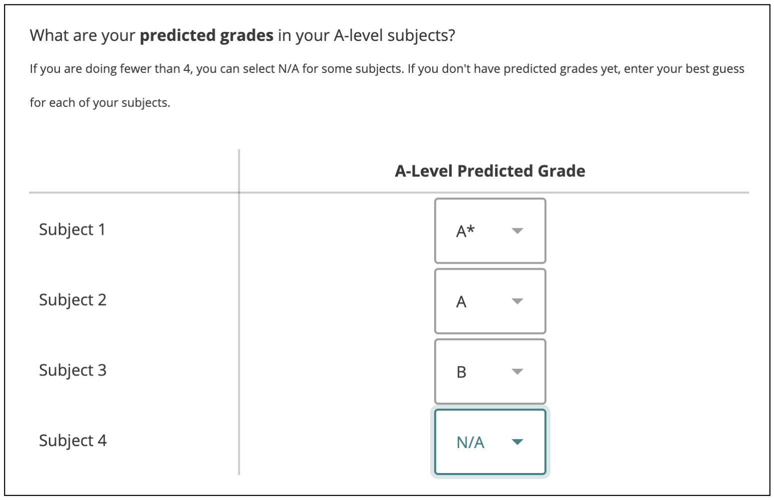

What qualifications are you studying for?

A-levels

BTECs

T-levels

Figure E4: Student predicted grades

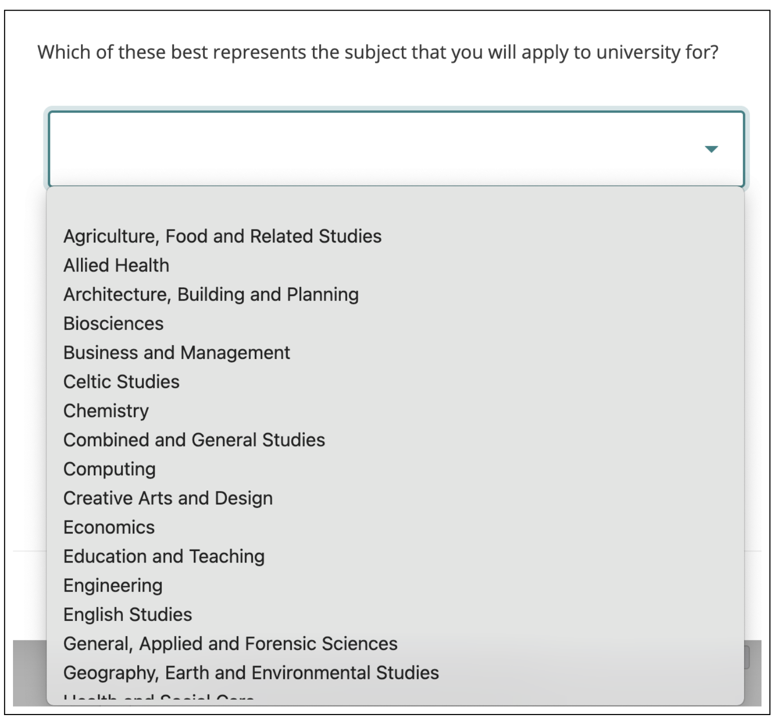


What are your **predicted grades** in your A-level subjects?

If you are doing fewer than 4, you can select N/A for some subjects. If you don't have predicted grades yet, enter your best guess for each of your subjects.

| | A-Level Predicted Grade |
|---|---|
| Subject 1 | A* |
| Subject 2 | A |
| Subject 3 | B |
| Subject 4 | N/A |

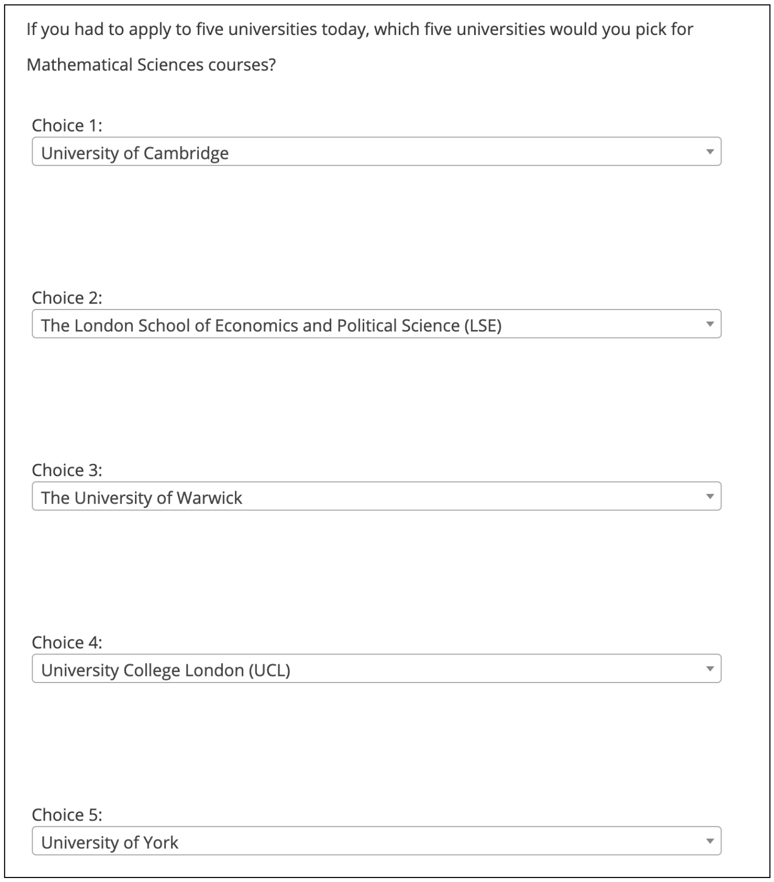
Figure E5: Student major choice



Which of these best represents the subject that you will apply to university for?

Agriculture, Food and Related Studies
Allied Health
Architecture, Building and Planning
Biosciences
Business and Management
Celtic Studies
Chemistry
Combined and General Studies
Computing
Creative Arts and Design
Economics
Education and Teaching
Engineering
English Studies
General, Applied and Forensic Sciences
Geography, Earth and Environmental Studies

Figure E6: University choice


If you had to apply to five universities today, which five universities would you pick for Mathematical Sciences courses?

Choice 1:
University of Cambridge

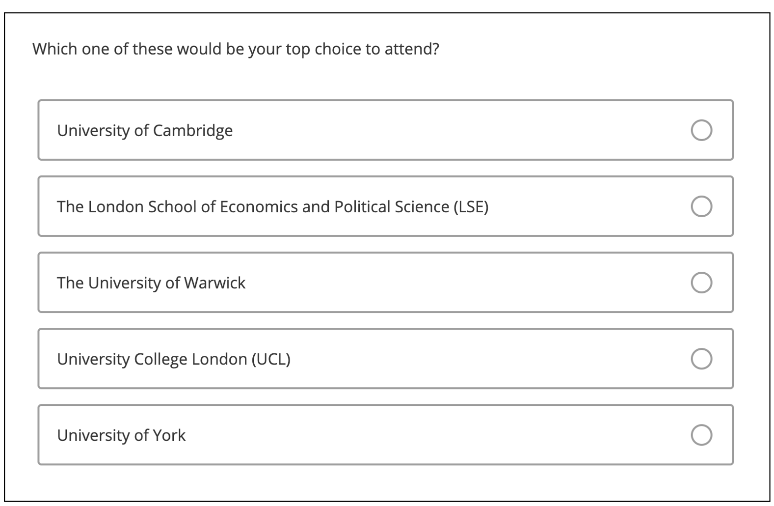
Choice 2:
The London School of Economics and Political Science (LSE)

Choice 3:
The University of Warwick

Choice 4:
University College London (UCL)

Choice 5:
University of York

Figure E7: University top choice


Which one of these would be your top choice to attend?

University of Cambridge

The London School of Economics and Political Science (LSE)

The University of Warwick

University College London (UCL)

University of York

Figure E8: University choice motivation (open-text)

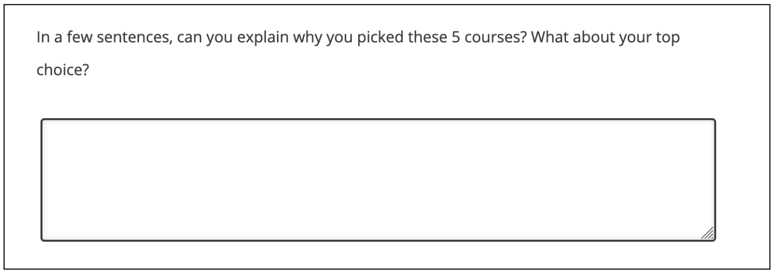



Figure E9: Admission beliefs

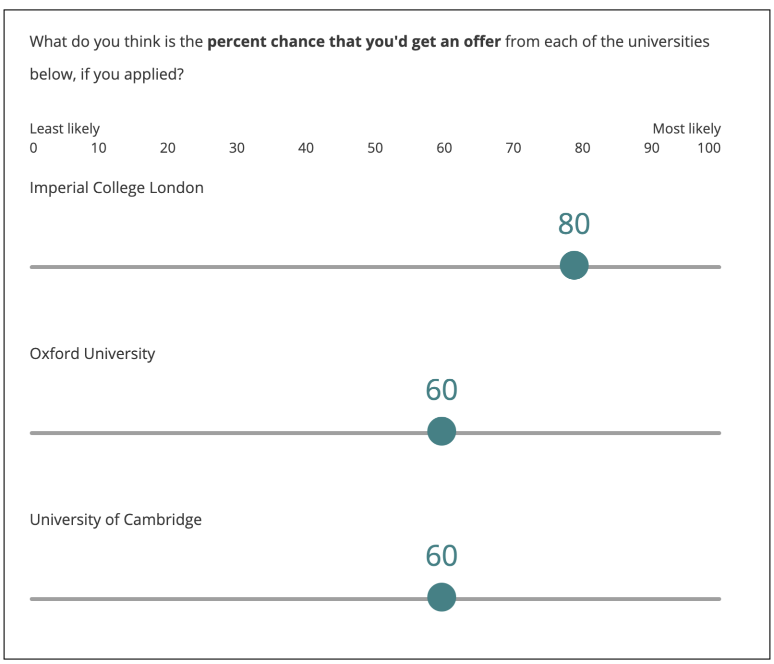


Figure E10: Social fit beliefs

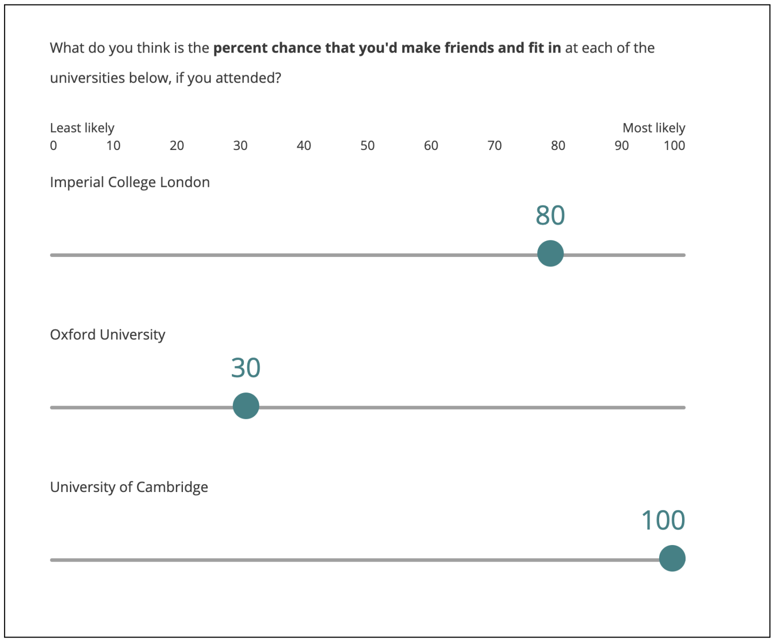



Figure E11: Graduation beliefs

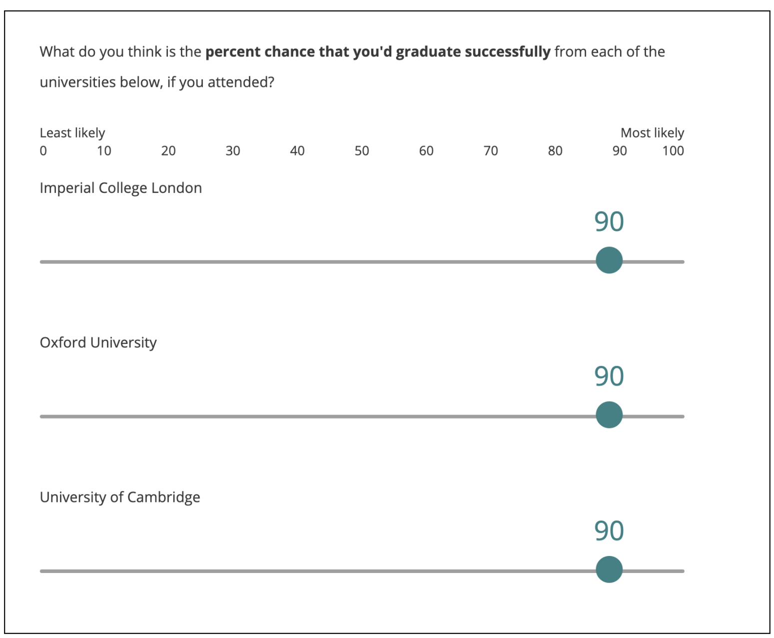


Figure E12: Typical grades beliefs

What do you think are **typical A-level grades (e.g. ABB)** for students at each of the universities below in your subject?

Imperial College London

Oxford University

University of Cambridge

Figure E13: Beliefs about typical university at students' school

Which university do you think **students at [redacted] most commonly attended** in the past few years?

The University of Liverpool

Figure E14: Introduction to videos

Next, you will watch two videos from current university students describing their application process and university experience. **Please watch both videos carefully.** We can tell if you watch the videos in full or not, so please try to watch both of them to the end. We hope that these videos will be helpful for your university application process!

Figure E15: Video page

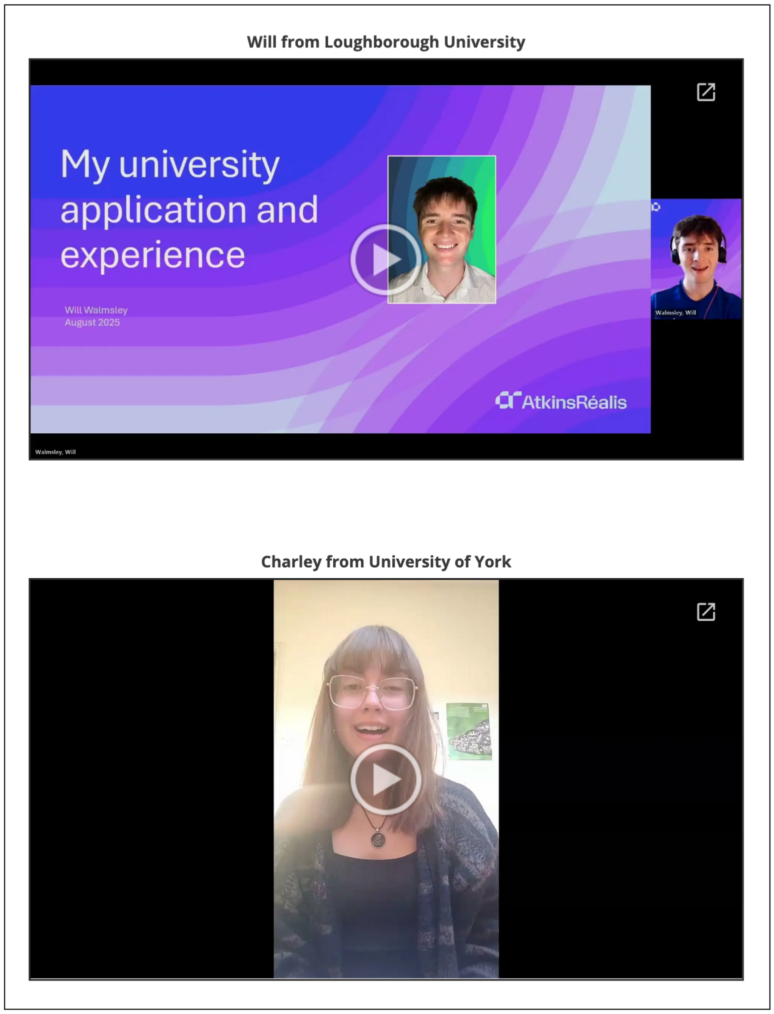



Figure E16: Video text responses

***Optional***: tell us something here about what you took away from the videos. For example, were there some things that surprised you? What did you find most helpful? Least helpful? Are there any follow up questions you would like to ask the speakers?

Figure E17: Re-elicitation of admission beliefs after video

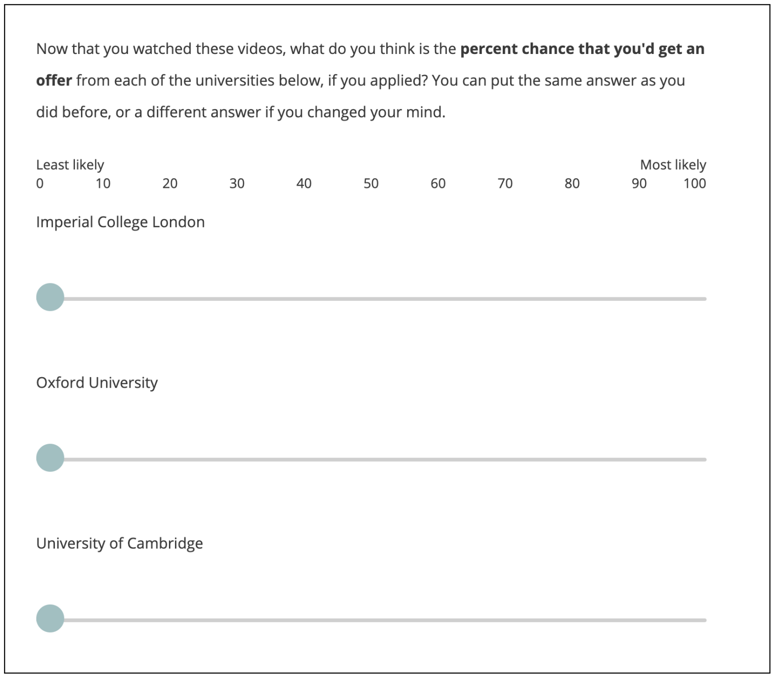



Figure E18: Re-elicitation of social beliefs after video

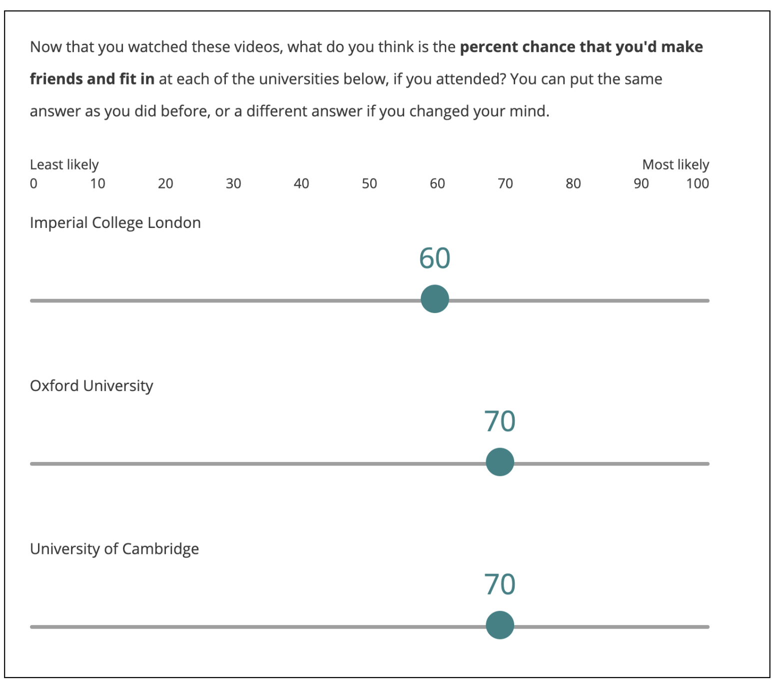


Figure E19: Re-elicitation of graduation beliefs after video

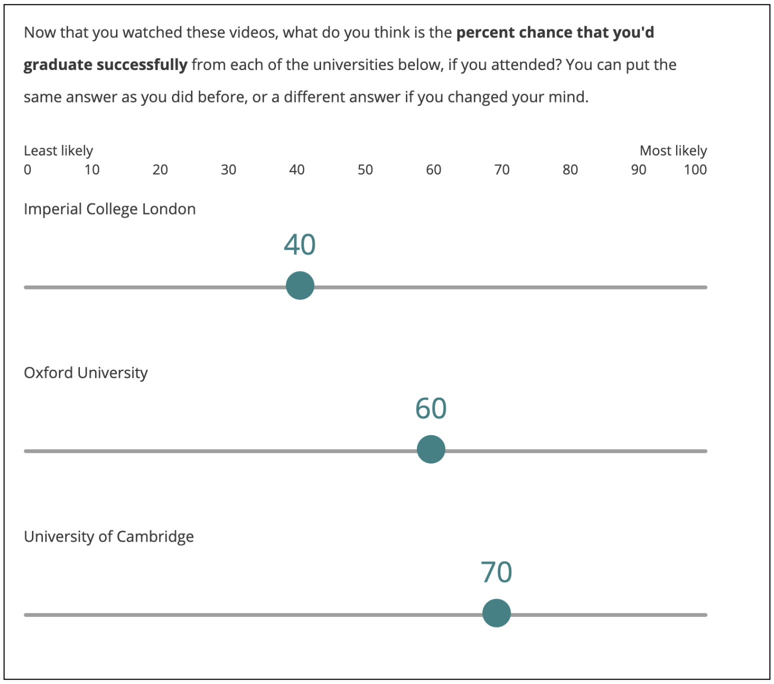
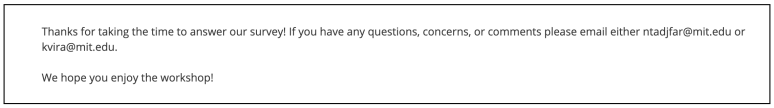
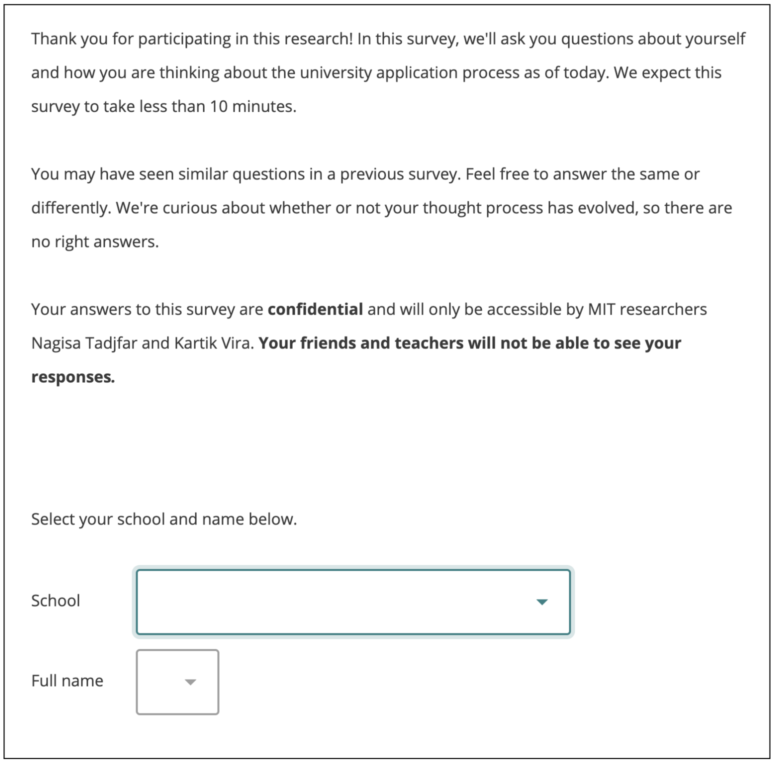



Figure E20: End of survey message



### E.2 Midline survey

Figure E21: Student name and school



Thank you for participating in this research! In this survey, we'll ask you questions about yourself and how you are thinking about the university application process as of today. We expect this survey to take less than 10 minutes.

You may have seen similar questions in a previous survey. Feel free to answer the same or differently. We're curious about whether or not your thought process has evolved, so there are no right answers.

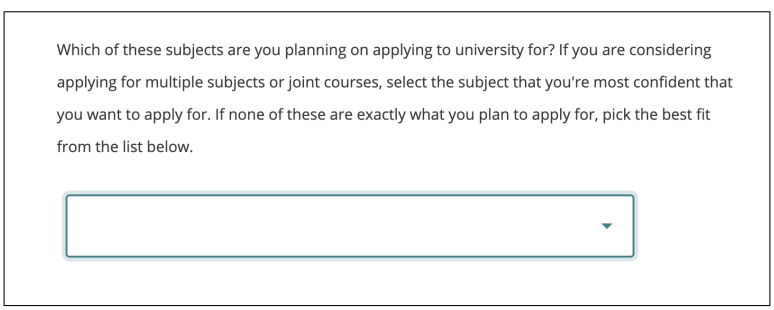
Your answers to this survey are **confidential** and will only be accessible by MIT researchers Nagisa Tadjfar and Kartik Vira. **Your friends and teachers will not be able to see your responses.**

Select your school and name below.

School

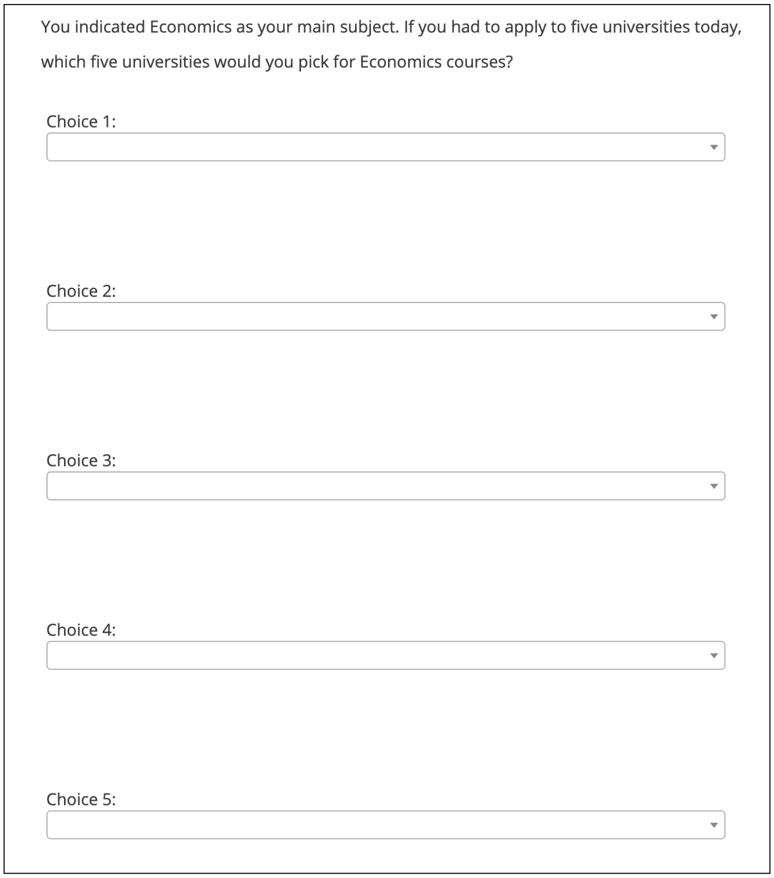
Full name

Figure E22: Student major choice



Which of these subjects are you planning on applying to university for? If you are considering applying for multiple subjects or joint courses, select the subject that you're most confident that you want to apply for. If none of these are exactly what you plan to apply for, pick the best fit from the list below.

Figure E23: University choice


You indicated Economics as your main subject. If you had to apply to five universities today, which five universities would you pick for Economics courses?

Choice 1:

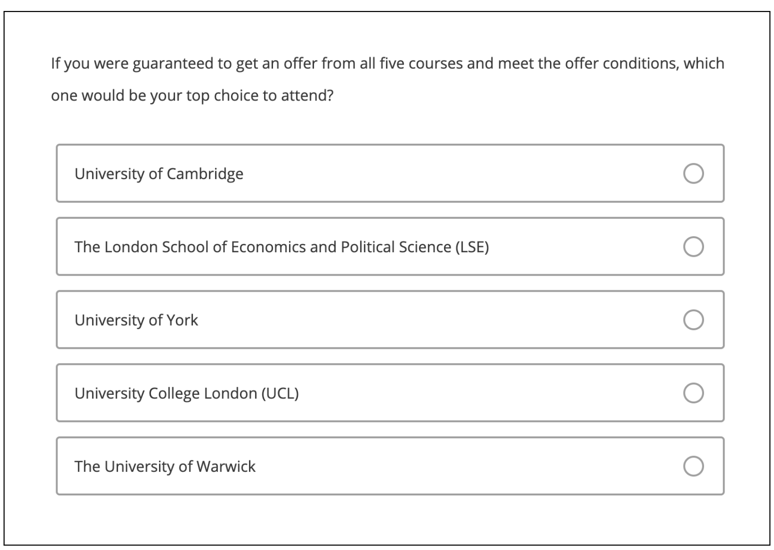
Choice 2:

Choice 3:

Choice 4:

Choice 5:

Figure E24: University top choice


If you were guaranteed to get an offer from all five courses and meet the offer conditions, which one would be your top choice to attend?

- University of Cambridge
- The London School of Economics and Political Science (LSE)
- University of York
- University College London (UCL)
- The University of Warwick

Figure E25: Family university attendees

Which of these applies to you? Select as many boxes as apply.

- [x] I have a parent or guardian who went to university
- [ ] I have an older sibling who went to or is currently in university
- [ ] I have another close relative who went to or is currently in university
- [ ] I have an older friend who went to or is currently in university
- [ ] None of the above

Figure E26: Universities attended by family members

You indicated above that you have a parent or guardian who went to university. If they went to university in the UK, please select the university(s) that they attended.

You indicated above that you have an older sibling who went to or is currently in university. If they went to university in the UK, please select the university(s) that they attended.

Figure E27: Names of friends at school

Please list the full names of 3 friends you talk to regularly at your school.

| | First name | Surname |
|---|---|---|
| Friend 1 | | |
| Friend 2 | | |
| Friend 3 | | |

Figure E28: A-level subjects and predicted grades

What are the subjects that you're taking and your predicted grades in each one? Enter up to 4 subjects below.

| | Qualification (e.g. A-level) | Subject title (e.g. History) | Predicted grade |
|---|---|---|---|
| Subject 1 | | | |
| Subject 2 | | | |
| Subject 3 | | | |
| Subject 4 | | | |

Figure E29: Ethnicity

What best describes your ethnic origin?

- White
- Black/African/Caribbean
- Asian (Indian, Pakistani, Bangladeshi, Chinese, any other Asian background)
- Mixed two or more ethnic groups
- Other (Arab or any others)
- Prefer not to say

Figure E30: Gender

What is your gender?

- Male
- Female
- Non-binary
- Prefer not to say

Figure E31: Student home postcode

What is your home postcode?

Figure E32: Interest in mentorship

After this workshop, many of you will be matched with current university student mentors who can answer any university-related questions you might have. Please let us know about your preferences for mentors in the questions below.

Would you be interested in being given a mentor?

- Yes, definitely
- Yes, probably
- Maybe
- Probably not
- Definitely not

Figure E33: Mentor preferences

What would be your **top** choice of university to get connected to a current student from?

What would be your **second** choice of university to get connected to a current student from?

What would be your **third** choice of university to get connected to a current student from?

Figure E34: Mentor preference explanation

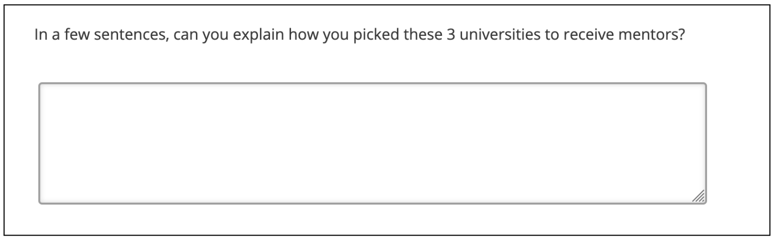
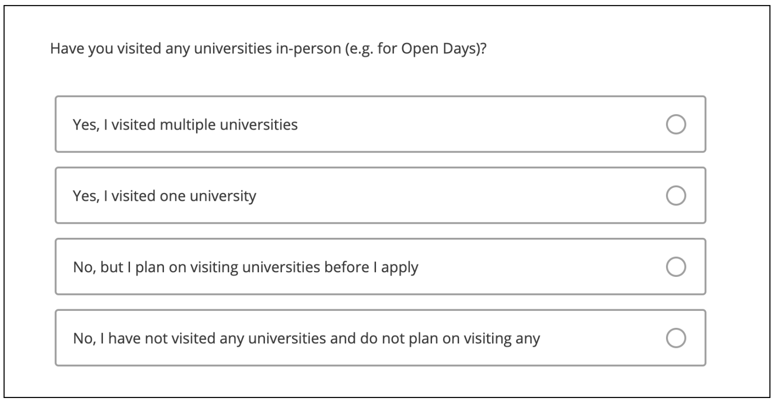
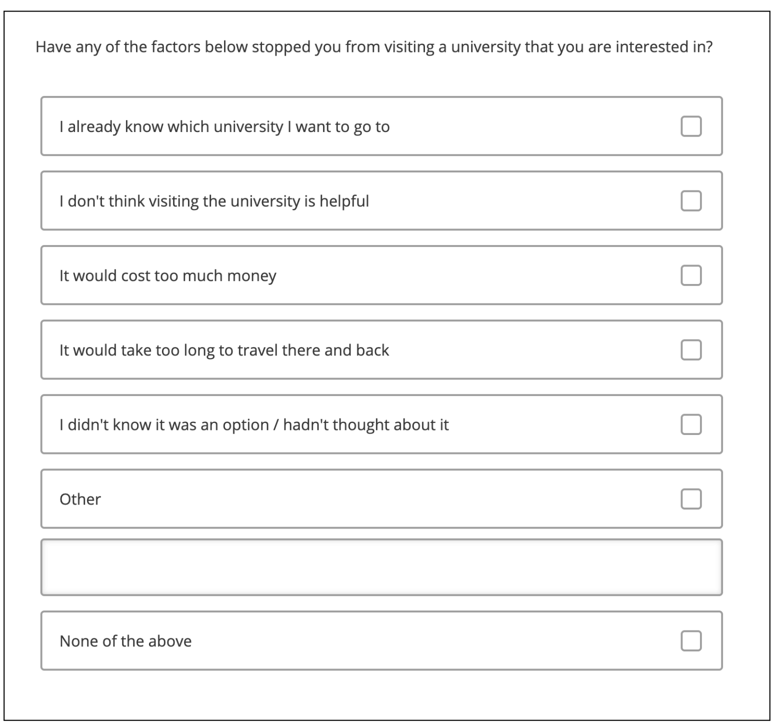

In a few sentences, can you explain how you picked these 3 universities to receive mentors?

Figure E35: Number of universities visited previously


Have you visited any universities in-person (e.g. for Open Days)?

- Yes, I visited multiple universities
- Yes, I visited one university
- No, but I plan on visiting universities before I apply
- No, I have not visited any universities and do not plan on visiting any

Figure E36: Obstacles to university visits


Have any of the factors below stopped you from visiting a university that you are interested in?

- I already know which university I want to go to
- I don't think visiting the university is helpful
- It would cost too much money
- It would take too long to travel there and back
- I didn't know it was an option / hadn't thought about it
- Other
- None of the above

Figure E37: Names of universities visited

Which universities have you visited?

Figure E38: Request for university to visit

As part of this programme, **many of you will receive up to £75 in travel funding to visit a university** of your choice in person. Which university would you like to visit using these funds? You will only be reimbursed to visit this particular university, so please choose carefully.

Figure E39: Admission beliefs

What do you think is the **percent chance that you'd get an offer** from each of the universities below, if you applied?

Least likely — Most likely

0 10 20 30 40 50 60 70 80 90 100

London School of Economics

University of Oxford

The University of Leeds

University of Strathclyde

Figure E40: Social fit beliefs

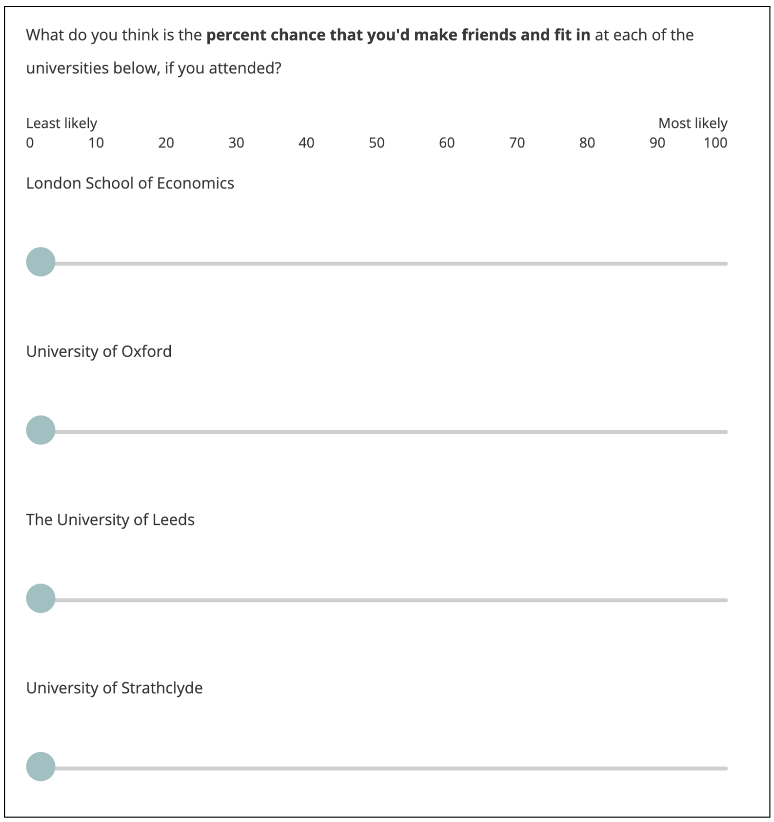
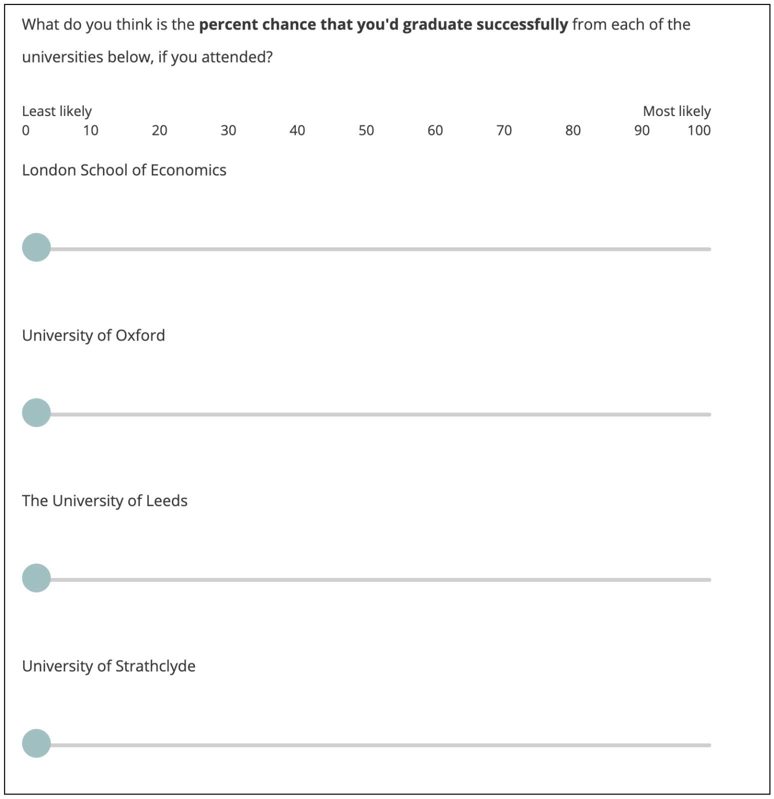


Figure E41: Graduation beliefs



What do you think is the **percent chance that you'd graduate successfully** from each of the universities below, if you attended?

Least likely | Most likely
0 10 20 30 40 50 60 70 80 90 100

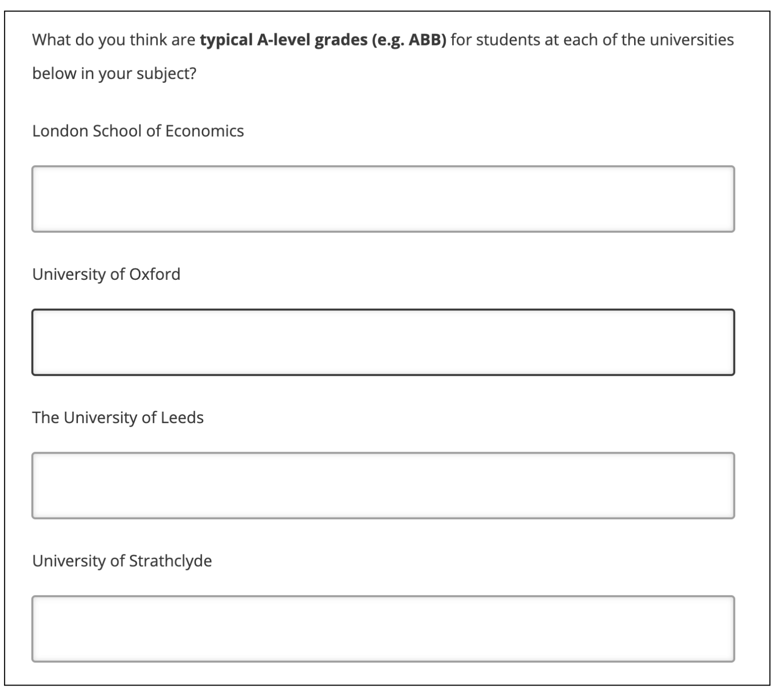
London School of Economics

University of Oxford

The University of Leeds

University of Strathclyde

Figure E42: Typical grades beliefs



What do you think are **typical A-level grades (e.g. ABB)** for students at each of the universities below in your subject?

London School of Economics

University of Oxford

The University of Leeds

University of Strathclyde

Figure E43: Ending screen


# F Experimental materials

## F.1 Workshop slides

We present a selection of the key slides from the workshop, excluding some transition slides and slides that were less relevant to university application decisions.

Figure F1: Presenter introduction

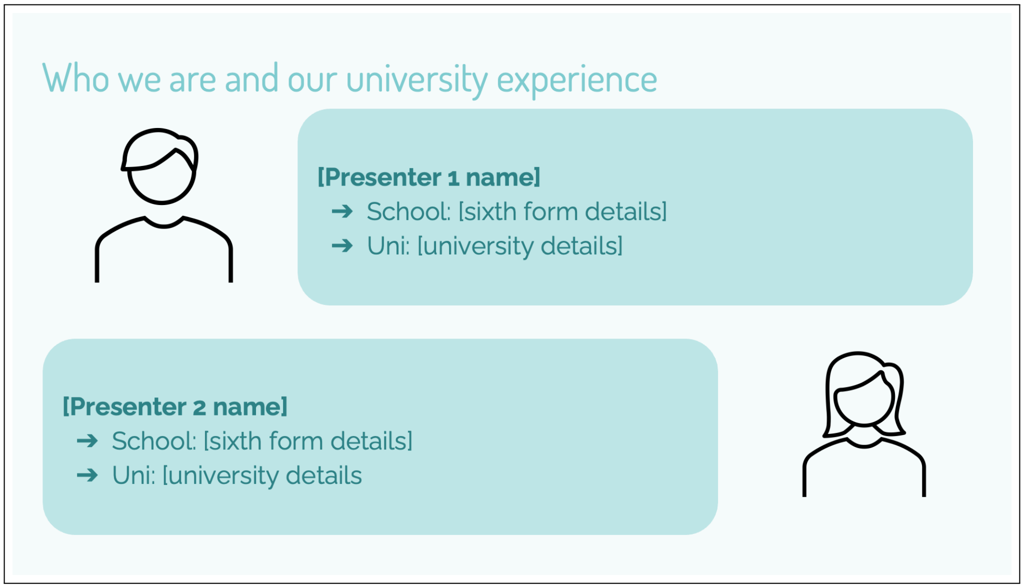



Figure F2: Application timeline

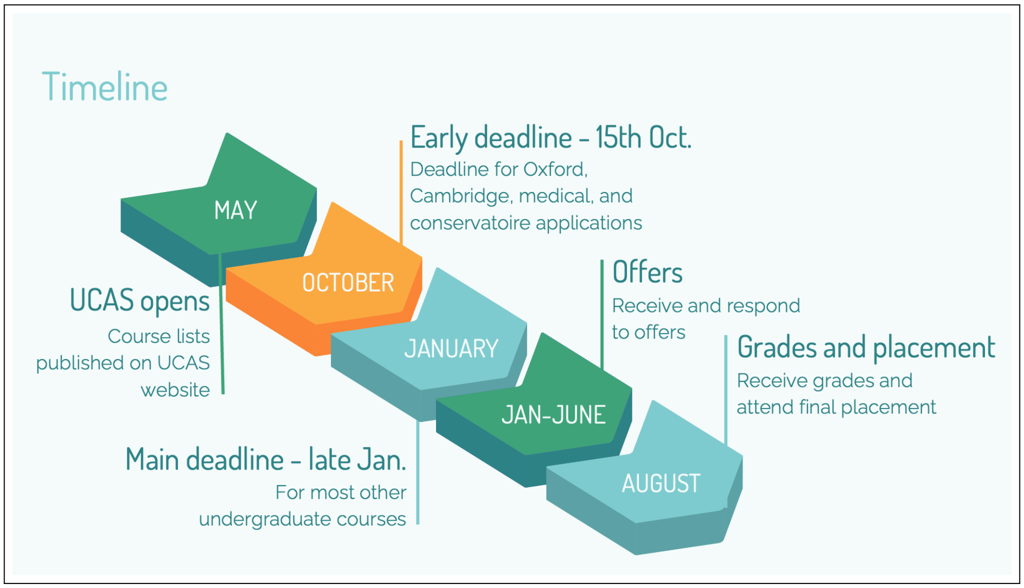


Figure F3: How universities make admissions decisions

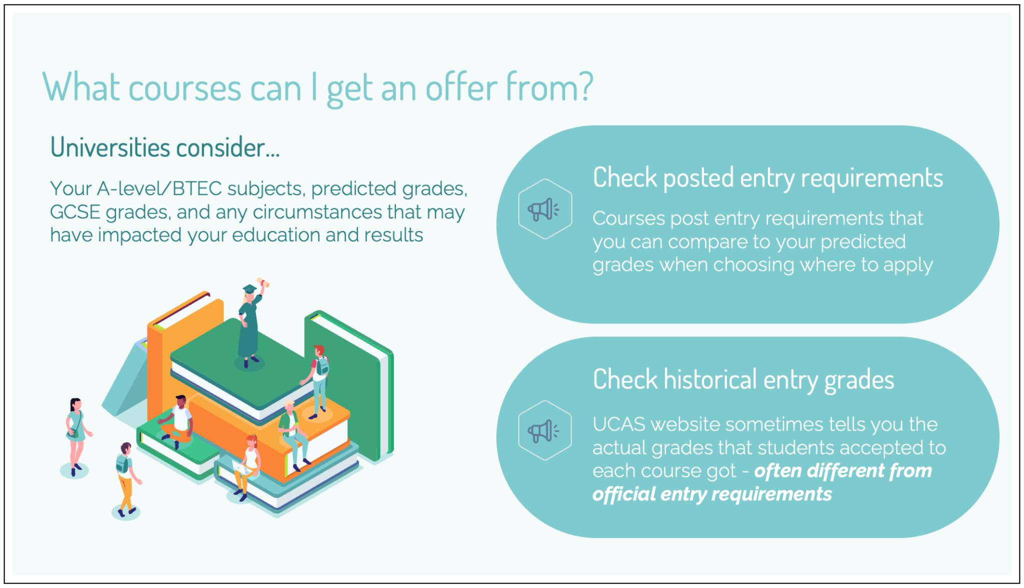



Figure F4: Contextual admissions

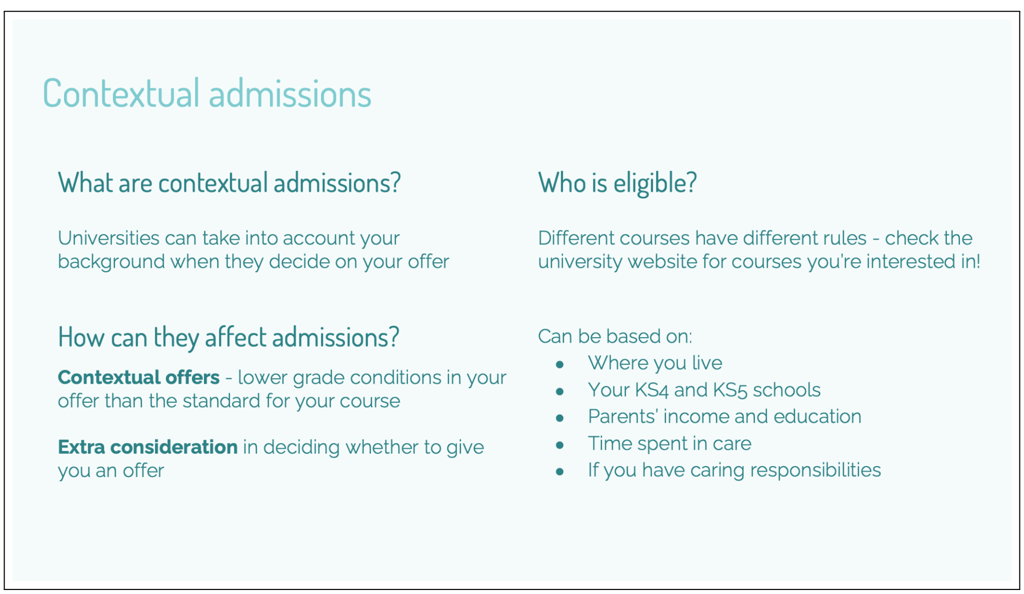


Figure F5: Russell Group student qualifications

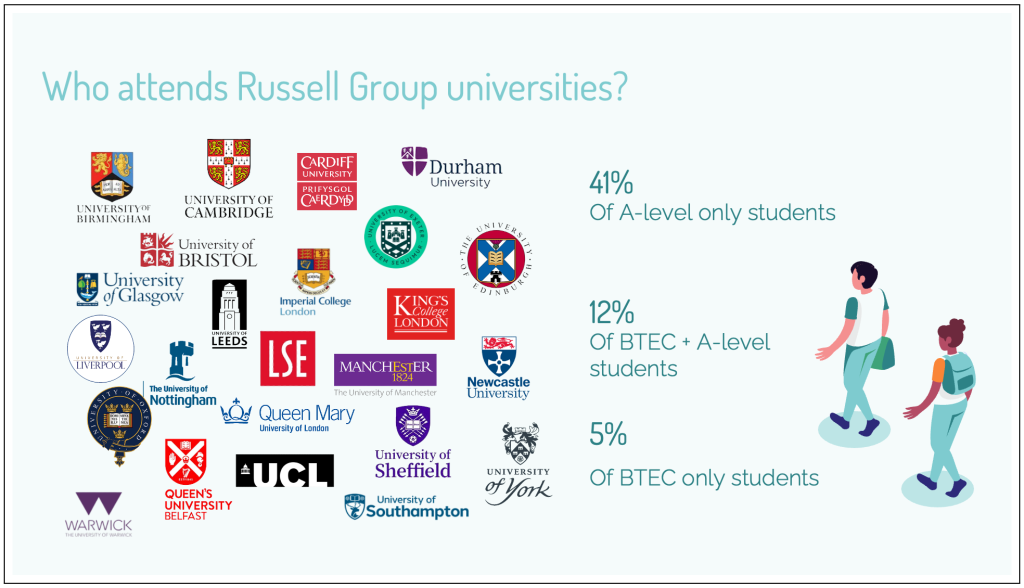



Figure F6: Russell Group student grades

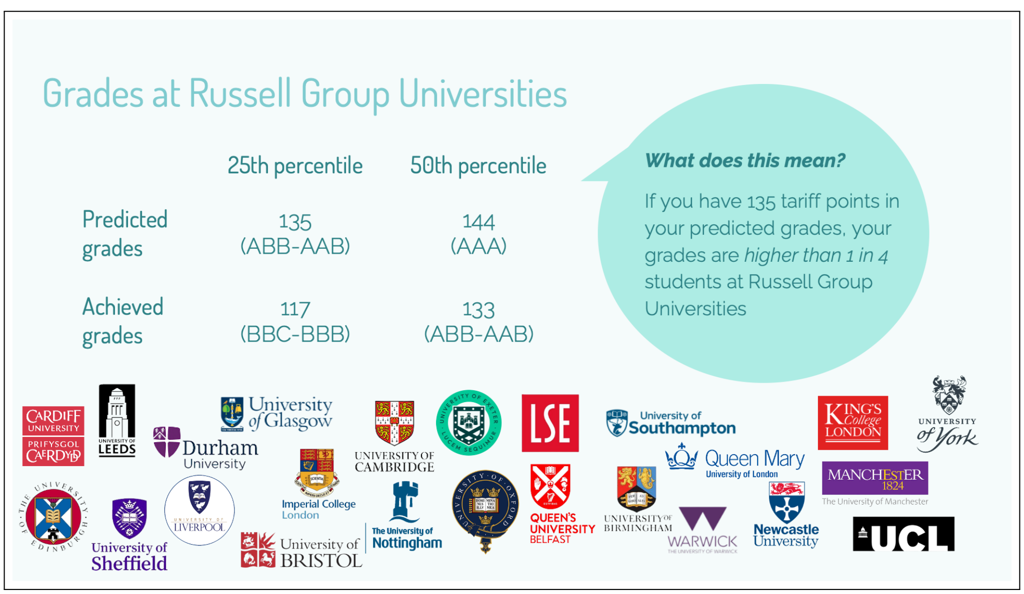


Figure F7: Required grades at different universities

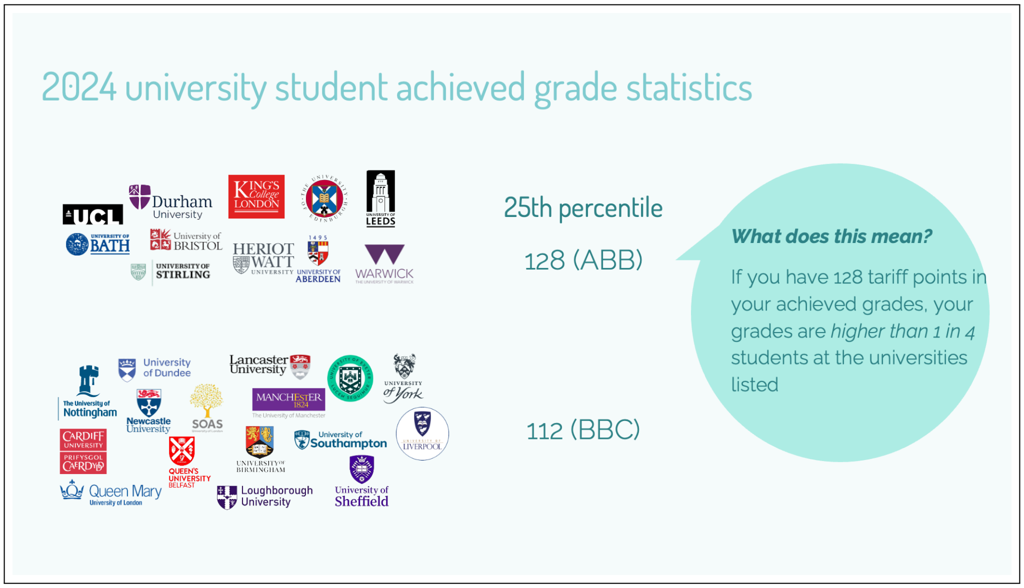



Figure F8: Earnings at different universities

Figure F9: Student regret statistics

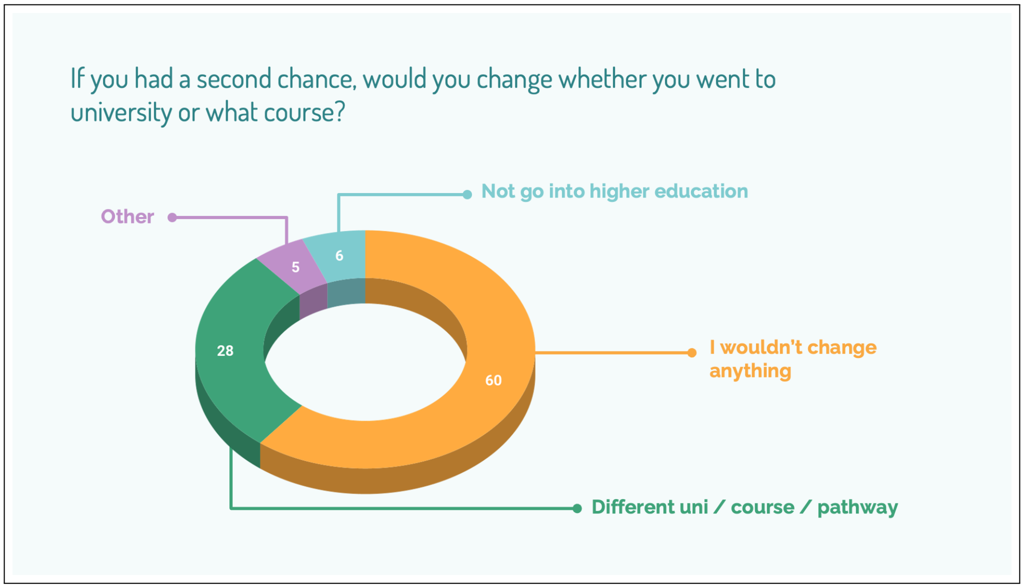



Figure F10: Student belonging and course statistics

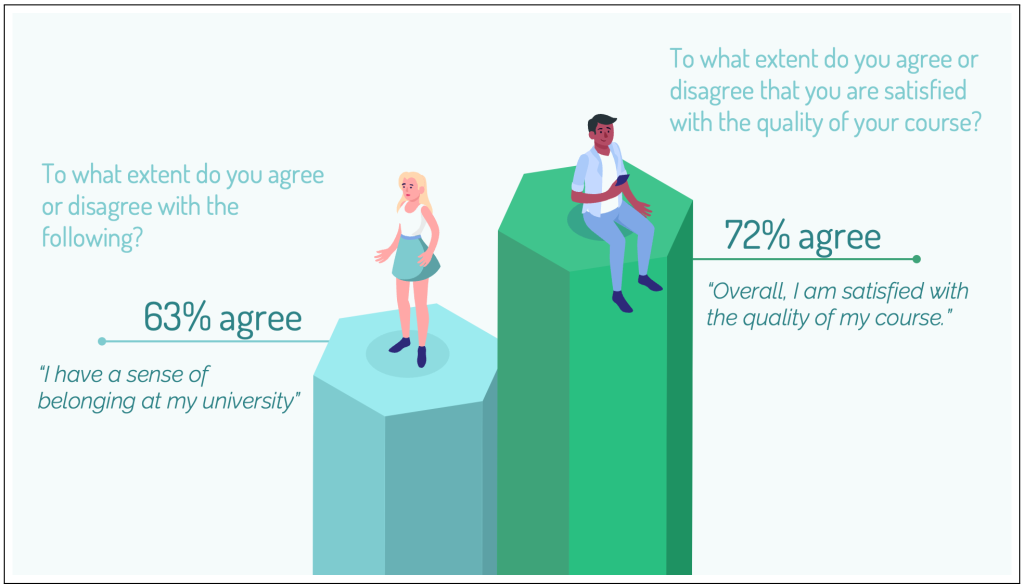


Figure F11: Responding to offers


Figure F12: Tuition and cost of living

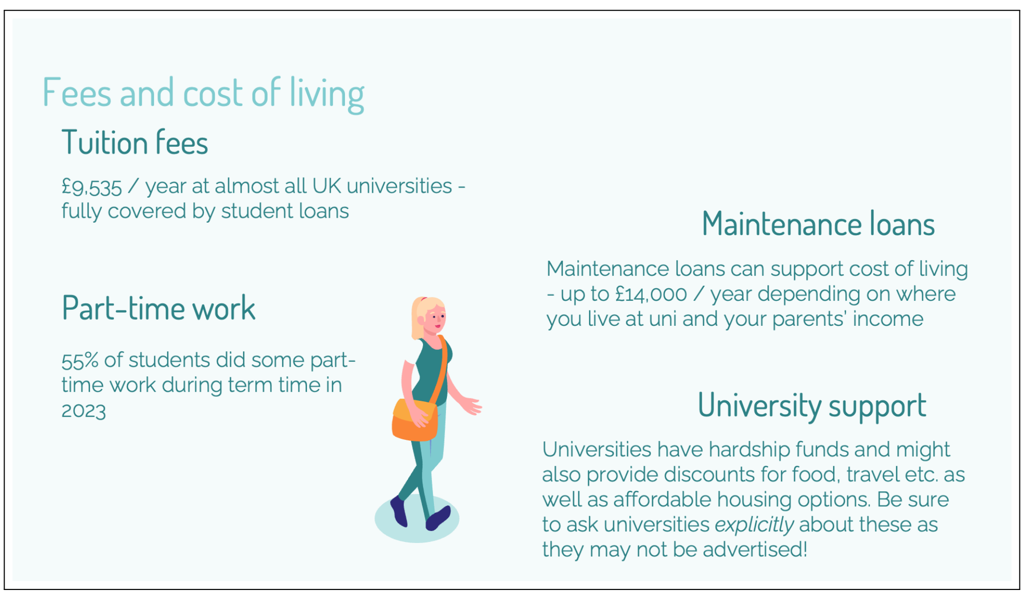


Figure F13: Personal statement format

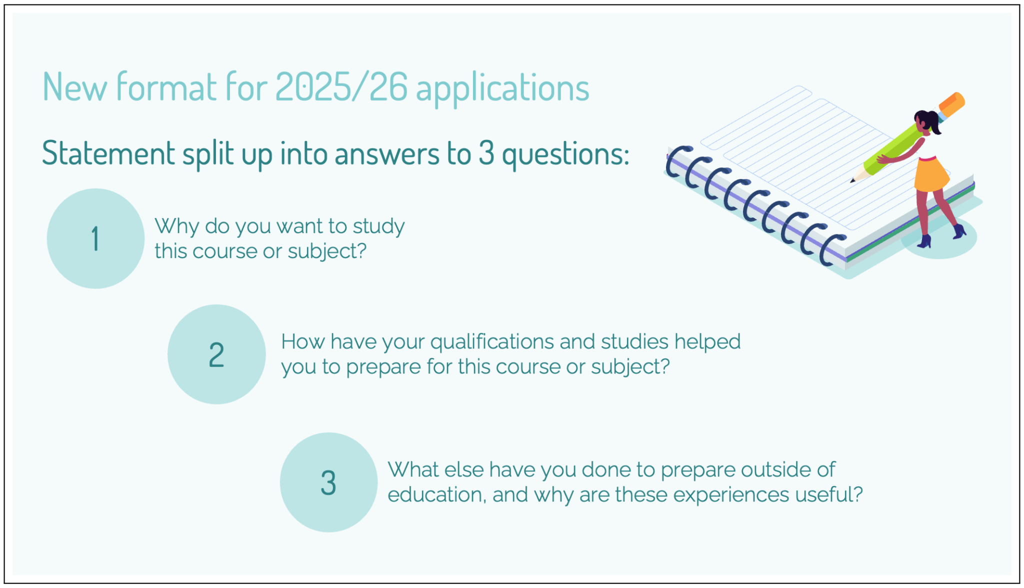



Figure F14: Personal statement advice – what not to do

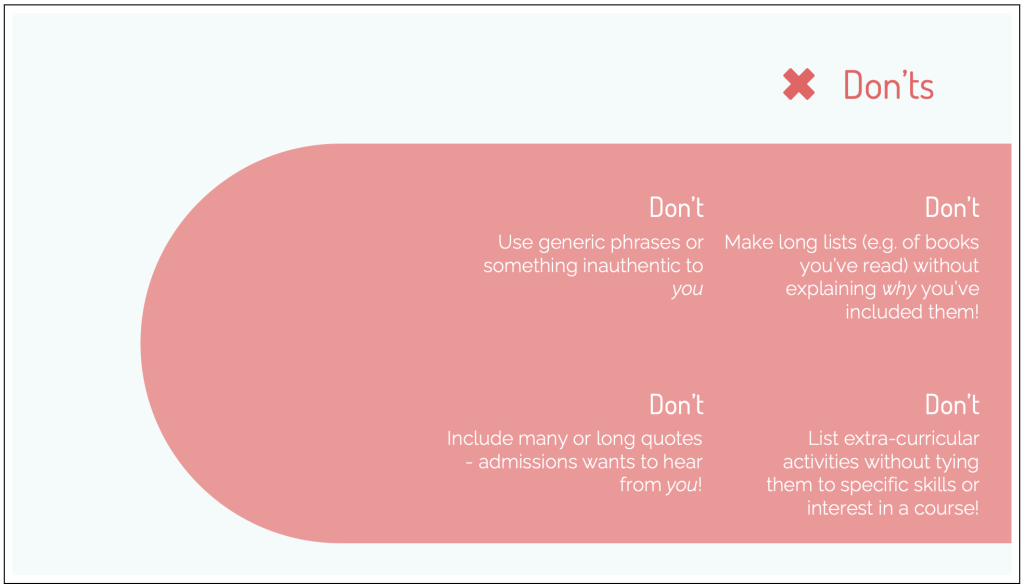


Figure F15: Personal statement advice – what to do

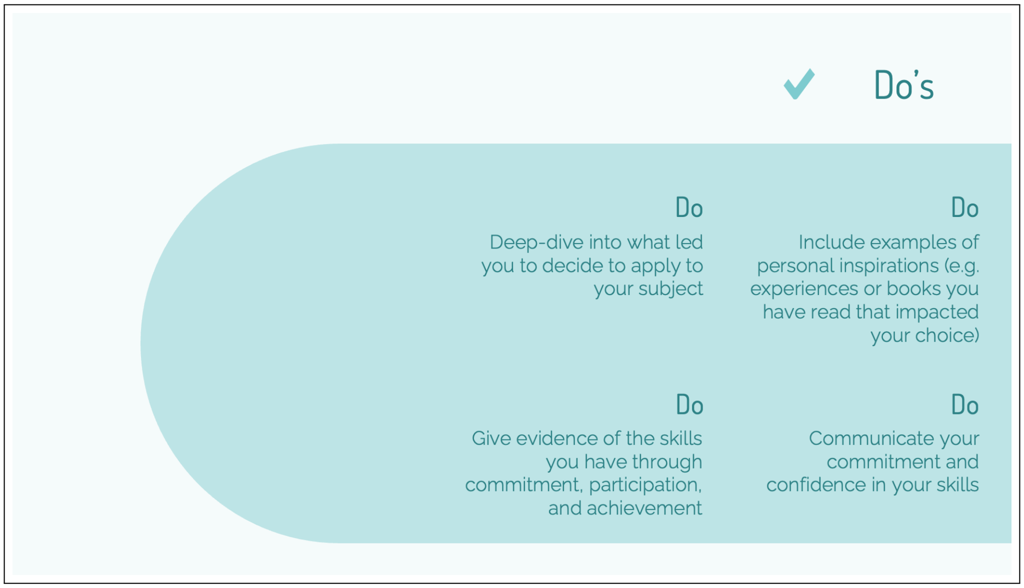



Figure F16: Online resources

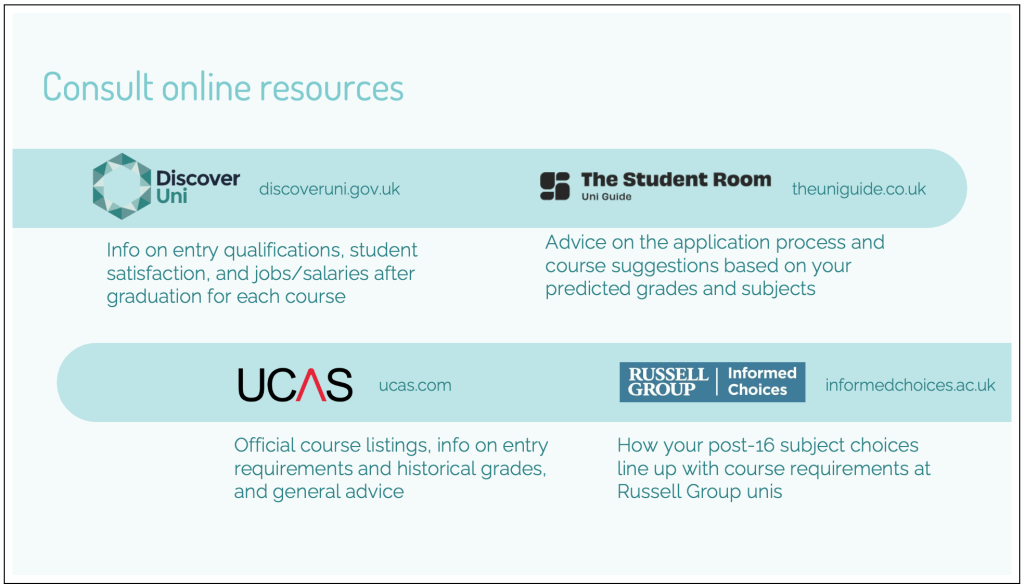


Figure F17: Other sources of information

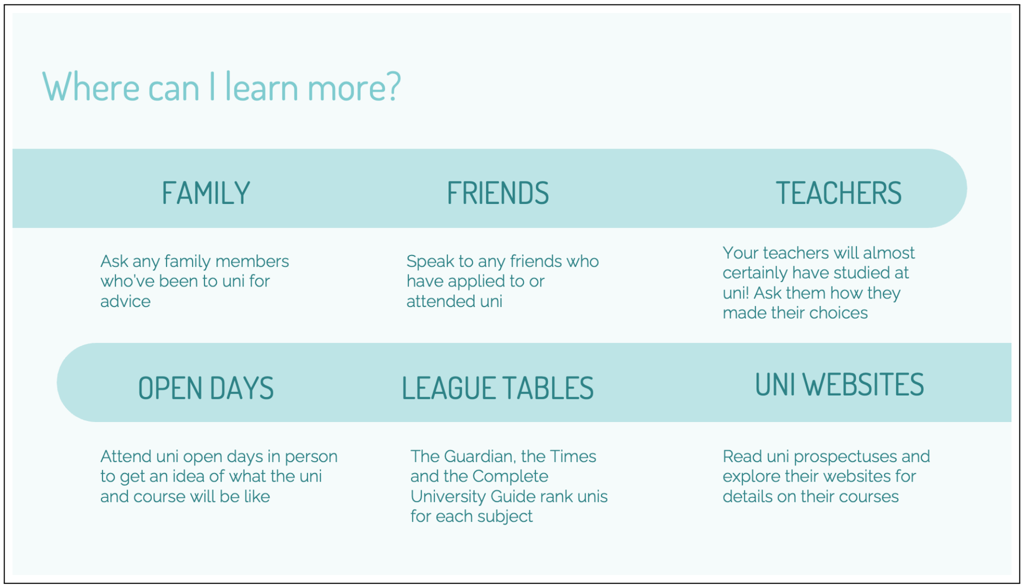



Figure F18: University visits

Figure F19: Description of mentorship / visit treatments

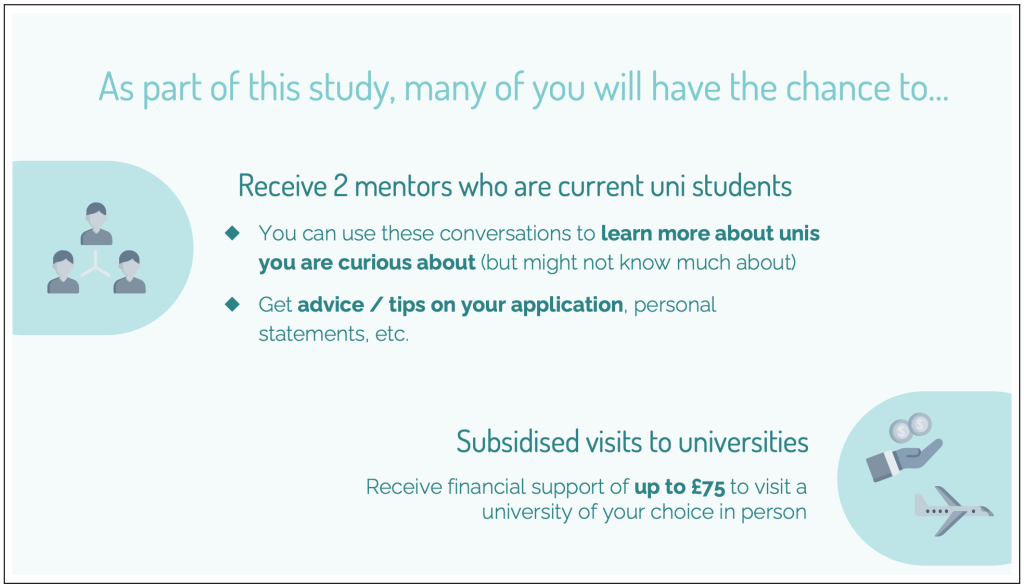



Figure F20: Survey QR code