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Research Article

Undergraduate student messengers: Reinforcing young people's higher education ambitions?

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Abstract: This study draws upon previous research to establish if '*low cost, high volume'* university outreach interventions change subsequent application behaviour. The results of two distinct but related randomised controlled trials with two-armed designs (RCT 1, n=2,199; RCT 2, n=1,166) compared application outcomes between recipients and non-recipients of messages from existing undergraduates. The research sought to determine if undergraduate student messaging reinforced prior exposure to the University outreach programme and thereby influenced recipient behaviour, in terms of applications and acceptances to that specific institution.
The first trial found moderate statistical evidence that sending an email, written by and addressed from an existing undergraduate, to prospective applicants resulted in the opposite of the intended effect; reducing the rate of applications to the University. The second trial found no statistical evidence of any difference in application or acceptance rates amongst the treatment cohort, who received a personal letter in the post from two current undergraduate students, in comparison to the control group who received no correspondence. This reinforces the notion that there is no '*one size fits all*' programme of widening participation interventions; successful 'messaging' is not necessarily transferrable, and can even backfire, given different characteristics of activity providers and recipient cohorts.

Keywords: Widening participation, Outreach, Nudges, Higher education, Role models

Supplements: Open materials

Introduction

Despite two decades of UK sector policy interventions aimed at narrowing higher education (HE) participation gaps between under-represented groups and their more advantaged counterparts, there remains a dearth of causal evidence of the impact of widening participation interventions (Robinson and Salvestrini, 2020).[1] By definition, some of the most intensive outreach activities are '*low volume, high cost*', rendering experimental evaluation design problematic. '*Low cost, high volume'* outreach, however, delivered via role model or trusted messenger communications as part of a wider package of interventions, is ideally suited for experimental research.

A recent UK experimental study found that communications written by current undergraduates to potential applicants significantly increased the recipients' chances of applying to a specific group of research intensive universities, as well as increasing their chances of receiving and accepting offers from those institutions (Sanders et al, 2019). These findings were consistent with a similar US experiment (Hoxby and Turner, 2013). Yet, whilst these studies demonstrate the opportunities for meaningful impact of high-volume, low-cost outreach, these

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nudges were characteristically context specific. They focused on increasing applications and enrolments to more 'selective' institutions or groups of institutions using letters written by students. Crucially, these studies have found little evidence of impact across the HE sector as a whole. To date, there have been no known comparative studies in the UK with 'less selective' institutions, so it is not yet known if such methods of nudging are transferrable.

This study adds to the gap in the research relating to trusted messenger communications and the associated influence on HE applications. Similar messaging experiments were undertaken to those in the aforementioned previous studies. However, both the recipient cohorts, *and* the type of university the messengers were affiliated to, differed considerably from previous and comparable studies. More specifically, our messengers were existing undergraduates studying at a 'post-92' modern university[3], designated 'medium tariff' status by the UK's University and College Admissions Service (UCAS)[4] based on the average pre-entry qualification grades of its undergraduate intake. Invariably, this manifests a different socio-economic and demographic student profile to more 'selective' universities (Schwartz, 2004).

In both of our trials, the research participants had previously engaged with the university through its outreach programme, which specifically targets and works with disadvantaged young people. Therefore, our experiment can be seen as an additional nudge to serve as a reminder of this prior experience, and specifically considers the impact of nudging disadvantaged learners. Whilst offering similar messaging to previous research, therefore, our study is very different in terms of context. Nevertheless, if the findings of the aforementioned previous studies held for our specific experiments, this would support a larger scale roll-out of such minimal resource-intensive interventions, to further test their efficacy.

Nudging interventions and trusted messengers

It has long been established that a perceived lack of role models has acted as a major deterrent to the educational expectations of young people from disadvantaged backgrounds (Marchbank, 2002). The deployment of undergraduate role models can present a low cost means of enhancing the equality of opportunities for young people from disadvantaged backgrounds to enter HE (Wilson et al, 2014; Castleman and Lindsay, 2015) and our studies apply the same thinking to deploying trusted messengers. Whilst 'multiple agents' contribute to decision-making (Sanders et al, 2019), it has been suggested that individuals are more likely to respond positively when information is shared by credible others, often similar in age and other socio-economic characteristics to themselves (Dolan et al, 2010). The person from whom the information is communicated, is therefore key to a successful nudge.

Evidently, we cannot assume that the mere involvement of trusted messengers in nudging activities provides the 'magic bullet'; as Graham et al (2017: 43) assert, interventions should be "***more imaginative than simply assume the sending of reminder emails***." Nudges may have heterogeneous effects, and universal adoption may be inappropriate, in the absence of context and location specific evidence. Indeed, an intervention that results in positive outcomes for some, may even backfire and result in negative effects for others (Damgaard and Nielsen, 2018).

Research Aims

The studies we detail below are rooted in the question that, if messages were delivered by undergraduates of a different university to different cohorts and tweaked accordingly, would this demonstrate comparable results? It is important to acknowledge that experiment 2 is not a reaction to experiment 1. Instead, these are two independent studies that ask whether different methods of communicating with potential students lead to different outcomes: experiment 1 focuses on emails, and experiment 2 focuses on letters. These experiments seek to determine if messages from existing undergraduates reinforced their prior exposure to the University of X's outreach programme and thereby influenced recipient behaviour, in terms of applications and acceptances to that institution. In addition to being different modes of communication, in experiment 2, consistent with previous UK research (Sanders et al, 2019), two letters were distributed to the treatment group, whereas in experiment 1 a single email was sent. This permitted the effects of different, independent modes of communication to be explored, with a view to expanding the learning gleaned in future experiments.

Experiment 1: Email to former year 12 participants of University of X's Outreach Programme

In this randomised controlled trial, we asked a final year undergraduate student of the University of X to compile an email to describe in their own words their transition to HE, noting some of their experiences whilst studying for their undergraduate degree. Whilst the specific University was named, and the city in which the University was located was positively described, the messaging was to be of subtle and honest encouragement, and not designed to overly 'sell' the University. The email, which included the recipients' first names in the introduction (Sanders & Kirkman, 2019), was sent to the treatment group in September 2017, when they were scheduled to have started their second year of post-compulsory education and aged 17 or recently turned 18. This date was chosen to coincide with the University's upcoming Open Days, being a period when students are typically thinking about applying to HE. A transcript of the email is provided in Supplement 1.

Method

Identifiable data relating to 16-17 year old (year 12) participants of the University of X's 'post-16' outreach programme delivered in the 2016/17 academic year were recorded on a secure database, with participant consent received for subsequent tracking purposes. A total of 2,198 unique participant records were randomised into two groups and stratified by the school/college the student attended, as the last institution attended is known to be a key influencer on the type of HE provider a student applies to (Schwartz, 2004). Although, due to the existing data protection protocols employed, additional potentially influential data fields were limited for this study, two additional covariates that were known to influence HE progression (Office for Students, 2020; Higher Education Access Tracker, 2020) were pre-determined and included in the subsequent statistical modelling (Appendix 3):

- ACORN category (ordinal, based on participant home postcode; ACORN is a commercial geodemographic tool that segments the UK population into five socio-economic categories).
- Location of activity (factor, based on whether activity was held on campus or off-campus).

A random number generator was applied to determine which group in the two-armed design each research participant was allocated to. The treatment group (n=1,099), who were to receive the email, and the control group (n=1,099) who would receive no communication, were split 50/50.

All participant email addresses (both treatment and control) had been pre-verified, with the treatment group added to a distribution list. According to the University of X's records, there were zero errors reported in sending. Whilst it was possible to track which participants of the treatment group subsequently opened (and assumed read at least some of) the email, this was deemed irrelevant to the study. A recipient who already has a pre-existing interest in the institution will be more likely to proactively read the information provided (Wainer et al, 2011), and this group cannot be compared with a sub-sample of the control group. Therefore, an intention to treat analysis was adopted and thus every research subject in both the treatment and control group was tracked in terms of applications and acceptances to the University of X for the 2018/19 admissions cycle. This also further ensured that the initial prognostic balance generated from the original treatment allocation was maintained (Gupta, 2011).

Results

Data were analysed in Genstat 20th edition statistical software. Our first result of interest was a comparison of application rates to the University of X between our treatment and control group (Table 1a). Column 1 presents the marginal effects from a logistic regression model, regressing application outcomes on the treatment/control binary variable.

We find moderate statistical evidence (p=0.014) against the null hypothesis of no difference in application rates between treatment and control. However, perhaps unexpectedly, the effect size was negative. The addition of the covariates (Column 2) made little difference to the effect size or the statistical significance (p=0.015). Table 1b reports average application rates to the University of X for both treatment and control, with 95% confidence intervals either side. We find that the treatment was associated with an average application rate of 19.1%, compared with 23.4% control; a difference of 4.3 percentage points.

Our next result of interest was the conversion of applications into acceptances; i.e. the percentage of applicants who subsequently converted their application to the University of X, to an unconditional firm place at the institution (which effectively means they are expected to enroll as a full-time student). This time, as shown in Appendix 1a, we see no evidence against the null hypothesis of zero difference in conversion rates; both with (p=0.93; Column 2) and without ((p=0.86; Column 1) covariates included. Appendix 1b confirms similar average application conversion rates between treatment (21.9%) and control (22.2%).

By combining the application and conversion rates described above, we can estimate the percentage of the former outreach participants who effectively became a full-time undergraduate at the University of X[5]. We can see in Appendix 1d that the average 'accepted applicant' rate for the treatment group was 4.1%, compared with 5.3% control, although this was not statistically significant (p=0.21), due to a wider margin of error.

Table 1a
Logit regressions of applications to the University of X on treatment allocation (Experiment 1)

	Dependent variable: Applied to University of X	
Variables	(1)	(2)
Treatment: Student emailed	-0.256*	-0.254*
	(0.104)	(0.105)
ACORN classification		-0.077
		(0.034)
Location: On campus		0.141
		(0.185)
Constant	-1.187***	-0.970***
	(0.071)	(0.121)
Observations	2,198	2,198

Note: Standard errors in parentheses; *** p<0.001; ** p<0.01; * p<0.05

Table 1b
Average Application Rates to University of X and 95% Confidence Intervals by Treatment (Experiment 1)

	Average Application Rate	Lower bound CI	Upper Bound CI
Control	23.4%	21.0%	26.0%
+ Email to prospective applicant	19.1%	16.9%	21.5%

Discussion

The results of our first experiment took us somewhat by surprise. Whilst we were not necessarily expecting applications to significantly increase amongst the treatment group, we did not expect the intervention to backfire, as our results suggest. However, nudging interventions backfiring is not unprecedented. One recent study found strong evidence that a 'social norms' intervention, which previous studies had found to foster desirable behaviour towards the norm out of fear of non-conformity, actually resulted in the opposite effect (John and Blume, 2018). As John and Blume suggest, a single trial can result in unintended consequences that may not hold through repeated experiments. Further similar trials are therefore warranted before we make rash judgements about their efficacy. However, at this juncture, it is worth exploring why the sending of emails may have backfired in this particular case.

First, we cannot assume that studying at the University of X was this particular cohort's optimum decision. Perhaps our intervention did indeed nudge the students into making 'better' choices (Sanders et al, 2019). Remember, these students had already had recent exposure to the University of X through its outreach programme and they may well have had an existing pre-disposition towards applying to this institution. It may be that the messaging served to encourage the research participants to look beyond the University of X and towards a 'better', more informed choice for them. In this case, the unintended outcomes may have been the right outcomes for the participants, if not the facilitating university.

It is also worth considering the potential effect of email '*marketing fatigue'*, which may serve to provoke unintended counter-productive behaviour amongst messaging recipients (Cao et al, 2019; Damgaard and Gravert, 2018). Nudges may be seen by the recipient of contradicting their individual freedom to make their own decisions (Goodwin, 2012), which may prompt them to rebel. Once an individual becomes aware of these nudges they may 'defy the system' and provoke non-conformity (Mols et al, 2015; Graham et al, 2017; John and Blume, 2018). On a related note, this particular messaging may have been interpreted as a signal of desperation; if the University has so much to offer, why does it need to rely on marketing itself in this way?

It is also worth reflecting on the content of the email itself (supplement 1). It is plausible that the messaging prompted some recipients to change their prior view of the institution in a negative way. For example, the email contains the revelation that "*I didn't get the best of grades at A-level, but having a place at university meant that I wanted to push myself to achieve the best that I could whilst I was there*." Whilst this might be relatable to a would-be applicant, it does potentially send an implicit negative quality signal. It is entirely possible that some recipients would interpret this as a sign that the University accepted students with poor grades and, by association, signaled a lack of quality. Therefore, on reflection, it is possible that the content of the email was given insufficient scrutiny in the research design. However, the experiment hinged upon the idea that these nudges were created by current students, not guided by marketing strategies or jargon, and thus it was important that the undergraduates who composed the letters and emails were able to talk about their experiences authentically and accessibly.

It has been suggested that students prefer text messages to other forms of communication, including email, and that, crucially, text messages are acted upon immediately, when the same information from other sources had not prompted similar action (Harley et al, 2007). There have been numerous examples of nudging students with text messages that have highlighted the potential for high-volume low-cost interventions, provided the messengers and messaging is carefully considered (Yeung and Nguyen-Hoang, (2020); Miller et al. (2016). We should therefore consider if email was the right medium for our type of nudges and our particular cohort.

Experiment 2: Letters to former year 7 to 11 participants of University of X's Outreach Programme

In our second randomised controlled trial experiment, we asked two current University of X first year undergraduates to each write a letter describing in their own words their transition to the University of X and their experiences so far. The specific University was named, both in the letter content and on the letter headed paper, and the recipient's first name used in the introduction. The first letter was delivered by post to the treatment group in May 2018, when the recipient cohort would be towards the end of their first year of post-compulsory education (year 12; aged 16 or 17), should they have progressed to that level of study. The second letter, which referred to the original letter written by the other undergradaute, was delivered to the same treatment group four months later in September 2018; the start of the next academic year. The two dates were purposefully chosen; the May letter was seen as a way of reacquainting the recipients to the University of X and encouraging them to think about their next steps over the summer break. The following September letter coincided with the University's upcoming Open Days, and potentially provided an additional nudge to this cohort when they would typically be thinking about applying to HE. A transcript of the two letters is provided in Supplements 2a and 2b.

Method

With appropriate parental/guardian consent, information on young people who had taken part in the University of X's 'pre 16' outreach programme had been systematically collected and stored on a database for several years. A total of 1,166 unique former participants reached the requisite age (18) to enter HE by 1st September 2019. Whilst the cohort was age-specific, they may have commenced participation in the outreach programme in different years. For example, the sample comprised a mix of pupils who first engaged with the programme in the 2012/13 academic year (aged 11), through to those who first participated in 2016/17 (aged 15).

Like experiment 1, the participants were randomised into two groups and stratified by the school the pupil had attended whilst participating in the outreach programme. Several additional covariates, pre-determined through existing internal and external evidence (e.g. Higher Education Access Tracker, 2020; Office for Students, 2020) were included in the statistical modelling (Appendix 3):

- Location of activity (factor, based on whether activity was held on campus or off-campus)
- Student ethnicity (factor, based on whether participant was black, Asian and Minority Ethnic or white)
- Student free school meals eligibility (factor, based on whether participant had been eligible for free school meals, not eligible, or unknown)
- Student gender (factor, based on whether participant was female or male)

It has long been established that prior educational attainment is by far the strongest predictor of the likelihood of a young person entering HE (Chowdry et al, 2012). Ideally, therefore, this variable would have been included in the model. Moreover, similar studies have included high pupil attainment as a pre-requisite for inclusion in the trial (e.g. see Sanders et al, 2019). Unfortunately, due to data protection regulations, these data were not available. Whilst this could be seen as a methodological weakness, the school stratification measures, the covariates included, and the randomisation process ensure that these limitations were minimised. Furthermore, the context of our study, and the sample size available to us, meant that it would not be appropriate to determine eligibility based on student attainment. In effect, every young person who had previously taken part in an outreach programme and given consent for further research were included in our trials, thus eradicating any potential selection bias.

A random number generator was applied to determine which group in the two-armed design each research participant was allocated to. The treatment group (n=583), who were to receive the two letters, and the control group (n=583) who would receive no communication, were split 50/50. Full participant home addresses complete with postcode are mandatory fields in the participant database, hence letters were sent to all 583 participants. Of course, some of the research participants may have moved house since the data were collected, and there was no way of establishing how many letters were opened and read. However, the research aimed to establish if the specific act of sending the letters influenced subsequent behavior, and therefore an intention to treat analysis was conducted, with all research participants in both the treatment and control group tracked in terms of applications and acceptances to the University of X for the 2019/20 admissions cycle.

Results

Data were again analysed in Genstat 20th edition statistical software. This time we find no evidence against the null hypothesis of no difference in application rates between treatment and control (Table 2a), both with ($p=0.46$; Column 2) and without covariates ($p=0.60$; Column 1). Unlike experiment 1, we find that the effect sizes for the treatment are positive (Table 2b), with average application rates of 13.2% for the treatment group, and 11.8% for control, although as noted, these differences were statistically insignificant.

We also find a positive effect size of the treatment in terms of conversions of applications to acceptances (Appendix 2a), with average conversion rates 34.2%, compared with 30.9% control (Appendix 2b). However, with a diminishing sample size (hence only participants who applied to University of X are included) this was, again statistically insignificant (p=0.60 with covariates, Column 2; p=0.67 without covariates, Column 1).

By combining the application and conversion rates, we find that average 'accepted applicant' rates were 4.6% for the treatment group and 3.7% for the control group (Appendix 2d), although with a very large margin of error, these results could feasibly have been derived by chance (Appendix 2c; p=0.44 with covariates, Column 2; p=0.47 without covariates, Column 1).

Table 2a
Logit regressions of applications to the University of X on treatment allocation (Experiment 2)

	Dependent variable: Applied to University of X	
Variables	(1)	(2)
Treatment: Student letters	0.090	0.128
	(0.173)	(0.175)
Location: School		0.077
		(0.197)
Student Ethnicity: BAME		0.405
		(0.208)
Student FSM: FSM eligible		-0.761***
		0.223
Student FSM: FSM Not known		-1.050*
		0.534
Student gender: Male		-0.265
		0.177
Constant	-1.928***	-1.725***
	(0.124)	(0.170)
Observations	1,166	1,166

Note: Standard errors in parentheses; *** p<0.001; ** p<0.01; * p<0.05

Table 2b
Average Application Rates to University of X and 95% Confidence Intervals by Treatment (Experiment 2)

	Average Application Rate	Lower bound CI	Upper Bound CI
Control	11.8%	9.4%	14.7%
+ Letters to prospective applicant	13.2%	10.6%	16.2%

Discussion

The messaging medium of our second experiment - the sending of letters to the treatment group – was more closely aligned with other studies that had found positive nudging effects on applicant behaviour (Sanders et al, 2019). Our study found a null effect. Importantly, our cohort was predominantly comprised of former outreach participants who were targeted for the original interventions based on socio-economic criteria, such as eligibility for free school meals. We know that low income students are significantly under-represented in HE (Department for Education, 2020), so we would expect a relatively low application rate to HE amongst the cohort, including applications to the University of X.

Although no significant difference was detected, the sample size available to us was relatively small, and a large positive effect size would have been required to eliminate chance as the main influencer. Whilst our effects were indeed positive, for both applications (13.2% treatment versus 11.8% control) and conversion to accepted places (34.2% treatment versus 30.9% control) we cannot rule out that these outcomes occurred by chance. Nevertheless, one null value from a single trial does not preclude further investigation, just as a statistically significant positive result from a trial should not necessarily prompt immediate large scale rollout, as findings may not be replicated (e.g. see Scandone et al, 2020; Miller et al, 2016; Groot et al 2017).

Conclusion

Our two designed experiments were hypothesised to increase applications and acceptances to the participating university, based on the results of previous similar trials, albeit with very different contexts. Our first experiment had a statistically significant negative effect; application rates amongst the treatment cohort were lower than our control group, who had received no additional correspondence.

Without follow up research, we can only speculate as to why our first intervention may have backfired. As a sector, we are often guilty of making unevidenced assumptions about how learners want to hear from universities and what will result in desired outcomes, and maybe we have fallen foul of this ourselves here. Perhaps email was the wrong mode of communication for our specific cohort. Whilst we did not have access to participants' mobile numbers, there is no reason why these cannot be collected for future studies, to ascertain if there are any changes in outcomes when texting potential students.

Following on from this, we should ask whether the negative effects were the product of the nudges creating negative opinions of University of X. We cannot rule out email 'contact fatigue' as an influencing factor and have already asked whether email was the right mode of communication. This 'contact fatigue' refers not only to the quantity of communications a young person may receive from universities, but also asks whether these emails position University of X as an institution which heavily markets and, by extension, that might be perceived as less selective or prestigious. It is particularly notable that the revelation of poor student entry grades contained in the email in experiment 1 was not replicated in either of the two letters in experiment 2.

Whatever the reason for our negative effect, we must learn from it. Marketing strategies should be appropriately evidenced through trials such as this one. Policy makers could be wasting resource, or worse, experience unintended and unwanted consequences without considered evaluation.

Our second trial was a little more encouraging. Although there was no convincing evidence that letters from undergraduates of a given institution increased applications or acceptances to that institution, there was a positive direction in the results. Therefore, further trials, ideally with larger sample sizes, should be considered. When one considers that the cost of such a trial is effectively less than what the recruitment of just one additional student would glean (based on three years of study at circa £9,000 tuition fees per year), further research makes financial sense, in addition to quenching our thirst for more evidence.

We have seen that, although studies have been few and far between, there is evidence that, in specific contexts, high volume, low cost outreach can demonstrably influence recipient behaviour as hypothesised. However, this does not necessarily lend itself to universal modelling and, consequently, reveals the extent to which educational nudges are invariably context dependent and specific. There is no '*one size fits all*' programme of nudging behaviour through trusted messengers. Successful messaging is not necessarily transferrable, and can even backfire, given different characteristics of activity providers and recipient cohorts.

Notes

1. Widening participation is a collective term used in the UK which aims to address discrepancies in the take-up of HE opportunities between different groups of students. For further information see https://www.officeforstudents.org.uk/annual-review-2019/a-new-approach-to-fair-access-participation-and-success/
2. See https://www.officeforstudents.org.uk/advice-and-guidance/promoting-equal-opportunities/access-and-participation-plans/
3. These refer to former polytechnics, central institutions or colleges of HE that were given university status by the UK government in 1992, through the Further and Higher Education 1992 Act. For further details see https://www.legislation.gov.uk/ukpga/1992/13/contents
4. See https://www.ucas.com/
5. Internal analysis demonstrates that circa 95% of the University of X's accepted applicants subsequently enroll at the institution.

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Appendix 1a: Logit regressions of conversion of applications to acceptances to the University of X on treatment allocation (Experiment 1)

	Dependent variable:	
	Conversion of application to acceptance at University of X	
VARIABLES	(1)	(2)
Treatment: Student emailed	-0.038	-0.021
	(0.223)	(0.224)
ACORN classification		0.116
		(0.074)
Location: On campus		0.216
		(0.374)
Constant	-1.123***	-1.610***
	(0.149)	(0.281)
Observations	466	466

Note: Standard errors in parentheses; *** p<0.001; ** p<0.01; * p<0.05

Appendix 1b: Average Application to Acceptance Conversion Rates to University of X and 95% Confidence Intervals by Treatment (Experiment 1)

	Average Conversion Rate	Lower bound CI	Upper Bound CI
Control	22.2%	17.6%	27.7%
+ Email to prospective applicant	21.9%	16.8%	28.0%

Appendix 1c: Logit regressions of accepted applications to the University of X on treatment allocation (Experiment 1)

	Dependent variable:	
–	Acceptance at University of X	
VARIABLES	(1)	(2)
Treatment: Student emailed	-0.243	-0.251
	(0.201)	(0.201)
ACORN classification		0.035
		(0.065)
Location: On campus		0.337
		(0.321)
Constant	-2.887***	-3.025***
	(0.135)	(0.252)
Observations	2,198	2,198

Note: Standard errors in parentheses; *** p<0.001; ** p<0.01; * p<0.05

Appendix 1d: Average Accepted Application Rates to University of X and 95% Confidence Intervals by Treatment (Experiment 1)

	Average Accepted Applicant Rate	Lower bound CI	Upper Bound CI
Control	5.3%	4.1%	6.7%
+ Email to prospective applicant	4.1%	3.1%	5.5%

Appendix 2a: Logit regressions of conversion of applications to acceptances to the University of X on treatment allocation (Experiment 2)

	Dependent variable:	
	Conversion of application to acceptance at University of X	
VARIABLES	(1)	(2)
Treatment: Student letters	0.177	0.151
	(0.343)	(0.353)
Location: School		0.423
		(0.389)
Student Ethnicity: BAME		-0.191
		(0.420)
Student FSM: FSM eligible		-0.127
		0.465
Student FSM: FSM Not known		0.270*
		1.060
Student gender: Male		0.631
		0.357
Constant	-0.796**	-1.123***
	(0.250)	(0.352)
Observations	154	154

Note: Standard errors in parentheses; *** p<0.001; ** p<0.01; * p<0.05

Appendix 2b: Average Application to Acceptance Conversion Rates to University of X and 95% Confidence Intervals by Treatment (Experiment 2)

	Average Conversion Rate	Lower bound CI	Upper Bound CI
Control	30.9%	21.3%	42.5%
+ letters to prospective applicant	34.2%	24.5%	45.5%

Appendix 2c: Logit regressions of accepted applications to the University of X on treatment allocation (Experiment 2)

	Dependent variable:	
–	Applied and Accepted to University of X	
VARIABLES	(1)	(2)
Treatment: Student letters	0.205	0.225
	(0.285)	(0.289)
Location: School		0.335
		(0.309)
Student Ethnicity: BAME		0.238
		(0.352)
Student FSM: FSM eligible		-0.728*
		0.376
Student FSM: FSM Not known		-0.584
		0.748
Student gender: Male		0.172
		0.288
Constant	-3.192***	-3.252***
	(0.210)	(0.298)
Observations	1,166	1,166

Note: Standard errors in parentheses; *** p<0.001; ** p<0.01; * p<0.05

Appendix 2d: Average Accepted Application Rates to University of X and 95% Confidence Intervals by Treatment (Experiment 2)

	Average Accepted Applicant Rate	Lower bound CI	Upper Bound CI
Control	3.7%	2.4%	5.6%
+ letters to prospective applicant	4.6%	3.1%	6.6%

Appendix 3: Statistical Modelling Applied

Our main dependent factors are:

1. Applications to the specific HE institution (Tables 1a, 1b, 2a and 2b)
2. Conversion of these applications to accepted applications; i.e. expected to enroll at the specific HE institution (Appendices 1a, 1b, 2a and 2b)
3. A combination of 1 & 2 (i.e. whether the research participant applied to, and was accepted at the specific HE institution (Appendices 1c, 1d, 2c & 2d)

Each of these outcomes are intrinsically binary; e.g. participants either did or did not apply to the HE institution. Therefore, binary logistic regression models were developed for each separate dependent factor. The linear logistic regression model for the dependence of p_i (probability of outcome of interest) on the corresponding values of k explanatory variables $x_1, x_2, \ldots x_{i,k}$, is illustrated as follows:

$$\text{logit}(p_i) = \log\left(\frac{p_i}{1-p_i}\right) = \alpha + \sum_{j=1}^{k} \beta_j x_{i,j}.$$

The regression coefficients of this model are estimated by the method of maximum likelihood. The logit function, logit(p) = log[$p/(1-p)$], is a link function.

From this we calculate the probability of the outcome of interest (e.g. participant applying to specific HEI) given the fixed effects of the covariates, as follows:

$$\text{p} = e^{\alpha + \sum \text{B}_j x_{i,j}} / \left(1 + e^{\alpha + \sum \text{B}_j x_{i,j}}\right)$$