# Mixed methods efficacy randomised controlled trial of a psychologically informed coaching model for care experienced young people

# Statistical Analysis Plan

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Table 1: Project Overview

|  |  |
| --- | --- |
| **PROJECT TITLE** | Mixed methods efficacy randomised controlled trial of a psychologically informed coaching model for care experienced young people |
| **DEVELOPER (INSTITUTION)** | 1625ip |
| **EVALUATOR (INSTITUTION)** | Behavioural Insights Team |
| **PRINCIPAL INVESTIGATOR(S)** | Hazel Wright Giulia Tagliaferri |

Table 2: SAP version history

|  |  |  |  |
| --- | --- | --- | --- |
| **VERSION** | **DATE** | **REASON FOR REVISION** |  |
| **1.1** **[LATEST]** |  |  |  |
| **1.0 [ORIGINAL]** | 18.09.23 |  |  |

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## Introduction

This Youth Futures Foundation (YFF) funded randomised controlled trial (Reboot III) seeks to identify whether a programme of one-to-one coaching based on a psychological therapy model has a causal effect on increasing the proportion of care experienced young people in education, employment or training (EET) and thus improving their life outcomes.

The Reboot III programme will target young people aged 16-25 (Young Persons - YPs now on) who are care-experienced in either the 3 local authorities of the West of England Combined Authority or the North Somerset local authority (i.e. across the 4 local authorities of Bristol, Bath and North East Somerset (BaNES), North Somerset (N. Somerset) and South Gloucestershire (S. Glos)), and who are not in education, employment or training (NEET) or at risk of being NEET.

The evaluation will be an individual level RCT with the programme delivered over a period of 2 years for each YP in the treatment group (minimum sample size: 409 YPs), and outcomes measured during the last 6 months of the programme. The aim of this evaluation is to assess the impact of Reboot III on a series of outcomes for YPs: EET status (primary outcome), employment and earnings. The evaluation also includes a mixed method implementation and process evaluation (IPE). This is detailed in the Trial Protocol and is not part of the Statistical Analysis Plan (SAP).

## Design overview

See “Trial Design” in the Trial Protocol document.

## Sample size calculations

### How many participants do we need?

1625ip is receiving funding to provide 265 Reboot places. To ensure the trial is sufficiently powered, whilst also ensuring Reboot places are filled, we estimate **we will need at minimum, a total of 409** **participants randomised to the trial**.

Based on our power calculations (see Annex A in this document and ‘Sample size calculations/Power’ section in the Trial Protocol) we believe a control group of 144 participants would be needed to be sufficiently powered **(thus 144 in control + 265 in reboot = a total sample size of 409 at referral).** Any additional YP (over the target of 409) will be allocated to the control group. Randomisation will be done on a month-by-month basis, see ‘Randomisation’ in the Trial Protocol.

We aim to randomise 265 people into the treatment group to protect against the risk of attrition (between referral and starting Reboot) and ensure that at least 250 people start the Reboot programme. If the additional 15 young people do in fact join the programme, 1625ip have confirmed they will be able to support them. The next subsection provides an estimate of the likely number of eligible YP.

### How many eligible participants are there?

In 2022 each of the four LAs shared detailed figures with us on the number of care experienced young people in their area. Our estimates of the number of eligible participants are primarily based on these figures. Based on their data, there are approximately 1,500 young people in the four local authorities who are either:

* Care leavers with an open case (a Personal Advisor (PA) assigned)
* 16-17-year-old young people in care
* Unaccompanied asylum seeking children

The subgroups with the highest potential rate of referrals are:

* 18–20-year-old care leavers (estimated total: 425)
* 16–17-year-old young people **in care** and care leavers (those who will turn 18 during the programme) (estimated total: 363)

These subgroups have a combined estimated total of 788 YP. Based on the available evidence we assume that 50% of these subgroups meet all eligibility requirements[[1]](#footnote-2), resulting in 394 eligible young people.

Additionally, there are two subgroups of young people who are eligible but considered more challenging to target and retain:

* Unaccompanied asylum-seeking children (estimated total: 156)
* YP aged 21 and over with open cases (estimated total: 386)

The estimated total size of these two subgroups is 542. Assuming that 25% of these subgroups meet all eligibility requirements, this adds another 136 young people to the potential sample size.

**Overall, this means that we estimate that there is a potential sample size of eligible 530 young people for the trial (394+136).** A sample size of 530 young people would result in a treatment group and control group of 265 participants each. We would need 77% of this total to be referred to the trial to reach our minimum target of 409. This gives us confidence that enough YP exist to meet our minimum target.

Table 3. Sample size summary

|  |  |
| --- | --- |
| Sample size | 409 - 530 |
| Sample / Arm | 265 treatment participants, 144 - 265 control participants |
| Justification: | Based on the number of Reboot III places and our estimate of available eligible young people. |
| Attrition: | We assume 10% data attrition at the end of the trial. |

## Power

We have conducted power calculations for the primary outcome variable (EET). Analysis was conducted in R and the code can be found in Annex A.

Table 4. Power calculations

|  |  |  |  |
| --- | --- | --- | --- |
|  | | **PROTOCOL** | **RANDOMISATION** |
| **MINIMUM DETECTABLE EFFECT SIZE (MDES)** | | 13.1pp increase in EET % (Cohen’s H of 0.27) |  |
| **PRE-TEST /**  **POST TEST CORRELATIONS** | LEVEL 1 (PARTICIPANT) | R² = 0.2. The predictive power of a baseline measure of being in EET, individual characteristics and educational data. Conservative estimate based on previous research.[[2]](#footnote-3) |  |
| **ALPHA** |  | 0.05 | 0.05 |
| **POWER** | Note: as there is only one primary outcome, a multiple comparisons correction is not required for the primary outcome. | 0.8 | 0.8 |
| **ONE SIDED OR TWO SIDED?** |  | two sided |  |
| **AVERAGE CLUSTER SIZE**  **AND / OR SITE SIZE** |  | n/a |  |
| **NUMBER OF CLUSTERS** | INTERVENTION | n/a |  |
| CONTROL | n/a |  |
| TOTAL | n/a |  |
| **NUMBER OF PARTICIPANTS** | INTERVENTION | 265 |  |
| CONTROL | 144 |  |
| TOTAL | 409 (minimum) |  |

### Assumptions for power calculations

Table 5: Assumptions for power calculations

|  |  |  |
| --- | --- | --- |
| **ASSUMPTION** | | **RATIONALE** |
| **Alpha (significance level)** | 5% | Standard assumption |
| **Power** | 80% | Standard assumption |
| **Total planned sample size** | 409 | See our sample size section |
| **Attrition** | 10% | Attrition can happen if data collection is not possible at the end of the trial. This can happen if the LA is unable to get in touch with the YP during the outcome data collection period. We’ve been told this is rare for YP they are in touch with (all YP under 21 and a proportion of YP over 21). 10% attrition was agreed in discussion with 1625ip. |
| **Predictive power from covariates** | R² = 0.2 | The predictive power of a baseline measure of being in EET, individual characteristics and educational data. Conservative estimate based on previous research.[[3]](#footnote-4) |
| **Number of trial arms** | 2 | Reboot (treatment) and Usual Local Offer (control) |
| **Base rate** | 30% in EET | 30% of Reboot I cohort was in EET at baseline (using our definition of being in EET 2 out of 3 measure points 2 months apart) |
| **What is the calculated MDES for this trial?** | 13.1pp increase in EET % (Cohen’s H of 0.27) | See power calculation table 6 below |
| **What *substantive* effect size do you anticipate from the intervention?** | 13pp increase in EET % (Cohen’s H of 0.26) | No published data or studies identified that measure the impact of a programme as substantial as Reboot. The most similar ones we found saw effect sizes of 2-13 pp on EET status/outcomes. Due to the higher intensity of the Reboot programme compared to the studies we found, we believe it’s reasonable to anticipate an impact in line with the upper bound of these studies.  The proportion of Reboot I participants who would have been considered in EET according to the proposed indicator definition increased by 11pp, from 30% in the first 6 months of Reboot to 41% at the last 6 months of the two-year period. This is not a robust impact estimate as there is no counterfactual group to compare against. Note also that Reboot might already have had an impact on the young people’s EET status during the first 6 months, and thus this figure might underestimate the true impact of the programme.  More details on the anticipated effect of the intervention are available in the SAP. |
| **Is the planned MDES the same as or smaller than the anticipated effect of the intervention?** | **Roughly the same, but with uncertainty** | The calculated MDES is fractionally higher than the anticipated effect size. We have several mitigations in place to improve the MDES, including aiming for a higher sample size and including covariates. If we reach our target sample size of 530 our MDES would be 10.9pp. |
| **Have you corrected for multiple comparisons?** | No (not necessary) | We only have 1 primary outcome measure and 1 secondary outcome measure, thus we do not need to correct for multiple comparisons. |

### Anticipated effect of the intervention

#### There is limited literature available on the effect size of an intensive long-term training programme on EET outcomes among care leavers. Papers that analysed the impact of EET support programs on EET outcomes among young people found impacts that ranged between 2pp (not significant) and 13pp, with the evaluations most similar to this one finding a significant impact of 11p and 13pp on employment/education.[[4]](#footnote-5),[[5]](#footnote-6),[[6]](#footnote-7) For example, a matching analysis of the Activity Agreement model[[7]](#footnote-8) found an approximate impact of 13pp on EET status of 16-17-year olds with extra needs 3 months after the intervention.[[8]](#footnote-9) It is worth noting that a matching analysis is likely to overestimate the effect of the intervention compared to an RCT. Additionally, our intervention includes older YPs, among whom the proportion who are NEET tends to be higher. This means that a larger effect would be possible.

#### Estimated effect of Reboot I

The proportion of Reboot I participants who would have been considered in EET according to the proposed indicator definition (in EET at least 2 of the last 3 measure points) increased, from 30% in the first 6 months of Reboot to 41% at the last 6 months of the two-year period (so an increase of 11pp). However, this is not a robust impact estimate as no counterfactual group could be compared against. We don’t know whether without Reboot support the EET % would have gone up, down or remained the same. In addition, if Reboot support impacts the young people’s EET status during the first 6 months, this figure underestimates the true impact of the programme as the pre-measure is taken over the first six months of support.

#### Power calculations results

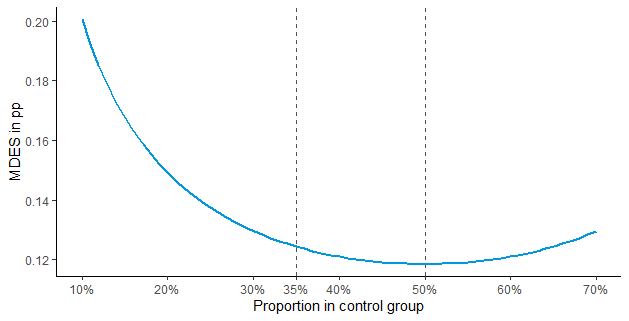
Table 6 provides the result for the power calculations for 3 scenarios:

1. **Sample size substantially less than expected, equal distribution.** In this first scenario we assume that recruitment numbers were significantly lower than our minimum target (288 instead of 409). We assume that to maximise statistical power we distributed them evenly across treatment and control (which means many of the Reboot places would not be filled). In this scenario, we are powered to detect an impact of 15.0pp (Cohen’s h = 0.31). To reach the same MDES with all Reboot places filled, we’d have to recruit an additional 77 young people.
2. **[MAIN SCENARIO] We reach the minimum target sample size (409)**. If we reach our minimum target of 409 young people, we are powered to detect an effect size of 13.1pp (Cohen’s h = 0.27). The same MDES can be reached with 37 less young people if participants were evenly distributed across the treatment and control group.
3. **We reach our stretch target sample size (530).** If we reach our ideal target of 530 young people, we are powered to detect a difference of 10.9pp (Cohen’s h = 0.23).

Table 6. Power calculation results for primary outcome variable (EET status)

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| **# of Reboot participants** | **# in comparison group** | **Total sample size** | **EET** | | |
| **Cohens’ H**  **Effect size** | **MDES** | **% EET in Reboot at endline^** |
| 144 | 144 | 288 | 0.31 | 15.0pp | 45% |
| **265** | **144** | **409** | **0.27** | **13.1pp** | **43%** |
| 265 | 265 | 530 | 0.23 | 10.9pp | 41% |

One of our objectives is to ensure that all 250 Reboot places are filled. If we reach our minimum target sample size, we can achieve this by allocating 35% of participants to control and 65% to treatment. As previously mentioned, this allocation comes with a slight reduction in power compared to allocating 50% to both groups. Figure 1 below illustrates the relationship between the proportion of participants allocated to the control group and the minimum effect size the trial will be powered to detect. The figure indicates that allocating between 35% and 50% of participants to the control group results in only a minimal difference in the minimum effect size. However, if the proportion is reduced to below 35%, the decrease in power becomes significant.

Figure 1. MDES of trial, assuming a total sample size of 409 YP.

## Analysis

The analysis will be an Intention to Treat - comparing the outcomes of YPs assigned to the treatment and control group. The analysis will be done at the YP level (unit of randomisation). The methods of analysis were chosen *a priori* (before data collection took place). The analysis will be conducted in R or Stata.

The following tables summarises the outcomes and the analysis plan.

Table 7. Summary of RQs for the impact evaluation and related outcomes

|  |  |  |  |
| --- | --- | --- | --- |
| **RQ** | **QUESTION** | **OUTCOME** | **HOW IT IS MEASURED** |
| PRIMARY | Does offering Reboot support increase the likelihood of being in EET among care experienced young people? | EET status 18-24 months after randomisation[[9]](#footnote-10) | First best (if viable – see Trial Protocol BOX 1: Alternative data sources for constructing the EET outcome measure): EET status constructed using LEO data.  Second best: LA data. A YP will be deemed to be in EET if they are in EET at least 2 out of 3 touch-points in the six months between 18-24 months from randomisation (i.e. from the date each individual is randomised, which will be a different calendar date for each person). |
| SECONDARY | (I) Does offering Reboot support increase the likelihood of being employed for care experienced young people? | Employment status 18-24 months after randomisation | HMRC data. A YP will be deemed in employment if they are employed for at least two thirds (66%) of days employed during the 6 months’ equivalent to a 5-day working week (Mon-Sunday)- where the 6 months are occurring between 18-24 months from randomisation (i.e. from the date each individual is randomised, which will be a different calendar date for each person). |
| (II) Does offering Reboot support increase the time spent in employment for care experienced young people? | Days in employment 18-24 months after randomisation | HMRC data. We will calculate the total number of calendar days a YP has been employed in the 6 months occurring between 18-24 months from randomisation (i.e. from the date each individual is randomised, which will be a different calendar date for each person).  We will consider a person to be employed if they have a contract or are self-employed AND they have received compensation for the work. |
| (III) Does offering Reboot support increase the average earnings for care experienced young people? | Total earnings 18-24 months after randomisation | HMRC data. This will be the sum of a YP’s monthly earnings in the 6 months occurring between 18-24 months from randomisation (i.e. from the date each individual is randomised, which will be a different calendar date for each person) - for all YPs with total earnings > 0. |
| EXPLORATORY | Does offering Reboot support promote the progression towards employment for care experienced young people? | Experimental EET scale 18-24 months after randomisation (different date for each person) | LA data. The outcome is the position on the scale, ranging from 1 to 3 (NEET/PT/FT). This will be informed by the same data we are using for the primary outcome variable, collected between 18-24 months from randomisation (i.e., from the date each individual is randomised, which will be a different calendar date for each person) |

### 

Table 8. Summary of planned analyses and outcomes

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
|  | **Primary** | **Secondary (I)** | **Secondary (II)** | **Secondary (II)** | **Exploratory** |
| Model type | Logistic | Logistic | OLS | OLS | OLS |
| Outcome measure | EET status | Employment status | Time in employment | Total earnings | Position on EET scale |
| Data source | LEO or LA data - see the TP for more details | HMRC | HMRC | HMRC | LA data |
| Main independent variable | A binary indicator for the treatment arm | | | | |
| Additional covariates | The local authority the individual lives in the month of referral (MM/YY)  Age at referral  Gender  EET status at referral  Additional covariates from the NPD: KS4 attainment for Maths and English; Absence rates; ethnicity; disability status.  Dummy variable indicating occasional refusal (missingness of EET status in at least one touchpoint). | | | | |
| Purpose | Estimated treatment effect. This result will determine the main recommendation for further funding/scaling | Estimated treatment effect | | | Methodological Estimated treatment effect |
| Confidence intervals | 95% CI | | | | |
| Multiple comparison adjustment? | No | Presenting result both adjusted and unadjusted for multiple comparisons (using the Benjamini-Hochberg procedure) | | | No |

**Equations**

***Primary outcome measure: EET status***

We will estimate a logistic regression for the primary outcome variable, with local authority, month of referral, and individual characteristics included as coefficients.

Formally, we will estimate a model of the form:

Where

* is a binary outcome variable, equal to 1 if young person *i* was in EET, 0 otherwise
* is a constant
* is a binary variable, equal to 1 if individual *i* was randomised into the Reboot group, 0 if randomised into the control group.
* is a matrix of control variables, which will include *at a minimum* the ones outlined in table 11.

***Secondary outcome measure (I): Employment***

We will repeat the analysis using the same specification as the primary analysis, but with binary outcome variables indicating whether the young person was in employment.

***Secondary outcome measure (I1): Time spent in employment.***

We will estimate a linear regression with local authority, month of referral, and individual characteristics included as coefficients.

Formally, we will estimate a model of the form:

Where

* is a continuous outcome variable representing the number of days in employment
* is a constant
* is a binary variable, equal to 1 if individual *i* was randomised into the Reboot group, 0 if randomised into the control group.
* is a matrix of control variables, which will include *at a minimum* the ones outlined in table 11
* is an iid error term with a normal distribution

***Secondary outcome measure (III): Earnings***

We will repeat the analysis using the same specification as for time spent in employment.

***Exploratory outcome measure: EET scale***

We will repeat the analysis using the same specification as for time spent in employment.

### Interim & follow-up analysis

In summer 2026 we will aim at having a view whether accessing LEO (that would give the most robust estimate and, if so, will provide the sole primary outcome) will be feasible or not - more details are available in the Trial Protocol.[[10]](#footnote-11) If so, BIT would still produce analyses using self-reported EET status as outcome in Autumn 2026 - BIT & YFF will consider these results as ‘interim’, not definitive of the impact of Reboot on YP’s EET status.

If administrative data appears not to be a feasible option, we will need to rely on EET status as self-reported by YPs. For reporting in Autumn 2026, we will need to rely on YPs' self-reported EET status, acknowledging the limitations of the data collection method and making clear that, if LEO will be available in the future (follow-up analysis), results from LEO will supersede these results.

### Imbalance at baseline

At the randomisation stage, we will check the balance on age, gender and EET status at referral because these are the characteristics we receive through the referral form. We chose to be parsimonious and do not ask for other characteristics at referral, because they either are special characteristics (such as ethnicity) or collecting them adds additional burden to the LA. We will check for balance across each LA and the overall sample each time after randomisation. Due to the high number of strata we don’t do balance checks for each stratum.

In the final report, we will present a table of these baseline descriptive characteristics for all YPs as they were randomised, and for those analysed. The former will inform whether randomisation was successful at obtaining a balanced sample, while the latter will provide evidence of whether sample attrition might have introduced an imbalance. For continuous variables, we will report means and standard deviations. For categorical variables, we will report counts (including the numerator and denominator) and percentages in each category. We will discuss any differences in the report.

### Missing data

### In the outcomes collected by LA (EET status and position on EET scale)

Missing values can happen if the LA is unable to contact the young person to collect their EET status. This can happen systematically (a YP refuses contact with the PA) or occasionally (the PA fails to collect EET status at one isolated touchpoint).

* Systematic refusal: all three touchpoints for EET status are missing values. This is equivalent to attrition, from an evaluation point of view. This means that this YP will be excluded from the analysis.
  + In order to explore the nature of missingness, an indicator (1= YP drops out of data collection; 0 = outcome data available for the YP) will be regressed against the YP’s observable characteristics measured at baseline, a set of dummies for each LA and month of referral. This will allow us to investigate if any variables are predictive of missingness.
  + If attrition in the treatment group is more than 5 percentage point different to the attrition in the control group, we will consider the adoption of a weighting procedure.
* Occasional refusal: at least one datapoint (touchpoint) for EET status is present. For the missing data point, we will assume that the status is unchanged from the previous touchpoint.
  + Calling the last four touchpoints -1 (the one before the last 6 months of the trial), 1 (first touchpoint in the last 6 months of the trial), 2 (second touchpoint in the last 6 months of the trial), 3 (third touchpoint in the last 6 months of the trial):
    - if 1 (or 1 and 2) is missing, a person is EET if they are EET in -1
    - if 2 (or 2 and 3) is missing, a person is EET if they are EET in 1
    - if 3 is missing, a person is EET if they are EET in 2
  + Regressions will include a dummy variable indicating occasional refusal (missingness of EET status in at least one touchpoint).

#### In the outcomes created using HMRC data (employment status, days in employment, total earnings)

HMRC monthly tax returns are an administrative data source and as such we do not expect it to have missing data. This is because the data are used for important purposes such as calculating income tax and national insurance contributions, and thus the quality and completeness of the data is expected to be very high. If data is missing, the most likely reason for that is that the data doesn’t exist (i.e. the young person did not have any taxable income). The structure of the dataset is such that it will have a row for each trial participant. If a YP has an employment contract in a given month, values for that contract will appear in the monthly dataset; if not, values will be blank. This means that we will interpret a missing value as “a contract is not in place”/ “the person is not in employment”.

We recognise that HMRC data has some limitations that may increase the volatility of earnings/employment duration. First, in some instances, the period for which monthly payments relate could relate at least in part to work done in the previous month. Second, sometimes submissions to HMRC are missed and corrected with the next submission. This can create the impression of a break in an employment spell when none existed.

#### In the covariates

* System variables: The local authority the individual lives in; the month of referral (MM/YY)
  + These are generated at referral and cannot be missing by construction.
* Variables from referral form: Age at referral; gender; EET status at referral
  + These are collected within the referral form. If missing, we will impute the average (if the var is continuous) or an additional category for missing (if categorical) and adding additional set of dummies to flag the imputed values.
* Variables from DfE’s NPD
  + Being this an administrative dataset, the most likely reason for missing data is a failure to link the young person to the appropriate data in the external dataset. This will happen if the young person doesn’t have a Unique Pupil Number (UPN), for example if the young person did not attend state school in the UK (in our trial this will most likely be unaccompanied asylum-seeking children who did not attend school in the UK). If so, we will impute the average (if the var is continuous) or an additional category for missing (if categorical) and adding additional set of dummies to flag the imputed values.

### Compliance

Compliance will be analysed as part of the Implementation and Process Evaluation (IPE). More details are available in the relevant section in the TP.[[11]](#footnote-12) We expect to receive for each YP assigned to the treatment group, the number of sessions that the YP took as part of the Reboot III programme - as recorded by the service provider 1625ip.

### Presentation of outcomes

Effect sizes will be presented as estimated regression coefficients from the OLS regressions (estimated betas from the regressions presented in the section ‘Analysis’ above). Point estimates will be presented with the associated standard error and p-value. When presenting the results graphically (e.g. in a bar graph), the height of the bar related to the control group outcome will be the value of the outcome for the control group (unadjusted mean); the height of the bar related to the treatment group outcome will correspond to the sum of the control group bar height and the estimated beta treatment effect from the regressions; confidence intervals will be presented around the treatment group bar.

### 

**Annex A: Power analysis code**

###########################

library(TREX)

library(pwr)

reboot.size <- c(144,265,187,265,265)

control.size <- c(144,99,187,144,265)

R2 <- 0.2

# EET

counterfactual=0.3

results <- data.frame(reboot.size = reboot.size,

control.size = control.size,

MDES.adj = numeric(length(reboot.size)),

cohens.H = numeric(length(reboot.size)))

for (i in 1:length(reboot.size)){

cohen.h <- pwr.2p2n.test(n1=reboot.size[i],n2=control.size[i],power=0.8)$h

cohen.h.adj <- cohen.h\*(sqrt(1-R2))

MDES.adj <- cohensH(p1=counterfactual,h=cohen.h.adj)$p2 - cohensH(p1=counterfactual,h=cohen.h.adj)$p1

results[i, "MDES.adj"] <- MDES.adj

results[i, "cohens.H"] <- cohen.h.adj

}

results

1. Eligibility definitions (including inclusion, exclusion and discretionary criteria) are detailed in the ‘Participants’ section of the main Trial Protocol. [↑](#footnote-ref-2)
2. Britton, J., Gregg, P., Macmillan, L., & Mitchell, S. (2011). *The early bird ... preventing young people from becoming a NEET statistic.* Department of Economics and CMPO, University of Bristol. [↑](#footnote-ref-3)
3. Britton, J., Gregg, P., Macmillan, L., & Mitchell, S. (2011). *The early bird ... preventing young people from becoming a NEET statistic.* Department of Economics and CMPO, University of Bristol. [↑](#footnote-ref-4)
4. Nafilyan, V., Newton, B., Speckesser, S., Maguire, S., Devins, D. and Bickerstaffe, T (2014) *The Youth Contract for 16-17 year olds not in education, employment or training evaluation*. [online] Department for Education. [↑](#footnote-ref-5)
5. Alzua, M., Cruces, G. and Lopez-Erazo, C. (2013) *Youth training programs beyond employment. Mimeo: Evidence from a randomized controlled trial*. [↑](#footnote-ref-6)
6. Zinn, A.E., and Courtney, M.E. (2017) *Helping foster youth find a job: a random-assignment evaluation of an employment assistance programme for emancipating youth*. Child & Family Social Work, 22, 155-164. [↑](#footnote-ref-7)
7. An Activity Agreement is an agreement between a young person and their PA that the young person

   will take part in a programme of tailored learning and activity which helps them to become ready for formal learning or employment. [↑](#footnote-ref-8)
8. Young People Analysis Division (2010) *What works re-engaging young people who are not in education, employment or training (NEET)? Summary of evidence from the activity agreement pilots and the entry to learning pilots*. [online] Department for Education. [↑](#footnote-ref-9)
9. The trial protocol refers to

   * A data collection window of 6 months
   * A period 18-24 months from when a YP is randomised.

   We use these two definitions interchangeably. Further work with the LAs will be needed to establish whether data collection will last 6 months or 7 months (inclusive of month 24), and whether the exact timing of data collection can be mandated. BIT will conduct a workshop with the relevant stakeholders ahead of data collection to finalise any details still unresolved. This will guarantee that (a) the approach will be up to date with the latest softwares/systems adopted by the LAs (b) we will have the buy-in (and the attention) of the PAs who will perform the data collection. [↑](#footnote-ref-10)
10. See ‘Box 1’ in Trial Protocol [↑](#footnote-ref-11)
11. Please see ‘Fidelity Assessment’ section in the Trial Protocol. [↑](#footnote-ref-12)