

Evaluation Summary

Age range	Year 10, pupils aged 14 -15
Number of pupils	Approx. 780 pupils ~26 per school
Number of schools	30 schools
Design	Randomised controlled trial, individual level randomisation
Primary Outcome	GCSE Attainment, assessed using Attainment 8 score.

Background

Intervention

This intervention aims to develop participants' sense of self-efficacy, social confidence¹ and teamwork skills by supporting year 10 pupils to design and deliver a fundraising campaign for a charity chosen by the pupils. It combines project work in small teams with mentoring by local business leaders. The groups participate in a combination of in-school sessions, business-hosted sessions and cross-school events. The results are judged as part of a cross-school competition, in which all teams are assessed and one team is chosen as a winner based on a final presentation.

The intervention will be delivered by Envision, an organisation founded in 2000 dedicated to enabling young people to 'become more aware of social and environmental problems and empowered with the self-belief and skills they need to build a better world'.

The model that will be tested takes place over a 12 week period and includes several elements:

- a 6 hour cross-school event allowing pupils to be exposed to a variety of local charities who pitch for their support - pupils then choose which charity they'd like to fundraise for.
- four 2 hour business mentor sessions, in which teams attend workshops with trained volunteers from local companies. Session material is provided and sessions are generally delivered at the business premises
- eleven 1 hour sessions in school, in which the pupils are supported by Envision coaches to develop their ideas for fundraising and a fundraising plan; and reflect on their progress
- a final 3 hour 'Apprentice-style' cross school event and boardroom challenge in which pupils present their results against other schools in their area

¹ 'Social confidence' is defined by Envision as a young person's confidence in communicating with new people (especially unfamiliar adults) in one-to-one, group and presentation settings.

Some variation in the positioning of delivery is likely in practice, given schools cannot all deliver the programme at the same time of day. School-based sessions will take place either during lunch break, directly after school, or in curriculum time. Pupils will need to volunteer to participate, though each school will play a role in encouraging participation from target groups.

Significance

‘Youth social action’ can broadly be defined as activities in which young people are supported to take ‘practical action in the service of others’². Social action ranges from informal volunteering through to structured programmes of learning, but share the ultimate aim of instigating positive change within a community. High-quality social action is believed to generate a dual benefit: society is positively impacted as community or social problems such as educational under-achievement, youth unemployment and social segregation are addressed. And at the same time, participating individuals experience enhanced personal well-being and develop useful new skills. Evidence for this dual benefit is steadily accumulating, with current research on social action emphasising the secondary benefit accrued by participating young people³. Programmes of youth social action are increasingly seen as a form of careers education that may help support young people’s future employability⁴.

A longitudinal study conducted by the US Corporation for National and Community Service reported volunteers in programmes like AmeriCorps as being 27 per cent more likely to find employment than those who had not volunteered⁵. The initial emphasis on employability came in response to continued feedback from employers and organisations that schools do not sufficiently cultivate the soft-skills necessary to succeed in today’s competitive labour market⁶. Landmark research conducted by Nobel Laureate James Heckman and Tim Kautz indicates that when it comes to labour market success, attributes such as conscientiousness, which is linked to self-control and perseverance, and agreeableness, which is linked to characteristics like empathy, modesty and trust are as important as educational attainment⁷. There is growing empirical evidence to suggest that participation in youth social action programmes can help young people develop these non-cognitive skills and positive character traits. Participation in youth social action may, for instance, spur employability by increasing self-efficacy. Evaluations of social development and community action initiatives such as the National Citizens Service (NCS) indicate positive impacts of involvement on participants’ self-belief that they have the skills and experience to attain employment in the future⁸. Increases in participants’ confidence in their ability to get things done on time and to try new activities were also reported.

² Pye, J. & Michelmores, O. (2017). *National Youth Social Action Survey 2016*. London: Ipsos MORI.

³ Birdwell, J., Scott, R. & Reynolds, L. (2015). *Service Nation 2020*. London: Demos.

⁴ Birdwell, J., Scott, R. & Reynolds, L. (2015). *Service Nation 2020*. London: Demos.

⁵ Spera, C., Ghertner, R., Nerino, A., & DiTommaso, A. (2013). *Volunteering as a Pathway to Employment: Does Volunteering Increase Odds of Finding a Job for the Out of Work?* Washington, DC: Corporation for National and Community Service, Office of Research and Evaluation.

⁶ Birdwell, J., Scott, R. & Reynolds, L. (2015). *Service Nation 2020*. London: Demos.

⁷ Heckman, J. J. & Kautz T. (2014). Fostering and measuring skills: Interventions that improve character and cognition. In Heckman, J. J., Humphries, J. E., & Kautz T. (Eds.), *The Myth of Achievement Tests: The GED and the Role of Character in American Life* (pp. 341–430). Chicago, IL: University of Chicago Press.

⁸ Cameron, D., Stannard, J., Leckey, C., Hale, C. & Di Antonio, E. (2017). *National Citizen Service 2015 Evaluation: Main report*. London: Department for Culture, Media & Sport.

Similar research conducted by the Behavioural Insights Team (BIT), including the evaluation of a similar *Envision* programme, found significant increases in a range of outcomes including attitudes to education and grit⁹. These non-cognitive skills are thought to underpin achievement in both school and work environments¹⁰.

There is some, albeit limited, evidence to support the notion that non-cognitive skills promoted by participation in youth social action programmes may also influence academic outcomes. One mechanism through which youth social action could help to improve attainment is by enhancing young people's attitudes towards school, and by motivating students to apply themselves to their studies to achieve better grades¹¹. Findings from The Effective Provision of Pre-School Education (EPPE) Project, demonstrate that self-reported enjoyment of school at age 11 is associated with higher attainment in maths at Key Stage 3 assessments¹². Within the US, schools that have implemented social and emotional learning programmes have seen a gain of 11-percentile-points in the average student's academic performance compared to the average control student¹³.

Another way in which youth social action may help to improve educational outcomes is by enhancing young people's self-confidence and well-being. Evidence from the 2013 Demos report *Service Nation* demonstrated that 80 per cent of social action programme participants reported increases in their sense of self-confidence¹⁴. Gutman and Schoon's review on the impact of non-cognitive skills on youth outcomes indicates that adaptive coping emotions such as confidence have been positively associated with achievement in academic settings. A comprehensive report by the Department for Education analysed the relationship between different dimensions of well-being at ages 7 to 13 and educational outcomes (using national exam scores)¹⁵. Higher levels of emotional, behavioural, social, and school well-being were found to be correlated with higher levels of academic achievement and school engagement at both ages 7 to 13 and ages 11 to 16. Interestingly, the analyses further indicated that over time, these dimensions of wellbeing become more influential in explaining school engagement than demographic variables. In terms of older cohorts, a recent evaluation of the NCS initiative reported that programme involvement increased self-reported intention to study for a degree or other higher education qualification among those aged 15-17¹⁶.

Despite efforts to draw parallels, the causal relationship between non-cognitive skills and educational outcomes remains unclear. To date, much of the research has been unsuccessful in rigorously

⁹ Kirkman, E., Sanders, M., Emanuel, N. & Larkin, C. (2016). *Evaluating Youth Social Action: Does Participating in Social Action Boost the Skills Young People Need to Succeed in Adult Life?* London: The Behavioural Insights Team.

¹⁰ Gutman, L. M. & Schoon, I. (2013). *The impact of non-cognitive skills on outcomes for young people*. London: Education Endowment Foundation.

¹¹ Birdwell, J., Scott, R. & Reynolds, L. (2015). *Service Nation 2020*. London: Demos.

¹² Sylva, K., Melhuish, E.C., Sammons, P., Siraj-Blatchford, I. & Taggart, B. (2012). *Effective Pre-school, Primary and Secondary Education 3-14 Project (EPPSE 3-14) - Final Report from the Key Stage 3 Phase: Influences on Students' Development from age 11-14*. London: Department for Education.

¹³ Durlak, J. A., Weissberg, R. P., Dymnicki, A. B., Taylor, R. D. & Schellinger, K. B. (2011). The impact of enhancing students' social and emotional learning: A meta-analysis of school-based universal interventions. *Child Development*, 82(1): 405–432.

¹⁴ Birdwell, J., Scott, R. & Reynolds, L. (2015). *Service Nation 2020*. London: Demos.

¹⁵ Gutman L.M., & Vorhaus J. (2012). *The Impact of Pupil Behaviour and Wellbeing on Educational Outcomes*. London: Department of Education.

¹⁶ Cameron, D., Stannard, J., Leckey, C., Hale, C. & Di Antonio, E. (2017). *National Citizen Service 2015 Evaluation: Main report*. London: Department for Culture, Media & Sport.

demonstrating programme impacts due to a lack of comparison groups or counterfactuals and a failure to randomly allocate participants to groups making it impossible to account for exogenous factors¹⁷. Even in studies that employ experimental designs with appropriate controls, evidence remains ambiguous due to mixed results, missing data and high rates of attrition. An example of this can be seen in a large-scale evaluation of Social and Character Development programmes implemented in 20 primary schools across the United States¹⁸. The programmes were designed to foster positive student behaviours and increase educational attainment. Evidence for their efficacy was mixed, however; on average, the seven programmes failed to improve students' social and emotional competence, behaviour, academic achievement, or student and teacher perceptions of school climate. With respect to academic achievement specifically, data relating to grades and standardized test scores were not uniformly available and data quality varied to such an extent that academic achievement could not be robustly assessed. The results relating to learning engagement were mixed.

More recently, another large-scale RCT study, this time conducted by the EEF, attempted to shed some light on the issue¹⁹. The trial investigated whether participation in school-based uniformed youth organisations such as the Scouts or the Fire Cadets would promote academic performance. Maths and English scores at Key Stage 2 (KS2) acted as the pre-intervention attainment metric and were intended to be compared to Key Stage 3 scores. Data access issues rendered internal school assessment records at the end of year 9 the only available proxy for Key Stage 3 scores. Overall, the results of the trial were inconclusive. The findings indicated no significant differences in students' academic performance between KS2 exams and class assessments at the end of year 9. The authors, however, cautioned that the fact that internal school records only weakly correlate with standardised test scores undermines the reliability and validity of this finding. Given the current paucity of evidence, this evaluation will seek to determine whether improvements in academic outcomes can be achieved, alongside three critical skills for employment: self-efficacy, social confidence and teamwork through participation in youth social action programmes.

The project is being funded as part of a partnership between EEF, the Careers and Enterprise Company and Bank of America Merrill Lynch.

Research Plan

Research questions

The primary objective of this evaluation is to test whether Envision's Community Apprenticeship Programme delivered during year 10, improves pupils' overall GCSE performance at the end of year

¹⁷ Gorard, S., Huat See, B., Siddiqui, N., Smith, E. & White, P. (2016). *Youth Social Action Trials: Youth United Evaluation report and executive summary*. London: Education Endowment Foundation.

¹⁸ Ruby, A., & Doolittle, E. (2010). *Efficacy of schoolwide programs to promote social and character development and reduce problem behavior in elementary school children. Report from the Social and Character Development Research Program*. Washington, DC: Social and Character Development Research Consortium.

¹⁹ Gorard, S., Huat See, B., Siddiqui, N., Smith, E. & White, P. (2016). *Youth Social Action Trials: Youth United Evaluation report and executive summary*. London: Education Endowment Foundation.

11, as measured by the capped average point scores across eight best GCSEs (Attainment 8).

The evaluation will also address the following questions:

- What is the impact of the programme on both Maths and English attainment at the end of year 11, as assessed by GCSE point score in each subject? (English scores to be averaged across English Language and English Literature).
- What is the impact of the programme on pupils' self-efficacy, as measured by the 'New General Self Efficacy Scale' (NGSE) at the end of the programme in year 10?
- What is the impact of the programme on pupils' social confidence, as measured by the 'Self-Perceived Communication Competence Scale' (SPCC) at the end of the programme in year 10?
- What is the impact of the programme on team behaviour, as measured by the 'Teamwork Scale for Youth' (TSY) at the end of the programme in year 10?
- What is the impact of the programme on GCSE performance for pupils eligible for Free School Meals (Ever 6 FSM), as measured by Attainment 8 and GCSE point scores in both Maths and English? (English scores to be averaged across English Language and English Literature).
- Does the programme impact on pupils' progression into further social action activity, as measured through participation in the National Citizen Service (by November 2019, subject to data availability)?

BIT will also seek to obtain data to determine the impact of the programme on three other outcomes. These are pupils' likelihood of progression to higher education, the impact on their post 16 attainment outcomes, and their employment status including occupation and income. Investigation of these additional longer- term outcomes is considered exploratory and further details would be specified as part of future protocol amendments by November 2019 once further information is available on potential data linkage.

Design

This will be a two-arm randomised controlled trial, with randomisation at the pupil level, stratified by school and FSM Status. The two arms are:

- 1) The treatment arm, in which pupils are assigned a place on the Envision's Community Apprentice programme
- 2) The control arm, in which pupils continue with routine school activity

Three main constraints, related to the capacity of the delivery organisation and the timing of the trial, are placed on the design of the trial. First, Envision can deliver the intervention in a maximum of 30 schools, with a maximum of 13 pupils participating per school. Second, whilst they are able to deliver the intervention to two consecutive groups per school (one starting in September and one in January), they are only able to include the second cohort of pupils in the trial. This is because there is not enough time between recruiting the schools and starting the September cohort to administer the consent and

randomisation processes. Third, as this is the first time that Envision will be delivering this intervention with this age group, the timing of the sessions in the school day and the participant selection process was left largely up to each school to decide. Envision does not believe that it is possible to recruit the number of schools and pupils necessary for the trial without giving partner schools these two degrees of freedom.

The trial design is set out in the diagram below.

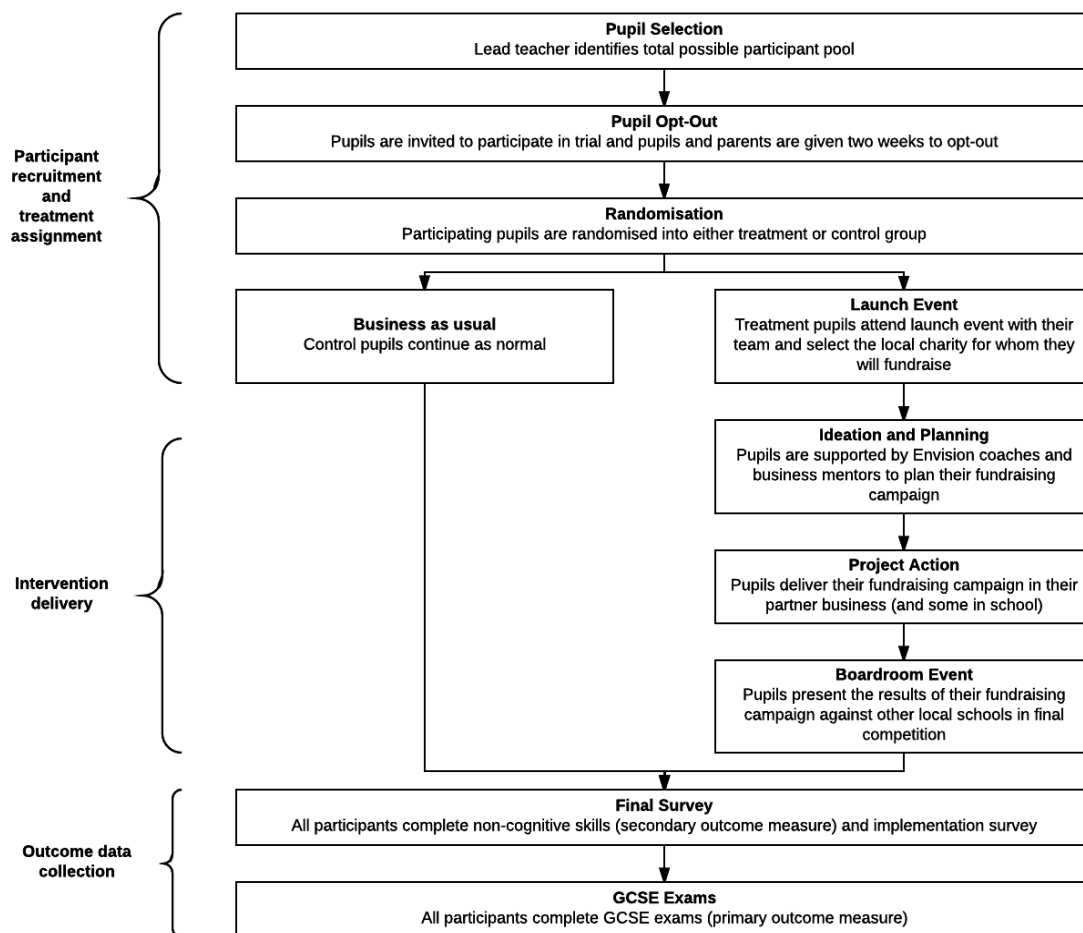


Figure 1: Trial design

The trial will be complemented by an implementation and process evaluation (IPE) that will focus on the following domains and serve three purposes:

- **Mechanisms:** To contribute to our understanding of mediating mechanisms
- **Fidelity:** To assess the extent to which the intervention is delivered as intended
- **Context:** To assess how participant characteristics and the implementation context relate to the effectiveness of the treatment

Randomisation

Schools will select students for participation in September and October. Following pupils' initial agreement to take part, there will be a two week opt-out period for pupils and parents/carers. Schools will provide lists to BIT of the selected students who have not opted out of the evaluation in the first week of December.

The total number of pupils who are able to take part in the intervention in each school is limited to 13, as this reflects Envision's capacity to deliver the programme. Pupils will be randomised within schools -- 13 will be randomised into the treatment group, and the remaining 13 assigned to the control group. Where the number of pupils selected exceeds 26, BIT will randomly select 26 pupils to be included in the evaluation, and randomisation will be conducted as described above. Where the number of pupils selected is less than 26, 13 will be randomised into the treatment group and the remaining number will be assigned to the control group. Randomisation will be stratified by the Ever 6 FSM variable that refers to a pupil's Free School Meals status as this covariate is likely to be correlated with our primary outcome variables, given it is associated with lower attainment on average. The procedure will be conducted using statistical software, Stata Version 14. The Stata code used to carry out the randomisation will be reviewed and quality assured by a researcher who is not part of the trial and will be recorded and reported in the final report.

Participants

The trial will be conducted with year 10 pupils in state secondary schools in Birmingham, Bristol and London (the three operating locations of Envision). Schools that meet the following criteria are eligible to participate in the trial:

- **Their location:** Schools must be based in Birmingham, Bristol or London.
- **The feasibility of delivering the intervention:** Teaching and management staff need the capacity to support the administration of the project and the flexibility in their timetable to implement the Community Apprentice programme.
- **Pupil disadvantage:** Schools must agree that it is feasible to nominate 30% of pupils to the trial who are eligible for free school meals (Ever 6 FSM). Data containing free school meal status for each pupil will be collected prior to randomisation (details below).
- **Consent Procedures:** Schools must agree with the opt-out consent procedure that we plan to use with pupils and parents.

Envision is aiming to recruit 30 schools that meet these four eligibility criteria.

Each school will then be responsible for selecting 26 pupils (13 for each trial arm) who meet the following two eligibility criteria:

- 1) **Year group:** Pupils must be in Year 10.
- 2) **Pupil disadvantage:** Schools must aim to select pupils such that 30% of the sample are eligible for free school meals (Ever 6 FSM), but the school will not be excluded from the total sample if it falls short of this percentage.

As mentioned in the discussion of delivery constraints above (pp. 5-6), Envision do not believe that it is possible to place any further constraints on the pupil selection process due to the timings of the trial setup and the newness of the intervention.

This gives a total sample size of 780 pupils. All pupils will follow the same consent procedures. Pupils in each school who are allocated to the control group will continue with business as usual activity for the duration of the trial.

Schools wishing to participate in the trial will be asked to sign a Memorandum of Understanding (MOU) (see Appendix 4) before enrolment, agreeing to the required activities for both the intervention and evaluation. This includes the requirement for each school to provide a named 'School Project Co-ordinator' to ensure MOU commitments are met.

Opt-out consent will be sought from pupils and from the parents (or legal guardians) of all eligible pupils. Consenting participants and their parents (or legal guardians) will be agreeing to pupil participation in the study, randomisation into treatment or control, and the sharing of the Unique Pupil Number (UPN) and other personal pupil data with BIT (see Appendix 1 and 2 for full details). After removing those who have opted out, schools will share pupil level data with BIT for the purpose of randomisation. All participating schools must be willing to supply the necessary pupil data, which are:

- UPN
- First name
- Last name
- Date of birth
- Postcode
- Gender
- FSM indicator as indicated by the NPD variable *EVERFSM_6_P*

Pupils will be informed of their trial arm allocation by December 2017.

Sample size calculations

Minimum detectable effect sizes (MDESs) are calculated below based upon the following assumptions:

- **Sample size:** Envision is aiming to recruit 30 schools to trial with 26 participants in each school. This gives a total sample size of 780 pupils. However, school recruitment is not yet complete, and we expect some attrition during the trial so we have presented MDESs for a range of scenarios:
 - **1:** 780 pupils (30 schools, 26 pupils per school, no attrition)
 - **2:** 600 pupils (30 schools, 26 pupils recruited, of which 20 complete)
 - **3:** 594 pupils (27 schools, 26 pupils per school, of which 22 complete)

- o **4:** 540 pupils (27 schools, 26 pupils recruited, of which 20 complete)
- **Randomisation:** Performed at the pupil level, within schools.
- **Trial arms:** Two trial arms (a treatment and control) of approximately equal size.
- **Hypotheses:**
 - o **Null hypothesis:** There will be no difference in GCSE Attainment 8 score between pupils who take part in the programme and pupils who do not.
 - o **Alternative hypothesis:** There will be a difference in GCSE Attainment 8 score between pupils who take part in the programme and pupils who do not (i.e. a two-sided alternative hypothesis).
- **Adjusting for baseline covariates:** We are assuming that 50%²⁰ of the variance in the model is explained by baseline scores. In order to maximise explanatory power, we plan to use both Key Stage 2 (KS2) Maths and English scores.
- **Power: 80%; Significance level: 5%.** These are standard assumptions.
- For the purpose of this power calculation, we err on the side of conservatism by calculating as though the treatment effect is zero on those pupils who do not complete the treatment. The results are presented below.

Scenario	# Pupils recruited per school	# Schools	Total # pupils recruited	Effect (Cohen's d)	Baseline factor	MDES (Cohen's d ²¹)	Proportion of pupils completing	Minimum ITT effect
1	26	30	780	0.2	0.71	0.142	1.00	0.142
2	26	30	780	0.2	0.71	0.142	0.77	0.185
3	26	27	702	0.21	0.71	0.150	0.85	0.176
4	26	27	702	0.21	0.71	0.150	0.77	0.194

Table 1: Summary of power calculation results for whole sample

Envision is currently working towards delivering Scenario 2 (although they currently only have 27 schools recruited). Under these assumptions, the trial has a MDES of 0.185 standard deviations. A meta-analysis of effect sizes across 113 randomised controlled trials in education puts the mean effect size at 0.17, with a median of 0.1. Our estimates for the current trial suggest that we are therefore only powered to detect an effect on academic attainment (our primary outcome) that is above the mean for interventions in this field.²² It is therefore advisable for Envision to aim to retain more than 10 pupils in each trial arm per school (especially if they are unable to recruit any more schools).

²⁰ Anders, J. (2016). Author's own analysis of the Department for Education Longitudinal Study of Young People in England.

²¹ This Cohen's D refers to the SD of the original outcome.

²² Sanders, M and Ni Chonaire, A. (2015). Powered to detect small effect sizes: You keep saying that. I do not think it means what you think it means. *Working Paper No. 15/337*, Centre for Market and Public Organisation.

Ever 6 FSM MDES calculations

We will also estimate effects for the Ever 6 FSM subgroup denoted by the NPD variable *EVERFSM_6_P* in our analysis. Assuming 30% of pupils in our sample are Ever 6 FSM eligible (the target that schools have been set for pupil selection), the table below shows the MDES for this subgroup in our four scenarios. We are assuming that the drop-out rate for these pupils is the same as for the wider cohort. The R code used for these calculations can be found at Appendix 6.

- **Hypotheses:**

- **Null hypothesis:** There will be no difference in GCSE Attainment 8 score between Ever 6 FSM pupils who take part in the programme and Ever 6 FSM pupils who do not.
- **Alternative hypothesis:** There will be a difference in GCSE Attainment 8 score between Ever 6 FSM pupils who take part in the programme and Ever 6 FSM pupils who do not (i.e. a two-sided alternative hypothesis).

All other assumptions are as above. The results are presented below.

Scenario	# Ever 6 FSM pupils	Effect (Cohen's d)	Baseline factor	MDES (Cohen's d)	Proportion of pupils completing	Effective effect
1	234	0.37	0.71	0.263	1.00	0.263
2	234	0.37	0.71	0.263	0.77	0.342
3	211	0.39	0.71	0.277	0.85	0.327
4	211	0.39	0.71	0.277	0.77	0.360

Table 2: Summary of power calculation results for Ever 6 FSM subgroup

Example code

The following code was run in R (amending the assumptions for each scenario) for the power calculations.

```
##### Power calculation assumptions - Scenario 1:
```

```
#Total sample = 780, so n = 390 (equal arms)
```

```
#Significance level = 0.05
```

```
#Power = 0.8
```

```
#SD = 1 (result of power calc will be in Cohen's d)
```

```
power.t.test(n = 390, sig.level = 0.05, power = 0.8, delta = NULL, sd = 1, alternative = 'two.sided',  
type = 'two.sample', strict = TRUE)
```

Outcome measures

The primary outcome is overall GCSE performance, as per Attainment 8 score obtained via the NPD. This will be assessed at the end of year 11 (summer 2019). Attainment 8 measures pupils' average grade across eight subjects and is a freely available, highly significant academic qualification.

The evaluation will also consider five secondary outcomes²³:

- **English attainment**, as assessed by GCSE assessments taking place at the end of students' year 11 (summer 2019) with both English GCSEs point scores being used to create a final English score.
- **Maths attainment**, as assessed using GCSE Maths point score from summer 2019 GCSE assessment.
- **Self-efficacy**, as assessed by the "New General Self-Efficacy scale" (NGSE).²⁴ This is a seven item self-report survey that contains items relevant to pupils' sense of self-efficacy. Participants are asked to respond on a scale of 1 (strongly disagree) to 5 (strongly agree). There is no specific training required by raters, beyond the instructions. The BIT team will be responsible for survey administration and data entry. Individual pupil mean scores will be used during analysis. This scale is a revised version of the "General Self Efficacy Scale", with higher construct validity and reliability than its predecessor.²⁵ It has also been found to predict self-efficacy in a number of different contexts.
- **Teamwork**, as assessed by the "Teamwork Scale for Youth" (TSY).²⁶ This is an 8 item self-report survey, in which participants mark on a Likert scale from 1-5 the accuracy of a series of statements covering examples of positive team behaviour. The scale demonstrates good evidence of validity, strong reliability in terms of internal consistency, and strong factorial invariance.²⁷ The BIT team will be responsible for survey administration and data entry. Individual pupil mean scores will be used during analysis.
- **Social confidence**, as assessed by the "Self-Perceived Communication Competence Scale" (SPCCS).²⁸ This is a 12 item self-report survey that measures self-perceived communication in a range of contexts. Participants are asked to rate their competence in 12 situations between 1 and 100. This scale has generated good alpha reliability estimates and performs well in predictive validity tests against similar scales.²⁹ It will be administered by BIT alongside the Teamwork and Self-efficacy surveys. We also aim to measure part of this construct through

²³ See Appendix 5 for the full non-cognitive skills survey.

²⁴ Chen, G., Gully, S. M., & Eden, D. (2001). Validation of a new general self-efficacy scale. *Organizational research methods*, 4(1), 62-83.

²⁵ Chen, G., Gully, S. M., & Eden, D. (2001). Validation of a new general self-efficacy scale. *Organizational research methods*, 4(1), 62-83.

²⁶ Lower, L. M., Newman, T. J., & Anderson-Butcher, D. (2015). Validity and reliability of the teamwork scale for youth. *Research on Social Work Practice*.

²⁷ Lower, L. M., Newman, T. J., & Anderson-Butcher, D. (2015). Validity and reliability of the teamwork scale for youth. *Research on Social Work Practice*.

²⁸ McCroskey, J. C., & McCroskey, L. L. (1988). Self-report as an approach to measuring communication competence. *Communication Research Reports*, 5:2, 108-113.

²⁹ McCroskey, J. C., & McCroskey, L. L. (1988). Self-report as an approach to measuring communication competence. *Communication Research Reports*, 5:2, 108-113.

the GCSE English Speaking and Listening exam (if this data is available for all participants). Individual pupil mean scores will be used during analysis.

Self-efficacy, teamwork and social confidence assessments will be administered in a single survey shortly after pupils have completed the final programme activity (May 2018) by BIT Research Assistants (RAs). The RAs will be recruited by BIT and provided with detailed instructions to ensure assessments are delivered consistently. Only those eligible for the intervention and who have agreed to take part will have their data collected for secondary outcomes (in both treatment and control arms). This will be during the same period allocated to the Community Apprentice sessions, either during a curriculum time slot or at a lunch break, depending on the school. This self-report assessment should take no more than 15 minutes and will be completed on paper.

Four longitudinal outcomes will be examined where it is possible to access the relevant data. These are:

- Post-16 attainment (Key Stage 5) sourced through the NPD;
- progression to higher education, sourced via the Longitudinal Education Outcomes (LEO) dataset;
- employment status, sourced through the LEO dataset; and
- progression into further social action, sourced via the NCS.

We will only be able to report on social action outcomes in the final report, as we are expected to deliver the draft final report in January 2020. At that time point, pupils will still be in their first year of Key Stage 5 and will not have progressed to higher education or employment.

An additional measure is that of dosage, which will be used for defining participant compliance and in the Complier Average Causal Effect (CACE) analysis. Compliance will be based on whether or not pupils attend the minimum number of sessions required, and the correct combination of activities as defined by Envision (see p. 19).

Analysis plan

Summary

Our primary analysis will focus on GCSE Attainment 8 Score. This outcome variable will be regressed using a least squares linear model. This approach is detailed below. This will be assessed at the end of year 11 (Summer 2019), with GCSEs to be administered by the school as per routine school procedure for these exams.

Analysis will be conducted on an intention-to-treat basis (ITT), including all children randomised into the treatment and control group. Analyses will be conducted in Stata version 14, using 2-sided significance tests, at the 5% significance level.

By recording attendance data throughout the trial, we are also able to estimate the treatment effects in the presence of minimal compliance to the programme.

We note that participants assigned to the control group may experience something similar to the intervention, in the form of another youth social action programme (or similar volunteering opportunity). Our aim is to gather participation information from participants in both treatment and control group to capture the additivity of this programme.

Baseline characteristics

Baseline characteristics observed in the NPD (gender, age, Key Stage 2 Maths and English scores, Ever 6 FSM and attendance) will be summarised by treatment arm.

Trial completion

A CONSORT diagram will be used to present the summary of the flow of eligible children and their schools from recruitment through randomisation, post intervention assessment and analysis. The number of children and schools included or excluded at each stage will be clearly stated and the reasons for exclusion will also be stated.

Primary analysis

Analysis will be carried out using an OLS regression,

$$Y_i = \beta_0 + \beta_1 \cdot T_i + X_i \cdot \alpha + \epsilon_i$$

where:

- Y_i is the outcome for the overall GCSE performance, measured by capped average point scores across eight best GCSEs (Attainment 8)
- T_i is a binary indicator for the treatment (1 if the student is treated and 0 if not),
- X_i is a vector of individual-level stratification variables (school and FSM status) and the baseline attainment measured through separate KS2 raw English and Maths scores, and
- ϵ_i is the individual level error term. The analysis will use Huber-White robust standard errors.

While point scores are bounded, we assume that the response to the treatment will be locally linear so an OLS will be appropriate (in any case OLS gives the best linear approximation).

Analysis will be ITT, in which we test the hypothesis that being assigned a place on the programme has a beneficial impact on attainment.

Secondary analysis

For the secondary analysis, we will replace Y_i in our specification of the primary analyses with variously:

- GCSE Maths point score (using only KS2 Maths score as a covariate),

- GCSE English point score (using only KS2 English score as a covariate),
- Self-efficacy score,
- Teamwork score,
- Self-perceived communication competence score,
- Longitudinal outcomes.

For the self-efficacy and teamwork scores, we have an ordinal scale with a small number of responses. As such, we will use an ordinal logistic regression with the same covariate set as a robustness check.

There is no pre-test for any of the non-academic outcomes so the analysis will be on post-test only in these cases.

We will also conduct the analysis for the subgroup of pupils who are registered for free school meals (Ever 6 FSM) in the NPD (using the *EVERFSM_6_P* variable), using the same model as our primary analysis, with the addition of an interaction between treatment assignment and FSM status, to assess whether there is a significant difference in the treatment effect between FSM students and others. We will also compare FSM and non-FSM students within the treatment group to see if there is a significant difference in treatment effect.

Treatment effects in the presence of non-compliance

We will undertake two types of exploratory analysis.

First, we will use an instrumental variables approach for a CACE analysis, which will provide the average treatment effect for compliers. In the context of the trial, the compliers are those students in the treatment group who have attended a minimum number of sessions and activities (as defined on p. 19). In order to estimate the treatment effect for compliers, we will employ an instrumental variables approach. This approach relies on having a variable which is associated with whether someone receives the treatment but not with the outcome variable of interest. This is known as the instrument. In the context of the trial, the instrument is treatment assignment, which is assumed to influence whether you participate in the programme but not the outcome variable in its own right.

More specifically, two key assumptions need to hold for this approach:

- 1) Being assigned to the treatment increases participation in the treatment. In this instance, students may only participate in the programme if they are assigned to treatment. We believe we will have sufficient participation among treatment group students for this assumption to hold. There is no ability to participate in the programme outside of the assignment to the treatment group.
- 2) Random assignment does not itself impact outcomes. We have no reason to believe that the offer of the programme would improve attainment on its own, but instead believe the impact on attainment is achieved through participation in the programme.

It is important to note that we do not know the true minimal amount of dosage needed to generate a treatment effect, so the cut-off chosen for minimal compliance is our best estimate, which was defined in coordination with the delivery organisation. If we have chosen the wrong benchmark for minimum

dosage, we will be underestimating the effect of the programme.³⁰ Regardless, this analysis is likely to generate treatment effects that exceed those generated by ITT (unless the treatment is detrimental).

The CACE estimation would proceed using a standard two-stage least squares (2SLS) approach. We estimate:

$$T_i = \gamma_0 + \gamma_1 Z_i + X_i \delta + u_i(1)$$

$$Y_i = \beta_0 + \beta_1 \hat{T}_i + X_i \alpha + \epsilon_i(2)$$

where:

- Z_i is a binary indicator for the treatment assignment (1 if the student is assigned to treatment and 0 if not),
- T_i is whether a student met the minimal compliance threshold,
- X_i are individual covariates, specifically the stratification variables (school and FSM status), and separate Maths and English raw KS2 Scores,
- u_i are Newey-West robust standard errors,
- ϵ_i are Baum–Schaffer–Stillman 2SLS errors,
- \hat{T}_i are the predicted levels of compliance with the programme from (1),
- Y_i is the outcome for the overall GCSE performance, measured by capped average point scores across eight best GCSEs (Attainment 8).

Our second exploratory analysis will include a descriptive dosage analysis, in which we explore the relationship between dosage and our outcome variables. This analysis will not be causal, because dosage will be driven by unobserved factors such as motivation, so that although whether or not someone participates in the programme (at all) is influenced by the randomly assigned treatment variable, the decision within the treatment group to attend 4 classes rather than 6 is not random. The analysis will aim to provide useful evidence on how attendance relates to the programme's intended outcomes and may be useful for developing participant recruitment and retention goals, or as a focus for future, causal, research.

To begin with, we will generate graphs plotting dosage against each of the outcome variables to explore the nature of the relationship. For example, we may find a linear relationship that shows a steady increase in attainment as dosage increases, or we may find a non-linear relationship that shows little impact on attainment at lower dosage levels and a marked increase at higher dosage levels.

If we observe a large increase in our outcome variable between dosage levels, we will test for whether that increase is significant using the Chow test. The Chow test entails splitting the data at the point where we observe the change and running separate regressions on each half of the split data. We will then compare the coefficients of the regressions to determine whether they are significantly different. While these changes in the outcome variable cannot be fully attributed to the increased dosage, we will also explore how other observable characteristics change at various dosage levels. This would provide indicative evidence of areas of future research into the optimal

³⁰ Setting the minimal compliance requirement too high will result in “treated” students in the control group; setting the requirement too low will result in “untreated” students in the treatment group.

dosage for the intervention.

If visual inspection of the graphed relationships between dosage and our outcome variables warrants it, we will attempt to model these relationships formally through the appropriate regression analyses.

Implementation and process evaluation methods

The IPE of the programme aims to understand how the programme is delivered and implemented and what elements may be effective at bringing about change. All questions have been developed and refined in collaboration with Envision and guided by the Logic Model (see below). It does this through focussing on two core areas:

1. To what extent do the programme activities lead to ultimately increasing academic attainment?
2. To what extent do the students engage with the programme to develop students' self-efficacy, social confidence (measured using the SPCCS) and teamwork abilities?

This will be explored through a range of sub-questions:

1. Programme delivery

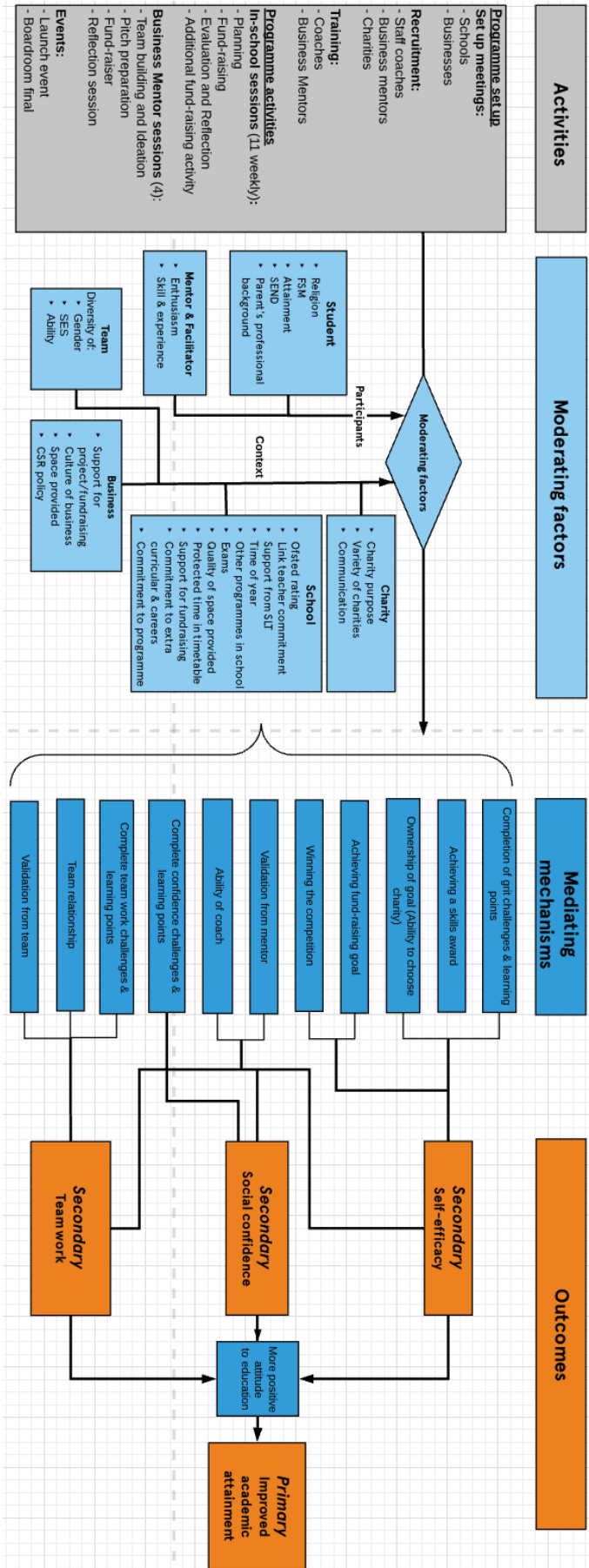
- a. To what extent is the programme delivered as intended (fidelity and dosage)?
 - i. To what extent do the young people get the exposure as planned?
 - ii. To what extent do coaches and business mentors adhere to the intended treatment model?
 - iii. How much - and what kind of - variation is there across schools? (adaption)
- b. How well is the programme delivered (quality)?
 - i. Do the young people receive positive reinforcement and validation from the business mentors? What impact does this have on the three secondary non-cognitive outcomes?
 - ii. Do the coaches' abilities impact delivery?
- c. How distinct is the programme from existing practice (programme differentiation)?
 - i. Do the schools offer any other programmes of a similar nature (e.g. fundraising, careers engagement)?
 - ii. Are the pupils in the control group engaged in similar activities (monitoring of control group)? (These could be social action based opportunities or non-social action based activities that aim to achieve similar outcomes)

2. Programme engagement

- a. To what extent is the programme delivered to its intended recipients (reach)?
 - i. How many of the intervention group students are eligible for free school meals (as defined by the Ever 6 FSM variable) (in line with required 30%)?
 - ii. What motivates students to participate or not?
 - iii. What are the barriers/facilitators to student participation?
- b. To what extent are recipients engaged during the delivery of the programme (responsiveness)?
 - i. To what extent do the participants meaningfully engage with the programme? What impact does completing 'challenges' or 'learning points' have? Do they feel that they have ownership of their goal, and what impact does this have?
 - ii. How does the relationship within the young people's team contribute to engagement?
 - iii. What impact does winning the 'Boardroom final' have?
 - iv. To what extent does the school engage in the programme?

Logic Model

An IDEA workshop was held, utilising the TIDieR framework, to develop a logic model in collaboration with Envision (see following page). The Logic Model will be instrumental in directing the IPE. Throughout the IPE, we will attempt to monitor the proposed mediating mechanisms as well as understand the role played by potential moderators. Data on moderators is outlined in the Logic Model and will be collected from Envision and through interviews for the students in the case studies.



Definitions of compliance

The follow criteria have been outlined by Envision as the minimum level of compliance required for the intervention to impact the outcomes. Envision will provide attendance data to BIT one month into the trial, 3 months into the trial and after the trial ends.

To be considered as minimally compliant with the treatment, a student must have attended at least the following combination of sessions:

- i) 5 weekly sessions
- ii) 1 business mentoring session

The purpose of this measure is to estimate the CACE in the presence of non-compliance, as well as to contextualise the process evaluation and validate the logic model³¹.

Methods

We will answer these process evaluation questions through a combination of methods. We will discuss them chronologically as they arise across the project. Please refer to the methods map, which outlines the data collection methods that will be used to answer the specific questions. A chronological timeline is outlined below this.

Chronology of data collection

January - July 2018

Five case study visits will be carried out across a range of school settings. We will use purposive sampling to target schools representing diversity in location, Ofsted rating, business type and engagement. Location, Ofsted rating and business type information will be obtained using administrative data. Data on engagement will be obtained from Envision and will comprise of:

- Senior Leadership Team (SLT) buy-in at set-up meeting (measured by number of SLT present)
- Compliance data
- Student data: completion of 'mini challenges' and 'learning points'

The case studies will involve observation of a combination of business mentor sessions and weekly sessions. The nature of observations will vary per school, depending on what stage they are at in the programme (i.e., some schools will involve a business mentor session observation session, some will involve a weekly session). This will provide an overview of different contexts and time points of the intervention. In addition to this, we will conduct semi-structured interviews with students, business mentors, contact teachers and members of the SLT. A minimum of five interviews will be conducted at each setting, including at least one of each (student, business mentor, SLT) from every school selected. Documentary analysis of online data and field notes may also be drawn upon to triangulate and contextualise the interview data. Three case study schools will be visited during the programme, and we will conduct two case study visits after the boardroom final; one to a school that wins the

³¹ Humphrey, N., Lendrum, A., Ashworth, E., Frearson, K., Buck, R., & Kerr, K. (2016). *Implementation and process evaluation (IPE) for interventions in education settings: An introductory handbook*. London: Education Endowment Foundation.

competition in their region and one to a school that loses. The purpose of this is to monitor change throughout and to assess the impact of winning or losing the challenge.

The interviews will be used to ascertain the fidelity, adaptation, quality, reach and responsiveness of the programme. All interviews will be digitally recorded with the participants' permission and the recordings transcribed. Data from the observations will be collected on a proforma designed for the evaluation, which will explore factors such as student engagement, relationship with mentor, relationship with coaches, and engagement with tasks and challenges. Individuals will be made aware that their participation is voluntary and of the confidentiality and anonymity of the research.

An endpoint student survey across both the control and treatment group will be conducted alongside the outcome measures at the end of the programme. This will contain questions about what other activities of a similar nature (both social action and non-social action) the students have taken part in throughout the programme. The purpose of this survey is to assess programme differentiation and to monitor the control group.

Qualitative analysis

The data will be analysed using thematic analysis to draw out recurring themes and triangulated across research methods. Additionally, fieldwork notes from observations will be analysed to understand potential relationships to certain positive or negative outcomes.

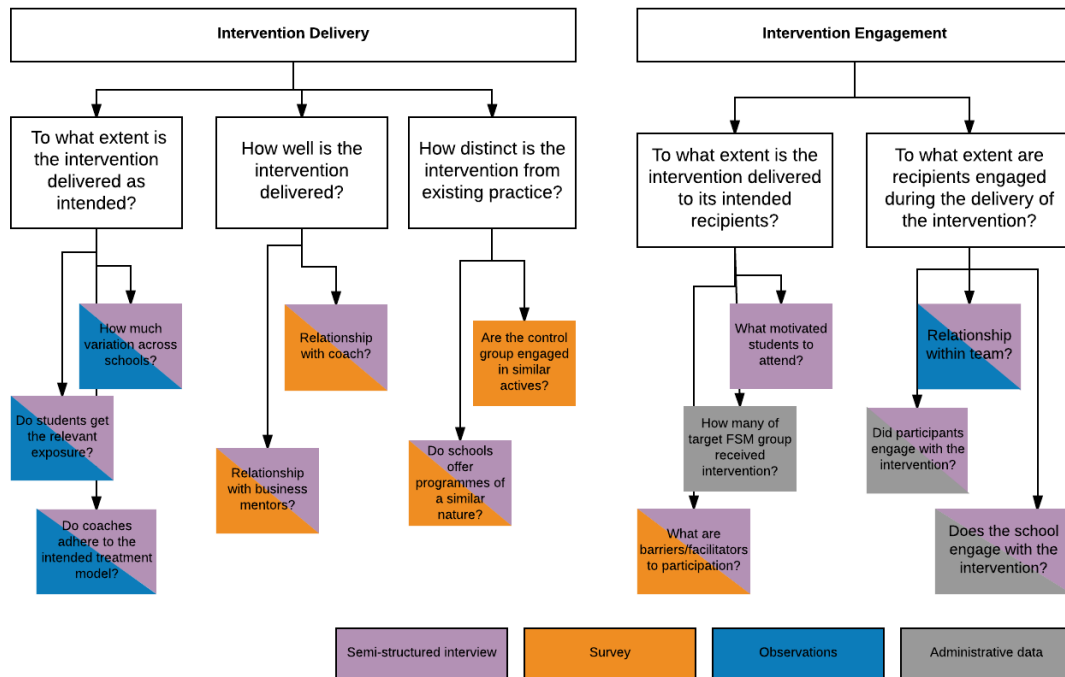
Costs

An estimate of the per-pupil cost of the programme will be calculated by the evaluation team. This estimate will focus on cost from the perspective of a participating school and will be based on the marginal, financial costs of the programme.

The cost estimates will in part make use of information from the project team (particularly with regard to the actual cost of delivering the programme), as well as that collected directly by the evaluation team. Information on costs, especially any hidden costs or resource implications, will be explored through the process evaluation as part of the interviews with teachers and school visits. The purpose of collecting such data in the process evaluation is to identify the main areas of expenditure required by the project. This process will also help to establish whether it may be appropriate to include any questions on costs/resource use in the survey. This will need to strike a balance between collecting sufficient cost information and not reducing response rates; it will also need to take account of whether a teacher is well placed to provide accurate information on particular types of costs.

Time costs will be reported separately from the financial costs – such as the amount of time for which schools need to arrange supply cover for teachers to attend training. Any costs in terms of prerequisites will also be considered. Any costs associated purely with the evaluation will be excluded.

Implementation and Process Evaluation Methods Map



Ethics and registration

Parents and pupils will be provided with an information sheet giving details of the aims of the research. This form will offer parents (or legal guardians) the opportunity to opt out of the trial. It will also offer the opportunity for parents to withhold consent to accessing their child(ren)'s NPD records. We judge opt-out consent is sufficient for NPD data linkage in this case.

Data will be transmitted and stored using the security principles underlined in the institutional Data Security policy (attached in Appendix 3). This includes secure transfer of data and use of password-protection and encryption as appropriate during data storage.

The trial will be registered at www.controlled-trials.com.

Personnel

Project team

- Jennie Butterworth, CEO – Envision
- Hannah Matthews, Quality and Impact Manager – Envision
- Dan Heffernan, Birmingham Manager – Envision
- Ben Harding, Bristol Manager – Envision

- Jenny Welsh, London Manager – Envision

Evaluation team

- Pantelis Solomon (Principal Investigator) – BIT
- Hazel Northcott – BIT
- Kim Bohling – BIT
- Patrick Taylor – BIT
- Jess Heal – BIT
- Doireann O’Brien - BIT

The teams will have the following roles within the evaluation:

Design of the trial

- sample size calculations – BIT
- refinement of randomisation approach – BIT

Delivery of the intervention

- recruitment of schools –Envision
- delivery of intervention – Envision

Measurement and collection of outcomes

- Attainment 8 attainment – BIT
- self-reported outcome surveys; self-efficacy, social confidence and teamwork – BIT RAs
- application and linkage to outcomes – BIT

Impact analysis – BIT

Qualitative analysis – BIT

Risks

The data security policies of BIT and Envision and the Data Sharing Agreement between BIT and Envision are included with this protocol.

Some of the key risks are listed below:

- School drop-out after randomisation reduces the integrity of the experimental design. To reduce the risk of drop-out, it will be important to ensure schools are well-informed about the programme and the trial from the start, so that they are clear as to what is expected of them before they commit to taking part. Schools will be asked to sign a memorandum of understanding as a signal of their commitment. It will also be important to maintain good

communications with schools throughout the project in order to maximise retention. There may also be difficulties in recruiting schools to the trial. Records will be kept of schools approached and where possible, their reasons for not participating, to provide an indication of external validity.

- There is a considerable time gap between signing up for the programme and being notified of the randomisation result, which may lead to student drop-out (students may lose interest, enrol in competing activities, etc). The time delay between sign-up and randomisation is unavoidable, as parents must be given at least two weeks to opt out of the evaluation and there are breaks in advance of the start of each cohort (summer break for cohort 1 and Christmas break for cohort 2). We chose to evaluate cohort 2, as we deemed the shorter Christmas break would lead to fewer drop-outs than the longer summer break before cohort 1.
- If individuals do not consent to participating in the evaluation, this has the potential to reduce the sample size and affect the internal and external validity of the trial. As consent is collected pre-randomisation, it should not affect the internal validity of the trial, as any withholding of consent should be just as prevalent in the treatment and control groups. As only opt-out consent is required, we judge that the risk of a large number of opt-outs is low. As a mitigation strategy, schools have been asked to over-recruit at the initial pupil selection stage.
- If pupils are not present on the day of testing, this may also reduce the sample size by reducing the number of pupils for whom we are able to obtain a post-test; furthermore, it may introduce some bias if it is a non-random group of pupils who are absent. When arranging dates for secondary outcome tests, the question of how times can be chosen to minimise absenteeism will be discussed with the schools. In those where a higher proportion of pupils are absent, mop-up visits will be carried out to attempt to minimise this risk. As academic attainment will be assessed through GCSE exams, the risk of low turnout is much lower for the primary outcome.
- There is a possibility that the delivery of the intervention will vary across schools. However, this reflects the reality of implementing such a programme; impact estimates therefore relate more to type of treatment likely to prevail in practice rather than that which might be observed under ideal conditions. Nevertheless, understanding treatment variation is important and will be explored as part of the process evaluation.
- In regards to accessing outcome data through NPD under the General Data Protection Act (GDPR):
 - When the evaluation launched, we were operating under the Data Protection Act 1998. Under that legislation, using consent as the lawful basis for processing required only opt-out consent.
 - As such, BIT provided schools with opt-out consent forms, which were distributed to parents in November and December.
 - The form included information on the data we were requesting from the school and the NPD and the length of the evaluation. It stated clearly that parents could withdraw from the evaluation at any time by returning the form and that they could contact a member of the evaluation team with questions or concerns.

- Parents were given two weeks to opt-out before we enrolled the student in the evaluation. We did have one parent opt their student out after the two week period, and we have deleted that student's data.
- As you know, the GDPR sets a higher standard for processing under the lawful basis of consent, and requires explicit, opt-in consent. Accordingly, the consents we obtained previously would not, if we implemented under the GDPR, be sufficient to satisfy the new requirements.
- However, the GDPR does not require that consent obtained under the DPA 1998 be 'refreshed' or re-papered in all cases. The ICO guidance on this topic (found at <https://ico.org.uk/for-organisations/guide-to-the-general-data-protection-regulation-gdpr/consent/whats-new/>) indicates that in cases where consents obtained under the DPA 1998 are not sufficient to satisfy the regime under the GDPR, processing can take place under a different lawful basis.
- If we had launched the trial under the new GDPR, given the practical issues with obtaining opt-in consent we would have instead relied on the lawful basis of legitimate interests. As EEF is aware, we have utilised the legitimate interests basis in relation to a number of similar trials funded by EEF that have been implemented under the new GDPR standards. In this case, we would have had schools send home informational sheets providing the same information that was provided in the opt-out consent form with the same opportunity to withdraw their student's data at any time by returning a form or contacting the evaluation team.
- In the present case, given the passage of time and the changeover in school cohorts it would be highly difficult to obtain refreshed opt-in consent. Hence we cannot rely on that basis under the GDPR and instead look to the legitimate interests basis. As we have already given students and parents an opportunity to object to inclusion in the study, and made clear that the subjects of the study are able to object to their inclusion at any stage, we have in all practical terms already completed what we would otherwise have been required to do under the GDPR in order to satisfy the requirements of the legitimate interests test.
- Accordingly, given that:
 - parents and students have already been given (and in one case exercised) a right to object to inclusion in the study;
 - it would be impractical, and in many cases impossible, to obtain refreshed consent from study participants;
 - were we to implement the study starting today, we would have followed, in all practical terms, the same process as has been undertaken already in relation to this study; and
 - the process implemented to date would have satisfied the GDPR requirements sufficiently to allow us to rely on the lawful basis of legitimate interests when processing the data.
- In our view, after balancing the interests of the subjects of the study, we can rely on the lawful basis of legitimate interests under the GDPR and process the data relating to the study as originally envisaged without the need to obtain any further consent from or provide further information to parents/students.

Timeline

Date	Activity
October-November 2017	Schools select eligible pupils for trial and confirm student interest in participation
By end November 2017	Opt-out consent forms are provided to students and parents/carers
WC 4th December 2017	School sends BIT student data for those who have not opted out
WC 11th December 2017	Students are randomly allocated to two groups by BIT
9th January 2018 17th January 2018	Intervention begins in Birmingham Intervention begins in Bristol and London
2nd January 2018	BIT begins case study data collection
27th April 2018 4th May 2018	Intervention ends in Birmingham Intervention ends in Bristol and London
30th April – 15th June 2018	BIT collects secondary outcome data (and participant IPE data)
20th July 2018	Case study data collection ends
2nd July 2019	Students sit GCSEs
1st December 2019	BIT collects primary outcome data from the NPD
31st January 2020	BIT submits draft evaluation report to EEF
23rd March 2020	EEF peer review and feedback complete
24th April 2020	EEF and BIT publish report

Appendix 1: Student opt-out consent form

The Community Apprentice Programme and Research Project Information for Students

What is the purpose of this project?

We want to be able to give students the best possible learning experiences. One way we are doing this is by being involved with research projects. These projects help the school to find out new ways to help students develop the skills they need to succeed.

That's why we are working with Envision on the Community- Apprentice Programme. The Behavioural Insights Team (a research company) is working on the project too, to understand how the project could benefit young people. We would like you to participate, but you do not have to.

What is the Community Apprentice Programme?

Community Apprentice is a bit like The Apprentice on television – only a lot more friendly. Students work in teams to develop and run their own projects to see who can make the most money for a local charity chosen by them.

How you do it is entirely up to you, but Envision who are running the programme, can provide lots of help. You'll get your own Team Coach to help you develop your ideas and support from a local business who will match-fund what you raise in school.

You'll also get to go to events with students from other schools taking part and take part in fun and inspiring activities.

Although you'll compete with young people in other schools', you'll also share one common goal: to see how much you can collectively raise for small local charities who really need your help. You'll also get something out of it for yourself. You'll learn to organise events and persuade other people to do things – skills that are really useful in other areas of your life.

What happens if I want to take part in this project?

The programme is very popular, so places are limited. To ensure everyone has a fair chance of participating in the programme, students who are interested will be picked at random to join one of two groups. The first group starts the Community- Apprentice Programme. The second does not, and continues with normal school activity. Both groups will continue to attend lessons in school, just as they normally would. Both groups are a very important part of the research, as they both help researchers understand the benefits of the programme. By taking part in the research, you allow your school to share your data with researchers of the Behavioural Insights Team. If you like, we can give you a record of any data that is shared.

If I participate, how will my privacy be protected? What happens to my data?

The data will be confidential and only used for this project. We will use a random number to label and store data instead of your name once the study is complete.

Your data will ONLY be used for the purposes of this project. You will not be identifiable in any of the reports produced after the project.

What are the risks and benefits of participation in this project?

We see no likely risks or discomforts for you. You will not be paid for participating, but you could benefit from the programme as it is designed to help you develop important skills. It is possible that you might not get a place on the programme this time, as places are limited - but you will have the same chance as any other pupil in the school to get a place.

If I have any questions or concerns about this project, who can I talk to?

If you have questions or concerns, you can speak to either [contact at school] at [School] or e-mail Patrick Taylor (Patrick.taylor@bi.team) at the Behavioural Insights Team.

What if I change my mind?

If you change your mind, you can withdraw from the research at any time by e-mailing patrick.taylor@bi.team or by telling your school teacher.

Not participating or withdrawing from the research will not affect your relationship with the school or have any other penalty.

Pupil Withdrawal Form

If you are happy to take part in project, you do not need to do anything; you will be automatically enrolled in the research and you will have a 50/50 chance of taking part in the Community Apprentice programme.

If you do not want to participate, please fill out the details below and return the form to [INSERT NAME OF CONTACT TEACHER AT SCHOOL].

Student Name (please write in block capitals:

Signed _____

Date _____

Appendix 2: Parent/guardian opt-out consent form

Dear parent/carer,

We are working with a charity called Envision to run a programme called 'the Community Apprentice' that is designed to help young people develop the skills they need to succeed, particularly in the workplace. It is part of an exciting national research project which aims to understand how the programme supports pupil development.

We would like your child to take part in this project. Before you make a decision it is important for you to know why the research is being done and what it will involve. If you are happy for your child to take part, you do not need to do anything, they will be automatically included in the project. If you do not want you or your child to take part, we will ask you to tick the relevant box on the withdrawal form (see below) and ask your child to return it to their tutor before Friday 3rd November.

What is the Community Apprentice Programme?

The programme uses practical project based learning to enable pupils to develop the skills most valued by employers. Loosely based on the TV series The Apprentice, pupils will work in teams to develop their own projects to raise as much money as they can, except in their case all the money is for a local charity, which will be chosen by each team. Teams are supported to raise funds by a local business which also provides volunteer mentors. The programme will involve trips to their workplace to take part in workshops. Pupils will be accompanied by teachers on all these trips.

Throughout the programme, pupils will undertake a range of challenges designed to develop their self-confidence, grit and teamwork. Points will be earned for challenges undertaken and will contribute as much to the final score as the amount of money raised. Nine other schools from across the city will be taking part in the competition with us and it should be fun and rewarding experience.

What happens if my child takes part?

Places on the programme are limited, so not every pupil who is interested will receive a place. Pupils will be randomly assigned to one of two groups, one which takes part in the Community Apprentice Programme, and one that continues with normal school activity. This means that there is a 50/50 chance that your child will take part in the programme. Though some pupils will not take part in the programme, both groups form an important part of the research. This is because the evaluation team will compare results from both to identify what difference the programme made. Because places are limited, it is not possible for a child to take part in the programme but not the research.

We are asking for your permission to obtain information about your child from the school, including their name, date of birth and Unique Pupil Number. Information provided will be shared with a small group of researchers at BIT. We will use the information provided to link with the National Pupil Database (which is held by the Department for Education) to collect GCSE grades in 2019. It will be shared with the Department for Education, Education Endowment Foundation (EEF - the funder of this project), EEF's data contractor FFT

Education and in an anonymised form to the UK Data Archive for research purposes. Once your child's information is included in the data set, the data will be anonymised and no one will be able to identify individual pupils.

If your child participates, how will your child's privacy be protected? What happens to this data?

Every child participating in the research, whether taking part in the Community Apprentice programme or not, will have their data collected. All information that is collected about your child during the course of the research will be kept strictly confidential and will not be used in any way by the school to assess your child's progress. Research folders will be kept securely in a locked office at all times. Access to these folders will be restricted to study investigators and statisticians. Any information that is stored electronically will be kept securely on Behavioural Insights Team (BIT – the research team) computers.

The research team will collect data via the school and your child's data will ONLY be used for the purposes of this project. If you like, you can be provided with a record of any data that is shared. You and your child can withdraw at any time from the study by emailing Patrick Taylor at BIT (patrick.taylor@bi.team). Whichever group your child is allocated to, he/she will not be identifiable in any resulting research reports.

What are the possible risks and benefits of participation in this project?

Your child may benefit from participating in the programme, as it is designed to help them develop the skills required for future employment success. The programme leaders and research team do not anticipate any likely risks or discomforts for your child. Your child will not be paid for participating.

If I have any questions or concerns about this project, who can I talk to?

If you have questions or concerns, you can speak to either [contact at school] at [school] or e-mail Patrick Taylor at BIT (patrick.taylor@bi.team).

How long will the study last?

- The programme itself will run until May 2018
- Final research won't be published until April 2020 because we will be using your child's GCSE results as part of the research
- No practical participation will be required from your child after May 2018

What if I change my mind?

You can change your mind about any part of your child's participation at any time you like, and do not have to give a reason for doing so. Your decision to take part or not will in no way impact on your relationship with the school, now or in the future. If your child changes their mind, they can withdraw at any time by returning the form below to [INSERT CONTACT TEACHER NAME].[1]

Many thanks
[School Signature]

Parent Withdrawal Form

If you are happy for your child to take part, you do not need to do anything, you will be automatically enrolled in the research project.

If you **do not** want your child to participate in this project, please complete this form and return it to **[INSERT CONTACT TEACHER NAME]** by Friday 3rd November.

Parent Name: _____

Student Name: _____

Signed _____

Date _____

Appendix 3: BIT External Data Security Policy

Attached as a separate document.

Appendix 4: School MOU

COMMUNITY APPRENTICE RANDOMISED CONTROL TRIAL, MEMORANDUM OF UNDERSTANDING

THE PROJECT

The Community-Apprentice Programme is a term-long programme of structured activity for KS4 students. It is designed and facilitated to improve the ability of students to work with peers on a group task whilst building their skills and confidence. It achieves this whilst enabling them to make a contribution to a local charity of their choosing.

The programme is delivered to two cohorts, each of 13 students, the first in the autumn term and the second in the spring term.

Each programme consists of three inter-related parts:

- 11 weekly group activities facilitated by Envision
- 4 group mentoring activities, which take place at the office of a local company
- 2 off-site cross-school events - one at the launch and end of each programme.

The programme works as an inter-school competition. Each school competes in a cluster of five schools. Teams meet other competing teams at cross-school events.

At the one-day launch event students receive presentations from local charities and decide which one their team would like to support. Each team is required to conduct at least one fundraising activity in their school. They also have support from their business mentors to raise additional funds from their supporting company.

At the end of the programme, students present their achievements to a panel of judges in the Community- Apprentice boardroom. The competition is judged on two criteria – the success of the student's projects (money raised) and well they evidence the skills they have developed.

Responsibilities of the project team

- To provide a single point of contact for the school – the 'Envision Project Co-ordinator' who will provide a key point of contact and ensure that the commitments below are fulfilled.
- To assign a member of the Envision SLT (the signatory of the MoU) to be a point of contact in the event that problems are not resolved between the Project Co-ordinators. They will also attend a mid-point project review meeting.
- To provide places for all participating students at two cross-school events per cohort, one at the start and one at the end of each programme.
- To provide appropriately trained coaches to facilitate 11 weekly group activities for each cohort.
- To provide appropriately trained staff to facilitate 4 group business mentoring sessions for each cohort.
- To identify a local business to provide volunteers and coordinate timings for business mentoring sessions.
- To ensure that all Envision coaches and staff are DBS checked (business mentors will not be checked as they will be engaged in activities facilitated by Envision staff) and insured.
- To ensure that all of site activity is risk assessed.

Responsibilities of the school

- To clearly assign responsibility for the project to a named 'School Project Co-ordinator' who will provide a key point of contact and ensure that the commitments below are fulfilled.
- To assign a member of the SLT (who will sign this MoU) to be a point of contact in the event that problems are not resolved between the project leads. They will also attend a mid-point review meeting.
- To recruit 13 students before the end of the summer term for cohort 1 and 26 students for cohort 2, ideally ensuring that at least 30% are eligible for pupil premium.
- To secure parental consent for all off-site trips.
- To complete data-sheets for all participating students.
- To ensure that all events, mentoring and session dates are booked in the school calendar and the slots are protected.
- To support students to complete a minimum of 20 hours of provision and participates in all employer engagement sessions.
- To provide an appropriate member of staff to accompany students on all off-site events.
- To provide a minibus and driver to transport students to all off site events (in the event that this is not possible then the school agrees to pay 50% of the cost of any transport Envision has to arrange).NOT LONDON
- To ensure an appropriate classroom is provided for all weekly sessions.

THE RANDOMISED CONTROL TRIAL

A randomised control trial will be conducted on the project during the spring term. It will not involve any students included in the autumn term cohort.

Structure of the evaluation

The Behavioural Insights Team (BIT) will be responsible for conducting the independent evaluation of the programme. Random assignment of students to two groups is essential to the evaluation as it is the best way of understanding what effect the programme has on project outcomes. Before the programme begins, students who are interested will be randomly assigned to one of two groups, one which starts the programme, and one that continues with normal school activity.

Though some students will not start, both groups form an important part of the research. This is because the evaluation team will compare results from both to identify what difference the programme made. It is important that schools understand and consent to the random allocation process.

Pupils and parents will be given an opportunity to opt-out of the evaluation before it begins, via an opt-out consent form.

The evaluation timeline is as follows:

1. Students are selected/volunteer to take part by 20th Oct 2017
2. Opt-out consent forms are provided to students and parents/carers by 20th Oct 2017
3. Students and parents/carers are given 2 weeks to opt-out of the trial – 3rd Nov 2017
4. School sends BIT student data for those who have not opted-out by 6th Nov 2017
5. Students are randomly allocated to two groups by 13th Nov 2017

6. The intervention group receive the programme, while the second group continue with routine school activity [spring term]
7. Survey outcome measures are collected after 14th May (to be agreed with school)
8. Students sit GCSES – May/June 2019
9. BIT collects student attainment data from the National Pupil Database – Autumn 2019
10. Evaluation report produced January 2020

Once the programme is complete, the BIT research team will work with the school to conduct a survey with the students. This survey will take no more than 45mins to complete. Once the evaluation has been completed a report will be written to describe the trial and its results. It is intended that the report will be published after it has been circulated to Envision and The Education Endowment Foundation. A further series of documents (for example blog posts, leaflets or presentations) to disseminate the learning from the trial may also be developed by these parties once the trial has been concluded.

Use of data

All data, including student test responses, will be treated with the strictest confidence. BIT shall comply with the provisions of the Data Protection Act 1998 and all other applicable laws and guidance relating to data protection. BIT has also entered into a Data Sharing Agreement with Envision to ensure data required for the trial is protected.

Every child participating in the research, whether allocated a place on the programme or not, will have their data collected. This is important as it allows the Research Team to understand the effects of the programme. The research team will collect this data directly via the school and it will ONLY be used for the purposes of this project. Survey data will be collected by BIT researchers. Attainment 8 outcome data will be collected via the National Pupil database. Students may also be asked to take part in a case study for the qualitative element of the research. This is not compulsory and may be offered following the completion of the programme. All information that is collected about students during the course of the research will be stored electronically by BIT. It will be kept in a locked office at all times to ensure security, and access to this data will be restricted to study investigators and statisticians.

Any transfer of personal data between parties will be performed in a secure manner (i.e. using password encrypted files).

Schools will need to provide the following student level information to BIT:

- First and last name
- Unique Pupil Number (UPN)
- Gender
- Ever 6 FSM and Pupil Premium
- Postcode

Test data will be collected at your school by BIT researchers. This data will be collected through a survey and will take approximately 45mins for each student.

Attainment 8 data will be collected to enable the researchers to determine the effect of the programme on attainment. This will be collected by BIT via the National Pupil Database (NPD).

Whichever group the pupil is allocated to, and whether or not they are part of the case study, they will not be identifiable in any resulting research reports. No other parties will have access to the data collected.

Responsibilities of the school

- To recruit 26 students to the evaluation
- To provide data on these 26 students to BIT
- To work with Envision to ensure that as many as possible of the selected students attend sessions.
- To organise for all 26 students (whether in the control group or participating in the programme) to participate in the end of programme survey
- To support BIT to interview students and the School Project Co-ordinator for a case study if required.

Responsibilities of the evaluation team

- To assign responsibility for the evaluation to a named contact and evaluation lead at Behavioural Insights, who will provide a key point of contact and ensure that the commitments below are fulfilled
- To provide schools with opt-out letters to send to parents/carers for participation and data collection
- To provide schools with opt-out letters for students
- To conduct the randomisation of students within participating schools and communicate student's allocation to Envision and participating schools
- To collect and analyse the data from the project to estimate the impact of the programme
- To publish a report on the findings of the project

SCHOOL AGREEMENT

I agree for my school _____
to take part in the Community-Apprentice programme and randomised control trial and I accept the responsibilities outlined above.

SLT lead name _____

SLT lead Signature: _____ Date: __/__/____

SLT lead Email Address: _____

School Project Co-ordinator name _____

School Project Co-ordinator Email Address _____

School Telephone Number: _____

Appendix 5: Non-cognitive skills survey

School Name:	Test Academy
Student Name:	Joe Bloggs
UPN:	000000000

About this survey

This survey asks you to think about yourself and your abilities in a range of different ways. There are no right or wrong answers – just put what feels right for you. We won't share your individual scores with anyone.

Part 1

Read each statement below and circle a number on the scale. The scale goes from 1 to 5 (where 1 = 'Strongly disagree' and 5 = 'Strongly agree').

Survey Item	Scale				
	Strongly disagree	Disagree	Neutral	Agree	Strongly agree
1. I will be able to achieve most of the goals that I have set for myself	1	2	3	4	5
2. When facing difficult tasks, I am certain that I will accomplish them	1	2	3	4	5
3. In general, I think that I can achieve things that are important to me	1	2	3	4	5
4. I believe I can succeed at most any endeavour to which I set my mind	1	2	3	4	5
5. I will be able to successfully overcome many challenges	1	2	3	4	5
6. I am confident that I can perform effectively on many tasks	1	2	3	4	5

7. Compared to other people, I can do most tasks very well	1	2	3	4	5
8. Even when things are tough, I can perform quite well	1	2	3	4	5

Part 2

Think about the times when you work in a team (either in or out of school). Read each statement below and circle a number on the scale. The scale goes from 1 to 5 (where 1 = 'Strongly disagree' and 5 = 'Strongly agree').

Survey Item	Scale				
	Strongly disagree	Disagree	Neutral	Agree	Strongly agree
1. I think that team-work is important	1	2	3	4	5
2. People who work in teams can learn more if they work by themselves	1	2	3	4	5
3. I feel confident in my ability to work in a team	1	2	3	4	5
4. I know how to give my team members feedback that will not hurt their feelings	1	2	3	4	5
5. I ask others for feedback	1	2	3	4	5
6. I make an effort to include other members of my group	1	2	3	4	5
7. I value the contributions of my team members	1	2	3	4	5
8. I treat my team members as equal members of the team	1	2	3	4	5

9. I am good at communicating with my team members	1	2	3	4	5
10. I feel confident in my ability to be a leader	1	2	3	4	5
11. I worked well with my business mentors	1	2	3	4	5
12. I worked well with my Envision coach	1	2	3	4	5

Part 3

Below are 12 situations in which you might need to communicate. Some people find it easier or harder to communicate in one situation over another. Please indicate how good you think you are at communicating in each of the situations described below by giving yourself a score out of 100 (where 0 = really bad and 100 = really good).

Survey Item	My Score out of 100
1. Present a talk to a group of strangers	
2. Talk with an acquaintance	
3. Talk in a large group of friends	
4. Talk in a small group of strangers	
5. Talk with a friend	
6. Talk in a large meeting of acquaintances	
7. Talk with a stranger	
8. Present a talk to a group of friends	

9. Talk in a small group of acquaintances	
10. Talk in a large meeting of stranger	
11. Talk in a small group of friends	
12. Present a talk to a group of acquaintances	

Part 4

We'd like to know a bit about the extra-curricular clubs and activities that you do.

Survey Item	Answer	
	Yes	No
1. Do you take part in any extracurricular clubs or activities? (This could be something like Young Enterprise, Scouts, Guides, Duke of Edinburgh, a drama club or a sports club).		
2. If you answered 'Yes' to question 1, please write down the name of the activities here:		

Thank you!

Appendix 6: Power Calculations

The following code was run in R (amending the assumptions for each scenario) for the power calculations.

```
#### Power calculation assumptions - Scenario 1:
```

```
#Total sample = 780, so n = 390 (equal arms)
```

```
#Significance level = 0.05
```

```
#Power = 0.8
```

```
#SD = 1 (result of power calc will be in Cohen's d)
```

```
power.t.test(n = 390, sig.level = 0.05, power = 0.8, delta = NULL, sd = 1, alternative = 'two.sided',  
type = 'two.sample', strict = TRUE)
```