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 a 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, a high geographical density of universities, and extensive university outreach programmes. Disparities in applications explain a substantial share—around 40%, as reported in section 2.4—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 examine how application decisions respond to the application choices of past cohorts at a student's school, and the consequences of these 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.

Our analysis is motivated by three facts that we document about universities in the UK. First, graduates of the top 10 most selective universities earn £4,600 (US$5,900) than their peers after controlling for observables and students' university offer sets, but there is little evidence of an earnings-selectivity gradient for lower-ranked universities. Second, low-income students are less likely to enrol at these top 10 universities conditional on grades, and application disparities explain around 40% of enrollment disparities. Third, low-income students are less likely to attend schools that frequently send students to top 10 universities. These results indicate that access to the top 10 universities is particularly consequential for students' earnings, and that low-income students are both less likely to apply to these universities and less likely to be exposed to past attendees from their school.

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 in which 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 where the breakthrough occurs, 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 across equally selective universities. Instead, we see a concomitant decline in application portfolios that include only 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 pointing 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 that induces more ambitious university enrollment. We see some increase in the number of students who do not place at university in the application cycle, but the students who do go on to attend the top universities are successful. Students who enrol following a breakthrough are at least as likely to graduate, or graduate with a passing grade, as typical students in 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. 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 application, we find that compliers who apply in response to a breakthrough are high-ability relative to their school but no less economically disadvantaged than the typical student at their school. Compared to typical students at the university that they enrol in 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.

What are the mechanisms that drive this response to past cohorts' decisions? To help clarify the potential belief mechanisms, we first provide a conceptual framework in which observing the outcomes of a past student changes application behaviour by shifting beliefs about how the academic and social payoffs at a 'risky' university depend on students' academic ability and social type. Exposure to a university—observing a past student—will tend to encourage applications on average, and will shift beliefs more in dimensions where students have higher prior uncertainty.

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 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, scalable forms. Students in an active control arm receive an in-school informational 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 allow us to isolate the particular beliefs that shift in response to exposure.

We first find that students are more likely to apply to a university after watching a video from a past attendee. Students randomised into watching these videos have a 30% increase in their stated intent to apply to that university—about 5 percentage points—in a later survey. 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 the treatment effects of videos, we find precise null effects on beliefs about admissions probability or successful graduation from the video universities. This null applies to both mean belief updating and the distribution of updates. The result is consistent with the 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 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. Taken together, this evidence indicates that students' perceptions of the social environment at a university are an important mechanism through which exposure affects university applications.

Overall, we consistently find across both evidence from breakthroughs and from 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 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 points 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.

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 (Hoxby and Turner 2013, 2015; Andrews, Imberman, and Lovenheim 2020; Dynarski et al. 2021; Sanders, Chande, and Selley 2017; Cohodes, Ho, and Robles 2022); our interventions target providing role models and exploiting the effects of exposure and beliefs about social fit.

Beyond higher education choice in particular, 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 across different types of peers and aspects of exposure, and unpacking the interactions and mechanisms that underlie these 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 underlying mechanisms.

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.

The educational system includes regular standardized exams embedded in the core school curriculum. 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 administrative data analysis as a measure of students' academic ability. Students then spend the last two years of secondary education completing academic qualifications (A-levels) or vocational qualifications.

Virtually all British universities accept applications only through the Universities and Colleges Applications Service (UCAS), which centralises the application process. 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. Students do not rank their applications. Upon application, 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 independently decide whether to make an offer to an applicant or reject them outright, and final university placements are then decided based in part on students' end-of-year grades.

The typical length of an undergraduate degree in the UK is three years, although a substantial minority of courses last four 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. The latter is generally considered to be the threshold for a 'good degree', and is often required of new graduates by employers.[2]

Finally, financial frictions are less relevant to university choice than they are in countries such as the US. University tuition is uniform across universities in England and Wales.[3] 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 universities charge tuition fees exactly at the cap, meaning that there is no variation in tuition between universities; financial considerations

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. 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.

3. 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.

thus only enter into the choice *between* universities to the extent that a student's cost of living differs across universities.

2.2 Administrative data

Our administrative data are 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 from different data providers, along with consistent anonymised individual identifiers that enable these datasets to 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 ununiversities, offers, student responses to offers, and final offers accepted. The Higher Education Statistics Agency (HESA) collates data provided on a mandatory basis by universities about student enrollment, graduation, degree class, course studied, and various other details about outcomes 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) extend 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.

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 do not apply at all are excluded, as are any applications submitted 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 generally more specialised courses, such as arts academies or music conservatoires, rather than traditional university courses.

LEO pseudonymises all institutional identifiers, so we cannot 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). This extract provides data on GCSE and A-level grades, demographics, and university applications, offers and acceptances, but does not provide linkages to the other components of LEO.

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 details on the design of this RCT in Section 6, but outline the setting here.

We conducted the RCT between the autumn of 2024 and 2025 at over 20 schools across England and Wales. Schools were recruited to participate in the programme through various channels, including 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, but has a network of coeducational schools that we worked with to recruit students. 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 the basis of socioeconomic status. Table 1 presents summary statistics for students participating in our RCT, as well as comparable statistics drawn from the administrative data. 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 in 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 population of age-18 school leavers 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.

2.4 Patterns in university access and returns to university

We present three facts about UK universities that provide context for the analysis that we conduct in the rest of the paper. Appendix C provides more detail on the methodology for each of these results.

First, graduates of the top 10 most selective universities earn substantially more than graduates from lower-ranked universities after we control for selection. Unconditionally, at age 27, graduates of top 10 universities earn £35,800 on average, compared to £29,400 for graduates of universities ranked 11–30 and £23,200 for lower-ranked universities (see Appendix Figure C1a). Controlling for observables (test scores, demographics, and major), as well as student offer sets (following Dale and Krueger 2002, 2014) shrinks these gaps substantially. After including these controls, there is still a clear earnings premium for the top 10 universities relative to the modal university (ranked 56), averaging £4,600. Below the top 10 universities, however, there is little evidence of an earnings-selectivity gradient (see Appendix Figure C1c). Thus, access to the top 10 universities is particularly important for students' earnings outcomes and equality of opportunity.

Second, disparities in application rates between rich and poor students explain around 40% of overall disparities in enrollment at the top 10 universities. If low-income students were as likely to apply to top 10 universities as high-income students, while offer rates and enrollment conditional on receiving an offer remained the same, enrollment disparities for the highest-ability students (at or above the 95th percentile of age-16 GCSE grades) would fall from 6–10pp to 3–6pp (see Appendix Figure C2). Closing gaps in applications is therefore a consequential margin for closing overall gaps in university enrollment between low-income and high-income students.

Third, poorer students are much more likely to attend a school that has sent no-one to a top 10 university, meaning they lack exposure to top universities through their school. 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. Controlling for other observables, such as gender, ethnicity, and test scores, shrinks this disparity, but there is still a 10pp disparity in students' probability of being at a school with recent attendees at top 10 universities. (See Appendix Figure C3.) Low-income students are thus less likely to be exposed to attendees at top universities through their school.

Taken together, we see that access to top universities is consequential for students' earnings, that low-income students are less likely to apply to these universities, and that these students are less likely to be exposed to these universities through their schools. We now use breakthroughs to evaluate how changes in enrollment patterns at a school affect the application choices of subsequent cohorts and their labour market outcomes.

3 Design for breakthrough analysis

3.1 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 in our data. To implement the strategy outlined above, we identify, for each university, the schools where no students enrolled at the university between 2007 and 2009, the first three years of our data. We then define the first year 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:

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

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 show trends in applications at the affected universities transparently, 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.[4]. We exclude the breakthrough student themself 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 for each university as described above. We then stack these datasets, indexing data from each by the breakthrough university u, and then run the following stacked event study regression:

$$Y_{istu} = \alpha_{su} + \gamma_{tu} + \delta_u X_{istu} + \sum_{\tau \neq -1} \beta_\tau \mathbb{I}(t - T_{su} = \tau) D_{su} + \varepsilon_{istu} \tag{2}$$

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 each university, a measure of the university's selectivity.

We also report results for certain outcomes using the analogous difference-in-differences specifi-

4. 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.

cation of these effects, which pools effects across the post-treatment periods:

$$Y_{ist} = \alpha_s + \gamma_t + \delta X_{ist} + \beta \mathbb{I}(t - T_s \geq 0) D_s + \varepsilon_{ist} \tag{3}$$

$$Y_{istu} = \alpha_{su} + \gamma_{tu} + \delta_u X_{istu} + \beta \mathbb{I}(t - T_{su} \geq 0) D_{su} + \varepsilon_{istu} \tag{4}$$

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

3.2 Matching for earnings effects

To understand the effects of breakthroughs on student welfare, we can examine their effects on students' earnings, leveraging 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', though note that this set of students includes both compliers (who apply to the breakthrough university only 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 regress earnings at age 27 on pair fixed effects and an indicator for applying to the breakthrough university.

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.

3.3 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_{istu} indicate whether a student i applies to the breakthrough university u. For any observable characteristic X_{istu}, we regress

$$X_{istu}a_{istu} = \alpha_{su} + \gamma_{tu} + \beta a_{istu} + \varepsilon_{istu} \tag{5}$$

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

4 Effects of university breakthroughs

4.1 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 1a plots coefficients on the relative time indicators from (1), where the treated schools are those that experience a breakthrough to Cambridge and the outcomes are application to Oxford and Cambridge as indicated; Figure 1b 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. This rules out any explanation that attributes the increase in applications following a breakthrough to a common shock that would affect applications to a range of universities.

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*.

4.2 Where breakthroughs draw applications from

Figure 2 generalises these results to a broader range of universities, pooling across universities with similar ranks as described in Section 3.1. 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 2 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 (4). 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 3.1, 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%,[5] 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

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

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.

4.3 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, 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 3 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, among those induced to enrol by the breakthrough, almost all students graduate, as 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 enrol 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 patterns similar to those in applications: an increase in attendance at the breakthrough university and a decrease in attendance at 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 riskier, and some students lose out by failing to attend university in that cycle. On the other hand, students are free to reapply in the next application cycle, so this result may overstate effects on final university attendance. The results below indicate that students who attend the breakthrough university benefit, and that the average earnings effect for induced applicants is positive, but there may be costs for some students.

We can also compare outcomes for students who enrol at a university from a breakthrough school with those of typical enrollees at the university. To do this, we can simply regress graduation and degree outcomes on an indicator for whether a student enrolled following a breakthrough. 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 induced students having lower income and 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 or a First are the same as typical enrollees. This analysis thus 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.[6] 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 3.2. Note that this includes all induced applicants, so we are not restricting to students who successfully place at the breakthrough university. Figure 4 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 Appendix Figure C1c, 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 that students who apply following a breakthrough are made worse 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 there is no real growth in the earnings premium for induced applicants, so this premium 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

6. 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.

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.

4.4 Heterogeneity in responses to breakthroughs

Who are the students who respond to breakthroughs? First, following the method described in section 3.3, 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 with those of other student populations. 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 come from low-income neighbourhoods or be 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 to promote social mobility and reduce undermatching. Breakthroughs diversify the socioeconomic backgrounds of the intake at top universities.

These results summarise characteristics of students who respond to breakthroughs. If breakthroughs affect applications by inducing exposure to students who have attended top universities, we would also expect stronger effects for students who are more closely connected to breakthrough students. We cannot observe social connections directly in administrative data, but students who have demographic variables in common with the breakthrough 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 E1). 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 fewer opportunities to interact 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 it is common—but not ubiquitous—for students in the UK to change school after completing their GCSEs at age 16.

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-differences 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.

4.5 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 2.4, 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 also saw in Section 2.4 that around 40% of the differences in enrollment 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 provides 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 Δ_u. Then we can predict

$$Pr(\text{apply}_u \mid l, e_u^h) = Pr(\text{apply}_u \mid l, e_u^l) + \Delta_u(e_u^h - e_u^l) \tag{6}$$

To align more closely with the analysis in Section 2.4 (detailed in Appendix C), we can condition these calculations on GCSEs. We pool GCSE grades into ventiles (since we are not powered to estimate treatment 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

$$Pr(\text{apply}_u \mid g, l, e_u^h) = Pr(\text{apply}_u \mid g, l, e_u^l) + \Delta_u(g)\left(e_u^h(g) - e_u^l(g)\right) \tag{7}$$

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

$$Pr(\text{apply top 10} \mid g, l, e^h) = Pr(\text{apply top 10} \mid g, l, e^l) + \sum_{u \in \{1,\ldots,10\}} \Delta_u(g)\left(e_u^h(g) - e_u^l(g)\right) \tag{8}$$

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 5 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 (8); 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.

4.6 Interpretation of breakthrough effects

What could explain the patterns that we see above? The key result 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. This pattern rules out common shocks such as a sudden increase in students' ability or academic performance, as such shocks 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,[7] 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.

7. 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.

Explanations in terms of a *university-specific* common shock affecting all students at the school are potentially more challenging. 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,[8] 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[9]. 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. This table presents event studies for students near or far from the breakthrough university, using different definitions. Across the different specifications, there are significant increases in applications following a breakthrough even for the students who are more distant from the breakthrough university.

An explanation that is consistent with university-specific effects is the arrival of a new teacher who has a connection to a specific university, and encourages applications to this university. In the UK system, the teachers that students spend most time with are teachers for specific 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 clear and substantial increase in the application rate for students who do not share any subjects with the breakthrough indicates that teacher effects cannot fully explain the effects of breakthroughs.

The patterns of heterogeneity in breakthrough effects described in section 4.4 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

8. Data on university outreach activities are 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 the Department for Education to implement this linkage.

9. See Burtonshaw et al. 2024 for an evaluation of Uni Connect.

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.[10] 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 E1). The heterogeneity in effects within schools is evidence against interpretations of the breakthrough effects in terms of shocks 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.

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 6 and 7. 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.

5 Conceptual Framework

5.1 A framework for belief updating about a risky university

Why would a student's application decisions react to past 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 from 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 with unknown payoff. If the individual attends the risky university, they get a payoff

$$U_i = \kappa W(u_i) + (1 - \kappa)W(v_i) \tag{9}$$

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 γ, 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 students' test scores in the data. Social type should be

10. 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.

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 θ_i and a_i with no uncertainty. They know that u_i, v_i depend on θ_i and a_i, but are uncertain about the precise *relationship* between characteristics and payoffs. Specifically, assume that payoffs take the linear parametric forms

$$u_i = \beta_0^u + \beta_1^u \theta_i + \beta_2^u a_i + \varepsilon_i^u = x_i' \beta_u + \varepsilon_i^u \tag{10}$$

$$v_i = \beta_0^v + \beta_1^v \theta_i + \beta_2^v a_i + \varepsilon_i^v = x_i' \beta_v + \varepsilon_i^v \tag{11}$$

where:

$$\beta^u \sim N(b^u, V^u), \beta^v \sim N(b^v, V^v) \tag{12}$$

$$\varepsilon_i^u \sim N(0, \sigma_u^2), \varepsilon_i^v \sim N(0, \sigma_v^2) \tag{13}$$

$$Cov(\beta^u, \beta^v) = 0, Cov(\varepsilon_i^u, \varepsilon_i^v) = 0. \tag{14}$$

As payoffs u_i, v_i are normally distributed and the utility function $W(\cdot)$ is CARA, the certainty equivalent of the payoff from the risky university is

$$CE_i = \kappa \left(x_i' b^u - \frac{1}{2} \gamma \left(x_i' V^u x_i + \sigma_u^2 \right) \right) + (1 - \kappa) \left(x_i' b^v - \frac{1}{2} \gamma \left(x_i' V^v x_i + \sigma_v^2 \right) \right) \tag{15}$$

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

$$(\tilde{u}_j = u_j + \eta_j^u, \tilde{v}_j = v_j + \eta_j^v, x_j) \tag{16}$$

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 β^u, β^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 formalises how students update their beliefs in response to exposure to a student.

Updating β^u and β^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 β^u is $\beta^u \sim N(\tilde{b}^u, \tilde{V}^u)$, where

$$\tilde{V}^u = V^u - \frac{V^u x_j x_j' V^u}{\sigma_u^2 + s_u^2 + x_j' V^u x_j} \tag{17}$$

$$\tilde{b}^u = b^u + \frac{1}{\sigma_u^2 + s_u^2 + x_j' V^u x_j} (\tilde{u}_j - x_j' b^u) V^u x_j \tag{18}$$

Proposition 1 summarises how an observation affects beliefs in this framework.

Proposition 1. *Following an observation of a student j, the change in expected payoff is*

$$\begin{aligned}
\Delta E(u_i) &= x_i'(\tilde{b}^u - b^u) && (19)\\
&= \left(\frac{1}{\sigma_u^2 + s_u^2 + x_j'V^u x_j}\right)\left(\tilde{u}_j - x_j'b^u\right)\left(x_i'V^u x_j\right) && (20)\\
&= Corr(u_i, \tilde{u}_j)\sqrt{\frac{Var(u_i)}{Var(\tilde{u}_j)}}(\tilde{u}_j - x_j'b^u) && (21)
\end{aligned}$$

and the change in the payoff variance is

$$\begin{aligned}
\Delta Var(u_i) &= x_i'(V^u - \tilde{V}^u)x_i && (22)\\
&= \frac{x_i'V^u x_j x_j'V^u x_i}{\sigma_u^2 + s_u^2 + x_j'V^u x_j} && (23)\\
&= \left(Corr(u_i, \tilde{u}_j)\right)^2 Var(u_i) && (24)
\end{aligned}$$

The resulting changes in certainty equivalents for each payoff component are:

$$\Delta CE_i^u = \Delta E(u_i) - \frac{1}{2}\gamma\Delta Var(u_i), \Delta CE_i^v = \Delta E(v_i) - \frac{1}{2}\gamma\Delta Var(v_i) \quad (25)$$

and the change in the overall certainty equivalent of the risky university is

$$\kappa\Delta CE_i^u + (1-\kappa)\Delta CE_i^v \quad (26)$$

Implications of this framework for how beliefs update are summarised in Proposition 2.

Proposition 2. *The following properties hold:*

(i) $\Delta Var(u_i) < 0$.

(ii) *Taking the expectation over realisations of the signal* $\tilde{u}_j$,
$E\left[\Delta E(u_i)\right] = 0, E\left[\Delta Var(u_i)\right] < 0, E\left[\Delta CE_i^u\right] > 0.$

(iii) *The absolute magnitude of* $\Delta E(u_i)$ *and* $\Delta Var(u_i)$ *are increasing in prior uncertainty* $Var(u_i)$.

(iv) *The absolute magnitude of* $\Delta E(u_i)$ *and* $\Delta Var(u_i)$ *are decreasing in the signal noise* s_u^2.

5.2 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 θ 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 θ_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. 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, both the mean and variance of beliefs about both academic and non-academic payoffs can update in response to the signal observed. Per Proposition 2.i, the posterior variance is always lower following a signal; the signal provides information that reduces the student's posterior uncertainty about their payoffs. Proposition 2.ii notes that the average change in mean beliefs about expected signal variance is 0, and so on average, exposure to another student will raise the certainty equivalent of the risky university and encourage applications for risk-averse students.

Proposition 2.iii indicates that *ceteris paribus*, both the posterior mean and variance of beliefs will update more about the payoff that the student is least certain about to begin with, and thus the effects of exposure will operate primarily through this payoff. 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 β^u than of β^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, whereas beliefs about social payoffs will be much more sensitive to exposure.

Proposition 2.iv indicates that updating also depends on the precision of the signals 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.

The framework we provide here illustrates how beliefs about universities and application decisions respond to exposure to past students. We show that exposure tends to encourage applications to a risky university by increasing the precision of students' beliefs about that 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.

6 RCT design

6.1 Mechanisms for the effects of breakthroughs

Our analysis of the quasi-experiments provided by breakthroughs indicates that changes do 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.

In the framework set out above, effects of exposure on applications imply 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 v_i, and uncertainty about the non-academic experience that they would have u_i—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 $Var(u_i) > Var(v_i)$: students are likely to be more uncertain and have a lower certainty equivalent about non-academic experience. If this is the case, students would tend to seek out information about the social environment when they get a chance to talk to a student who has attended the university, 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 the design of outreach programmes. 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 highlights the importance of the precision of a signal from another student (s_u^2, s_v^2)—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 in the conceptual framework. We measure proxies for $E[W(u_i)]$ and $E[W(v_i)]$ separately along with effects on applications; the treatment effects on these proxies indicate whether exposure affects applications by shifting academic or social beliefs. The different types of exposure provided vary the precision of signals s_u^2, s_v^2, and difference in effects of each of these treatments indicate how precise signals must be to affect beliefs and applications. Finally, variation in whether students and mentors are demographically matched lets us estimate whether exposure has larger effects when θ_i and θ_j are more similar.

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 plausible. Second, breakthroughs are by definition students from the same school, and students might update either about their own ability or the perception of students from their school by universities from enrolment decisions; 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 4.6—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.

6.2 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 D1 and in the text below. Figure 6 provides an overview of the design. More detail on each of the RCT procedures is provided in Appendix D.1.

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, but in some cases recruited by the school from their alumni. In each case, the presenter or presenters talked through a slide deck created by the research team that provided students with information about the university application process, and added their own commentary based on their experience of university applications and life at university. The slide deck is included in Appendix G.1.

The information discussed included background on the application process and how to put together a strong application, as well as statistics on the grades and earnings required for different universities. The information in the workshop was publicly available online, so an interested student could (at least in principle) find it themselves.[11] The workshop served to bring all experimental participants to a similar baseline of information on the application process and outcomes at different universities.

Videos. We asked students in our mentor pool to record videos about their university application

11. 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.

process and life at university. We provided students with a fixed list of topics to discuss, as detailed in Appendix D.1. Videos lasted 3–4 minutes, and were aimed at replicating the information that a student could learn about a university from a conversation with a past attendee. We introduced the videos as examples of students talking about their application process, and did not imply that the research team encouraged students to apply to that university or ask video speakers to promote their university.

Each student was assigned to two videos, one recorded by a male speaker and one by a female speaker. We grouped both participants and universities into academic tiers based on grades, and then assigned videos to participants from speakers at universities in the same academic tier. This meant that participants were matched with videos from universities that were aspirational but realistic given their grades. We assigned videos for all students using the same procedure, but only showed students videos if they were in the appropriate treatment arm; this allows us to measure beliefs about and applications to the assigned video university even in the control group. Videos were embedded in the baseline survey.

Mentors. We recruited 245 volunteer mentors from a variety of sources; the majority were drawn from STEM Ambassadors, a STEM-focused volunteering platform that we partnered with for recruitment. All mentors had attended a UK university for their undergraduate degree. 59% of mentors were current university students, while a minority were older; 76% of mentors were aged 18–25.

All participants were asked to name three universities from which they would like to receive a mentor when they completed the midline survey. We then assigned each student to up to two mentors, subject to capacity: one from one of the universities that they requested, and one from an academically matched university that they did *not* request, where we use the same procedure to academically match participating students and universities as we do for the video treatment. Both mentors are demographically matched for students in arms 2a and 2b (meaning they share at least one characteristic out of gender, ethnicity, and region of the UK with the mentor), and demographically unmatched in arms 1a and 1b (meaning they share no characteristics).

Students in the relevant treatment arms were then connected with their mentors via email and text messages, and were encouraged to schedule a video call with their mentor. We asked mentors to have at least one 15-minute call with each of their assigned mentees. 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. This treatment aimed to replicate an organic conversation with a past university attendee; the opportunity to choose topics and ask questions means that it provides a more precise signal of another student's payoffs at university to participants.

Subsidised visits. We provide some students with a subsidy of up to £75 ($100) for travel costs to a university they selected in the midline survey. All students are asked to report the university they would like a subsidy to visit in the midline survey, and students were informed of their treatment assignment after reporting this. We allowed students to choose a university to request a subsidy for, rather than assigning a university, to encourage takeup of the subsidy. Students generally used this subsidy to attend Open Days organised by universities for prospective applicants. An in-person visit provides an even more detailed signal of the payoffs from past students at a university, providing both opportunities for conversations with students and opportunities to see the campus and learn more experiential information about 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 D1 provides an overview of the treatment arms in each wave.

Students were randomly assigned to one of the treatment arms when they completed their first survey, either the baseline or the midline survey. In some cases, we were unable to get students to complete the baseline survey before the workshop, but we wanted to allow them to participate in the workshop and complete the remaining components of the programme. Since students were shown videos during the baseline survey, students 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 5 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 D6 provides counts of the numbers of students who completed different surveys and different treatments, and Appendix Table D7 presents an analysis of differential takeup of the mentorship treatment.

6.3 Outcomes

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

Beliefs are elicited in our surveys on Qualtrics; Appendix Figures F9–F11 provide screenshots of the belief elicitation portion of the survey. We ask students to report the percentage chance that they would (a) get an offer from, (b) make friends and fit in at, or (c) graduate successfully from a range of universities. Measuring beliefs about these binary outcomes is easier for participants to understand than trying to elicit a continuous distribution of payoffs in different dimensions. Reported probabilities closer to 50% as reflect higher uncertainty in beliefs.

At baseline, we ask students about these beliefs for a randomly selected one of the two universities that they were assigned a video for, for their top choice university, and for 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.

In the midline survey, we repeat the elicitation for these three universities, and also elicit beliefs for the universities for which the students were assigned mentors and the university for which they requested a visit. 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 F5–F7). 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 that subject. Stated applications in these surveys are used to construct intermediate application outcomes. We validate these outcomes by additionally collecting data on actual applications from schools; schools routinely record university applications for all students. 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 individual-level 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. We instead use them to provide descriptive evidence of the topics that students are interested in discussing when interacting with past university attendees.

6.4 Implementation

We implement the RCT at schools recruited to participate with the help of our partner organisations. Figure 7 illustrates the locations of these schools; we have broad coverage across different regions of England and different levels of university exposure across participating schools. Table 1 includes summary statistics for the RCT population as well as for the administrative data by comparison. As discussed in Section 2.3, participants in the RCT are disproportionately male and non-white relative to the average school student, but have typical levels of deprivation.

After school recruitment, students were first sent the baseline Qualtrics survey, and then all students were invited to the active control workshop. The baseline survey assigned participants to treatments and embedded the videos for treated students. Students completed the midline survey immediately after the workshop, and were then told their assignment to receive mentors and/or visits. Finally, after students had the opportunity to talk to mentors and use visits, they complete an endline survey and put in their final applications to universities.

Appendix Section D.2 provides further details on the recruitment procedures for schools and study timing. We now turn to the results from the RCT, and discuss the light that this sheds on application choices.

7 RCT results

7.1 Effects of video exposure on intended applications

We focus here on the effects of the video treatment on relevant outcomes, as we have not yet observed 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 6 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.[12]

Is this just driven by a short-run salience effect? Students completed the midline survey a median of 4 days after the baseline survey, so the students in question listed 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 6), we find a nearly identical point estimate of 0.052, although the estimate is no longer statistically significant ($p = 0.121$)—see Appendix Table E2.

We will be able to validate whether effects persist over a longer period when we collect endline survey data and final application outcomes. 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 ($\beta = 0.017, p = 0.309$). However, effects do persist for at least several days. This is consistent with an interpretation of the breakthrough results that attributes effects to exposure to past university attendees.

12. The much higher baseline application rate compared with the application rate to a given university in the administrative data reflects the fact that videos are tailored to students' ability, and the fact that the application rate is the share of students who apply to either one of the two assigned video universities.

7.2 Beliefs about academic success and social fit

We first describe differences in beliefs measured in the baseline survey. 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 which indicate that 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. Table 7 reports the effects of the video treatment on beliefs in the midline survey, controlling for baseline beliefs. There are no significant effects on beliefs about offer or graduation probability, either in mean levels or in the probability of updating more positively or negatively. By contrast, students in the video treatment group are 6 percentage points more likely to update their probability of fitting and making friends at the corresponding university positively between the two surveys ($p = 0.090$), and 10 percentage points less likely to update negatively ($p = 0.002$), than those in the control group. The mean posterior is 2.1 percentage points higher in the treatment group controlling for baseline beliefs, though this effect is not statistically significant ($p = 0.221$).

Appendix Figure E1 illustrates the full distributions of belief updates between the baseline and midline survey. We see a distinct rightward shift in the distribution of belief updates about fitting in and making friends for students who received the video treatment relative to those who didn't, shown in panel (b), with no effect on the probability of receiving an offer or graduating successfully. 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.

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 Appendix Table E3, 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 their social beliefs positively. 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 rather than 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.

7.3 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 gender-matched video was stronger.

Surprisingly, we find the opposite pattern: students consistently respond *more* to videos recorded by students with a *different* gender. Table 8 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 for which beliefs were elicited 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 4.4 we find a positive interaction effect of gender match between the breakthrough student and the induced student. Given that videos have most impact 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 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 we only observe applications, and it is possible that the latter effect dominates there.[13]

7.4 Information sought by students from mentors

As discussed in section 6, we did not prescribe topics for students to talk about with mentors. 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. They are incentivised in the sense that students need to actually ask mentors about a topic to learn relevant information. 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 9, 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 than other topics, but that mentors thought what students learned from this would affect their application choices. This is consistent with the results from Section 7.2 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

13. 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: 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 E1).

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.

8 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 students 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 induced 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 important for students' decision-making. At least in this context, information and beliefs about the social environment—an aspect of choice students have less baseline information about—therefore seem to be the primary mechanism for the effects of exposure on applications.

These results have implications for strategies 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—particularly social concerns—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 of this analysis; an important avenue for future research is to develop a fuller understanding of how intensive and personalised feedback needs to be to have an effect.

From the government's perspective, findings like those in this paper suggest that policies such as 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.

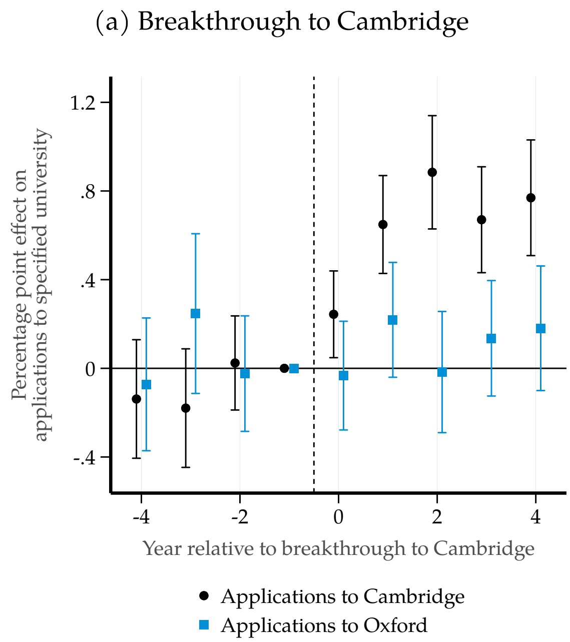
9 Exhibits

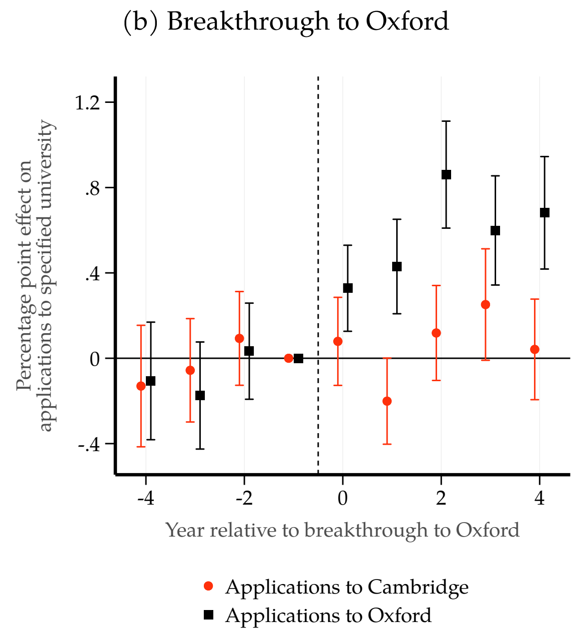
Table 1: Summary statistics

	(1) Age-18 school leavers	(2) University applicants	(3) Event study sample	(4) 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	805

Notes: Summary statistics comparing outcomes in different subsets of the LEO data and the RCT.
Data: LEO (columns 1–3), RCT sample (column 4).
Samples: Age-18 school leavers are all students who attend school to age 18 – we exclude anyone who leaves full-time education before this age. University applicants are students who appear in the university applications dataset and apply to at least one university that is linked to university enrollments data. The event study sample consists of students 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 analyses 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.

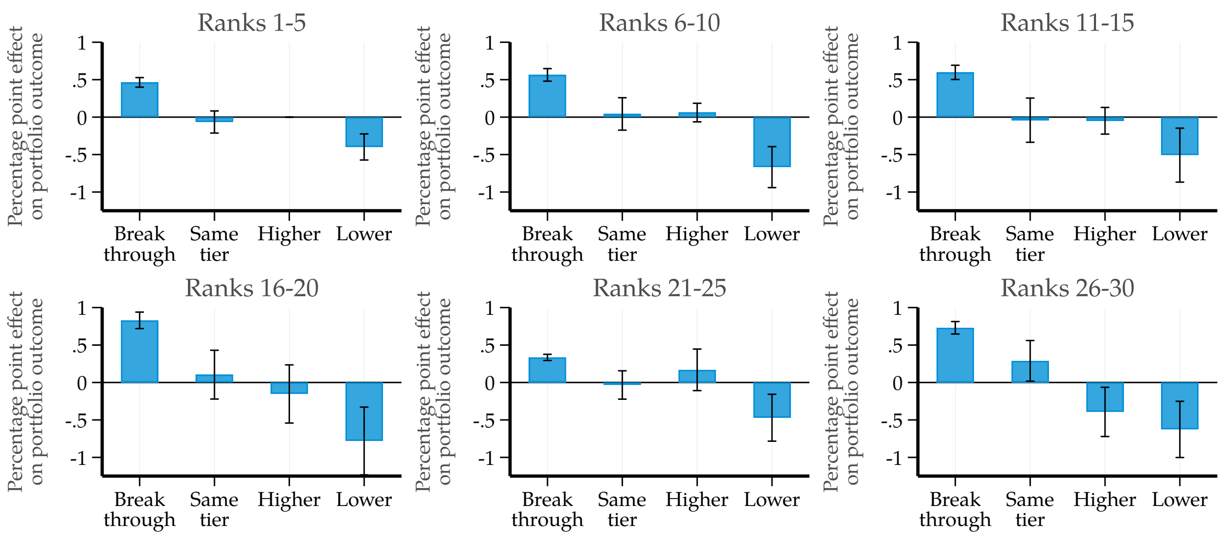
Figure 1: Event study of applications to Cambridge and Oxford following a breakthrough to each university





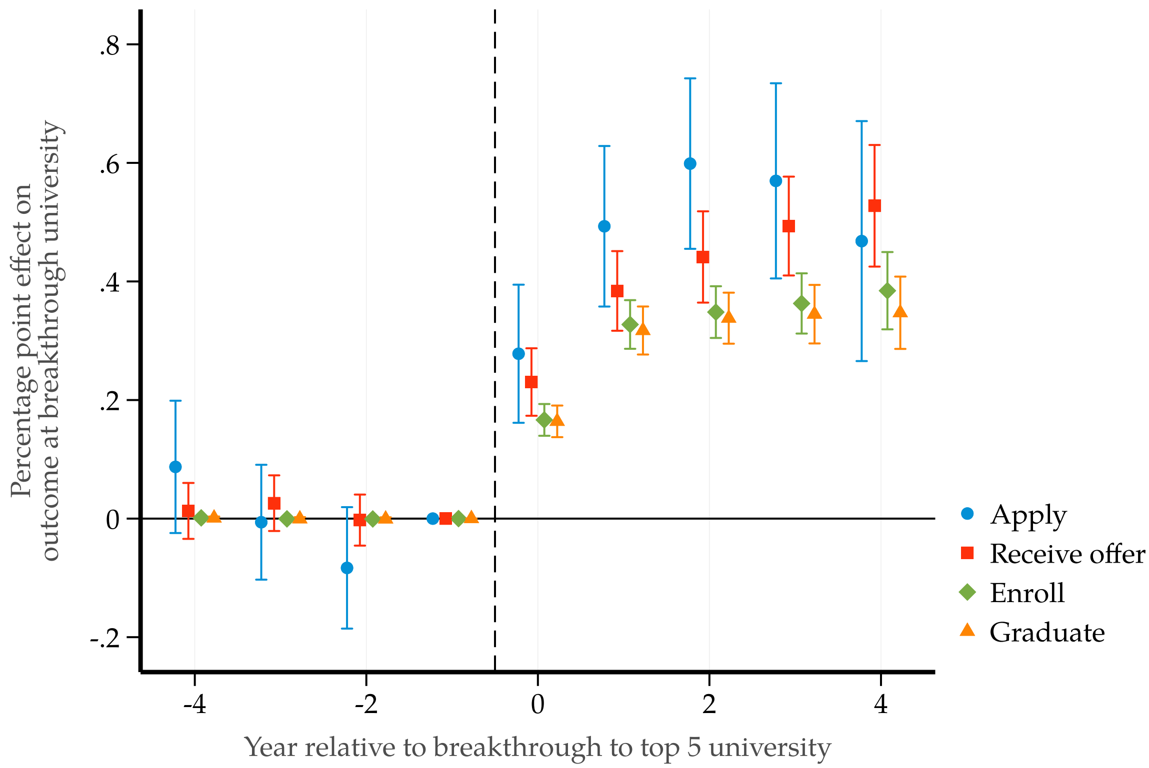
Notes: Data: UCAS. Coefficients from an event study of breakthroughs to the specified university (equation 1) using the Sun and Abraham estimator, where the outcome is applications to Cambridge or to Oxford. Coefficients are multiplied by 100 so they can be interpreted in percentage point terms. Regressions 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 2: 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 application portfolio outcomes (equation 4). 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. Universities are ranked by the mean A-level grades of students enrolled at the university. The four outcomes are mutually exclusive and exhaustive, so coefficients mechanically sum to zero in each sub-plots. 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 3: 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 (equation 2), using the Sun and Abraham estimator. Outcomes are indicators for applying to the university, 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 effects on 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

Outcome variable	(1) No ability controls	(2) 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)

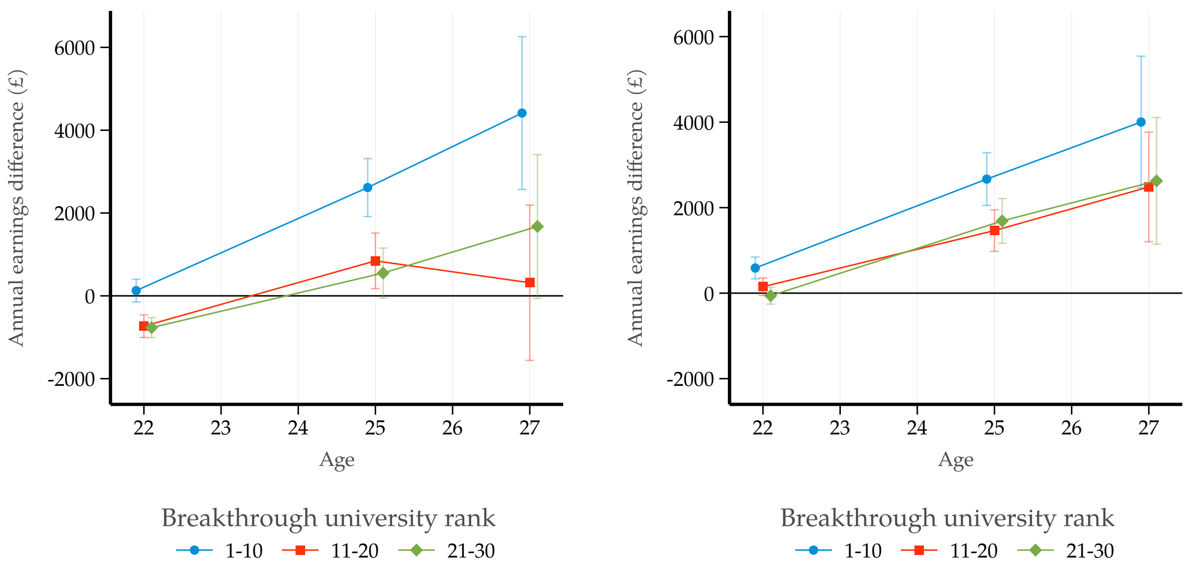
Standard errors in parentheses

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

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 4: 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 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	(1) Complier mean	(2) Mean for enrollees at breakthrough universities	(3) Event study sample mean	(4) Event study treated schools mean
	Panel A: Breakthroughs to universities ranked 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: Breakthroughs to universities ranked 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: Breakthroughs to universities ranked 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 (5); other characteristics are raw means within the specified sample. 'FSM eligible' denotes eligibility for Free School Meals. 'Low-income' denotes students in the poorest two quintiles of our neighbourhood deprivation measure. GCSE percentiles are average percentiles of age-16 test scores estimated separately by year. Taking $\geq$ 3 A-levels is the standard qualification for academic-track students.

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)

	(1) Apply to breakthrough uni.
Post × Treated	-0.000345
	(0.00029)
Post × Treated × Num shared chars.	0.00184***
	(0.00009)
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

	(1) Income	(2) Ethnicity	(3) FSM	(4) Gender	(5) Age-16 school
Post × Treated	0.00403***	0.00455***	0.00403***	0.00411***	0.00284***
	(0.00022)	(0.00027)	(0.00023)	(0.00020)	(0.00020)
Post × Treated × Shared	0.00212***	0.00134***	0.00215***	0.00243***	0.00507***
	(0.00020)	(0.00030)	(0.00024)	(0.00018)	(0.00027)
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

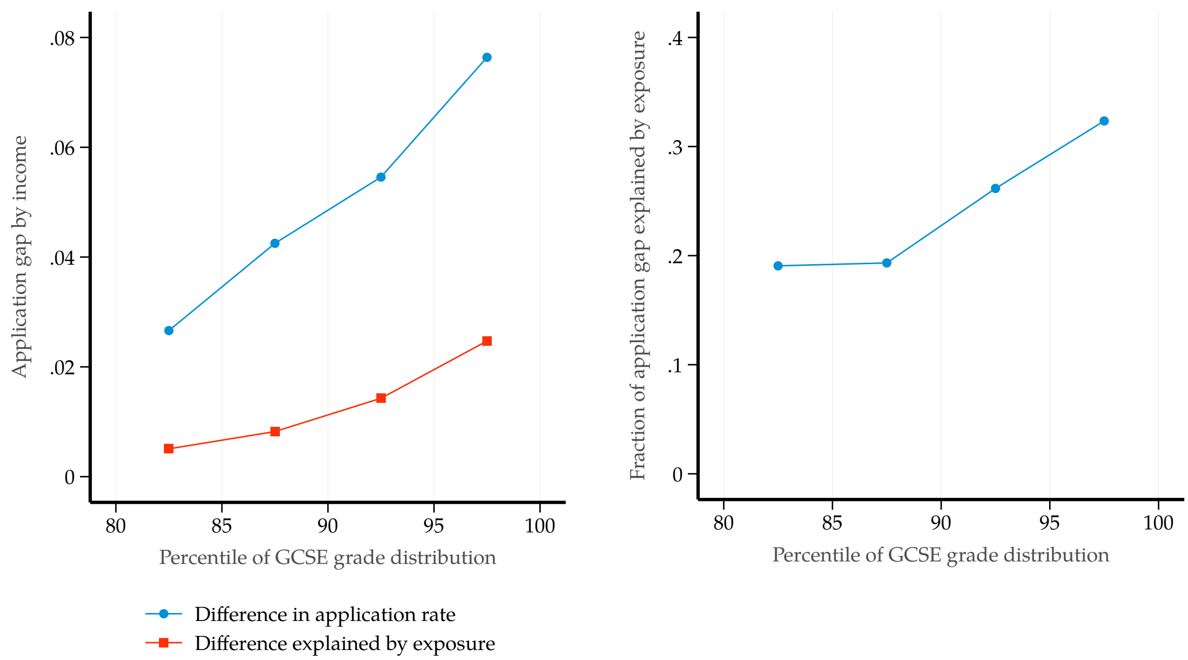
Standard errors in parentheses

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

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 the Treated × Post 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 Treated × Post indicator with indicators for sharing each individual characteristic with the breakthrough student. Difference-in-difference regressions are pooled across the top 30 universities. Regressions include school-by-breakthrough-university and year-by-breakthrough-university fixed effects. Standard errors clustered at the school level.

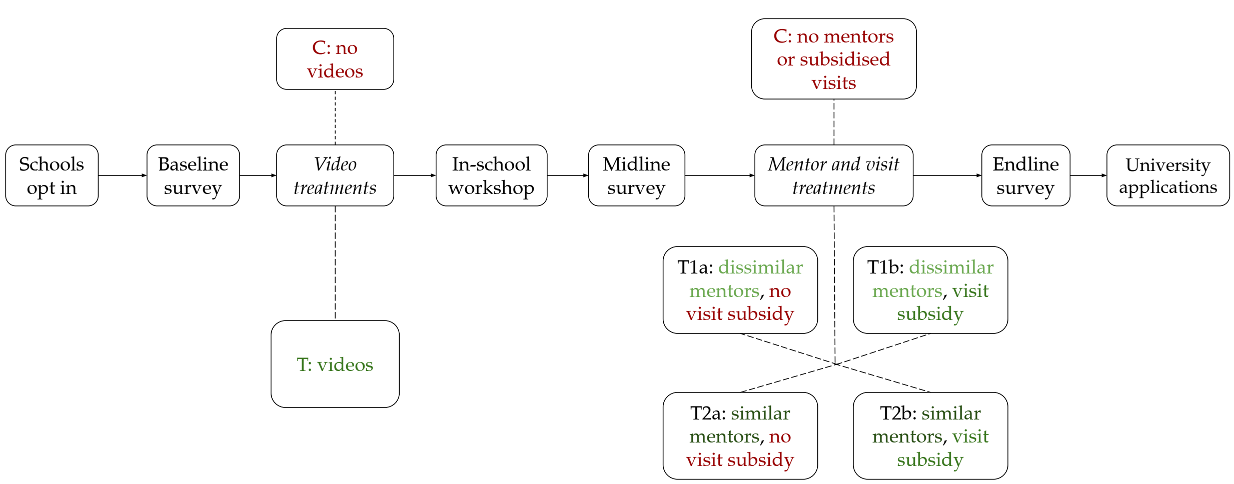
Figure 5: Application gaps between low-income students and other students that are explained by differences in exposure given treatment effects

(a) Total application gaps (b) Share of application gap explained by exposure



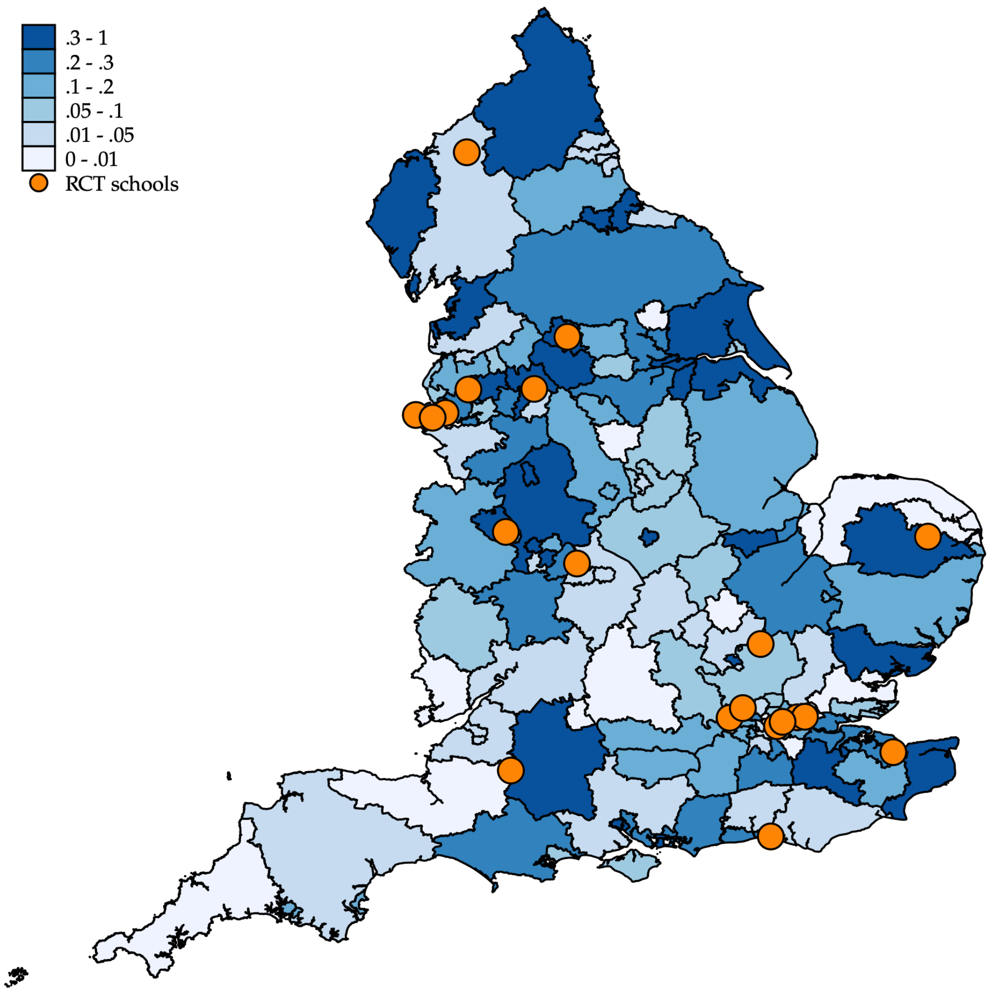
Notes: Difference in application rate to top 10 universities between low-income (bottom 40% of neighbourhood deprivation index) and higher-income students (all other students), and the amount of this that can be explained by exposure. Panel (a) plots overall application gaps, and the gap that can be explained by exposure as estimated in equation 8. Panel (b) simply divides the difference explained by exposure by the total difference at each GCSE ventile to illustrate the percentage effect.

Figure 6: Overview of RCT design



Notes: Diagram outlining stages of the RCT and treatment arms, as implemented in the Spring 2025 wave.

Figure 7: 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 C3c, 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. One participating school in Wales is omitted from the map.

Table 5: RCT balance table

Variable	(1) Control mean	(2) Treatment	(3) 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 ≥ 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. Column (1) reports the control group mean and column (2) 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 6: Treatment effects of video on intended applications to video university

	(1) Apply midline	(2) Apply midline	(3) 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

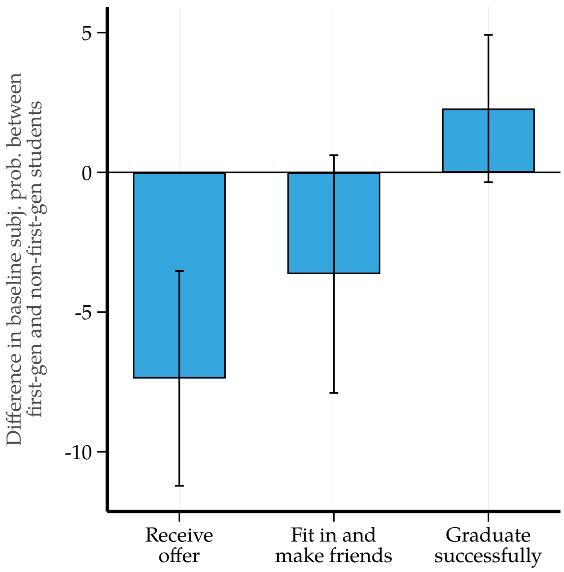
Standard errors in parentheses

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

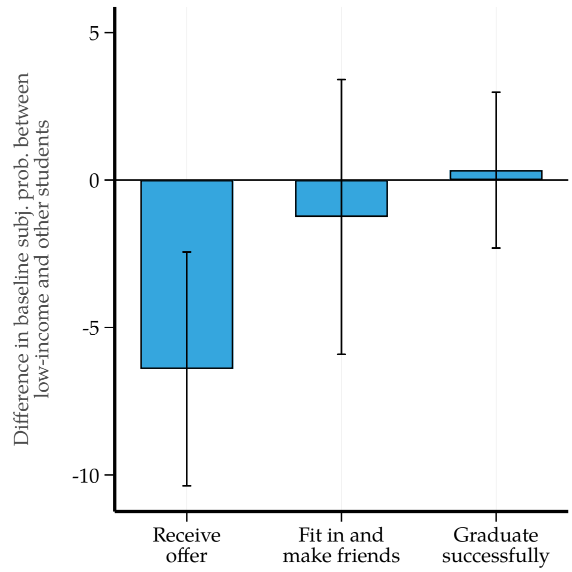
Notes: Data: RCT. Sample restricted to students who completed both the baseline and the midline survey. Estimates of treatment effect of videos on intended applications. Students are assigned two videos about different universities regardless of their treatment status. The outcome in the regression is 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), along with the specified controls. Standard errors are robust to heteroskedasticity.

Figure 8: Beliefs about outcomes at Oxford University, by social disadvantage indicators

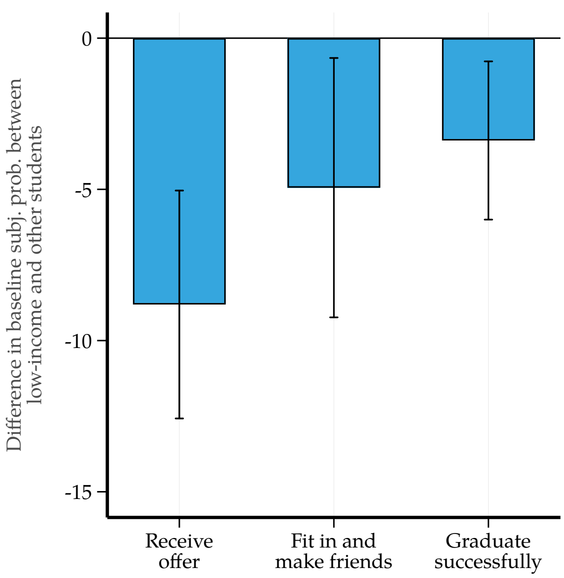
(a) Coefficient on first-generation indicator



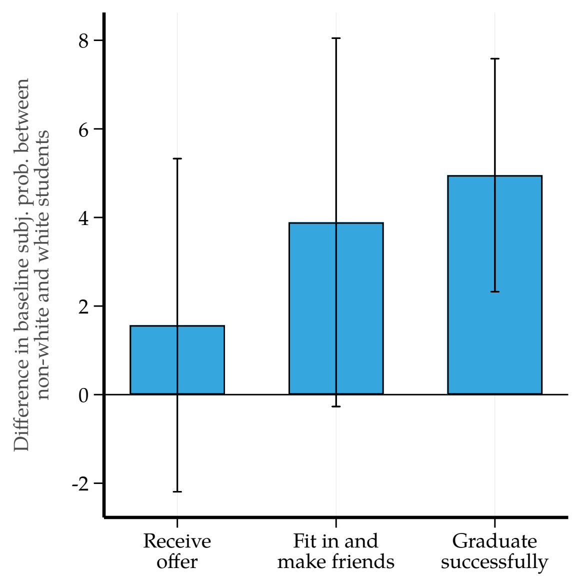
(b) Coefficient on low-income indicator



(c) Coefficient on female



(d) Coefficient on non-white indicator



Notes: Data: RCT. Sample restricted to students who completed both baseline and midline surveys. 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 grades, and report the coefficient on the indicator. Low-income students are those whose home postcode is in the bottom 40% of neighbourhood deprivation. 90% confidence intervals are reported based on heteroskedasticity-robust standard errors.

Table 7: 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. Sample restricted to students who completed both baseline and midline surveys. Treatment effects on mean belief updates and on the direction of belief updating between the baseline and midline survey, controlling for baseline beliefs. The video treatment indicator is an indicator for being assigned to any of the arms that receive video treatments in the baseline survey, pooling across arms T1a, T1b, T2a and T2b. Standard errors are robust to heteroskedasticity.

Table 8: 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: Data: RCT. 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. Sample restricted to students who completed both the baseline and the midline survey. 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 9: 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 factor 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.

University tuition for domestic students is capped by the government. As almost all universities charge tuition at the cap, this means that there is no variation in tuition costs for students between universities. 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. Thresholds, repayment rates, and write-off periods have changed over time, but the essential income-contingent structure of loans has been in place since reforms to tuition fees in 2012. 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. Prior regimes

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 C4, 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.

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} × 1{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} × 1{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} × 1{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} × 1{Treated}	0	0	0	0	0	0
	(0)	(0)	(0)	(0)	(0)	(0)
1{t= 0} × 1{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} × 1{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} × 1{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} × 1{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} × 1{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	≤ 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.

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 Patterns of university access and returns to university

C.1 Earnings effects of graduating from selective universities

From an economic perspective, the primary reason to care about access to elite universities is that the university a student attends is consequential for their labour market outcomes. Since our administrative data link university applications and attendance to earnings, test scores, and demographics for a very large sample of students, we can precisely estimate earnings for graduates of individual 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 grades of students who are enrolled at each university.

We first plot the age profile of earnings for graduates from different universities from 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.[14] Figure C1a illustrates how mean annual earnings change over time for students with different university outcomes. University attendees earn less than non-attendees until age 22, but start to earn substantially more than non-attendees 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. [15] Figure C5 illustrates the distribution of earnings at age 27 by university attended, showing that the distribution for top-10 graduates lies to the right of that of earnings for graduates of lower-ranked universities.

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

$$Y_{itu} = \alpha + \beta X_i + \theta_u + \delta_t + \varepsilon_{itu} \tag{27}$$

where X_i is a vector of individual-level observables, θ_u is a university fixed effect, and δ_t a year fixed effect. The omitted university dummy is the modal university in the sample.

We plot the fixed effect for each university θ_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

14. See appendix B.1 for an explanation of the precise earnings outcome used. Figure C4 presents earnings age profiles that include individuals with 0 earnings.

15. 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.

θ_u and $\mu_\theta, \sigma^2_\theta$ be the mean and variance of the estimated θ_u coefficients across universities; then

$$\theta^*_u = \frac{\sigma^2_\theta}{\sigma^2_\theta + s^2_u}\theta_u + \frac{s^2_u}{\sigma^2_\theta + s^2_u}\mu_\theta \tag{28}$$

$$s^*_u = \sqrt{\frac{\sigma^2_\theta}{\sigma^2_\theta + s^2_u}}s_u \tag{29}$$

While we cannot identify universities by name,[16] 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 C1b plots the results of this analysis. Specification 1 includes no controls in X_i and thus reflects only the raw differences in earnings across universities after applying empirical Bayes shrinkage. Specification 2 controls for observables: demographics, test scores, and major. 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 portfolios proxy for unobserved components of ability. If we could assume that university choice conditional on an offer portfolio is independent of ability, as argued in Dale and Krueger (2002, 2014) and Mountjoy and Hickman (2021), these estimates could be interpreted as causal effects on earnings. However, this is a strong assumption in this context, so we instead interpret offer portfolios as another observable that can help us absorb more variation in ability.

Figure C1c plots the coefficients for 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 university) after shrinkage. We see that most of the top 10–20 universities have distinctly higher returns than the median university – 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. 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 are likely to perpetuate income inequality.

C.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

16. See Britton et al. (2022) for analysis of 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.

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[17] can be decomposed as

$$\begin{aligned} P(\text{enroll}) =& P(\text{apply}) \\ & \times P(\text{receive offer} \mid \text{apply}) \\ & \times P(\text{accept offer} \mid \text{receive offer}) \\ & \times P(\text{enroll} \mid \text{accept offer}) \end{aligned} \tag{30}$$

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 (30) produces an additive decomposition in terms of log points; we can also predict the enrollment rate for low-income 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 enrollment. Figure C2 presents this decomposition conditional on students' ability, as measured by their percentile in the national GCSE distribution. We focus on the probability of enrollment in 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 application 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

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

similar test scores, and a significant fraction of the disparity stems from differing application rates. As we saw above, these universities have higher earnings returns, so these disparities are likely to perpetuate inequality, leaving low-income students with less access to the top universities.

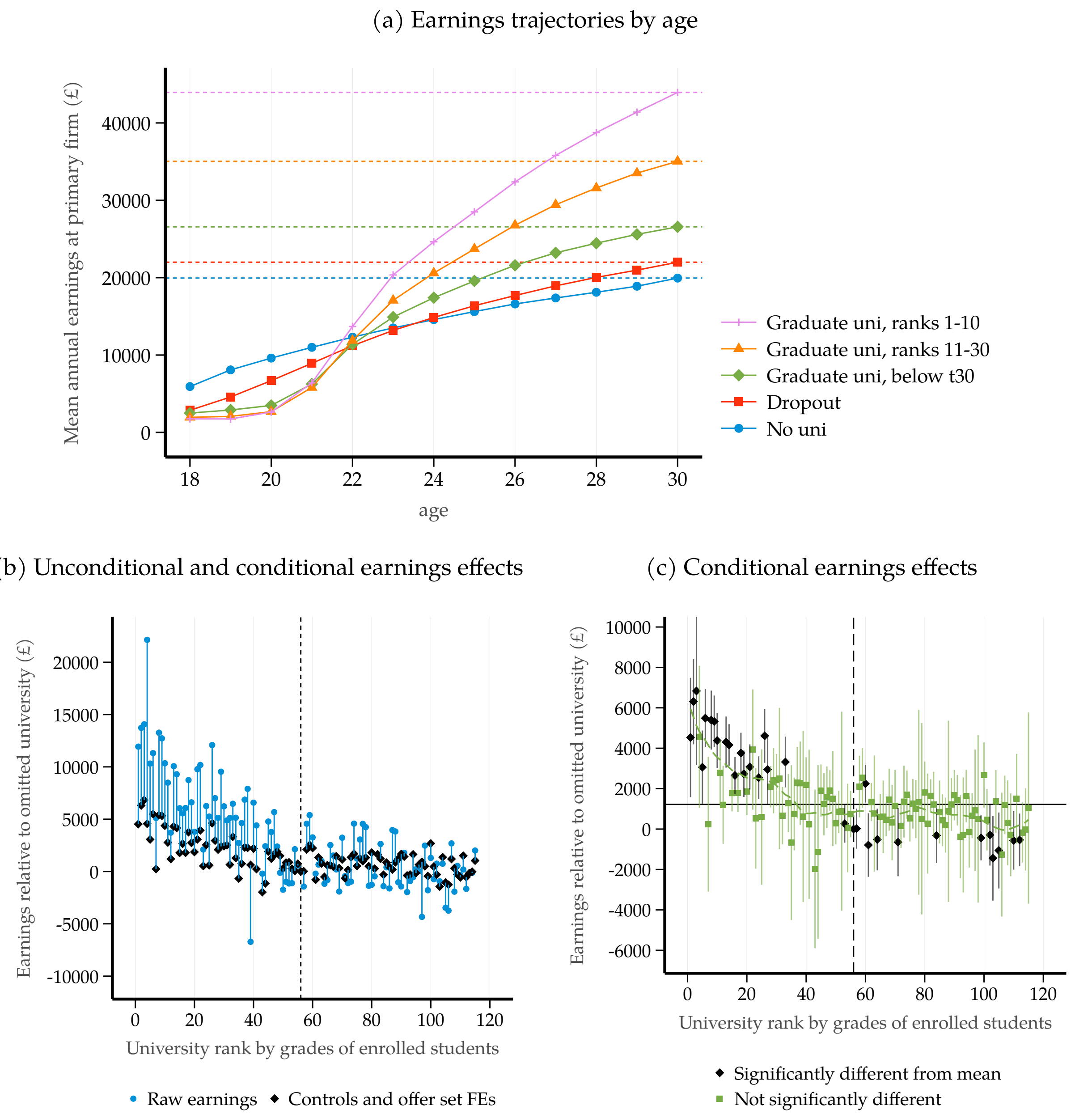
C.3 Income disparities in university destinations of past school cohorts

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 C3a 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 C3b plots the same differences conditional on test scores and other demographics,[18] showing that a 10 percentage point discrepancy remains after adjusting for these differences. Figure C3c 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 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 3.1 and 4, which we proceed to next.

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

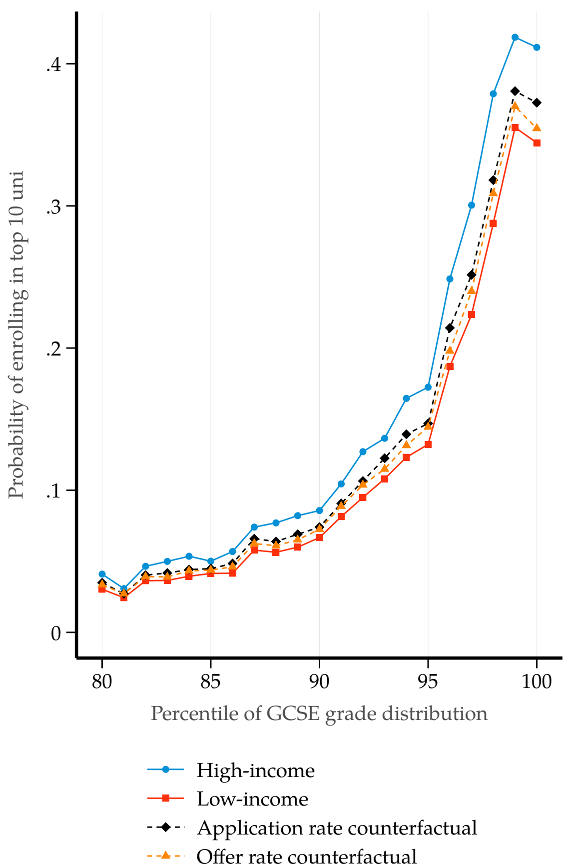
Figure C1: Earnings returns to universities at age 27 across the distribution of university academic selectivity



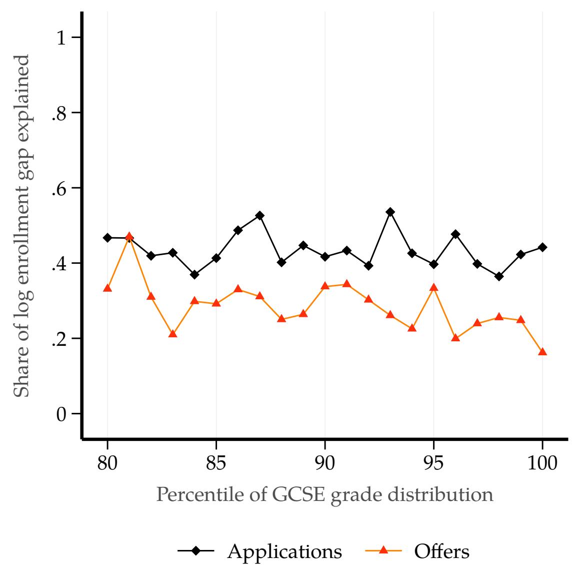
Notes: Estimates of the earnings return to different universities. Data: LEO. All earnings are in GBP and adjusted to inflation in 2018. Panel (a) plots mean earnings conditional on age and university outcome, and on positive earnings. We restrict to students starting a degree at age 18 for dropouts 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 after applying empirical Bayes shrinkage clustered at the high school level). Estimates that are significantly different from the mean at the 5% level are highlighted in black.

Figure C2: Decomposition of gaps in attendance at top 10 universities

(a) Actual and counterfactual enrollment rates



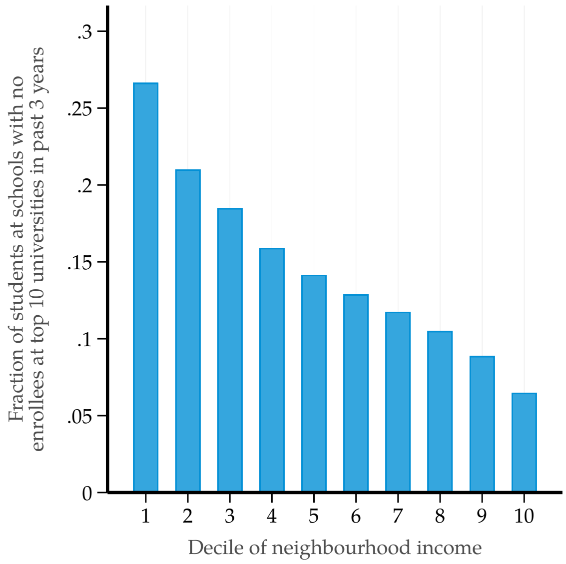
(b) Share of enrollment gap explained



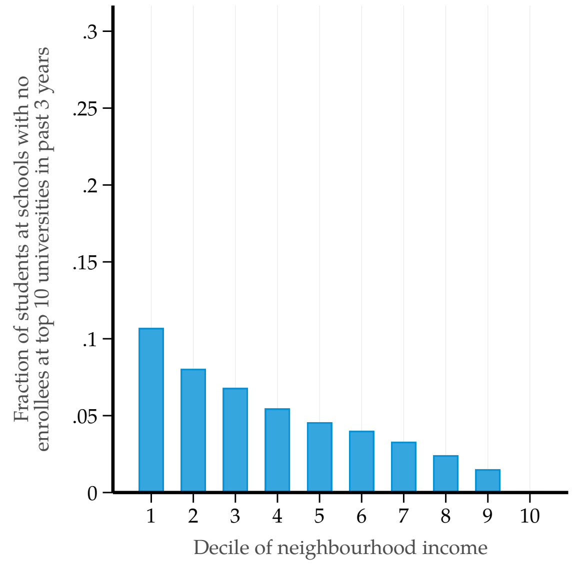
Notes: Data: LEO. In panel (a), solid lines plot the probability of enrolling at one of the top 10 universities conditional on percentile in the national GCSE grade distribution (truncating at 80 since applications to top universities are very low below this rate), and conditional on enrolling through 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 C3: Share of students attending schools that sent no-one to a top 10 university in the preceding three years, by neighbourhood income

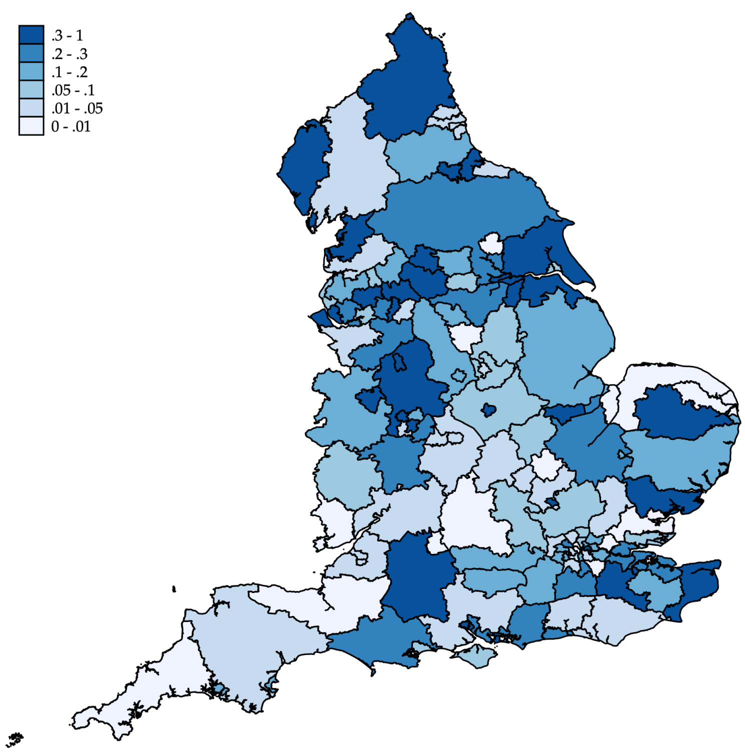
(a) Unconditional non-exposure rate



(b) Conditional non-exposure rate

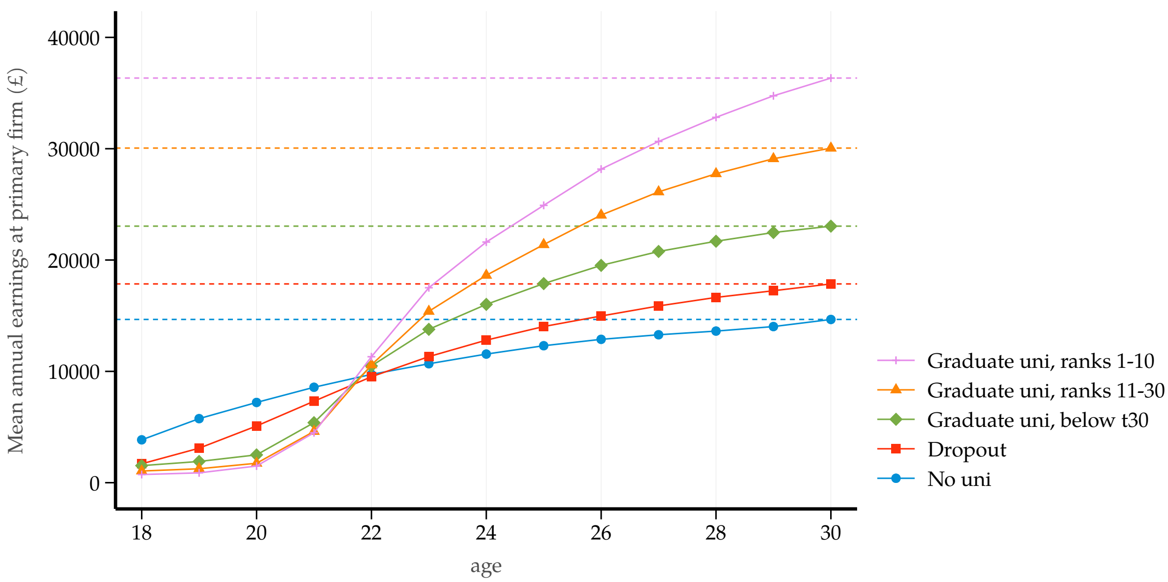


(c) Unconditional non-exposure rate by region of England



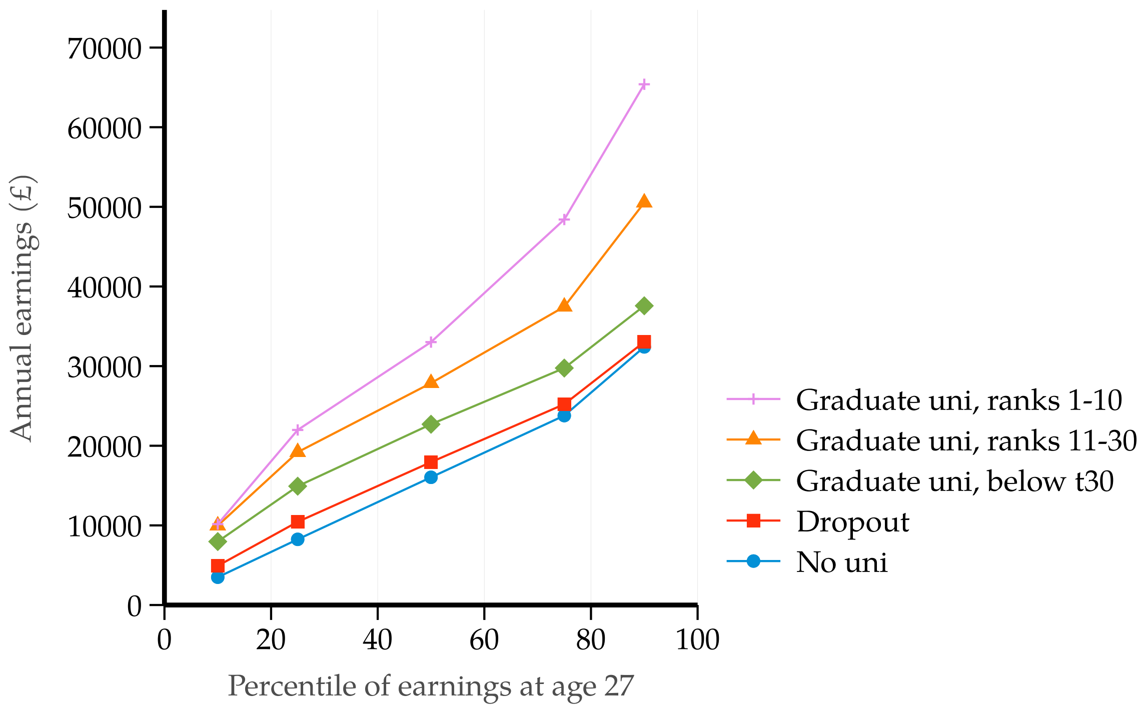
Notes: Data: LEO. Unconditional specification in panel (a) reports the share of students attending high schools that sent no-one to a top 10 university in the preceding three years, by decile of the student's neighbourhood income; lower deciles are more deprived. Conditional specification in panel (b) 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 C4: 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 C5: 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.

D RCT design

D.1 Details of treatments

Active control workshop

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 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 G3–G10 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 available to all students rather than just the treatment group was important for school recruitment, as schools would have been less enthusiastic about a programme in which a large fraction of their students would get no benefit. 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 G19).

Videos

Students in the relevant treatment groups were shown two videos towards the end of the baseline survey. Appendix Figure F15 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 F14, 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 four 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 D3 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 D3 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 D5 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 in 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.[19] 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 D.3.

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

For some students, we provide a travel subsidy for visits to a university, motivated by discussions with students at pilot schools in which they talked about 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

19. 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.

D.2 Implementation details

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 participation 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 be likely to 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.[20]

Figure 7 illustrates the geographic distribution of schools in our sample, overlaid on the map from Figure C3c illustrating the local probability of not being exposed to a top 10 university.[21] We have broad coverage across different regions of England, including several schools in the relatively deprived north of England and in areas with low university exposure.

Table 1 included summary statistics for the RCT sample alongside summary statistics from the administrative data. To better understand how characteristics of schools participating in the RCT compare to the general population of schools, Appendix Table D4 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 D4 is restricted to university applicants, but this restriction holds

20. 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 than that of other participating schools.

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

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.

We conducted the study in two waves, working with different schools and sets of students in each. Table D2 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 liaised 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 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 survey. 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. Thus the midline survey was completed before students knew their assignments to mentors and visits.

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.

D.3 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 D3). 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.

D.4 Supplemental tables

Table D1: 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.

Table D2: 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.

Table D3: 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 D4: 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 D5: 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
N	245

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

Table D6: Student counts in experiment

	Fall 2024	Spring 2025	Total
Baseline survey	176	1275	1451
Midline survey	106	841	947
Baseline and midline survey	93	712	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	35	35

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

Table D7: Takeup of mentorship treatments by demographics

	(1)	(2)	(3)	(4)
	Called mentor	Called mentor	Any contact with mentor	Any contact with mentor
Female	0.0392	0.0251	0.0172	-0.00731
	(0.0448)	(0.0444)	(0.0505)	(0.0496)
Parent	0.118***	0.113**	0.113**	0.108**
attended uni.	(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.

E RCT results

Table E1: Effects of shared characteristics on probability of two individuals being friends

	(1) Student pair are friends (OLS)	(2) 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 E2: 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

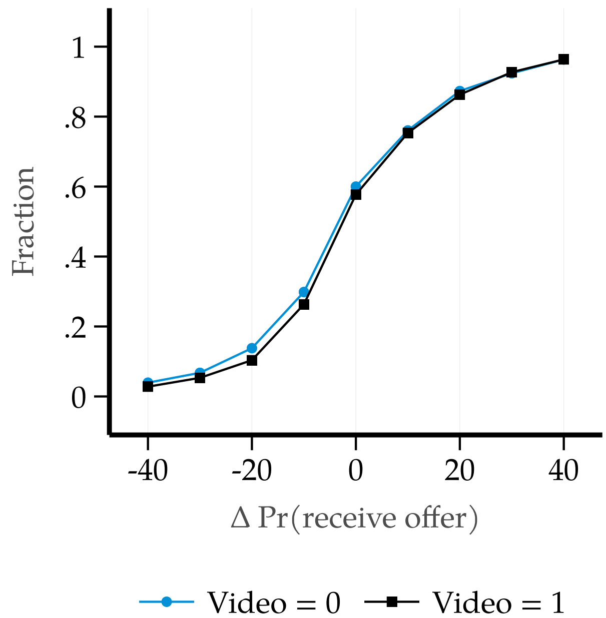
Standard errors in parentheses

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

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).

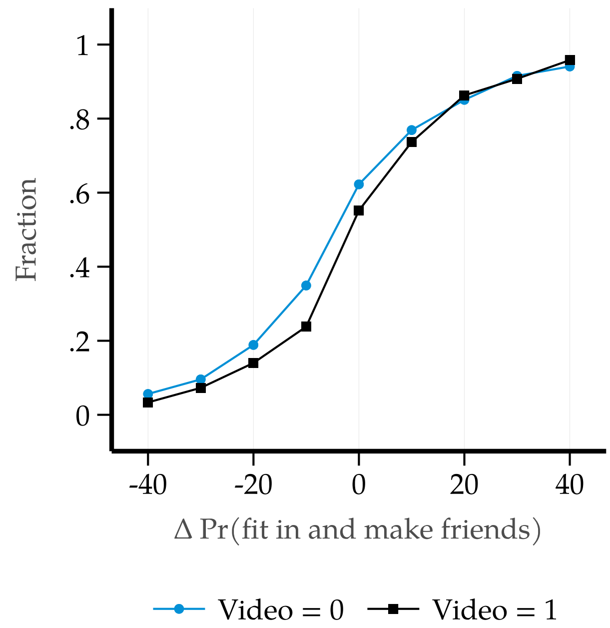
Figure E1: Distribution of belief updates about video university between baseline and midline survey by video treatment status

(a) Probability of receiving an offer



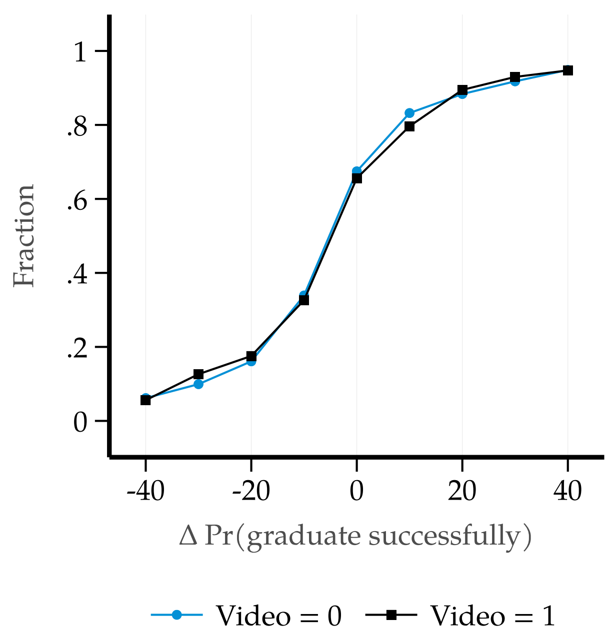
Mean update: b=1.80 (p=0.266)
Kolmogorov-Smirnov test: p=0.971

(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 E3: 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 ≤ 0	0.00139	
	(0.0311)	
Video treatment × social update ≤ 0	-0.0219	
	(0.0446)	
Offer update ≤ 0		-0.0135
		(0.0326)
Video treatment × offer update ≤ 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

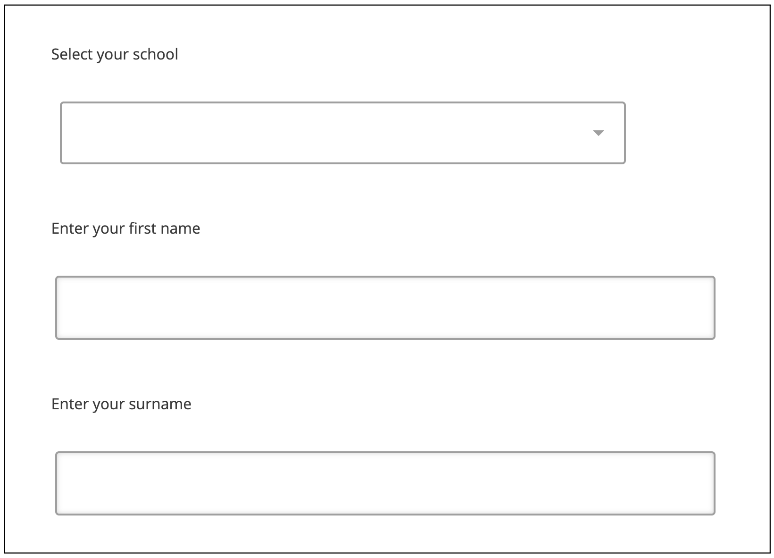
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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.

F Survey materials

F.1 Baseline survey

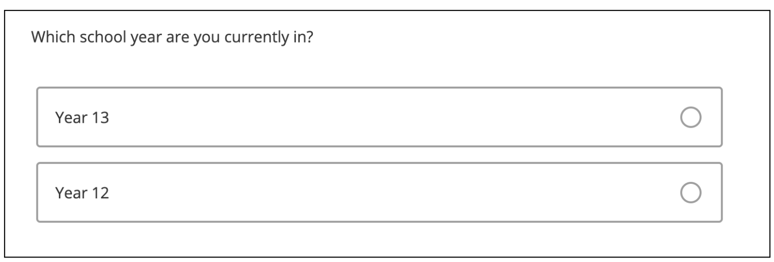
Figure F1: Student name and school


Select your school

Enter your first name

Enter your surname

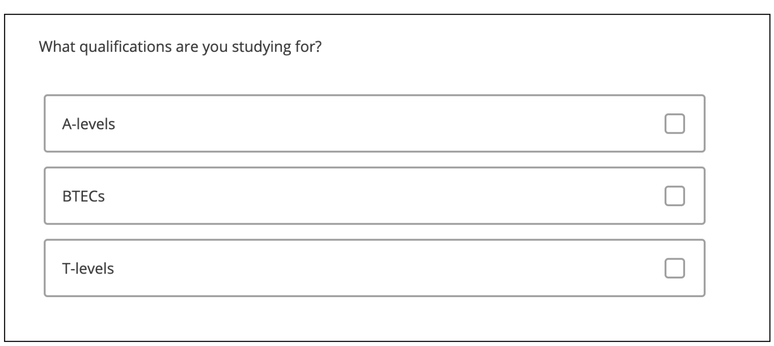
Figure F2: Student academic year


Which school year are you currently in?

Year 13

Year 12

Figure F3: Student qualifications

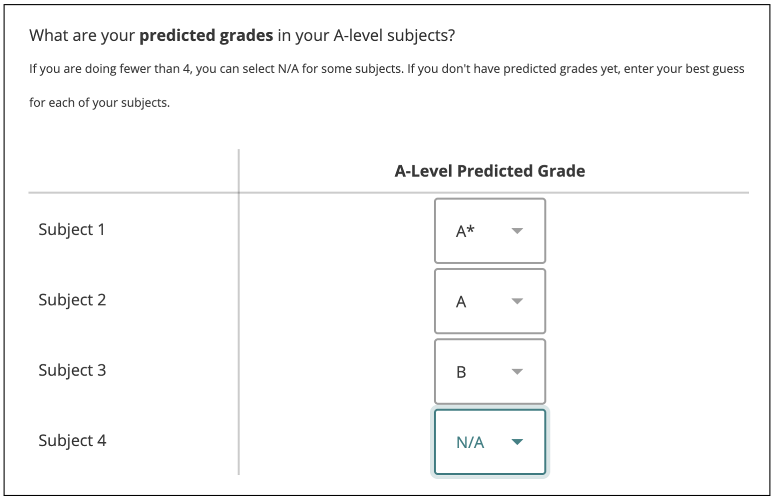

What qualifications are you studying for?

A-levels

BTECs

T-levels

Figure F4: Student predicted grades

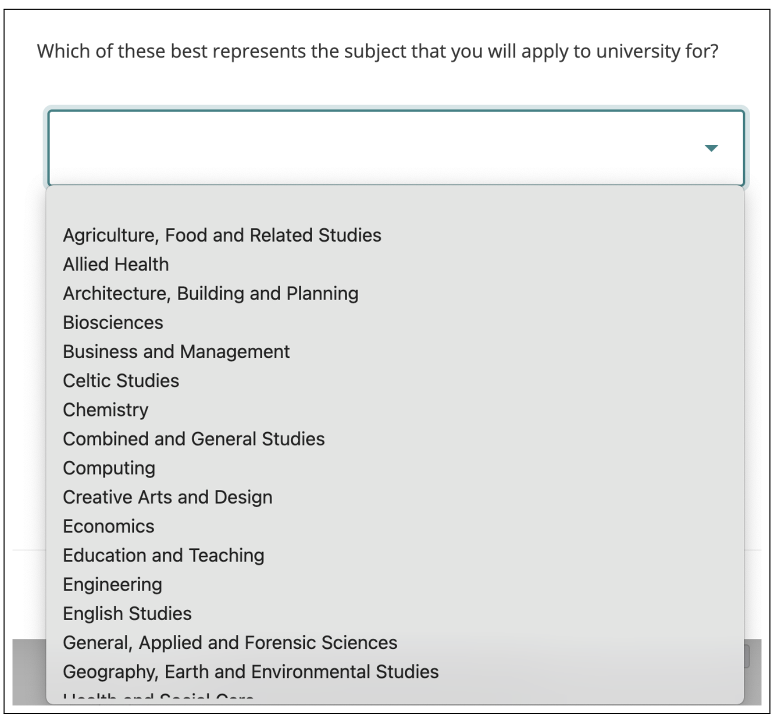


What are your **predicted grades** in your A-level subjects?

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 F5: 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 F6: 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 F7: 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 F8: University choice motivation (open-text)

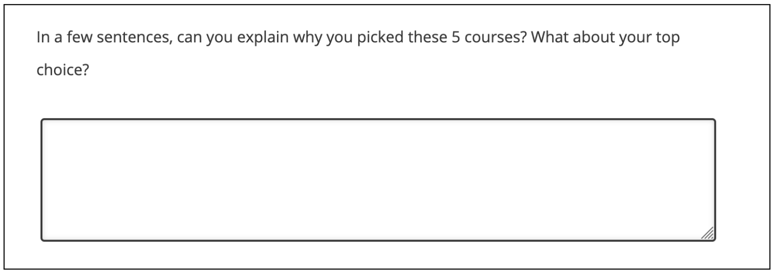
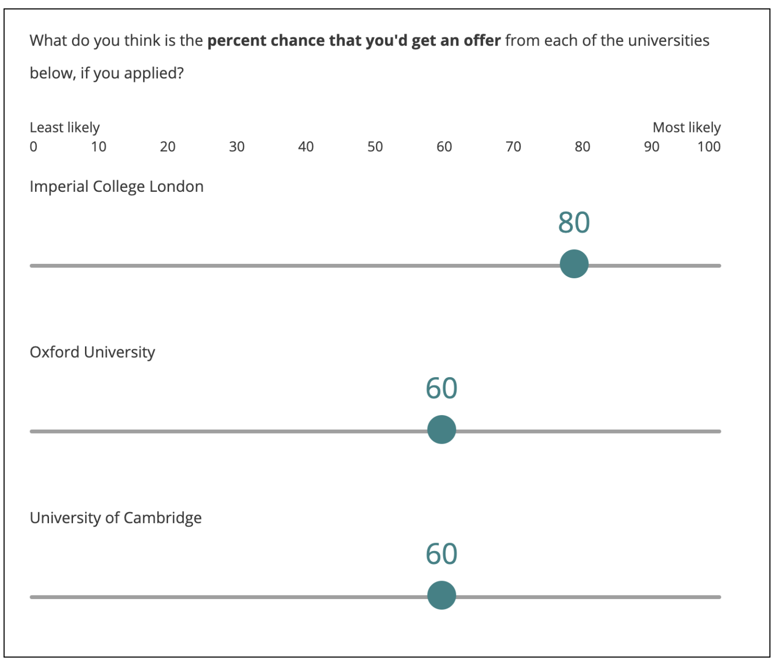

In a few sentences, can you explain why you picked these 5 courses? What about your top choice?

Figure F9: 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

Imperial College London

80

Oxford University

60

University of Cambridge

60

Figure F10: Social fit beliefs

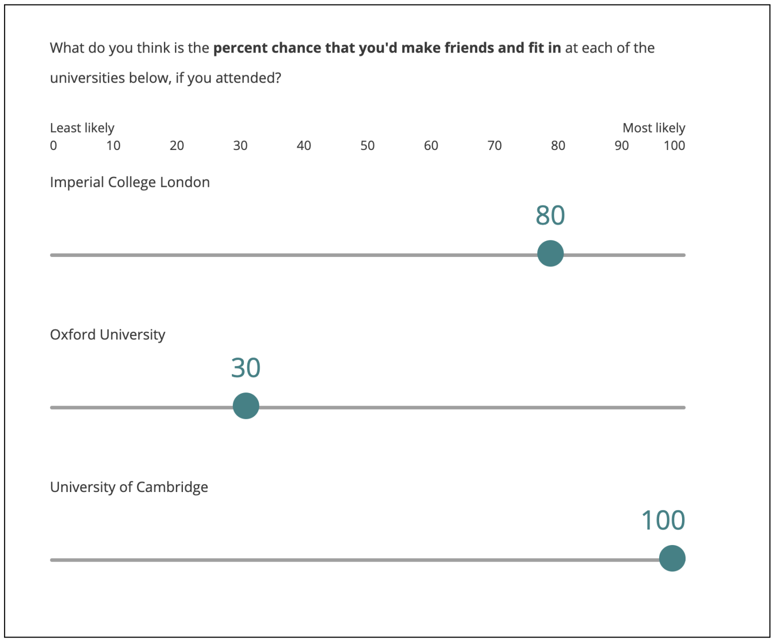


Figure F11: Graduation beliefs

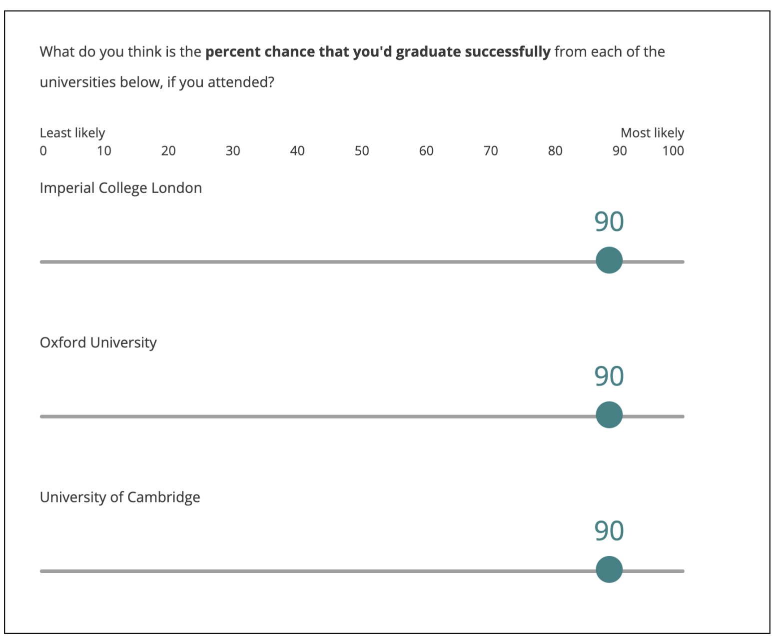


Figure F12: Typical grades beliefs

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 F13: 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 F14: 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 F15: Video page

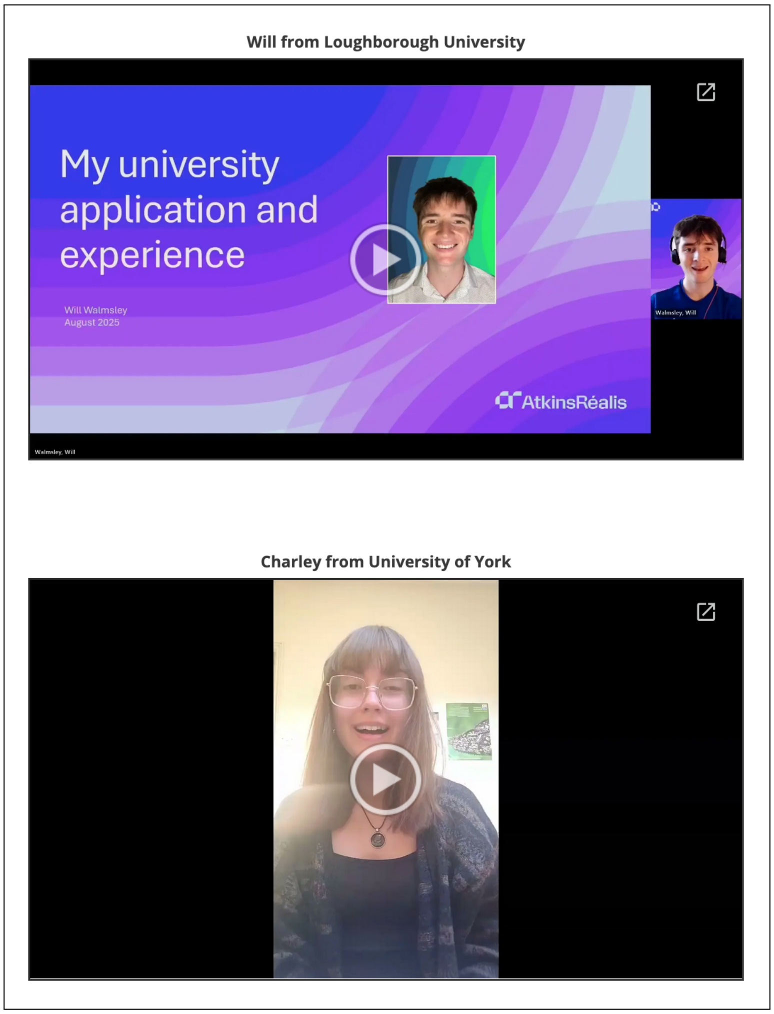


Figure F16: Video text responses

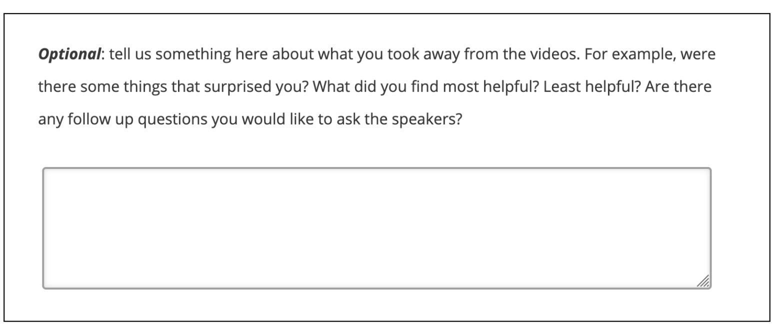

Optional: tell us something here about what you took away from the videos. For example, were there some things that surprised you? What did you find most helpful? Least helpful? Are there any follow up questions you would like to ask the speakers?

Figure F17: Re-elicitation of admission beliefs after video

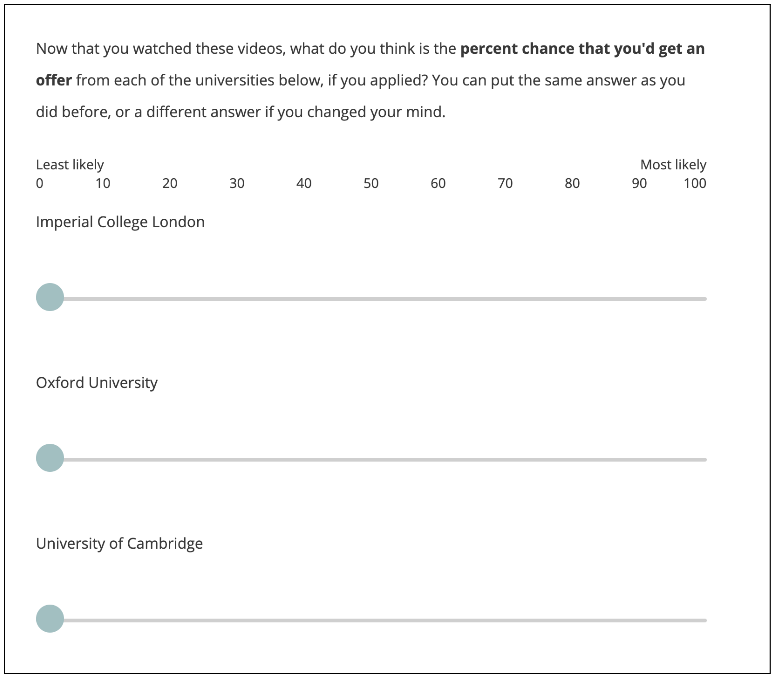


Figure F18: Re-elicitation of social beliefs after video

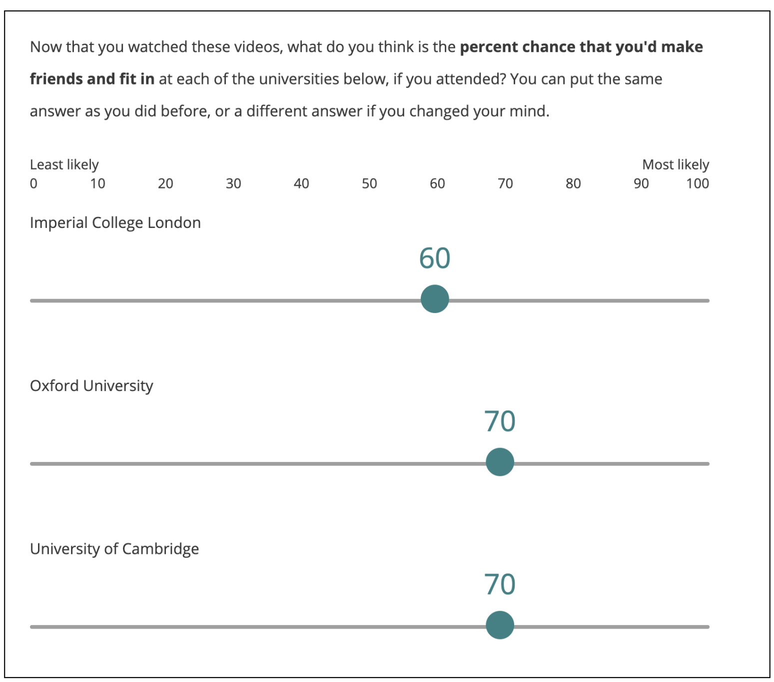
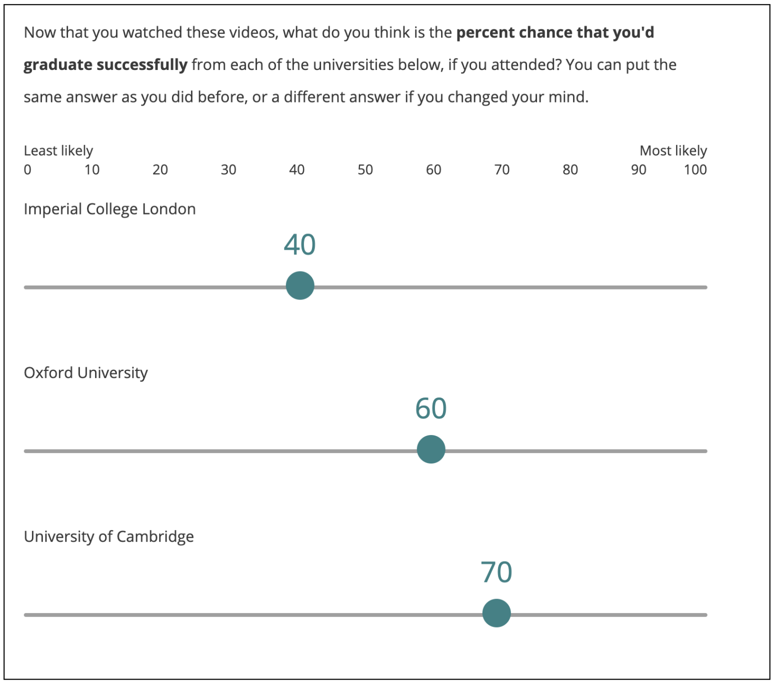


Figure F19: Re-elicitation of graduation beliefs after video


Now that you watched these videos, what do you think is the **percent chance that you'd graduate successfully** from each of the universities below, if you attended? You can put the same answer as you did before, or a different answer if you changed your mind.

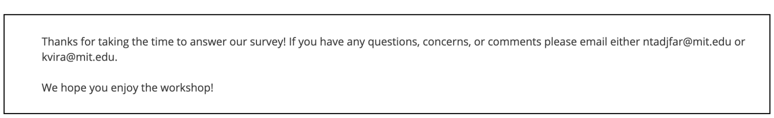
Least likely | Most likely
0 10 20 30 40 50 60 70 80 90 100

Imperial College London
40

Oxford University
60

University of Cambridge
70

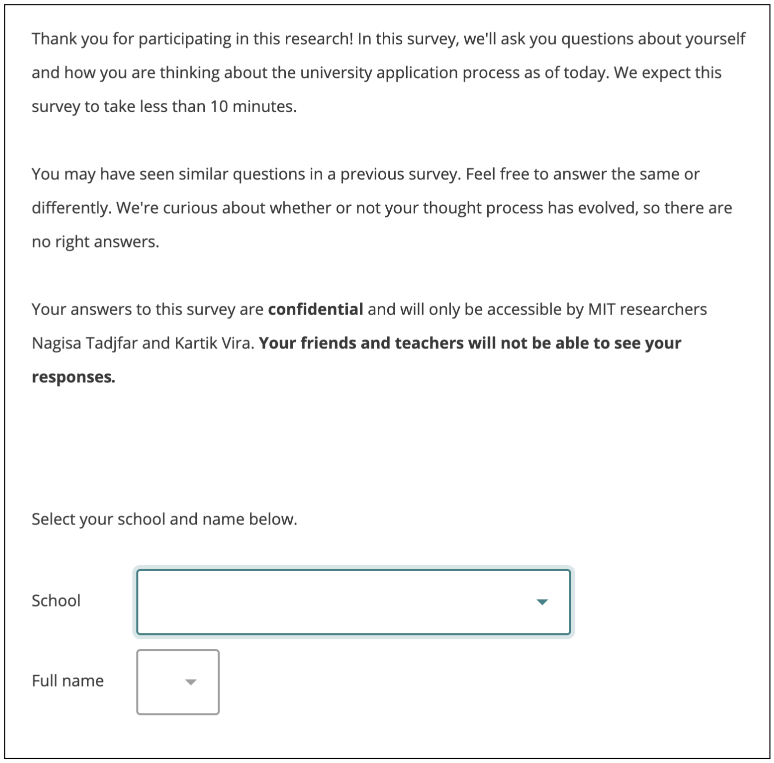
Figure F20: End of survey message


Thanks for taking the time to answer our survey! If you have any questions, concerns, or comments please email either ntadjfar@mit.edu or kvira@mit.edu.

We hope you enjoy the workshop!

F.2 Midline survey

Figure F21: 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.

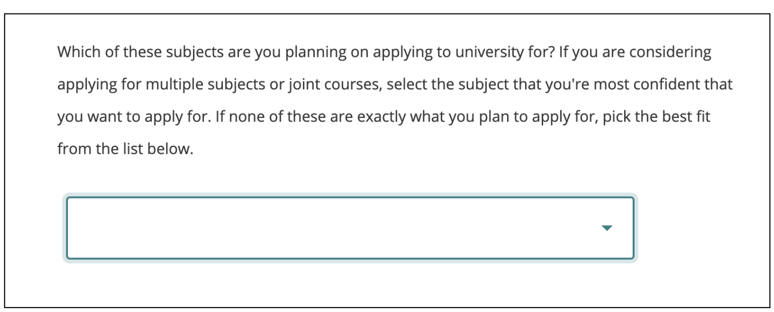
Your answers to this survey are **confidential** and will only be accessible by MIT researchers Nagisa Tadjfar and Kartik Vira. **Your friends and teachers will not be able to see your responses.**

Select your school and name below.

School

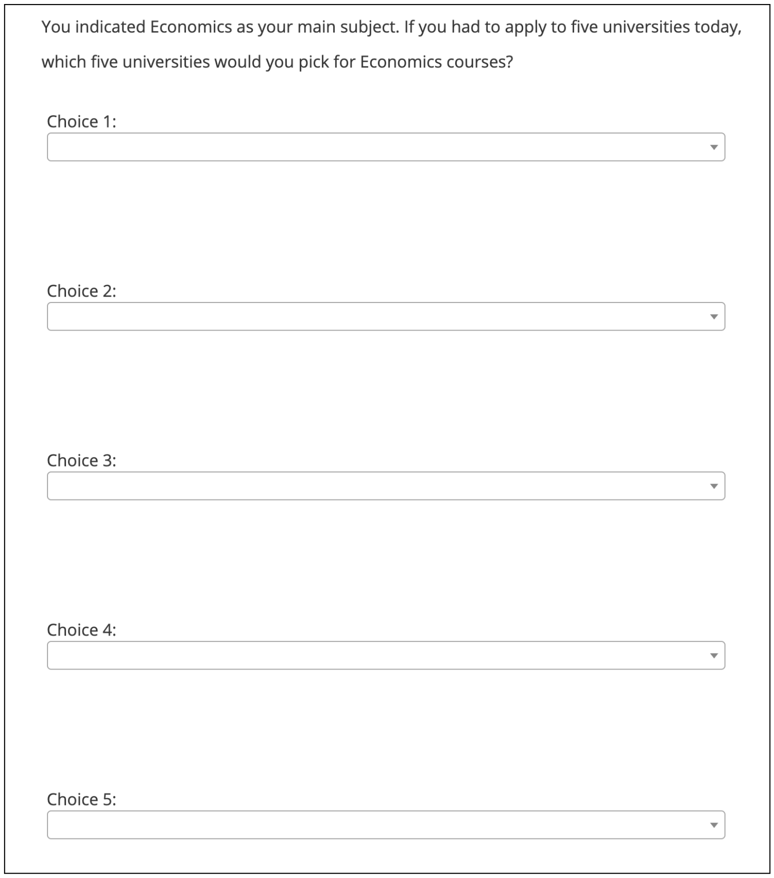
Full name

Figure F22: 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 F23: 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:

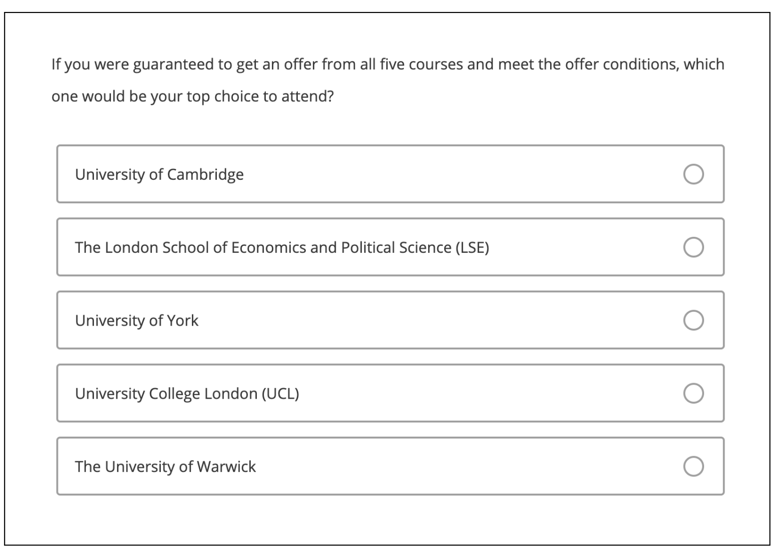
Choice 2:

Choice 3:

Choice 4:

Choice 5:

Figure F24: 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 F25: 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 F26: 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 F27: 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 F28: 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 F29: 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 F30: Gender

What is your gender?

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

Figure F31: Student home postcode

What is your home postcode?

Figure F32: 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

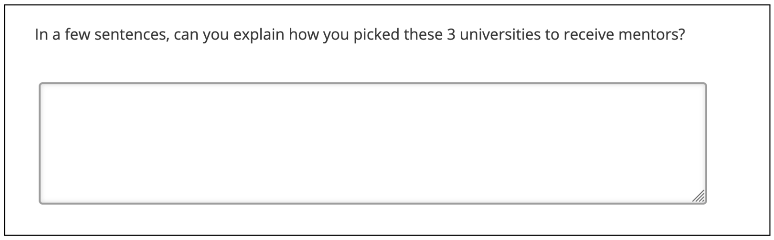
Figure F33: Mentor preferences

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?

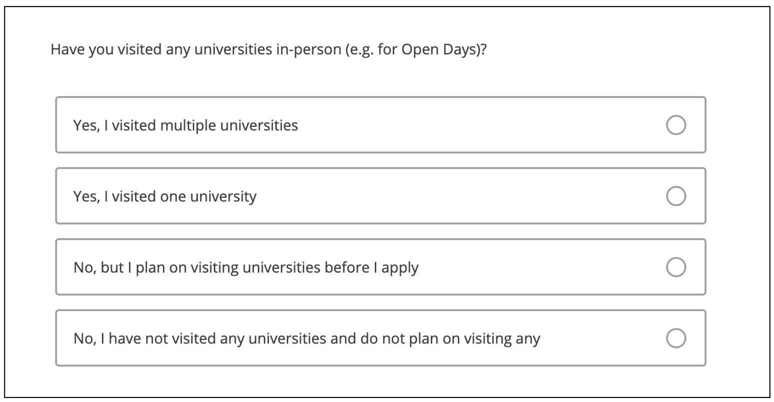
What would be your **third** choice of university to get connected to a current student from?

Figure F34: Mentor preference explanation



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

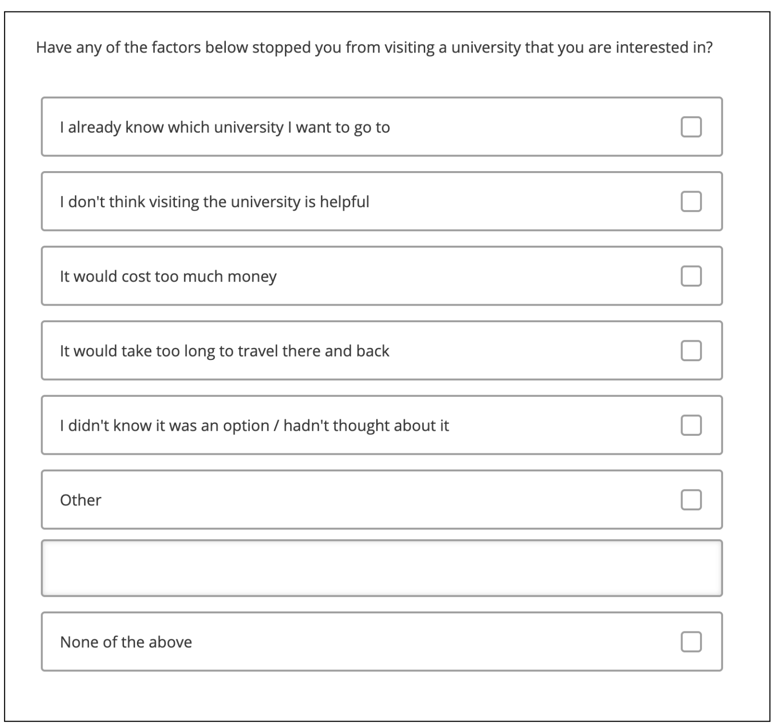
Figure F35: Number of universities visited previously



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 F36: 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 F37: Names of universities visited

Which universities have you visited?

Figure F38: 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 F39: 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 F40: Social fit beliefs

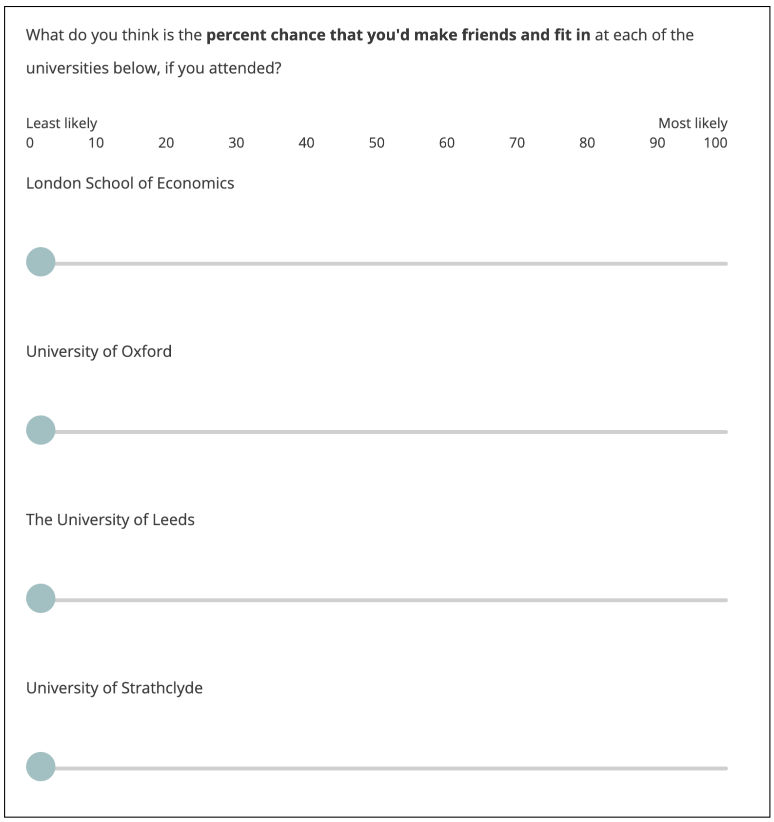
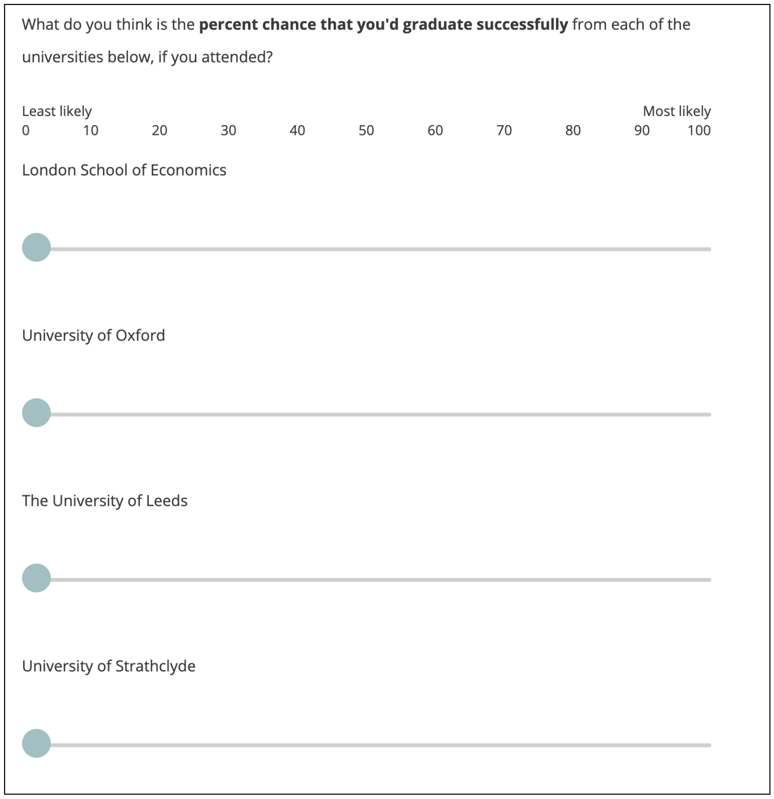


Figure F41: Graduation beliefs



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

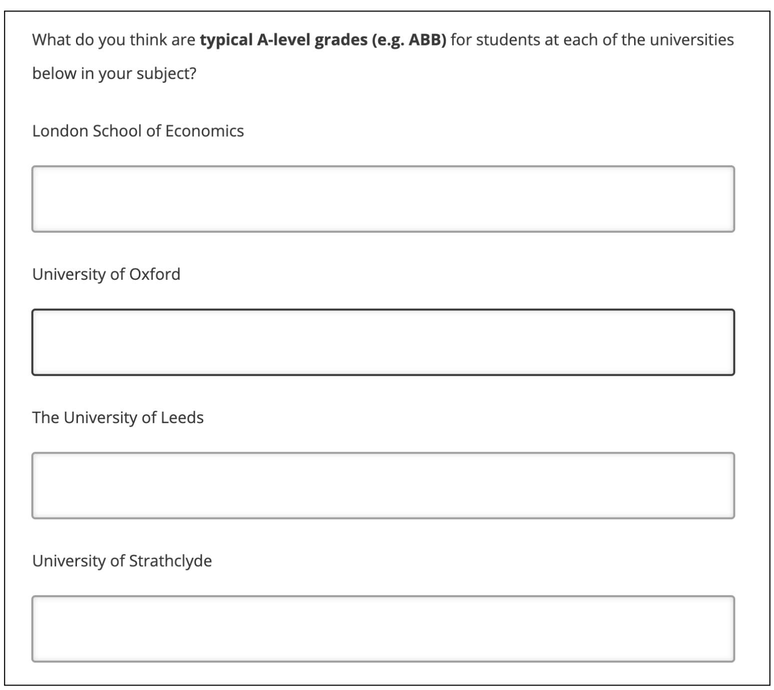
London School of Economics

University of Oxford

The University of Leeds

University of Strathclyde

Figure F42: 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 F43: Ending screen


Congratulations! You **are** eligible to be matched with up to two mentors after this workshop.

Kartik Vira (kvira@mit.edu) or Nagisa Tadjfar (ntadjfar@mit.edu) from the MIT research team will reach out to you to to put you in touch with your mentor(s). We'll contact you over email and over text using the details that you provided, so watch out for messages from us.

Mentors have generously volunteered their time to help you with the university process, and have a lot of useful information that should help you feel more comfortable with the application process and life at uni, so please make sure to reply to your mentors once we put you in touch with them!

Thanks for participating in this workshop - we hope you found it useful and that it'll help with your university decisions. Good luck!

G Experimental materials

G.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 G1: Presenter introduction

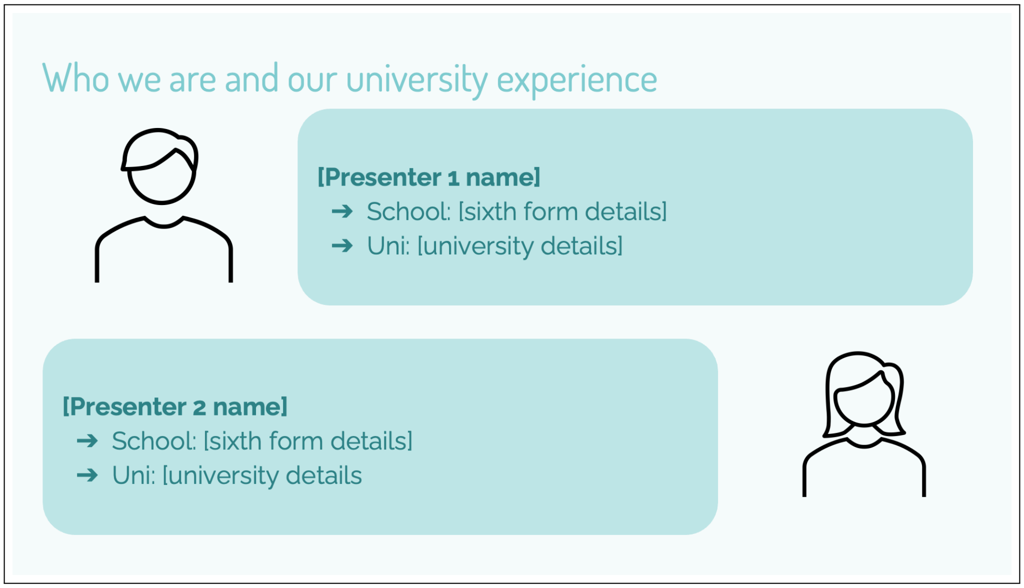


Figure G2: Application timeline

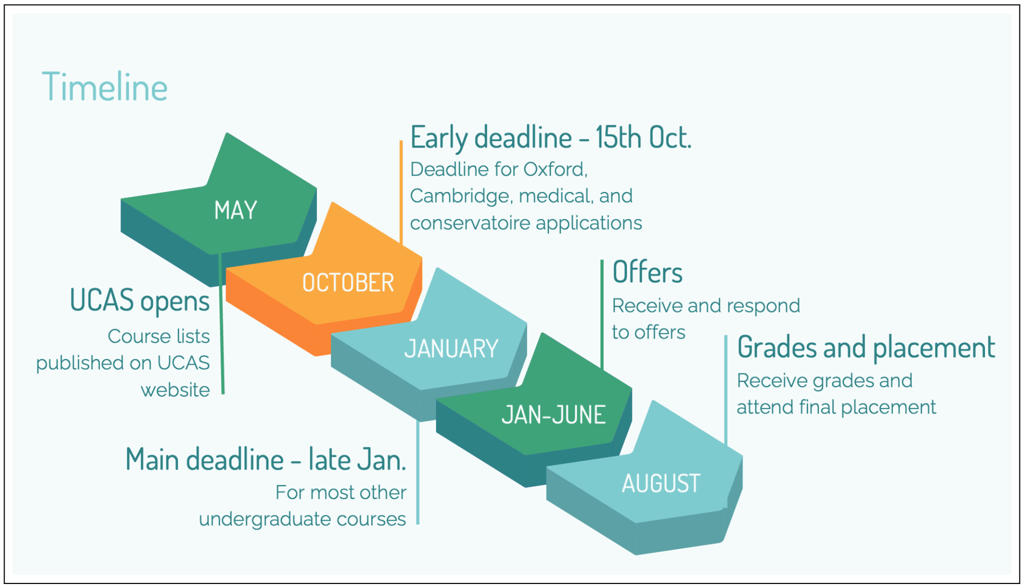


Figure G3: How universities make admissions decisions

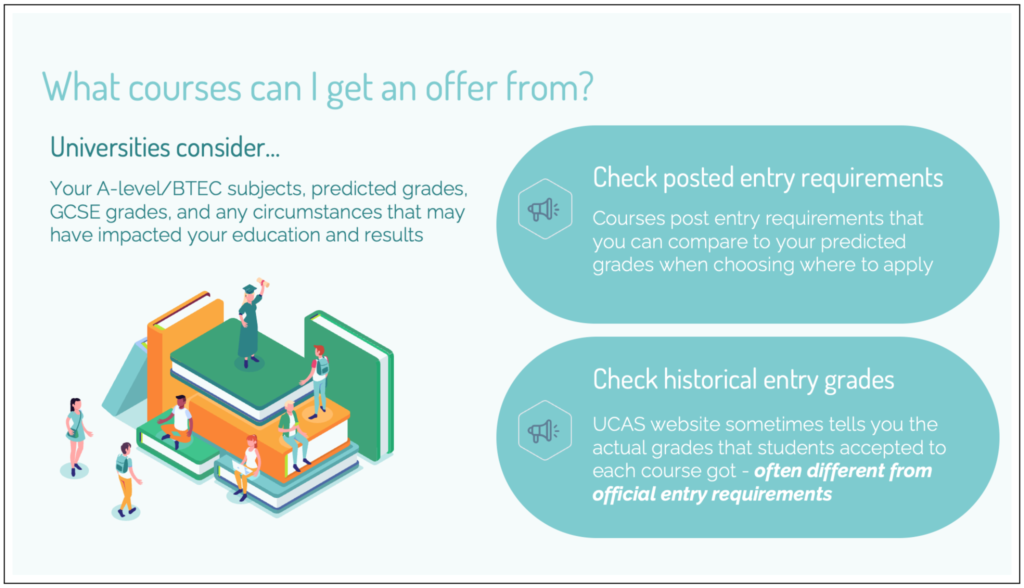


Figure G4: Contextual admissions

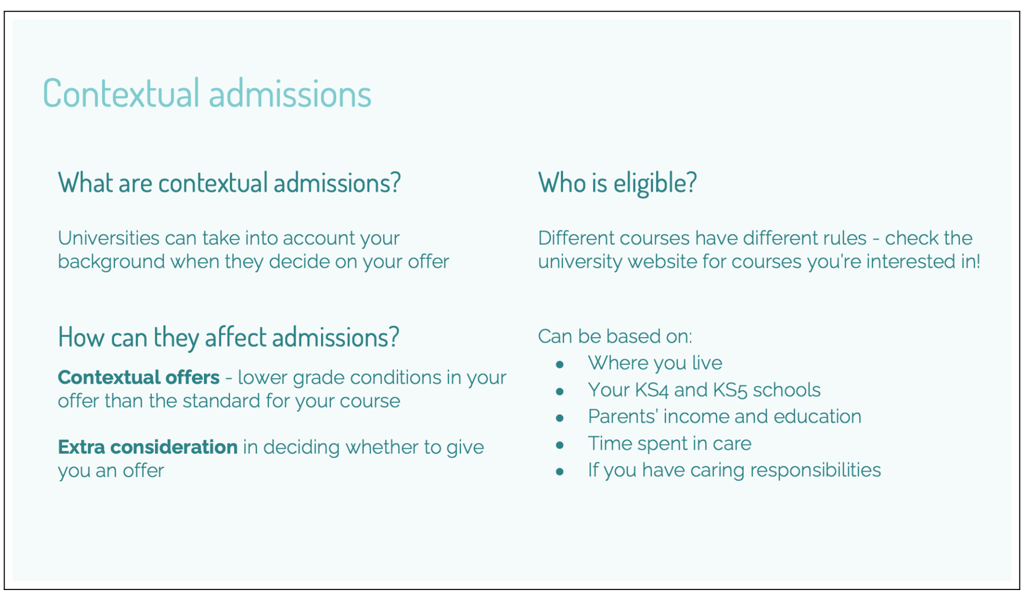


Figure G5: Russell Group student qualifications

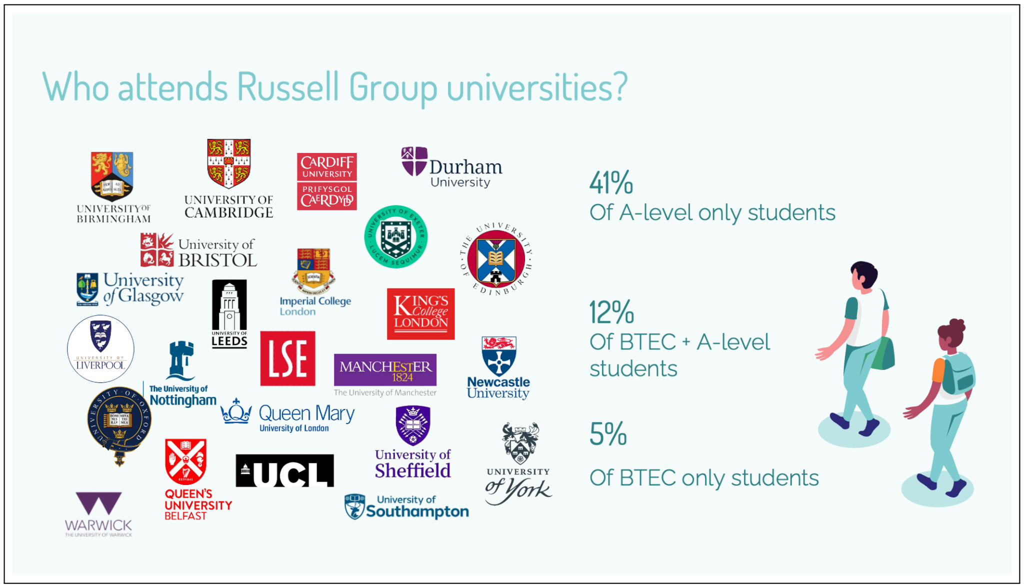


Figure G6: Russell Group student grades

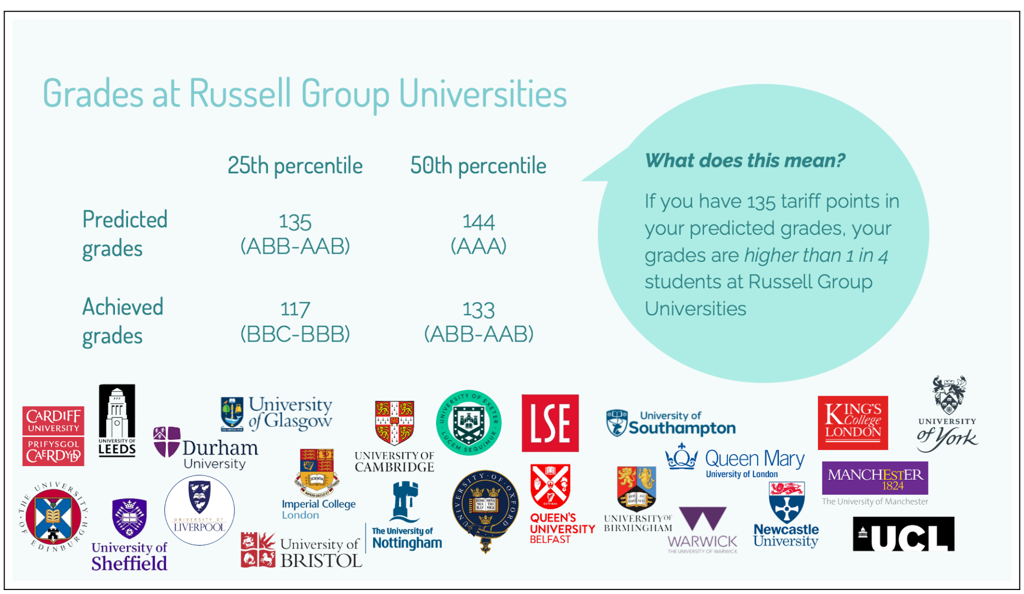


Figure G7: Required grades at different universities

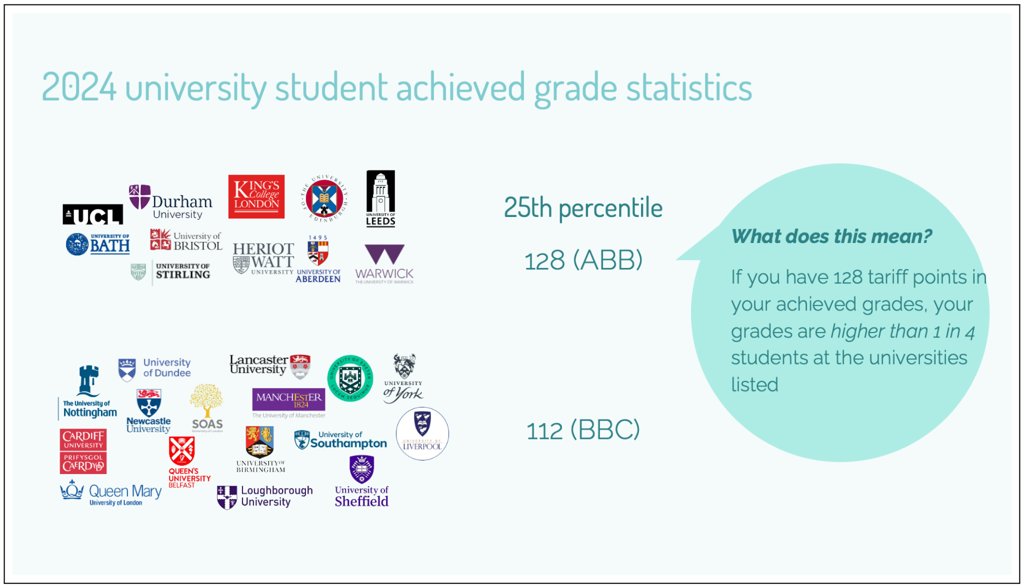


Figure G8: Earnings at different universities

Figure G9: Student regret statistics

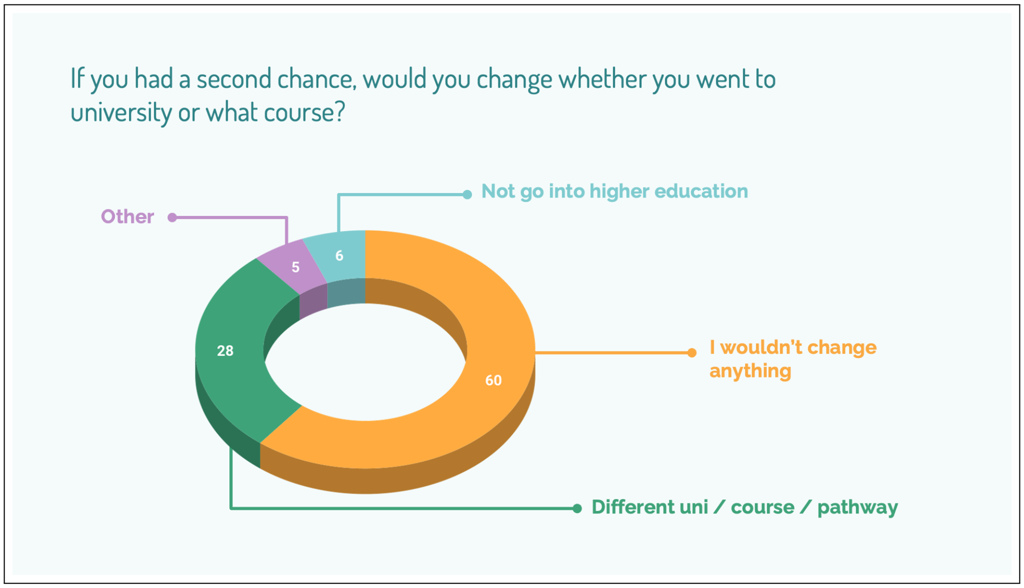


Figure G10: Student belonging and course statistics

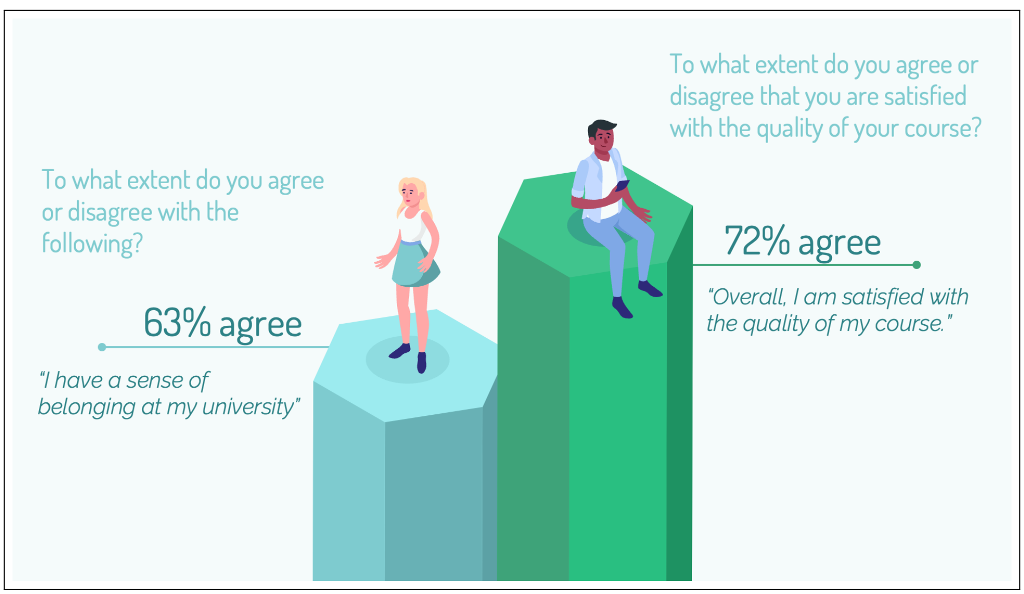


Figure G11: Responding to offers

Figure G12: Tuition and cost of living

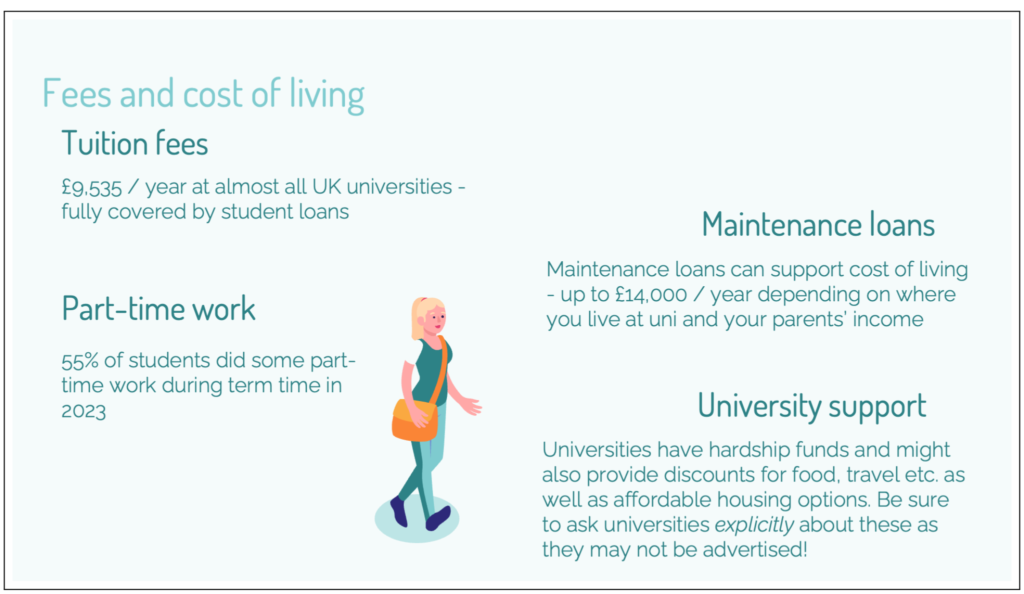


Figure G13: Personal statement format

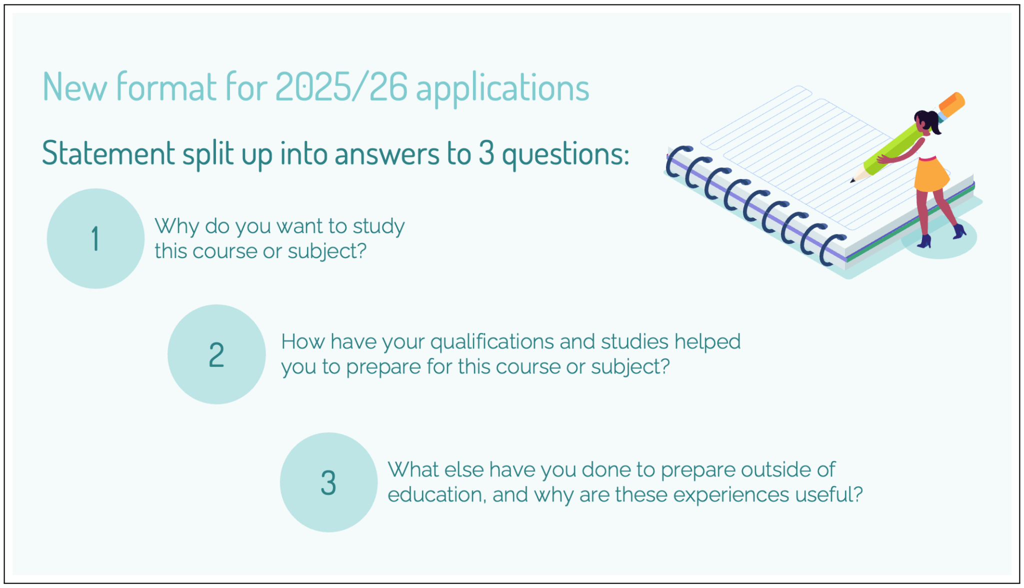


Figure G14: Personal statement advice – what not to do

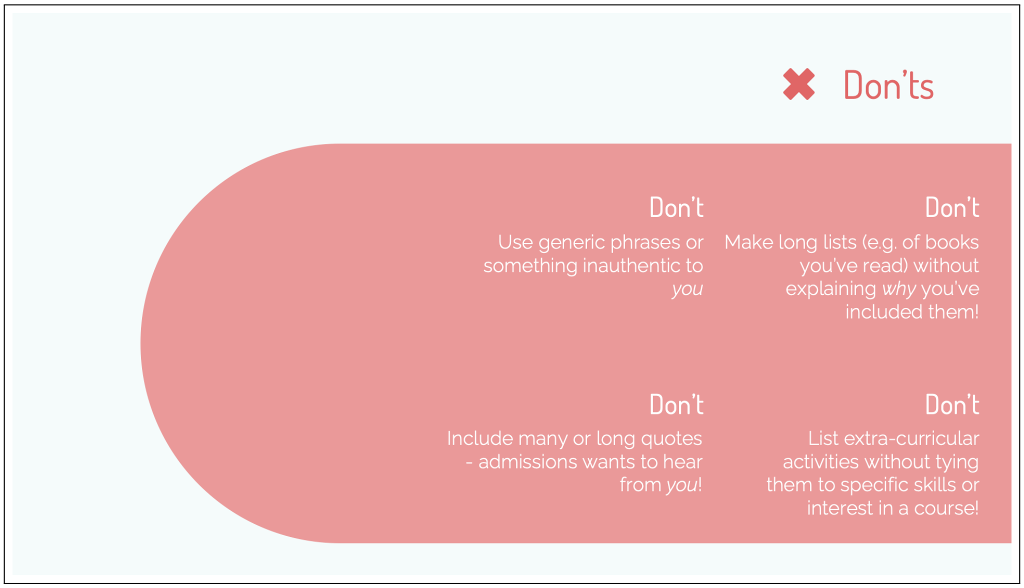


Figure G15: Personal statement advice – what to do

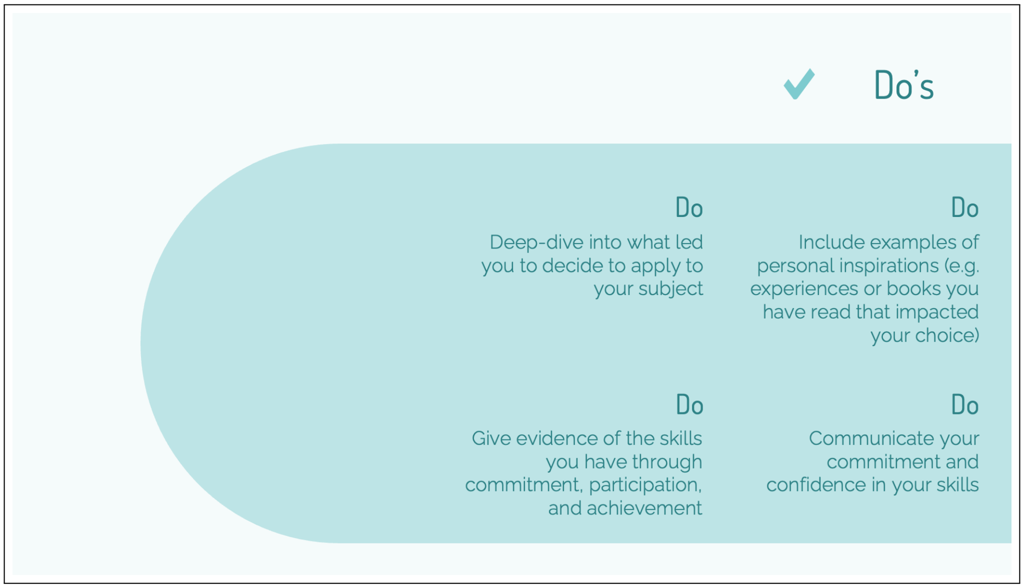


Figure G16: Online resources

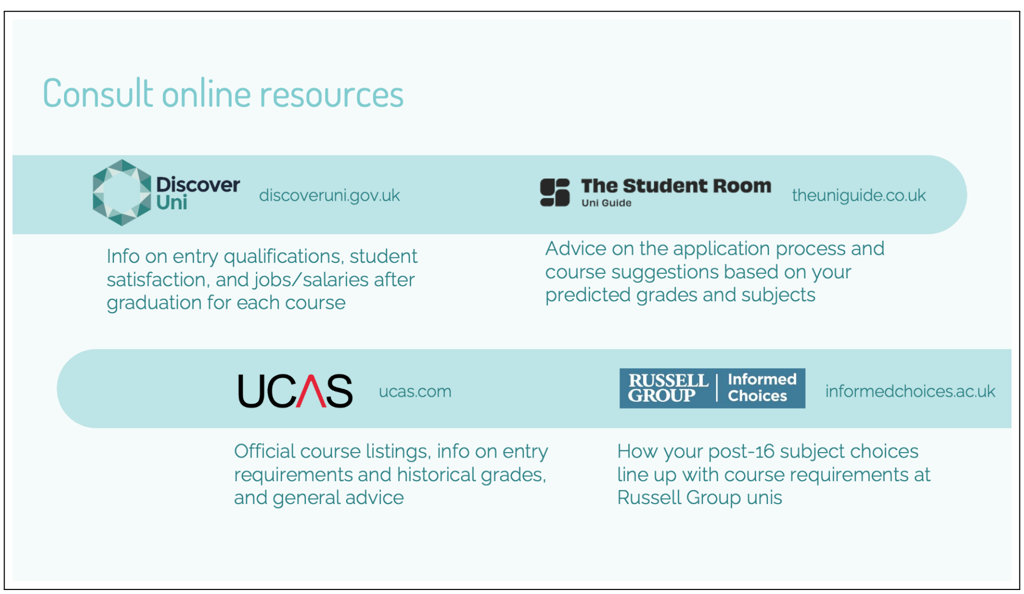


Figure G17: Other sources of information

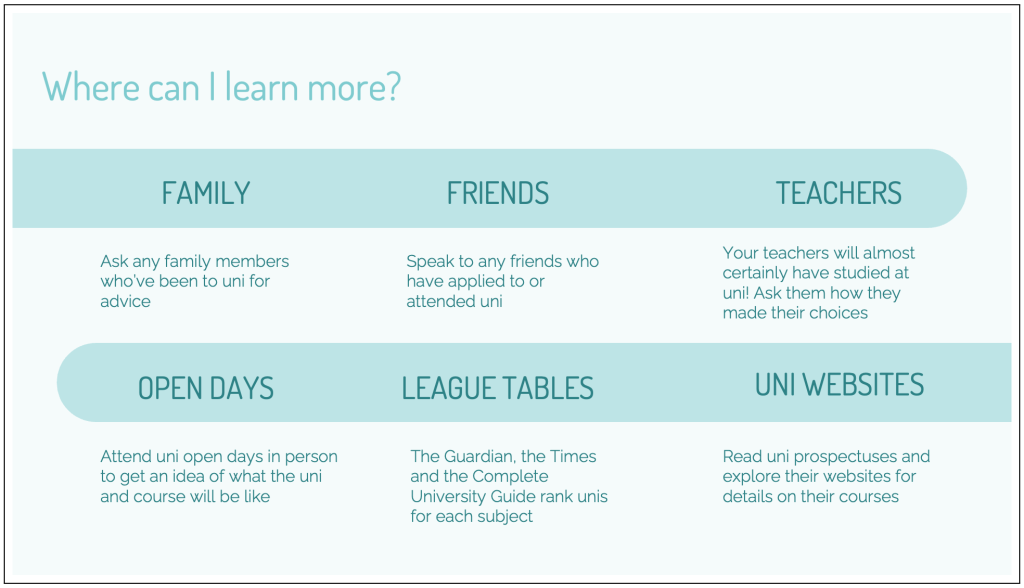


Figure G18: University visits

Figure G19: Description of mentorship / visit treatments

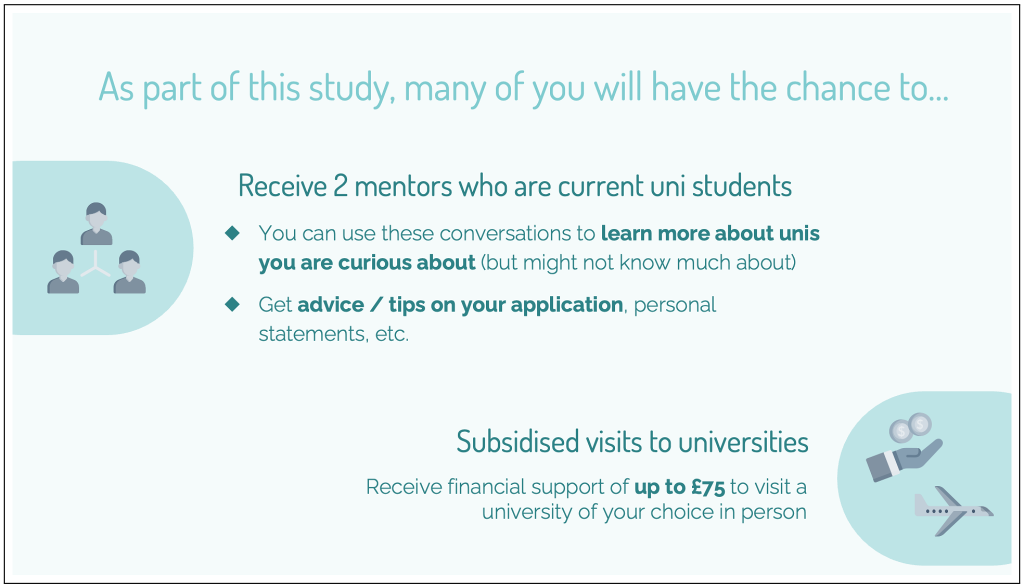


Figure G20: Survey QR code