Transforming Access and Student Outcomes in Higher Education

TASO Online Summer Schools Evaluation: Interim analysis report of exploratory outcomes

July 2022 (Revised November 2023)

NB. This report has been updated to align with the 2023 interim report for the in-person summer schools evaluation.

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View the full protocol for this study can be found on the TASO website.

The study was pre-registered on OSF Registries: https://osf.io/j5dpy.

1. Summary

Background: This project is a collaboration between the Centre for Transforming Access and Student Outcomes in Higher Education (TASO), four Higher Education Providers (HEPs) and the Behavioural Insights Team (BIT). In summer 2021, sixteen summer schools were planned across these four HEPs with the aim of widening participation (WP) in higher education (HE) among participants. This report presents the interim findings from the impact evaluation of these summer schools.

Aims: The aim of the project is to investigate the efficacy of summer schools as a widening participation activity. The aim of the widening participation agenda is to increase progression to HE among students from disadvantaged or under-represented groups.

Intervention: HEP summer schools, mostly delivered online, either for students in pre-16 or post-16 education.

Design: This study is a two-arm, parallel group randomised controlled trial (RCT).

Outcome measures: The outcomes analysed in this interim report are survey measures of participants' self-reported applications to HE, and self-reported attitudes to HE, covering their likelihood of going on to further academic study, their self-efficacy relating to HE, the compatibility of HE with their social identity, and their perception of practical barriers to HE.

Analyses: Ordinary Least Squares (OLS) regressions are used to estimate effects.

Results: The point estimates for five outcomes are positive, and for one outcome is negative. No effects are statistically significant at the 5% level. A high and differential rate of attrition has led to a small sample and possible bias in some of the estimated effects.

Conclusions: There is early evidence of promise that these summer schools may have had a small positive effect on self-efficacy relating to HE and a smaller positive effect on self-reported applications to HE, compatibility of HE with social identity, and perception of practical barriers to HE. The analysis also suggests that there was no effect on students' self-reported likelihood of attending HE or post-16 academic study (depending on their age). This is probably because most applicants to HE summer schools already intend to follow these paths. The more robust test of the intervention will come in 2023 when we have administrative data on students' entry to HE.

2. Introduction

2.1. Background

This project is a collaboration between the Centre for Transforming Access and Student Outcomes in Higher Education (TASO), four Higher Education Providers (HEPs) and the Behavioural Insights Team (BIT). In summer 2021, a series of summer schools were delivered with the aim of widening participation in HE among participants. Three types of evaluation are being conducted with these summer schools: an impact evaluation, a cost evaluation and an implementation and process evaluation (IPE). **This report gives the interim findings from the impact evaluation.**

BIT is responsible for:

- designing, analysing and reporting for the impact evaluation;
- randomly assigning participants to the treatment or control group for the impact evaluation;
- designing, analysing and reporting for the cost evaluation; and
- collecting the university entry data from the Higher Education Statistics Agency (HESA) and covariate data from the National Pupil Database (NPD).

TASO is responsible for:

- collecting all data for the impact evaluation (except for NPD and HESA data), from HEPs, and through the Higher Education Access Tracker (HEAT);
- collecting all data for the cost evaluation; and
- designing and implementing the IPE.

The four HEPs (listed in Table 1) are responsible for:

- delivering the summer schools;
- collecting registration data from summer school applicants;
- collecting online survey data from participants; and
- participating in the IPE and cost evaluation.

A research assistant (RA), funded by TASO, was recruited by the majority of HEPs to support them with their evaluation responsibilities. In other cases, existing staff in the Evaluation/WP teams supported the project. Table 1 summarises the key project personnel for each organisation.

Table 1: Project personnel

Organisation	Name	Role and responsibilities

BIT	Dr Patrick Taylor	Principal Investigator and Evaluation QA
	Dr Laure Bokobza	Evaluation Manager
	Pujen Shrestha	Data Analyst
	Dr Giulia Tagliaferri	Evaluation Supervisor
	James Lawrence	Evaluation QA
	Dr Alex Sutherland	Evaluation QA
	Ruth Persian	Evaluation QA
TASO	Dr Helen Lawson	Research Programme Manager. Responsible for the day-to-day management of the study.
	Sarah Chappell	Research Manager. Supporting the team on the day-to-day management of the study.
	Dr Eliza Kozman	Deputy Chief Executive. Responsible for overseeing the implementation of the study.
	Jessica Hunt	Maternity cover for Deputy Chief Executive.
University College London (UCL)	Shireen Quraishi	Project lead at UCL. Responsible for implementing randomisation and data collection there.
	Emily Burchell	RA, supporting data collection.
University of Leeds	Liz Hurley	Project lead at the University of Leeds. Responsible for implementing randomisation and data collection there.
	Rebecca Talbot	RA, supporting data collection.
Nottingham Trent University (NTU)	Peter Cassidy	Project lead at NTU. Responsible for implementing randomisation and data collection there.
	Laura Hope	Co-project Lead.
University of Kent	Marta Almeida	Project lead at the University of Kent. Responsible for implementing randomisation and data collection there.

	Amy Burt	Co-project Lead.

The project is funded by TASO, and TASO is funded by the Office for Students (OfS), the independent regulator of higher education in England.

2.2. Aims

The aim of the project is to investigate the efficacy of summer schools as a widening participation activity. The aim of the widening participation agenda is to increase progression to HE among students from disadvantaged or under-represented groups.

There is currently limited evidence on this topic. A recent review commissioned by TASO found evidence of positive correlations between summer school participation and confidence and aspirations, but mixed effects on applications and entry to HE (Robinson & Salvestrini, 2020, pp.32-34). The review also noted the limited quality of the current evidence, with most existing studies using no comparison group. The two studies identified in this review that did use comparison groups did not do so robustly; for example, comparing participants of summer schools with failed applicants, or with young people who had not applied at all (Hoare & Mann, 2011, p.1). The one UK-based RCT of university summer schools identified found no effect on participants' likelihood of application to HE, though the sample size for this study was small and attrition was high (Bowes et al. 2019, p.57). An evaluation of eight summer 'bridge programs' in the US, that used an RCT design, found positive effects on the pass rates of first year college maths and writing courses (Barnett et al., 2012). However, it found no effect on course participation (the number of credits earned or attempted) and no effect on persistence at college. The sample for this study was also different in important ways to the population of interest in the current evaluation, and the time gap between intervention and the targeted outcome is longer in the case of the current evaluation. In the US study, the sample was made up of young people who had recently graduated from high school, 100% of whom had the intention of attending college at the end of the summer. The present evaluation is focussing on young people who are not as close to participation in HE; a pre-16 cohort who have not yet taken their GCSEs (let alone applied to university), and a cohort who are in their first year of post-16 education.

In summary, there is currently no strong evidence on the causal effects of this type of summer school on widening participation. This present study aims to begin to fill this gap, by answering the following questions. Among disadvantaged or under-represented groups, what is the effect of summer schools on:

1. entry to HE (the primary outcome)?;

2. entry to the HEP that delivers the summer school (the secondary outcome)?

Exploratory analysis has also been prespecified to estimate the effects of summer schools on three proximal outcomes and three potential mediating mechanisms. **This interim report presents the findings on these exploratory outcomes** (described in section 3.6).

To answer these questions, outcomes are compared between the participants in the trial summer schools (the treatment group), and eligible applicants who were not selected to participate (the control group). The eligibility criteria applied by HEPs has ensured that the trial sample is composed solely of disadvantaged or under-represented groups (see section 3.4 for more detail on this).

2.3. Intervention

This study is evaluating a collection of interventions, summarised in Table 2. Participating HEPs have delivered their own summer schools, either for students in pre-16 or post-16 education. Each summer school had its own specific characteristics, but all had the same broad aims and involved similar activities. All summer schools took place in the summer of 2021. One of pre-16 providers was due to deliver their summer schools in-person, but these were cancelled post-randomisation and before taking place due to COVID-19 outbreaks. The rest took place online due to the context of COVID-19. The normal delivery model for all HEPs in the trial is to conduct summer schools in-person. The tested (online) interventions therefore required a lot of new design work and are substantially different from business as usual. Appendix I contains a description of each summer school, broken down by provider.

Table 2: Summary of summer school delivery

Summer School	Target group	Summer school took place / cancelled post-randomisation
University A (Languages)	Post-16	Took place
University A (Biosciences)	Post-16	Took place
University A (Maths)	Post-16	Took place
University A (Psychology)	Post-16	Took place
University A (Social Sciences)	Post-16	Took place
University D (Architecture)	Post-16	Took place

University D (Health and Wellbeing Data Science)	Post-16	Took place
University D (Biosciences)	Post-16	Took place
University D (Chemical Engineering)	Post-16	Took place
University D (Astrophysics)	Post-16	Took place
University D (History)	Post-16	Took place
University D (Natural Sciences)	Post-16	Took place
University D (Economics)	Post-16	Took place
University F	Pre-16	Took place
University G School 1	Pre-16	Cancelled
University G School 2	Pre-16	Cancelled

3. Methods

3.1. Design

This study is a two-arm, parallel group randomised controlled trial (RCT), testing for superiority of the treatment condition over the control condition. Eligible applicants to the summer schools were randomly assigned to either the treatment or control group. Each summer school programme had a different number of places available, and a different number of eligible applicants so the ratio of assignment differed by programme. See section 3.2 below for details of the assignment procedure.

Research activities are taking place between January 2021 and November 2024[1] (including final reporting). Fig. 1, in section 4.1, gives an overview of the research flow and timeline up to the point of final data collection, in the form of a CONSORT diagram.

3.2. Randomisation

Four practical constraints were imposed by the programme that affected the randomisation[2]:

[1] This is due to the linked HESA-NPD dataset needed for the final analysis not being available until 2024.
[2] Five constraints are described in the trial protocol, but only four were applied in the randomisation. This is because the fifth constraint (the quotas that HEPs wanted to fill in the intervention group; for example, a

i. some HEPs guaranteed places for applicants meeting certain criteria (e.g. care leavers), so these applicants could not be part of the trial;
ii. all HEPs had a certain number of places on their summer school(s) that they needed to fill;
iii. application timelines varied by HEP, so applicants had to be randomised in batches; and
iv. students were allowed to apply to more than one summer school.

These constraints added complexity to the randomisation, so the detailed step-by-step process that was followed is provided below. Randomisation was conducted at the individual level and was blocked, with the block influencing the probability of assignment. The characteristics of the blocks were defined by each summer school, based on the characteristics of their applicant pools. Individuals in the same block had the same probability of assignment. As randomisation was conducted within blocks (and not across blocks), this was a stratified randomisation, in which each block was a strata. The randomisation strategy differed from a standard stratification strategy in that we did not randomly allocate half of the candidates to the treatment and half to the control group. Instead we allocated the required number of candidates to the treatment group (corresponding to the available summer school places) and the remainder to the control group. The differences in probabilities of assignment between blocks are accounted for in the analysis by including a categorical control variable in the regression model that indicates the individual's block (block fixed effects).

Randomisation procedure

TASO provided BIT with a series of spreadsheets containing a list of all eligible applicants for each individual summer school. The variables that were used for randomisation were as follows.

- TASO unique ID
- Name of summer school (the randomisation block)
- Guaranteed place (Y/N)

Randomisation was conducted in R, with each batch of the randomisation being quality assured by a separate researcher at BIT before the final randomised dataset was sent to TASO, who shared it with the relevant HEP. We allocated applicants to treatment/control conditions on a rolling basis in five batches, as follows.

50/50 male-female split) was dropped as per the decision-making rules on stratification and block sizes detailed in the protocol.

Batch 1

The first batch of randomisation was conducted by TASO. This was because the first group of HEPs needed to inform applicants of their places and therefore have applicants randomised by a certain time, and this was before data sharing was permitted between TASO and BIT through a Data Processing Agreement. This first randomisation was quality assured by TASO and was subsequently quality assured by BIT.

For this batch, which included more than one summer school, the following procedure was carried out.

1. TASO appended applicant lists from different summer schools.
2. TASO assigned guaranteed places. All applicants with a characteristic that guarantees them a place were assigned to participate in the summer school, *but not included in the trial analysis*.
3. For each applicant applying to more than one summer school in the batch, TASO randomly selected which summer school they were to be considered for, using a random number generator. TASO created a variable (ENTERRAND) taking value 1 if the applicant entered randomisation for that summer school, 0 otherwise. This strategy was used to ensure that if two applicants in the same batch applied to the same set of summer schools, they could not be selected to participate in the randomisation for the same summer school.
4. For each summer school in the batch, TASO assigned applicants with ENTERRAND = 1 to treatment or control in the following way.
    a. TASO assigned all applicants a computer-generated random number.
    b. TASO sorted the random numbers in ascending order.
    c. TASO allocated the available places on the summer school to the corresponding number of applicants at the top of the list.
    d. TASO allocated all remaining applicants to the control group.

Batches 2,3,4 and 5

The subsequent batches of randomisation were conducted by BIT using the method specified here.

1. We checked if any applicants appeared in a previous batch using TASO's unique ID.[3] If so, we assigned ENTERRAND=0 to the applicant for the summer schools

[3] NB: Prior to sending applicant data to BIT, TASO identified duplicate applicants and flagged these duplicates in the dataset (with a new variable) before sharing with BIT.

in the current batch (so that they could not be assigned to either the treatment OR control group in this batch). This did not apply to participants with guaranteed places, who were given places in all summer schools to which they applied (and for which they met the criteria for being guaranteed a place).
2. We repeated steps 3 and 4 outlined above for the first batch.

The strategy used means that the order in which a batch entered the randomisation process affected the number of students who could enter the randomisation for each summer school. In later batches, every applicant who applied to a summer school in a previous batch was automatically excluded from entering randomisation.

Trial participants and intervention deliverers were not blind to assignment. Participants had to read and consent to participate in the research, and intervention deliverers and participants knew who had been assigned to the treatment group because they were delivering or receiving the only intervention being tested.

3.3. Outcome measures

The outcomes being measured in this trial are described in Table 3. They are broken down into three categories: primary, secondary and exploratory, defined as follows.

- **Primary outcome:** The main change that the intervention is trying to make.
- **Secondary outcomes:** The other changes that the intervention is trying to make, that are also considered to be valuable ends in themselves.
- **Exploratory outcomes:** There are two types of exploratory outcome in this study:
    - *Proximal outcomes:* Short-term indicators of primary or secondary outcomes.
    - *Mediating mechanisms:* Intermediate changes that explain how the intervention causes the primary or secondary outcomes, that are not considered to be valuable ends in themselves (distinguishing them from secondary outcomes).

These definitions are used here to help clarify the intervention's theory, but also to determine some important analytic choices. The primary outcome was used as the basis for power calculations and the primary/secondary/exploratory distinction is used to make choices about adjustments for multiple comparisons. The headline findings of the impact evaluation will be the estimated effects on the primary and secondary outcomes. **This interim report covers the exploratory outcomes (highlighted in green in Table 3).** Data on these outcomes was gathered from two surveys: one administered at the end of summer school delivery ('survey 1') and the one in January 2022, after HE applications had closed ('survey 2').

The sample is made up of two different age groups (those in pre-16 education and those in post-16 education). Not all outcome data is available for both cohorts. The final column of Table 3 indicates which cohort the relevant data is available for and, therefore, defines the sample for analysing each outcome.

Table 3: Outcome measures

Outcome measure	Data to be collected	Aggregation of items	Point of collection	Sample
PRIMARY: Progression to HE	Does the individual enter HE in the academic year 2022/23 according to the HESA dataset? Binary**:** yes/no	NA	After endpoint (June 2023)	Post-16 only
SECONDARY: Progression to host university	Does the individual go on to study at the HEP that delivers the summer school applied to according to the HESA dataset? Binary: yes/no	NA	After endpoint (June 2023)	Post-16 only
EXPLORATORY 1 (PROXIMAL): Application to HE	Survey 2: Have you applied to university? Binary: yes/no	NA	After endpoint (January 2022)	Post-16 only
EXPLORATORY 2 (PROXIMAL): Likelihood of going to HE	Survey 1: How likely are you to apply to university? Likert: 7-point "Extremely likely to extremely unlikely"	NA	Baseline After endpoint (Aug and Sept 2021)	Both
EXPLORATORY 3 (PROXIMAL): Likelihood of progressing to academic study post-16[4]	Survey 1: How likely is it that you will study at school or a sixth form after you've finished Year 11? Likert: 5-point "Extremely likely to extremely unlikely"	NA	Baseline After endpoint (Aug and Sept 2021)	Pre-16
EXPLORATORY 4 (MEDIATOR): Self-efficacy relating to HE	Survey 1: 1. How confident are you that you could make a successful application to university? 2. How confident are you that you could succeed at university?	Mean average	Baseline After endpoint (Aug and Sept 2021)	Both

[4] This is a short-term indicator of a secondary outcome (actual progression to academic study), but the latter will not be measured as part of this study as it falls outside of the study timeline.

	Likert: 5-point "Extremely confident" to "Not confident at all"			
EXPLORATORY 5 (MEDIATOR): Compatibility of HE with social identity	Survey 1: How much do you agree with the following: "University is for people like me"? Likert scale: 5-point ''strongly agree to strongly disagree''	NA	Baseline After endpoint (Aug and Sept 2021)	Both
EXPLORATORY 6 (MEDIATOR): Perception of practical barriers to HE	Survey 1: 1. How confident are you that you could afford to go to university? 2. How confident are you that you know how to apply to university? Likert: 5-point "Extremely confident" to "Not confident at all"	Mean average	Baseline After endpoint (Aug and Sept 2021)	Both

3.4. Sample selection

Due to the COVID-19 pandemic all summer schools that took place in the trial were conducted online. Two summer schools in the sample were planned to be face-to-face, in host secondary schools. In the event, these two summer schools were cancelled, but the data that we have on applicants to them has been analysed as per the intention-to-treat analysis specified in the protocol.

The study sample is made up of all applicants to the trial summer schools who met the HEPs' eligibility criteria. The criteria varied slightly by HEP, but the following list covers all criteria used across providers in the study. To have been eligible for consideration, an applicant must have had one or more of the following characteristics to indicate disadvantage/underrepresentation[5]:

- identify as coming from a black or minority ethnic background;
- live in an area of deprivation (as defined by the most deprived quintile (Q1) of the Index of Multiple Deprivation (IMD);
- live in an area of low participation in HE as defined by the POLAR classification (Q1 or Q2);
- be in care or a care-leaver;

[5] This list is shorter than that in the trial protocol because not all HEPs in the protocol made it to randomisation. One of these was University B who had two unique selection criteria (students indicating an interest in a subject offered by the HEP; and/or indicating an interest in studying close to home) which have been removed from this list.

- be a young carer;
- have a disability;
- be the first in their family to attend HE;
- be eligible for free school meals;
- be a refugee or asylum seeker.

University F had the additional criterion that all applicants must attend a school that partners with the university.

University D also required students to be on track to achieving the qualifications and grades required to attend the relevant degree programme.

The sample was divided into two age groups: a pre-16 and post-16 group. The pre-16 group contained individuals from Years 9 and 10. The post-16 group contained individuals from Year 12/first year of post-16 education.

Recruitment of study participants was carried out by the HEPs who agreed to participate in the trial. The size of the sample was determined by the number of eligible applicants to the summer schools run by these HEPs. The size of the treatment group was determined by the number of places available in each summer school.

3.5. Analytical strategy

This interim report covers the analysis of the exploratory outcomes. The specification for the analysis of each of these outcomes is described below.

Exploratory outcome 1

The following model has been used to estimate the effects of the intervention on exploratory outcome 1. Analysis has been conducted on an intention-to-treat basis, including all complete cases in the post-16 sample.

$$Y_i \sim bernoulli(p_i)\ ;\ logit(p_i) = \beta_0 + \beta_1 T_i + \beta_2 X_i$$

where,

- Y_i is a binary indicator of whether the individual has reported that they have applied to university by January 2022 (1 if they have, 0 if not);
- p_i is the probability of Y_i;
- T_i is binary indicator of treatment assignment (1 for treated, 0 for control); and
- X_i is a vector of pre-treatment covariates (summer school applied to, sex, ethnicity, postcode-level marker of disadvantage, FSM status, whether anyone in the family

has been to university, academic year group, school ID, KS4 attainment 8 score, and an indicator of the block from which the individual was randomised).[6]

Exploratory outcome 2

The following model has been used to estimate the effects of the intervention on exploratory outcome 2, using ordinary least squares (OLS) regression. Analysis has been conducted on an intention-to-treat basis, including all complete cases across both cohorts.

$$Y_i = \beta_0 + \beta_1 T_i + \beta_2 X_i + \epsilon_i$$

where,

- Y_i is the self-reported **likelihood that the individual will apply to university** (the score on a 7-point Likert scale);
- T_i is the binary indicator of treatment assignment (1 for treated, 0 for control);
- X_i is a vector of pre-treatment covariates (summer school applied to, sex, ethnicity, postcode-level marker of disadvantage, FSM status, combined KS2 Maths and English scores, whether anyone in the family has been to university, academic year group, school, and an indicator of the block from which the individual was randomised); and
- ϵ_i is the heteroskedasticity robust residual error term.

Exploratory outcome 3

The following model has been used to estimate the effects of the intervention on exploratory outcome 3, using ordinary least squares (OLS) regression. Analysis has been conducted on an intention-to-treat basis, including all complete cases in the pre-16 sample.

$$Y_i = \beta_0 + \beta_1 T_i + \beta_2 X_i + \epsilon_i$$

[6] In this and subsequent models, we don't include the combined KS2 Maths and English scores and the KS4 attainment 8 score as covariates at the stage of the interim report as we don't have this data yet. We also don't include the block from which the individual was randomised (other than the summer school) as these constraints on randomisation were dropped by the HEPs. We include two postcode-level markers of disadvantage/low participation: the IMD quintile and the POLAR4 quintile.

where,

- Y_i is the self-reported **likelihood that the individual will go on to study at school or a sixth form after Year 11** (the score on a 5-point Likert scale);
- T_i is the binary indicator of treatment assignment (1 for treated, 0 for control);
- X_i is a vector of pre-treatment covariates (summer school applied to, sex, ethnicity, postcode-level marker of disadvantage, FSM status, combined KS2 Maths and English scores, whether anyone in the family has been to university, academic year group, school ID, and an indicator of the block from which the individual was randomised); and
- ϵ_i is the heteroskedasticity robust residual error term.

Exploratory outcome 4

The following model has been used to estimate the effects of the intervention on exploratory outcome 4, using ordinary least squares (OLS) regression. Analysis has been conducted on an intention-to-treat basis, including all complete cases across both cohorts.

$$Y_i = \beta_0 + \beta_1 T_i + \beta_2 X_i + \epsilon_i$$

where,

- Y_i is the individual's **self-efficacy relating to HE** (the mean average of two scores on two 5-point Likert scales);
- T_i is the binary indicator of treatment assignment (1 for treated, 0 for control); and
- X_i is a vector of pre-treatment covariates (summer school applied to, sex, ethnicity, postcode-level marker of disadvantage, FSM status, combined KS2 Maths and English scores, whether anyone in the family has been to university, academic year group, school ID, and an indicator of the block from which the individual was randomised); and
- ϵ_i is the heteroskedasticity robust residual error term.

Exploratory outcome 5

The following model has been used to estimate the effects of the intervention on exploratory outcome 6, using ordinary least squares (OLS) regression. Analysis has been conducted on an intention-to-treat basis, including all complete cases across both cohorts.

$$Y_i = \beta_0 + \beta_1 T_i + \beta_2 X_i + \epsilon_i$$

where,

- Y_i is the level of **compatibility of HE with the individual's social identity** (the score on a 5-point Likert scale);
- T_i is the binary indicator of treatment assignment (1 for treated, 0 for control); and
- X_i is a vector of pre-treatment covariates (summer school applied to, sex, ethnicity, postcode-level marker of disadvantage, FSM status, combined KS2 Maths and English scores, whether anyone in the family has been to university, academic year group, school ID, and an indicator of the block from which the individual was randomised); and
- ϵ_i is the heteroskedasticity robust residual error term.

Exploratory outcome 6

The following model has been used to estimate the effects of the intervention on exploratory outcome 7, using ordinary least squares (OLS) regression. Analysis has been conducted on an intention-to-treat basis, including all complete cases across both cohorts.

$$Y_i = \beta_0 + \beta_1 T_i + \beta_2 X_i + \epsilon_i$$

where,

- Y_i is the individual's **perception of practical barriers to HE** (the mean average of two scores on two 5-point Likert scales);
- T_i is the binary indicator of treatment assignment (1 for treated, 0 for control); and
- X_i is a vector of pre-treatment covariates (summer school applied to, sex, ethnicity, postcode-level marker of disadvantage, FSM status, combined KS2 Maths and

English scores, whether anyone in the family has been to university, academic year group, school ID, and an indicator of the block from which the individual was randomised); and

- ϵ_i is the heteroskedasticity robust residual error term.

4. Results

4.1. Participant flow

Figure 1 presents a CONSORT flow diagram of the trial so far, with an overview of the timings and sample numbers for recruitment, intervention delivery and follow-up. Students are considered to have participated in a summer school if they passed the threshold defined by the HEP for the compliance analysis (see section 12.12 of the trial protocol for a list of these thresholds broken down by HEP). The proportion of compliers in the intervention group was 52%. This participation information is included in the flow diagram for completeness, but has not been used in the analysis for this report, which is all done on an intention-to-treat basis. A complier average causal effect (CACE) will be estimated for the primary outcome in the final report.

Figure 1: Study flow diagram

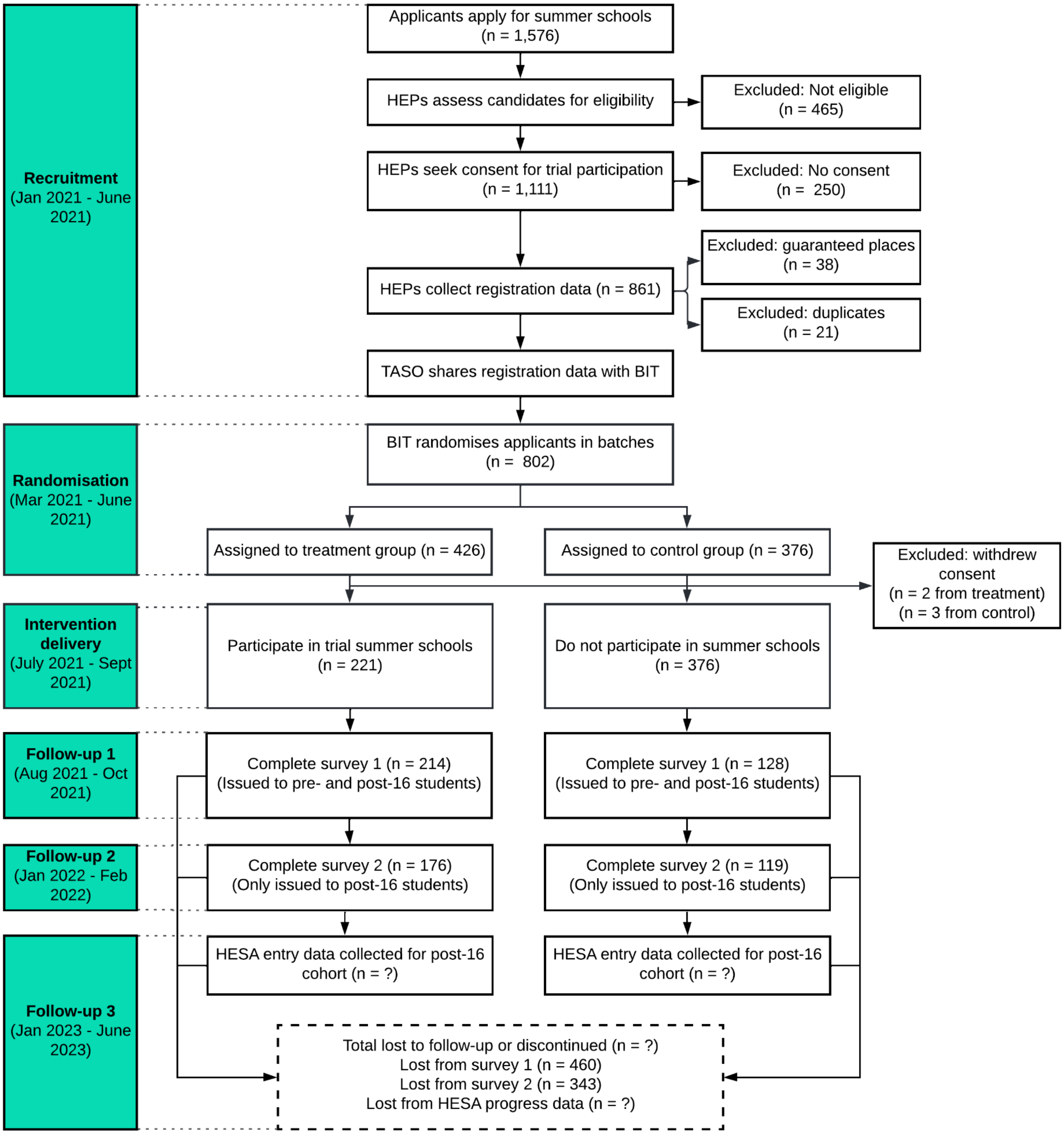


Table 4 summarises the attrition so far in the trial. Attrition for the outcome surveys analysed in this interim report was high and occurred at different rates in the intervention and control groups for both surveys. Analysis in the balance checks section below also reveals some differential attrition within this on one characteristic, which could introduce bias to the estimated effects from survey 1. The survey 2 sample is well-balanced on all observable characteristics, although we can of course not exclude the possibility of imbalance on unobservable characteristics. The level of attrition also means that the samples for the outcomes analysed from both surveys are small and likely underpowered (hence the wide confidence intervals on the estimated effects reported in section 4.3).

Table 4: Summary of survey 1 and survey 2 attrition

		Treatment	Control	Total
Number of Students	Total sample for survey 1	426	376	802
	Analysed for outcome survey 1	214	128	342
	Total sample for survey 2	341	297	638
	Analysed for outcome survey 2	176	119	295
Student attrition	Number lost from outcome survey 1	212	248	460
	Percentage lost from outcome survey 1	49.7%	66.0%	57.4%
	Number lost from outcome survey 2	165	178	343
	Percentage lost from outcome survey 2	48.3%	60.0%	54.0%

There was some unplanned variation in the timing of and approach to the survey 1 data collection by HEP, as summarised in Table 5. For two of University A’s summer schools (Psychology and Biosciences), this survey was not issued by the university to some participants at the agreed time (in September) due to an administrative error. When this error was realised, the survey was sent to these participants (in late October) and a shorter time interval was given for completion (1 week instead of 3 weeks for other participants). This may have led to the lower completion rates for these two summer schools, relative to most of the other post-16 summer schools. The delay in sending out the survey for University A (Psychology and Biosciences) may also have led to smaller measured effects among these students, as we might expect these to attenuate over time.

There was also some variation in the use of incentives for survey 1 completion, with prize draws being used by 6 out of 8 of the summer schools in the trial. This may have contributed to the differential completion rates by HEP shown in Table 5, but there is no clear pattern here. Two out of three of the summer schools (those run by University G) that did not use any incentives experienced very high attrition (89% and 98%). However, the third (University D) also used no incentive and experienced only 53% attrition (a similar level to all other summer schools). The more likely explanation for the high attrition in University G's summer schools is that they were cancelled. The differential attrition by summer school, and the difference in the timing of data collection, will not have introduced any bias into the estimated effects because randomisation was stratified by summer school. Survey 2 was issued to all relevant students at the same time and with the same incentives as Survey 1.

Table 5: Data collection and attrition by HEP

	Survey 1			Survey 2		
Summer School	**Attrition Rate**	**Survey Period**	**Survey Incentive**	**Attrition Rate**	**Survey Period**	**Survey Incentive**
University A Languages	45.2%	20/09/21 - 11/09/21	£25 amazon voucher prize draw	41.1%	27/1/2022 - 17/2/2022	£25 amazon voucher prize draw
University A Biosciences	61.2%	Wave 1: 20/09/21 - 11/09/21 Wave 2: 20/10/21 - 27/10/21	£25 amazon voucher prize draw	56.6%	27/1/2022 - 17/2/2022	£25 amazon voucher prize draw
University A Maths	51.7%	20/09/21 - 11/09/21	£25 amazon voucher prize draw	50.0%	27/1/2022 - 17/2/2022	£25 amazon voucher prize draw
University A Psychology	55.1%	Wave 1: 20/09/21 - 11/09/21 Wave 2: 20/10/21 - 27/10/21	£25 amazon voucher prize draw	49.3%	27/1/2022 - 17/2/2022	£25 amazon voucher prize draw

University A Social Sciences	57.6%	20/09/21 - 11/09/21	£25 amazon voucher prize draw	63.6%	27/1/2022 - 17/2/2022	£25 amazon voucher prize draw
University D	52.9%	06/09/21 - 27/09/21	None	56.5%		None
University F	41.1%	06/09/21 - 27/09/21	£10 amazon voucher prize draw	-	-	-
University G	89.4%	06/09/21 - 27/09/21	None	-	-	-
University G	97.7%	06/09/21 - 27/09/21	None	-	-	-

4.2. Description of data

Sample demographics

Table 6 shows the baseline demographic characteristics for each group in three samples: randomised participants, survey 1 responders, and survey 2 responders. The survey 1 sample is very different to the general population in two ways. Compared to the population of England at the same age, the total sample contains the same proportion of students eligible for free school meals (FSM) (20%[7]), but a higher proportion of girls (77% vs 49%[8]), and a smaller proportion of white students (54% vs 82%[9]). The latter two comparisons are as expected, because ethnicity was used by HEPs as a selection criterion, and a greater proportion of girls and ethnic minority students enter HE (so greater proportions would be expected to apply for HE summer schools). The similar rate of FSM students between the survey 1 sample and the English population is perhaps surprising. FSM status (along with other indicators of socio-economic status) was a selection criterion for most summer schools so we would expect to see a higher proportion of students eligible for FSM in the sample. This result seems to be partly explained by differential attrition (in the randomised sample, 25% of students are eligible for FSM).

[7] https://explore-education-statistics.service.gov.uk/find-statistics/free-school-meals-autumn-term/2020-21-autumn-term

[8] https://www.ons.gov.uk/peoplepopulationandcommunity/populationandmigration/populationestimates/bulletins/annualmidyearpopulationestimates/mid2020

[9] https://www.ethnicity-facts-figures.service.gov.uk/uk-population-by-ethnicity/demographics/age-groups/latest

The survey 2 sample is similar to survey 1 sample. It contains a slightly higher proportion of FSM students (22%), but this is not much higher than that in the English population and still lower than the randomised sample. The survey 2 sample also contains a higher proportion of female students and a lower proportion of white students (76% and 50% respectively) than the population of England at the same age.

Table 6: Distribution of covariates by treatment group

	Randomised sample (N = 802)		Survey 1 (N = 342)		Survey 2 (N = 295)	
	Intervention (N = 426)	Control (N = 376)	Intervention (N = 214)	Control (N = 128)	Intervention (N = 176)	Control (N = 119)
Eligible for FSM						
Yes	99 (23.2%)	101 (26.9%)	42 (19.6%)	25 (19.5%)	42 (23.9%)	23 (19.3%)
No	309 (72.5%)	247 (65.7%)	161 (75.2%)	96 (75.0%)	127 (72.2%)	89 (74.8%)
Unknown	18 (4.2%)	27 (7.2%)	11 (5.1%)	7 (5.5%)	7 (4.0%)	7 (5.9%)
Missing	0 (0%)	1 (0.3%)	0 (0%)	0 (0%)	0 (0%)	0 (0%)
Sex						
Female	295 (69.2%)	277 (73.7%)	154 (72.0%)	104 (81.3%)	139 (79.0%)	88 (73.9%)
Male	126 (29.6%)	97 (25.8%)	58 (27.1%)	24 (18.8%)	36 (20.5%)	31 (26.1%)
Other	5 (1.2%)	2 (0.5%)	2 (0.9%)	0 (0.0%)	1 (0.6%)	0 (0%)
Ethnicity						
White	222 (52.1%)	188 (50.0%)	119 (55.6%)	66 (51.6%)	91 (51.7%)	58 (48.7%)
Asian	111 (26.1%)	96 (25.5%)	51 (23.8%)	27 (21.1%)	49 (27.8%)	35 (29.4%)

Black	46 (10.8%)	54 (14.4%)	25 (11.7%)	21 (16.4%)	18 (10.2%)	14 (11.8%)
Other	44 (10.3%)	32 (8.5%)	19 (8.9%)	12 (9.4%)	17 (9.7%)	10 (8.4%)
Missing	3 (0.7%)	6 (1.6%)	0 (0%)	2 (1.6%)	1 (0.6%)	2 (1.7%)
Year group						
12	341 (80.0%)	297 (79.0%)	180 (84.1%)	113 (88.3%)	176 (100%)	119 (100%)
10	46 (10.8%)	27 (7.2%)	31 (14.5%)	12 (9.4%)	0 (0%)	0 (0%)
9	39 (9.2%)	52 (13.8%)	3 (1.4%)	3 (2.3%)	0 (0%)	0 (0%)
Notes: Totals do not add up to 100% due to rounding.						

Balance checks

Table 7 presents balance checks on FSM status, ethnicity, sex and year group on the survey 1 sample. To assess balance, the magnitude of the differences in mean scores between the two groups is calculated for each covariate.[10] Rather than reporting simple differences in means for each covariate, normalised differences are presented to aid comparison between covariates that have different units, and to facilitate comparisons across studies.

The normalised difference is defined as the difference in means between the two groups, divided by the pooled standard deviation. Normalised differences with a magnitude of 0.1 or less indicate a negligible correlation between the covariate and assignment to treatment group, which can usually be addressed through covariate adjustment in the regression (Austin 2009, p.1233), as planned here. Following this interpretation of the magnitude of differences, the analytic sample appears to be well-balanced on FSM status, ethnicity and year group, but imbalanced on sex. This may

[10] A common alternative is to report whether differences between groups are statistically significant at a certain level of confidence (often $p < 0.05$ in the social sciences). This approach is not particularly helpful because it only tells us whether the sample is large enough to detect a difference, and leaves open the question as to whether any observed differences – and any associated bias – can be addressed through simple covariate adjustment (the approach taken in the analysis for this study) (Imbens & Rubin 2015, p.311).

mean that the point estimates reported in the results below are biased, though it is not possible to accurately estimate the size or direction of this bias. Females are more likely to participate in higher education than males[11], so attendance at a summer school may have a smaller effect on their future participation in HE as compared to males. If this is the case, the greater proportion of females in the control group could contribute to some downward bias.

Two points about randomisation are important to note. First, the imbalance on one observed covariate does not mean that the joint effect of all relevant covariates (many of which are unobserved) is not balanced; this quantity is unknown. Second, the purpose of randomisation is not to ensure that point estimates are unbiased by achieving perfect balance on relevant covariates. The purpose of randomisation is to ensure that the potential distribution of estimated treatment effects (reported in the results below as 95% confidence intervals) is unbiased; i.e. if we ran the experiment 100 times, the true effects would be in the 95% confidence intervals 95% of the time.

Table 7: Balance checks on survey 1 sample

	Intervention		Control		
	Mean	**(S.D.)**	**Mean**	**(S.D.)**	**Normalised difference**
FSM	0.20	0.40	0.20	0.40	0.00
White	0.56	0.50	0.52	0.50	0.08
Female	0.72	0.45	0.81	0.39	-0.22
Year 12	0.84	0.37	0.88	0.32	-0.12

Notes: N = 342. All variables are binary indicators, so mean averages represent proportions of the group. The ‘Unknown’ category in FSM is coded as missing in the dataset, so the reported means and S.D.s are of the non-missing sample.

11 https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment_data/file/843542/Publication_HEIPR1718.pdf.

To investigate the source of the observed imbalance, we have repeated the balance checks for the pre-attrition sample (all randomised students). Table 8 presents the results of these checks. It shows that the pre-attrition sample is well-balanced (by the definition used in this discussion) on all covariates. Comparing Table 7 and Table 8 shows that the sample was better balanced on sex at the point of randomisation, but that males and females dropped out from survey 1 completion at different rates in the intervention and control groups, leading to the imbalance observed in Table 7. This means that the intervention could have affected both the outcomes and the type of students who completed the outcome survey. The covariate adjustment used in the analysis below will partly adjust for this. However, it is still likely that there are differences between treatment conditions in unobserved characteristics which are non-random, are not fully correlated with our observed covariates, and will therefore lead to some bias in the results. The compliance analysis in the final report will check whether this imbalance in the outcome data is also seen in summer school participation. We do not expect to see it in the data for the primary and secondary outcomes because these data come from administrative sources.

Table 8: Balance checks on randomised sample

	Intervention		Control		
	Mean	**(S.D.)**	**Mean**	**(S.D.)**	**Normalised difference**
FSM	0.23	0.42	0.27	0.44	-0.09
White	0.52	0.50	0.50	0.50	0.04
Female	0.69	0.46	0.74	0.44	-0.10
Year 12	0.80	0.40	0.79	0.41	0.03

Notes: N = 802. All variables are binary indicators, so mean averages represent proportions of the group. The 'Unknown' category in FSM is coded as missing in the dataset, so the reported means and S.D.s are of the non-missing sample.

Table 9 shows the results of the balance checks for the survey 2 sample. It shows that this sample is well balanced on all characteristics.

Table 9: Balance checks on survey 2 sample

	Intervention		Control		
	Mean	**(S.D.)**	**Mean**	**(S.D.)**	**Normalised difference**
FSM	0.24	0.43	0.19	0.40	0.11
White	0.52	0.50	0.49	0.50	0.06
Female	0.79	0.41	0.74	0.44	0.12

Notes: N = 295. All variables are binary indicators, so mean averages represent proportions of the group. The 'Unknown' category in FSM is coded as missing in the dataset, so the reported means and S.D.s are of the non-missing sample.

Descriptive statistics for outcomes

Table 10 presents the means and standard deviations for the outcomes, broken down by treatment group. In general, it appears that both the intervention and control group performed similarly, with the intervention group responding more positively across four outcomes and the control group responding more positively on one outcome. Appendix II presents a more detailed breakdown of each outcome by the responses that make up the scales. This shows that across both conditions students were generally more likely to respond positively (rather than neutrally or negatively) to the survey questions. This is probably because students who apply for a university summer school are more likely to have favourable attitudes towards HE. We can also see that the self-reported rate of application to HE among the post-16 sample by January 2022 was very high in both the intervention and control group (94% and 91% respectively).

Table 10: Average outcome scores by treatment group

Outcome	Intervention	Control
	Mean (SD)	**Mean (SD)**
Likelihood of going to HE (7-point likert scale) (N = 342)	6.60 (0.99)	6.60 (0.98)

Likelihood of progressing to academic study post-16 (5-point likert scale) (N = 49)	4.71 (0.52)	4.73 (0.46)
Self-efficacy relating to HE (5-point likert scale) (N = 331)	4.06 (0.66)	3.91 (0.79)
Compatibility of HE with social identity (5-point likert scale) (N = 337)	3.97 (0.95)	3.83 (0.97)
Perception of practical barriers to HE (5-point likert scale) (N = 330)	3.38 (0.95)	3.26 (0.97)
Applied to HE (binary yes/no) (N = 295)	0.94 (0.23)	0.91 (0.29)
Notes: N per outcome included in brackets above.		

4.3. Outcome of analysis

Pre-specified analysis

Table 11 presents the estimated average effects of the summer schools on the outcomes of interest. The full regression tables are included in Appendix III. Likelihood of going to HE was measured using a 7-point Likert scale, and all other survey 1 outcomes were measured using a 5-point scale. Whether or not a student reported applying to university by January 2022 (the survey 2 item) was measured using a binary 'yes/no' question (coded as 1 for 'yes' and 0 for 'no'). Effects are also presented as Hedges' *g* to make it easier to compare between outcomes and with other studies[12]. Fig. 2 visualises the effect sizes with 95% confidence intervals.

The estimated effects are based on Model 1, which was the main model pre-specified in the trial protocol. For all outcomes, it includes a series of pre-treatment covariates in the regression[13]. Results can be interpreted as follows: The mean reported likelihood of

[12] Hedges' *g* is largely similar to the other common measure of standardised effect size, Cohen's *d*, but it relies on slightly different assumptions for the calculation of the standard errors, making it more conservative. It is also more robust to small sample sizes. We rely on the same formula as the one from the Education Endowment Foundation's Statistical Analysis Guidance (2022).

[13] As noted p.14, attainment measures are not included in the models as we don't have the data yet. Randomisation blocks are also not included because this constraint was not enforced by the HEPs. The variable "school ID", indicating the school students are from, is dropped from all regression analyses as there are too few observations for each level of this variable, which makes sense given the sample size. The inclusion of this covariate leads to issues when trying to fit the model, so we choose to exclude it from the models. Furthermore, "school year" also drops from the regression as it is fully collinear with the summer school indicator variable.

going to HE in the control group is 6.60 on a 7-point Likert scale. The estimated effect size in Model 1 is 0.06, which means that on average, and controlling for other variables in the regression, students in the treatment group scored 0.06 points higher on that scale, but this difference is statistically insignificant.[14] As another example, the mean reported self-efficacy related to HE in the control group is 3.91 on a 5-point Likert scale. The estimated effect size in Model 1 is 0.15, which means that on average, and controlling for other variables in the regression, students in the treatment group scored 0.15 points higher on that scale, and the difference is statistically significant at the 10% level.

Five of the estimated effects are directionally positive and one is directionally negative (though very close to zero). The results provide early evidence that the summer schools may have a null or very small positive effect on participants' self-reported likelihood of progressing to HE, and a small negative effect on self-reported likelihood of progressing to academic study post-16 (depending on their age). However, none of the estimates are significant at the 5% level. While this may partly be due to the small size of the sample, we cannot conclude with sufficient certainty that the results represent true effects as opposed to random noise. The confidence interval around the estimate of the (negative) effect on progression to academic study post-16 is particularly wide, due to the very small sample size.

The results also provide early evidence that the summer schools may have had a small positive effect on self-efficacy relating to HE and a smaller positive effect on self-reported applications to HE, compatibility of HE with social identity, and perception of practical barriers to HE. Again, these effects are not significant at the 5% level and are small overall (although the confidence intervals are relatively wide). The estimated (positive) effect on self-efficacy relating to HE is significant at the 10% level ($p = 0.08$).[15]

Table 11: Estimated effects for the outcomes of interest

Outcome	Estimated effect (score on scale)	Standard error	Estimated effect (Hedges' g)	P-value

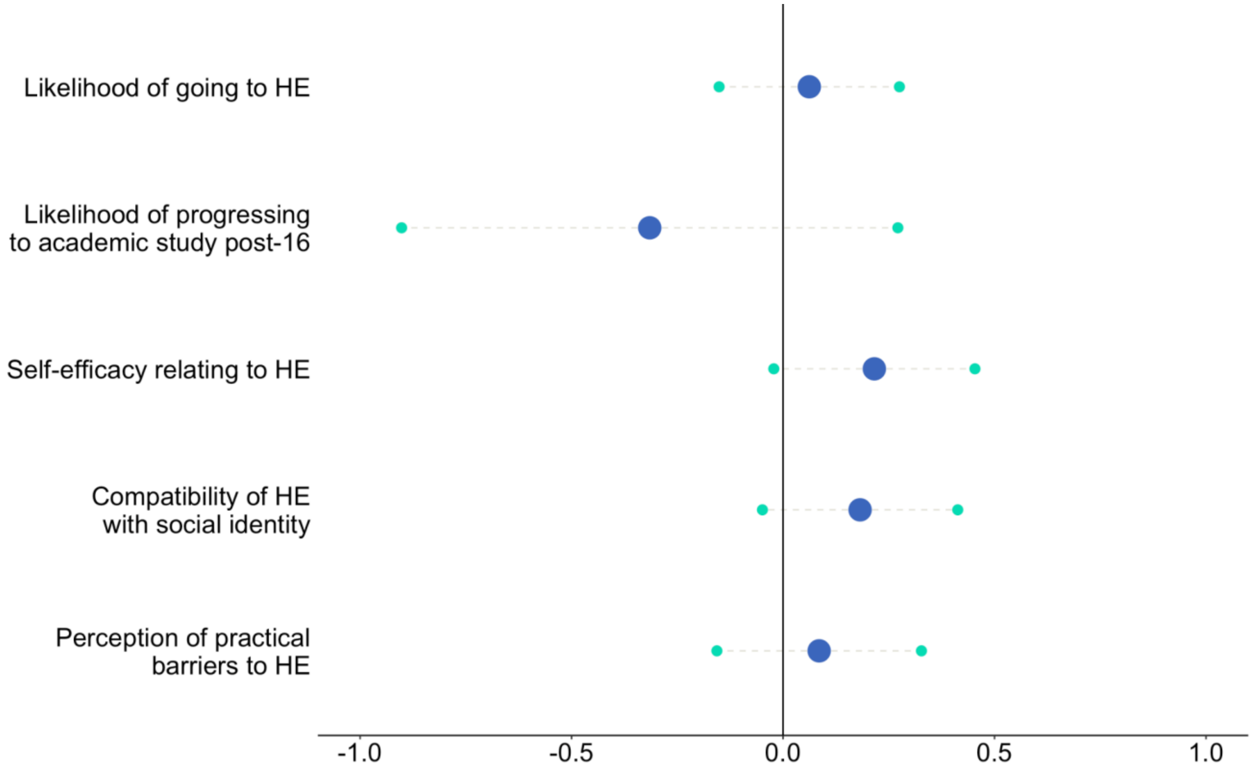
[14] Note that this estimated effect size is not equal to the difference in the unadjusted means of this outcome for the treatment and control group respectively, as reported in Table 10, as Model 1 includes covariates. The raw difference is equal to the effect size for Model 4 (no covariates), reported in Table 12.

[15] This study includes a large number of statistical tests. This increases the chance that a finding is statistically significant when there is no real effect. P-values can be adjusted to account for this issue. In this interim analysis, in line with the pre-specified protocol, we have not adjusted the p-values because the analysis is only exploratory. These findings are, as a result, less secure.

	Linear regression results			
Likelihood of going to HE (7-point likert scale) (N = 339)	0.06	0.11	0.06	0.57
Likelihood of progressing to academic study post-16 (5-point likert scale) (N = 49)	-0.16	0.15	-0.31	0.29
Self-efficacy relating to HE (5-point likert scale) (N = 328)	0.15^{+}	0.09	0.22	0.08
Compatibility of HE with social identity (5-point likert scale) (N = 334)	0.17	0.11	0.18	0.12
Perception of practical barriers to HE (5-point likert scale) (N = 327)	0.08	0.12	0.09	0.69
	Logistic regression results			
Applied to HE (binary yes/no) (N = 292)	0.32	0.55	–	0.56

Notes: N of observations for each outcome for Model 1 included in brackets above.
'Likelihood of going to HE' and 'Applied to HE' were computed for the post-16 sample only.
'Likelihood of progressing to academic study post-16' was computed for the pre-16 sample only.
All other effects were computed for the combined pre- and post-16 sample.
+ p<0.1, * p<0.05, ** p<0.01, *** p<0.001

Figure 2: Estimated effects size in Hedges' *g* for the outcomes of interest



Exploratory robustness checks

At the time of specifying the protocol, we did not know that baseline outcome data would be collected for the survey outcomes analysed here. As a result, we did not include baseline outcomes as covariates in the pre-specified analysis above. However, they are likely to be predictive of the post-intervention outcomes and to thus increase our statistical power.

To account for these baseline values, we have re-estimated the effects of all five outcomes above using the following three covariate combinations:

- The covariates specified in protocol plus the baseline measure of the outcome (Model 2)
- The baseline measure of the outcome only (Model 3)
- No covariates (Model 4)
- Linear estimation of the main model with covariates for the binary outcome (Applied to HE; Model 5)

Table 12 presents the estimated effects from the pre-specified model (Model 1) alongside the effects from these alternative models. For most outcomes, the results from the pre-specified analysis are broadly robust to these different covariate specifications. The directions of all point estimates remain the same and the confidence intervals remain wide. For the reported likelihood of going to HE, the direction of the effect switches between positive and negative, but it remains close to 0 and statistically insignificant across models. No results are statistically significant at the 5% level.

Table 12: Estimated effects with different model specifications

	Estimated effects (SE)				
Outcome	**Model 1**	**Model 2**	**Model 3**	**Model 4**	**Model 5**
Likelihood of going to HE (7-point likert scale)	0.06 (0.11)	0.04 (0.11)	-0.02 (0.11)	0.00 (0.11)	–
N.obs	339	339	342	342	
Likelihood of progressing to academic study post-16 (5-point likert scale)	-0.16 (0.15)	-0.08 (0.11)	-0.04 (0.11)	-0.03 (0.15)	–
N. obs	49	49	49	49	
Self-efficacy relating to HE (5-point likert scale)	0.15⁺ (0.09)	0.09 (0.08)	0.11 (0.07)	0.15⁺ (0.08)	–
N. obs	328	328	331	331	
Compatibility of HE with social identity (5-point likert scale)	0.17 (0.11)	0.14 (0.10)	0.11 (0.10)	0.14 (0.11)	–
N. obs	334	334	337	337	
Perception of practical barriers to HE (5-point likert scale)	0.08 (0.12)	0.10 (0.10)	0.10 (0.10)	0.11 (0.11)	–
N. obs	327	327	330	330	
Applied to HE (binary yes/no) (N = 295)	0.32 (0.55)	-	-	0.53 (0.45)	0.03 (0.03)
N. obs	292	-	-	295	292

Notes:
Model 1 = model specified in protocol.
Model 2 = model specified in protocol plus the baseline measure of the outcome.
Model 3 = the baseline measure of the outcome only.
Model 4 = no covariates.
Model 5 = linear estimation of Model 1 for binary outcome (applied to HE) .
+ p<0.1, * p<0.05, ** p<0.01, *** p<0.001

5. Discussion

Interpretation

This interim analysis suggests that the summer schools may have had a small positive effect on self-efficacy relating to HE and a smaller positive effect on self-reported applications to HE, compatibility of HE with social identity, and perception of practical barriers to HE. The evidence most strongly supports the idea that the summer schools have had a positive effect on participants' self-reported self-efficacy relating to HE. This is their confidence in their ability to apply to, and succeed at, university. However, none of these positive effects are significant at the 95% confidence level and the results are also consistent with negative and null effects. The analysis also suggests that there was no effect on students' self-reported likelihood of attending HE or post-16 academic study (depending on their age). This is probably because most applicants to HE summer schools already intend to follow these paths (as evidenced by the fact that over 90% of survey 2 respondents in both the intervention and control groups reported applying for HE by January 2022).

Generalisability

We can think about generalisability in two ways: i. the extent to which the results might be realised by other summer schools; and ii. the extent to which the results might be realised in different populations. On the first type of generalisation, it seems quite likely that the average effects achieved by the summer schools in this study would be achieved by other summer schools operating online. This is because a range of different types of summer school were included in this study (different subjects and different approaches). These different types may be more or less effective (we are not powered to test this), but the average effects are likely to be similar across all online summer schools that share similar aims and approaches.

On the second type of generalisation, we have shown that both the trial sample and the analytic samples (post-attrition) differ substantially from the general population of England in at least two important ways; the study included a much lower proportion of white young people and a much higher proportion of girls. We would, therefore, be unlikely to observe similar average effects if the same summer schools were run with a group of students that was representative of the wider English population. Having said this, summer schools that aim to widen participation in HE would be unlikely to aim for this kind of representation. The extent to which these summer schools saw similar effects would partly depend on the extent to which their cohort of students matched the characteristics of the cohort in this study. These cohorts are more likely to have greater proportions of female students and students from ethnic minority backgrounds (in line with the analytic sample in this study), but they may also have a higher-than-average proportion of FSM students (which our analytic sample did not).

Trial limitations

Three issues with the study have been discussed in this report. First, only a small proportion of the total sample at least partially completed survey 1 and survey 2 (43% and 46% respectively). For each outcome, a smaller proportion still had the complete outcome and covariate data required for the analysis. This means that the study may well be underpowered to detect the effects we are trying to estimate (hence the wide confidence intervals on the estimated effects). Second, some imbalance has been identified in the observed covariates for survey 1, with a greater proportion of female students in the control group as compared to the intervention group. The balance checks suggest that this imbalance is due to some differential attrition, which is unlikely to be fully dealt with by covariate adjustment, especially where it also led to imbalance on unobservables. Females are more likely to participate in higher education than males[16], so attendance at a summer school may have a smaller effect on their future participation in HE as compared to males. If this is the case, the greater proportion of females in the control group could contribute to some downward bias. Third, there was some variation in the timing and delivery of the survey 1 data collection by HEP. This may have led to lower completion rates for students from HEPs who issued their surveys late, and may also have led to attenuated effects among students from these HEPs. This variation has not introduced any bias into the results because randomisation was blocked at the summer school level.

While the estimates produced are imprecise, and there are some question marks over the validity of the results, the challenges that have led to this were expected. An outcome survey issued to students by email is unlikely to yield a high response rate, and it was also likely that certain types of student would be more likely to complete the survey (leading to differential attrition and potential bias). The intention of this interim

16 https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment_data/file/843542/Publication_HEIPR1718.pdf.

report was to provide early evidence of the effects of the interventions, before more robust and complete outcome data becomes available. The more robust test of the intervention will come in 2023 when we have administrative data on students' entry to HE.

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Appendix I: Intervention descriptions by HEP

The following descriptions summarise the activities in each summer school in the trial. This list does not cover all summer schools who began the trial and are listed in the trial protocol. This is because some summer schools dropped out of the trial before randomisation due to low applicant numbers.

University A (Languages)

A five-week programme with two hours of sessions each week focused on either French, German or Spanish, depending on the student's choice. The online workshops were designed to give a taster of studying languages at the university, expose students to the career opportunities available to graduates, and to provide the opportunity to meet current languages students. Further sessions included Joint Honors taster sessions, life at university as a languages student, informal networking/social sessions, applying through UCAS, writing a personal statement, Q&A with university alumni and a final celebration to showcase student learnings.

University A (Social Science)

The focus of this five-day event was on the 2021 United Nations climate change 'conference of the parties' (COP26). Students took part in a range of activities and workshops to tackle and understand how various Social Science subjects engage with climate change, learned about the upcoming COP26 conference, and considered how we can save the planet. There were six interactive academic workshops, giving students an insight into studying various subjects at university, and how these engage and respond to the topic of climate change. There was also an opportunity to speak to current students at the university regarding what it is like as a student, moving away from home, finances, the transition from school or college to university, clubs and societies, and anything else to do with student life.

University A (Bioscience)

This was a three-day online summer school for year 12 students who were interested in exploring biological sciences at the next level and finding out where it can lead. As well as taking part in lectures, students were involved in a project of their preference, getting to experience what research is really like from start to finish. Students had the opportunity to meet academics and current students from the Faculty of Biological Sciences and take part in a Q&A session to find out more about the university's undergraduate degree programmes and future career pathways. Sessions also focussed on enhancing a UCAS application, with an opportunity to speak to admissions staff, and receive advice on the best way to use what students have learnt at the event in their personal statements.

University A (Maths)

This short Zoom course for students in their first year of sixth form provided an introduction to mathematics at the university. Students were offered a preview of university life plus an insight into how mathematics develops at degree level. The university tutors extended and enriched student's existing study of mathematics at A-level (or equivalent) through lectures and interactive workshops. They also gave students an invaluable insight into the structure of mathematics degrees, the courses, admissions procedure and how to make the most of their application to university. Other sessions focussed on careers specific to this degree, and application support for writing a personal statement and student finance.

University A (Psychology)

This summer school was designed to give Year 12 students an insight into life studying Psychology at university, and the science behind why we behave like we do. Taking place over four days online, participants took part in subject masterclasses on different areas of psychology, heard from current students about their experiences studying psychology, and learned more about the process of applying to university. This included sessions specific to UCAS, writing a personal statement and applying for student finance.

University D

Eight subject specific summer schools were run. Several information and guidance (IAG) sessions were common across all the subjects. The sessions were delivered virtually through both synchronous and asynchronous modes of delivery, via virtual platforms including Blackboard Collaborate, Zoom and a bespoke platform developed by the Sutton Trust.

Common elements

This includes sessions covering personal statement writing, finances and careers, as well as information sessions for parents. There were also social sessions, including an online escape room, quiz and a takeaway evening. The week opened and closed with two large group events to welcome students and celebrate their completion of the summer school.

Subject specific sessions

Architecture

The theme of this summer school was "Patterns of Living". Students discovered each other's daily rituals and undertook a critical investigation of their own daily routines in an architectural context through walking, looking, drawing, and making. Students navigated the week through a series of lectures, podcasts, readings, demonstrations and hands-on tasks.

Astrophysics

Subject specific sessions included a virtual tour of the university Observatory; lectures (i.e., on coding using Python, space weather and earth management) with supporting Q&A sessions; support sessions; and a practical workshop (develop a research proposal for a space mission).

Biosciences

Subject specific sessions included introduction to the staff and summer school; lectures (i.e. preparation for plastic pollution in the oceans and molecular machines); team work on presentations for the end of the week, supplemented by presentation skills and support sessions.

Chemical Engineering

Subject specific sessions included introduction to the teaching team and departmental staff; conducting experiments themselves, both on the computer and in their homes, with supporting results discussion and Q&A; live demonstrations; research skills lecture; designing own experiment that was presented to the group through a poster; talks with both current students and admissions tutor.

Economics

Subject specific sessions included introduction to the staff and the summer school along with an economics walk; lectures (i.e., economics, sports and social media and economists save the world); guest talks on economics graduate experiences; poster creation and presentation; meeting the career and admissions tutors.

Health and Wellbeing Data Science

Subject specific sessions included introduction to the summer school and staff; lectures including recommended reading; seminar sessions to discuss readings and do tasks; practical sessions (i.e., Excel).

History

Subject specific sessions included general introduction to the staff and summer school, including an icebreaker session, and then daily intro sessions; lectures (i.e. place, space and material culture); seminars (i.e. material culture); independent study with tasks (presentation preparation) and miscellaneous events (tour of British Museum); admissions talk from tutors and student ambassadors.

Natural Sciences

Subject specific sessions included introduction to staff and the programme; lectures; workshops discussing material from lectures; project work sessions, working towards a presentation at the end of the week; admissions and career talk.

University F

The summer school was a four-day online event for Year 10 students, using both Zoom and Thinkific. Day one involved ice-breakers and an introduction to current university students, followed by activities which focussed on barriers to happiness and approaches to positive psychology. The second day focussed on academic barriers. Students undertook a practical research session to enable them to answer questions around this topic, drawing on examples from multiple disciplines. The third day focused on the ‘Big Question’ around community barriers – ‘How can we make our communities better places in the wake of COVID-19?’ which involved students creating a submission for a digital time capsule. Students worked in small groups to brainstorm ideas for focuses and mediums for their response and worked independently engaging with relevant materials. The final day was a launch and celebration event, which parents/carers were invited to. Whilst most activities were live, students could carry out independent activities at any time using Thinkific.

University G (cancelled due to COVID-19)

The summer schools were intended to be a one-day programme, held in two different schools, designed to give pupils a miniature experience of university life. Year 9 pupils would have taken part in a number of activities alongside a mini research project, for which they would have received a university style criteria and grading upon completion. Pupils would have selected which mini-lecture (out of a possible 6 courses) they would like to ‘attend’ and would have been given a question to answer in the form of an academic poster. They would have been given time during the day to complete their poster and would have been able to utilise the support of the Outreach Ambassadors and members of staff to do this. Pupils would have been supervised and supported by members of university staff and student ambassadors throughout this process but pupils themselves would have been in control and responsible for producing their academic poster. Attendees would also have had the opportunity to take part in an activity based on a society or club that the university offers, for example, knitting. Finally, pupils would have received some information, advice, and guidance on the main aspects of university (e.g., courses, finance, and extracurricular activities.)

Appendix II: Distribution of responses to outcome survey questions

Table 13: Distribution of responses to outcome survey questions

	Intervention (N = 214)	Control (N = 128)
Likelihood of going to HE		
Extremely likely	169 (79.0%)	103 (80.5%)
Likely	25 (11.7%)	12 (9.4%)
Somewhat likely	11 (5.1%)	4 (3.1%)
Neutral	4 (1.9%)	7 (5.5%)
Somewhat unlikely	1 (0.5%)	1 (0.8%)
Unlikely	0 (0%)	0 (0%)
Extremely unlikely	4 (1.9%)	1 (0.8%)
Missing	0 (0%)	0 (0%)
Likelihood of progressing to academic study post-16		
Extremely confident	25 (73.5%)	11 (73.3%)
Quite confident	8 (23.5%)	4 (26.7%)
Neutral	1 (2.9%)	0 (0%)
Not that confident	0 (0%)	0 (0%)
Not confident at all	0 (0%)	0 (0%)
Missing	0 (0%)	0 (0%)
Self-efficacy relating to HE		

Extremely confident	46 (21.5%)	24 (18.8%)
Quite confident	133 (62.1%)	73 (57.0%)
Neutral	24 (11.2%)	20 (15.6%)
Not that confident	3 (1.4%)	6 (4.7%)
Not confident at all	1 (0.5%)	1 (0.8%)
Missing	7 (3.3%)	4 (3.1%)
Compatibility of HE with social identity		
Strongly agree	73 (34.1%)	33 (25.8%)
Agree	75 (35.0%)	52 (40.6%)
Neither agree nor disagree	50 (23.4%)	29 (22.7%)
Disagree	12 (5.6%)	8 (6.3%)
Strongly disagree	2 (0.9%)	3 (2.3%)
Missing	2 (0.9%)	3 (2.3%)
Perception of practical barriers to HE		
Extremely confident	21 (9.8%)	10 (7.8%)
Quite confident	76 (35.5%)	46 (35.9%)
Neutral	73 (34.1%)	39 (30.5%)
Not that confident	29 (13.6%)	27 (21.1%)
Not confident at all	6 (2.8%)	3 (2.3%)

Appendix III: Full regression tables for the main model with covariates (Model 1)[17]

	Outcome variable:		
	Likelihood of going to HE (OLS)	**Likelihood of progressing to study post-16 (OLS)**	**Self-efficacy relating to HE (OLS)**
Treatment	0.06 (0.11)	-0.16 (0.15)	0.15⁺ (0.09)
Sex: Male	-0.02 (0.12)	-0.19 (0.18)	0.26** (0.10)
Sex: Other/Unknown	0.53** (0.17)	0.88** (0.20)	0.59** (0.18)
Ethnicity: Black	-0.04 (0.14)	0.34 (0.29)	0.25⁺ (0.13)
Ethnicity: Missing	0.27 (0.29)		-0.81 (0.72)
Ethnicity: Other	-0.26 (0.19)	-0.32 (0.22)	0.09 (0.19)
Ethnicity: White	-0.27⁺ (0.15)	0.29⁺ (0.16)	-0.01 (0.11)
IMD: Quintile 2	-0.31* (0.13)	0.24 (0.16)	-0.08 (0.13)
IMD: Quintile 3	-0.11 (0.16)	0.20 (0.21)	0.22 (0.14)

[17] In all the models, attainment measures (combined KS2 Maths and English scores and KS4 attainment 8 score) are not included as covariates as we don't have the data yet. Randomisation blocks are also not included because this constraint was not enforced by the HEPs. The variable "school ID", indicating the school students are from, is dropped from all regression analyses as there are too few observations for each level of this variable, which makes sense given the sample size. The inclusion of this covariate leads to issues when trying to fit the model, so we choose to exclude it from the models. Furthermore, "school year" also drops from the regression as it is fully collinear with the summer school indicator variable.

IMD: Quintile 4	-0.14 (0.19)	-0.12 (0.22)	0.16 (0.17)
IMD: Quintile 5	-0.11 (0.19)	0.57^{*} (0.28)	0.06 (0.16)
POLAR4: Quintile 2	-0.11 (0.20)	0.03 (0.20)	0.10 (0.15)
POLAR 4: Quintile 3	0.19 (0.15)	0.31 (0.21)	-0.001 (0.16)
POLAR4: Quintile 4	-0.13 (0.19)	0.41^{+} (0.21)	-0.01 (0.16)
POLAR4: Quintile 5	-0.08 (0.19)		-0.10 (0.18)
FSM: Yes	-0.27^{+} (0.16)	-0.30 (0.20)	0.06 (0.11)
FSM: Unknown	-0.23 (0.26)	0.29 (0.32)	0.02 (0.15)
Parent attended HE: No	-0.01 (0.11)	-0.28^{+} (0.16)	-0.06 (0.09)
Intercept	6.84^{**} (0.24)	4.57^{**} (0.29)	3.44^{**} (0.19)
Summer School ID	Yes	Yes	Yes
N. obs	339	49	328
Note:			$^{+}p<0.1$; $^{*}p<0.05$; $^{**}p<0.01$

Outcome variable:

	Compatibility of HE with social identity (OLS)	Perception of practical barriers to HE (OLS)	Applied to HE (logistic)
Treatment	0.17 (0.11)	0.08 (0.12)	0.32 (0.55)
Sex: Male	0.09 (0.14)	0.16 (0.13)	1.25 (0.84)
Sex: Other/Unknown	-0.27 (0.17)	0.20 (0.61)	18.09 (17,730.37)
Ethnicity: Black	-0.02 (0.17)	-0.03 (0.19)	-0.56 (0.94)
Ethnicity: Missing	-0.32 (0.31)	-0.19 (0.76)	15.31 (8,951.70)
Ethnicity: Other	0.09 (0.20)	0.22 (0.22)	0.60 (1.26)
Ethnicity: White	0.04 (0.14)	0.02 (0.15)	0.63 (0.70)
IMD: Quintile 2	-0.24^{+} (0.14)	0.11 (0.17)	-1.19 (0.94)
IMD: Quintile 3	-0.01 (0.18)	0.27 (0.18)	-1.33 (1.04)
IMD: Quintile 4	-0.25 (0.20)	0.37^{+} (0.21)	-2.10^{+} (1.10)
IMD: Quintile 5	-0.37 (0.23)	0.12 (0.23)	-1.73 (1.17)
POLAR4: Quintile 2	0.31^{+} (0.18)	-0.26 (0.23)	17.11 (2,462.97)
POLAR 4: Quintile 3	0.18 (0.18)	-0.08 (0.20)	-1.10 (1.25)
POLAR4: Quintile 4	0.14 (0.18)	-0.12 (0.19)	-1.80 (1.30)

POLAR4: Quintile 5	0.04 (0.22)	0.01 (0.23)	-0.55 (1.39)
FSM: Yes	-0.30* (0.14)	-0.07 (0.15)	-0.61 (0.68)
FSM: Unknown	-0.44 (0.27)	-0.42 (0.27)	0.41 (1.28)
Parent attended HE: No	-0.13 (0.12)	-0.05 (0.13)	-1.19+ (0.65)
Intercept	3.49** (0.26)	3.03** (0.24)	4.91** (1.45)
Summer School ID	Yes	Yes	Yes
N.obs	334	327	292
Note:			+p<0.1; *p<0.05; **p<0.01