Equity Frontiers Trial Protocol

"The First Year Success Program: Do Bridging Courses Improve Success and Retention at University for Equity Students?"

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1 Trial Overview

Trial Title:	The First Year Success Program: Do Bridging Courses Improve Success and Retention at University for Equity Students?		
Trial Start Date:	1/12/2025	**Trial Finish Date:**	31/12/2026
Principal Investigator Name:	Prof. Peter Siminski		
Principal Investigator Contact Details:	Peter.Siminski@uts.edu.au		
Sponsoring institution name(s)	University of Technology Sydney, Centre for Social Justice and Inclusion		
Trial summary (<300 words)	The intervention is the First Year Success Program (FYSP). FYSP consists of a range of activities which aim to help equity students transition to university life. These include academic bridging courses, workshops, social events, and peer mentoring. Our trial involves randomly offering eligibility to participate in the FYSP. Our study population consists of low SES, regional and remote, indigenous, and first-in-family students, as well women in non-traditional areas including STEM and economics. To encourage participation from students who receive an offer, we will provide information about the potential benefits as well as financial assistance to cover attendance costs. The primary objective is to estimate the effects of this established pre-university transition program on the success of various groups of equity students at university. Observational evaluations are generally encouraging as to the benefits of this and similar programs. To our knowledge, however, there is no experimental, or quasi-experimental evidence on the effectiveness of such programs in Australia. This trial will fill this gap in the evidence base. The study will provide the most in-depth evidence in Australia on the effects, and cost-effectiveness, of this broad pre-university transition program. It will attempt to identify which of its many components have the largest impact, and which groups of equity students benefit the most.		
Funding (total, and funding sources)	$163,024 from the Australian Centre for Student Equity and Success (ACSES)		
Ethics approval (grantor, date, reference ID)	$163,024 from the Australian Centre for Student Equity and Success (ACSES)		

2 Background and intervention

2.1 Background

Overall, previous research shows that well-designed interventions targeting disadvantaged university students can improve performance and reduce attrition. We provide an overview of this research below. This research stresses the importance of student take-up for adequate statistical power (Paloyo et al., 2016, Azzam et al., 2022, Pugatch & Wilson, 2024).

Similar to our intervention are the following quasi-experimental studies of various remedial courses designed to aid academically underprepared students transition to university. Lavy & Schlosser (2005) found that these programs increased enrolment into university in Israel while Bettinger & Long (2009) estimate that the remedial programs lead to increased college persistence in the state of Ohio in the US. Martorell & McFarlin Jr. (2011) find little impact of these remedial courses on academic outcomes in the state of Texas in the US, demonstrating the importance examining the effectiveness in different contexts.

Gordanier et al (2019) study the additional academic support offered early in the semester to underperforming students in the US. Using a quasi-experimental methodology, they find that the program increased scores on the final exam by around 7 percentage points. The effect is more pronounced for students with poor prior academic achievement in math.

Albert and Wozny (2024) study the effect of mandatory academic probation in the US Air Force Academy, noting their concern that an intensive intervention may lead to students dropping out. Using a quasi-experimental design, they find that the program increased academic performance and degree completion, especially for STEM degrees and don't find any negative impacts on attrition.

Pugatch and Wilson (2018) advertised their program using postcards in an attempt to increase take-up of a US college's peer tutoring services. Their simple and cost-effective treatment increased use of the program by 7 percentage points. Despite the effect on take-up, they did not find a statistically significant effect on grades due to lack of power in their design, stemming from a low sample size in their treated group (N<100).

Brownback and Sadoff (2024) evaluate the effect of attending summer school on degree completion for a US university. Randomly assigning scholarships to attend summer school, they find a large impact on degree completion. They find that students with stronger preferences for other programs actually have higher treatment effects than those who preferred the offered program. This suggests, in their context, that students who are less likely to seek out the offered program may stand to gain the most from it.

Leuven et al (2010) randomly assign financial incentives for students to pass all their subjects in the first year. They find that the effect is positive for high-ability students and negative for low ability students. This is consistent with the behavioural theory of crowding out of intrinsic motivation, as students may be no longer studying because they fundamentally want to.

In the Australian context, there is a lack of causal evidence on the effectiveness of academic interventions designed to help disadvantaged students. The study by Paloyo, Rogan, and Siminski (2016) estimates the causal effect of peer assisted study sessions (PASS) on students' academic performance at the University of Wollongong. Using a randomized encouragement design, they offered financial incentives for a random set of students to attend PASS throughout the semester. This increased take-up of the program relative to the control group, as the random incentive led to the treatment group being 4.6 percentage points more likely to attend eight or more sessions and 4.2 percentage points less likely to not attend any sessions. They estimated that one hour of PASS increased grades by 0.065 standard deviations but note that their estimate has a wide confidence interval, reflecting limited statistical precision.

Existing observational research provides an overall positive portrayal of bridging and enabling programs in Australian universities. Crawford (2014) provides a qualitative analysis of University of Tasmania's enabling program, called the University Preparation Program, and found that former participants viewed it as useful for academic skills, developing confidence, and connections.

Li et al. (2023) examines academic performance based on entry pathways to university for students across 16 Australian universities. They show that students admitted via alternative pathways generally have poorer academic scores, especially for students from vocational education. However, they find that students from enabling and bridging programs perform better academically compared to secondary school entrants.

Similarly, Pitman et al. (2016) evaluate outcomes for students transitioning to university via multiple pathways including enabling and bridging programs, with a focus on equity group students. Whilst the study is observational, they note that bridging and enabling programs resulted in better retention rates, especially for equity group students. Aboriginal and Torres Strait Islander students in particular experience positive retention outcomes when transitioning through enabling programs and low-SES students, students from non-English speaking backgrounds, and women enrolled in non-traditional areas of study transitioning via enabling programs also experienced better success rates.

2.2 Intervention

The intervention is the First Year Success Program (FYSP). There are three main components of the FYSP: 1) an introductory "HeadStart" workshop, 2) academic bridging courses, and 3) peer mentoring. The HeadStart workshop is the first of the FYSP sub-programs and takes place in mid-January. It provides students with important details and guides for starting university. The peer mentoring program runs throughout the first semester, where students will have access to a peer who is an existing University of Technology Sydney (UTS) student with whom they can ask questions and seek guidance from. The academic bridging courses are delivered in-person and on campus. These courses are delivered by the UTS science faculty and independently organised outside of the FYSP. The bridging courses start after the HeadStart workshop and run from late January to mid-February, prior to the semester commencing. There are five separate bridging courses: mathematics preparation, mathematics, physics, chemistry, and biology. The courses are designed to provide students with foundational skills in the areas needed for their degree, which is assumed prior knowledge. The courses run between 8–10 days and each daily session is on average three hours, resulting in contact hours of around 30 hours per course (with slight variations across each course). In 2025, the cost is $396.00 per course and are available to all students at this price. However, students in the FYSP are able to access the courses for free.

2.3 Objectives

The purpose of the trial is to provide causal evidence on the effectiveness of the FYSP. The FYSP is designed to help students from disadvantaged backgrounds (equity students) transition to university. It is well known that these students tend to have lower academic performance and have higher rates of attrition (Australian Universities Accord, 2024). The objective is to provide causal evidence on whether the FYSP can improve academic performance and reduce the likelihood of dropping out for equity students. The hypothesis to be tested in this trial is that the FYSP will improve each outcome measured on average for all equity groups involved. This is supported by the theory that both the academic and non-academic components of the FYSP will reduce the impact that the shock of transitioning to university has on students, especially for those who may be less prepared for university. Academically, providing students with foundational knowledge and skills which is assumed for their degrees will ensure that they don't begin the semester behind, which should lead to improved academic performance. If students are better equipped for university and don't struggle academically, they should also be less likely to drop out. In addition, the non-academic components should help provide other support and opportunities for students to meet new people as well, thus improving their social capital and also reducing the likelihood of dropping out.

3 Trial design

3.1 Outcomes

Our main outcomes of interest are standardised student grades in their first-year subjects and whether they are re-enrolled in the second semester of their first year or have left or paused their studies. Information on student marks and enrolment are directly measured through the UTS administrative system. Grades are measured at the subject level as a raw score on a 0 to 100 scale, and we transform this into a standardised measure with a mean of zero and a standard deviation of one. This is done by subtracting the mean of all grades from each student's score for each subject and then dividing by the standard deviation of the grades. The resulting outcome will be standardised student grades at the subject level. Enrolment in the second semester will be a binary indicator equal to zero if the student re-enrols in their subjects and one if the student has left or paused their degree (i.e. they do not re-enrol in subjects for their second semester). Attrition is calculated at the student level, meaning that the attrition rate is not subject specific. The mean of this variable is the attrition rate or probability that a student fails to continue their studies. We also use success rate as a secondary outcome, measured as the number of subjects passed divided by the number of subjects taken.

3.2 Trial design

We implement a Randomized Controlled Trial. Participants in our trial will be randomly allocated into treatment and control groups. We use stratified block randomisation to achieve balanced group sizes over each round of offers and within each equity group and faculty. We conduct the randomisation and send out the offers over multiple rounds to allow enough time for as many students as possible to opt-in (but each round the randomisation is conducted in the same way via stratified blocks at the equity and faculty group level). The treatment group will receive an offer to participate in the FYSP whilst the control group will not receive anything. This means there will be non-compliance from both treatment and control, as not everyone in the treatment will attend and some of the control group may attend the bridging courses (which can be accessed for a fee). In the offer letter and email, the treatment group will be encouraged to participate in the three programs.

For the bridging courses we inform the treated students that they can attend the course, which is normally priced at $396, for free. We also inform these students that they will be paid if they attend the bridging courses. The payment is calculated and sent via email for an e-voucher daily. The maximum payment a student can receive is $100. This payment is framed as payment cover any personal costs associated with attendance, rather than as an incentive for attending. The framing of the payment is consistent with other experimental evidence (Leuven et al., 2010) which shows that financial incentives can crowd out student's intrinsic motivation to attend, leading to lower attendance. Put simply, students will be more likely to attend because they are intrinsically motivated to do so instead of being compensated for it. These initial round of communications to the students in the treatment group will also include small informative nudges, designed to highlight the potential difficulties of transitioning to university and how attending this program may help them. We also inform participants who receive an offer that the number of places are limited (the exact amount of budgeted places varies depending on if students attend the entire bridging program or not).

3.3 Detailed trial process

Upon receiving an offer to study at UTS and meeting one of the equity categories outlined below, students are contacted via email by the research team to be recruited into the study. We recruit students by highlighting potential benefits of being involved in the trial (a 50% chance of receiving an offer to participate in the program) and the importance of contributing to a better understanding of what helps students succeed at university. They aren't paid or promised anything in return for their participation in the study, *per se*. The email contains a link to an online form which contains the participant information sheet and collects their opt-in responses. If a student consents and agrees to be involved, they are included in the final sample. We compare the distribution by equity status and faculty of all students contacted with the final sample of students to understand how our final sample is different from the overall study population.

We perform the randomisation in two stages, which allows for as much time as possible for students to opt-in to the study. We implement stratified block-randomisation to ensure a similar proportion of treated and control within each equity and faculty group. If a student is randomly allocated to the treatment group, they receive an offer to participate in the FYSP. The offer is communicated via letter and email. The email contains a link to an online form where they can get more information and register for the Introductory workshop and the bridging courses. If a student is randomly allocated to the control arm, they do not receive any communications or offers or financial payments from the researchers. Only students who are in the treatment group will receive an offer and only those attend the bridging courses will receive financial payment.

After students have been sent the initial offer, they will then receive a tailored follow-up reminder via text message. If they have registered, the text message will remind them that they have registered and there is a spot waiting for them. If they haven't registered, the text message will encourage them to register and attend. The students can then attend the

Introductory workshop and bridging course. Attendance for each program is recorded and measured in order to correctly estimate the effect and administer electronic visa gift vouchers via email. This is also done routinely within the existing administration of the FYSP. The amount of the voucher depends on the number of days a student attends the bridging courses

Students will then continue their process of commencing their first semester. After the first semester has finished, we will obtain the outcome data for our final sample including their marks in each subject and whether they have re-enrolled for the second semester. Outcome data beyond the first semester may be collected in order to assess the persistence of any effects over time.

3.4 Participants

Our study population is the 2026 commencing cohort of undergraduate equity students at UTS who meet at least one of the following categories: First-in-family, women in STEM/Economics, low SES, regional or remote, and indigenous. The size of this population at UTS has historically been approximately 3,500 students. We will contact all of these students, inviting them to participate in the study. It is hard to know the proportion of students who will agree to participate in the research. We benchmark our result to a similar study (Pugatch & Wilson, 2024) conducted by researchers at the university of Oregon which had a recruitment rate of 85%. Program take-up (the number of students who actually attend the FYSP out of those who it is offered to) is very important for statistical power; as take-up increases, statistical power improves. Similarly to the proportion of students who opt-in, it is also inherently difficult to predict the take-up proportion.

Our main outcome used to define the success of the program is standardised grades at the subject level. We also use the attrition rate or the number of students who leave their studies out of the total number of students. The expected effect size is hard to precisely predict, and similar studies have found a range of magnitudes (Pugatch & Wilson, 2024, Brownback & Sadoff, 2024). Our intervention is by no means “light touch”, as it involves three separate academic and non-academic components separately. The bridging courses, if fully attended, pose a significant educational investment specific to the field of study of the student. As such, we anticipate a moderate sized effect of between 0.2–0.4 standard deviations for standard grades and between 5 to 8 percentage points for the attrition rate.

The introduction of the opt-in mechanism alters the standard power calculations and introduces a broader range of possibilities. The final sample size is now determined by how many students agree to participate in the study and this has implications for statistical power. In presenting the power calculations we show a variety of scenarios for both the number of students opting in to the study (consent) and the number of students who participate in the study (take-up). In determining the minimum detectable effect (MDE), we follow the standard calculations presented in Equation 1 in the Appendix. We then display all the parameter values in table 1 in the appendix. The MDE is a function of the level of statistical power, conventionally set at 80%. The level of statistical significance is set to 5% (with the associated t-statistics denoted by $t_{power}\ and\ \ t_{significance}$). The calculation assumes that 50% of the sample allocated to the treatment group (denoted by P, which maximises power) σ captures the variance of the outcome, whilst C, T, and N represent consent rate, take-up rate and total number of observations respectively. Our outcome of student marks will be standardised (i.e. to have a mean of 0 and variance of 1). Our outcome of attrition will be measured as a binary outcome (i.e. equal to one if a student leaves their studies and zero otherwise), the variance is estimated based on the assumed mean of 20% attrition.

The corresponding MDE values for different scenarios of opt-in and take-up rates are presented in table 2 and 3 for the attrition rate and standardised grades respectively. It is evident that high levels of recruitment and program take-up provide the largest amount of statistical power (these are the scenarios where the MDE is the lowest). It is difficult to ascribe likelihoods over the various scenarios shown in each of the tables. However, we believe that our recruitment rate is likely to lie between 50%–70% and our take-up rate between 30%–50%. This yields the following range of MDEs for attrition between 9.14 and 18.03 percentage points and for standardised grades between 0.229 and 0.451 standard deviations.

3.5 Participant withdrawal criteria and procedures

We provide students with the option to withdraw from the study when we first obtain their contact details. This is done prior to randomisation and classification of students into treatment and control. We give students a soft deadline of March 1st for which they should inform us that they would like to withdraw from the study, as after this, the data will become de-identified, and it will be difficult to remove a given participant from the data (although not impossible). Students are also reminded that they retain the right to withdraw at any point in the study, but no further dates or reminders are given. Withdrawn participants are not replaced.

4 Data management and statistical analysis

4.1 Statistical methods

We firstly report the proportion of students who participate in at least one FYSP activity out of all those who are in the treatment group. This is the overall take-up rate, and it is equivalent to estimating the effect that the offer has on overall participation when the control group doesn't have access to the activity. Then, we report the take-up rates for each activity, including the net take-up rate for the bridging courses (take-up among treated students minus the take-up rate among the control). Where an activity can be measured continuously, we report the average number of sessions attended by participants. This allows us to see how significant the impact of the offer is on FYSP attendance and also allow us to comment on the strength of our instrument, which will be relevant for our main analysis.

When conducting the main analysis, we use the following equations. When estimating the effect of the FYSP on standardised grades, we need to refer to equations (1) and (2) where the analysis is at the student (i) - subject (j) level.

$$Y_{ij} = \gamma D_{ij} + \boldsymbol{\delta}'\boldsymbol{X_{ij}} + \nu_{ij} \tag{1}$$

$$D_{ij} = \pi Z_{ij} + \boldsymbol{\phi}'\boldsymbol{X_{ij}} + \xi_{ij} \tag{2}$$

When estimating the effect on the rate of attrition (measured as a binary variable equal to one if the student drops out and zero otherwise) and the success rate (the number of subjects passed divided by the number of subjects attempted), we refer to equations (3) and (4), where the analysis is conducted at the student (i) level:

$$Y_i = \gamma D_i + \boldsymbol{\delta}'\boldsymbol{X_i} + \nu_i \tag{3}$$

$$D_i = \pi Z_i + \boldsymbol{\phi}'\boldsymbol{X_i} + \xi_i \tag{4}$$

In the equations above, Y is the outcome of interest, D is a binary variable equal to 1 if the student participates in at least one FYSP activity, Z is a binary variable equal to 1 if the student received a random offer and $\mathbf{X}$ contains a set of baseline characteristics including gender, SES, basis of admission, ATAR, and indicators for faculty, degree, and subject. ν and ξ are the error terms in our regression. We report the Ordinary Least Squares (OLS) estimate of the effect of D on Y as well as the intention-to-treat (ITT) estimate of the effect of Z on Y. The main result is our Two-Stage Least Squares (2SLS) estimate of the effect of D on Y or the effect of participating in the FYSP on the academic outcomes. This estimation accounts for selection and actual program attendance. Therefore, it is our main estimate of the causal effect of attending the FYSP on student's academic outcomes. The comparison of the 2SLS estimate with the OLS and ITT estimate is useful as the OLS estimate doesn't account for selection into the program, while the ITT effect is the un-scaled effect of our program. Comparing these estimates shows how selection into participation by treated students and the program's take-up rate have each influenced our main effect.

Going beyond the overall effect of the program, we provide estimates of the effect of the separate FYSP components. It is not sufficient to run a regression with separate indicators for each activity as we do not conduct separate random offers for each activity (i.e. separate treatment arms). As such, this estimation would be biased as different attendance patterns are not likely to be random, and this would not identify the separate effect of each activity. As a result, we employ a different strategy by partitioning the program attendance into academic and non-academic components. The academic component of the FYSP is just the bridging courses, while the non-academic components include the Introductory workshop and the peer mentoring program. In order to separately identify the effect of these two components using 2SLS we need an additional instrument. We construct this additional instrument by exploiting the fact that the academic component is more relevant to students studying in degrees that require underlying knowledge in mathematics, chemistry, physics, and biology. Therefore, the identifying assumption is that students attend bridging courses at higher rates because they are enrolled in degrees relevant to the courses. We then obtain the effect of the academic (A) and non-academic (NA) components of the FYSP through estimating the equations below:

$$Y_i = \gamma^A {D_i}^A + \gamma^{NA} D_i + \boldsymbol{\delta}'\boldsymbol{X_i} + \nu_i \tag{5}$$

$$D_i = \pi^A Z_i Q_i^A + \pi^{NA} Z_i + \boldsymbol{\phi}'\boldsymbol{X_i} + \xi_i \tag{6}$$

$${D_i}^A = \lambda^A Z_i Q_i^A + \lambda^{NA} Z_i + \boldsymbol{\phi^{A'}}\boldsymbol{X_i} + {\xi_i}^A \tag{7}$$

Where the variables maintain their definitions from before but now ${\mathrm{D}_\mathrm{i}}^\mathrm{A}$ represents attendance for the bridging courses (for which we will use both binary and continuous measures). The resulting estimates for FYSP attendance (D_i) now correspond to the effects of the non-academic components, hence the subscripts NA over the respective coefficients γ, π, and λ. $\mathrm{Q}_\mathrm{i}^\mathrm{A}$ is a binary indicator for whether a student is enrolled in a degree which is relevant to one of the bridging courses. Similar to our analysis of the overall program effect, we report the OLS and ITT estimates alongside the 2SLS estimate.

In order to interpret this as the separate effect of each program, some additional assumptions are required. These include that (a) the average treatment effect of the FYSP is the same among students in the academic and non-academic components, and (b) that there are no interaction effects among the separate components. We acknowledge that these are strong assumptions, and as a result, the analysis on the separate components will carefully consider this. In addition, we note that the interpretation of this effect can vary depending on additional factors relating to student take-up patterns (Kormos et al., 2025). Additional tests and required assumptions are considered where relevant. If identifying the effects of different program components isn't feasible (for example due to violation of one of the assumptions outlined in Kormos et al., 2025), we expect our treatment effect heterogeneity analysis by faculty to be sufficiently informative. Specifically, we examine overall program effects for students in STEM versus non-STEM faculties (see section 4.5).

Our chosen level of significance is 5%, in line with conventional practice. We expect that the FYSP activities, when fully attended, pose a substantial educational investment by students. As such, we anticipate that our effect size would be moderate, in the range of 0.2–0.4 standard deviations for standardised grades and 5–8 percentage points for the attrition rate.

4.2 Sample cleaning

The sample is cleaned to ensure that there are no duplicate records. The dataset is at the student subject level, so each record pertains to a student in 2026 taking a certain subject in a given semester. Duplicate records can occur in the administrative database due to a number of factors such as multiple enrolments or degree changes. Any other data sample cleaning in addition to this will be clearly outlined in the data description (for example grade description not matching mark value).

4.3 Sample inclusion/exclusion

We begin by contacting all eligible students who have received an offer from UTS. This amount is expected to be approximately around 3,500 students based off historical enrolments. Students then choose whether to opt-in to the study. We report the difference in distribution of contacted versus recruited students by equity group, faculty, and gender. After this, only the data of consenting students is used in the analysis. The expected number of successfully recruited students can vary greatly. All randomised participants will be included in the analysis.

4.4 Data Management

The list of eligible students will be obtained from the admissions team. This list will contain the contact information for all contact mediums listed above including email and postal addresses and phone numbers. This dataset will then record opt-in status, treatment group status from randomisation, and attendance information for each sub-program. When the outcome data becomes available, the records for students who have opted-in will be matched using their student identifier number. The final resulting dataset will have all necessary information for student outcomes, treatment, and attendance information as well as student information including equity group status, degree, faculty, gender, ATAR, and basis of admission. The analysis of this dataset is performed in Stata.

4.5 Additional analyses

We perform analysis for the overall program take-up and take-up for each activity (both using a binary and continuous measure) and report the results by equity group, faculty, gender, basis of admission, and ATAR. This will allow us to see for which types of students the offer had a larger impact on. We expect that certain aspects of the program may be more appealing to different types of students, such as the bridging courses for students in the STEM faculties.

We estimate the overall program effect across the three outcomes of interest separately for each faculty and equity group as well as by gender, basis of admission, and ATAR. We also estimate the effect of the separate components where possible across faculty, equity group, gender, basis of admission, and ATAR. This is constrained by the group's definition. For example, it will not be possible to estimate the effect of the separate components for women in STEM as there won't be any variation in the additional instrument.

4.6 Quality control & quality assurance

We compare the composition of the total contacted sample with the final sample of those agreeing to be involved in the study across faculty and equity group. This allows us to comment on whether these two different samples are different from each other and the implications for the external validity of our results. We expect that the offer of the bridging courses to be more relevant for certain faculties such as science or engineering and IT. As such, certain equity groups may be more likely to enrolled in these faculties, either by preference or by construction (for example, women in STEM). We perform conventional balance checks to ensure that our block randomisation has been conducted successfully. Our main goal is to test that approximately half of our final sample has been allocated to each group and the characteristics of each group is similar. We test statistically for differences between the two groups through the conventional t-test. Our blocks will be faculty and equity group. Other characteristics we will compare will be gender, SES status, basis of admission, ATAR, and number of subjects enrolled in. We also compare how different the entire treatment group is from the proportion that participate in the FYSP (classified by attending at least one activity). We compare equity groups, faculties, gender, basis of admission, ATAR, and number of subjects enrolled in. This helps inform discussions about the external validity of the results and, in particular, how representative of the broader equity student population are the FYSP participants.

We perform the following checks to ensure the robustness of our results. Firstly, we will formally estimate the first stage regressions which correspond to equations (2), (4), (6), and (7) and report the F–statistic. This is the best indicator of the strength or relevance of the instrument and allows us to comment on the consequences for our 2SLS estimates. We report the first stage F–statistic for all sub-groups we consider in the corresponding heterogeneity analyses and conclude whether the sub-group estimation will be reliable (with F–statistics of 10 or higher being our indicator of sufficient instrument strength). Students will be allowed to withdraw from the study at any point prior to publication. As such, we will comment on and report any non-random attrition. We will use baseline characteristics such as faculty and equity group to assess the impact of any non-random attrition. All standard errors reported will be robust standard errors which have been adjusted for heteroskedasticity. For analysis at the student-subject level, we will cluster standard errors by student. We will measure standardised grades using both unweighted and weighted measures. The unweighted measure treats all subjects as they were of equal importance. For weighted measure, we will first calculate the standardised scores using the mean and standard deviation of all students. Then, we will apply subject-level weights to the standardised scores which will weight each score according to how many credit-points the subject is worth out of the total amount of credit points taken in that semester. The weighted success rate will also be used in addition to the usual success rate. This will be calculated by adding up all the credit points for subjects passed and dividing this by the sum of all credit points taken that semester.

5 Publication

5.1 Plans for publication and dissemination of trial results (including any limitations)

A final report will be prepared and published for ACSES. One or more academic articles will be produced for submission to relevant journals.

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7 Appendices

Equation 1: Minimum Detectable Effect

$$MDE = (t_{power} + t_{significance})\sqrt{\frac{1}{P(1-P)}}\sqrt{\frac{\sigma}{C \times N}}\frac{1}{T}$$

Table 1: Parameters for Power Calculation

Parameter	Value	Notes
Statistical power	0.8	Convention
Statistical significance	0.05	Convention
T-statistic for power (t_{power})	0.845	Test statistic associated with that probability
T-statistic for significance ($t_{significance}$)	1.984	Test statistic associated with that probability
Proportion allocated to treatment (P)	0.5	Proportion that maximises power
Outcome mean: standardised grades	0	By definition
Outcome mean: attrition rate	0.2	Conservative forecast based off existing data
Outcome variance: standardised grades (σ)	1	By definition
Outcome variance: attrition rate (σ)	0.16	Y*(1-Y) where Y is the mean of the binary variable
Observations (N)	3,500	Based off previous year

Table 2: MDE for the attrition rate measured in percentage points

		Take-up rate (T)				
		10%	30%	50%	70%	100%
Consent rate (C)	20%	85.54	28.51	17.11	12.22	8.55
	50%	54.10	18.03	10.82	7.73	5.41
	70%	45.72	15.24	9.14	6.53	4.57
	90%	40.32	13.44	8.06	5.76	4.03
	100%	38.26	12.75	7.65	5.47	3.83

Table 3: MDE for standardised grades measured in standard deviations

		Take-up rate (T)				
		10%	30%	50%	70%	100%
Consent rate (C)	20%	2.139	0.713	0.428	0.306	0.214
	50%	1.353	0.451	0.271	0.193	0.135
	70%	1.143	0.381	0.229	0.163	0.114
	90%	1.008	0.336	0.202	0.144	0.101
	100%	0.956	0.319	0.191	0.137	0.096