Friends in Higher Places: social fit and university choice

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Abstract

Elite university access is highly unequal. Low-income students are less likely to apply to and attend than equally qualified high-income peers. 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%. Students induced into elite universities by a breakthrough are lower-income, but graduate at typical rates. Access induced by breakthroughs promotes upward mobility: marginal entrants earn £4,000 more annually than matched control students. Why were these students not applying previously? Using a field experiment in British schools, we show that the primary barrier is students' beliefs about social fit at top universities rather than beliefs about admissions chances or success at university. At baseline, low-income students are more pessimistic about their chances of fitting in at an elite university, but not about their chances of admission or graduation. 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. While students' expectations of fitting in and making friends shift, beliefs about admission chances or graduation do not. Students randomly matched with mentors primarily discuss social life at university, and the most important factor participants raise with mentors is whether they would fit in and enjoy their time. Our findings highlight perceptions of the social environment at elite universities as a central barrier and illustrate how scalable interventions can promote social mobility.

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1 Introduction

Universities, particularly highly selective universities, are a potentially crucial vehicle for social mobility. 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) and many elite professions are disproportionately comprised of graduates of just two universities, Oxford and Cambridge: 71% of senior judges, 56% of senior civil servants, 44% of newspaper columns, and 24% of Members of Parliament attended one of these two universities, though they educate less than 1% of the population (Sutton Trust 2019). Students from low-income backgrounds who manage to attend the most selective universities often see similar labour market outcomes to their high-income counterparts, indicating that university access for these students can indeed support mobility into the top of the income distribution (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. Equality of access to selective universities is thus a political and regulatory priority in the UK and other developed countries. Yet despite a context with uniform tuition fees across universities, generous government loans for tuition and living costs, extensive university outreach programmes, British students from low-income neighbourhoods are around 10–15% less likely to apply to top universities than those from high-income neighbourhoods with the same standardised test scores. These disparities in applications explain a substantial share 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.

Recent literature has found that students are more likely to apply to a university in response to the enrollment decisions of their siblings (Altmejd et al. 2021), their neighbours (Avdeev et al. 2024), or past cohorts of students at their school (Bechichi and Kenedi 2024). Each of these are proxies for students' *exposure* to attendees of that university. We say a student is exposed to a university if they have the opportunity to interact with someone who attended that university. Given that disadvantaged students are less likely to be exposed to top university attendees through any of these sources, these effects may contribute to the disparities in applications to top universities across the income distribution. But this literature leaves open two questions. First, do students who are induced to attend a top university by exposure succeed in the labour market? Second, what effects on students' information, beliefs, or preferences explain how exposure changes students' application decisions?

We use a combination of national administrative data and a field experiment with university applicants in the UK to establish similar patterns in the British context, and then extend this literature to address both of these questions. First, in administrative data linking grades and university applications to labour market outcomes, we show that the students encouraged to apply in response to the enrollment of past cohorts at their school tend to succeed: they graduate at typical rates for the university, and see higher earnings by age 27 than observably similar control students. Second, we use a field experiment and elicited beliefs to understand the mechanisms for exposure effects. We find no evidence that exposure affects applications by changing students' perceptions of whether they could be admitted to the university or their belief that they could succeed academically and graduate successfully from it, or by ensuring that students have friends at the university who they can interact with. Instead, our results indicate that exposure improves students' perceptions of their non-academic experience at university, making them feel more likely to fit in and make friends at these universities, and that students are more likely to discuss life at university with past attendees than other topics. In summary, we argue that students care about the social environment at top universities when making application decisions, that this is consequential for their labour market outcomes, and that exposure to students who have previously attended top universities can improve students' perceptions of the the social environment and encourage applications.

We begin by presenting some descriptive facts to motivate the questions that we study in the remainder of the paper, combining results from the administrative data and from our RCT surveys. First, to illustrate the importance of the university earnings decision for careers, we estimate the earnings returns to each university following the methodology in Dale and Krueger (2002, 2014); we find a substantial age-27 earnings premium for graduating from one of the top 10–20 ranked universities in the UK, but below this ranking the earnings-selectivity gradient is largely flat. Second, we illustrate that there are substantial gaps in enrollment at top 10 universities between low-income and high-income students; mechanically decomposing these gaps, we find that differences in students' application rates (conditional on test scores) explain around 40% of them, so there are substantial applications disparities. Third, we show that low-income students are less likely to be exposed to attendees of top universities, and illustrate the disparities in exposure across the geography of England. Thus, we show that university choice matters in the labour market, that there are socioeconomic disparities in application choices, and that these are correlated with lack of exposure as low-income students have less exposure to top universities.

To illustrate exposure effects in the UK context, we study the effects of *breakthroughs* to a university – cases where a student attends a particular university after several years where no student from their school had attended it – taking these as discrete changes in exposure to a university through a student's school. In an event study framework we find, concordant with the past literature discussed above, that breakthroughs persistently encourage applications to the specific university to which there is a breakthrough at their school, raising application rates by around 30–50%. However, they have no effect on applications to other, similarly ranked universities, so the effect of breakthroughs seems to be to provide university-specific exposure rather than raising the overall ambition of applications. We also do not see a decrease in applications to similarly ranked universities, ruling out the possibility that breakthroughs simply move applications horizontally between similarly-ranked universities. Instead, we see a concomitant decline in application portfolios that only include lower-ranked universities, so breakthroughs to top universities draw applications away from lower-ranked universities.

Next, we investigate which students are most affected by breakthroughs. Treating breakthroughs as an instrument for applications, we find that compliers who apply in response to a breakthrough are high-ability relative to their school but no less likely to be economically disadvantaged than the typical student at their school; compared to typical students at the university they enroll at following a breakthrough, they are slightly lower-ability and substantially more disadvantaged. 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.

The welfare implications of these application effects depend on longer-run outcomes. We use linked administrative data for England to build on these results, providing novel evidence that the students who attend top universities following a breakthrough seem to be well-suited to these universities. We show, first, that students are likely to receive offers, enroll at, and graduate successfully from the universities in question; we find little evidence that would point towards overmatch or mismatch in the sense of e.g. Arcidiacono et al. (2011), where marginal disadvantaged students are made worse off by a policy change inducing more ambitious university enrollment. In addition, we find that compliers are no less likely to graduate, or graduate with a passing grade, than typical students on their university course. Two different comparisons of students who apply to breakthrough universities ranked in the top 10 with untreated students matched on observables find that these 'induced applicants' earn around £4,000 more than their matched controls, an effect that is comparable in magnitude to the estimated earnings premia for top universities described above and that corresponds to roughly 15% of median earnings at this age for university graduates.

Taken together, these results imply that *lack* of exposure to top universities discourages applications from students who are disproportionately from low-income neighbourhoods and would see substantial labour market returns from attending these universities. Finally, we conduct a simple back-of-the-envelope calculation in which we estimate that 15–30% of the gap in applications between low-income and high-income students would be closed by raising low-income students' exposure to top universities to the levels for high-income groups. The treatment effects of breakthroughs thus imply that inequality in exposure plays a substantial role in overall application disparities.

These results demonstrate *that* breakthroughs to a university affect application decisions, and that this in turn can serve to promote social mobility, but do not explain why students respond to decisions. It is difficult for policy to reliably and scalably induce breakthroughs at underserved schools, but if we understand how beliefs and preferences respond to exposure in order to change application decisions, this allows government and universities to design policy responses that target these mechanisms while being more scalable. We consider three mechanisms that might explain the effects of breakthroughs on university application choices and are consistent with the lack of spillovers in applications to to similarly-ranked universities. First, 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. Second, knowing a student who is currently attending a particular university might directly connect the student to social networks at that university. Third, interactions with students who attended the university might make students feel more likely to be able to fit in with the social environment at that university, even if they do not plan to directly interact with the student when at university. These mechanisms are challenging to distinguish in administrative data that includes choices alone.

To address this question, we conduct a field experiment with university applicants at over 20 schools in England and Wales, and conduct surveys of students to directly elicit relevant beliefs and preferences, creating a novel dataset including inputs to the university decision-making process that are unobservable in administrative data. The experiment randomly provides students with some exposure to students who went to a particular university, in different forms that are more scalable and more comparable to the strategies used by university outreach programmes in practice than the exposure provided by past cohorts at a student's school. We show students video clips describing the experience at a university, connect them with mentors who are able to have one-on-one conversations with students, and provide them with a travel subsidy to encourage them to visit a university and interact with current students in person. Different students are exposed to different universities, and we additionally vary whether mentors are demographically similar or dissimilar to the student. In contrast with their siblings or students from the same school as them, students are unlikely to be directly introduced to social networks at the university by mentors or students, so effects of treatments on application decisions in the RCT are not driven by this mechanism. If exposure provides information about the application process or academics, we would then expect students to become more confident about their chances of being admitted to the university or graduating successfully from it in response to the treatments, while if social fit plays a stronger role we would expect students beliefs about whether they can fit in and make friends to respond.

We find that exposure to a video about a university, in which a university alumnus discusses what life was like at that university as well as their application process, raises the probability of students intending to apply to that university in a survey taken a median of 4 days later by around 5 percentage points; this corresponds to a 30% increase relative to the baseline application probability, comparable in magnitude to the effects of breakthroughs in the administrative data. Since students have no way of contacting the student who recorded this video, these effects are not driven by forming direct social connections at the university. Turning to beliefs, we find precise null effects on mean belief updating and the distribution of updates about students probability of being admitted to the university or graduating successfully from the university, indicating that information affecting academic beliefs is not an important mechanism for exposure effects. However, we do find that video exposure induces a rightward shift in the distribution of belief updates about the probability of fitting in and making friends: students are more likely to update positively and less likely to update negatively about these beliefs between the two surveys conducted. Furthermore, students who spoke to mentors were more likely to discuss student life at university and fitting in than careers, university academics, or advice about how to choose courses to apply to, and their mentors were more likely to report that life at university was the most important factor in students' decisions than any other factor.

Given that exposure seems to operate in large part through beliefs about social fit, we might expect that there are stronger effects who share characteristics with the breakthrough student that predict friendship and social interaction. However, we find that both beliefs about social fit and application intentions update more when students see a video from a student of the *opposite* gender than one from the same gender. This is surprising given that we find in the breakthrough analysis that students respond more to a breakthrough student of the same gender, and that there is strong gender homophily in social networks within a school. One possible explanation is that sharing a characteristic such as gender increases the *probability* of interactions with the breakthrough student in that setting, but *conditional* on an interaction like seeing a video, gender matters less – or that students are even more attentive to students of the opposite gender conditional on interaction, potentially reflecting the marriage market role of the social environment at college for straight students. The margin of takeup of interactions is also highlighted by our results on which students interact with their assigned mentors: we consistently find that students who had parents who attended university are *more* likely to contact their mentors, even though these students are likely to have more pre-existing information to support their university applications.

Taking our results together, we consistently find across both evidence from breakthroughs and exposure treatments that exposure to universities encourages applications to them, and that the effect sizes imply that disparities in exposure explain 15–30% of overall disparities in applications. Our analysis of breakthroughs suggests that marginal students who are encouraged to apply to a university by exposure graduate at typical rates for their university, despite tending to be more socioeconomically disadvantaged, and receive higher earnings than similar students not exposed to a breakthrough. And evidence from our surveys and randomised exposure treatments point to students' beliefs about the probability of fitting in and making friends at particular universities as an important mechanism for these effects: disadvantaged students are more pessimistic about social fit at these universities, exposure tends to make students more optimistic, and students are more likely to discuss life at university and fitting in at university with their mentors than any other topics. We conclude with a brief discussion of implications for higher education policy and the effects of exposure on decision-making in other contexts.

Related literature. This paper relates most closely to a strand of literature that documents the influence of different peers' university enrollments on students' higher education decisions. Altmejd et al. (2021) illustrate that younger siblings are strongly influenced by their older students' college destinations, while Bechichi and Kenedi (2024) find effects of past cohorts at a school on subsequent cohorts' applications, similar to the breakthrough effects that we study; other studies finding similar effects for siblings and neighbors include Barrios-Fernández (2022), Avdeev et al. (2024). Most of this prior work is limited to studying university enrollment, and sometimes applications and graduation depending on the context; we are able to further study long-run outcomes such as earnings. In addition, this literature has focused on analysis of administrative data; our RCT provides formally randomised evidence on the effects of exposure, and our surveys allow us to provide new evidence on the mechanisms for these effects.

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). We highlight a potential mechanism for these effects – differential exposure to top universities inducing differential application decisions – and explore why mechanism affects applications. 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. We also contribute to literature evaluating interventions aimed at increasing college access and enrollment (Carrell and Sacerdote 2017; Andrews, Imberman, and Lovenheim 2020; Dynarski et al. 2021; Sanders, Chande, and Selley 2017; Cohodes, Ho, and Robles 2022); our interventions are targeted at providing role models and exploiting the effects of exposure and social beliefs.

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

2 Background and data

2.1 The UK educational system

The UK has different educational systems in England, Wales, Scotland and Northern Ireland. We focus primarily on England and Wales in this paper; our administrative data covers only English students, and the sample of schools in our RCT includes schools in England and Wales. We provide a brief overview of the system in this section, and provide further institutional details in Appendix A.

Students in England and Wales take compulsory 'GCSE' exams at age 16 in mathematics, English, science, and other optional subjects; we use GCSE grades in the 'core' subjects of maths, English and science, converted to a percentile, throughout the paper as a measure of students' academic ability. Students then spend the last two years of secondary education completing academic or vocational qualifications; students aiming for selective universities will generally take 3 A-levels (the more academic qualification) in specific subjects that they choose. Some students complete these qualifications at the same school they completed their GCSEs at if the school offers both types of provision, while others switch to a different institution, often to a dedicated 'sixth form college' or 'further education college' that provides only age 17–18 education.

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

Universities choose whether to make an offer to a student or reject them outright. If they accept the student, they can choose to make the student an offer that is *conditional* on receiving certain grades in their A-level or vocational qualifications – which are completed after students receive offers – or can choose to make an unconditional offer. Most offers are conditional and are frequently

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.

based on a students' top 3 A-level grades, particularly at higher-ranked universities.[2] After receiving offers, students choose an offer to accept. Once they receive their grades, they can attend the university they received an offer from if they meet the grade conditions; if they fail to meet them, they can instead attend an 'insurance' choice from their offers with lower grade conditions, enter 'Clearing' (an after-market scramble coordinated by UCAS), or choose not to attend a university in that cycle. They are free to reapply in future years based on their final grades.

The typical length of an undergraduate degree in the UK is three years, although a substantial minority of courses last 4 years – particularly those that embed a requirement for a year abroad or a year in industry as part of the course – and medical courses last 6 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.

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 courses charge tuition fees exactly at the cap, meaning that there is no variation in tuition between universities; financial considerations thus only enter into the choice *between* universities to the extent that a student's cost of living differs between different universities.

2.2 Administrative data and breakthroughs

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

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

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.

includes both undergraduates and enrollees in graduate school. 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 datasest, and exclude students who do not apply to any university. Since our data on longer-run outcomes (university graduation and earnings) extends only until 2021, we lack data on long-run outcomes for later application cohorts – for instance, we can only observe earnings at age 27 for cohorts from 2012 and earlier. Appendix table B1 indicates the cohorts (indexed by the year at which they complete their high school education and apply to university) for which each 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 who do not apply at all are excluded, as are any applications at different ages. Second, we exclude students who do not apply to at least one university that can be linked with HESA data. UCAS courses that do not link with HESA data are frequently more specialised courses, such as arts academies or music conservatoires, rather than traditional university courses.

One limitation of the administrative data is that it does not include detail on parental background for all participating students. We observe parents' education in HESA data, but this is only available for students who enroll at a university, and we do not observe parental income for any students.[4] We therefore use an index of neighbourhood deprivation, defined at the Lower-Level Super Output Area (LSOA) level, as our primary measure of socioeconomic status; we focus on a binary low-income indicator defined by being in the bottom two quintiles of this index. See Appendix section B.1 for details.

Another limitation to note is that LEO anonymises all institutional identifiers, so we are not able to identify particular schools or universities. For analysis that requires this, we use a different administrative dataset provided directly by UCAS, which does not have these restrictions. The standalone UCAS extract is essentially the same as the UCAS data provided in LEO, but it includes all university applicants regardless of domicile (whereas LEO includes only students from England) and applicants of all ages (whereas LEO includes only applications at age 18). The limitation of this data is that it does not include the linkages to any other datasets, but the standalone UCAS

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

extract provides data on GCSE and A-level grades, demographics, and university applications, offers and acceptances, which is sufficient to use it for some analysis where identifying institutions is necessary.

2.3 Randomised controlled trial setting

In addition to our analysis of administrative data, we conducted a randomised controlled trial working with university applicants across the UK. We provide detail on the design of this RCT in Section 6, but outline the setting here.

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

Schools were recruited to participate in the programme through various channels, including school networks from our partners at WISE and AtkinsRealis, as well as direct outreach to schools. Within each school, 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.

Table 1 presents summary statistics for students participating in our RCT, as well as comparable statistics drawn from the administrative data for comparison. Differences in composition may result from both non-random selection of schools into participation and from changes in the composition of the student population over time (as stated above, we observe administrative data from 2007–2021, while RCT data is drawn from 2024–25).

Compared to the national administrative data, we find there are substantial differences on ethnicity (the RCT sample is less white) and gender (the RCT sample is less female). In terms of disadvantage, RCT participants are comparable to the overall KS5 population and somewhat more disadvantaged than the typical university applicant, based on the shares of students from low-income neighbourhoods. However, they are slightly more likely to be taking 3 A-Levels (the most academic set of qualifications) than even the typical university applicant. Overall, the RCT's population does not look heavily selected on ability or disadvantage.

3 Background on applications to university in the UK

3.1 Graduating from one of the top 10 most selective universities raises earnings at age 27 by £4,600 relative to the median university

We care about sorting across universities to the extent that which university a student goes to matters for their wellbeing, and particularly for their earnings. Since our administrative data links university applications and attendance to earnings and rich test score data and demographics for a very large sample of students, we are able to precisely estimate earnings returns to universities across the distribution of university selectivity. We use this to illustrate the potential returns to students going to a higher-ranked university.

Our primary outcome in this analysis is annual earnings from an individual's primary employer, conditional on receiving positive earnings. Figure 1a illustrates how mean annual earnings change over time for students with different university outcomes. University attendees earn less than non-attendees until age 22, as we would expect, but substantial gaps open up after this age. We also clearly see that graduating from a higher-ranked university is associated with higher earnings, and that the gaps between higher- and lower-ranked universities are larger than those between lower-ranked universities and non-attendees. By age 30, non-attendees at university have annual earnings around £20,000, graduates from an average university (below the top 30) have earnings around £26,000, and graduates from a top 10 university have earnings around £44,000. We also see from this figure that the relative earnings differentials have largely stabilised by age 27, suggesting that earnings at this age are a reasonable proxy for future earnings differences. This motivates our focus on earnings at age 27 as our primary measure of earnings in the remainder of this paper.[5]

We next attempt to adjust these outcomes for selectivity, now looking university-by-university at earning sffects. For student i who graduates from university u in year t, we regress

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

and plot the fixed effect on each university θ_u after applying empirical Bayes shrinkage to point estimates, where the omitted category is the modal university in the sample. Specifically, for the empirical Bayes shrinkage, let s_u be the standard error on the coefficient θ_u and $\mu_\theta, \sigma^2_\theta$ be the mean

5. 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.

and variance of the estimated θ_u coefficients across universities; then

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

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

While we cannot identify universities by name,[6] we plot earnings against each university's rank by the mean of the top 3 A-level grades, converted to UCAS tariff points (see Appendix B.1), achieved by students who enroll at the university. The modal (omitted) university is ranked 56, close to the median.

Figure 1b plots the results of this analysis. Specification 1 includes no controls in X_i, and thus just reflects raw differences in earnings across universities after applying empirical Bayes shrinkage. These raw gaps are large even at the early stage in workers' careers that we study; the average earnings coefficient for the top 10 universities is £12,761. Specification 2 controls for demographics (gender, ethnicity, neighbourhood income, free school meal status, and home region of the UK), school test scores (GCSE and A-Level grades), and fixed effects for the major that a student studies. It also introduces fixed effects for the exact portfolio of universities that the student receives an offer from (a subset of those that they apply to). This latter effect is in the spirit of Dale and Krueger (2002, 2014), and more recently Mountjoy and Hickman (2021): controlling for offer portfolios may capture earnings-relevant ability that is not captured by the available demographics and test scores, driven by the combination of student self-selection into applications based on private information about their ability and university offer decisions that take into account unobservable-to-the-researcher signals such as the personal statement. We match on the exact set of universities that a student receives an offer from. Thus, earnings comparisons used to estimate treatment effects are made among students who received offers from the same set of universities but ultimately attended and graduated from different universities. We can compare the resulting coefficients between two universities even if there is no pair of students with the same set of offers who attended each of the different universities, as long as there is a connected set across the offer set fixed effects that includes both of these universities. The large sample size of our data, as well as the dimensionality limitation provided by the cap of 5 applications in the UK context, means that all universities appear in the largest connected set, so comparisons between universities across the entire range of the distribution are valid under the assumption that treatment effects do not themselves vary by offer set (Mountjoy and Hickman 2021).

Figure 1c plots the coefficients on each university from just this latter specification, to better illustrate the range of estimated earnings effects, and adds confidence intervals to the estimates. Points in black have estimates that are significantly different from the mean effect after shrinkage.

6. See Britton et al. (2022) for analysis the earnings returns to different universities in the UK that is able to name universities, although that paper is not able to include offer set controls as we do in this analysis.

We see that most of the top 10–20 universities have distinctly higher returns – the average coefficient for the top 10 universities, relative to the omitted university, is £4,614. This is much lower than the descriptive gaps in earnings, but still a substantial increase; if there were no growth in the real value of earnings differentials for these individuals over time, projecting a £4,614 gap from age 27 until retirement age (68 for the cohorts used in this analysis) and discounting at 3% per year yields a lower-bound lifetime earnings effect of £111,000. Outside the top few 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. Our findings here are consistent with those of Britton et al. (2022), who similarly find high earnings returns for elite universities and a relatively flat distribution outside the top institutions. Access to these universities is thus likely to have large effects on students' earnings potential, and socioeconomic disparities in students' access to these universities is likely to perpetuate income inequality.

3.2 High-achieving low-income are 5–10 percentage points less likely to attend top 10 universities than students from richer neighbourhoods; gaps in applications explain around 40% of this gap

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

Adapting a decomposition described by Chetty, Deming, and Friedman (2023) to our context, we observe that the probability of enrollment at a given university through the main UCAS application scheme[7] can be decomposed as

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

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 (equation) 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

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

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

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

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

3.3 Lower-income students are 15 percentage points more likely to attend a school where no-one has attended one of the top 10 universities in the last 3 years

Disparities in application and enrollment rates at top universities across income levels naturally result in differences in whether students are likely to have opportunities to interact with students who went to top universities. Figure 3a illustrates that low-income students are substantially more likely to attend a school that sends no-one to top universities than their higher achieving peers. Specifically, students from the poorest decile of neighbourhood deprivation are 20 percentage points more likely to be at a school that has sent no-one to one of the top 10 universities in the preceding three years than students from the richest decile. Figure 3b plots the same differences conditional on test scores and other demographics,[8] showing that a 10 percentage point discrepancy remains after adjusting for these differences. Figure 3c plots variation in exposure across England, indicating the share of students in each region attending schools where no one has attended one of the top 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 enroll at these universities, and that they are less likely to be exposed to past enrollees at these universities in their school. We now turn to analysis of breakthroughs to understand how changes in school enrollment patterns and exposure to a university affect applications and longer-term outcomes.

4 Administrative design

4.1 Breakthrough event studies

To understand how variation in exposure to universities across may affect application behaviour, we study the effects of breakthroughs to universities. Consider two schools, School A and School B, that have both had none of their graduates attend University X for several years. If a student from School A is then admitted to and attends University X, we refer to this as a breakthrough to University X at school A, and refer to the student who is first admitted as a breakthrough student. This is a discrete change in the exposure to students attending University X at School A; students at the school in the next year will now potentially be aware that someone from their school attended University X and may be able to directly discuss the experience at University X with them. 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.

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

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

$$Y_{ist} = \alpha_s + \gamma_t + \delta X_{ist} + \sum_{\tau \neq -1} \beta_\tau \mathbb{I}(t - T_s = \tau) D_s + \varepsilon_{ist} \quad (4)$$

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. X_{ist} includes 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.[9]. We exclude the breakthrough student themselves from the sample (or if there are multiple breakthroughs to the same university in the same year, we drop one of the breakthrough students), in order to isolate the effect of a breakthrough on the rest of the breakthrough student's cohort in period $\tau = 0$. To allow for heterogeneous treatment effects by treatment year, we use the Sun and Abraham (2021) estimator for event studies.

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

$$Y_{istu} = \alpha_{su} + \gamma_{tu} + \delta_u X_{istu} + \sum_{\tau \neq -1} \beta_\tau \mathbb{I}(t - T_{su} = \tau) D_{su} + \varepsilon_{istu} \quad (5)$$

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

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

$$Y_{ist} = \alpha_s + \gamma_t + \delta X_{ist} + \beta \mathbb{I}(t - T_s \geq 0) D_s + \varepsilon_{ist} \quad (6)$$

$$Y_{istu} = \alpha_{su} + \gamma_{tu} + \delta_u X_{istu} + \beta \mathbb{I}(t - T_{su} \geq 0) D_{su} + \varepsilon_{istu} \quad (7)$$

9. 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.

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 including the breakthrough university; portfolios that exclude the breakthrough university, but include a different university in the same 5-university tier; portfolios that include no universities from the breakthrough tier but include at least one university from a higher tier; and portfolios including only applications from lower tiers than the breakthrough university. For each university tier, the four difference-in-difference coefficients from regressions with each of these outcomes sum to zero, thus decomposing where breakthrough applications are drawn from.

4.2 Conditional effects

To understand the effects of breakthroughs on student welfare, we can go on to look at the effects on students' earnings, making use of the linkage with tax data in LEO. The most natural design would perhaps be to extend the event study described above to look at earnings as an outcome. However, we will find that these results are generally underpowered, since the vast majority of students at a school are not affected by breakthroughs in the first stage either because they are always-appliers or never-appliers. Noise introduced into the estimation from these individuals' earnings means that it is challenging to isolate an effect of breakthroughs in the event study design.

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

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

5 Effects of university breakthroughs

5.1 Breakthroughs persistently increase applications to that university, but not other universities that are of similar selectivity

First, we present an illustrative example using breakthroughs to the universities of Oxford and Cambridge. 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). Figure 4a plots coefficients on the relative time indicators from (4), where the treated schools are those that experience a breakthrough to Cambridge and the outcomes are application to Oxford and Cambridge as indicated; Figure 4b does the same, but for schools that experience a breakthrough to Oxford. We see an increase in applications to Cambridge in panel (a) at the year of the breakthrough to Cambridge, rising by 0.6–0.8 percentage points, while the application rate to Oxford does not significantly increase. Similarly, in panel (b), there is an increase of 0.4–0.8pp in applications to Oxford following the breakthrough 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' enrollment at the university.

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

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

5.2 Breakthroughs draw applications away from lower-ranked universities

Figure 5 generalises these results to a broader spectrum of universities, pooling across universities as described in equation (7). 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. We split these 30 universities into 3 tiers of 10 universities each. The first bar in each panel of Figure 5 indicates the effect of breakthroughs in a difference-in-differences framework (replacing the relative time indicators from the event study above with a single post-treatment indicator). We see an increase in application rates of around 0.5–1 percentage point across the range of universities. The pattern that breakthroughs increase applications thus generalises beyond the most elite universities, suggesting that the mechanism involved is not unique to these universities.

If applications to breakthrough universities increase following a breakthrough, this must draw applications away from other universities, given that the cap of five applications is typically binding in the UK. As described in section 4.1, we construct mutually exclusive 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; 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. 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%, reinforcing the point that the influence of breakthroughs is highly concentrated at the university in question.

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

5.3 Students encouraged to apply by a breakthrough tend to graduate and see higher earnings

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

Figure 6 pools across the top 5 universities, and plots the event study first for applications to the breakthrough university, and then for the outcomes of receiving an admission offer, accepting the offer, enrolling at the university, and graduating successfully from the university. Enrollment and graduation from the university are mechanically 0 in the pre-period, but the magnitude of the increase in enrollment and graduation is still informative. While enrollment increases less than applications, the increase in enrollment is persistent and is a substantial fraction of the increase in application rate. Furthermore, of those induced to enroll by the breakthrough, almost all students graduate given that the coefficients in the enrollment 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 apply by breakthroughs are mismatched at these universities; most students who are induced to apply go on to graduate successfully.

We can also compare outcomes for students who enroll at a university from a breakthrough school to typical enrollees at the university. To do this, we can simply regress an indicator for enrolling at a university following a breakthrough on student characteristics within each university. We control for university-by-major-by-year fixed effects, so that breakthrough students are being directly compared to the other students on their course. Table 2 provides the results of this comparison. Despite breakthrough students being lower income and having somewhat lower GCSE grades, they are, if anything, slightly more likely to graduate successfully from their course than typical enrollees. They are less likely to receive a first-class degree than typical enrollees, but more likely to receive a 2:1, and their odds of receiving a 2:1 or higher are on par with typical enrollees. Thus this analysis also produces no evidence of direct mismatch; the degree classification results suggest that breakthrough students are around the middle of the performance distribution for their university rather than the top, but there is no evidence that they are failing at high rates.

Given the relatively small magnitude of breakthrough effects in absolute terms – breakthroughs tend to increase applications by around 0.5–1 percentage points – event studies using earnings as the outcome are underpowered.[10] As an alternative, we identify students who apply to the

10. The reduced form effect of breakthroughs on earnings, pooling across the top 10 universities in a difference-in-

breakthrough university following a breakthrough at their school and compare them with untreated students who are matched on observables, as described in section 4.2. Figure 7 illustrates that across both matching schemes (matching with students at control group schools applying in the same year for the same major, and matching with students at the same school in years before the treatment), induced applicants to the top 10 universities have earnings that are several thousand pounds higher than those of their matched controls. By age 27, induced applicants have earnings £4,414 higher than matched controls from control group schools, and £4,003 higher than matched controls from pre-treatment years at their own school (adjusted for inflation). The magnitudes of these effects are large, and broadly consistent with the effect sizes for the top 10 universities presented in Figure 1c, where the average graduate of a top 10 university earns £4,600 more than a student attending a university near the median of the quality ranking. Again, at a minimum, there is no evidence of the students who apply following a breakthrough being made worse off.

5.4 Breakthroughs have strongest effects on students who are more similar to the breakthrough student

Who are the students who respond to breakthroughs? To begin with, 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 in response to a breakthrough. In particular, let a_{ist} indicate whether a student i applies to the breakthrough university. For any observable characteristic X_{ist}, we regress

$$X_{istu}a_{istu} = \alpha_{su} + \gamma_{tu} + \beta a_{istu} + \varepsilon_{istu} \tag{8}$$

instrumenting for a_{istu} with the post-treatment dummy, $I(t - T_{su} \geq \tau)D_{su}$; the resulting coefficient β then estimates the mean of X for compliers. (See for instance Angrist, Hull, and Walters 2023). Table 3 provides the results of this analysis, with comparisons to the mean of each variable in the event study sample, at treated schools before treatment, and among the full set of enrollees at the relevant universities. Compliers who respond to a breakthrough by applying are about as likely to be economically disadvantaged as the typical student at their school, but have substantially higher academic ability. Compared to the typical enrollee at their university, they have slightly lower academic ability but are much more likely to be from low-income neighbourhoods or eligible for free school meals, and are less likely to be white. So the marginal students induced to apply to these universities by breakthroughs are disproportionately high-ability, low-income students, which is exactly the population that policymakers would like to encourage to apply to these universities in order to promote social mobility and reduce undermatching. Breakthroughs serve to diversify the

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.

socieconomic status of the intake at top universities.

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

Table 4 illustrates how breakthrough effects vary by this heterogeneity, pooling across breakthroughs to all of the top 30 universities. In panel (a), we report the difference-in-difference coefficients interacting with the number of shared characteristics, illustrating how shared characteristics in general affect breakthrough effects. We see that there are essentially no effects on applications if a student shares no characteristics with the breakthrough student, and substantially larger effects for students who share more characteristics. In panel (b), we break this out by specific characteristic, recording the difference-in-difference coefficients and the interaction with an indicator for sharing the specified characteristic. Sharing gender, ethnicity, neighbourhood income or FSM eligibility each raises the effect of a breakthrough by around 0.1–0.25 percentage points (around 30–50% of the total 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 more likely to reflect idiosyncratic variation in whether schools offer 17–18 education than factors correlated with ability, but are plausibly a strong proxy for the strength of social connections at the school and interactions with the breakthrough student. The results are at least consistent with an explanation of breakthrough effects in terms of exposure to top universities through the breakthrough student at a school.

5.5 Breakthrough effects imply that 15–30% of the application gap between low-income and high-income students results from differences in past cohorts' enrollment at top universities

We've now established that applications to a top university increase by around 0.5–1 percentage points following a breakthrough, which induces a discrete change in exposure at a school. We also know that low-income students are less likely to be exposed to one of the top 10 universities at their school, as indicated in section 3.3, so they are more likely to be at schools that have not had a breakthrough to top universities and to have commensurately lower application rates. Finally, we saw in section 3.2 that around 40% of the differences in application rates between low-income and high-income students can be explained by lower-income students applying at lower rates. How much of this application gap can in turn be explained by differences in exposure? We can combine estimates of differences in exposure to top universities between high-income and low-income students with our estimates of the treatment effects of breakthroughs. Taking the latter as the causal effect of inducing exposure to a university on applications, we can now conduct a simple back-of-the-envelope exercise to quantify the effect that equalising exposure across income groups would have on application rates. We do not conduct a full counterfactual exercise, but this back-of-the-envelope exercise serves to provide a benchmark for the magnitude of the estimated effects on applications, and the plausible magnitude of effects on 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 no-one 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

$$Pr(\text{apply}_u \mid l, e_u^h) = Pr(\text{apply}_u \mid l, e_u^l) + \Delta_u(e_u^h - e_u^l) \tag{9}$$

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

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

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

$$Pr(\text{apply top 10} \mid g, l, e^h) = Pr(\text{apply top 10} \mid g, l, e^h) + \sum_{u \in \{1,\ldots,10\}} \Delta_u(g) \left(e^h_u(g) - e^l_u(g)\right) \qquad (11)$$

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

Figure 8 plots top 10 application rates for low-income and high-income students by GCSE grades, and the counterfactual application rate $Pr(\text{apply top 10} \mid g, l, e^h)$ calculated as in (11); 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.

6 RCT design

6.1 Mechanisms for the effects of breakthroughs

Our analysis of the quasi-experiments provided by breakthroughs indicates that changes does encourage applications, and that students who respond to breakthroughs to top universities tend to succeed at these universities, graduating at typical rates and seeing higher earnings. Evidence that effects are stronger for more similar students – and particularly that there are substantially stronger effects for students who have attended the same school for longer – suggests that these effects in part reflect exposure: the opportunity to interact with a student who has attended a particular university affects where students choose to apply. But why should students respond to exposure? University choices are made in an information-rich environment: there is detailed information available about universities and courses available from university websites, from online forums and social media, from events run by universities, from school programmes supporting university application, and from university prospectuses and promotional materials. Universities provide relatively clear information about the grades required to be admitted to specific courses. In an environment where students have full information about their own ability, university characteristics, and university admissions, idiosyncratic variation in whether students interact with someone who attended a particular university should not affect application decisions. Why do breakthroughs still affect applications in a relatively information-rich environment?

One potential explanation for this phenomenon is that seeing someone from their own school generically improves students' confidence about their own academic ability and ability to succeed at top universities. But this is hard to reconcile with the lack of evidence of spillovers on applications to other, similarly-ranked universities; an explanation based on confidence should encourage applications to any highly selective university, not just the particular one where students saw exposure. Instead, we propose three potential explanations for breakthrough effects that we see in the observational data consistent with university-specific effects. First, while there is fairly rich availability of information about academic requirements at different universities, exposure to students who have attended a particular university might provide more detailed information that affects students' perceptions of whether they can be admitted to or succeed academically at this particular university. Second, students may stay in touch with students who attended the specific university when they attend it themselves, and this social connection may directly benefit students when they attend the university. Third, even if students do not form a durable connection with past students at the university in question, interacting with students who have attended the university might make the university's student body seem more relatable, improving students' perception of whether they will be able to fit in at the university. All of these mechanisms may have effects that are mediated by similarity between the students – students may respond more to information, be more likely to join social networks, or update more about the social environment at university, when they interact with someone more like them – and we also seek to understand the extent to which this variation matters.

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. We provide exposure to students who went to different schools 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 vary the degree of similarity between students and their mentors, and survey participating students to elicit baseline beliefs and perceptions of universities and treatment effects on beliefs.

We use the experiment to distinguish between these mechanisms. If the different interactions with university attendees that we induce in the experiment provide information that affects students' academic prospects at universities, then these treatments should affect student beliefs about their probability of admission to the university and / or successful graduation from the university as well as their application decisions. Video and mentorship exposure treatments do not involve students meeting other students who they are likely to stay in touch with after enrolling at university, so if these treatments have an effect on applications, this is evidence against connection to social networks being the key mechanism, while if only visits have effects this is evidence that stronger interactions that can create such connections may be necessary to encourage applications. If video and mentorship treatments nevertheless affect beliefs about students' ability to fit in and make friends at university, this is evidence in favour of the mechanism that interactions with past attendees makes students feel they are better suited to the social environment at the school. And if students respond more to exposure to more similar students, this directly provides evidence on whether similarity mediates the effects of each of these three mechanisms.

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 C1 and in the text below.

Active control: workshop about university applications

We invited all students, regardless of treatment arm, who were participating in the programme to attend a workshop about university applications that we organised in their school. The workshop was delivered by a current university student or recent graduate, generally drawn from our pool of mentors (see below for more details on the recruitment and composition of this pool), but in some cases recruited by the school from their alumni. In each case, the presenter or presenters talked through a slide deck that the research team created that provided students with information about the university application process, as well as adding their own commentary based on their experience of university applications and life at university. Information in the workshop was publicly available online, so an interested student could find the same information themselves.[11]

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 workshop served several purposes. Conceptually, providing the information 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*. Having a component of the programme that was available to all students rather than just students in the treatment group was also important for school recruitment, as schools would have been less enthusiastic about a programme that a large fraction of their students would get no benefit from. The workshops served as a focal point for schools' organisation of the programme, acting as a

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.

deadline for completion of the baseline surveys and a centralised point for students to complete their midline surveys in school. Finally, 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.

Videos

Students in the relevant treatment groups were shown two videos towards the end of the baseline 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 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.

We selected videos to show students relating to universities that were aspirational but realistic given their predicted grades. Earlier in the baseline survey, we ask students to enter their predicted grades, and converted them into UCAS tariff points, a metric provided by UCAS to compare grades across different qualifications (see Appendix B.1). We then grouped universities into three tiers based on the distribution of students' grades, as recorded in the public statistics on the discoveruni.gov.uk website. Specifically, universities were assigned to one of four tiers based on the 25th percentile of UCAS tariff points of enrolled students at the university. Table C2 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 table C2 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.

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 C4 describes the characteristics of the mentors taking part in the programme. The majority are current undergraduate students, but a substantial minority were older. The sample of mentors is disproportionately female, whereas our student sample is disproportionately male, but the mentor sample has similar levels of economic disadvantage compared to the RCT sample. As our RCT sample, there is a much lower share of white students in the sample of mentors compared with the broader student population.

Students assigned to the mentorship treatment were connected with 1–2 mentors from this pool. We sent students and mentors an email to connect them, and encouraged them to get in touch to arrange a call to talk about university applications and life at university. We suggested that mentors and mentees could discuss "[mentor's] course, life at [mentor's university], uni life in general, and the application process", but did not provide a script for mentors or prescribe topics, as we wanted to allow for organic conversations and to treat the topics that students chose to discuss as an outcome of interest. Mentors were asked to have at least one 15-minute call with their students, to answer any further questions over email, and to arrange follow-up calls if the student was interested and the mentor was available. Students were also able to ask questions of their mentors over email if they preferred not to have a call.

During the midline survey, 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. Subject to constraints on the capacity of our mentor pool, we connected each student in the relevant treatment arm with a mentor from one of the treated schools. We also identified a second mentor from a less familiar university to connect students with, based on their university tier and the subject that they intended to apply for. In both cases, students in treatment arms 1a and 1b were matched with mentors with whom they did *not* share a gender, ethnicity, or home region of the UK, while students in arms 2a and 2b were matched with mentors with whom they shared at least one of these characteristics. We describe the matching algorithm in full in Appendix Section C.1.

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

Subsidised visits

Finally, for some students, we provide a travel subsidy for visits to a university, motivated by discussions with students in which they discussed how visiting a university before applying was important, but that cost was a barrier. We subsidised costs of up to £75 ($\approx$ \$100); this cap bound for only 35% of submitted reimbursement requests, indicating that this cap covered a substantial share of typical travel costs to universities. For visits, we asked students to nominate a university that they would like to visit in the midline survey (prior to students being informed whether they would be paid), and then offered a visit subsidy to that university to students in treatment arms 1b or 2b. We did not algorithmically assign a visit university to students because of concerns that this would lead to low takeup: even if travel costs are covered, the time costs of a visit to a university are high – generally requiring a full day – and students are unlikely to be willing to do this for a university that they do not have some pre-existing interest in. However, we encouraged students to select a university that they would not otherwise be able to visit in our communications and during the workshop. Students were sent a form where they could submit receipts for their travel to us and claim reimbursement in the form of an Amazon gift card or PayPal payment. Generally, students used these visits to attend organised Open Days, where universities invite prospective applicant to sign up to attend sessions providing details on the university's environment and specific courses.

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

Treatment arms

The treatment arms combined assignments of these treatments. The assignments worked differently in each wave (see below for more details on the timing of each wave). In wave 1, we had a control arm C and a treatment arm T: students in the control arm received only the active control workshop, while students in the treatment arm received videos and mentor connections. In wave 2, we introduced two additional dimensions of treatment variation. First, for students assigned to receive videos and mentors, we varied whether these mentors would be demographically matched (on at least one dimension of gender, ethnicity and region of the UK) or unmatched with the student. Second, a subset of students who were assigned to receive videos and mentors were also offered subsidised visits. This yields the active control arm C and 4 treatment arms: T1a, with videos and dissimilar mentors; T1b, with videos, dissimilar mentors, and visit subsidies; T2a, with videos and similar mentors; T2b, with videos, similar mentors, and visit subsidies. Table C1 provides an overview of the treatment arms in each wave.

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

We pre-registered relevant pooled comparisons between these arms as well as the comparisons of individual treatment arms. Following the discussion in Muralidharan, Romero, and Wüthrich (2025), pooled comparisons should be interpreted as a weighted average of the effects of one treatment averaging over assignment to other treatments – so the pooled comparison of (T2a, T2b) vs. (T1a, T1b) can be interpreted as the effects of demographic match pooling across whether or not the student was assigned a visit. Since many of our outcomes are university-specific and the universities assigned for videos, mentors and visits frequently differ, interaction effects are unlikely to differ across university. Table 6 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 C5 provides counts of the numbers of students who completed different surveys and different treatments, and Appendix Table C6 presents an analysis of differential takeup of the mentorship treatment.

6.3 Outcomes

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

Beliefs are elicited in our surveys on Qualtrics. We ask students the following belief questions (presenting them with emphasis as below):

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

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

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

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

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

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

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

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

6.4 Recruitment and selection of schools for the RCT

We work within a sample of schools in the UK who were recruited for the experiment via our partner organisations, as well as contacts at local authorities. Vertical selection into taking part likely took place on two countervailing dimensions. First, teachers at schools that opted in would need to have been engaged with our promotional materials distributed via WISE, and then be open to putting in additional work to support the programme with the aim of supporting their students' university applications. This would likely select for schools with teachers who are particularly engaged and interested in supporting their students' applications, which will typically be more successful schools. On the other hand, the interventions we provide would be redundant at schools that already provide extensive support for university applications, or where students already apply ambitiously with the support they receive, which would tend to rule out the most successful and most economically advantaged schools.[12]

Figure 10 illustrates the geographic distribution of schools in our sample, overlaid on the map from Figure 3c illustrating the local probability of not being exposed to a top 10 university.[13] We have a cluster of participating schools near Liverpool (in the north-west of England), thanks to a connection with the Liverpool City Region Combined Authority, as well as several participating schools in the Greater London area and others from across England.

Table 1 included summary statistics for the RCT sample alongside summary statistics from the administrative data, but these statistics are potentially hard to compare; the RCT took place four years after the last year observed in the administrative data, so differences between the samples also reflect time trends in average outcomes. To better understand how characteristics of schools participating in the RCT compare to the general population of schools, Appendix Table C3 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 C3 is restricted to university applicants, but this restriction holds consistently in all columns of the table. Results from this analysis largely corroborate the results from the summary statistics in Table 1, though differences are generally less stark: schools participating in our RCT are generally more heavily male than average, are

12. 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 school.

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

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.

6.5 Study timing and waves

We conducted the study in two waves, working with different schools and sets of students in each. Table 5 outlines the timing of different components of these waves. The first wave of interventions took place in Fall 2024 with Year 13 students (those in their final year of high school), and the second wave in Spring – Summer 2025 with Year 12 students (those in their penultimate year). In each wave, after schools opted in to the study, all students in the relevant cohort at the school were sent a baseline survey to complete online via Qualtrics, and encouraged to complete the survey by the teacher we liased with at their school. We used Qualtrics randomisation to assign treatments in this survey, and students in the relevant treatment groups were shown videos embedded into this baseline survey. After students completed their baseline surveys, we conducted an in-school workshop that we invited all students to participate in, regardless of treatment assignment. The workshop was generally led by one of the volunteers from our pool of mentors, though in some cases we worked with the school to find alumni of the school who were able to deliver the workshop.

Students then completed a midline survey immediately after the workshop. Following this, we connected students in the relevant treatment groups with mentors and informed them about how to claim a subsidised visit. Students who completed the baseline survey had their treatment 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. Mentor assignments were conducted using a custom web service that allowed the mentors to be assigned as students completed the midline survey, meaning that we could match students to mentors and elicit beliefs about the mentor universities in the same survey.

We followed up with matched students and mentors over the weeks after being matched by text and email, and in cases where a matched mentor was non-responsive we set up a new match with an active mentor, re-running the same algorithm after removing inactive mentors. We also reminded students about their opportunities to visit universities. In October 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.

7 RCT results

7.1 Exposure to videos increases probability of students intending to apply to those universities

We focus on the effects of the video treatment on relevant outcomes, as we do not yet observe outcomes after mentor calls and university visits. We pool across the different arms in both the fall and spring waves that provided video treatments. We first show 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 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 7 illustrates that once we control for an indicator for baseline stated applications to the video universities, there is a 5 percentage point increase in the probability of listing one of the video universities on the midline survey. Relative to a baseline mean application rate (to either university) of 18.3%, this constitutes around a 30% increase in the probability of applying to one of the video universities, which is very similar to the percentage effect of breakthroughs that we estimate in the administrative data. The much higher baseline application rate compared with the application rate to a given university in the administrative data reflects the facts that videos are tailored to students' ability, and that the application rate is the share of students who apply to either one of the two assigned video universities.

Is this just driven by a short-run salience effect? Students completed the midline survey a median of 4 days later than the baseline survey, so the students in question were listing this university in our survey multiple days after seeing the video for the university. Furthermore, if we condition on taking the midline survey 4 or more days after the baseline, and similarly regress applications at midline on applications at baseline and the video treatment (as in specification (2) of table 7), we find a nearly identical point estimate of 0.052, although the estimate is no longer statistically significant ($p = 0.121$). So exposure to videos has effects that seem to persist at least for several days. We will be able to validate whether they persist over a longer period when we collect endline survey data and final application outcomes. However, video exposure does not seem to raise the probability of students requesting mentors from either exposed university ($\beta = 0.014, p = 0.507$) or requesting a subsidised visit to either of these universities ($\beta = 0.017, p = 0.309$).

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

7.2 Social beliefs become more optimistic following video exposure

In the baseline survey, we ask students for the probability that they will 'make friends and fit in at', 'get an offer from', or 'graduate successfully from', different universities: the university that they are assigned a video about (which is an academically aspirational university given the student's predicted grades), the University of Oxford (a well-known university with an elite and selective reputation), and the student's top choice at baseline. We also elicit students' postcodes, enabling us to link to the neighbourhood income measures that are also available in the administrative data. Focusing on beliefs about Oxford as a proxy for beliefs about elite universities in general, we find that first-generation university attendees (students whose parents did not attend university), low-income students, and female students all have more pessimistic beliefs about their probability of fitting in and making friends at Oxford than their counterparts with similar grades (we control for A-level tariff points in the regressions). However, we do *not* see similar effects 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. We do not see the same pattern for ethnicity, where non-white students are somewhat more optimistic about receiving an offer than their white counterparts; this is consistent with other results from the British context that indicate ethnicity is not as strongly associated with economic disadvantage and poor university outcomes as it is in the United States. To the extent that these results generalise to the broader population, they 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.

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

Table 8 illustrates that we do not find *average* treatment effects on any of the belief variables, controlling for baseline beliefs. However, students exposed to the video are directionally more likely to update positively, and less likely to update negatively, about their social beliefs, while we see no such effect for beliefs about offer or graduation probability. So we find precise null effects of videos on beliefs about admission or graduation probability, while social beliefs tend to update positively in response to a video. In combination with the result that low-income students' baseline pessimism about elite universities also relates to social beliefs not admission or graduation beliefs, these results strongly suggest that exposure to universities can encourage applications by improving students' perception of their social fit at particular universities.

7.3 Students respond more to a video from a student with a different gender

Who is most affected by videos? Since students were shown a video from one student of each gender, we can evaluate whether the treatment effect of the video was stronger for the gender-matched video. Surprisingly, we find the opposite pattern: students consistently respond *more* to videos recorded by students with a *different* gender. Table 9 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 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 given that in section 5.4 we find a positive interaction effect of gender match between the breakthrough student and the induced student. Given that videos have most effects on social beliefs, however, it's not implausible that students would react more to interactions with the opposite gender; college plays an important role in marriage markets (Kirkebøen et al. 2021), which may mean that (heterosexual) students infer more about aspects of the social environment that they care about when they hear about the experience of the opposite gender.

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

7.4 Students tended to ask mentors about social life at universities

In our video treatments, we prescribed a fixed set of topics for students watching the videos to include in their discussion, so that video content was relatively standardised. By contrast, we did not prescribe topics for calls with mentors. As described in section 6.2, in our emails connecting students with mentors, we told them that they could discuss "[mentor's] course, life at [mentor's university], uni life in general, and the application process". We did not prescribe topics to discuss beyond this. When recruiting mentors, we described the programme in terms of evaluating ways to support access to university, but did not discuss specific mechanisms that we were interested in testing. The topics that students discuss with their mentors thus reflect what students are most interested in learning from past university attendees and choose to discuss with them. The topics of mentor conversations are incentivised in the sense that students are not able to learn information about a particular topic from mentors without asking them about that topic. This avoids potential demand effects that would arise if we directly ask students what they are considering in their university applications, as students may think they are 'supposed' to decide based on factors like course content and teaching quality that are usually reported in university guides, and respond with these answers.

In Table 10, we report the topics that students discuss with mentors, as reported by mentors in a survey. We ask mentors to select the topics from a multiple-choice list that they discussed with each of their mentees; the topics selected are reported in panel (a). The two most commonly discussed topics are student life at the mentor's university (70% of conversations) and life at university and fitting in (68% of conversations). Thus in these organic conversations students are most likely to want to discuss aspects of social fit, substantially more than advice about choosing applications (53%), careers after university (36%), or how to succeed academically at university (30%).

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

Taken together, these results indicate not only that students were likely to talk about social life and fitting in with mentors relative to other topics, but that mentors thought what they learned from this would affect their application choices. This is consistent with the results from section 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.

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 student can be explained by differences in exposure given the effects of exposure that we estimate in our analysis of breakthroughs.

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

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

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

From the government's perspective, findings like those in this paper might justify a rule like the Texas Top 10% policy, wherein the top 10% of graduating students at any school are guaranteed admission to state universities; admitting a high-achieving student from a school with little history of sending students to that university has positive externalities by encouraging 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 – could help to close gaps in applications.

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

9 Exhibits

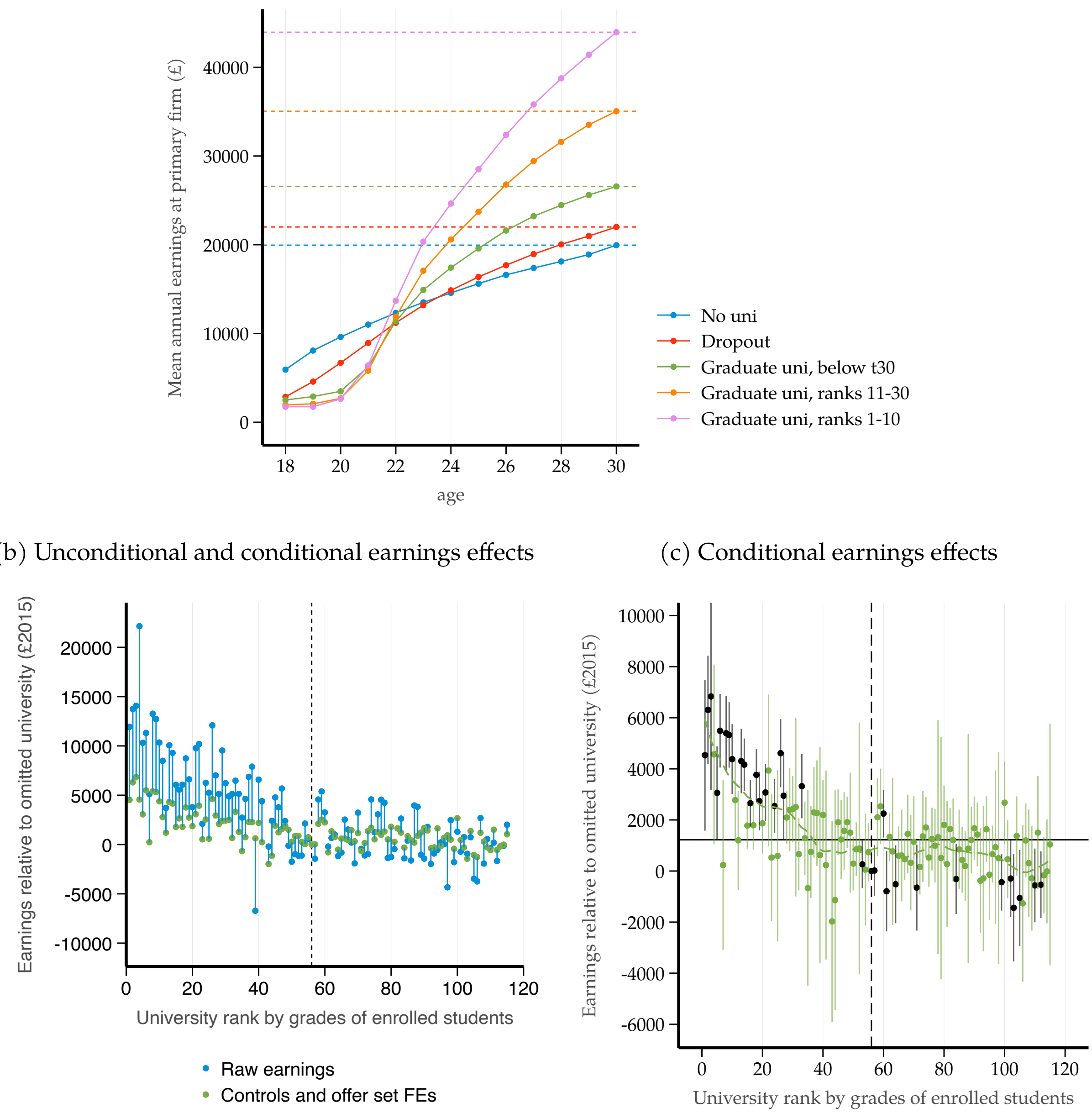
Table 1: Summary statistics

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

Notes: Summary statistics comparing outcomes in different subsets of the LEO data and the RCT.
Data: LEO (columns 1–3), RCT (column 4).
Samples: KS5 school leavers are all students who attend school to age 18 – we exclude anyone who leaves full-time education before this date. University applicants are students who have a link to UCAS and apply to at least one university in HESA. The event study sample is students who are at a school that either experiences a breakthrough to, or is in the control group for, at least one of the breakthroughs to different universities. The RCT sample is drawn from students who completed both the baseline and midline survey, our primary sample for most analysis in the RCT data.
Variables: 'Low-income neighbourhood' is defined as a student's home postcode being in the bottom 40% of neighbourhoods as ranked by the Index of Multiple Deprivation. A-Level tariff points are a standard conversion of letter grades into a 0–56 numerical metric; we take the top 3 grades for each student, so the maximum possible tariff points is 168.
[†] Data on parental university attendance in LEO is only available for students who themselves attend university.

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

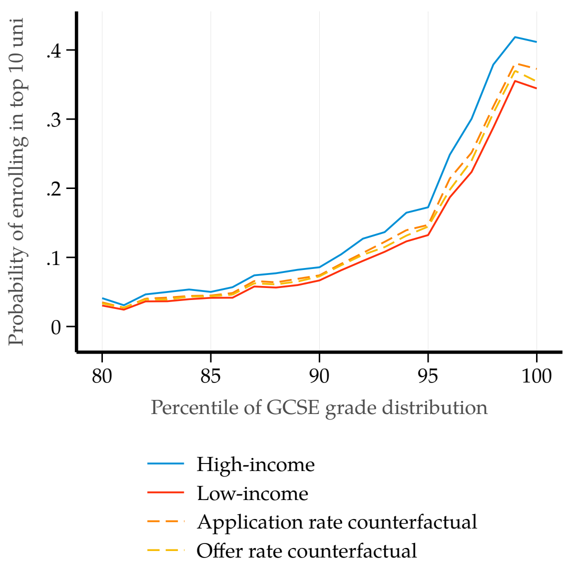
(a) Earnings trajectories by age



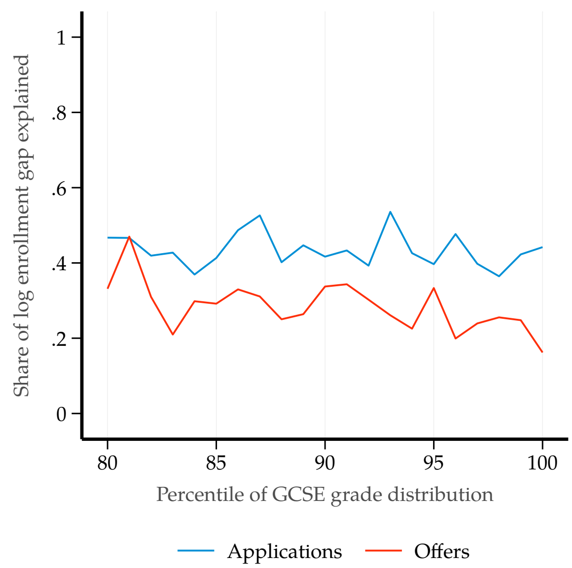
Notes: Estimates of the earnings return to different universities. Data: LEO. All earnings are in GBP and adjusted to inflation in 2018. Panel (a) plots mean earnings conditional on age and university outcome. We restrict to students starting a degree at age 18; dropouts are those who do not earn a degree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

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

(a) Actual and counterfactual enrollment rates



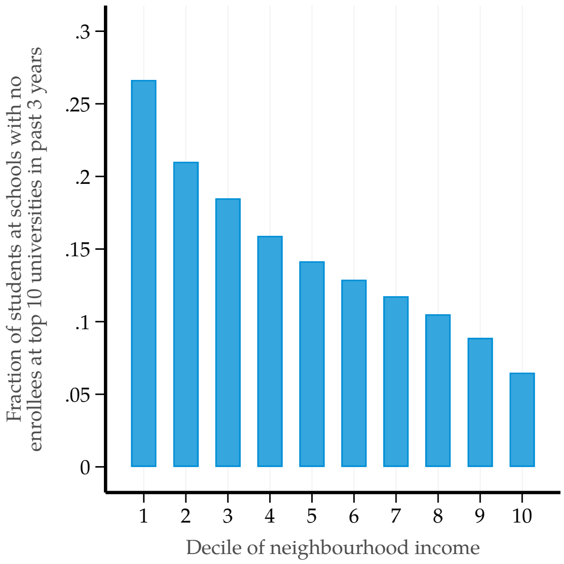
(b) Share of enrollment gap explained



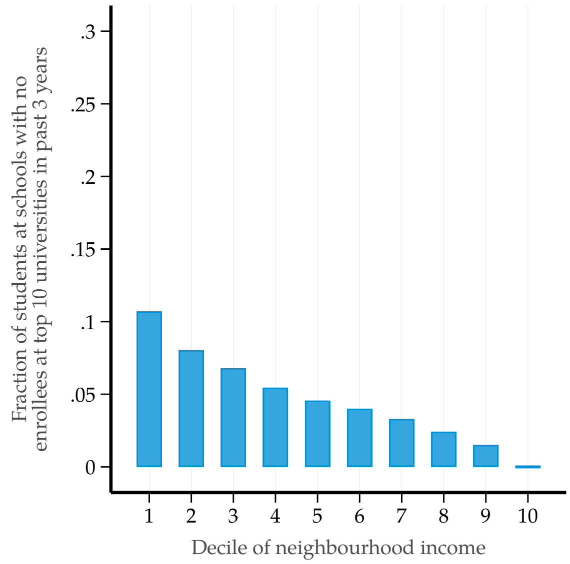
Notes: Data: LEO. In panel (a), solid lines plot probability of enrolling at one of the top 10 universities conditional on percentile in the national GCSE grade distribution (truncating at 80 since applications to top universities are very low below this rate), and conditional on enrolling through the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

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

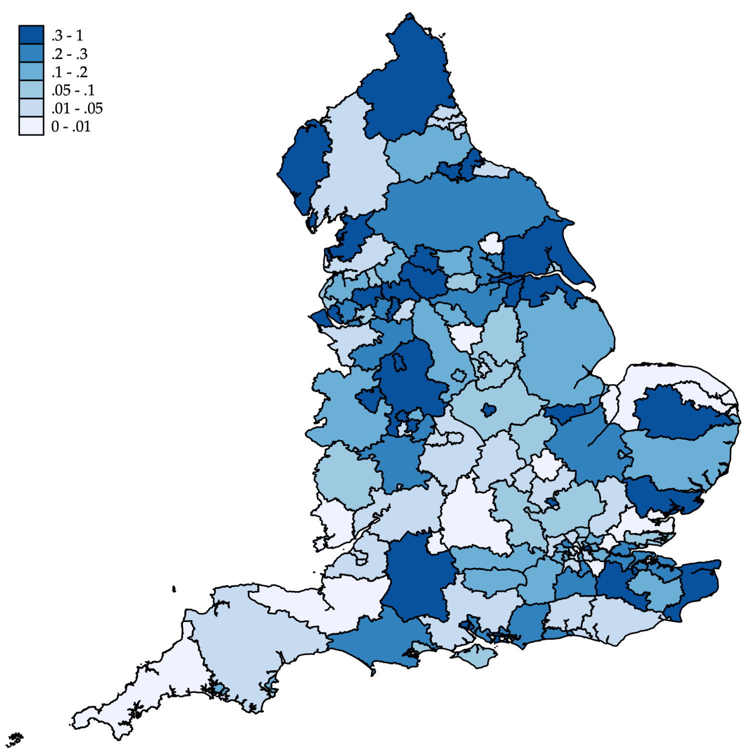
(a) Unconditional non-exposure rate



(b) Conditional non-exposure rate

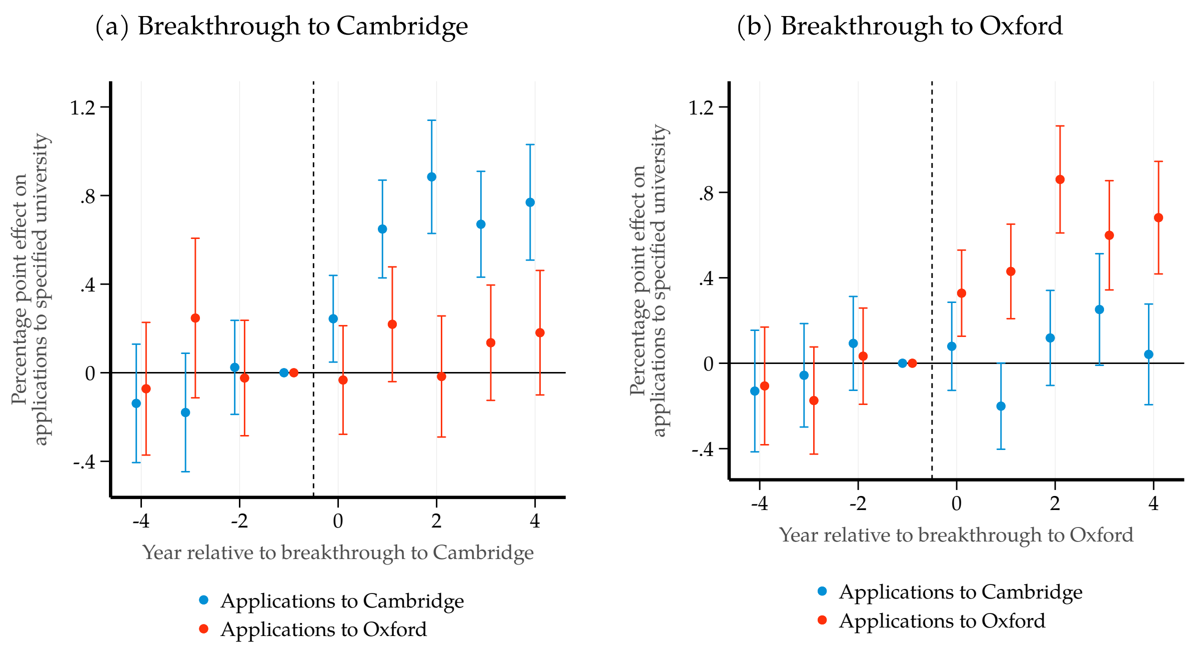


(c) Unconditional non-exposure rate by region of England



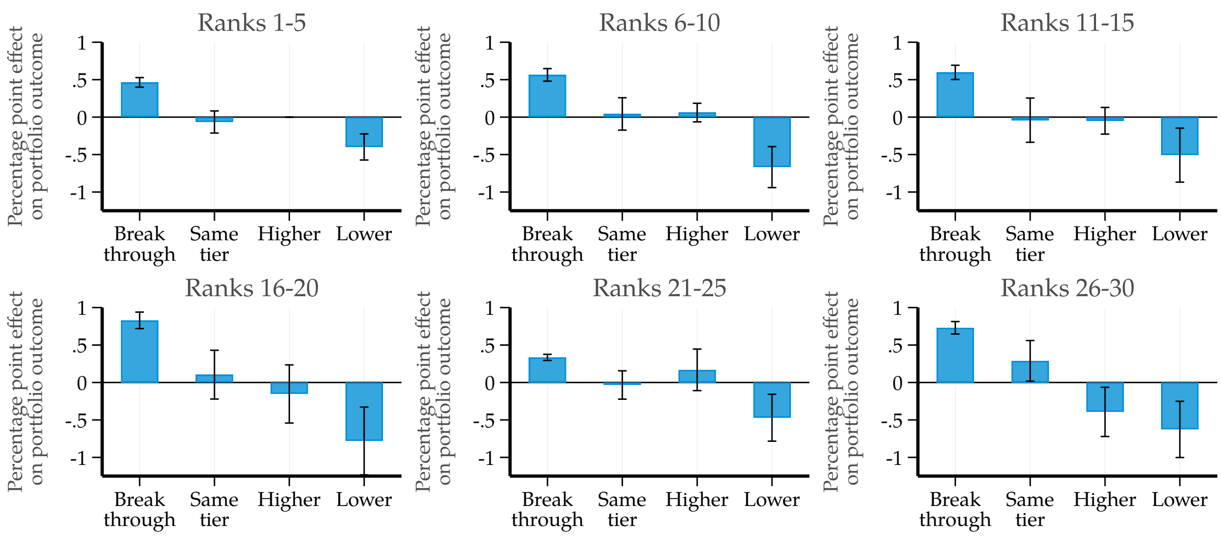
Notes: Data: LEO. Unconditional specification in panel (a) reports the share of students attending high schools that sent no-one to a top 10 university in the preceding three years, by decile of the student's neighbourhood income; lower deciles are more deprived. Conditional specification in panel (b) reports regression coefficients from a regression of the same outcome on IMD decile and controls for gender, ethnicity, free school meal eligibility, and GCSE grades, with the coefficient on decile 10 normalised to 0. Panel (c) plots the share of students in each NUTS3 region of England who attended high schools that sent no-one to a top 10 university in the preceding three years, as of 2018. All figures are at the individual level, as there is variation in neighbourhood income decile by school.

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



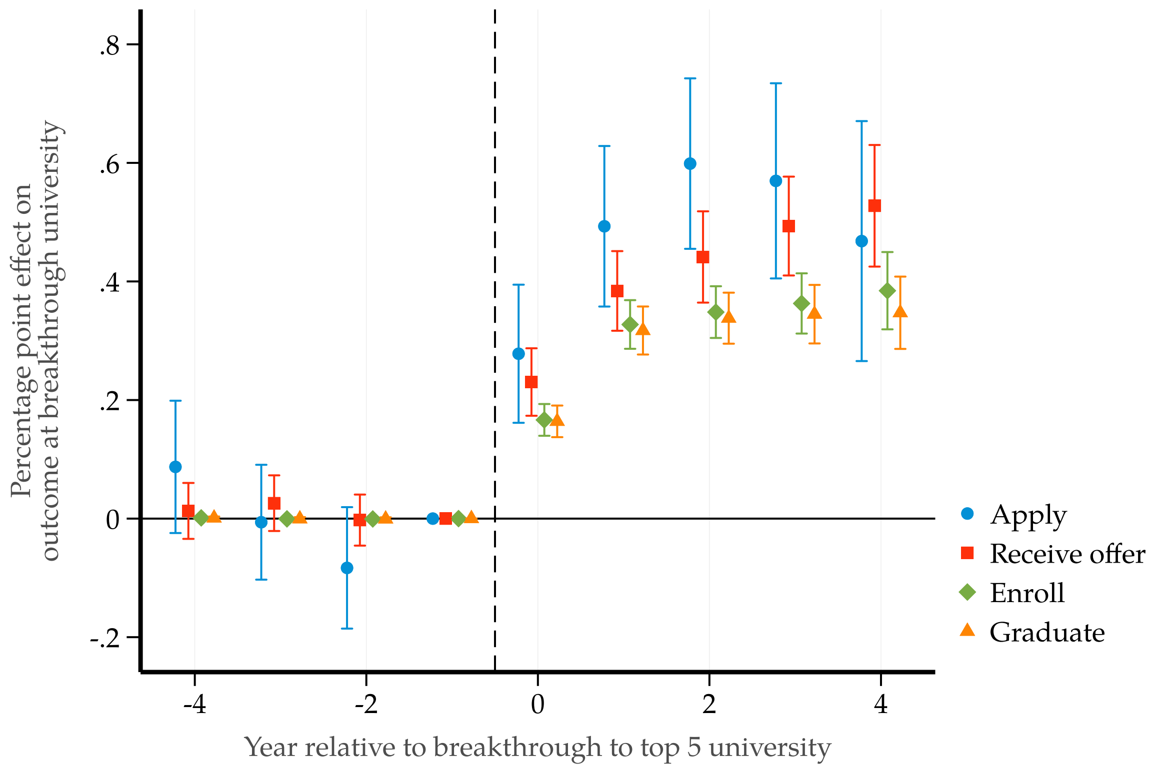
Notes: Data: UCAS. Coefficients from an event study of breakthroughs to specified university using the Sun and Abraham estimator, where the outcome is applications to Cambridge or to Oxford as specified. Coefficients are multiplied by 100 so they can be interpreted in percentage point terms. Regressions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

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



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

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



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

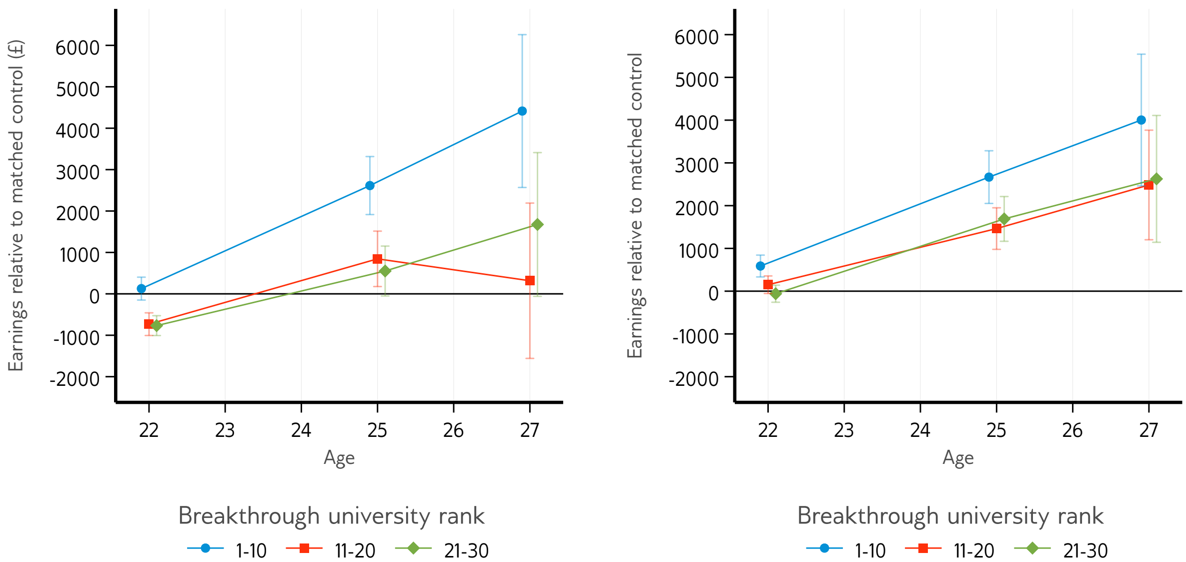
Table 2: Outcomes for breakthrough students to top 5 universities relative to their course

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)

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

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

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



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

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

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

Notes: Comparison of mean characteristics for compliers – treating breakthroughs as an instrument for applications to the breakthrough university – with the population of all students who enroll at the breakthrough university (in column 2), for all students in the analysis sample for event studies (in column 3) and for all students at treated schools in the analysis sample (in column 4). Characteristics for compliers are estimated as in equation (8); other characteristics are raw means within the specified sample. 'FSM eligible' denotes eligibility for Free School Meals, a proxy for disadvantage. 'Low-income' denotes students in the poorest two quintiles of our neighbourhood deprivation measure.

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

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

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

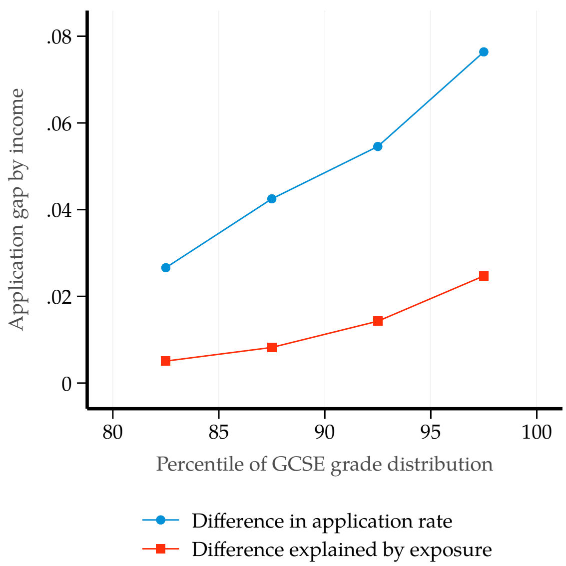
(b) Difference-in-difference coefficients for each component of similarity index

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

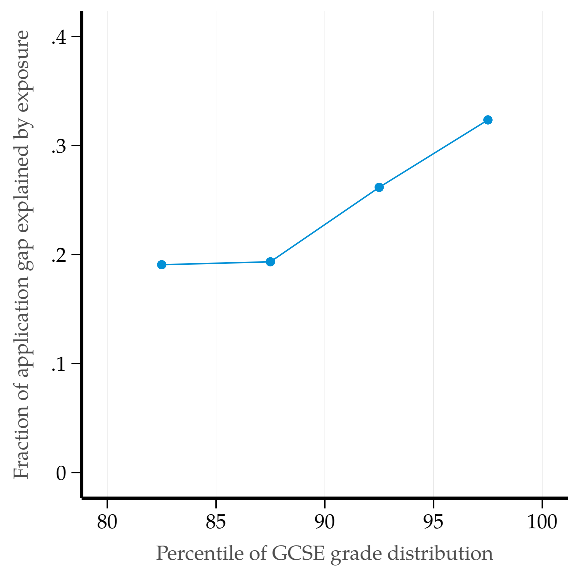
Notes: Data: LEO. Coefficients from difference-in-difference regressions estimating the effect of breakthroughs on applications to the breakthrough university, interacting the Treated × Post indicator with measures of similarity between each student in the sample and the breakthrough student at their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

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

(a) Total application gaps



(b) Share of application gap explained by exposure



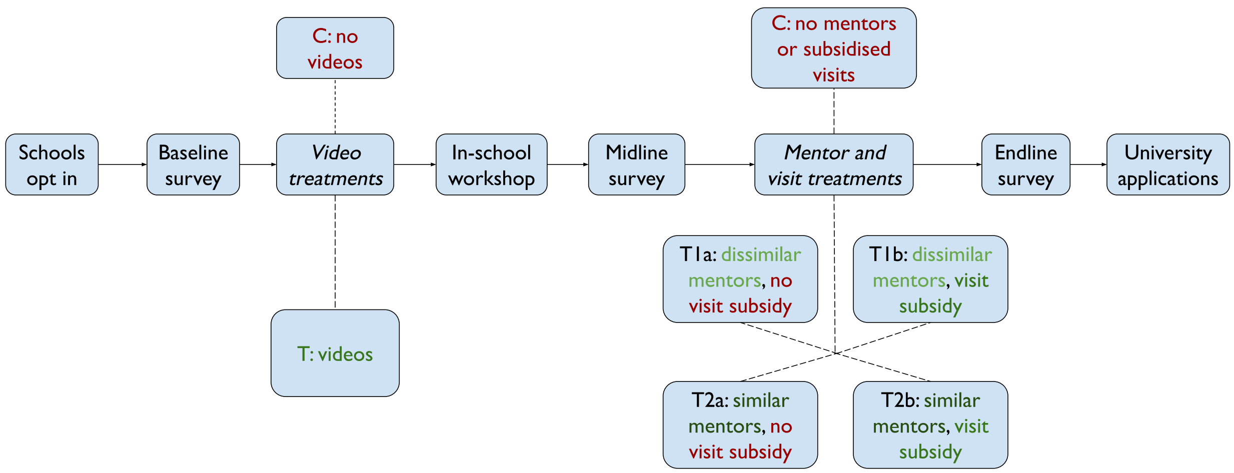
Notes: Difference in application rate to top 10 universities between low-income and higher-income students, and the amount of this that can be explained by exposure. We calculate treatment effects of breakthroughs on applications conditional on GCSE ventile for each university, $\Delta_u(g)$, and then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

Table 5: Timelines for each of the experimental waves

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

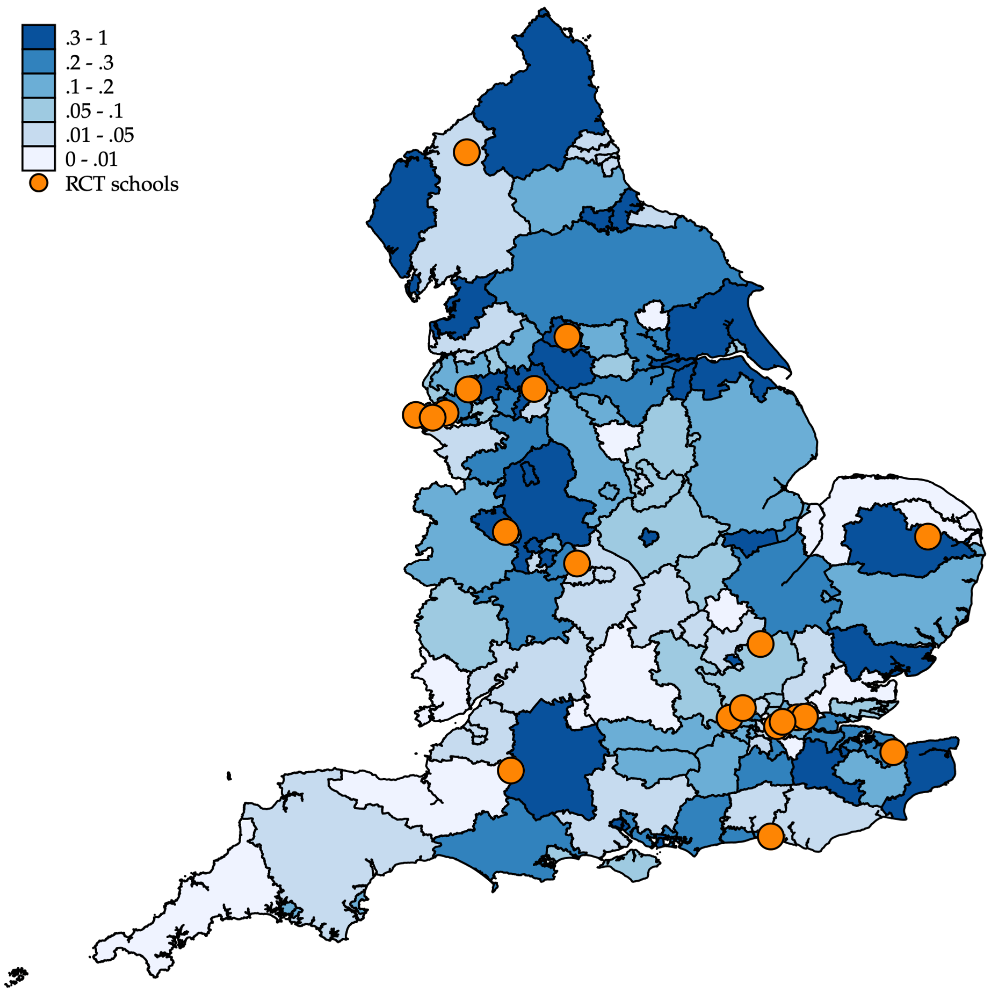
Notes: Timing of experiment and outcomes in Fall 2024 and Spring 2025 waves.

Figure 9: Overview of RCT design



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

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



Notes: Map of the locations of schools participating in our RCT. Each orange dot represents a participating school. The underlying map plots the share of students in each NUTS3 region of England who attended high schools that sent no-one to a top 10 university in the preceding three years, as of 2018.

Table 6: RCT balance table

Variable	Control mean	Treatment	N
Female	0.463	-0.041	805
	(0.025)	(0.035)	
Low-income neighbourhood	0.338	0.054	788
	(0.024)	(0.034)	
Parents attended university	0.520	0.021	805
	(0.025)	(0.035)	
White	0.562	-0.006	805
	(0.025)	(0.035)	
Black	0.094	-0.027	805
	(0.015)	(0.019)	
Asian	0.233	0.024	805
	(0.021)	(0.030)	
Northern England	0.374	0.005	805
	(0.024)	(0.034)	
The Midlands	0.032	-0.010	805
	(0.009)	(0.011)	
Southern England	0.347	-0.007	805
	(0.024)	(0.033)	
London	0.223	0.022	805
	(0.021)	(0.030)	
Taking ≥ 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)	

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

Table 7: 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)
Ethnicity controls	No	No	Yes
N	701	701	701
Baseline mean	0.183	0.183	0.183

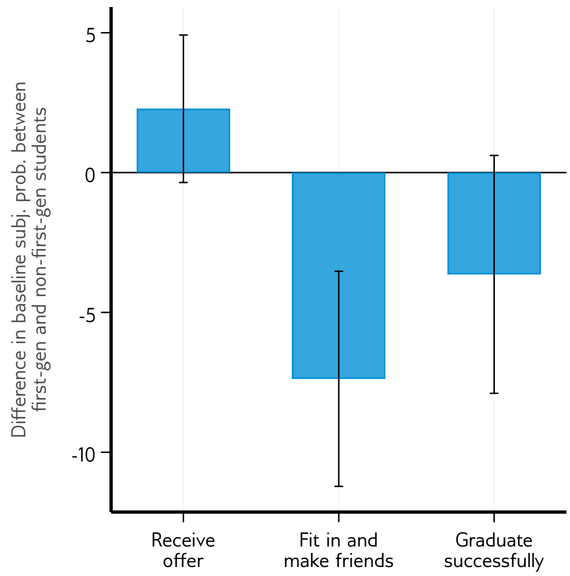
Standard errors in parentheses

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

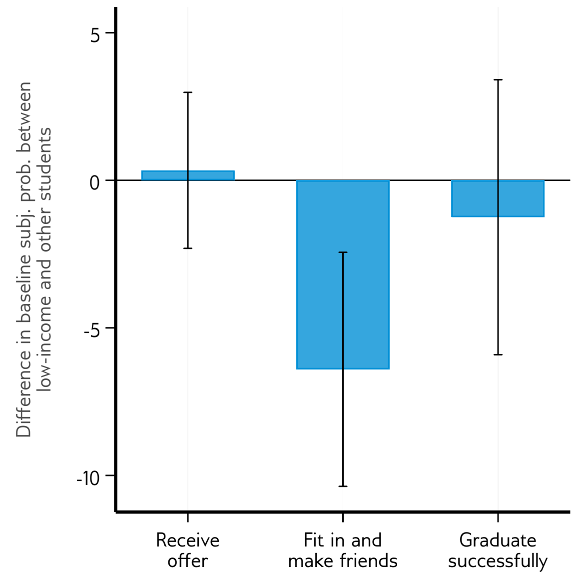
Notes: Estimates of treatment effect of videos on intended applications. Students are assigned two videos about different universities regardless of their treatment status; outcome in the regression is an indicator for listing either of these universities as one of the five they intend to apply to in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

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

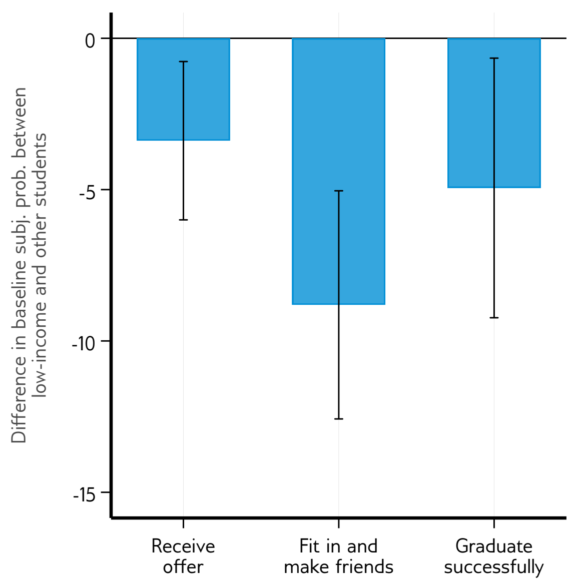
(a) Coefficient on first-generation indicator



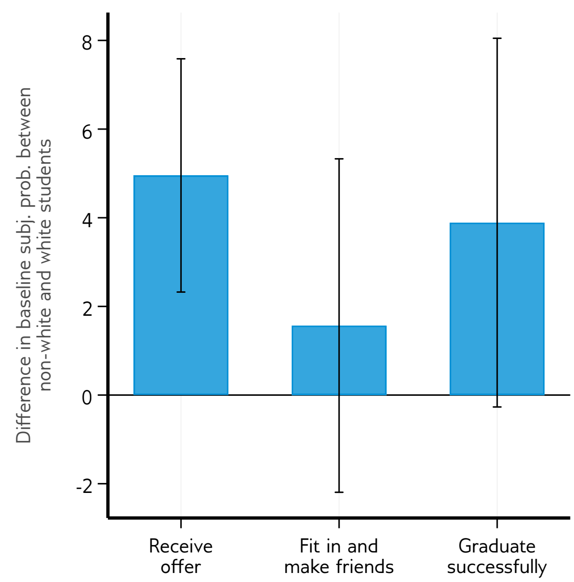
(b) Coefficient on low-income indicator



(c) Coefficient on female



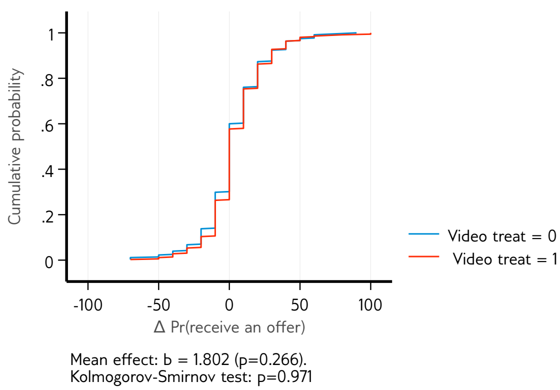
(d) Coefficient on non-white indicator



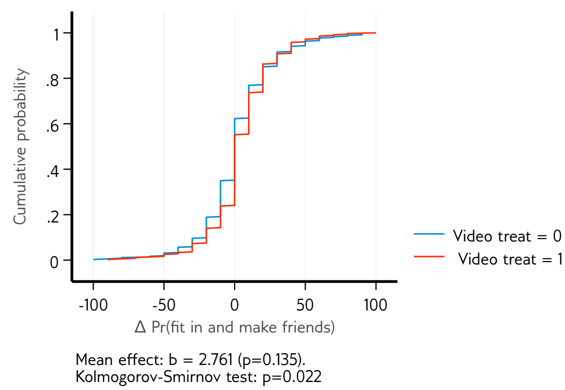
Notes: Data: RCT. Outcomes are subjective probabilities, between 0 and 100, of 'receiving an offer from', 'fitting in and making friends at', or 'graduating successfully from' Oxford University. We regress each outcome on the specified indicator and a control for the student's predicted A-level tariff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

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

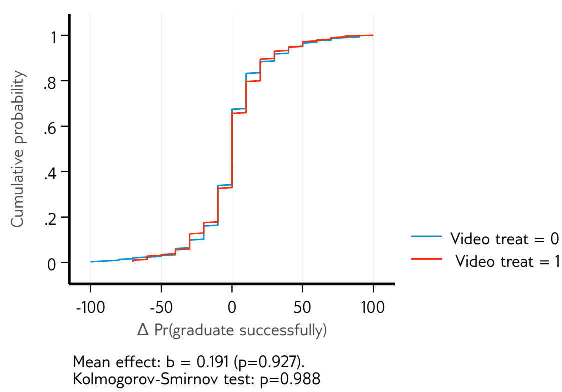
(a) Probability of receiving an offer



(b) Probability of fitting in and making friends



(c) Probability of graduating successfully



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.

Table 8: Treatment effects on belief updating and direction of belief updating between baseline and midline surveys

(a) Beliefs about offer probability

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

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

(b) Probability of fitting in and making friends

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

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

(c) Probability of graduating successfully

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

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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

Table 9: Responses to videos by gender

(a) Gender heterogeneity in video effects on applications

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

Standard errors in parentheses

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

(b) Gender homophily in video effects on beliefs

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

Standard errors in parentheses

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

Notes: Treatment effects of videos on applications and beliefs, separately by match between the student's gender and the gender of the individual in the video. In panel (a), we regress an indicator for applications to either the university in the matched-gender video, or the university in the unmatched-gender video, 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 10: Mentor reports of conversations with students

(a) Topics discussed with mentors

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

(b) Factors affecting students' decision-making

Factor	Mean importance (1–5 scale)	Share of conversations where topic was most important (%)
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

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

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

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

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

B Administrative data

B.1 Definitions of constructed variables

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. GCSEs are given letter grades (A* – G) until 2017, after which the grading switched to a 9–1 numerical scale. To create a comparable measure across time, we 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*).

B.2 Supplemental tables

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.

C RCT design

C.1 Algorithm for mentor assignment

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

We seek to identify a tier-based and / or a preference-based mentor for each student. 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 then restrict to universities in the same tier that the student was assigned to (see Table C2). Next, we try to match on students' majors. We start by looking for a match on the exact major; if there are no available mentors within the exact major, we then use a more aggregated definition of major, and if there are still none then we take all mentors within the remaining universities. We then assign this mentor as the tier-based mentor; if there are multiple available mentors suitable to be matched, we take the mentor with the highest remaining capacity.

C.2 Supplemental tables

Table C1: 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 C2: 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 C3: 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 C4: Characteristics of mentors

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

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

Table C5: Student counts in experiment

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

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

Table C6: Takeup of mentorship treatments by demographics

	(1)	(2)	(3)	(4)
			Any contact	Any contact
	Called mentor	Called mentor	with mentor	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 fro whether the student has requested a visit reimbursement, restricting to students assigned a visit subsidy. Standard errors are robust to heteroskedasticity.