

EVALUATION REPORT

Boys' Development Programme

Efficacy study

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Contents

Introduction	17
1 Background	17
1.1 Context	17
1.1.1 National context	18
1.1.2 School absence, exclusion and the risk of offending	19
1.1.3 Disproportionality in absence and exclusion	20
1.1.4 Links between school engagement, absence and exclusion	21
1.1.5 Evidence that social-emotional approaches improve school engagement	22
1.2 Rationale for the Boys' Development Programme	23
1.3 Rationale for a randomised controlled trial evaluation of the Boys' Development Programme	25
1.4 About the Boys' Development Programme	26
1.4.1 Theory of change	26
1.4.2 Who does the Boys' Development Programme aim to work with?	28
1.4.3 How does the Boys' Development Programme work with boys?	29
1.4.4 What does the Boys' Development Programme aim to achieve?	32
1.5 Evaluation objectives	33
1.5.1 Impact evaluation	33
1.5.2 Implementation and process evaluation	33
1.6 Ethics and trial registration	34
1.7 Data protection	35
1.8 Evaluation team and delivery teams	36
1.9 Timeline	37
2 Methods	39
2.1 Overview	39
2.2 Impact evaluation methods	39
2.2.1 Trial design	39
2.2.2 Race equity, equality, diversity and inclusion	40
2.2.3 Randomisation	42
2.2.4 Outcome measures	43
2.2.5 Sample size calculations	48
2.2.6 Recruitment rates	50
2.3 Participant journey	51
2.3.1 Participant referrals, eligibility and screening	53
2.3.2 Collecting informed consent	53
2.3.3 Data collection	54
2.3.4 Treatment phase and business-as-usual	57
2.4 Statistical analysis	58

2.4.1	Primary analysis	58
2.4.2	Secondary analysis.....	59
2.4.3	Longitudinal analysis	60
2.4.4	Subgroup analyses	61
2.4.5	Analysis in the presence of non-compliance.....	62
2.4.6	Mechanism of change analysis	64
2.4.7	Analyses of school administrative data.....	65
2.4.8	Missing data analyses	67
2.4.9	Estimation of effect sizes	68
2.5	Implementation and process evaluation methods.....	68
2.5.1	Research questions.....	68
2.5.2	Research methods	69
2.5.3	Analysis.....	73
3	Impact evaluation results	75
3.1	Overview.....	75
3.2	Participant characteristics and flow through the trial	75
3.2.1	Participant flow	75
3.2.2	Participant characteristics at randomisation	77
3.2.3	Baseline distributions of outcome measures	80
3.2.4	Attrition	81
3.2.5	Participant characteristics throughout the trial	82
3.3	Outcome analyses.....	87
3.3.1	Overview	87
3.3.2	Primary analysis	88
3.3.3	Secondary analysis.....	88
3.3.4	Longitudinal analysis	91
3.3.5	Subgroup analyses	94
3.3.6	Analysis in the presence of non-compliance.....	97
3.3.7	Mechanism of change analysis	100
3.3.8	School administrative data analysis	103
3.3.9	Missing data analysis	107
4	Implementation and process evaluation results.....	111
4.1	RQ1: How effectively has the Boys' Development Programme been implemented in schools?	111
4.1.1	Reach.....	111
4.1.2	Dosage	118
4.1.3	Fidelity	121
4.1.4	Quality	123
4.1.5	Responsiveness	124
4.1.6	Intervention differentiation	126

4.2	RQ2: Which factors have acted as enablers of or barriers to implementation of the Boys' Development Programme?	127
4.2.1	Organisational factors.....	127
4.2.2	School and wider community-level factors	129
4.3	RQ3: What are young people's experiences of support?	130
4.3.1	Which aspects of the BDP have supported positive outcomes?.....	131
4.3.2	Perceived outcomes from support.....	132
4.3.3	How have experiences of support differed across subgroups	136
4.4	Lessons from the Boys' Development Programme.....	140
4.4.1	Potential adaptations to the programme.....	140
4.4.2	Implications for future replication and scale of the Boys' Development Programme	141
5	Cost information.....	143
5.1	Overview.....	143
5.2	Pre-requisite costs.....	144
5.3	Set-up and recurring costs	145
5.4	Detailed breakdown of cost estimates	146
5.5	Cost composition and implications	147
6	Conclusion	149
6.1	Key conclusions	149
6.2	Evidence to support the BDP's theory of change.....	149
6.3	Interpretation.....	153
6.3.1	Answering the key research questions.....	153
6.3.2	Policy context and contribution to knowledge.....	155
6.3.3	Considerations to develop the BDP and similar programmes in the future	157
6.4	Limitations and lessons learned	158
6.4.1	Limitations of the evaluation.....	158
6.4.2	Lessons for future evaluations	160
6.4.3	Generalisability of findings.....	162
6.5	Future research and publications	162
7	References	164
8	Appendix A: Effect size estimation.....	169
9	Appendix B: Baseline distributions of outcome measures	170
10	Appendix C: Monitoring data analysis	175
11	Appendix D: Subgroup analysis	181
12	Appendix E: School administrative data sample	184
13	Appendix F: Recruitment documents	184

14	Appendix G: Topic guides	214
15	Appendix H. YEF Security Rating	227

About the evaluator

Cordis Bright was commissioned to undertake an efficacy study randomised controlled trial of Future Men's Boys' Development Programme (BDP), as part of the YEF's and EEF's joint-funded 'A safe, positive place to learn'-themed grant round.

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Abbreviations and definitions

Abbreviations

General abbreviations	
ACME	Average causal mediation effect
ADE	Average direct effect
ANCOVA	Analysis of covariance
BAU	Business as usual
BDP	Boys' Development Programme
CACE	Complier average causal effect
CI	Confidence interval
EAL	English as an additional language
EDI	Equity, diversity and inclusion
EEF	Education Endowment Foundation
EHCP	Education, Health and Care Plan
FSM	Free school meals
FTE	Full-time equivalent
GDPR	General Data Protection Regulation
IPE	Implementation and process evaluation
IT	Information technology
ITT	Intention to treat
IV	Instrumental variable
MAR	Missing at random
MCAR	Missing completely at random
MDES	Minimum detectable effect size
MI	Multiple imputation
MNAR	Missing not at random
ONS	Office for National Statistics
OR	Odds ratio

General abbreviations

REDI	Race equity, equality, diversity and inclusion
RCT	Randomised controlled trial
SAP	Statistical analysis plan
SCQ	School Connectedness Questionnaire
SDQ	Strengths and Difficulties Questionnaire
SEL	Social–emotional learning
SEND	Special educational needs and disabilities
SSRS	Social Support and Rejection Scale
T1	Time 1; baseline before randomisation
T2	Time 2; follow-up at 12 weeks post-randomisation
T3	Time 3; follow-up at 24 weeks post-randomisation
YEF	Youth Endowment Fund

Definitions

- **Boys’ Development Programme (BDP).** The BDP is a targeted, manualised social–emotional learning programme delivered through 12 weekly one-to-one sessions. It aims to support boys at risk of disengagement or exclusion from school by strengthening their emotional regulation, communication, conflict management skills and sense of school belonging. This is the intervention tested in this randomised controlled trial.
- **BDP delivery group.** In this evaluation, the BDP delivery group refers to boys who are allocated to receive support through the BDP.
- **Signposting group.** In this evaluation, the Signposting group refers to boys who are allocated to business-as-usual support. As part of this, Future Men staff introduce boys to an appropriate and trusted school adult (for example, a pastoral lead, SENCo (Special Educational Needs and Disabilities Co-ordinator), or another relevant staff member) who can offer ongoing pastoral or behavioural support that is available within the school’s usual systems.
- **Project co-ordinators.** Project co-ordinators are trained Future Men practitioners responsible for delivering the BDP. They provide one-to-one sessions, liaise with school staff, collect monitoring data and uphold safeguarding and fidelity to the programme model. Each project co-ordinator typically works across two schools per term, delivering to 20 boys.
- **Future Men senior leadership and management team.** The senior leadership and management team provides strategic oversight and management for the programme. This includes the programme’s project manager and Head of Programmes (Boys’ Development team).

- **Future Men team.** The Future Men team includes all operational and delivery staff involved in the programme and evaluation. This includes the project co-ordinators, the project manager, research assistants supporting evaluation data collection and the Head of Programmes overseeing the Boys' Development team.

Note on the use of terms

- **Minority ethnic backgrounds.** Throughout this report, in line with the YEF's guidance, we use the term minority ethnic to describe boys who experience structural disadvantage and systemic inequity due to their ethnicity. We acknowledge that marginalisation is produced by social and institutional structures, rather than being an inherent characteristic of individuals or communities. We recognise that terminology and self-identification vary, and different individuals prefer different descriptors for their racial, cultural and ethnic identities. Where possible, we use the specific ethnic categories provided by participants and school partners, in line with Office for National Statistics standards.
- **Marginalised groups.** Similarly, the term marginalised groups is used to refer to young people who face disadvantage due to socioeconomic, racial, educational or other structural inequities. However, we also recognise that individual preferences vary, and language should always be guided by the preferences of the individual.
- **Boys and young people.** This report uses the terms boys and young people in different sections for clarity and consistency with the evaluation design and literature:
 - Boys is used throughout the evaluation findings, in line with the programme's eligibility criteria and target group (boys aged 11–16). This reflects the intervention's focus and the language used during delivery and recruitment.
 - Young people is used primarily in the literature review and contextual sections, where the evidence base typically refers to mixed-gender or broader adolescent populations.

We emphasise that the young people participating in this study are children aged 11 to 16. Maintaining child-appropriate language is important to avoid adultification, a well-documented issue that disproportionately affects Black boys and can contribute to inequitable disciplinary responses and exclusion practices. Throughout the report, we therefore take care to use language that recognises the participants' developmental stage and avoids implying adult-level agency, responsibility or culpability.

Statistical definitions

- **Analysis of covariance (ANCOVA).** A statistical model used to compare average scores between groups (e.g. BDP delivery vs signposting) while adjusting for their starting scores. It is used when the outcome is continuous (e.g. when comparing subscale scores from the Strengths and Difficulties Questionnaire).
- **Negative binomial regression.** A statistical model used to compare average scores between groups (e.g. BDP delivery vs signposting), controlling for their starting scores. It is used when the outcome is a count (e.g. the number of different types of offences a young person reports), and when the data is more variable than expected.
- **Multiple imputation.** A statistical method used to handle missing data. It uses statistical software to create several complete versions of the dataset by filling in different plausible values based on patterns

in the existing data. These datasets are then analysed separately, and the results are combined to give more accurate and reliable conclusions.

- **Unadjusted mean.** The simple average outcomes score for a group at follow-up, without accounting for any baseline differences.
- **Adjusted mean.** The average score for a group, calculated after accounting for differences at the start of the trial between groups (e.g. baseline scores). This gives a fairer comparison between groups.
- **Hedges' g.** A standardised effect size which measures the size of the difference between groups. It is a version of Cohen's *d*. In line with YEF's technical guidance (2025) for its evaluations, the thresholds used in this report to interpret effect sizes are summarised in Table 1 below.

Table 1: Mapping Hedges' g to the Youth Endowment Fund's (YEF's) impact rating thresholds

Hedges' g or Cohen's d	YEF impact rating
Positive effects	
$-0.02 \leq g < 0.02$	No effect
$0.02 \leq g < 0.10$	Small positive effect
$0.10 \leq g < 0.25$	Moderate positive effect
$g \geq 0.25$	Large positive effect
Negative effects	
$0.00 \geq g > -0.02$	No effect
$-0.02 \geq g > -0.10$	Small negative effect
$-0.10 \geq g > -0.25$	Moderate negative effect
$-0.25 \geq g$	Large negative effect

NB: In this table, the starting point of each given range is part of the relevant effect size, while the end point marks the start of the next interval.

- **Confidence interval (CI).** A range that we are fairly sure contains the true effect. For example, a 95% CI of 0.89 to 1.27 means we are 95% confident that the true result lies between those two numbers. If the range includes 1 (for ratios) or 0 (for effect sizes), the effect is not statistically significant, and we cannot determine if the true effect is different from 0.
- **p-value.** A p-value indicates how compatible the data are with the assumption that there is no true effect. By convention, a p-value below 0.05 (often referred to as statistically significant) suggests that the observed result would be unlikely (i.e. occurring less than 5% of the time) if there were genuinely no difference between groups.
- **T-statistic.** A value that tells us how big the difference is between two groups once we account for how much variation there is in the data. The larger the t-value (in absolute terms), the bigger the difference between groups relative to the noise in the data. If the t-value is large enough (typically above 2), it suggests the difference we see is unlikely to have happened by chance.
- **Incidence rate ratio (IRR).** A number used in models that look at how often something happens (for example, exclusion incidents). It compares the rate of events between two groups. An IRR of 1 means both groups experienced events at the same rate, an IRR below 1 means the event was less common in

the BDP delivery group and an IRR above 1 means it was more common in the BDP delivery group. For instance, an IRR of 0.65 means the BDP delivery group experienced about 35% fewer events than the comparison group.

The project

The Boys' Development Programme (BDP) is a targeted, manualised, school-based, one-to-one social and emotional learning programme. It aims to support boys at risk of disengagement or exclusion from school by developing their social and emotional skills, strengthening their engagement with school (how connected a boy feels to school) and reducing the likelihood of exclusion. It is delivered by Future Men, a Black-led charity based in London. The programme provides 12 weekly face-to-face sessions lasting around an hour each. Sessions are delivered during lesson time in secondary schools and cover topics including rules and boundaries, masculinity, emotion management, conflict management, communication and aspirations. Additional content is tailored to each boy's needs, such as managing emotions, communication or goals and aspirations. Support is delivered by practitioners who received training on delivering the BDP as well as safeguarding and culturally responsive practice. Boys were eligible if they were in Years 7 to 11, in areas of high deprivation and at risk of exclusion or disengagement from school.

The Youth Endowment Fund (YEF) and the Education Endowment Foundation funded a randomised controlled trial to establish whether BDP improved school engagement. It also measured the impact of BDP on behavioural and emotional difficulties, peer relationships and teacher bonding, with additional exploratory analyses on behavioural engagement and pro-social behaviour and analyses of administrative data on attendance and exclusions. In total, 486 boys across eight secondary schools participated in the trial, with 243 receiving the BDP and 243 receiving signposting to other services. The study also included an implementation and process evaluation (IPE), which looked at implementation fidelity, boys' experiences of the support and implications for future scaling. The IPE drew on monitoring data, alongside 28 interviews with boys in the BDP delivery group, four interviews with BDP stakeholders involved in delivering and overseeing the programme, and nine interviews with school staff (including pastoral leads, heads of year and deputy heads). The trial took place in Wandsworth, Southwark and Lambeth between September 2023 and July 2025. Of the 486 boys recruited to the trial, 55% were Black or Black British, 17% were White, 12% were from mixed or multiple ethnic groups, 7% were from another ethnic group and 6% were Asian or Asian British.

Key conclusions

The BDP had a **small positive impact** on boys' school engagement immediately after the programme. Boys who received the BDP had slightly higher levels of school engagement than their counterparts in the control group. This result has a **very high security rating**.

The BDP showed a moderate positive impact on reducing behavioural difficulties and a moderate positive impact on reducing emotional problems. It had a small positive impact on improving teacher bonding and a moderate negative impact on peer difficulties, meaning peer difficulties increased. These are secondary outcomes and should be treated with more caution.

As an exploratory analysis, the evaluation also measured the impact of the programme 12 weeks after the end of the programme. At this time point, the BDP had a moderate positive impact on school engagement, suggesting that improvements in school engagement may have strengthened after the intervention ended.

The BDP reached its intended cohort. The BDP successfully engaged boys from minority ethnic and low-income backgrounds, supported by clear referral and screening processes, relatable practitioners and engaging programme content. A few challenges emerged around referrals for boys whose needs were not clearly suited to the BDP, accessibility for boys with language or learning needs and the ability to reach boys involved with children's social care.

An exploratory analysis suggests that the quality of the relationship between boys and their BDP practitioners (known as project co-ordinators) helped explain later improvements in school engagement. This was supported by qualitative findings, in which boys described project co-ordinators as approachable, trustworthy and easy to talk to.

YEF security rating

These findings have a **very high** security rating. The trial was a well-designed randomised controlled trial, which is. It was a large trial – larger than most previous evaluations of social and emotional learning programmes. Relatively few children (4%) who started the trial were not included in the final analysis.

Interpretation

The BDP had a **small positive impact** on boys' school engagement immediately after the programme. Boys who received the BDP had slightly higher levels of school engagement compared to their counterparts in the control group. This result has a **very high security rating**. Although the size of the impact is small, this is consistent with the programme's relational model. Changes in trust, self-regulation and behaviour may take time to translate into stronger engagement with school. The BDP showed mostly positive results on the secondary outcomes: a moderate positive impact on reducing both behavioural difficulties and emotional problems. It also had a small positive impact on improving teacher bonding. It demonstrated a moderate negative impact on peer difficulties, meaning peer difficulties increased. These are secondary outcomes and should be treated with more caution. There is statistical uncertainty regarding almost all outcomes.

As an exploratory analysis, the evaluation also measured the impact of the programme 12 weeks after the end of the programme and the impact on suspensions and attendance. The BDP had a moderate positive impact on school engagement 12 weeks after the end of the programme, suggesting that improvements in school engagement may have strengthened after the programme ended. The impact of the BDP on secondary outcomes 12 weeks after the end of the programme was mostly positive. The BDP also showed a moderate positive impact the number of exclusions, a large positive impact on the length of suspensions, and a small negative impact on attendance rates. These results were exploratory and the analysis on attendance, suspensions and exclusions was conducted on a sample of five (out of eight) schools and should be treated with caution.

Boys who developed stronger relationships with their BDP practitioner were more likely to show improvements in school engagement. Boys described their BDP practitioners as approachable, non-judgemental and easy to talk to, and valued opportunities to reflect on their emotions, behaviour and relationships. This suggests that trusted relationships were an important part of how the programme worked, which is consistent with the programme's theory of change. These relationships may have helped boys feel more understood and supported and may explain why effects on school engagement were stronger at the later follow-up point, as changes in trust, self-regulation and behaviour may take time to translate to improved school engagement.

The BDP successfully reached and engaged its intended cohort, including high proportions of boys from minority ethnic and low-income backgrounds. This was supported by clear referral and screening processes, relatable practitioners and engaging programme content. Some challenges remained around borderline referrals, accessibility for pupils with language or learning needs, and reaching boys involved with children's social care. This indicates that future delivery may need clearer eligibility guidance for schools, and further support for boys whose needs may make a talking-based intervention harder to access. While the BDP was generally delivered as intended, school withdrawals and staff turnover suggest the need for future research to test whether implementation quality can be maintained if the programme is delivered more widely.

This report, and the primary and secondary outcome findings, only present the findings of one study. When considering implications, frontline professionals, policy makers, and service commissioners, should carefully consider the process evaluation, the wider evidence base, and their own professional judgement.

Summary of impact

Outcome	Effect size (95% confidence interval)	Impact	Evidence security	No. of children	P - value
School engagement	0.04 (-0.14, 0.23)	Small positive		466	0.639

Introduction

This report presents the findings from an efficacy study randomised controlled trial (RCT) and implementation and process evaluation (IPE) of the Future Men's Boys' Development Programme (BDP). The efficacy study was jointly funded by the Youth Endowment Fund (YEF) and Education Endowment Foundation (EEF) and conducted by [Cordis Bright](#).

The BDP is a targeted, manualised, one-to-one social–emotional learning (SEL) programme delivered across 12 weekly 50- to 60-minute-long sessions. It aims to develop the social and emotional capacity and skills of boys in Years 7 to 11 who are at risk of exclusion and disengagement from school to improve school engagement and inclusion and reduce the likelihood of exclusion.

The efficacy study took place across two academic years in eight secondary schools in South London between September 2023 and July 2025.

1 Background

This section sets out the theoretical and scientific background, policy and practice context, and rationale for the study. It explains why school engagement, absence and exclusion matter; who is most affected; how these factors interact; why targeted relationship-based social–emotional approaches provide a plausible response; and why an RCT design was required to support the development of the evidence base.

1.1 Context

The BDP seeks to address the following interrelated contextual challenges within the English secondary school system:

- **A national rise in persistent absence and school exclusions**, particularly since the COVID-19 pandemic, which raised concerns about widespread disengagement from school (Department for Education, 2024; Department for Education, 2025a)
- **Strong associations between school exclusion and later involvement in the criminal justice system**, with emerging evidence suggesting exclusion may contribute causally to youth custody risk (Behavioural Insights Team, 2023; Office for National Statistics [ONS], 2022; Timpson, 2019)
- **Marked disproportionality in absence and exclusion**, particularly affecting boys, pupils from minority ethnic backgrounds and pupils experiencing socioeconomic disadvantage (Department for Education, 2023b; Department for Education, 2025a)
- **Evidence that school engagement, absence and exclusion operate in a reinforcing cycle**, whereby low behavioural participation and weak identification with school increase the likelihood of behavioural incidents and sanctions, and exclusion further weakens engagement on return to school (Finn, 1989; Noltemeyer et al., 2015; Skiba et al., 2014)
- **Growing evidence that relationship-based SEL approaches can strengthen school engagement and behaviour**, though robust UK causal evidence on targeted one-to-one interventions in secondary schools remains limited (Durlak et al., 2011; Gaffney et al., 2021; Taylor et al., 2017; Valdebenito et al., 2019).

The remainder of this section explores each of these contextual drivers in more detail and sets out the rationale for evaluating the BDP within this landscape.

Understanding school engagement

School engagement refers to the degree to which pupils actively participate in school life and feel emotionally connected to their school community. Throughout this report, school engagement is broadly conceptualised in line with Finn's Participation–Identification model (Finn, 1989). This model distinguishes two core and mutually reinforcing dimensions of school engagement:

- **Participation (behavioural engagement).** The extent to which pupils take part in school life and learning activities. This includes observable behaviours, such as attending school, completing classwork and homework, contributing in lessons and complying with classroom routines. Lower participation can be expressed as withdrawal behaviours, including reduced effort, lateness and persistent absence.
- **Identification (affective engagement).** The extent to which pupils feel a sense of belonging and connection to school. This includes feeling accepted and valued, perceiving relationships with teachers and peers as supportive and seeing school as a place where they fit. Lower identification may be reflected in feelings of alienation, low belonging and strained relationships with school staff or peers.

Other influential frameworks conceptualise engagement as three components: behavioural engagement, emotional engagement and cognitive engagement (Fredricks et al., 2004). However, for the purposes of this study, we focus primarily on Finn's two core constructs (behavioural participation and affective identification), which are most directly aligned with the programme's theory of change.

Further details on the links between school engagement, absence and exclusions are presented in section 1.1.4.

1.1.1 National context

England is currently facing sustained challenges with both school attendance and disciplinary exclusion. Persistent absence rates (defined as missing 10% or more of sessions, with a session defined as a half-day of school) rose sharply following the COVID-19 pandemic, increasing from 10.9% in 2018–19 to 24.2% in 2022–23 (Department for Education, 2024). Similarly, suspension and permanent exclusion rates have risen to their highest levels in over a decade. In 2018–19, the overall suspension rate was 5.36 suspensions per 100 pupils. By 2022–23, this had risen to 9.33 per 100 pupils, representing an increase of nearly four suspensions per 100 pupils over five years (Department for Education, 2023a). While permanent exclusions remain comparatively rare in absolute terms (approximately 9,000 pupils in 2023–24), rates remain above pre-2019 levels, rising from 0.10 per 100 pupils in 2018–19 to 0.11 per 100 pupils in 2022–23.

These trends have prompted significant policy attention. In response, the Department for Education has introduced a national attendance strategy, expanded attendance hubs,¹ strengthened expectations on schools regarding daily attendance monitoring and updated statutory guidance on behaviour in schools (Department for Education, 2022). At the same time, debates continue regarding the appropriate balance between maintaining school order and reducing reliance on exclusion as a disciplinary tool. The Timpson Review emphasised that exclusion should be understood not only as a behaviour management response but also as a marker of vulnerability that requires earlier preventative intervention (Timpson, 2019).

Combined, the near doubling of persistent absence rates and the substantial increase in suspensions reflect broader concerns about school disengagement in English secondary schools. Understanding and interrupting this cycle of disengagement, absence and disciplinary escalation is therefore a central priority for both policy and practice.

1.1.2 School absence, exclusion and the risk of offending

Evidence consistently shows a strong association between suspension or exclusion from school and the risk of antisocial or offending behaviour (All-Party Parliamentary Group on Knife Crime, 2019; Farrington, 2020; Hemphill et al., 2006; Rosenbaum, 2020). ONS (2022) reports that 73% of young people aged 23 to 24 with custodial sentences had been excluded from school, compared with just 9% of those without convictions. Similarly, 53% of young people with custodial sentences had been persistently absent from school, i.e. missing a fifth or more of sessions in a single school year, compared with 11% of those without criminal conviction (ONS, 2022). These figures demonstrate strong associations but do not, on their own, establish causality.

To strengthen causal inference, the Behavioural Insights Team (2023) used linked administrative data and an instrumental variable (IV) approach to estimate the impact of exclusionary sanctions on later youth custody. This study provides evidence consistent with a causal contribution of exclusion to later custody risk, although estimates depend on modelling assumptions and should be interpreted cautiously. Other longitudinal studies also suggest that exclusion can trigger or exacerbate a cycle of disengagement and risk, even after accounting for pre-existing risk factors (Hemphill et al., 2006; Rosenbaum, 2020; Valdebenito et al., 2019). Qualitative and policy literature highlights plausible mechanisms by which exclusion may cause future offending, including reduced access to support, disrupted routines and increased exposure to negative peer influences (House of Commons Education Committee, 2018; Just for Kids, 2020).

The Timpson Review of School Exclusion showed that exclusion and poor educational outcomes signal vulnerability to youth violence and social harm (Timpson, 2019). It called for stronger preventative support for pupils at risk of disengagement and exclusion, including the design and evaluation of interventions that reduce pathways into exclusion (Timpson, 2019; Valdebenito et al., 2019). The Department for Education's Working Together to Improve School Attendance guidance reaffirmed the importance of early and relational approaches to supporting engagement in school (Department for Education, 2022).

¹ Attendance hubs are part of the Department for Education's national attendance strategy. They are partnerships between schools, led by high-performing hub schools, designed to share effective practice and provide peer support to improve pupil attendance. Hub schools work with partner schools to strengthen systems for monitoring attendance, early intervention and engagement with families, with the aim of reducing persistent absence (Department for Education, 2022; 2023a).

1.1.3 Disproportionality in absence and exclusion

Persistent absence, suspension and exclusion do not affect pupils evenly. National statistics show marked disparities by sex, ethnicity and socioeconomic disadvantage (Department for Education, 2023b; Department for Education, 2025a). These disparities often intersect, creating compounded risks for some groups (Graham et al., 2019; Timpson, 2019):

- **Sex.** Boys account for a clear majority of suspensions and permanent exclusions in secondary schools. In the 2023–24 academic year in England, the suspension rate for boys was 1.5 times the suspension rate for girls (at 13.98 and 8.58 suspensions per hundred pupils for each group, respectively²) (Department for Education, 2025a). Furthermore, the permanent exclusion rate for boys was more than twice the rate for girls in the same period, at 0.18 and 0.08 respectively. This aligns with wider studies, which suggest that boys are more likely than girls to display behavioural difficulties, including aggression and classroom disruption, which are strongly associated with disciplinary sanctions (Paget et al., 2018). These behavioural patterns do not occur in isolation. In addition, gender norms may shape how boys express distress, respond to authority and seek support, while teacher interpretations of behaviour may also be influenced by gendered expectations (Kessels et al., 2018; Pustjens et al., 2009; Skelton, 2010). As a result, boys may be more likely to enter disciplinary pathways that culminate in suspension or exclusion.
- **Ethnicity.** Department for Education statistics show marked disparities in suspension and permanent exclusion rates based on ethnicity. In the 2022–23 academic year, the overall suspension rate in England was 9.33 suspensions per 100 pupils. However, rates were substantially higher for certain minority ethnic groups: 18.09 per 100 for Black Caribbean pupils and 17.42 per 100 for pupils from mixed White and Black Caribbean backgrounds, almost double the national average. Permanent exclusion rates show a similar pattern: The overall permanent exclusion rate was 0.11 per 100 pupils, compared with 0.37 per 100 for Black Caribbean pupils and 0.30 per 100 for mixed White and Black Caribbean pupils (Department for Education, 2023b).

Research suggests these disparities are not fully explained by socioeconomic status or prior attainment and may reflect structural inequalities, differential disciplinary practices and racialised perceptions of behaviour (Demie, 2021; Gaffney et al., 2021; Wallace & Joseph-Salisbury, 2022). For example, researchers report that Black Caribbean boys face disproportionate disciplinary intervention, even when presenting similar behaviours to peers (Graham et al., 2019). These dynamics suggest that exclusion reflects both pupil behaviour and institutional response and may intensify disengagement by weakening identification with school.

- **Low-income backgrounds.** Pupils eligible for free school meals (FSM) experience substantially higher rates of exclusion and persistent absence than their peers. In 2022–23, persistent absence rates were notably higher among pupils eligible for FSM (39.3%) than the average (24.2%). In 2023–24, FSM-eligible pupils were more than four times as likely to be suspended and around five times as likely to be permanently excluded as pupils who were not FSM-eligible (Department for Education, 2025a). Timpson (2019) outlined that children who are eligible for FSM are around 40% more likely to be

² Suspension rates are calculated as the total number of suspensions divided by the total number of pupils multiplied by one hundred.

permanently excluded than those who are not, after controlling for other differences. Research suggests that deprivation may impact school engagement through multiple drivers, including (1) overlapping adversity, such as family unemployment, ill health or homelessness, (2) financial constraints on participation in school life, which can in turn lead to social isolation and the risk of bullying and (3) unequal dynamics in parent–school relationships (Graham et al., 2019).

Disparities in absence and exclusion rarely operate along a single dimension, and it is important to consider how they intersect. While national statistics are not available for intersecting categories, studies suggest that sex, ethnicity and socioeconomic disadvantage frequently interact, producing compounded risks. For example, Timpson (2019) found that the majority of permanently excluded pupils experience at least one additional form of vulnerability, and many experience several simultaneously. When characteristics such as being male, belonging to a minority ethnic background and experiencing socioeconomic disadvantage combine, the likelihood of suspension, persistent absence and exclusion increases substantially. Understanding these intersecting patterns strengthens the case for targeted preventative interventions that address both skills and belonging, particularly for boys facing overlapping structural disadvantage (Graham et al., 2019; Timpson, 2019).

1.1.4 Links between school engagement, absence and exclusion

A substantial evidence base indicates that school engagement, absence from school and exclusion are closely interrelated, often forming a mutually reinforcing cycle over time. Defining school engagement as encompassing both behavioural participation and affective identification with school (Finn, 1989), research suggests that absence and exclusion can both result from low school engagement and act as mechanisms of withdrawal that further weaken connection to school. This operates through several pathways:

- **School engagement as a precursor to attendance and behavioural outcomes.** Empirically, low school engagement is linked to absenteeism, behaviour difficulties and eventual withdrawal from school (Klem & Connell, 2004). Conversely, longitudinal evidence suggests that higher rates of school engagement predict stronger school-related outcomes and lower truancy over time, supporting the direction of effect from engagement to attendance behaviours (Virtanen et al., 2021).
- **Attendance as an indicator and amplifier of disengagement from school.** Attendance is often treated as a behavioural indicator of low school engagement; however, evidence suggests that persistent absence can also worsen engagement over time. Research shows that increased absence is associated with poorer attainment and adverse outcomes, and the Department for Education (2025b) reports a clear association between attendance and attainment in England. In addition, chronic absenteeism has been linked to reduced school belonging, weaker peer and teacher relationships and declining motivation, suggesting that absence can erode affective identification as well as behavioural participation. In this way, poor attendance may not only reflect low engagement but may further weaken connection to school, increasing the likelihood of continued disengagement. Department for Education statutory guidance emphasises that improving attendance requires building relationships and strengthening school connectedness (Department for Education, 2022), reinforcing the centrality of engagement mechanisms.
- **Exclusion as a driver of school disengagement.** Research supports a developmental pathway linking low school engagement to behavioural difficulties and then to suspension and exclusion (Li & Lerner, 2011; Skiba et al., 2014). However, evidence also indicates that exclusionary sanctions can further

diminish school engagement upon return to school. Longitudinal studies show that suspension predicts subsequent declines in academic engagement and school connectedness, even after accounting for prior behaviour (Hemphill et al., 2006; Noltemeyer et al., 2015). Students who experience suspension report lower belonging, weaker relationships with teachers and reduced commitment to school following exclusion (Arcia, 2006). In the UK context, Timpson (2019) highlighted concerns that exclusion can entrench trajectories of disengagement where reintegration support is limited. Exclusion therefore appears not only as an outcome of low engagement but also as a mechanism that may further weaken both participation and identification with school.

In addition, criminological research also suggests that engagement with school may function as a protective factor against involvement in violence (Boxford, 2013). For example, analyses from the Pittsburgh Youth Study found that boys who reported a more positive attitude towards school in early adolescence were significantly less likely to be convicted of violent offences later in adolescence (Jolliffe et al., 2016).

This supports a plausible bidirectional and potentially self-reinforcing cycle, where low school engagement leads to increased behavioural difficulties and withdrawal from school, resulting in an increased risk of absence, suspension and exclusion. In turn, increased rates of absence, suspension and exclusion weaken attachment and participation in school, further reducing levels of school engagement. This cyclical pattern strengthens the rationale for interventions that aim to improve school engagement as a means of reducing the risk of suspension and exclusion and ultimately reducing the risk of offending and involvement in youth violence.

1.1.5 Evidence that social–emotional approaches improve school engagement

SEL programmes aim to strengthen emotional regulation, communication, problem solving and relationship building. These competencies plausibly influence both behavioural engagement and affective engagement with school and may also reduce behaviours that lead to disciplinary sanctions (Durlak et al., 2011).

School disengagement and exclusion reflect influences across multiple ecological levels, including family circumstances, peer networks, school climate, disciplinary practices and wider structural inequalities (Bronfenbrenner, 1979). However, interventions differ in the level at which they aim to effect change. The BDP primarily operates at the individual and relational levels through structured one-to-one sessions delivered in schools. As such, this section focuses on social–emotional competencies and relational processes that align closely with the BDP programme model.

An international evidence base supports the effects of SEL programmes on social–emotional skills, behaviour and attitudes towards school. In a meta-analysis of 213 SEL programmes, Durlak et al. (2011) reported statistically significant improvements across social–emotional skills, attitudes, behaviour and academic outcomes. In a follow-up meta-analysis, Taylor et al. (2017) found that many SEL benefits persisted at follow-up (six months or longer), suggesting that well-implemented programmes can produce durable change. However, these reviews largely examine universal provision, and many studies do not focus specifically on exclusion outcomes or targeted high-risk secondary cohorts.

Evidence more directly related to exclusion and suspension remains limited but emerging. Reviews highlight that interventions combining relationship-based support with individual skill development (including mentoring and counselling elements) show promise for reducing exclusion risk, although effect sizes tend to be modest and sensitive to implementation quality (Gaffney et al., 2021; Valdebenito et al.,

2019). In a systematic review, Valdebenito et al. (2019) noted stronger effects for approaches that increase self-control, include one-to-one support and address counselling or mental health alongside broader academic or behavioural needs. More recent evidence reinforces the limited and mixed effectiveness of interventions targeting school exclusion. An updated systematic review by Valdebenito et al. (2025) similarly found that while some programmes show promise in reducing exclusion-related outcomes, the overall effects are modest and appear to have weakened when compared with earlier reviews. The review also found limited evidence of spillover effects on wider outcomes, such as offending. However, most of the evaluated programmes were implemented in the United States, highlighting the continued need for rigorous evaluations in other contexts.

Overall, a plausible causal pathway links SEL to school engagement and exclusion-related outcomes:

1. SEL approaches aim to strengthen self-regulation, communication and relational skills (Durlak et al., 2011; Taylor et al., 2017).
2. Improved communication skills improve relationships with peers and teachers, and self-regulation promotes active participation in school life, thereby improving school engagement (Finn, 1989; Klem & Connell, 2004).
3. Improved school engagement reduces behavioural difficulties and persistent absence from school, lowering the likelihood of suspension and exclusion (Skiba et al., 2014; Timpson, 2019).

While this pathway is supported by indirect evidence, UK causal evidence on targeted one-to-one SEL approaches in secondary schools remains limited (Gaffney et al., 2021). This evidence gap supports the rationale for a rigorous RCT of the BDP.

1.2 Rationale for the Boys' Development Programme

The BDP is a targeted, evidence-based intervention for boys at risk of disengagement or exclusion from school. It aims to strengthen boys' social and emotional competencies, build positive relationships and promote school engagement through 12 structured one-to-one sessions focusing on emotional regulation, communication, conflict management, masculinity and goal setting.

Each session is designed to strengthen protective factors and mitigate known risks for school disengagement and exclusion. The BDP draws conceptually from established SEL and mentoring models (e.g. Durlak et al., 2011; Valdebenito et al., 2019). Its one-to-one format and emphasis on a consistent, trusted adult relationship align with reviews suggesting that mentoring and counselling elements commonly feature in more promising approaches for pupils at heightened risk of exclusion (Gaffney et al., 2021; Valdebenito et al., 2019).

The BDP aims to build several protective factors against the risks of school disengagement, absence and exclusion (Bowman-Perrott et al., 2013; Graham et al., 2019; Hawkins et al., 2000; Timpson, 2019). These include:

- Strengthening emotional literacy and self-regulation, thereby reducing impulsive or conflict-driven behaviour
- Improving communication and conflict management skills to support more constructive peer and teacher relationships

- Promoting positive identity development and aspirations to increase identification with school and future-oriented motivation
- Providing a consistent, supportive adult relationship within the school context

A distinctive feature of the BDP is its **explicit focus on masculinity** as a contextual factor shaping boys' engagement and behaviour. Evidence suggests that traditional or restrictive masculine norms may discourage emotional expression, empathy, help-seeking and pro-social behaviour and may be associated with increased externalising behaviours and conflict (Harland et al., 2005; Harland, 2008; Kilmartin, 1994; Hong, 2000; Logoz et al., 2023; McCready, 2005). Gender norms may therefore exacerbate established social-emotional risk factors for school disengagement, including poor self-regulation, reactive aggression and strained relationships with authority figures. These dynamics provide a theoretical rationale for a gender-responsive intervention that explicitly addresses identity alongside skills development. The BDP aims to support boys in developing more adaptive, emotionally literate and relationally constructive ways of understanding themselves and interacting with others by creating space to critically reflect on masculinity, identity (including the roles of ethnicity, culture, social class and community) and peer influence.

In addition to its gender-responsive focus, the BDP is designed to be **culturally responsive** to the diverse backgrounds of boys participating in the programme. The programme operates with an explicit awareness of the structural and systemic contexts shaping boys from minority ethnic backgrounds' experiences of school discipline and engagement. Project co-ordinators receive training in the wider context and on delivering culturally responsive practice. In addition, the team includes practitioners with relevant lived and professional experience, including male staff and staff from minority ethnic backgrounds, to provide visible role models for boys.

The BDP's one-to-one format enables project co-ordinators to tailor discussions to each boy's lived experience, including their cultural background and how this contributes to their identity and conception of masculinity, rather than applying a uniform or prescriptive script. While the programme is manualised, it allows flexibility in the use of language, contextual examples and discussion to ensure relevance and encourage authenticity. In addition, project co-ordinators are encouraged to adopt a strengths-based and high-expectations approach, avoiding deficit narratives and modelling growth-oriented beliefs about boys' capacity for change. The emphasis on relational trust, dignity and reflective dialogue is intended to reduce the risk of further marginalisation or escalation through overly punitive responses. In this way, the programme seeks to improve individual social-emotional skills through relational and identity-based dimensions of school engagement, particularly for boys who may experience compounded structural disadvantage.

Through this combined focus on self-regulation, communication, relational trust and identity development, the BDP seeks to influence both dimensions of school engagement outlined earlier, ensuring relevance and accessibility for the cohort of boys it seeks to support. It aims to strengthen behavioural participation by improving classroom functioning and reducing behavioural escalation and to strengthen affective identification by enhancing belonging, trust and positive relationships with teachers and peers.

Table 2 summarises the risks and protective factors addressed by each core session topic. Further information on the BDP is provided in section 2.4.

Table 2: Risk factors and protective factors addressed by each Boys' Development Programme (BDP) session topic

Risk factor	Protective factor(s)	BDP core sessions
Difficulty managing emotions	- Increasing emotional knowledge and regulation - Developing an emotional language to identify feelings other than anger - Understanding and identifying physical responses associated with feelings - Practising self-regulating techniques	Managing emotions Conflict management
Poor communication skills	- Developing enhanced communication skills - Understanding the importance of communication - Practising communicating feelings and thoughts	Managing emotions Communication Conflict management Masculinity Goals and aspirations
Conflict with peers and adults	- Understanding personal responses in conflict situations - Developing non-violent strategies for communicating through conflict - Understanding adult and own roles and responsibilities	Managing emotions Communication Conflict management Masculinity
Association with deviant peer groups	- Improving social problem-solving skills - Identifying the costs and benefits of association with antisocial peers - Clarifying personal values and ethics - Practising strategies for non-engagement with antisocial peers or behaviour.	Masculinity Communication Conflict management

1.3 Rationale for a randomised controlled trial evaluation of the Boys' Development Programme

Although there is a substantial international evidence base supporting SEL approaches, significant uncertainties remain regarding their effectiveness in reducing suspension and exclusion in UK secondary school settings, particularly among pupils facing intersecting disadvantage (Gaffney et al., 2021).

As outlined above, meta-analyses demonstrate that SEL interventions can improve social–emotional skills, attitudes towards school and behavioural outcomes (Durlak et al., 2011; Taylor et al., 2017). However, relatively few studies have examined school engagement, exclusion or suspension, and evidence of sustained long-term reductions in exclusion is limited (Valdebenito et al., 2019). Moreover, many studies have been conducted in universal primary school populations or outside the UK.

Within the UK, evidence on targeted interventions to reduce exclusion among secondary-aged boys, particularly those from minority ethnic or socioeconomically disadvantaged backgrounds, remains limited. Reviews have highlighted both the scale of the exclusion challenge and the need for robust evaluation of preventative, relationship-based approaches (Gaffney et al., 2021; Graham et al., 2019).

This efficacy trial therefore tests whether allocation to the BDP improves school engagement and related outcomes among boys identified as being at risk of disengagement or exclusion. The evaluation is designed to provide robust causal evidence on whether a manualised, relationship-centred, one-to-one SEL programme improves school engagement and related behavioural and exclusion-related outcomes beyond business-as-usual (BAU) support.

1.4 About the Boys' Development Programme

This section describes the BDP and sets out the BDP's theory of change.

1.4.1 Theory of change

Table 3 presents the BDP's theory of change, formatted in line with the Early Intervention Foundation's [10 Steps for Evaluation Success](#). This was co-developed by Future Men, the YEF and Cordis Bright as part of the study's co-design phase, based on a document review, co-design workshops and a rapid review of the evidence base.

Table 3: Boys' Development Programme's (BDP's) theory of change

Why?		Who?	How?	What? Outcomes		
Context	Evidence	Participants	Intervention	Short term	Medium term	Long term
The BDP was developed in response to evidence that showed: a) Low school engagement, exclusion from school and a commitment to a deviant peer group can lead to increased risk of involvement in youth violence and crime. b) Exclusion and disengagement from secondary school disproportionately affects young people from minority ethnic backgrounds and those from low-income households. c) Social-emotional approaches can improve young people's educational outcomes.	Risk factors associated with low school engagement and exclusion from school are: • Poor communication skills • Difficulty managing emotions • Rejection by non-deviant peer group • Conflict with peers and teachers Protective factors are: • Raising aspirations • Improving relationships with peers and adults • Improving conflict management and communication skills • Developing a positive attitude to learning • Improving self-awareness and self-regulation • Developing problem-solving and decision-making skills • Improving self-esteem, empathy and cooperation	Boys in Years 7–11 (aged 11–16) in areas of high need/deprivation who are identified as being at high risk of disengagement or exclusion from school. Exclusion criteria are: • Behaviour and circumstances indicate that clinical support would be more appropriate. • The school intends to exclude the pupil and is using the programme to demonstrate that support was offered. • The boy has more advanced learning or communication needs which would make a talking intervention hard to access.	A minimum dosage of 12 one-hour weekly 1:1 sessions that take place over a 12-week period. Sessions are tailored to boys' needs, but topics can include: • Masculinity/boys' development • Emotions • Communication (including social media) • Conflict (including peer interactions and pressure) • Goals and aspirations • Home and school expectations The sessions focus on developing protective factors which strengthen young people's personal characteristics and mitigate any potential negative peer-group influence. These sessions aim to give boys a positive relationship with a trusted adult with whom they can identify and model positive behaviour. This is the key mechanism of change.	Boys have: • Strategies for managing feelings and behaviours • Strategies for dealing with conflict situations • Improved recognition of their own emotions • Improved understanding of communication strategies • Improved understanding of male development • Improved understanding of how communication impacts relationships	Boys: • Have stronger engagement with school • Remain in school • Reach their potential at school • Get along better with their peers • Get along better with their teachers • Have improved emotional regulation • Have reduced misbehaviour and aggression • Have higher aspirations for the future	Boys: • Have improved educational engagement • Have reduced exclusions • Experience a reduction in opportunities for engagement in antisocial behaviour/ crime • Thrive and progress in school and beyond • Have improved future opportunities and life chances

1.4.2 Who does the Boys' Development Programme aim to work with?

The BDP's theory of change shows that the programme's target cohort is boys in Years 7 to 11 (aged 11 to 16) in areas of high need and deprivation who are identified as being at high risk of disengagement and exclusion from school.

Within this, the project team typically expected most boys to be from minority ethnic backgrounds and many to be from low-income households. This expectation reflects well-documented disproportionality within the education system, whereby boys from certain minority ethnic backgrounds, particularly Black boys, and boys from socioeconomically disadvantaged households are overrepresented in exclusion statistics and indicators of school disengagement. The programme's targeting criteria therefore align with patterns of structural inequity in risk exposure, rather than being based on ethnicity itself.

Inclusion criteria for the programme are:

- a) **Boys at risk of exclusion (due to poor behaviour records and/or significant behaviour incidents).** This is assessed by incidents of poor behaviour in individual behaviour logs – for example, incidents in the classroom or around the school and/or situations involving teachers and/or other pupils.
- b) **Boys at risk of disengagement (indicated by achieving below their potential and/or poor attendance).** This is assessed by school attendance rates as a proxy for engagement.
- c) **Boys at risk of poor outcomes due to known adversity outside school (indicated by child poverty and previous or ongoing child-protection concerns).** This is assessed by socioeconomic information collected by schools, such as eligibility for FSMs, care status or English as an additional language (EAL).

Exclusion criteria are:

- a) The boy's behaviour and circumstances indicate that clinical support would be more appropriate – for example, where there is evidence of significant trauma for which no prior support has been received.
- b) The school intends to exclude the boy and is using the programme to demonstrate that support was offered. This exclusion criterion is ensured by providing explicit guidance to schools that boys in this situation should not be referred into the programme.
- c) The boy has a more advanced learning or communication need which would make a speaking and listening intervention hard to access. However, boys with special educational needs and disabilities (SEND) or an Education, Health and Care Plan (EHCP) are not automatically excluded from the programme.

Boys do not need to meet all the inclusion criteria above to be accepted into the BDP.

As part of the referral process, Future Men receive anonymised referrals from schools based on the referral guidance that schools use for their initial referrals. These are outlined in Table 4. Following receipt of anonymised referrals, the Future Men team meet with school staff to discuss each referral on a case-by-case basis to ensure they are appropriate, using the inclusion and exclusion criteria and their professional judgement to guide these discussions. Once referrals are agreed to be appropriate, informed consent is then collected from parents/carers before non-anonymised referrals are made to

the BDP. This process ensures that all referrals into the BDP are in line with the inclusion and exclusion criteria outlined below.

Table 4: Boys' Development Programme eligibility guidance for schools

Eligibility	Eligibility guidance for schools
Not eligible: Needs are <u>above</u> eligibility thresholds	Language or learning needs that require specialist intervention
	Behaviour stemming from trauma or neurodiversity that requires therapeutic intervention
	Pattern of violent behaviour where the risk to staff cannot be moderated
	Exclusion process has already started or is likely to start soon
Eligible: Needs <u>meet</u> eligibility thresholds	Low attendance approaching levels of persistent absence (90%)
	Low educational attainment resulting from behavioural, communication or relationship challenges in or out of school
	Behavioural challenges or patterns of behaviour that place the student at risk of exclusion without intervention
	Any other factor that places the young person at potential risk of exclusion that could be prevented by this intervention
	Low-level behaviour that affects engagement with school
Not eligible: Needs are <u>below</u> eligibility thresholds	Limited, infrequent behaviour issues that fall within the school's disciplinary framework
	Attainment below the school's expectation but not linked to wider concerns
	Isolated incidents or comments expressing misogynistic views

1.4.3 How does the Boys' Development Programme work with boys?

Table 5 describes the BDP in line with the Template for Intervention Description and Replication framework (Hoffman et al. 2014).

Table 5: About the Boys' Development Programme

TiDieR item	Description
Brief name	Future Men's Boys' Development Programme (BDP)
Why?	The BDP was developed to address: a) Evidence that low school engagement, exclusion from school and a commitment to an antisocial peer group can lead to antisocial behaviour, weapon-carrying and crime b) Evidence that targeted social-emotional approaches can improve young people's educational outcomes c) Evidence that exclusion and disengagement from secondary school disproportionately affects Black Caribbean and dual heritage Caribbean boys

TiDieR item	Description
Who delivers?	Support is delivered by three full-time BDP project co-ordinators. The project co-ordinators are overseen by a BDP project manager. Each project co-ordinator has prior experience of working with boys and young men and receives in-house training on approaches to delivering the BDP and the BDP toolkit. They also receive mandatory safeguarding training during induction. Project co-ordinators receive training in culturally responsive practice, including awareness of disproportionality in exclusion rates and the wider structural context shaping boys' school experiences. The delivery team includes male project co-ordinators and project co-ordinators from minority ethnic backgrounds, and staff bring relevant lived and professional experience to the role. Each project co-ordinator works across two secondary schools at a time, supporting 10 boys per term per school, i.e. 80 boys over the course of the trial. For the purposes of the study, two evaluation research assistants were recruited and managed by the Future Men team. They supported evaluation data collection (both primary and secondary data) including at baseline, T2 and T3. This role also supported collecting informed consent and conducting the randomisation process.
What is delivered?	The BDP is a targeted, manualised, social and emotional learning programme. It aims to develop the social and emotional capacity and skills of boys in Years 7–11 who are at risk of exclusion and disengagement from school to improve their educational outcomes, including improving school engagement and reducing the likelihood of exclusion. The programme is delivered in line with the BDP manual, which was developed in collaboration between Future Men and Cordis Bright. The sessions focus on developing protective factors and addressing risk factors to strengthen boys' personal characteristics and mitigate potential negative peer-group influence. Sessions are tailored to boys' needs but typically cover the following six core topics: • Introduction meeting (rules and boundaries) • Masculinity • Managing emotions • Conflict management • Communication • Goals and aspirations There is a core session delivered on each of the six core topics and additional supplementary sessions to choose from on each of these topics. All boys should cover the core topics in each session, and the focus of the remaining six sessions can be tailored and adapted for each young person. While BDP is delivered within a manualised framework, project co-ordinators are encouraged to adapt the language, contextual examples and discussion prompts to reflect participants' lived experiences and cultural backgrounds, supporting authenticity and relevance.

TiDieR item	Description
	Each session broadly takes the following format: 1. Check-in 2. Target review (i.e. revisiting goals set in the previous session) 3. Self-assessment exercise on wellbeing, behaviour, schoolwork (effort), conflict/communication and coping 4. Optional warm-up game 5. Main activity, which is tailored and utilises a flexible range of core learning material 6. Open questions using question cards and prompts 7. Fun checkout activity that builds rapport and ends every session on a positive note (for example, rock-paper-scissors or a preferred activity) 8. Target setting for the next session The sessions aim to give boys access to a positive relationship with a trusted adult with whom they can identify and who can model constructive behaviour and high expectations, providing visible role modelling within the school context. This relationship is the key mechanism of change and is intended to lead to improved school engagement. Further information about what was delivered is provided in the implementation and process evaluation (IPE) results in Chapter 5.
When and how much?	The programme model consists of 12 weekly one-to-one sessions (covering a full school term) each lasting around 50–60 minutes. Where feasible within the school term, boys who miss sessions are offered additional sessions to support them to catch up and receive the intended dosage. For this evaluation, support was delivered to four cohorts over two academic years, i.e. from September 2023 to July 2025.
How?	Support is delivered face-to-face on an individual basis.
Where?	Support is delivered in secondary schools during lesson times and is typically scheduled on a rotating basis to ensure the same lesson is not missed systematically. Sessions took place across eight secondary schools in Wandsworth, Southwark and Lambeth.
Tailoring?	Sessions are tailored to individual needs based on assessment and discussion during the introduction meeting, including consideration of the young person's identity, lived experience and school context. While core topics are consistent, project co-ordinators exercise structured flexibility in delivery to ensure cultural relevance and responsiveness.
How well?	Fidelity to the BDP throughout the evaluation was assessed against the programme's theory of change and documented approach. This assessment used monitoring data and was conducted as part of the IPE.

Note: TiDieR = Template for Intervention Description and Replication; T2 = Time 2; follow-up at 12 weeks post-randomisation; T3 = Time 3; follow-up at 24 weeks post-randomisation

1.4.4 What does the Boys' Development Programme aim to achieve?

As shown in the BDP's theory of change, the BDP aims to achieve the following outcomes:

Short-term outcomes

The short-term outcomes of the BDP are that boys have:

- Strategies for managing feelings and behaviours
- Strategies for managing conflict situations
- Improved recognition of their own emotions and how they interact with thoughts and behaviours
- Improved understanding of communication strategies
- Improved understanding of male development
- Improved understanding of how communication impacts relationships

Medium-term outcomes

The medium-term outcomes of the BDP are that boys:

- Have stronger engagement with/bonds to school
- Remain in school
- Reach their potential at school
- Get along better with their peers
- Get along better with their teachers
- Have improved emotional regulation
- Have reduced misbehaviour and aggression
- Have higher aspirations for the future

Long-term outcomes

The long-term outcomes of the BDP are:

- Improved educational engagement
- Reduced exclusions
- Reduced opportunities for engagement in antisocial behaviour/crime
- Boys thrive and progress through school and beyond
- Improved future opportunities and life changes

Please note that timescales for outcomes were not specified in the initial theory of change because, while the evaluation captures outcomes at baseline, 12 weeks and 24 weeks post-randomisation, the wider timeframe over which changes in attitudes, relationships and behaviours might plausibly occur was uncertain and may vary across boys and contexts. The potential sequencing and timing of these changes are discussed further in the conclusion, along with recommendations for future theory development.

Implementation proceeded largely as planned across the course of the study. However, a small number of contextual challenges were encountered. Two schools withdrew from the study following Year 1: one due to wider capacity constraints and one because the school was unable to identify a sufficient number of eligible boys to sustain delivery. In addition, staff turnover affected delivery in two schools for one term, resulting in 10 boys working with three different project co-ordinators over the course of the programme and another set of 10 boys working with two project-co-ordinators.

Aside from these instances, schools were recruited as planned, participant take-up was high and the BDP was broadly delivered in line with the manual and intended dosage. Further detail on recruitment, delivery and fidelity is presented in the IPE results in Chapter 4.

1.5 Evaluation objectives

1.5.1 Impact evaluation

In line with the [efficacy study protocol](#) and the [efficacy study statistical analysis plan \(SAP\)](#), the primary research question for the efficacy study was:

Does a targeted social–emotional learning programme for boys at risk of disengagement and exclusion improve school engagement in comparison to business as usual?

The primary time point is Time 2: follow-up at 12 weeks post-randomisation (T2) or immediately after programme completion.

Additional secondary research questions were:

1. **Delivery.** Can the BDP work under ideal circumstances?
2. **Impact.** a) What is the impact of the BDP? b) Do different subgroups of young people have different outcomes, e.g. those from minoritised/marginalised groups?
3. **Unintended consequences.** a) Does the BDP have any unintentional consequences? If so, what are these? b) Do different groups of young people experience these differently?
4. **Iatrogenic effects.** Are there any serious negative effects attributed to the BDP on any outcomes?
5. **Mechanisms.** Which factors contribute most to observed outcomes?

1.5.2 Implementation and process evaluation

The IPE was designed in line with the YEF's guidance on feasibility studies and IPEs and the EEF's guidance on IPEs.

The primary objectives of the IPE were to:

- Understand the association between aspects of the BDP's implementation and successful outcomes
- Gather data to support guidelines for successful implementation of the BDP

IPE research questions were as follows:

1. Dimensions of implementation. How effectively has the BDP been implemented in schools?

- Fidelity.** To what extent has support been delivered in line with the BDP's theory of change and protocols?
- Dosage.** How much of the BDP has been delivered? How much of the BDP needs to be delivered to have an impact?
- Quality.** How well has the BDP been delivered?
- Reach.** How well has the BDP reached its intended cohort?
- Responsiveness.** To what extent have young people engaged with the intervention?
- Intervention differentiation.** To what extent is the BDP sufficiently different from existing practices within schools?
- Adaptation.** Are any changes needed to accommodate context and need?

2. Factors affecting implementation. Which factors have acted as enablers of or barriers to implementation of the BDP?

- Community-level factors.** Which factors have impacted implementation at the school and wider community levels? For example, level of need, readiness for change and/or policy, practice and funding context?
- Organisation-level factors.** Which factors have impacted implementation at the organisational level? For example, capacity, skills and training, co-ordination, and resources?
- Unexpected factors.** Which other factors have had an impact?

3. Experiences of support. What are young people's experiences of support?

- Which aspects of the BDP have supported positive outcomes?
- How have experiences of support differed across subgroups, e.g. those from racially minoritised/marginalised backgrounds, from low income households or with SEND?

4. Guidelines for future implementation. What are the implications for future replication, scale and spread?

1.6 Ethics and trial registration

Ethical approval was granted for the study by the Royal Holloway University Research Ethics Committee under reference: REC/3756. This involved the submission of a detailed application, which was subject to

review and scrutiny from YEF and Future Men colleagues. The trial was publicly registered on the ISRCTN registry once delivery commenced in September 2023, with registration number ISRCTN12897947 (available [here](#)).

Information on how agreement to participate in the trial is provided in section 2.3, and copies of recruitment documentation are provided in Appendix F.

1.7 Data protection

Cordis Bright was the controller of personal data throughout the evaluation, as specified in YEF data guidance (available [here](#)). We delivered the evaluation in line with our [Data Protection and Information Governance Policy](#), which sets out our approach to storing and handling personal data for the evaluation. Cordis Bright is also registered under the Data Protection Act, has Cyber Essentials Plus accreditation and is registered under the National Health Service (NHS) Data Security and Protection Toolkit.

We conducted a data protection impact assessment and developed a signed data-sharing agreement with Future Men. The processing of personal data was conducted under Article 6(1)(e) of the UK General Data Protection Regulation (GDPR), which permits processing for tasks in the public interest (i.e. conducting research and evaluating youth justice interventions). Where special category data (e.g. ethnicity data) were processed, this was justified under Article 9(2)(j) of the UK GDPR, which allows processing for research purposes where safeguards (e.g. anonymisation and/or pseudonymisation) are in place.

This legal basis was appropriate because the evaluation aims to generate evidence that informs policy and practice, aligning with Cordis Bright's legitimate interest in conducting high-quality research and the YEF's public-interest role in funding evidence-based interventions.

For this evaluation, we implemented:

- **A clear legal reason** for sharing data with us, e.g. public interest/public task
- **A robust process to transfer data.** Future Men transferred all data by secure methods using Switch Egress. Monitoring data were obtained from Future Men's case management system. All questionnaires were completed anonymously using an anonymous ID number online (i.e. Smart Survey). Only the evaluation team had access to the responses.
- **Secure storage of data.** Data were saved on Cordis Bright's secure cloud-based Microsoft 365 servers using the unique ID number. Personal or sensitive data had additional encryption, with access restricted to a designated/authorised member of the evaluation team. Participants were informed that all information about them was stored in this way (see Appendix F). Personal data were stored separately from questionnaire data.
- **Secure survey software.** Outcome measures were collected via SmartSurvey. This stores all data, both during transmission and at rest, in an encrypted, secure, UK-based server in line with GDPR.³
- **Anonymisation and pseudonymisation.** All participants were assigned a unique ID number, and pseudonyms were used for interview transcripts. No personally identifiable information appears in

³ For more information see <https://www.smartsurvey.co.uk/gdpr>.

any reports or publications. Only the minimum necessary personal data were collected and processed.

- **Informed consent.** We implemented processes to fully inform participants of data protection considerations regarding data collection, the YEF archive and their data collection rights. After participants agreed to participate, they were allotted an ID number. Participants were informed that all information about them would be stored securely. Data obtained from participants through questionnaires and interviews were kept separately from identifying information.
- **Clear retention periods.** All project data will be deleted securely six years after project completion, i.e. in June 2032. We also followed the YEF's guidance on data protection, which includes producing privacy notices and information sheets. We will securely delete all names and other personal data from the datasets we hold after submitting the data for archiving in line with [YEF guidance](#).

1.8 Evaluation team and delivery teams

The evaluation of the BDP was led by Cordis Bright and took a collaborative approach, with input from Future Men and the YEF.

The BDP was designed by Future Men. During the set-up phase of the evaluation, Cordis Bright provided support to the programme by (a) supporting the refinement of the theory of change, (b) drafting and refining the BDP manual in collaboration with the Future Men team and (c) supporting the re-design of the BDP referral form and monitoring data processes to support screening and monitoring fidelity.

Delivery of the BDP and the evaluation were funded by the YEF. There are no conflicting interests which we are aware of that may be perceived to influence the design, conduct, analysis or reporting of the study.

Details of the key BDP delivery and Cordis Bright evaluation team members are presented below.

Future Men's delivery team

- **Ronan McGhee, Project Manager (Year 2).** Oversaw day-to-day delivery and co-ordination of the BDP across participating schools, including line management of project co-ordinators and monitoring delivery fidelity. Ronan took over from David for Year 2 of the study.
- **David Hopkinson, Project Manager (Year 1).** Oversaw day-to-day delivery and co-ordination of the BDP across participating schools, including line management of project co-ordinators and monitoring delivery fidelity for Year 1 of the study.
- **Ann Cayenne, Operations Director (Head of the Boys' Development Team).** Provided strategic and operational oversight for the BDP, ensuring alignment with Future Men's safeguarding, quality assurance and equity frameworks.
- **Courtney Augustin, Joshua Laval and Christopher Duféal (Project co-ordinators).** Delivered the BDP in schools, liaised with school staff and recorded activity and dosage data. Maintained fidelity to the BDP model and safeguarding practice.
- **Laurie Zoro and Louis Anani (Research assistants).** Administered baseline, 12-week and 24-week surveys and supported data collection from the partner schools.

- **Chris Stein (Director of Marketing, Fundraising and Advocacy at Future Men).** Provided senior oversight, ensuring the programme's delivery and evaluation aligned with Future Men's wider organisational strategy and partnerships.

Cordis Bright's evaluation team

- **Dr Stephen Boxford (Principal Investigator and Project Director).** Ensured the evaluation was delivered to a high standard by providing quality assurance throughout the project, including shaping evaluation design, approaches, tools, analysis and outputs.
- **Emma Andersen (Co-Principal Investigator and Project Manager).** Oversaw day-to-day project delivery and acted as the main point of contact for the YEF and the project delivery team. Responsible for evaluation design, shaping tools, and conducting impact and IPE analysis and reporting.
- **Professor Darrick Jolliffe (Co-Principal Investigator).** Responsibilities included quality assuring the evaluation design, research tools, and evaluation outputs.
- **Camilla Antrobus (Co-Principal Investigator).** Conducted school engagement and approach and supported evaluation design, shaping approaches, designing tools, conducting fieldwork and quality assuring evaluation outputs.
- **Keiran Matters (Co-Principal Investigator).** Provided input on safeguarding and consultation with young people and supported evaluation design, shaping approaches, designing tools, conducting fieldwork and quality assuring evaluation outputs.
- **Karim Bukleb (Researcher).** Provided ongoing support to BDP staff by providing administration services for evaluation tools, conducting fieldwork, drafting the IPE analysis and supporting report drafting.

1.9 Timeline

Table 6 provides a detailed timeline of the study, including key activities, timings, and roles and responsibilities for the efficacy study.

Table 6: Project activities and timeline

Dates	Activity	Staff responsible
Evaluation set-up phase (April 2023 to August 2023)		
April 2023	• Efficacy randomised controlled trial commences after Stage Two applications finalised	Cordis Bright YEF
June 2023	• Ethics application submitted to the Royal Holloway University Research Ethics Committee • Information sheets and privacy notices finalised • Recruitment meetings held with prospective schools • Primary outcome measure agreed	Cordis Bright YEF Future Men
July 2023	• Efficacy study trial protocol drafted	Cordis Bright Future Men

Dates	Activity	Staff responsible
	Schools recruited and in place before the end of the summer term	
August 2023	- Efficacy study trial protocol finalised in light of feedback - Ethical approval confirmed from Royal Holloway University - Evaluation handbook developed for the Future Men team administering tools - Training on tool administration delivered to the Future Men team by Cordis Bright	Cordis Bright YEF Future Men
Efficacy study trial delivery in the field (September 2023 to January 2025)		
September 2023 to July 2025	- Efficacy study delivered in the field, i.e. referrals, randomisation, data collection and support delivery - Data quality audits completed for the first 20 boys - Monthly data quality audits conducted throughout the efficacy study	Future Men Cordis Bright
January 2025	- Baseline outcomes data collection and randomisation complete - Start of interviews with Future Men staff and stakeholders - Start of interviews with boys receiving BDP support	Future Men Cordis Bright
May 2025	- Completion of interviews with Future Men staff and stakeholders - Completion of interviews with boys receiving BDP support	Cordis Bright
July 2025	Completion of all data collection	Future Men Cordis Bright
Analysis and reporting (September 2025 to January 2026)		
July to October 2025	Analysis	Cordis Bright
November 2025	Submission of draft final evaluation report	Cordis Bright
February 2026	Submission of final, peer-reviewed evaluation report	Cordis Bright YEF
March 2026	Evaluator supports the YEF publication process	Cordis Bright YEF
June 2026	Evaluator submits data for the YEF archive	Cordis Bright YEF

Note: YEF = Youth Endowment Fund; BDP = Boys' Development Programme

2 Methods

2.1 Overview

This section presents the methods used through the efficacy study. It covers:

- Impact evaluation methods
- IPE methods

2.2 Impact evaluation methods

2.2.1 Trial design

The efficacy study was a two-arm, parallel RCT with randomisation conducted at the individual level, with allocation on a one-to-one basis and stratification at the school level. Table 7 presents an overview of the efficacy study trial design, and each aspect is explained in more detail below. No important changes have been made to the efficacy study design since the [efficacy study protocol](#).

The trial compared the BDP (BDP delivery group) with a signposting control condition (signposting group). Boys allocated to the BDP delivery group received 12 weekly one-to-one sessions from a trained project co-ordinator focused on developing social and emotional skills. Boys allocated to the signposting group were signposted to existing in-school pastoral or behavioural support, receiving BAU provision available within their school. Further detail on both arms is provided in section 3.3.4.

Table 7: Trial design summary

Trial design, including number of arms		Two-arm parallel randomised controlled trial with random allocation at the young person level
Unit of randomisation		Individual participant
Stratification variables		School
Number of participants		480, i.e. 240 in the treatment group and 240 in the signposting group.
Primary outcome	Variable	School engagement
	Measure	School Connectedness Questionnaire (Marsh & Randolph, 2020).
Secondary outcome(s)	Variable(s)	- Behavioural difficulties - Emotional symptoms - Relationships with peers - Relationships with teachers
	Measure(s)	- Behavioural difficulties measured by the Strengths and Difficulties Questionnaire externalising behaviours score (Goodman, 2005) - Emotional symptoms, measured by the Strengths and Difficulties Questionnaire emotional symptoms subscale (Goodman, 2005) - Relationships with peers, measured by the Strengths and Difficulties Questionnaire peer difficulties subscale (Goodman, 2005)

Trial design, including number of arms		Two-arm parallel randomised controlled trial with random allocation at the young person level
		- Relationships with teachers, measured by the School Connectedness Questionnaire teacher bonding and attachment subscale (Marsh & Randolph, 2020).
Baseline for primary outcome	Variable	School engagement
	Measure	School Connectedness Questionnaire (Marsh & Randolph, 2020).
Baseline for secondary outcome(s)	Variable	- Behavioural difficulties - Emotional symptoms - Relationships with peers - Relationships with teachers
	Measure	- Behavioural difficulties, measured by the Strengths and Difficulties Questionnaire externalising behaviours score (Goodman, 2005) - Emotional symptoms, measured by the Strengths and Difficulties Questionnaire emotional symptoms subscale (Goodman, 2005) - Relationships with peers, measured by the Strengths and Difficulties Questionnaire peer difficulties subscale (Goodman, 2005) - Relationships with teachers, measured by the School Connectedness Questionnaire teacher bonding and attachment subscale (Marsh & Randolph, 2020).

2.2.2 Race equity, equality, diversity and inclusion

As discussed in section 1.1, the BDP addresses the clear and consistent evidence that boys from minority ethnic backgrounds, particularly those from Black Caribbean, and dual heritage Caribbean backgrounds, are disproportionately affected by school exclusion and disengagement (Graham et al., 2019; Wallace & Joseph-Salisbury, 2022; Demie, 2021). As such, delivering the evaluation in line with principles of race equity, equality, diversity and inclusion (REDI) was central to our approach.

The BDP is delivered in a context in which structural and systemic factors are widely argued to contribute to disproportionate exclusion and disengagement among boys from minority ethnic backgrounds. The evaluation recognises this context and seeks to ensure that analysis and interpretation do not attribute patterns of engagement or outcomes solely to individual characteristics but consider the wider systems within which boys and schools operate.

All of Cordis Bright's evaluation work is delivered in line with our equity, diversity and inclusion (EDI) strategy (available [here](#)) and EDI project toolkit (available [here](#)). This sets out our commitment, principles and approaches to ensure that our work is accessible to all. We commit to:

- **Provide equal opportunities** in all aspects of employment and ensure that we do not discriminate in recruitment or employment on the basis of a protected characteristic or any other characteristics or identity.

- **Oppose discrimination in all its forms**, be it at a structural or institutional level or an interpersonal level. This includes direct discrimination, indirect discrimination, discrimination by association, discrimination by perception, victimisation, harassment and bullying.
- **Seek to build our understanding** of the barriers created by discrimination and inequity and ensure fair, equal and inclusive treatment for our staff, clients and the people whom our work aims to support.

In line with these commitments, we embedded REDI considerations throughout the design, delivery and analysis of this efficacy study. Specifically, we:

- **Provided clear, accessible information** about the programme and evaluation, ensuring that materials were written in plain language, culturally appropriate and available in formats suited to different literacy and accessibility needs
- **Used informed consent processes and materials aligned with best practice guidance** (Government Social Research Unit, 2018; YEF, 2021), ensuring that they were inclusive, sensitive to cultural context and appropriate for the age and experiences of participants
- **Monitored demographic and socioeconomic characteristics of all participants** across the BDP delivery group and signposting group, enabling us to analyse patterns of recruitment, participation and retention and assess representativeness relative to the wider school population
- **Deployed staff with cultural competency training**, including experience in equity and inclusion projects addressing disproportionality in school exclusion and youth outcomes among minority ethnic boys
- **Conducted exploratory subgroup analyses of the primary outcome** (school engagement) by ethnicity, comparing outcomes between White and minority ethnic groups (because of sample size limitations, ethnicity categories were collapsed in line with ONS classifications)
- **Worked with Future Men to ensure a broadly representative sample of boys** participated in qualitative interviews for the IPE, enabling us to explore how race, identity and context shaped boys' engagement with the BDP. Reflexive analytic practice, including team discussions on positionality and bias during qualitative analysis, formed part of this approach. Table 13 shows the demographic breakdown of boys interviewed.
- **Engaged in continuous reflection and dialogue** with Future Men and YEF colleagues to identify and address any barriers to equitable participation and to strengthen the inclusivity of both delivery and evaluation practice

In this way, the evaluation sought to produce evidence that is both rigorous and socially just, reflecting the lived realities of boys most affected by school exclusion and disengagement. While the study was not designed to directly evaluate the structural causes of educational inequity, embedding REDI principles from design through to data collection and analysis ensured that findings were interpreted within their broader social context and contribute to evidence on relationship-based interventions in settings shaped by inequality.

2.2.3 Randomisation

Randomisation approach

This efficacy study was a two-arm, parallel RCT. Randomisation was conducted at the individual level, with stratification at the school level. All boys who were referred to the programme, met the eligibility criteria, consented to be part of the evaluation and completed a baseline questionnaire were allocated at random to the BDP group (treatment group) or signposting group (control group) on a one-to-one basis. Randomisation was conducted using 24 blocks of 20 pupils (i.e. one block of 20 boys per term for each of the six schools participating in the trial at the same time), followed by block sizes of four. This design was agreed for the following reasons:

1. The small sample size of six participating schools meant that randomisation at the school level would not control for school-level variation. Randomising at the individual level with equal allocation within schools controlled for school-level effects.
2. Implementation-wise, it was the most elegant design, as it meant that all schools received some input from the BDP, which improved both the buy-in from schools and the likelihood of the study being informed by higher-quality data.
3. Schools were referred into the programme in batches of 20 boys per term. Using block sizes of 20 therefore ensured that 10 boys were allocated to the BDP delivery group across each of the four terms, which ensured that Future Men were able to deliver at capacity. Using block sizes of 20 also minimised the likelihood of predictability compared to using five blocks of four per school.
4. Subsequent block sizes of four were used as needed to account for any attrition that had been experienced throughout the trial. The programme sought to fill spaces in the BDP group that were made vacant due to attrition only if it was still possible to deliver eight or more one-to-one sessions that term.

Sequence generation

The randomisation sequence was generated digitally using online software, Sealed Envelope.⁴ This was generated at the start of the study for all 480 boys using 24 blocks of 20 (i.e. four blocks per school, one per school per term), followed by block sizes of four (in the event of any attrition within schools). The randomisation sequence was stored securely on Cordis Bright servers and was not accessible to Future Men colleagues.

Randomising and auditing

Once informed consent and baseline measures were collected, the Future Men team conducted randomisation. They entered each boy's unique identifier and school name, along with the confirmation that informed consent and baseline measures were collected, into the Sealed Envelope software, which then returned the randomisation outcome. The boy was then informed of the randomisation outcome,

⁴ See <https://www.sealedenvelope.com/>

which was also noted and stored in the evaluation monitoring dataset. Cordis Bright delivered training and guidance to the BDP evaluation research assistants and project co-ordinators on how to inform boys of the result neutrally to avoid feelings of winning or losing.

As trial administrators, the evaluation team had access to a data audit log of all randomisations which had been conducted. Throughout the study, Cordis Bright regularly audited this log against the evaluation monitoring data to ensure the integrity of the randomisation process was intact.

Allocation concealment

Allocation concealment was ensured using the digital randomisation software Sealed Envelope, i.e. randomisation outcomes were computer-generated. Within this software, the Future Men team were able to generate randomisation outcomes, and the evaluation team were able to access the randomisation sequence and audit the randomisations which occurred. This separation of roles by the software enabled allocation concealment, as the Future Men team did not have access to the sequence.

Blinding

Due to the nature of the trial, it was not possible for participants or the Future Men team to be blind to allocation arm. Due to the nature of the trial and the data we collected in terms of activity, dosage and outcomes, the analysis was also not subject to blinding.

2.2.4 Outcome measures

Table 8 maps the outcomes from the BDP's theory of change to the validated measures which were used to capture them.

All outcomes and corresponding measures were discussed and refined through a series of outcome measure reviews and joint discussions between Cordis Bright, Future Men and the YEF. These reviews considered item relevance to the BDP's theory of change, the length of the scale, accessibility for the intended cohort and evidence of validity. Following these reviews, the YEF recommended that all measures be selected from the [YEF outcomes framework and measures database](#) to ensure alignment with its wider portfolio and comparability across evaluations. The School Connectedness Questionnaire (SCQ) was subsequently identified as the most appropriate measure for capturing school engagement from the available tools in the database.

All questionnaires (Time 1: baseline before randomisation [T1], T2 and Time 3: follow-up at 24 weeks post-randomisation [T3]) included the following scales:

- [SCQ](#)
- [Strengths and Difficulties Questionnaire \(SDQ\)](#)

In addition, to test the quality of the mentor-mentee relationship, T2 and T3 questionnaires also included:

- [Social Support and Rejection Scale \(SSRS\)](#)

Boys in the BDP delivery group completed the SSRS in relation to their BDP project co-ordinator. Boys in the signposting group were first asked whether they had a trusted non-parental adult; those who answered yes were then asked to complete the SSRS in relation to the individual they selected.

All measures were reviewed to ensure they were in line with Early Intervention Foundation evidence standards, i.e. that they are not amended, that they were standardised and validated and that they captured the project outcomes. In addition, measures were prioritised if they were brief, used clear and age-appropriate language and had been validated for use with boys of the same age and/or from marginalised backgrounds.

Table 8: Outcome measures

Outcome from the theory of change	Timescale	Measure	Subscale(s)	Items	Collection point(s)
Primary outcome measure					
Boys have stronger engagement with school	Medium term	SCQ	Full measure	10	Baseline (T1) 12 weeks (T2) 24 weeks (T3)
Secondary outcome measures					
Boys get along better with their peers	Medium term	SDQ	Peer difficulties subscale	5	Baseline (T1) 12 weeks (T2) 24 weeks (T3)
Boys have improved emotional regulation	Medium term	SDQ	Emotional symptoms subscale	5	Baseline (T1) 12 weeks (T2) 24 weeks (T3)
Boys have reduced misbehaviour and aggression	Medium term	SDQ	Externalising behaviours score (i.e. the sum of the hyperactivity and conduct subscales).	10	Baseline (T1) 12 weeks (T2) 24 weeks (T3)
Boys get along better with their teachers	Medium term	SCQ	Teacher bonding and attachment subscale	3	Baseline (T1) 12 weeks (T2) 24 weeks (T3)
Mechanism of change					
Positive relationship between project co-ordinator and young person	Inter-mediate	SSRS	Full measure and individual subscales	22	12 weeks (T2) 24 weeks (T3)

Note: SCQ = School Connectedness Questionnaire; SDQ = Strengths and Difficulties Questionnaire; SSRS = Social Support and Rejection Scale

Outcomes were measured at the individual level through the administration of self-report validated measures. Self-report data were collected in school settings by members of the Future Men team (e.g. the project co-ordinators and evaluation research assistants) who were independent from those delivering support to the boys. This was to minimise bias and ensure that boys felt able to respond openly and honestly about their experiences.

Data were collected at three time points:

- **T1.** Once informed consent had been achieved from parents/carers and boys and before randomisation was conducted

- **T2.** On exit from the BDP for those in the BDP delivery group and at a similar time for the signposting group
- **T3.** To examine any observed differences 12 weeks post programme completion.

Primary outcome measure

The primary outcome for the evaluation is improved school engagement. This was measured by the self-report SCQ (Marsh & Randolph, 2020), and collected at T1, T2 and T3. The efficacy study explored the impact of the BDP in comparison to the signposting group on the SCQ total score. The primary time point was T2, with T3 acting as a pre-specified, exploratory follow-up.

About the School Connectedness Questionnaire

The SCQ (Marsh & Randolph, 2020) is a 10-item self-report measure from the [YEF outcomes framework and measures database](#) and was agreed in collaboration with the YEF and Future Men. In this evaluation, the total SCQ score (i.e. the sum of all 10 items) is used as the primary outcome measure to assess overall levels of school engagement.

The SCQ comprises three subscales:

- Teacher bonding and attachment (three items, e.g. *“I like my teachers”*)
- Peer bonding and attachment (four items, e.g. *“I have more than one friend at school”*)
- School engagement (three items, e.g. *“I come to school every day”*)

The SCQ uses a 3-point Likert scale, which includes 1 (not true), 2 (somewhat true) and 3 (true). Higher scores indicate greater connectedness and engagement. At baseline, boys were asked to consider the previous six months. At T2 and T3, boys were asked to consider the previous four school weeks.

Operationalising school engagement

As part of operationalising school engagement, it is important to distinguish between:

- **Overall school engagement**, which is this trial’s primary outcome and is operationalised through the SCQ’s total score
- The SCQ’s **school engagement subscale**, which captures behavioural indicators of engagement (e.g. *“I do my classwork”*, *“I do my homework”* and *“I come to school every day”*)

In line with the trial’s definition of school engagement set out in section 2.1, the SCQ subscales can be said to conceptually map onto each dimension in the following way:

- The SCQ’s school engagement subscale reflects **behavioural engagement (participation)**, capturing observable indicators of school participation, such as attendance and completion of schoolwork.
- The teacher bonding and attachment and peer bonding and attachment subscales reflect **affective engagement (identification)**, capturing pupils’ sense of belonging, relationships with peers and teachers, and identification with school.

As such, the SCQ's three subscales together operationalise school engagement as a multidimensional construct, and their sum (the total SCQ score) serves as the primary outcome for the trial.

Psychometric properties

The SCQ was developed on both general education and special education students, using a diverse student population in terms of race and ethnicity, socioeconomic status, and age (6–18 years). For all three subscales, higher scores are more desirable.

Psychometric analyses showed that the scale has a clear three-factor structure (teacher bonding and attachment, peer bonding and attachment, and school engagement) and acceptable internal reliability (Cronbach's α is generally between 0.60 and 0.80). The measure demonstrated strong internal structural validity and was designed for use with diverse populations, including students with lower reading levels, making it suitable for evaluating school connectedness among boys with a range of learning needs and backgrounds.

Secondary outcome measures

The secondary outcomes measure whether the boys who receive the BDP:

- Get along better with their peers (SDQ peer relationships subscale)
- Have reduced behavioural difficulties (SDQ externalising behaviours score)
- Show reductions in emotional symptoms (SDQ emotional symptoms subscale)
- Get along better with their teachers (SCQ teacher bonding and attachment subscale)

All four secondary measures above were collected at T1, T2 and T3. These measures were agreed through discussions between Cordis Bright, Future Men and the YEF.

About the Strengths and Difficulties Questionnaire

The SDQ is a 25-item questionnaire measuring behaviours, emotions and relationships for 4- to 17-year-olds. It contains five subscales with five items in each:

- Emotional symptoms (five items, e.g. *"I worry a lot"*)
- Conduct problems (five items, e.g. *"I get very angry and often lose my temper"*)
- Hyperactivity/inattention (five items, e.g. *"I am restless; I cannot stay still for long"*)
- Peer problems (five items, e.g. *"I am usually on my own. I generally play alone or keep to myself"*)
- Pro-social behaviours (five items, e.g. *"I try to be nice to other people. I care about their feelings"*)

Each item is scored on a 3-point Likert scale, from 0 to 2, such that the scores for each subscale range from 0 to 10. For the pro-social values subscale, high scores are desirable (i.e. greater pro-social values), but for all other subscales high scores are not desirable (i.e. they suggest greater difficulties). At baseline, boys were asked to consider the previous six months. At T2 and T3, boys were asked to consider the previous month.

In this evaluation, behavioural difficulties were measured using the externalising behaviours score, which is calculated as the sum of the conduct problems and hyperactivity/inattention subscales. This composite score therefore ranges from 0 to 20, with higher scores indicating greater behavioural difficulties.

The SDQ has been shown to have good internal consistency (Cronbach's alpha = 0.73), cross-informant correlation (mean = 0.34) and retest stability after four to six months (mean = 0.62) (Goodman, 2001). The YEF aims for common outcome measures to be used where possible to maximise learning and has endorsed the SDQ for measuring behaviours, emotions and relationships.

Mechanism of change

We also measured the extent of positive relationships between boys and project co-ordinators (BDP delivery group) and significant adults (signposting group) using the SSRS (Roffman et al., 2000).⁵

The SSRS was collected at T2 and T3 for all boys in the BDP delivery group and for boys in the signposting group who reported having a trusting, non-parental relationship. This measure was selected in addition to the YEF's core measure (SDQ) because a positive relationship with a trusted adult who models behaviours and values is the key mechanism of change of the BDP. The SSRS was selected in agreement between Cordis Bright, Future Men and the YEF.

About the Social Support and Rejection Scale

The SSRS is a measure of the quality of the relationship between a boy and a trusted adult (signposting group) or project co-ordinator (BDP delivery group).

The SSRS has four subscales:

- **Feels valued** (six items, e.g. *"This person cares about how I am doing in school"*)
- **Trust** (five items, e.g. *"I feel safe when I am with this person"*)
- **Mentoring** (five items, e.g. *"This person introduces me to new ideas or experiences"*)
- **Negativity** (six items, e.g. *"I don't like things this person says or does"*)

Each item is scored from 1 (never) to 5 (always). Each subscale score is the average of items that make up the subscale. Higher scores on the negativity scale reflect higher levels of stress and negativity within the relationship. For the overall scoring of the scale, a high score represents a positive relationship. The SSRS does not use recall periods, i.e. boys were asked to consider how they currently feel about their project co-ordinator or trusted non-parental adult.

The SSRS has good internal reliability for each of the four subscales across ratings from clubs, schools and extended family (Cronbach's alpha = 0.67 to 0.76 for negativity, 0.74 to 0.81 for mentoring, 0.74 to 0.78 for trust and 0.81 to 0.88 for feels valued [Roffman et al., 2000]). More information about the SSRS (including

⁵ See <https://nationalmentoringresourcecenter.org/resource/measurement-guidance-toolkit/#mentoring-relationship-quality-and-characteristics--social-support-and-rejection-scale>. Last accessed 26 November 2025.

its subscales and validity) is available in the National Mentoring Resource Center's Measurement Guidance Toolkit.

2.2.5 Sample size calculations

This section sets out:

- Power calculations for the whole study cohort, which were conducted *a priori* as part of the initial trial design and co-design period based on the projected analytical sample
- Updated power calculations for both the whole cohort and the subgroup of boys eligible for FSM based on the achieved randomisation sample

The primary population of interest for this trial was the whole cohort, i.e. boys who were eligible for the BDP due to being at risk of exclusion or disengagement from school, in line with the eligibility criteria set out in section 2.4.2. Eligibility was based on school referral and indicators of behavioural, attendance or engagement concerns, rather than demographic characteristics.

In line with EEF priorities, sample sizes and power calculations are also presented for the FSM subgroup as a key cohort of policy interest. FSM status was not an explicit eligibility criterion; however, given the well-established association between socioeconomic disadvantage and risk of school exclusion, a substantial proportion of the eligible cohort were FSM-eligible.

Power calculations are summarised in Table 9 and explained in more detail below. All power calculations were conducted in PowerUp! (Dong & Maynard, 2013).

A note on sample sizes and the presented minimum detectable effect size

It is important to note that the original power calculations presented in the trial protocol and SAP were based on the anticipated final analytical sample, after accounting for expected attrition rates. Specifically, the study was designed to recruit up to 480 participants, which, assuming 12% attrition, yields an analytical sample of 422 participants (211 per arm) and a minimum detectable effect size (MDES) of 0.195.

Following updated guidance from the YEF, the final MDES estimates below are presented based on the randomisation sample (i.e. all participants randomised), rather than the projected analytical sample. This approach does not adjust for anticipated attrition and, therefore, results in a larger sample size and a correspondingly smaller MDES.

Table 9 presents both:

- The original *a priori* calculations based on the analytical sample (as part of trial design)
- Updated calculations based on the achieved randomisation sample, completed as part of final analysis and reporting

Differences between these columns reflect this change in approach rather than any substantive change to the underlying design assumptions.

Table 9: Sample size calculations⁶

		Trial design (projected analytical sample)		Achieved randomisation sample ⁷	
		Whole cohort	FSM	Whole cohort	FSM
Minimum detectable effect size		0.195	0.211	0.182	0.230
Pre-test/post-test correlations	Level 1 (participant)	0.7	0.7	0.7	0.7
	Level 2 (cluster)	N/A	N/A	N/A	N/A
Alpha		0.05	0.05	0.05	0.05
Power		0.80	0.80	0.80	0.80
One-sided or two-sided?		Two-sided	Two-sided	Two-sided	Two-sided
Number of participants	Intervention	211	180	243	145
	Control	211	180	243	159
	Total	422	360	486	304

Note: FSM = free school meals

Whole cohort

The initial *a priori* power calculations were conducted in line with YEF guidance and informed the trial design. These indicated that a total sample of 422 boys (211 per arm) in the final analytical sample would provide 80% power to detect an MDES of 0.195 at a two-tailed significance level of 0.05, if it exists, assuming a pre-test/post-test correlation of 0.7.

Our approach to these calculations was conservative and was guided by:

- **YEF guidance.** YEF guidance suggests that, in order to achieve a maximum security rating of 5, efficacy study RCTs should have an MDES of less than 0.20.
- **Available data on the SCQ and similar measures.** As no published pre-test/post-test correlations exist for the SCQ, we drew upon data from the [Student Engagement in Schools Questionnaire](#) (Lam et al., 2014), which reported a six-month correlation of 0.73. We therefore assumed a conservative pre-test/post-test correlation of 0.7.

⁶ The MDES calculations use design-stage assumptions specified in the SAP (including an assumed pre-test/post-test correlation of 0.70). In the achieved dataset, the observed correlation between baseline and follow-up measures was approximately 0.53 (R^2 on the full model was 0.28).

⁷ Please note that in the SAP, equivalent columns were based on the projected analytical sample (assuming 12% attrition from the achieved randomisation sample). In line with updated YEF guidance, the figures presented here are based on the achieved randomisation sample, i.e. they do not account for attrition at T2.

- **Practical delivery considerations.** Future Men's delivery capacity set an upper limit of 480 boys, which provided a suitable tolerance for 12% attrition (above YEF's guidelines of 10%) while maintaining sufficient power.

Following completion of baseline and randomisation, the randomisation sample comprised 486 boys (243 in the BDP delivery group and 243 in the signposting group). In line with updated YEF guidance, power calculations based on this randomisation sample (i.e. without adjusting for projected attrition) indicate a reduced MDES of 0.182. This means the study remains well powered to detect effects of a size typically observed in YEF efficacy trials if they exist.

Overall, the achieved randomisation sample confirms that the study is appropriately powered to identify statistically significant and policy-relevant effects, should they exist.

Free school meals subgroup

Separate power calculations were conducted for the subgroup of boys eligible for FSM⁸ after the trial had commenced, in response to a request from the YEF and EEF as part of the SAP drafting process. These calculations did not inform the original study design but were based on baseline data available for the first term's cohort of boys. At that stage, 75% of recruited boys were eligible for FSM, as reported by school partners in the initial referral form. We therefore estimated that this subgroup would comprise 360 boys (180 per arm) of the total sample, i.e. assuming that 75% of the entire analysis sample would be eligible for FSM. This sample size would provide 80% power to detect an MDES of 0.211.

Based on the achieved randomisation sample, 304 recruited boys (145 in the BDP delivery group and 159 in the signposting group) were eligible for FSM. Under the same assumptions set out above (i.e. power = 0.80, two-tailed, $\alpha = 0.05$, pre-test/post-test correlation = 0.7), this yields an updated MDES of 0.230 for the randomisation sample. Although this subgroup analysis remains exploratory and somewhat underpowered relative to the full cohort, the achieved sample still allows the detection of moderate-sized effects with reasonable precision.

The final MDES values of 0.182 (whole cohort) and 0.230 (FSM subgroup) for the randomisation sample confirm that the evaluation retains sufficient statistical power, and these updates do not materially affect the integrity or robustness of the trial design.

2.2.6 Recruitment rates

The efficacy study was originally designed to recruit approximately 60 boys to the BDP delivery group and 60 boys to the signposting group per term across six participating schools, with an expected average of around 20 boys per school per term (10 in the BDP delivery group and 10 in the signposting group) across two academic years.

In practice, eight secondary schools were involved across the course of the study. Two schools withdrew from the study after the first academic year, one because it was unable to identify a sufficient number of eligible boys to meet recruitment targets and the other because of organisational challenges that made

⁸ Please note that in this section and throughout the report, eligibility for FSM is defined as being eligible at the point of referral into the trial. These data were provided by school partners on the referral form.

participation impractical. To maintain the overall target sample size and school-level diversity, two additional schools were onboarded for the second academic year. These schools were selected using the same eligibility criteria and onboarding processes as the original schools, ensuring consistency in setting characteristics, delivery conditions and evaluation procedures.

This adaptive approach maintained both the planned recruitment volume and the integrity of the randomisation and evaluation design. All replacement schools followed identical referral, consent, randomisation and data collection procedures, with oversight from the evaluation and delivery teams to ensure comparability across cohorts. As a result, there is no evidence to suggest that these changes introduced systematic bias or compromised the study's internal validity.

As a result, the final achieved recruitment rates across all schools and terms are presented in Table 10.

Table 10: Participant recruitment flows (numbers include one-to-one allocation across the Boys' Development Programme and signposting groups)

	Autumn term 2023	Spring term 2024	Autumn term 2024	Spring term 2025	Total
School A	18	20	20	19	77
School B	21	21	20	21	83
School C	20	20	20	20	80
School D	20	19	0	0	39
School E	0	0	21	19	40
School F	0	0	22	21	43
School G	24	20	0	0	44
School H	20	20	20	20	80
Total	123	120	123	120	486

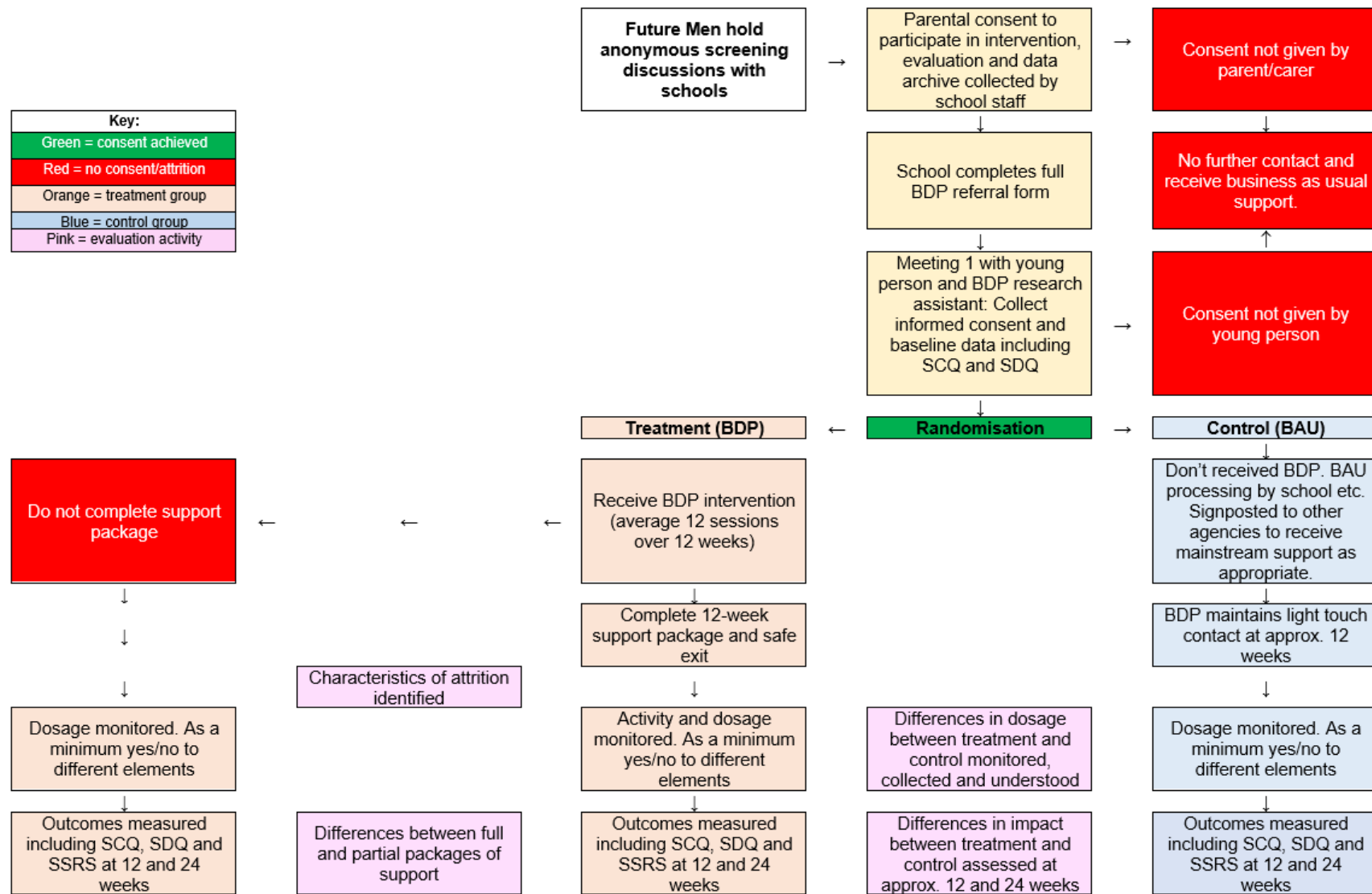
2.3 Participant journey

Figure 1 presents the participant flow diagram for the efficacy study. It summarises the following key steps:

- Referrals, identification and screening
- Collection of informed consent
- Data collection at baseline and two follow-up points
- Randomisation
- Treatment and support phases

The rest of this section describes how each of these processes was implemented during the efficacy study.

Figure 1: Efficacy randomised controlled trial pathway



Note: (BDP=Boys' Development Programme, BAU=Business as usual)

2.3.1 Participant referrals, eligibility and screening

Eligibility criteria

Eligible boys were identified by school leads, including heads, deputies, SENCos, pastoral leads and/or heads of year, using the inclusion and exclusion criteria and eligibility thresholds set out in section 1.4.2. Before referrals were made, trained and experienced members of the Future Men team met with school staff to explain the referral process, share the referral form and clarify the eligibility criteria and thresholds.

Each school was encouraged to make referrals on a term-by-term basis, i.e. around 20 referrals per school for the next academic term. The Future Men team then held an anonymous eligibility and screening discussion with school professionals to confirm the suitability of each boy for both the intervention and evaluation. This step ensured that only boys meeting the agreed criteria were included and that no identifiable information was shared before consent.

Once a boy was deemed eligible, school partners sought parental consent for him to participate in the programme and evaluation. Following consent, schools completed a detailed referral form designed and developed by Cordis Bright and Future Men, capturing eligibility thresholds, demographic information and socioeconomic data. Further detail on the data types collected at this stage is provided in section 3.3.3.

This structured referral and screening process ensured that eligibility decisions were transparent, consistently applied across schools and fully documented, strengthening the reliability and comparability of the evaluation sample.

2.3.2 Collecting informed consent

The role of the Boys' Development Programme research assistants

Future Men recruited two dedicated research assistants to support the efficacy study. They were independent from programme delivery, ensuring separation between evaluation and intervention functions. Their responsibilities included:

- **Meeting 1.** Explaining the programme and evaluation, obtaining informed consent from boys, administering baseline self-report surveys, conducting randomisation and informing participants of the allocation outcome. They also liaised with school leads to ensure that boys in the signposting group received BAU support.
- **Meeting 2.** Administering the 12-week follow-up surveys
- **Meeting 3.** Administering the 24-week follow-up surveys

In addition, research assistants were responsible for liaising with school leads to obtain and verify parent/carers consent and for collating school administrative data (attendance, exclusions and behaviour) for both trial arms.

The research assistants were supported in these tasks by the three BDP project co-ordinators in schools where they were not delivering. This approach optimised capacity while maintaining independence in data collection and minimising the risk of bias.

Both the boy and their parent/carers were asked to provide informed consent for participation. These processes were designed to adhere to best-practice guidance, including from the YEF and the Government

Social Research Unit, ensuring accessibility, inclusivity and cultural sensitivity. All consent materials, including information sheets and privacy notices, are provided in Appendix F.

Parents/carers

After eligibility screening, parents/carers were approached by trusted school staff to ensure familiarity and reassurance. They were provided with full written information about the programme, evaluation and data protection arrangements. Consent was collected electronically or in hard copy, according to the school's preference, and securely stored by Future Men. Paper forms were scanned and uploaded to Future Men's secure servers. All evidence of consent was transferred securely to Cordis Bright in accordance with the Data Protection Act and the GDPR.

Cordis Bright and Future Men worked collaboratively with school leaders to ensure that staff could explain the evaluation clearly and address any questions or concerns from parents.

Boys

Once parental consent had been received, the Future Men team met individually with each boy during school hours. They explained the evaluation, reviewed and discussed the information sheet, and obtained written assent. Boys were explicitly informed that participation was voluntary, that they could withdraw at any time without penalty and that withdrawal would not affect access to usual support.

Cordis Bright provided training and written scripts for all Future Men staff involved to ensure standardised communication and consistent delivery of consent information across schools. Feedback from research assistants and project co-ordinators was gathered regularly to monitor implementation fidelity and identify any issues.

Boys were informed about how their data would be stored, used and transferred, in line with GDPR. They were also informed of their right to request deletion of their information at any point up to two weeks after completing the final survey (T3) or to opt out of the YEF data archiving up to the end of the evaluation period (June 2026).

Evidence of written consent from boys was captured via secure digital photographs of signed consent forms uploaded to Future Men servers and shared securely with Cordis Bright.

These consent procedures ensured that participation was fully informed, voluntary and ethically compliant, with robust audit trails to evidence data integrity.

2.3.3 Data collection

Table 11 presents an overview of the different data types that are collected at each stage of the efficacy study. Each data type is then discussed in full throughout the remainder of this section.

Table 11: Data collection overview

Data type	Data collection source	Data collection point				
		Referral	Baseline	Treatment/ signposting phase	12-week follow- up	24-week follow- up
Eligibility	School partners	✓				
Demographic data	School partners	✓				
Self-report outcomes measures	Boys		✓		✓	✓
Administrative data from schools	School partners		✓		✓	✓
Activity and dosage data	Future Men team			✓		
Evaluation monitoring data	Future Men team	✓	✓	✓	✓	✓

Eligibility, demographic and socioeconomic data

Data on eligibility and demographic and socioeconomic characteristics were provided by school partners via the BDP referral form, developed collaboratively by Future Men, Cordis Bright and YEF. The form captured:

- Referral information, including referral reason, date and source
- Demographic data on sex, age, ethnicity, SEND status and need
- Socioeconomic data on FSM eligibility, care status and EAL
- Confirmation that the boy meets the inclusion criteria and thresholds outlined in section 1.4.2.
- Additional information about the young person, including accessibility requirements and clinical support needs

All quantitative data fields used harmonised data categories consistent with ONS and government standards, ensuring comparability across datasets. Data were securely stored on Future Men servers, shared with Cordis Bright via secure transfer and managed in full compliance with the Data Protection Act and GDPR.

Baseline outcomes collection

Following consent, baseline administration of the SDQ and SCQ was conducted by either a research assistant or a BDP project co-ordinator who was not delivering support in that school. This arrangement safeguarded the independence of data collection and reduced potential bias.

All staff received training and written scripts from Cordis Bright to ensure consistency and accuracy in survey administration. Staff assisted boys as needed (e.g. reading questions aloud or clarifying wording), emphasising confidentiality and the independence of the evaluation team.

Surveys were completed in one-to-one sessions in schools, in a private room during the school day. They were administered online via SmartSurvey, with data encrypted and stored on UK-based servers compliant with GDPR. Paper copies were available as a contingency, and this dual-mode approach was designed to support response rates and data completeness while maintaining data security and standardisation. However, in practice only the online versions were used.

If completing these tools was perceived as upsetting or as triggering welfare issues, a safeguarding intervention took place, whereby the BDP research assistant and school staff followed their internal safeguarding policies, referred the young person to school designated safeguarding leads and referred them to other support as required. In practice, no safeguarding incidents were prompted following the completion of evaluation tools.

Activity and dosage data collection

Activity and dosage data were collected by the BDP project co-ordinator who was responsible for delivering support to the young person. For those in the BDP delivery group, this involved collecting the following categories:

- The name of the BDP project co-ordinator
- For each session: the date, session type and session topic
- The total number of sessions completed for each young person
- The case closure date
- The case closure reason

These data enabled monitoring of programme fidelity, dosage and adherence to the BDP model, supporting later exploratory compliance analysis.

Follow-up collection

All boys also completed the SDQ, SCQ and SSRS at around 12 weeks and 24 weeks post-randomisation with support from the BDP research assistant. This took around 30–40 minutes to complete and was done during school time.

Administrative data

School administrative data were collected from partner schools to explore patterns of attendance and exclusions for boys in both the BDP and signposting groups. This was an exploratory component of the study, intended to supplement the self-report outcomes and provide an independent source of behavioural and attendance information.

All participating schools were invited to provide data extracts for boys across both groups. In total, five of the eight participating schools provided administrative data: attendance data for 234 boys (48% of the total cohort of 486) and exclusion data for 316 boys (65% of the total cohort). Appendix E shows the data availability across schools and cohorts. This shows that exclusion data were provided for 100% of participating boys in the five schools that provided data, while attendance data coverage within schools ranged from 88% to 95% in four schools, and was 25% in one school.

For the analysis, attendance and exclusion data were summarised by school half-term, covering the two half-terms prior to randomisation (T1) and the two half-terms immediately following the treatment period (T2). This differs from the original protocol, which proposed baseline data covering the preceding school year. The revised approach was adopted to ensure closer alignment with the timing of randomisation and follow-up data collection and to minimise the burden on school partners that were collating and sharing these data with the evaluation team.

The extent of missing data should be considered when interpreting the findings. While coverage within the five schools that provided administrative data was high (including complete coverage of participating pupils within those schools), not all schools were able to supply comparable datasets. As this was an efficacy trial with randomisation stratified within schools, the absence of administrative data from some schools is not expected to introduce bias into the estimation of treatment effects within contributing schools. However, it may limit the extent to which these exploratory findings can be generalised across all participating schools. Differences between schools that were and were not able to provide administrative data (for example in data systems or reporting practices) may also affect external validity. Future research may benefit from using centrally held administrative data (e.g. the National Pupil Database) to achieve more consistent and comprehensive coverage across schools.

Evaluation monitoring data

Throughout the evaluation, the BDP research assistant and project co-ordinators were responsible for monitoring and recording progress throughout the trial. This included tracking the following data types for each boy:

- Referral date, reason and source
- Informed consent
- Outcome measure completion rates (i.e. T1, T2 and T3)
- Randomisation outcomes
- Trial completion
- Trial withdrawals and attrition

Data were stored securely on Future Men's servers and shared monthly with Cordis Bright. These records enabled real-time monitoring of recruitment, retention and data completeness and provided a clear audit trail for assessing trial integrity. Regular data audits ensured that randomisation and data collection procedures were followed consistently across schools and cohorts.

2.3.4 Treatment phase and business-as-usual

Boys in the BDP delivery group received the BDP, consisting of around 12 one-to-one sessions with a trained project co-ordinator focused on developing social and emotional skills and competencies, in line with section 1.4. Sessions were typically delivered weekly over a 12-week period during school time, following the BDP's manualised structure but tailored to individual needs.

Boys in the signposting group were signposted to appropriate in-school provision by the BDP evaluation research assistant or project co-ordinator, who connected them to key pastoral or safeguarding staff (such

as safeguarding leads, SENCOs or heads of year). These boys then received the support or provision that the school would ordinarily provide under BAU conditions, i.e. the type of pastoral or behavioural support that would have been available in the absence of the BDP.

To maintain engagement and ensure consistent data collection, boys in the signposting group met with the BDP evaluation research assistant at 12 weeks and 24 weeks post-randomisation to complete the follow-up questionnaires. The evaluation team worked closely with each school to understand the nature of BAU provision and how it was implemented locally as part of initial recruitment. This included discussing the range of pastoral and behavioural support available, identifying referral routes and discussing the likely intensity and duration of provision. However, it was not feasible to quantify the precise content or frequency of BAU support for individual boys, given substantial variation in schools' systems for recording and reporting this information.

These limitations are common in school-based RCTs and are not judged to pose a material threat to internal validity, as randomisation at the individual level and stratification by school ensured that any such variation in BAU support was distributed evenly between groups. The evaluation team also captured qualitative evidence on BAU provision through the IPE (see Chapter 3), which supports interpretation of findings and provides context for understanding the counterfactual.

2.4 Statistical analysis

This section outlines our approach to statistical analysis. All analysis was conducted in SPSS 31.0.0 and RStudio 4.5.1.

2.4.1 Primary analysis

The primary outcome was school engagement, measured using the total score of the SCQ (Marsh & Randolph, 2020). All participants completed the SCQ at T1, T2 and T3.

The pre-specified primary endpoint was T2, as this time point corresponded to the end of programme delivery and therefore provided the most appropriate assessment of immediate programme impact. Selecting T2 as the primary endpoint also reflected pragmatic considerations, including minimising attrition and maintaining high follow-up rates within the school year, thereby strengthening internal validity and statistical power. T3 was included as a longer-term follow-up to explore whether any potential effects were sustained beyond programme completion.

For the primary analysis, we conducted a complete case analysis using an intention-to-treat (ITT) approach, i.e. all boys with valid scores for the SCQ total scores at T1 and T2 were included in the analysis in line with their original allocation. Scores were pro-rated⁹ for boys who had completed at least 80% of the scale, i.e. completed eight out of 10 items. This approach helps to maximise available data while maintaining the reliability of the outcome measure.

⁹ Pro-rating a scale means calculating a participant's score when they have completed most, but not all, items on that scale. For example, if a young person completed 8 out of 10 questions, this would involve calculating their average score across the eight completed items and multiplying this by 10 to estimate their total score.

The primary impact analysis used an analysis of covariance (ANCOVA) model, adjusting for baseline SCQ total scores with school fixed effects, to align with the stratified randomisation design. This model was implemented using complete cases for participants with valid SCQ data at both baseline and T2. The model took the following form:

$$Y_{i,T2} = \beta_0 + \beta_1 \text{Baseline}_i + \beta_2 \text{Group}_i + \delta^T \text{School}_i + \varepsilon_i \in N(0, \sigma^2) \quad (1)$$

For $i = 1, \dots, N$ boys

Where:

- $Y_{i,T2}$ is the SCQ total score for individual i at T2
- Baseline_i is the baseline SCQ total score
- Group_i is a dummy variable for allocation group, i.e. 1 for the BDP delivery group and 0 for the signposting group
- β_2 is the treatment effect at T2, i.e. the primary parameter of interest for the trial
- School_i is a vector of $K - 1$ binary school dummy variables corresponding to a vector of δ^T coefficients

This analysis was designed to estimate the impact of allocation to the BDP on school engagement at T2, controlling for baseline school engagement and school, compared to allocation to the signposting group.

2.4.2 Secondary analysis

Secondary outcomes were assessed using the same ANCOVA approach as the primary model, adjusting for baseline scores and school. Pre-specified secondary outcomes were:

1. Behavioural difficulties measured by the SDQ externalising behaviours score
2. Emotional problems measured by the SDQ emotional difficulties subscale
3. Peer relationships measured by the SDQ peer difficulties subscale
4. Teacher bonding measured by the teacher bonding SCQ subscale

Exploratory analysis was also conducted on additional outcomes, including the SCQ school engagement subscale and peer bonding subscale and the SDQ pro-social values subscale. While not a pre-specified secondary outcome, analysis of all SCQ subscales was pre-specified in the SAP. These analyses were run to provide further context for the total scores and were reported as exploratory and hypothesis-generating. No adjustment for multiple testing was applied, consistent with YEF and EEF guidance for exploratory analyses.

The secondary analysis used complete case analysis using an ITT approach, i.e. all boys with valid scores at T1 and T2 were included in the analysis in line with their original allocation. For each SDQ score, scores were pro-rated for boys who had completed at least 60% of the relevant items, i.e. completed three out of five items for the subscales or six out of 10 for the externalising behaviours score. This approach is consistent

with the [YEF's SDQ guidance](#) on handling missing data. More about the SDQ and its sub-scales and how they are calculated is presented in section 2.2.4.

For the SCQ, as subscale scores for this measure are based on three or four items, they were only calculated for those who had completed all items, i.e. scores were not pro-rated. More about the SCQ total score and how it is calculated is presented in section 2.2.4.

Our analysis of the impact of allocation to the BDP on secondary outcomes was conducted using an ANCOVA model. This took the same form as the primary analysis set out in Equation (1):

$$Y_{i,T2} = \beta_0 + \beta_1 \text{Baseline}_i + \beta_2 \text{Group}_i + \delta^T \text{School}_i + \varepsilon_i \in N(0, \sigma^2) \quad (2)$$

For $i = 1, \dots, n$ boys

Where:

- $Y_{i,T2}$ is the secondary outcomes score for participant i measured at T2
- Baseline_i is the secondary outcomes score for participant i measured at T1.
- Group_i is a dummy variable for allocation group, i.e. coded 1 for boys in the BDP delivery group and 0 for boys in the signposting group
- β_2 is the treatment effect

This analysis was designed to estimate the impact of allocation to the BDP delivery group on secondary outcomes, when controlling for baseline outcomes and school, compared to allocation to the signposting group.

2.4.3 Longitudinal analysis

To assess sustained effects, the primary and secondary models were repeated for outcomes measured at T3 (i.e. approximately three months after programme completion). In this evaluation, T3 forms a pre-specified, exploratory follow-up, as the study was not powered to account for attrition between T2 and T3.

The longitudinal ANCOVA model took the following pre-specified form:

$$Y_{it} = \beta_0 + \beta_1 \text{Baseline}_i + \beta_2 \text{Group}_i + \delta^T \text{School}_i + \beta_4 \text{Time}_{it} + \beta_5 (\text{Group}_i \times \text{Time}_{it}) + \varepsilon_{it} \quad (3)$$

For $i = 1, \dots, n$ boys and $t \in \{T2, T3\}$

This uses the same variables as the primary analysis set out in Equation (1) and the secondary analyses set out in Equation (2), with the following additional terms:

- Y_{it} is the outcome for boy i and follow-up time t
- Time_{it} is an indicator variable equalling 1 for T3 and 0 for T2
- β_2 is the treatment effect at T2

- $\beta_2 + \beta_5$ is the treatment effect at T3
- β_5 is the change in treatment effect between T2 and T3

This analysis was conducted in line with Twisk et al. (2018) and the pre-specified longitudinal ANCOVA approach in the SAP. This approach jointly models post-randomisation outcomes while adjusting for baseline values and stratification variables. By modelling the repeated outcome measurements simultaneously and accounting for the correlation between them within individuals, the approach can improve statistical precision and make more efficient use of the available data than analysing each follow-up time point separately. This increased efficiency is achieved without introducing bias and is recommended for randomised trials with repeated outcome measurements. For transparency, results from a standard baseline-adjusted ANCOVA using the T3 outcome alone are also presented to assess the sensitivity of the findings to the modelling approach.

2.4.4 Subgroup analyses

We assessed the presence of heterogenous treatment effects in line with REDI considerations. There is limited evidence about the effectiveness of interventions which aim to improve school engagement for those from marginalised groups in the UK. We therefore conducted two pre-specified exploratory subgroup analyses for the primary outcome of school engagement, examining differential treatment effects by ethnicity and FSM status. Specifically, we assessed whether the effect of the BDP differed between (1) boys from White and minority ethnic backgrounds and (2) boys who were eligible for FSM and those who were not. As the trial was not powered to detect subgroup effects, these analyses were exploratory and should be interpreted with caution.

Subgroup analyses included an interaction term between allocation group and subgroup variable, as shown below:

$$Y_{i,T2} = \beta_0 + \beta_1 \text{Baseline}_i + \beta_2 \text{Group}_i + \delta^T \text{School}_i + \gamma_s \text{Subgroup}_i + \theta_s (\text{Subgroup}_i \times \text{Group}_i) + \varepsilon_i \in N(0, \sigma^2) \quad (4)$$

This uses the same variables as the primary analysis set out in Equation (1), with the following additional terms:

- Subgroup_i is a binary indicator for the subgroup under investigation (ethnicity or FSM status).
- θ_s represents the interaction between allocation group and subgroup, and tests whether the treatment effect differs between subgroups.

Analytical extension: subgroup analysis

Extension from Time 2 to Time 3

Subgroup analyses for ethnicity and FSM eligibility were conducted at T2, as pre-specified in the SAP. These analyses were then extended from the primary time point (T2) to the later follow-up (T3) to assess the presence of any differences in outcomes by demographic group after the programme was completed. This was run to make full use of available data and to shed light on the BDP's potential mechanisms and the

equity of impact over time. However, they were not pre-specified and are, therefore, reported as exploratory and hypothesis-generating. Their inclusion is not considered to compromise the rigour or interpretability of the trial, as all core analyses and models remained consistent with the SAP.

Collapsing ethnicity categories

In the SAP, we proposed conducting subgroup analysis to compare all minority ethnic groups. However, it was not feasible to conduct separate analyses for each group with sufficient statistical power. To address this, we revised our approach and conducted subgroup analyses to compare boys from White backgrounds with boys from all minority ethnic backgrounds combined as one group. This grouping maximised statistical power while still enabling exploratory examination of differential effects by ethnicity.

This deviation from the pre-specified analysis plan is considered analytically justifiable, given the sample constraints, but results should be interpreted with caution. We recognise that combining boys from all minority ethnic groups into a single category risks obscuring important differences in the experiences and outcomes of specific groups because, in reality, there is significant diversity across cultural backgrounds, lived experiences and systemic barriers faced.

To mitigate this limitation, all descriptive statistics are presented using disaggregated ethnicity categories in line with ONS data harmonisation standards, and the qualitative analysis explores differences between groups where relevant and feasible. The decision to collapse categories was therefore limited to subgroup impact analyses and driven solely by statistical power considerations. Future research with larger samples would be required to robustly examine differential effects across specific ethnic groups.

Additional subgroup analyses

In addition to the pre-specified comparison between boys from White backgrounds and boys from minority ethnic backgrounds, we conducted an additional exploratory subgroup analysis focusing specifically on boys identified as Black Caribbean or mixed White and Black Caribbean. This extension was informed by national evidence highlighting the disproportionate rates of exclusion and school disengagement experienced by this group. Given these known disparities in educational and disciplinary outcomes, this analysis aimed to explore whether the impact of the BDP differed for boys from Black Caribbean and dual Caribbean heritage backgrounds compared with all other participants.

We also conducted exploratory moderation analysis by baseline level of risk, defined as having experienced at least one exclusion at baseline (≥ 1 exclusion vs. 0 exclusions). This analysis was designed to assess whether the programme's impact differed for boys who entered the trial at higher observable risk of school disengagement.

As with all subgroup analyses, these comparisons were exploratory and should be interpreted cautiously due to limited statistical power and modest subgroup sample sizes.

2.4.5 Analysis in the presence of non-compliance

In line with the YEF guidance, our primary and secondary impact analysis followed the ITT principle, in which boys were analysed according to their randomised allocation regardless of their level of participation. ITT provides an unbiased estimate of the effect of being offered the BDP but may underestimate the potential efficacy of the intervention if some participants do not adhere to their assigned treatment.

To explore whether programme effects varied with attendance, we estimated complier average causal effect (CACE) models using two-stage least squares regression.

The CACE model took the following form:

1. The first stage regressed compliance (defined in the box below) on the same covariates used for the primary analysis (i.e. baseline scores, school and treatment allocation). This was a linear regression model used to generate predicted compliance.
2. The second stage model regressed the SCQ total score on treatment allocation, baseline SCQ scores, school and the predicted values for compliance from each stage.

Two CACE specifications were run for the primary outcome (school engagement):

1. **Continuous model**, including the number of sessions received in Stage 1 in order to estimate the marginal effect per additional session on school engagement
2. **Binary model**, including a compliance status variable in stage 1, which equalled 1 for boys who received 11 or more sessions and 0 for boys who received 10 or fewer sessions, representing a high-engagement threshold close to the intended dosage

These analyses were run at T2, and then also at T3 to assess changes at follow-up. Both specifications showed strong instrument validity ($F > 100$) and consistent results across estimation methods.

Analytical extension: complier average causal effect analysis

Binary and continuous models

The SAP pre-specified a CACE analysis at T2, using a continuous indicator of the number of BDP sessions attended to assess the marginal effect of an additional BDP session on school engagement.

In practice, two models were conducted to explore compliance effects in full. The first followed the pre-specified continuous specification, estimating the marginal effect of each additional session attended on the SCQ total score. A second binary specification was added post-hoc, comparing boys who received 11 or more sessions (i.e. close to the intended maximum dosage of 12 sessions) with those who received 10 or fewer. This threshold model was introduced to examine whether the dose–response effect was linear and to reflect the clustering of attendance between eight and 13 sessions to explore whether high engagement produced distinct benefits. The threshold of 11 sessions was selected as it represents a level of exposure sufficiently close to the full intended dose to constitute a meaningful distinction while also maintaining an adequate sample size compared to a stricter definition based on completing all 12 sessions. Further detail on patterns of session delivery, average dosage and considerations around what constitutes a sufficient dose in practice is provided in the IPE (see Chapter 5).

Time 2 and Time 3 analyses

Both CACE specifications were run at T2 and again at T3 to examine whether effects among compliers emerged, strengthened or diminished over time. These analyses were exploratory and complementary to the ITT analysis. Given that randomisation was retained as the instrument and the analytic framework followed established conventions, this extension is viewed as methodologically appropriate and is not judged as compromising trial integrity.

2.4.6 Mechanism of change analysis

In line with the BDP's theory of change, the SSRS was included as a post-intervention measure of perceived relationship quality between boys and their project co-ordinators or equivalent adults. The SSRS was not conceptualised as an outcome but as a potential mechanism through which the BDP might generate change in school engagement.

The SSRS was administered at follow-up (T2 and T3) to all boys in the BDP delivery group and to a subsample of boys in the signposting group who reported having a regular relationship with a trusted adult (e.g. grandparent, older sibling or teacher). Average scores were computed for each subscale for boys who had completed at least 80% of the relevant items.

Following the approach outlined in the SAP and drawing on Gunzler et al. (2013), exploratory analyses were conducted, which used a mediation analysis framework to estimate both the direct and indirect effects of allocation to the BDP on school engagement. Analyses were conducted in two sequential stages:

Stage 1: predictive models

Firstly, we examined which aspects of relationship quality at T2 were most strongly associated with school engagement at T3. Separate linear regression models were estimated for each SSRS subscale at T2 (feels valued, mentoring, trust and negativity) to predict school engagement (measured by the SCQ total score) at T3, controlling for baseline school engagement, school and randomisation group. Each model took the following form:

$$Y_{i,T3} = \beta_0 + \beta_1 SSRS_{i,T2} + \beta_2 Group_i + \delta^T School_i + \beta_3 Baseline_{i,T1} + \varepsilon_i \quad (5)$$

Where:

- $SSRS_{i,T2}$ is each of the SSRS's subscale scores at T2

A combined model was then run that included all four subscales simultaneously to identify which relationship dimension remained a unique predictor of later school engagement:

$$Y_{i,T3} = \beta_0 + \beta_1 FeelsValued_{i,T2} + \beta_2 Mentoring_{i,T2} + \beta_3 Trust_{i,T2} + \beta_4 Negativity_{i,T2} + \beta_5 Group_i + \beta_6 School_i + \beta_7 Baseline_{i,T1} + \varepsilon_i \quad (6)$$

From this analysis, the mentoring subscale emerged as the strongest and most distinctive predictor of school engagement at T3 and was therefore selected as the mediator for subsequent analyses.

Stage 2: mediation model

To formally test whether mentoring relationships acted as a mechanism linking BDP participation to later engagement, a causal mediation analysis was conducted. The analysis estimated the average causal mediation effect (ACME; i.e. the indirect effect of allocation to the BDP on T3 engagement, which operates via mentoring quality) and the average direct effect (ADE), which is the remaining effect of allocation to the BDP on school engagement which does not operate through mentoring. The two constituent models were:

Mediator model:

$$Mentoring_{i,T2} = \alpha_0 + \alpha_1 Group_i + \delta^T School_i + \alpha_2 Baseline_{i,T1} + v_i \quad (7)$$

Outcome model:

$$Y_{i,T3} = \beta_0 + \beta_1 Mentoring_{i,T2} + \beta_2 Group_i + \delta^T School_i + \beta_3 Baseline_{i,T1} + \varepsilon_i \quad (8)$$

Indirect and direct effects were estimated using nonparametric bootstrapping, with 5,000 resamples to generate bias-corrected 95% CIs. Statistical significance was set at $p < 0.05$.

This approach allowed us to assess whether the BDP's effects on school engagement occurred indirectly, via improvements in mentoring relationships or through a direct pathway independent of relationship quality. The regression and mediation models combined provide a rigorous test of the programme's proposed mechanism of change: whether participation in the BDP enhances mentoring relationships with a trusted adult, which in turn promotes stronger school engagement.

2.4.7 Analyses of school administrative data

Analyses of school administrative data were exploratory and conducted only for the five schools which provided sufficiently complete and comparable datasets. These analyses were exploratory because they were not pre-specified in the SAP and because it is likely that these data will vary in quality, accuracy and comparability across schools. This is because schools have different behaviour and exclusion policies and may apply different recording practices across attendance and exclusion incidents.

Attendance outcomes were analysed using ANCOVA models, while exclusion outcomes were analysed using negative binomial regression models to account for skewed, over-dispersed count distributions.

Attendance data

We calculated the percentage of sessions (defined as a half-day of school) attended for each half-term. For analysis we defined the following two data collection periods:

- **P1** = the two half terms before the treatment period
- **P2** = the two half terms immediately after the treatment period.

Attendance outcomes were analysed using an **ANCOVA model**, which is consistent with the approach taken for the trial's primary and secondary outcomes. Attendance measures are continuous percentages between 0 and 100; ANCOVA provides unbiased estimates of treatment effects while adjusting for baseline values and school-level stratification. This took a similar form to the primary and secondary analyses and used the following model for pupil i in school j :

$$A_{i,P2} = \beta_0 + \beta_1 A_{i,P1} + \beta_2 Group_i + \delta^T School_i + \varepsilon_i \in N(0, \sigma^2) \quad (9)$$

Where:

- $A_{i,P2}$ and $A_{i,P1}$ are the average attendance rates for P2 and P1, respectively

The parameter of interest is β_2 , the adjusted mean difference in average attendance between boys in the BDP and signposting groups.

Exclusions data

Exclusion data included all disciplinary incidents recorded by schools, covering fixed-term suspensions, internal suspensions, managed moves, off-rolling, being directed off-site and permanent exclusions. These were aggregated into two composite outcome measures for analysis (as below). This approach was considered the most defensible and analytically robust, given the wide variation in how schools define and record disciplinary events; aggregating these events into a single measure maximised data availability and reduced potential measurement error.

Two exclusion analyses were conducted:

- **Total number of incidents** (i.e. count of all exclusion events), comparing the total number of exclusionary events across groups. We recognise that aggregating exclusion outcomes means that all incidents are counted equally, regardless of severity, and this is a limitation that should be factored into the interpretation. However, given the variation in disciplinary practices and recording across schools, we prioritised a consistent, comparable measure based on the number of incidents, rather than imposing severity weightings that may not be equivalent across contexts. This analysis was conducted using a negative binomial regression model which took the following form:

$$\log(\mathbb{E}[E_{i,P2}]) = \beta_0 + \beta_1 E_{i,P1} + \beta_2 \text{Group}_i + \delta^T \text{School}_i \quad (10)$$

Where:

- $E_{i,P2}$ is a count indicator of the number of incidents boy i experienced at P2
- $E_{i,P1}$ is a count indicator of the number of incidents boy i experienced at P1
- **Total duration of suspensions** (i.e. the number of days boys were excluded for, either as part of an internal suspension or a fixed term suspension). For suspension duration, analyses were restricted to boys who experienced at least one fixed-term suspension during the study period (i.e. > 0 suspension days at either baseline or follow-up). This restriction was applied to examine whether participation in the BDP was associated with changes in the length of suspension among boys who were subject to fixed-term exclusion. Suspension duration at follow-up (P2) was analysed using an ANCOVA model, with P2 suspension days regressed on baseline suspension days, randomisation group and school, taking the following form:

$$D_{i,P2} = \beta_0 + \beta_1 D_{i,P1} + \beta_2 \text{Group}_i + \delta^T \text{School}_i + \varepsilon_i \quad (11)$$

Where:

- $D_{i,P2}$ is the total number of days a boy was suspended for at P2

- $D_{i,P1}$ is the total number of days a boy was suspended for at P1

Suspension duration analyses were restricted to boys who had experienced internal and fixed-term suspensions. As permanent exclusions do not have a defined duration, boys who were permanently excluded were therefore excluded from this analysis but remained in the exclusion incident analysis.

These models were selected to align with the trial's main analytic framework while accommodating for the statistical properties of administrative outcomes. Exclusions are highly skewed, with a substantial number of zeroes: Negative binomial regression accounts for the over-dispersion in counts of exclusion incidents more effectively than a standard Poisson model. These analyses are exploratory and will be interpreted with caution, particularly given the potential variability in how schools record administrative data.

2.4.8 Missing data analyses

Throughout the trial, the evaluation team worked closely with the Future Men team to support the collection of high-quality and complete data for all boys. However, missing data may occur due to either item non-completion or sample attrition (i.e. boys who do not complete either T2 or T3 questionnaires).

In line with [YEF analysis guidance](#), we examined both the proportion of missing data in the trial and the extent and pattern of missingness in the data.

This involved an analysis of whether data were missing completely at random (MCAR), missing at random (MAR) or missing not at random (MNAR). MCAR and MAR mean that complete cases are unlikely to be biased subsequent to adjustment but may be underpowered, while MNAR suggests that structural bias has been introduced into the sample.

We sought to establish the missing mechanism (i.e. which variables in the data were predictive of non-response) through logistic regression models. These modelled the presence of missing outcomes data with additional information that may be predictive of missingness. This analysis was done in line with the flow chart in Figure 1 in the [YEF analysis guidance](#), which outlines the following approach:

- If the prevalence of missing data is less than 5%, no further action is required, as complete case analysis is unlikely to be biased.
- If outcomes data are MAR conditional on co-variables, include these co-variables in the primary analysis model and discuss the implications in full.
- If a covariate is MAR conditional on other covariates, conduct multiple imputation (MI). Treatment effects from the MI analysis will be reported in addition to estimates from the model outlined in section 2.4.1. The implications of any differences between the complete case analysis and the MI analysis should be discussed in full.
- If missing data cannot be fully explained by other variables in the dataset, data are likely to be MNAR. In this scenario, conduct a sensitivity analysis alongside the primary impact analyses.

The above analyses were completed for the primary outcome at T2 and T3 in line with YEF guidance. Further details and the results of our approach to examining missing data and mechanisms of missingness are set out in section 0.

2.4.9 Estimation of effect sizes

Effect sizes were calculated using Hedges' g , following standard practice:

$$g = J \times \frac{(\bar{Y}_T - \bar{Y}_C)_{adjusted}}{SD_{pooled}} \quad (12)$$

Where:

- $(\bar{Y}_T - \bar{Y}_C)_{adjusted}$ is the ANCOVA difference in means between the treatment and control groups adjusted for baseline outcomes measures and school, as specified in the primary and secondary outcomes models
- SD_{pooled} is the unconditional pooled standard deviation (SD) of the two groups¹⁰
- J is the small-sample correction factor applied to reduce the positive bias in the standardised mean difference when estimated from finite samples.

CIs were derived using the following formula:

$$g \pm \Phi^{-1}\left(1 - \frac{\alpha}{2}\right) g_{se} \quad (13)$$

Where:

- Φ^{-1} is the percent point function of the normal distribution
- g_{se} is the standard error of the g statistic (noted as ES above)¹¹

All estimates were interpreted according to the YEF's impact rating thresholds, as set out in Table 1 above, and uncertainty was expressed through 95% confidence intervals (CIs).

2.5 Implementation and process evaluation methods

2.5.1 Research questions

We conducted an IPE alongside the RCT to provide complementary insights into the implementation, delivery and perceived impact of the BDP between January and July 2025. In line with the EEF's (2022) and the YEF's (2021) guidance on IPEs, the IPE was designed to understand how the BDP was delivered in practice, identify contextual enablers and barriers, and explore boys' experiences of support.

¹⁰ $SD_{pooled} = \sqrt{\frac{(n_1-1)sd_1^2 + (n_2-1)sd_2^2}{n_1 + n_2 - 2}}$, where n_1 and n_2 are the sample size for groups 1 and 2 respectively, and sd_1 and sd_2 are the standard deviations of group 1 and group 2 respectively.

¹¹ $g_{se} = \sqrt{\frac{n_1+n_2}{n_1n_2} + \frac{g^2}{2(n_1+n_2)}}$

The key research questions that the IPE sought to answer were:

1. Dimensions of implementation. How effectively has the BDP been implemented in schools?

- **a. Fidelity.** To what extent has support been delivered in line with the BDP's theory of change and protocols?
- **b. Dosage.** How much of the BDP has been delivered? How much of the BDP needs to be delivered to have an impact?
- **c. Quality.** How well has the BDP been delivered?
- **d. Reach.** How well has the BDP reached its intended cohort?
- **e. Responsiveness.** To what extent have young people engaged with the intervention?
- **f. Intervention differentiation.** To what extent is the BDP sufficiently different from existing practices within schools?
- **g. Adaptation.** Are any changes needed to accommodate context and need?

2. Factors affecting implementation. Which factors have acted as enablers of or barriers to implementation of the BDP?

- **a. Community-level factors.** Which factors have impacted implementation at the school and wider community levels? For example, level of need, readiness for change and/or policy, practice and funding context?
- **b. Organisation-level factors.** Which factors have impacted implementation at the organisational level? For example, capacity, skills and training, co-ordination, and resources?
- **c. Unexpected factors.** Which other factors have had an impact?

3. Experiences of support. What are young people's experiences of support?

- a. Which aspects of the BDP have supported positive outcomes?
- b. How have experiences of support differed across subgroups, e.g. those from racially minoritised/marginalised backgrounds, from low income households or with SEND?

4. Guidelines for future implementation. What are the implications for future replication, scale and spread?

2.5.2 Research methods

The IPE adopted a mixed-methods design, integrating qualitative and quantitative data to provide a comprehensive understanding of implementation. The approach drew on established process evaluation frameworks (Carroll et al., 2007; Moore et al., 2015) that emphasise fidelity, dose, reach, quality and responsiveness as key dimensions of implementation quality.

Consistent with the EEF's and the YEF's IPE guidance, qualitative evidence from interviews and observations was triangulated with quantitative monitoring data from the RCT to assess how the BDP was delivered, experienced and adapted in context. This mixed-methods approach supported the development of evidence-informed recommendations for future delivery and evaluation.

Table 12 summarises the IPE methods and their contributions to the evaluation questions.

Table 12: Implementation and process evaluation methods

Research methods	Data collection	Participants	Analysis	Research questions	Implementation relevance
Data analysis	Activity and dosage data collected by BDP project co-ordinators	Boys who received the BDP (n = 245) and boys who did not (n = 242) ¹²	Descriptive statistics and bivariate analysis	RQ1	Dimensions of implementation
Semi-structured interviews	Interviews with boys	28 boys, involving a range of ages, year groups and ethnicities.	Thematic analysis	RQ1 to RQ4	Dimensions of implementation, factors affecting implementation, experiences of support and guidelines for future implementation
Semi-structured interviews	Interviews with Future Men stakeholders	Four key BDP stakeholders, including project co-ordinators, project managers and strategic staff.	Thematic analysis	RQ1 to RQ4	Dimensions of implementation, factors affecting implementation, experiences of support and guidelines for future implementation
Semi-structured interviews	Interviews with school stakeholders	Nine interviews involving school pastoral leads, SENCos and deputies.	Thematic analysis	RQ1 to RQ4	Dimensions of implementation, factors affecting implementation, experiences of support and guidelines for future implementation

Note: BDP = Boys' Development Programme; RQ = research question; SENCos = Special Educational Needs and Disabilities Coordinator

¹² Please note that two boys were allocated to the signposting group but mistakenly received the BDP. In the impact analysis, they have been analysed per their allocated group in line with our ITT approach. In IPE, they have been included in the analysis of BDP implementation for completeness.

Interviews with boys

We conducted in-depth, semi-structured interviews with 28 boys from the BDP delivery group between April and June 2025. This sample size was selected to balance depth and diversity, consistent with EEF guidance on achieving data saturation in process evaluations.

Sampling was purposive to ensure variation across schools, age groups, ethnic backgrounds and levels of engagement with the BDP (e.g. boys completing all sessions versus those disengaging early). While the achieved sample broadly reflected the demographic composition of the wider cohort, the intention was not to achieve statistical representativeness. Rather, the aim was to capture a range of perspectives sufficient to explore implementation processes and mechanisms of change. Table 13 provides an overview of the characteristics of the boys who took part in interviews, compared to the overall recruited cohort.

Table 13: Demographic characteristics of young people in the implementation and process evaluation (IPE) interview sample (n = 28) compared to the total recruited (Time 1 and randomised) cohort (n = 486)

	IPE sample (n = 28)		Total cohort (n = 486)
Ethnicity	n	%	%
Asian or Asian British	1	3.6%	5.8%
Black, African, Caribbean or Black British	10	35.7%	55.1%
Mixed or multiple ethnic groups	4	14.3%	12.3%
Other ethnic group	2	7.1%	7.2%
White	6	21.4%	17.1%
Not known	5	17.9%	2.5%
Sex	n	%	%
Male	28	100.0%	100.0%
Female	0	0.0%	0.0%
Non-binary	0	0.0%	0.0%
Age	M		M
Age	12.5		12.9
Free school meal eligibility			
Yes	10	50.0%	62.6%
No	14	35.7%	36.2%
Not known	4	14.3%	1.2%
Special educational needs and disabilities	n	%	%
Yes	7	25.0%	33.5%
No	18	64.3%	66.5%
Not known	3	10.7%	-

	IPE sample (n = 28)		Total cohort (n = 486)
School	n	%	%
School A	5	17.9%	15.8%
School B	3	10.7%	17.1%
School C	5	17.9%	16.5%
School D	0	0.0%	8.0%
School E	6	21.4%	8.2%
School F	2	7.1%	8.8%
School G	0	0.0%	9.1%
School H	7	25.0%	16.5%

Interviews explored boys' experiences of BDP delivery; relationships with project co-ordinators; perceived changes in school engagement, attitudes or behaviour; and suggestions for improvement. Topic guides were informed by the BDP's theory of change to ensure coherence and relevance. We used Cordis Bright's internal [Equality Diversity and Inclusion Toolkit](#) to ensure that all topic guides were designed through the lens of racial and cultural sensitivity and were accessible to all participants. These topic guides (see Appendix G) were shared and refined with Future Men and YEF colleagues before use.

Informed consent was obtained from both parents/carers and boys. Interviews were typically 30 minutes in duration and conducted by the research team, either face-to-face in schools during lesson time, via telephone or via video call, depending on logistical and safeguarding considerations. To minimise social desirability bias, project co-ordinators were not present during interviews, although boys were given the option to have another member of the Future Men team there for support if they chose to. However, in practice, no boys chose to have them present.

All interviewers were trained and experienced in conducting research with boys in school settings and were trained in safeguarding and ethical interviewing. Boys were provided with the option to have the conversation recorded or for the interviewer to take detailed contemporaneous notes. All notes and transcripts were stored on Cordis Bright's secure server in a password protected file, which was only accessible to evaluation team members. All boys were provided with a £20 high street voucher as a thank you for taking part in the interview.

A note on the use of pseudonyms

All boys quoted in this report have been assigned pseudonyms to protect participants' privacy. These pseudonyms are used consistently throughout to help readers follow who is speaking. In addition, schools have been assigned anonymised identifiers so that readers can observe school-level variations without revealing the identity of individual institutions or boys.

Other personal characteristics, such as age and ethnicity, have not been included alongside quotes to protect anonymity. However, readers should note that all boys who participated in IPE interviews were aged 11–14 and came from a range of ethnic backgrounds, as set out in Table 13.

Interviews with Boys' Development Programme and school stakeholders

We conducted semi-structured interviews with a sample of Future Men staff (n = 4; including project co-ordinators, managers and strategic leads) and a sample of school stakeholders (including SENCos, pastoral leads, heads of year and deputy heads; n = 9). Participants were identified using purposive sampling to ensure representation from all participating schools.

Invitations and information sheets were shared in advance, and informed consent was obtained before participation. Interviews lasted 30 to 60 minutes and were conducted virtually. Topic guides were co-developed with Future Men and the YEF to ensure alignment with research objectives and to capture insights into dimensions of implementation, factors affecting implementation, experiences of support and guidelines for further implementation. During the interviews we took contemporaneous notes that are stored on our secure server and only accessible to research team members.

Activity data analysis

Quantitative monitoring data were drawn from Future Men's client relationship management system. This allowed the integration of typically collected delivery data into the evaluation, avoiding duplication and reducing burden on project co-ordinators. Monitoring data provided by Future Men included:

- Programme information on all boys who were referred to the BDP (including numbers of referrals, accepted as eligible, consented, not consented, started, completed or dropped out)
- Background characteristics for all boys who consented to be involved in the evaluation (including age, ethnicity, sex and disability)
- Activity data, including dosage of support for boys in the treatment group (number of sessions and length of support) and topics covered in each session

These data were used to produce descriptive statistics and cross-tabulations to assess fidelity, reach and dosage. Data were securely transferred to Cordis Bright via Egress and stored on encrypted servers in accordance with GDPR.

2.5.3 Analysis

Qualitative data were coded using a thematic analysis approach (Braun & Clarke, 2006). Transcripts and notes were entered into a matrix mapping responses against the IPE research questions. A hybrid coding framework was applied, combining a priori codes based on the theory of change and implementation framework (Carroll et al., 2007), with inductive coding to capture emergent themes.

The qualitative analysis was conducted with attention to researcher reflexivity. The evaluation team comprised individuals from a range of ethnic backgrounds, and members of the evaluation team engaged in structured discussions throughout coding and theme development to reflect on how their professional backgrounds, prior experience and assumptions about exclusion and disadvantage might shape interpretation. Coding decisions and emerging themes were reviewed collaboratively to minimise individual bias and to ensure that interpretations remained grounded in participants' accounts rather than evaluative expectations. Particular care was taken to avoid deficit-based framing of boys' experiences and to situate findings within broader school and community contexts. This reflexive process formed part of the study's wider commitment to equity-informed evaluation practice.

In addition to examining implementation fidelity and participant experience, the analysis considered how implementation and boys' experiences were shaped by wider school and community contexts. This included attention to school disciplinary practices, relationships with staff, access to support and broader environmental factors described by participants. The IPE was not designed to identify or explain the root causes of structural inequalities. Rather, it explored how structural conditions were experienced and navigated by participants within the context of programme delivery. Particular care was taken not to interpret challenges or outcomes solely at the individual level but to situate them within structural conditions affecting engagement and exclusion. Findings are therefore interpreted as experiential insights into how context shapes engagement and implementation, rather than as causal explanations of structural disparities.

Quantitative data were analysed in Excel using descriptive statistics and bivariate analyses (frequencies, percentages and cross-tabulations).

To ensure that data were not analysed in isolation, qualitative and quantitative findings were triangulated, mapping both strands of evidence to the IPE research questions and the BDP theory of change. This integrated analytic strategy enabled a comprehensive assessment of implementation quality and participant experience, including the extent to which this varied across groups and contexts.

3 Impact evaluation results

3.1 Overview

This chapter sets out the impact evaluation results from the efficacy study. It presents:

- **Participant characteristics and flow through the trial**, including an assessment of balance at randomisation, attrition rates and participant flow through the trial
- **Outcomes analysis**, including primary, secondary, subgroup and further analyses and a discussion on missing data and analysis in the presence of non-compliance

3.2 Participant characteristics and flow through the trial

3.2.1 Participant flow

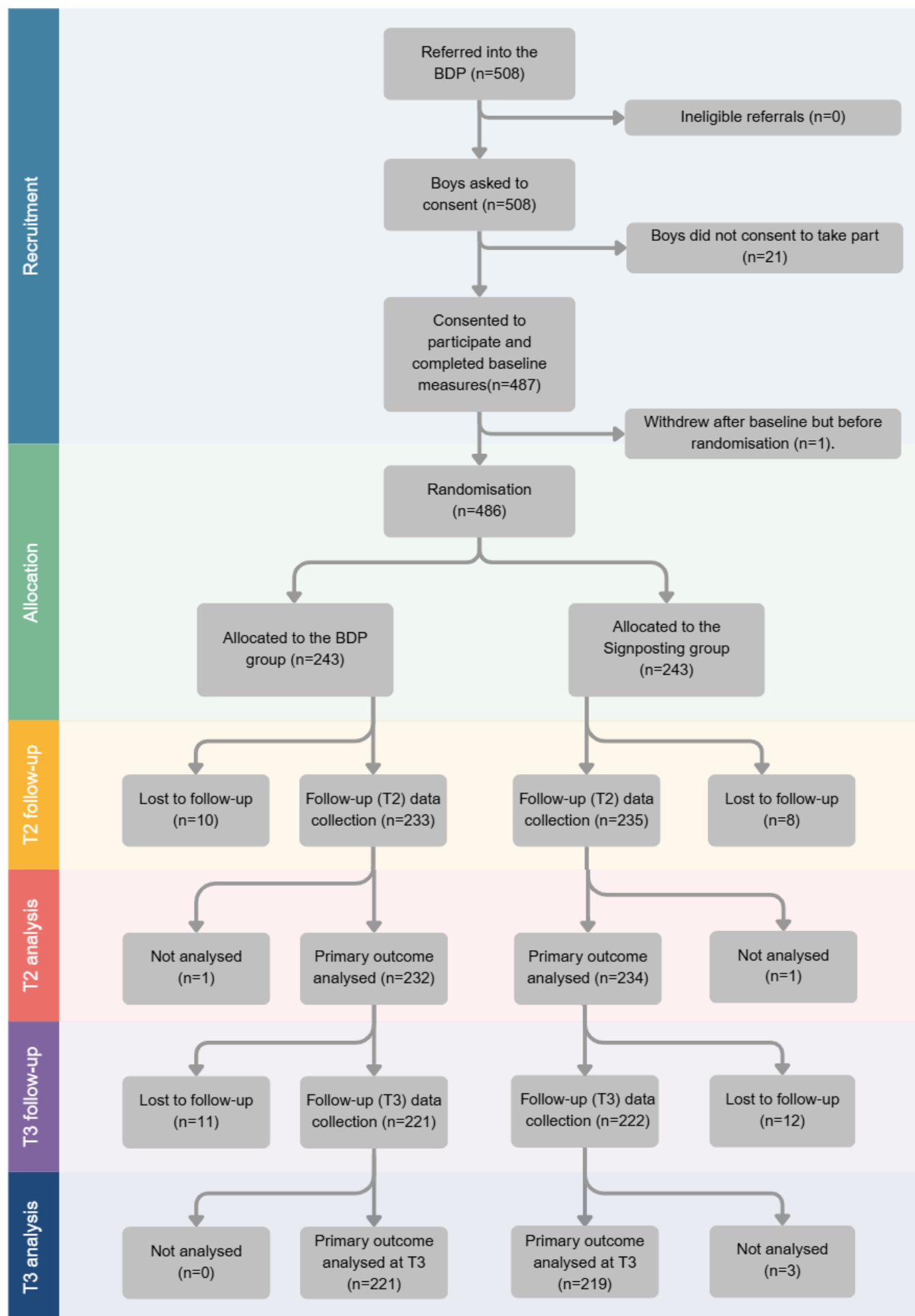
Figure 2 sets out the CONSORT diagram for the efficacy study, which shows participant flow from referral into the trial through to data collection at T3.

This shows that the recruitment, allocation, follow-up and analysis rates for participants in the efficacy study were as follows:

- A total of **508 boys were referred** into the BDP trial, all of whom were deemed eligible.
- Of these, **487 boys consented** to participate and completed baseline measures.
- One boy withdrew after completing baseline but before randomisation, resulting in **486 boys randomised** to either the BDP delivery group (n = 243) or the signposting group (n = 243).
- At T2 follow-up, **468 boys completed surveys**. This included 233 boys in the BDP delivery group and 235 boys in the signposting group.
- Of these, **466 boys were included in the final T2 analysis sample** for the primary outcome (SCQ): 232 in the BDP delivery group and 234 in the signposting group. Boys were excluded if they had missing or incomplete outcome data, which applied to two boys in total. This is in line with our complete-case analysis approach (see Chapter 6).
- **At T3 follow-up, 443 boys completed surveys**. This included 221 boys in the BDP delivery group and 222 boys in the signposting group.
- Of these, **440 boys were included in the T3 analysis sample** for the primary outcome (SCQ): 221 in the BDP delivery group and 219 in the signposting group. Three boys were excluded due to item non-completion.

It should also be noted that two boys were allocated to the signposting group but subsequently went on to receive the full BDP due to an administrative error. In line with the trial's pre-specified ITT approach, these participants were analysed according to their allocation group (signposting group) rather than the treatment they received.

Figure 2: CONSORT diagram for the Boys' Development Programme efficacy study



3.2.2 Participant characteristics at randomisation

Table 14 sets out the baseline characteristics of participants at randomisation, broken down by their allocation group. This includes age, school year, sex and ethnicity, alongside baseline scores on the primary and secondary outcome measures.

Theoretically, randomisation should create two groups that are broadly equivalent at the start of a trial. This means that any differences found later on can be more confidently attributed to the intervention, rather than to pre-existing differences between the groups. However, it is still important to check this balance in practice. Randomisation works by chance, and with smaller samples, there is always a possibility that one group could end up with more boys of a certain age, ethnicity or baseline level of difficulties than the other. Such imbalances do not undermine the validity of a randomised trial because they arise purely by chance and can be adjusted for analytically (e.g. through ANCOVA). Nonetheless, reporting baseline balance provides reassurance about the fairness of the comparison, supports transparent interpretation of the findings and helps to rule out alternative explanations for the results. Checking for baseline balance therefore provides reassurance about the fairness of the comparison and helps guard against alternative explanations for the results.

Overall, Table 14 suggests that the two trial arms were well balanced across demographic and outcome measures, suggesting that randomisation was successful.

- **Age and school year.** The distribution of boys across ages 11 to 16 and school years 7 to 11 was broadly similar between the BDP and signposting groups. The largest proportion of participants were aged 12 to 13 years (around half of the total sample), reflecting the intended focus on younger year groups. No notable differences in age or year distribution were observed between groups.
- **Sex.** In line with the eligibility criteria for the BDP, all participants (100%) were male, with no differences between groups.
- **Ethnicity.** Both groups had a similar ethnic composition. Across the trial sample, just over half the boys identified as Black, Black British, Black Welsh, Caribbean or African (56% to 58% across arms), with smaller proportions from Asian, Asian British or Asian Welsh backgrounds (6%), mixed or multiple ethnic backgrounds (12% to 13%), other ethnic groups (7% to 8%) and White backgrounds (17% to 18%). These proportions were very closely matched between groups.
- **FSM eligibility.** The proportion of boys eligible for FSM was broadly similar across the two groups, indicating good balance on this key indicator of socioeconomic disadvantage. Around two-thirds of boys in the signposting group were eligible for FSM (66.0%) compared to just over three-fifths in the BDP delivery group (60.7%). While this represents a small difference between groups, it is within the range expected by chance and does not indicate any meaningful imbalance at baseline.
- **Baseline outcome measures.** Mean baseline scores were also closely aligned between the two groups for both the primary and secondary outcomes. Any differences were negligible: SCQ total (25.0 vs 25.3), externalising behaviours (9.2 vs 9.2), emotional problems (3.0 vs 3.0) and peer problems (2.2 vs 2.4). Effect sizes were all very small (–0.07 to –0.01), with CIs spanning zero, further confirming balance at baseline.
- **Baseline administrative data (exclusions and attendance).** For the subset of schools that provided administrative data, baseline indicators for exclusions and attendance (considering the term before

randomisation) were also closely aligned between groups. The proportion of boys with at least one exclusion at baseline was similar across arms (28.5% in the BDP delivery group vs 31.0% in the signposting group), as was the proportion with no prior exclusions (71.5% vs 69.0%). Persistent absence at baseline (attendance $\leq 90\%$) was also comparable across groups (39.3% vs 37.4%). Mean baseline total incidents (0.4 vs 0.5) and mean baseline suspension duration (1.0 days in both groups) were nearly identical. These figures indicate that the two trial arms were well matched in terms of prior exclusion history and attendance risk at the point of randomisation.

The absence of systematic differences across characteristics between the groups at baseline **indicates that randomisation was effective**. This provides confidence in the internal validity of the trial, strengthening the assumption that any differences observed at follow-up can be attributed to the intervention rather than to pre-existing imbalances between groups.

Table 14: Participant characteristics at randomisation broken down by allocation group (n = 486)

	BDP delivery group		Signposting group		
Participant-level (categorical) ¹³	n/N (missing)	%	n/N (missing)	%	
Age	243/243 (0)		243/243 (0)		
11	38/243	15.6%	32/243	13.2%	
12	60/243	24.7%	77/243	31.7%	
13	64/243	26.3%	66/243	27.2%	
14	55/243	22.6%	41/243	16.9%	
15	25/243	10.3%	27/243	11.1%	
16	1/243	0.4%	0/243	0.0%	
School year	243/243 (0)		243/243 (0)		
Year 7	61/243	25.1%	69/243	28.4%	
Year 8	57/243	23.5%	58/243	23.9%	
Year 9	63/243	25.9%	61/243	25.1%	
Year 10	51/243	21.0%	44/243	18.1%	
Year 11	11/243	4.5%	11/243	4.5%	
Sex	243/243 (0)		243/243 (0)		
Male	243/243	100.0%	243/243	100.0%	
Female	0/243	0.0%	0/243	0.0%	

¹³ Additional baseline characteristics (e.g. accommodation type, looked after child status, physical disabilities, SEND status, current borough of residents) can be found in Table 16. Their baseline differences were negligible.

	BDP delivery group		Signposting group		
Ethnicity	238/243 (5)		236/243 (7)		
Asian, Asian British or Asian Welsh	14/238	5.9%	14/236	5.9%	
Black, Black British, Black Welsh, Caribbean or African	137/238	57.6%	131/236	55.5%	
Mixed or multiple ethnic groups	29/238	12.2%	31/236	13.1%	
Other ethnic group	18/238	7.6%	17/236	7.2%	
White	40/238	16.8%	43/236	18.2%	
Free school meal (FSM) eligibility	239/243 (4)		241/243 (2)		
Eligible for FSM	145/239	60.7%	159/241	66.0%	
Not eligible for FSM	94/239	39.3%	82/241	34.0%	
SEND status	241/243 (2)		235/243 (8)		
Yes	82/241	34.0%	77/235	32.8%	
No	159/241	66.0%	158/235	67.2%	
Cohort	243/243 (0)		243/243 (0)		
Cohort 1	62/243	25.5%	61/243	25.1%	
Cohort 2	60/243	24.7%	60/243	24.7%	
Cohort 3	61/243	25.1%	62/243	25.5%	
Cohort 4	60/243	24.7%	60/243	24.7%	
School exclusion data	158/243 (85)		158/243 (85)		
≥1 exclusion at baseline	45/158	28.5%	49/158	31.0%	
0 exclusions at baseline	113/158	71.5%	109/158	69.0%	
School attendance data	122/243 (121)		123/243 (120)		
Persistently absent at baseline (attendance ≤90%)	48/122	39.3%	46/123	37.4%	
Not persistently absent at baseline (attendance >90%)	74/122	60.7%	77/123	62.6%	

	BDP delivery group		Signposting group		
Participant-level (continuous)	n/N (missing)	Mean (SD)	n/N (missing)	Mean (SD)	Effect size
Average age	243/243 (0)	12.9 (1.24)	243/243 (0)	12.8 (1.19)	
SCQ total score	243/243 (0)	25.0 (2.5)	242/243 (1)	25.3 (2.6)	-0.07
SCQ teacher bonding score	240/243 (3)	6.4 (1.4)	240/243 (3)	6.5 (1.4)	0.05
SCQ peer bonding score	240/243 (3)	11.2 (1.1)	240/243 (3)	11.2 (1.1)	-0.04
SCQ school engagement subscale score	241/243 (2)	7.4 (1.4)	242/243 (1)	7.5 (1.2)	0.09
SDQ externalising behaviours score	243/243 (0)	9.2 (3.6)	242/243 (1)	9.2 (3.6)	-0.01
SDQ emotional problems score	243/243 (0)	3.0 (2.4)	242/243 (1)	3.0 (2.3)	-0.00
SDQ peer problems score	243/243 (0)	2.2 (1.4)	242/243 (1)	2.4 (1.5)	-0.01
Average attendance rates	116/243 (127)	90.4% (8.6)	118/243 (125)	90.6% (9.5)	
Average number of exclusion incidents at T1	158/243 (85)	0.4 (0.9)	158/243 (85)	0.5 (0.9)	
Average suspension duration at T1	158/243 (85)	1.0 (2.1)	158/243 (85)	1.0 (2.0)	

Note: BPD = Boys' Development Programme; SEND = special educational needs and disabilities; SD = standard deviation; SCQ = School Connectedness Questionnaire; SDQ = Strengths and Difficulties Questionnaire; T1 = Time 1

3.2.3 Baseline distributions of outcome measures

Histograms of baseline primary and secondary outcome measures were run to assess the distribution of baseline scores based on all participants with valid T1 data. These are provided in **Appendix B: Baseline distributions of outcome measures**.

Examining the distribution of outcome measures at baseline is important in a trial because it indicates whether the outcome measures will be able to capture meaningful variations across participants. If most boys score at the very top or bottom of a scale, there is little room left for improvement or deterioration to be detected. This can make it harder for the trial to identify real differences between groups, even if the intervention is having an effect. By contrast, measures with a wide spread across scores are more sensitive and better able to detect changes over time.

The histograms set out in Appendix B indicate variation in the extent to which measures captured a spread of responses across the cohort, which has implications for their sensitivity in detecting change. They show that:

- **Several measures showed a modest clustering of scores at the upper end of their scales**, indicating high baseline scores for many boys. The SCQ total score displayed a moderate negative skew, with most boys reporting high levels of school connectedness at baseline. A similar pattern was observed for the SCQ peer subscale, the SCQ engagement subscale and the SDQ pro-social behaviours subscale, in which responses were concentrated at the positive end of each scale. These negatively skewed distributions highlight a potentially small ceiling effect across these outcomes, possibly limiting the extent to which measurable improvement can be detected.
- **Emotional problems and peer difficulties (as measured by the SDQ) were concentrated at the lower end of their scales**, with most boys reporting few difficulties in these areas at baseline. This suggests that, for these outcomes, there may also be more limited room for improvement among boys.
- **Externalising behaviours scores and the SCQ teacher bonding subscale displayed more balanced distributions**, with scores spanning the full range and clustering around the mid-points of their respective scales. These more normally distributed measures demonstrated greater spread across low, moderate and higher scores, suggesting they may be comparatively more sensitive for detecting change. The SCQ teacher subscale captured a broad range of teacher-related connectedness, indicating that it may better reflect meaningful variation in baseline experiences than the more skewed peer- or engagement-related subscales.

It is also important to consider whether the modest clustering (outlined in the first bullet above) may suggest issues with programme targeting, and this question is explored further in the IPE. However, multiple sources of evidence indicate that the cohort reflected the intended population. Baseline administrative data showed that approximately 30% of the boys had experienced at least one exclusion at baseline and that average attendance was around 90%, with close to 40% of the boys persistently absent ($\leq 90\%$ attendance) in the term prior to randomisation. Baseline SDQ externalising scores also showed meaningful spread across the cohort. These indicators suggest that the SCQ distribution is unlikely to reflect substantial mistargeting. It may instead reflect features of self-report engagement measures in targeted samples, including potential ceiling effects or social desirability influences, which are considered when interpreting findings.

3.2.4 Attrition

This section presents participant-level attrition for the primary outcome across both follow-up points (T2 and T3).

Attrition rates are a key indicator of trial quality: high or differential dropout between treatment and control groups can introduce bias and undermine the comparability of groups, while low and balanced attrition supports confidence in the internal validity of the findings. For example, if boys with lower levels of school engagement were disproportionately lost to follow-up in one group, this could artificially inflate or mask differences in outcomes.

Table 15 shows:

- At the first follow-up (T2), **attrition was extremely low**, with 466 of 486 randomised participants (96%) included in analysis. Attrition rates were also closely matched across trial arms: 5% for the BDP delivery group (11 boys) and 4% for the signposting group (nine boys), a difference of two boys.
- At second follow-up (T3), **attrition remained low and balanced**, with 440 participants (91%) completing the primary outcome measure. Attrition rates were again well balanced across trial arms: 9% in the BDP delivery group (22 boys) and 10% in the signposting group (24 boys) – again a difference of 2 boys.

These patterns indicate that dropout was low, balanced and consistent across trial arms at both time points, substantially below the 12% attrition rate anticipated in the SAP. This provides strong reassurance that attrition is unlikely to have introduced systematic bias to the impact estimates and that the observed results can be interpreted with confidence. Further analysis on missing data is provided in section 0.

The low and even attrition rates reflect the success of Future Men’s data collection procedures and proactive follow-up with participants, as well as support and engagement from participating schools. This represents an important achievement in maintaining the integrity of the randomised design and ensuring a robust analytic sample for both the primary (T2) and longitudinal (T3) analyses. Further reflections on maintaining low attrition rates and learning for future studies are provided in the conclusion.

Table 15: Pupil-level attrition from the trial (primary outcome)

		BDP delivery group	Signposting group	Total
Number of participants randomised		243	243	486
Participant attrition at Time 2	Analysed at Time 2	232	234	466
	Number of missing participants	11	9	20
	Attrition rate from randomisation	5%	4%	4%
Participant attrition at Time 3	Analysed at Time 3	221	219	440
	Number of missing participants	22	24	46
	Attrition rate from randomisation	9%	10%	9%

Note: BDP = Boys’ Development Programme

3.2.5 Participant characteristics throughout the trial

Table 16 presents participant characteristics at three key stages of the trial disaggregated by trial arm: baseline (randomisation and T1), primary outcome analysis at T2 and primary outcome analysis at T3. The purpose of this table is to assess whether any differential attrition may have introduced bias into the trial findings.

Across all time points, the characteristics of boys in the BDP and signposting groups remained closely balanced in terms of age, ethnicity, socioeconomic background, school and other key indicators. This indicates that randomisation produced equivalent groups at baseline and that attrition did not meaningfully alter the composition of the sample available for analysis.

Specifically:

- **Age and year group.** The mean age at baseline was 12.9 years (SD = 1.22). The age and year-group distribution remained relatively stable across time points. Boys aged 12 to 14 made up the majority of the sample at both follow-ups.
- **Ethnicity.** The sample included boys from a range of minority ethnic backgrounds, with around 55% identifying as Black, Black British, Black Welsh, Caribbean or African, 17% as White and 12% as from mixed or multiple ethnic groups, consistent with the South London school context. The ethnic composition remained almost identical across groups and time points, and there was no evidence of disproportionate attrition by ethnicity.
- **Socioeconomic background and other characteristics.** Approximately 62% of participants were eligible for FSM, and this proportion was maintained at both T2 and T3, with minimal difference between arms. Similarly, proportions of pupils with SEND or an EHCP remained stable and comparable between groups. The small number of participants with care involvement (child in need or looked after child status) also remained proportionate over time.
- **School representation.** Participants were drawn from eight secondary schools. Representation from each school remained consistent across trial arms and time points, with no evidence that attrition was concentrated in particular schools or among boys attending certain types of schools.

The consistency of participant characteristics across randomisation, T2 and T3 provides strong reassurance that attrition did not introduce systematic bias. The demographic and school-level profiles of analysed samples closely reflect those of the randomised cohort, suggesting that any participant loss was random or evenly distributed across trial arms.

These findings, combined with the low and balanced attrition rates reported in Table 155 (4% at T2; 9% at T3), indicate that the internal validity of the trial is high. There is no evidence that selective dropout compromised the comparability of groups or biased the impact estimates.

Table 16: Demographic characteristics and flow through the trial¹⁴

Randomisation and T1 (n = 486)				T2 SCQ analysis (n = 466)			T3 SCQ analysis (n = 440)		
	Total n (%)	BDP n (%)	Signposting n (%)	Total n (%)	BDP n (%)	Signposting n (%)	Total n (%)	BDP n (%)	Signposting n (%)
Age									
11	70 (14.4%)	38 (15.6%)	32 (13.2%)	69 (14.8%)	37 (15.9%)	32 (13.7%)	68 (15.5%)	37 (16.7%)	31 (14.2%)
12	137 (28.2%)	60 (24.7%)	77 (31.7%)	133 (28.5%)	58 (25%)	75 (32.1%)	124 (28.2%)	54 (24.4%)	70 (32%)
13	130 (26.7%)	64 (26.3%)	66 (27.2%)	123 (26.4%)	62 (26.7%)	61 (26.1%)	120 (27.3%)	61 (27.6%)	59 (26.9%)
14	96 (19.8%)	55 (22.6%)	41 (16.9%)	92 (19.7%)	52 (22.4%)	40 (17.1%)	87 (19.8%)	48 (21.7%)	39 (17.8%)
15	52 (10.7%)	25 (10.3%)	27 (11.1%)	48 (10.3%)	22 (9.5%)	26 (11.1%)	41 (9.3%)	21 (9.5%)	20 (9.1%)
16	1 (0.2%)	1 (0.4%)	0 (0%)	1 (0.2%)	1 (0.4%)	0 (0%)	0 (0%)	0 (0%)	0 (0%)
Average age (M±SD)									
Age	12.9 ± 1.22	12.9 ± 1.24	12.8 ± 1.19	12.9 ± 1.20	12.9 ± 1.23	12.8 ± 1.18	12.8 ± 1.19	12.8 ± 1.21	12.8 ± 1.18
Ethnicity									
Asian, Asian British or Asian Welsh	28 (5.8%)	14 (5.8%)	14 (5.8%)	27 (5.8%)	13 (5.6%)	14 (6%)	26 (5.9%)	12 (5.4%)	14 (6.4%)
Black, Black British, Black Welsh, Caribbean or African	268 (55.1%)	137 (56.4%)	131 (53.9%)	259 (55.6%)	132 (56.9%)	127 (54.3%)	244 (55.5%)	124 (56.1%)	120 (54.8%)
Mixed or multiple ethnic groups	60 (12.3%)	29 (11.9%)	31 (12.8%)	56 (12%)	27 (11.6%)	29 (12.4%)	51 (11.6%)	26 (11.8%)	25 (11.4%)
Other ethnic group	35 (7.2%)	18 (7.4%)	17 (7.0%)	34 (7.3%)	17 (7.3%)	17 (7.3%)	33 (7.5%)	17 (7.7%)	16 (7.3%)
White	83 (17.1%)	40 (16.5%)	43 (17.7%)	78 (16.7%)	38 (16.4%)	40 (17.1%)	75 (17%)	37 (16.7%)	38 (17.4%)
Not known	12 (2.5%)	5 (2.1%)	7 (2.9%)	12 (2.6%)	5 (2.2%)	7 (3%)	11 (2.5%)	5 (2.3%)	6 (2.7%)

¹⁴ Please note that totals may not sum to 100% due to rounding.

Randomisation and T1 (n = 486)				T2 SCQ analysis (n = 466)			T3 SCQ analysis (n = 440)		
	Total n (%)	BDP n (%)	Signposting n (%)	Total n (%)	BDP n (%)	Signposting n (%)	Total n (%)	BDP n (%)	Signposting n (%)
Sex									
Male	486 (100.0%)	243 (100.0%)	243 (100.0%)	466 (100.0%)	232 (100.0%)	234 (100.0%)	440 (100.0%)	221 (100.0%)	219 (100.0%)
Female	0 (0.0%)	0 (0.0%)	0 (0.0%)	0 (0.0%)	0 (0.0%)	0 (0.0%)	0 (0.0%)	0 (0.0%)	0 (0.0%)
School									
School A	77 (15.8%)	38 (15.6%)	39 (16.0%)	73 (15.7%)	36 (15.5%)	37 (15.8%)	70 (15.9%)	35 (15.8%)	35 (16.0%)
School B	83 (17.1%)	40 (16.5%)	43 (17.7%)	79 (17.0%)	38 (16.4%)	41 (17.5%)	73 (16.6%)	34 (15.4%)	39 (17.8%)
School C	80 (16.5%)	40 (16.5%)	40 (16.5%)	76 (16.3%)	38 (16.4%)	38 (16.2%)	70 (15.9%)	36 (16.3%)	34 (15.5%)
School D	39 (8.0%)	20 (8.2%)	19 (7.8%)	39 (8.4%)	20 (8.6%)	19 (8.1%)	36 (8.2%)	18 (8.1%)	18 (8.2%)
School E	40 (8.2%)	21 (8.6%)	19 (7.8%)	40 (8.6%)	21 (9.1%)	19 (8.1%)	38 (8.6%)	21 (9.5%)	17 (7.8%)
School F	43 (8.8%)	22 (9.1%)	21 (8.6%)	41 (8.8%)	20 (8.6%)	21 (9.0%)	41 (9.3%)	20 (9%)	21 (9.6%)
School G	44 (9.1%)	22 (9.1%)	22 (9.1%)	39 (8.4%)	19 (8.2%)	20 (8.5%)	38 (8.6%)	18 (8.1%)	20 (9.1%)
School H	80 (16.5%)	40 (16.5%)	40 (16.5%)	79 (17%)	40 (17.2%)	39 (16.7%)	74 (16.8%)	39 (17.6%)	35 (16%)
School year									
Year 7	130 (26.7%)	61 (25.1%)	69 (28.4%)	128 (27.5%)	60 (25.9%)	68 (29.1%)	123 (28%)	58 (26.2%)	65 (29.7%)
Year 8	115 (23.7%)	57 (23.5%)	58 (23.9%)	111 (23.8%)	55 (23.7%)	56 (23.9%)	106 (24.1%)	53 (24%)	53 (24.2%)
Year 9	124 (25.5%)	63 (25.9%)	61 (25.1%)	116 (24.9%)	59 (25.4%)	57 (24.4%)	113 (25.7%)	58 (26.2%)	55 (25.1%)
Year 10	95 (19.5%)	51 (21%)	44 (18.1%)	92 (19.7%)	50 (21.6%)	42 (17.9%)	84 (19.1%)	45 (20.4%)	39 (17.8%)
Year 11	22 (4.5%)	11 (4.5%)	11 (4.5%)	19 (4.1%)	8 (3.4%)	11 (4.7%)	14 (3.2%)	7 (3.2%)	7 (3.2%)
FSM eligibility									
Yes	304 (62.6%)	145 (59.7%)	159 (65.4%)	288 (61.8%)	137 (59.1%)	151 (64.5%)	267 (60.7%)	128 (57.9%)	139 (63.5%)
No	176 (36.2%)	94 (38.7%)	82 (33.7%)	172 (36.9%)	91 (39.2%)	81 (34.6%)	167 (38%)	89 (40.3%)	78 (35.6%)
Not known	6 (1.2%)	4 (1.6%)	2 (0.8%)	6 (1.3%)	4 (1.7%)	2 (0.9%)	6 (1.4%)	4 (1.8%)	2 (0.9%)

Randomisation and T1 (n = 486)				T2 SCQ analysis (n = 466)			T3 SCQ analysis (n = 440)		
	Total n (%)	BDP n (%)	Signposting n (%)	Total n (%)	BDP n (%)	Signposting n (%)	Total n (%)	BDP n (%)	Signposting n (%)
SEND status									
Yes	159 (32.7%)	82 (33.7%)	77 (31.7%)	147 (31.5%)	75 (32.3%)	72 (30.8%)	140 (31.8%)	72 (32.6%)	68 (31.1%)
No	317 (65.2%)	159 (65.4%)	158 (65%)	311 (66.7%)	156 (67.2%)	155 (66.2%)	292 (66.4%)	148 (67%)	144 (65.8%)
Not known	10 (2.1%)	2 (0.8%)	8 (3.3%)	8 (1.7%)	1 (0.4%)	7 (3.0%)	8 (1.8%)	1 (0.5%)	7 (3.2%)
EHCP									
Yes	27 (5.6%)	14 (5.8%)	13 (5.3%)	26 (5.6%)	14 (6%)	12 (5.1%)	25 (5.7%)	14 (6.3%)	11 (5%)
No	450 (92.6%)	227 (93.4%)	223 (91.8%)	432 (92.7%)	217 (93.5%)	215 (91.9%)	408 (92.7%)	206 (93.2%)	202 (92.2%)
Not known	9 (1.8%)	2 (0.8%)	7 (2.9%)	8 (1.7%)	1 (0.4%)	7 (3.0%)	7 (1.6%)	1 (0.5%)	6 (2.7%)
Care status									
CIN	3 (0.6%)	1 (0.4%)	2 (0.8%)	3 (0.6%)	1 (0.4%)	2 (0.9%)	2 (0.5%)	0 (0%)	2 (0.9%)
CPP	0 (0.0%)	0 (0.0%)	0 (0.0%)	0 (0.0%)	0 (0.0%)	0 (0.0%)	0 (0.0%)	0 (0.0%)	0 (0.0%)
LAC	7 (1.4%)	5 (2.1%)	2 (0.8%)	6 (1.3%)	4 (1.7%)	2 (0.9%)	6 (1.4%)	4 (1.8%)	2 (0.9%)
None	466 (95.9%)	233 (95.9%)	233 (95.9%)	447 (95.9%)	223 (96.1%)	224 (95.7%)	424 (96.4%)	214 (96.8%)	210 (95.9%)
Not known	10 (2.1%)	4 (1.6%)	6 (2.5%)	10 (2.1%)	4 (1.7%)	6 (2.6%)	8 (1.8%)	3 (1.4%)	5 (2.3%)

Note: T1 = Time 1; T2 = Time 2; T3 = Time 3; SCQ = School Connectedness Questionnaire; BDP = Boys' Development Programme; FSM = free school meals; SEND = special educational needs and disabilities; CIN = child in need; CPP = child protection plan; LAC = looked after child

3.3 Outcome analyses

3.3.1 Overview

This section sets out the results from the primary, secondary and additional outcomes analyses. It also presents analysis relating to missing data and analysis in the presence of non-compliance.

Understanding the statistical terms in this section

This box explains some of the key statistical terms used in this section. These terms help us understand whether the BDP had an impact compared to BAU and how confident we can be in the results.

- **ANCOVA.** A statistical model used to compare the average scores between groups (e.g. BDP vs signposting) while adjusting for their starting scores. It is used when the outcome is continuous (e.g. when comparing subscale scores from the SDQ).
- **Unadjusted mean.** The simple average outcomes score for a group at follow-up, without accounting for any baseline differences
- **Adjusted mean.** The average score for a group, calculated after accounting for differences at the start of the trial between groups (e.g. baseline scores). This provides a fairer comparison between groups.
- **Hedges' g .** A standardised effect size which measures the size of the difference between groups. It is a version of Cohen's d that adjusts for small sample sizes. For measures where an increase in scores or positive effect size ($g > 0$) is interpreted favourably, in line with the YEFs Technical Guide (2025) (also set out in Table 1 at the start of this report), the thresholds used in this report are as follows:

$-0.02 \leq g < 0.02$ = No effect

$0.02 \leq g < 0.10$ = Small positive effect

$0.10 \leq g < 0.25$ = Moderate positive effect

$g \geq 0.25$ = Large positive effect

The same thresholds apply in the opposite direction for outcomes where lower scores indicate better outcomes, with the interpretation mirrored for negative effect sizes.

For measures where a decrease in scores or a negative effect size ($g < 0$) is interpreted favourably (i.e. fewer behavioural difficulties), the thresholds should be reversed before interpreting the Hedges' g . For example, a Hedges' g of -0.20 on behavioural difficulties would be interpreted as a moderate positive effect, while a Hedges' g of 0.34 would be interpreted as a large negative effect.

- **CI.** A range that we are fairly sure contains the true effect. For example, a 95% CI of 0.89 to 1.27 means we are 95% confident that the true result lies between those two numbers. If the range includes 0, the effect is not statistically significant, and we cannot determine if the true effect is different from 0.
- **p -value.** A p -value indicates how compatible the data are with the assumption that there is no true effect. By convention, a p -value below 0.05 (often referred to as "statistically significant") suggests that the observed result would be unlikely (i.e. occurring less than 5% of the time) if there were genuinely no difference between groups.

- **T-statistic.** A value that tells us how big the difference is between two groups once we account for how much variation there is in the data. The larger the *t*-value (in absolute terms), the bigger the difference between groups relative to the noise in the data. If the *t*-value is large enough (typically above 2), it suggests the difference we see is unlikely to have happened by chance.
- **Incidence rate ratio (IRR).** A number used in models that look at how often something happens (for example, exclusion incidents). It compares the rate of events between two groups. An IRR of 1 means both groups experienced events at the same rate, an IRR below 1 means the event was less common in the BDP delivery group and an IRR above 1 means it was more common in the BDP delivery group. For instance, an IRR of 0.65 means the BDP delivery group experienced about 35% fewer events than the comparison group.

Additional statistical terms are included and explained throughout this section where relevant.

3.3.2 Primary analysis

The primary outcome for the trial was **overall school engagement** measured using the SCQ total score (Marsh & Randolph, 2020). As described in section 2.2.4, the SCQ total score is a multidimensional construct comprising teacher bonding, peer bonding and behavioural engagement components. It ranges from 10 to 30 points, with higher scores and Hedges' *g* > 0 indicating greater engagement with school. An ANCOVA was conducted in line with the pre-specified SAP Equation (1), adjusting for baseline SCQ scores and school fixed effects, using an ITT sample of 466 complete cases.

Table 17 shows a **statistically non-significant small difference across groups at T2** favourable to the BDP (*g* = 0.04, *p* = 0.639). At T2 follow-up, demonstrating similar patterns to baseline scores, unadjusted mean SCQ scores were high and similar across both groups. These results indicate that within the detectable range of this study (MDES ≈ 0.19), no statistically significant or practically meaningful difference in self-reported school engagement was observed between groups at T2.

Table 17: Primary outcome analysis at Time 2

Outcome	Unadjusted means at T2				Effect size		
	BDP delivery group		Signposting group		Total n (BDP; Signposting)	Hedges' <i>g</i> (95% CI)	p-value
	n (missing)	Mean (95% CI)	n (missing)	Mean (95% CI)			
School engagement (SCQ total score)	232 (1)	25.14 (24.81; 25.47)	234 (1)	25.16 (24.85; 25.48)	466 (232; 234)	0.04 (-0.14, 0.23)	0.639

Note: BDP = Boys' Delivery Programme; CI = confidence interval; SCQ = School Connectedness Questionnaire

3.3.3 Secondary analysis

Secondary analyses were conducted on seven validated measures to assess the impact of the BDP on a range of social, emotional and behavioural outcomes. As described in section 2.4.2 and Equation (2), analyses were conducted on complete cases using an ITT approach, applying ANCOVA models to assess the impact of

allocation to BDP on each outcome at follow-up (T2), controlling for baseline scores and school. Sample sizes varied by outcome, with between 455 and 465 participants included in each model.

Four of these measures were pre-specified secondary outcomes:

- **Behavioural difficulties** measured using the externalising behaviours score from the SDQ, where lower scores and Hedges' $g < 0$ indicate fewer behavioural difficulties
- **Emotional problems** measured using the emotional difficulties subscale from the SDQ, where lower scores and Hedges' $g < 0$ indicate fewer emotional problems
- **Peer problems** measured using the peer difficulties subscale from the SDQ, where lower scores and Hedges' $g < 0$ indicate fewer peer problems
- **Teacher relationships** measured using the teacher bonding and attachment subscales from the SCQ, where higher scores and Hedges' $g > 0$ indicate better teacher bonding

In addition, we also present exploratory analyses of:

- **Peer bonding and behavioural engagement** measured using the peer bonding and school engagement subscales¹⁵ from the SCQ, respectively. These subscales were not pre-specified as secondary outcome measures, but the analyses of these subscales were pre-specified in the SAP to provide additional context for the primary outcome. For both of these subscales, higher scores and Hedges' $g > 0$ are interpreted positively and indicate better peer bonding and school engagement.
- **Pro-social behaviours, measured using the pro-social behaviours SDQ subscale.** This was not pre-specified and should be interpreted as exploratory. It is included for completeness, given its conceptual relevance to the BDP's theory of change. The BDP aims to strengthen social-emotional competencies, such as empathy, cooperation and positive peer relationships, all of which are reflected in the pro-social behaviours construct of the SDQ. Examining this subscale therefore provides useful supplementary evidence on whether the BDP may have influenced the development of these interpersonal and relational skills, which are central to many SEL programmes. For this subscale, higher scores and Hedges' $g > 0$ are interpreted positively and indicate more pro-social behaviours.

Table 18 presents the results of these analyses. This shows that **no statistically significant differences were detected on secondary outcomes at T2**, though several effects were directionally consistent with the programme's intended aims.

- For **behavioural difficulties**, boys allocated to the BDP delivery group reported fewer behavioural difficulties at T2. The estimated effect size was moderate and favourable to the BDP (Hedges' $g = -0.17$, $p = 0.062$). While this result does not meet conventional thresholds for statistical

¹⁵ Readers are reminded that the SCQ school engagement subscale captures behavioural indicators of participation, such as attendance and completion of classwork, and is therefore conceptually distinct from the overall SCQ total score used as the trial's primary outcome measure of school engagement.

significance, the pattern is in the expected direction and may suggest early signs of improvement in self-regulation and behaviour.

- Effects for **emotional difficulties** ($g = -0.11$, $p = 0.219$), **peer difficulties** ($g = 0.16$, $p = 0.087$) and **teacher bonding** ($g = 0.07$, $p = 0.466$) were not statistically significant. Effect sizes were small to moderate across all three measures, favourable to the BDP for emotional difficulties and teacher bonding, and unfavourable to the BDP for peer difficulties. As these estimates were statistically uncertain, with CIs encompassing zero, they provide no reliable evidence that allocation to the BDP affected emotional difficulties, peer relationships or perceived connection with teachers, either positively or negatively.
- Exploratory analyses of additional SCQ and SDQ subscales followed the same pattern. Non-significant moderate differences favoured the BDP delivery group for **school engagement** ($g = 0.13$, $p = 0.173$) and **pro-social values** ($g = 0.17$, $p = 0.061$), while **peer bonding** effects were unfavourable ($g = -0.14$, $p = 0.228$). These results should be interpreted cautiously, as they were exploratory and not pre-specified.

Overall, secondary outcome analyses at T2 provided **no consistent or statistically reliable evidence of short-term impact** on boys' emotional, behavioural or relational outcomes. The direction of effects varied slightly across measures; however, given the small size and precision of these estimates, there was no reliable evidence of either beneficial or adverse effects. As such, the secondary analyses provide no consistent evidence that allocation to the BDP produced measurable changes in social, emotional or behavioural outcomes at the T2 follow-up (i.e. the point at which delivery ended).

Table 18: Secondary analysis results at Time 2

Outcome	Unadjusted means at T2				Effect size		
	BDP delivery group		Signposting group		Total n (BDP; signposting)	Hedges <i>g</i> (95% CI)	<i>p</i> -value
	n (missing)	Mean (95% CI)	n (missing)	Mean (95% CI)			
Secondary outcomes (pre-specified)							
Behavioural difficulties (SDQ)	232 (1)	8.81 (8.32; 9.29)	233 (2)	9.35 (8.84; 9.86)	465 (232; 233)	−0.17 (−0.36; 0.00)	0.062
Emotional problems (SDQ)	232 (1)	2.71 (2.41; 3.00)	233 (2)	2.92 (2.62; 3.23)	465 (232; 233)	−0.11 (−0.30; 0.07)	0.219
Peer difficulties (SDQ)	232 (1)	2.37 (2.17, 2.56)	233 (2)	2.26 (2.08; 2.44)	465 (232; 233)	0.16 (−0.02; 0.34)	0.087
Teacher bonding (SCQ subscale)	225 (8)	6.42 (6.25, 6.59)	230 (5)	6.33 (6.16; 6.50)	455 (225; 230)	0.07 (−0.12; 0.26)	0.466

Outcome	Unadjusted means at T2				Effect size		
	BDP delivery group		Signposting group		Total n (BDP; signposting)	Hedges <i>g</i> (95% CI)	<i>p</i> -value
	n (missing)	Mean (95% CI)	n (missing)	Mean (95% CI)			
Additional outcomes (exploratory)							
Peer bonding (SCQ subscale)	229 (4)	11.19 (11.08, 11.31)	231 (4)	11.30 (11.18; 11.42)	461 (229; 231)	-0.14 (-0.29; 0.07)	0.228
Behavioural engagement (SCQ subscale)	223 (10)	7.56 (7.43, 7.69)	232 (3)	7.44 (7.31; 7.57)	455 (223; 232)	0.13 (-0.06; 0.32)	0.173
Pro-social values (SDQ)	232 (1)	7.30 (7.05; 7.55)	234 (1)	7.07 (6.82; 7.32)	466 (232; 234)	0.17 (-0.01; 0.36)	0.061

Note: BDP = Boys' Delivery Programme; CI = confidence interval; SDQ = Strengths and Difficulties Questionnaire; SCQ = School Connectedness Questionnaire

3.3.4 Longitudinal analysis

In line with the SAP and Equation (3) set out in section 2.4.3, primary and secondary analyses were repeated on data collected at T3 (12 weeks after programme completion, and around 24 weeks post-randomisation).

Table 19 shows the results of these analyses:

- **A statistically significant moderate effect on overall school engagement (SCQ total score) at T3.** Analysis of the primary outcome (school engagement, measured using the SCQ total score) indicated a statistically significant moderate effect favourable to the BDP for boys allocated to the BDP compared with those in the signposting group (Hedges' $g = 0.21$, $p = 0.027$). This represents a statistically significant improvement in self-reported engagement with school, suggesting that the positive effects of the BDP were detectable six months after randomisation, i.e. they emerged beyond the immediate post-intervention period.¹⁶
- **A statistically significant moderate effect on behavioural difficulties.** Among the pre-specified secondary outcomes, a statistically significant moderate effect favourable to the BDP was observed for behavioural difficulties, as measured using the SDQ externalising behaviours score. Boys allocated to the BDP reported statistically significantly lower behavioural difficulties than those in the signposting group, with a moderate effect size: Hedges' $g = -0.22$ ($p = 0.020$). This direction of effect is consistent with the improvements in self-regulation and behaviour management hypothesised in the programme's theory of change.
- **No evidence of change on the other pre-specified secondary outcomes.** Differences between groups on emotional difficulties, peer difficulties and teacher bonding were not statistically significant, with CIs encompassing the null. This suggests no reliable evidence of change on these domains at 24 weeks.
- **Statistically significant positive effects on behavioural engagement (SCQ school engagement subscale) and pro-social value (SDQ subscale) and no evidence of change on peer bonding (all exploratory).** Exploratory analyses of individual subscales provided further insight into the pattern of results. Statistically significant moderate effects favourable to the BDP were observed for behavioural engagement (SCQ subscale) and pro-social values (SDQ subscale), with effect sizes of 0.20 ($p = 0.042$) and 0.24 ($p = 0.012$), respectively. These findings may indicate potential longer-term benefits of the BDP in promoting pro-social attitudes and sustaining positive connections with school. Effects for peer bonding (SCQ subscale) were positive and favoured the BDP but were not statistically significant ($g = 0.14$, $p = 0.143$). As these analyses were not pre-specified, these findings should be interpreted as exploratory and hypothesis-generating for future research, rather than confirmatory.

At T3, boys allocated to the BDP **demonstrated statistically significant improvements in school engagement and reductions in behavioural difficulties relative to BAU**. While these findings are encouraging, they should be interpreted with caution. It is not possible to determine from the available data whether these effects would be sustained beyond the observed follow-up period; they may decrease over time, including through regression to the mean, or strengthen and continue to grow. In addition, differences in recall periods may contribute to variation in observed effects.¹⁷ These results would benefit from

¹⁶ Please note that this analysis was re-run using a simple ANCOVA model as a sensitivity analysis to the longitudinal model (i.e. regressing T3 scores on baseline scores by group and school). These results were similar in direction, magnitude and significance ($g = 0.19$, $p = 0.043$).

¹⁷ It should be noted that the SCQ and SDQ use a one-month recall period at T2, meaning that at T2, boys were reporting on a period from around eight weeks into the BDP through to programme completion at 12 weeks. It is possible that this timing limited

replication and longer-term follow-up to assess durability. Secondary outcomes remain exploratory and should be interpreted in light of multiple testing.

the sensitivity of T2 measures, particularly if improvements had not yet stabilised or were still developing. As such, the absence of measurable effects at T2 and their emergence at T3 may reflect genuine delayed change, the influence of recall periods or a combination of both factors.

Table 19: Longitudinal primary and secondary analyses

Outcome	Unadjusted means at T2				Effect size		
	BDP delivery group (n = 221)		Signposting group (n = 222)				
	n (missing)	Mean (95% CI)	n (missing)	Mean (95% CI)	Total n (BDP; signposting)	Hedges' g (95% CI)	p-value
Primary outcome (pre-specified)							
School engagement (SCQ total score)	221 (0)	25.52 (25.18; 25.85)	219 (3)	25.11 (24.73; 25.49)	440 (221; 219)	0.21 (0.02; 0.40)	0.027
Secondary outcomes (pre-specified)							
Behavioural difficulties (SDQ)	221 (0)	7.99 (7.42; 8.56)	220 (2)	8.78 (8.25; 9.30)	441 (221; 220)	-0.22 (-0.41; -0.04)	0.020
Emotional difficulties (SDQ)	220 (1)	2.79 (2.47; 3.10)	219 (3)	3.03 (2.70; 3.35)	439 (220; 219)	-0.11 (-0.29; 0.08)	0.262
Peer difficulties (SDQ)	221 (0)	1.80 (1.57; 2.02)	220 (2)	1.77 (1.56; 1.99)	441 (221; 220)	0.07 (-0.12; 0.26)	0.464
Teacher bonding (SCQ)	211 (10)	6.55 (6.35; 6.76)	214 (8)	6.45 (6.26; 6.64)	425 (211; 214)	0.11 (-0.08; 0.30)	0.253
Additional outcomes (exploratory)							
Behavioural engagement (SCQ)	209 (11)	7.60 (7.44; 7.77)	215 (7)	7.42 (7.25; 7.59)	421 (209; 215)	0.20 (0.01; 0.39)	0.042
Peer bonding (SCQ)	217 (4)	11.39 (11.26; 11.52)	214 (8)	11.25 (11.10; 11.41)	431 (217; 214)	0.14 (-0.05; 0.33)	0.143
Pro-social values (SDQ)	221 (0)	7.33 (7.09; 7.58)	222 (0)	7.01 (6.73; 7.28)	443 (221; 222)	0.24 (0.05; 0.43)	0.012

Note: BDP = Boys' Delivery Programme; CI = confidence interval; SDQ = Strengths and Difficulties Questionnaire; SCQ = School Connectedness Questionnaire

3.3.5 Subgroup analyses

Exploratory subgroup analyses examined whether the impact of the BDP on overall school engagement (SCQ total score) differed by ethnicity, socioeconomic disadvantage (measured by FSM eligibility) or baseline risk (defined as having ≥ 1 exclusion at baseline), in line with the approach set out in Equation (4).

These analyses were pre-specified for the primary time point (T2) but extended to the follow-up period (T3) to explore whether any emerging differences in outcomes were moderated by these factors. Results should be interpreted cautiously and considered exploratory, given the smaller subgroup sizes and limited statistical power for detecting interaction effects. The results from these analyses are set out in Table 20 and summarised below. A full overview of coefficient estimates and interaction terms is presented in Appendix D.

Ethnicity

Using a broad comparison between boys from White backgrounds and boys from minority ethnic backgrounds, there was no statistically significant evidence of differential impact at either T2 or T3:

- **At T2**, the estimated effect size for boys from White backgrounds was $g = 0.29$ ($p = 0.201$), while the estimate for boys from minority ethnic backgrounds was $g = -0.01$ ($p = 0.932$). The interaction between treatment and ethnicity was not statistically significant at T2 ($p = 0.231$).
- **At T3**, effect sizes were positive in both groups (White: $g = 0.29$, $p = 0.214$; minority ethnic: $g = 0.17$, $p = 0.106$), although CIs were wide and included zero. The treatment-by-ethnicity interaction was again not significant ($p = 0.651$), indicating no robust statistical evidence that impact differed by ethnicity.

Given the overrepresentation of Black Caribbean and dual heritage Caribbean pupils in exclusion statistics nationally, additional exploratory analyses examined outcomes for boys identified as Black Caribbean or Mixed Caribbean compared with all other boys:

- **At T2**, there was no evidence of differential impact between groups. The estimated effect size for boys who were Black Caribbean or Mixed Caribbean was null ($g = -0.01$, $p = 0.953$), while the estimate for boys who were not Black Caribbean or Mixed Caribbean was small ($g = 0.07$, $p = 0.541$). CIs were wide and crossed zero in both cases. The treatment-by-subgroup interaction was not statistically significant ($p = 0.745$), indicating no evidence that programme effects differed between these groups at T2.
- **At T3**, the estimated effect size for boys who were not Black Caribbean or Mixed Caribbean was moderate and statistically significant ($g = 0.22$, $p = 0.039$), while the estimate for Black Caribbean or Mixed Caribbean boys was small and not significant ($g = 0.06$, $p = 0.787$). However, the interaction term remained non-significant ($p = 0.494$), and subgroup sample sizes were modest with wide CIs. As such, these findings should not be interpreted as definitive evidence of differential effectiveness.

Overall, while point estimates at T3 suggest the possibility of stronger effects among boys who were not Black Caribbean or Mixed Caribbean, the absence of statistically significant interaction terms means that these differences should be interpreted cautiously. Subgroup sample sizes were modest, and findings remain exploratory.

Free school meal eligibility

Patterns of effectiveness by FSM eligibility were similar:

- **At T2**, there were no statistically significant differences in school engagement for either boys who were eligible for FSM ($g = 0.10$, $p = 0.395$) or boys who were not eligible ($g = -0.02$, $p = 0.883$). The treatment-by-FSM interaction was not statistically significant at T2 ($p = 0.525$), indicating no evidence of differential impact at this time point.
- **At T3**, there was a statistically significant large impact of the BDP on school engagement for boys who were eligible for FSM ($g = 0.27$, $p = 0.030$), while effects for those not eligible for FSM remained small and non-significant ($g = 0.07$, $p = 0.627$). However, the interaction term was not statistically significant ($p = 0.334$), meaning that despite differences in point estimates, there was no robust statistical evidence that programme effects differed by FSM eligibility.

Overall, while the pattern at T3 suggests the possibility of greater benefit among boys experiencing socioeconomic disadvantage, these findings should be interpreted cautiously. Subgroup analyses were exploratory and not powered to detect moderation effects, and the absence of statistically significant interaction terms limits firm conclusions regarding differential effectiveness.

Baseline level of risk (≥ 1 exclusion at baseline)

Exploratory analyses examined whether programme effects differed according to baseline exclusion status:

- **At T2**, boys who had experienced at least one exclusion at baseline showed a larger estimated effect size ($g = 0.36$, $p = 0.085$) than boys who had not experienced a prior exclusion ($g = 0.04$, $p = 0.782$). However, the treatment-by-baseline exclusion interaction was not statistically significant ($p = 0.193$), indicating no robust evidence of differential impact at this time point.
- **At T3**, a similar pattern was observed, with a larger estimated effect for boys with ≥ 1 baseline exclusion ($g = 0.38$, $p = 0.086$) than for boys with no prior exclusions ($g = 0.19$, $p = 0.177$). Nevertheless, the interaction term was again not statistically significant ($p = 0.457$), suggesting that differences in point estimates should not be interpreted as definitive evidence of moderation.

Overall, although the pattern of results is consistent with the possibility that boys at higher baseline risk may have derived greater benefit from the programme, these findings remain exploratory. Subgroup analyses were not powered to detect interaction effects, and conclusions regarding differential effectiveness should therefore be drawn with caution.

Table 20: Subgroup analyses at Time 2 and Time 3 for ethnicity and free school meal eligibility

Group	Unadjusted means at T2				Effect size		
	BDP delivery group		Signposting group				
	n (missing)	Mean (95% CI)	n (missing)	Mean (95% CI)	Total n (BDP; signposting)	Hedges’ g (95% CI)	p-value
Ethnicity: White/minority ethnic							
Time 2 analysis							
White background	38	25.32 (24.51; 26.15)	40	24.74 (23.93; 25.56)	78 (38; 40)	0.29 (−0.16; 0.74)	0.201
Minority ethnic background	189	25.14 (24.76; 25.51)	187	25.32 (24.98; 25.68)	376 (189; 187)	−0.01 (−0.21; 0.19)	0.932
Time 3 analysis							
White background	37	25.58 (24.93; 26.23)	38	24.95 (24.03; 25.87)	75 (37; 38)	0.29 (−0.17; 0.74)	0.214
Minority ethnic background	179	25.54 (25.16, 25.93)	175	25.22 (24.81; 25.64)	354 (179;175)	0.17 (−0.04; 0.38)	0.106
Ethnicity: Black Caribbean or Mixed Caribbean/not Black Caribbean or Mixed Caribbean							
Time 2 analysis							
Black Caribbean or Mixed Caribbean	38	25.24 (24.47, 26.02)	54	25.43 (24.75, 26.10)	92 (38; 54)	−0.01 (−0.43; 0.40)	0.953
Not Black Caribbean/Mixed Caribbean	189	25.16 (24.78, 25.54)	173	25.16 (24.80, 25.53)	362 (189; 173)	0.07 (−0.14; 0.27)	0.541
Time 3 analysis							
Black Caribbean or Mixed Caribbean	37	25.31 (24.58, 26.04)	50	25.23 (24.41, 26.04)	87 (37; 50)	0.06 (−0.37; 0.49)	0.787
Not Black Caribbean/Mixed Caribbean	179	25.60 (25.22, 25.98)	163	25.16 (24.73, 25.59)	342 (179; 163)	0.22 (0.01; 0.44)	0.039

Group	Unadjusted means at T2				Effect size		
	BDP delivery group		Signposting group		Total n (BDP; signposting)	Hedges' g (95% CI)	p-value
	n (missing)	Mean (95% CI)	n (missing)	Mean (95% CI)			
Free school meal (FSM) eligibility							
Time 2 analysis							
Eligible for FSM	137	25.30 (24.91; 25.69)	151	25.07 (24.67; 25.48)	288 (137; 151)	0.10 (−0.13; 0.33)	0.395
Not eligible for FSM	91	24.95 (24.33; 25.58)	81	25.35 (24.82; 25.89)	172 (91; 81)	−0.02 (−0.32; 0.28)	0.883
Time 3 analysis							
Eligible for FSM	128	25.51 (25.10; 25.93)	139	24.81 (24.31; 25.30)	267 (128; 139)	0.27 (0.03, 0.51)	0.030
Not eligible for FSM	89	25.57 (24.98; 26.16)	78	25.65 (25.07; 26.25)	167 (89; 78)	0.07 (−0.23; 0.38)	0.627
Baseline level of risk (≥1 exclusion)							
Time 2 analysis							
≥1 exclusion at baseline	43	25.01 (24.25; 25.77)	47	24.66 (23.99; 25.34)	90 (43; 47)	0.36 (−0.05; 0.78)	0.085
0 exclusions at baseline	111	25.32 (24.82; 25.82)	106	25.17 (24.66; 25.68)	217 (111; 106)	0.04 (−0.23, 0.30)	0.782
Time 3 analysis							
≥1 exclusion at baseline	40	25.55 (24.75; 26.35)	41	24.81 (23.78; 25.85)	81 (40; 41)	0.38 (−0.05; 0.82)	0.086
0 exclusions at baseline	108	25.71 (25.22; 26.21)	100	25.19 (24.63; 25.76)	208 (108; 100)	0.19 (−0.09, 0.46)	0.177

Note: BDP = Boys' Delivery Programme; CI = confidence interval

3.3.6 Analysis in the presence of non-compliance

The impact analyses presented so far have followed an ITT approach, meaning that all participants were analysed according to the group they were originally randomised to regardless of how many sessions they actually received. This approach preserves the benefits of randomisation and provides an unbiased estimate of the effect of being offered the BDP programme under real-world conditions, including incomplete attendance and variation in engagement.

As outlined in the IPE (section 5.1.2), overall compliance with the programme was high, with the majority of boys attending eight or more sessions and a substantial proportion receiving close to the full intended dosage. However, some variation in session attendance remained, and it is important to consider whether differing levels of engagement influenced the size or direction of the observed effects. In particular, it is useful to explore whether there are additional benefits associated with receiving the programme at close to its full intended dosage.

To better understand whether receiving more BDP sessions was associated with improved outcomes, we conducted a CACE analysis using IV methods, with randomisation allocation as the instrument for session attendance.

CACE analysis aims to answer the question “**What was the effect of the BDP for boys who received it as intended?**” This approach accounts for the fact that boys who attend more sessions may differ systematically from those who attend fewer sessions. While it provides insight into potential dose–response relationships, it no longer fully benefits from randomisation and should therefore be considered as exploratory.

Two versions of the CACE model were run for each time point:

1. A **continuous model**, estimating the marginal effect of each additional BDP session received
2. A **binary model**, comparing boys who received 11 or more sessions to those who received 10 or fewer

The binary specification was not pre-specified in the SAP but was added post hoc in response to the observed distribution of session attendance, which was tightly clustered between 8 and 13 sessions. In such cases, a binary cutoff point can help detect threshold effects. The cutoff of 11 sessions was selected to represent a high level of exposure, close to the intended maximum dosage, while retaining sufficient sample size for analysis (see Table 21 for the respective proportions). However, as this threshold was not pre-specified, it should be interpreted with caution.

Table 21: Proportion of boys who received 10 or fewer sessions or 11 or more (n = 243)

Number of Boys' Development Programme sessions received	n	%
≤ 10 sessions	146	60.1%
11+ sessions	97	39.9%
Total	243	100.0%

Table 22 shows the results of this analysis:

- **Statistically non-significant effects on overall school engagement at T2 for both the continuous and binary models.** For the continuous specification, the estimated marginal effect was null and non-significant (Hedges' $g = 0.00$, $p = 0.639$). Similarly, the binary threshold model (11+ vs ≤10 sessions) yielded a non-significant moderate effect ($g = 0.11$, $p = 0.640$).
- **Statistically significant large positive effects on overall school engagement at T3 for both models.** The continuous model produced a small and statistically significant estimate (Hedges' $g = 0.02$,

$p = 0.044$), suggesting a positive association between each additional session attended and overall school engagement scores. The binary threshold model (11+ vs ≤ 10 sessions) indicated a statistically significant large effect (Hedges' $g = 0.47$, $p = 0.046$). This suggests that three months post-completion, boys who had attended the majority of sessions experienced notably higher levels of school engagement than those who received fewer sessions or none.

All four models had strong instrument strength and non-significant Wu–Hausman tests, confirming that endogeneity was not a concern and that the two-stage least squares estimates were consistent with ordinary least squares estimates.

Overall, the CACE findings provide further evidence that the **impact of the BDP on school engagement emerged over time** and was concentrated among boys who fully engaged with the intervention. While no complier effects were detected during the intervention period (T2), statistically significant and substantively meaningful effects emerged by T3, consistent with a sustained or cumulative benefit of participation.

The large CACE effect size at T3 ($g = 0.47$) compared with the ITT estimate ($g = 0.21$) indicates that non-compliance likely reduced the overall impact in the ITT analysis. These results provide additional evidence that the BDP was effective when delivered and received as intended, suggesting that receipt of the full programme at its intended dosage is key to achieving behavioural and engagement-related change.

Table 22: Complier average causal effect (CACE) analysis on school engagement at Time 2 and Time 3

	Estimate	SE	CI_L	CI_U	Hedges' g (CI)	p-value	Weak IV test (F Statistic)	Wu-Hausman test (p-value)
Time 2 CACE analysis (n = 466)								
Continuous model	0.01	0.02	-0.03	0.05	0.00 (-0.01; 0.02)	0.639	6308.5	0.864
Binary model (11+ sessions vs ≤ 10 sessions)	0.23	0.49	-0.73	1.19	0.11 (-0.34; 0.55)	0.640	160.6	0.371
Time 3 CACE analysis (n = 440)								
Continuous model	0.05	0.03	0.00	0.10	0.02 (0.00; ¹⁸ 0.04)	0.044	6705.8	0.205
Binary model (11+ sessions vs ≤ 10 sessions)	1.16	0.58	0.02	2.30	0.47 (0.01; 0.94)	0.046	148.1	0.551

Note: CI = confidence interval; L = lower; U = upper; IV = instrumental variable; SE = standard error

¹⁸ Please note that the lower limit of this CI is 0.001, i.e. it does not include 0. It has been rounded to 0.00 for consistency with other outputs.

3.3.7 Mechanism of change analysis

The BDP is designed to build trusting relationships between boys and their project co-ordinators, which are theorised to underpin improvements to school engagement and behaviour. This section explores whether boys in the BDP delivery group reported stronger relationship quality than boys in the signposting group and whether stronger relationship quality was associated with better school engagement.

To measure the quality of relationships, the SSRS was administered at follow-up (T2 and T3) to all boys in the BDP delivery group and to boys in the signposting group who reported having a trusted adult.

The SSRS includes four subscales:

- **Feels valued** (e.g. “This person cares about how I am doing in school”)
- **Trust** (e.g. “I feel safe when I am with this person”)
- **Mentoring** (e.g. “This person introduces me to new ideas or experiences”)
- **Negativity** (e.g. “I don’t like things this person says or does”)

Higher scores on the first three subscales indicate stronger, more supportive relationships, while higher scores on the negativity subscale indicate a more negative relationship.

In the signposting group, the SSRS was administered only to those who answered “yes” to the question: “Do you have a relationship with a significant adult, other than a parent, who you see on a regular basis?” At T2, of the 232 boys in the signposting group who answered this question, 188 (81%) said “yes”. This was slightly higher at T3: of the 222 boys who answered this question, 188 (85%) said “yes” and therefore completed the measure. This means that the SSRS sample in the signposting group is selective, and not all boys completed the measure. For this reason, the SSRS analysis is descriptive and exploratory and should be interpreted with caution.

Differences in relationship quality across arms at Time 2 and Time 3

Table 23 summarises SSRS scores by group at both T2 and T3. This shows:

- **Boys in the BDP delivery group reported significantly better relationships with their project co-ordinators at T2** than boys in the signposting group reported with their trusted non-parental adults. Group differences in independent-samples t-tests were significant for feels valued ($p < 0.001$), mentoring ($p < 0.001$) and negativity ($p < 0.001$), with small to medium effect sizes favouring the BDP. Scores on the trust subscale did not differ significantly between groups.
- **These differences were largely sustained at T3.** Boys in the BDP delivery group continued to report higher scores for feels valued ($p = 0.007$) and mentoring ($p = 0.001$) and substantially lower scores for negativity ($p < 0.001$). As at T2, differences on the trust subscale remained non-significant.

This pattern suggests that boys in the BDP delivery group **perceived their relationships with their project co-ordinators significantly more positively** than boys in the signposting group perceived their relationships with their pre-existing trusted adults. This supports the idea that the BDP was successful in promoting access to strong, supportive connections with trusted adults, with positive perceptions of these relationships maintained three months after the programme had been completed.

Table 23: Independent-samples t-test comparing Social Support and Rejection Scale (SSRS) subscale scores across boys in the Boys' Development Programme (BDP) delivery group and the signposting group at Time 2 (T2) and Time 3 (T3)

SSRS subscale	BDP delivery group		Signposting group		T statistic	p-value
	n	Mean (SD)	n	Mean (SD)		
T2 analysis						
Feels valued	232	4.71 (0.58)	188	4.49 (0.57)	3.98	<0.001
Trust	230	3.51 (0.96)	188	3.59 (0.94)	−0.85	0.397
Mentoring	228	4.39 (0.74)	184	3.99 (0.93)	4.69	<0.001
Negativity	232	1.35 (0.89)	187	1.68 (0.73)	−4.18	<0.001
T3 analysis						
Feels valued	219	4.64 (0.68)	192	4.46 (0.66)	2.70	0.007
Trust	219	3.49 (1.01)	191	3.53 (0.95)	−0.33	0.738
Mentoring	220	4.33 (0.81)	190	4.05 (0.84)	3.35	0.001
Negativity	222	1.28 (0.71)	189	1.72 (0.76)	−6.03	<0.001

Note: SD = standard deviation

Mechanism analysis

To explore which, if any, aspects of relationship quality were linked with later improvements in overall school engagement (SCQ total score), a series of regression models were run. These analyses examined whether scores on each SSRS subscale at T2 (feels valued, mentoring, trust and negativity) predicted school engagement at T3 after accounting for boys' baseline engagement, their school and their allocation group.

Predictive models of later school engagement

The first model tested each SSRS subscale individually to see whether that particular relational quality on its own was associated with school engagement three months after programme completion (Equation (5)). This provides a simple way to identify which aspects of relationships appear most relevant to future school engagement outcomes.

Table 24 shows that when tested separately, **the feels valued, mentoring and negativity scores were all statistically significant predictors of school engagement at T3**. Boys who felt more supported and valued and those who viewed their adult more positively were more likely to report higher school engagement at follow-up. In contrast, higher negativity scores were associated with lower engagement. The trust subscale was not significant on its own ($p = 0.116$), suggesting that general feelings of trust were less strongly related to school engagement than the other relational dimensions.

A second model then included all four subscales together (Equation (6)), allowing us to see which aspects of relationship quality remained important after accounting for overlapping variance between subscales. In this combined model, only mentoring remained significant ($\beta = 0.57$, $p = 0.006$). This indicates that mentoring captures the most distinctive aspect of relational quality associated with later school

engagement, while the other dimensions, though correlated, appear to represent shared elements of the relationship construct.

Table 24: Results from predictive models regressing school engagement at Time 3 on Social Support and Rejection Scale subscale scores, controlling for baseline measures, school and allocation group

Parameter	n	β Coefficient	SE	95% CI	T	p-value
Individual models						
Feels valued	399	0.85	0.20	(0.45, 1.25)	4.18	<0.001
Mentoring	389	0.63	0.14	(0.35, 0.91)	4.44	<0.001
Trust	396	0.20	0.13	(-0.05, 0.44)	1.58	0.116
Negativity	396	-0.38	0.14	(-0.65, -0.10)	-2.65	0.008
Combined model						
Feels valued	389	0.34	0.28	(-0.22, 0.90)	1.18	0.238
Mentoring	389	0.57	0.21	(0.16, 0.97)	2.75	0.006
Trust	389	-0.17	0.16	(-0.48, 0.15)	-1.05	0.296
Negativity	389	-0.12	0.15	(-0.43, 0.18)	-0.79	0.433

Note: SE = standard error; CI = confidence interval

Interestingly, the coefficient for group allocation was no longer significant on any of these models. This suggests that **the association between BDP participation and later school engagement may have been statistically explained by the mentoring relationship**. In other words, it suggests that the BDP's influence on school engagement may work through its effects on relationships, i.e. boys who saw their adult as a mentor who listened to, guided and encouraged them were the most likely to stay engaged in school.

At the same time, it is plausible that this process is bidirectional: Boys may experience an initial mentoring relationship in early sessions, feel listened to and supported, and, as a result, they may be more likely to keep attending subsequent sessions. Ongoing attendance then provides further opportunities to strengthen the mentoring relationship, which in turn may support higher school engagement over time. This provides further support for the idea that the quality of the relationship between the project co-ordinator and the boy is the key mechanism of change, while also recognising that this mechanism likely involves a reinforcing cycle between feeling mentored and continuing to engage with support.

Mediation analysis

To test this mechanism formally, a mediation analysis was conducted. Mediation models examine whether a programme affects outcomes indirectly by first impacting another variable (the mediator), which in turn then influences the outcome (Equations (7) and (8)). We therefore tested whether participation in the BDP improved mentoring scores at T2, with mentoring scores then, in turn, predicting higher school engagement at T3. This model controlled for baseline school engagement and school.

The results confirmed a significant mediated effect operating through the mentoring subscale (ACME = 0.26, $p < 0.001$). This means that the BDP increased boys' mentoring relationship scores at T2, and these stronger

mentoring relationships in turn predicted higher school engagement at T3. The direct effect of group allocation was non-significant ($ADE = -0.09$, $p = 0.74$), suggesting that once mentoring was accounted for, the programme no longer had a separate direct effect on school engagement. This is the pattern we would expect to see if the quality of relationships operates as the key mechanism of change.

Although the total effect of group on school engagement in this reduced sample was small and statistically uncertain ($p = 0.462$), this likely reflects the smaller, self-selective SSRS subsample (restricted to boys with a trusted adult in the signposting group) and the limited variability in relational scores. The significant mediation pathway nonetheless provides indicative evidence that the BDP's positive effects on school engagement occur through improvements in the quality of mentoring relationships, supporting the programme's central theory of change.

Table 25: Mediation analysis

Parameter	Interpretation	Coefficient	95% CI	p-value
ACME	The indirect effect of the BDP on school engagement mediated by mentoring quality	0.26	(0.13, 0.43)	<0.001
ADE	The remaining direct effect of the BDP on school engagement after accounting for mentoring quality	-0.09	(-0.56, 0.41)	0.736
Total effect	Combined indirect and direct effects (ACME + ADE)	0.17	(-0.28, 0.66)	0.462
Proportion mediated	ACME / total effect	1.54	(-13.75, 13.21)	0.462

Note: CI = confidence interval; BDP = Boys' Development Programme; ACME = average causal mediated effect; ADE = average direct effect

Overall, these findings are **consistent with the central mechanism articulated in the BDP's theory of change**. Boys allocated to the BDP reported stronger mentoring relationships with their project co-ordinator than those in the signposting group, and the mentoring dimension of these relationships predicted subsequent school engagement. Mediation analysis indicated that the association between BDP allocation and later school engagement was statistically explained by enhanced mentoring relationships. However, given that the impact findings were mixed across time points and the T3 effect should be interpreted with caution, these results should be understood as providing provisional support for the proposed mechanism, contingent on the programme effect being genuine. Overall, the pattern of findings is consistent with the theory that relationship quality may be a key pathway through which engagement changes over time, but this interpretation warrants replication. The qualitative findings presented later in the report explore in more depth how and why trusting relationships may influence boys' engagement with school.

3.3.8 School administrative data analysis

This section presents the results of the exploratory analysis conducted on school administrative data, following the approach set out in section 2.4.7 and Equations (9), (10) and (11). It presents:

- A descriptive analysis across groups at P1 and P2 to enable the assessment of balance across arms
- An outcomes analysis on attendance rates

- An outcomes analysis on exclusions, including the number of incidents and the total duration of suspensions

Defining timescales in this section

As a reminder, for this analysis we defined:

P1 = the two half terms before the treatment period

P2 = the two half terms after the treatment period.

This means that, in practice, the P2 analysis presented in this section aligns with the T3 data collection time point (i.e. 12 weeks post-programme completion) for the self-reported outcomes analysis presented elsewhere in this report.

Descriptive statistics

Table 26 presents summary statistics for attendance and exclusion measures drawn from school administrative data and disaggregated by trial arm. This describes attendance rates and exclusion incidents for boys in the BDP and signposting groups at baseline (P1) and follow-up (P2):

- **Attendance.** At baseline, average attendance was closely matched across the two groups (BDP: 90.4%; signposting: 90.6%). This indicates good balance between groups at the point of randomisation and suggests that the randomisation process was successful in producing equivalent samples for this outcome. Standard deviations were similar (8.6 and 9.5 percentage points, respectively), suggesting comparable variability in attendance across groups.

At follow-up (P2), attendance declined slightly in both arms (BDP: 89.0%; signposting: 89.7%), possibly consistent with typical seasonal patterns of lower attendance in summer terms. The distributions were negatively skewed in both groups, reflecting that most boys maintained attendance above 90%.

- **Exclusion data.** At baseline, the mean number of exclusion incidents per pupil per term was low and comparable across groups (BDP: 0.44; signposting: 0.49), again suggesting balance was achieved through randomisation. Mean exclusion duration was also very similar (BDP: 0.98 days; signposting: 1.02 days). Both measures displayed substantial positive skew and kurtosis, reflecting that the majority of boys had no exclusions, while a small number accounted for multiple or longer incidents. Across the entire cohort, 95 boys had at least one exclusion incident at P1 (49 boys in the signposting group and 45 in the BDP delivery group).

At follow-up (P2), the mean number and duration of exclusions decreased in the BDP delivery group (incidents: 0.39; duration: 0.71 days) but increased slightly in the signposting group (incidents: 0.65; duration: 1.32 days). While these descriptive differences are small and should be interpreted cautiously, they suggest a potential trend towards fewer and shorter exclusions among boys allocated to the BDP relative to BAU. At P2, 98 boys had at least one exclusion incident (51 boys in the signposting group and 46 boys in the BDP delivery group).

Overall, the data show strong baseline equivalence between the two groups across both attendance and exclusion outcomes, supporting the internal validity of subsequent impact analyses. The positive skew and kurtosis values highlight that exclusions are highly concentrated among a small subset of pupils, which

justifies the use of nonparametric or count-based models (e.g. logistic or negative binomial regression) for inferential analysis, as outlined in section 2.4.7.

Table 26: School administrative data descriptive statistics

	BDP delivery group					Signposting group				
	N	Mean	SD	Skew	Kurtosis	N	Mean	SD	Skew	Kurtosis
Attendance										
Attendance at P1	116	90.4%	8.55	-1.40	2.87	118	90.6%	9.47	-1.97	5.02
Attendance at P2	116	89.0%	14.20	-2.51	6.98	118	89.7%	12.54	-3.21	16.43
Exclusions										
Total incidents at P1	158	0.44	0.88	2.81	9.56	158	0.49	0.93	3.00	14.30
Total incidents at P2	158	0.39	0.84	5.05	39.75	158	0.65	1.16	2.01	3.91
Duration at P1	158	0.98	2.06	2.74	8.64	158	1.02	2.02	2.45	6.79
Duration at P2	158	0.71	1.61	2.99	10.26	158	1.32	2.62	2.51	7.41

Note: BDP = Boys' Development Programme; SD = standard deviation; P1 = Two half terms before the treatment period; P2 = Two half terms after the treatment period

Attendance analysis

Table 27 presents the exploratory analysis of school attendance rates at follow-up (P2). This shows a **statistically non-significant, small and unfavourable effect on attendance rates** (Hedges' $g = -0.03$, $p = 0.792$). However, the CI includes zero, suggesting that the true effect may be either slightly positive or negative and that any apparent variation is consistent with chance. These results suggest that participation in the BDP did not produce a detectable change in attendance in the term following support relative to BAU provision. It should also be noted that the narrow dispersion, smaller sample size and high overall attendance levels also mean that modest changes would be difficult to detect with statistical confidence.

Table 27: Analysis on attendance rates at P2

Outcome	Unadjusted means				Effect size		
	BDP delivery group		Signposting group				
	n (missing)	Mean (95% CI)	n (missing)	Mean (95% CI)	Total n (BDP; signposting)	Hedges' g (95% CI)	p-value
Attendance (%)	116 (127)	89.0 (86.4, 91.6)	118 (125)	89.7 (87.4, 92.0)	234 (116; 118)	-0.03 (-0.29, 0.22)	0.792

Note: BDP = Boys' Development Programme; CI = confidence interval; P2 = two half terms after the treatment period

Exclusions analysis

Table 28 presents the analysis of exclusion and suspension outcomes for boys in the BDP and signposting groups at follow-up (P2). This shows:

- **Statistically significant moderate reductions in the total number of incidents, favourable to the BDP** ($g = -0.24, p = 0.024$). The IRR from the negative binomial regression model was 0.65 (95% CI: 0.45, 0.95, $p = 0.024$), suggesting that boys allocated to the BDP experienced **around 35% fewer exclusion incidents** than those in the signposting group.
- **For boys who experienced a fixed-term suspension, statistically significant large reductions in suspension durations, favourable to the BDP** ($g = -0.50, p = 0.004$). For suspension duration, analyses were restricted to the subset of boys who experienced at least one fixed-term suspension during the study period (i.e. >0 suspension days at either baseline or follow-up).¹⁹ This indicated statistically significant and large reductions in suspension duration at P2, favourable to the BDP ($g = -0.50, p = 0.004$).

These findings indicate that participation in the BDP was associated with **meaningful short-term reductions in both the frequency of exclusions and, among boys who were suspended, the duration of suspensions** compared with BAU support. While these results should be interpreted cautiously due to variation in schools' exclusion recording systems, smaller sample sizes and the exploratory nature of the administrative data, the consistency of effects across both metrics provides encouraging preliminary evidence that the BDP may reduce the number of disciplinary incidents and the time lost to suspension for boys at risk of exclusion.

These administrative findings are consistent with the observed reductions in behavioural difficulties (SDQ externalising behaviours scores) at follow-up, which also indicated favourable effects for the BDP. The convergence of improvements in self-reported behavioural difficulties and school-recorded exclusion outcomes strengthens the plausibility that the programme may have influenced behavioural functioning, with potential implications for subsequent disciplinary incidents.

However, it is also important to consider that schools were aware of which boys were participating in the BDP. This visibility may have influenced discretionary decisions about recording incidents or issuing suspensions, for example, if staff were inclined to give boys on the programme additional leeway or alternative responses. As such, some of the observed reduction in exclusions could reflect differences in school decision-making due to boys participating in the BDP rather than behavioural change alone. These results should therefore be interpreted as exploratory and potentially subject to detection or reporting bias.

¹⁹ Suspension duration analyses were restricted to boys who had experienced an internal or fixed-term suspension only. Boys who were permanently excluded during the period were excluded from this analysis to ensure comparability of outcomes, as permanent exclusion results in removal from school and therefore precludes a meaningful comparison of subsequent suspension durations.

Table 28: Analysis on exclusions and suspensions at P2

Outcome	Unadjusted means at P2				Effect size		
	BDP delivery group		Signposting group		Total n (BDP; signposting)	Hedges' g (95% CI)	p-value
	n (missing)	Mean (95% CI)	n (missing)	Mean (95% CI)			
Number of exclusion incidents (whole sample)	158 (85)	0.39 (0.26, 0.53)	158 (85)	0.65 (0.46, 0.83)	316 (158; 158)	-0.24 (-0.45, -0.03)	0.024
Suspension duration in days (fixed-term suspension subset)	66 (177)	1.70 (1.17, 2.22)	67 (176)	3.10 (2.31, 3.90)	133 (66; 67)	-0.50 (-0.85, -0.16)	0.004

Note: BDP = Boys' Development Programme; CI = confidence interval; P2 = two half-terms after the treatment period

3.3.9 Missing data analysis

This section assesses the role of missing data in the primary analysis on school engagement at T2 and T3. As described in the attrition section, if too many participants drop out or if drop-out patterns differ systematically between groups, this can bias the findings. The same principle applies to missing items within a survey: If, for example, boys with certain characteristics (e.g. those with higher levels of difficulties) tend to skip particular questions, the results could under- or over-estimate the true effect of the intervention. Looking at missing data, therefore, helps us check the quality of the outcome measures and whether we can have confidence that the trial results are not being distorted by systematic patterns of non-response.

As described in section 2.4.8, our approach to missing data involves (1) estimating the prevalence of missing data and (2) if necessary, interrogating the mechanisms of missingness, i.e. determining which factors may be predictive of missingness, and running further analyses accordingly. The next two sections set out findings related to both analyses.

Prevalence of missing data

Table 29 summarises the extent of missing data for the primary outcome (school engagement) at each follow-up point. Missing data can be introduced to an RCT via two mechanisms:

1. **Survey non-completion**, i.e. where a young person may have completed a survey at baseline (T1) but did not complete a survey at follow-up (T2). This is often referred to as missing units.
2. **Item non-completion**, i.e. where a young person completed a survey but did not complete enough items within that survey to enable the calculation of an outcomes score. This is often referred to as missing items.

Assessing these patterns helps determine whether the primary analyses might be affected by bias or reduced precision.

Table 29 shows:

- At **T2**, the overall rate of missing data for the school engagement measure was **very low (4.1%)**, with comparable proportions across trial arms (4.5% in the BDP delivery group and 3.7% in the signposting group). Most missing data resulted from complete non-response (unit missingness) rather than skipped items, with item non-response accounting for less than 1% of cases in each group.
- At **T3**, the overall rate of missing data increased slightly to **9.5%**, reflecting normal attrition over a longer follow-up period but still well within acceptable limits for RCTs. Missingness again remained balanced across arms (9.1% in the BDP delivery group and 9.9% in the signposting group), with no evidence of differential non-response. Item-level missingness was negligible ($\leq 1\%$), suggesting that the measure itself was well understood and consistently completed by participants.

In line with established guidance, the low levels of both survey non-completion and item non-completion and the balance across arms observed in this study provide strong reassurance about the internal validity of the trial. We can therefore have a high degree of confidence that the results presented are not driven by systematic patterns of missingness.

Table 29: Number of missing cases for the primary analysis (n = 486 randomised boys)

Time point	BDP delivery group			Signposting group		
	Unit missing n (%)	Item missing n (%)	Total missing n (%)	Unit missing n (%)	Item missing n (%)	Total missing n (%)
School engagement at Time 2	10 (4.1%)	1 (0.4%)	11 (4.5%)	8 (3.3%)	1 (0.4%)	9 (3.7%)
School engagement at Time 3	22 (9.1%)	0 (0.0%)	22 (9.1%)	21 (8.6%)	3 (1.2%)	24 (9.9%)

Note: BDP = Boys' Development Programme

Mechanisms of missingness

Before deciding how to deal with missing data, it is important to understand why data might be missing. This matters because if certain types of boys are more or less likely to complete follow-up questionnaires, this could bias the study results and affect how confidently we can draw conclusions about the impact of the BDP.

Generally, missing data is classified into three broad categories based on the likely reason that data are missing:

- **MCAR**. This missingness is unrelated to anything relevant to the study. For example, a boy might miss their survey meeting because of train strikes – this has nothing to do with who they are or what their outcomes are. This is the least problematic type of missing data.
- **MAR**. This missingness is related to other known information about the young person, such as age or baseline behaviour, but not to the missing outcomes data themselves. If MAR is a reasonable

assumption, we can use statistical techniques (such as MI) to account for the missing data without introducing bias to the study results.

- **MNAR.** This missingness depends on the unobserved outcome or on factors for which we do not have data. For example, if boys with worse school engagement were less likely to complete a T2 questionnaire than boys with stronger school engagement. This is the most difficult type of missingness to address, as it introduces potential bias that cannot be easily corrected using standard methods.

Missingness diagnostics at Time 2

Given that the proportion of missing data was less than 5% at T2, it is **reasonable to assume that data were MCAR**. This small percentage of missing data provides reassurance that the small amount of missing data is unlikely to have introduced bias, and we can therefore be confident that the results of the trial at T2 are based on a robust and representative sample of participants. As such, further analysis of the mechanisms of missingness or the application of more complex statistical techniques (such as MI) were not required. This approach is in line with [YEF analysis guidance](#) on handling missing data.

Missingness diagnostics at Time 3

Given that attrition rates were slightly higher at T3, logistic regression models were used to test whether missingness on the T3 primary outcome (school engagement) was systematically associated with participant or trial characteristics. Predictors in this model included treatment allocation, baseline school engagement score, school, age, ethnicity, SEND status and FSM eligibility. The outcome was coded such that having T3 data = 1 and being missing at T3 = 0; therefore, an odds ratio (OR) below 1 indicates that a characteristic is associated with a greater likelihood of missing T3 data.

Two variables were found to be significantly associated with missingness: **age** ($p = 0.004$, $OR = 0.64$)²⁰ and **FSM eligibility** ($p = 0.030$, $OR = 0.44$). This means that boys who were older and boys who were eligible for FSM were slightly more likely to have missing T3 data than younger boys or boys who were not eligible for FSM. No other predictors, including treatment allocation or baseline levels of school engagement, were significant at the 5% level. These results provide confidence that differential attrition by arm has not been systematically introduced and that data are not MNAR. They also indicate that data were **MAR conditional on age and FSM**.

To test whether this pattern of missingness influenced the results, a sensitivity analysis of the longitudinal model was run including age and FSM as additional covariates. This test assesses the robustness of the T3 results to any potential bias arising from non-random missing data.

Table 30 shows that **the results of this model were virtually identical to the original model**. In the pre-specified model, the adjusted difference between the BDP and signposting groups at T3 corresponded to a Hedges' g of 0.21 ($p = 0.027$). When age and FSM status were added as covariates, the effect remained

²⁰ Please note, only a small number of boys in the sample were in Year 11 ($n = 22$; see the IPE for further detail), representing around 5% of the baseline cohort. Referrals for Year 11 pupils were also prioritised for the autumn term to minimise potential clashes with the exam period. Given the small size of this group relative to the overall sample and the scheduling arrangements in place, it is unlikely that school-leaving in Year 11 was a substantial driver of missing outcome data, although it cannot be ruled out entirely.

statistically significant and of the same magnitude (Hedges' $g = 0.22$, $p = 0.025$). Mean group scores also remained stable, and the analytic sample size reduced only slightly (from $n = 440$ to $n = 434$) due to the exclusion of cases with "not known" FSM status.

These results provide **reassurance that the ITT findings are robust to potential attrition-related bias**. The small associations between missingness and demographic variables did not meaningfully alter the estimated impact of the BDP on school engagement, supporting the validity of the complete-case analysis at T3 and confirming that the study's conclusions are not driven by systematic differences in who provided follow-up data.

Table 30: Missing at random sensitivity analysis for school engagement at Time 3 (T3)

Outcome	Unadjusted means				Effect size		
	BDP delivery group (n = 221)		Signposting group (n = 222)		Total n (BDP; signposting)	Hedges' g (95% CI)	p-value
	n (missing)	Mean (95% CI)	n (missing)	Mean (95% CI)			
School engagement analysis at T3							
Original model	221 (0)	25.5 (25.2; 25.9)	219 (3)	25.1 (24.7; 25.5)	440 (221; 219)	0.21 (0.02; 0.40)	0.027
Original model plus age and FSM status	217 (4)	25.5 (25.2; 25.9)	217 (5)	25.1 (24.7; 25.5)	434 (217;217)	0.22 (0.03; 0.40)	0.025

Note: BDP = Boys' Development Programme; CI = confidence interval; FSM = free school meals

Finally, while the diagnostic analyses indicate that missingness at T2 and T3 was either MCAR or MAR, it remains theoretically possible that a very small proportion of missing follow-up data reflects unobserved experiences during the trial period (e.g. boys who were excluded being marginally less likely to complete follow-up surveys). However, this affects fewer than 4% of boys at T2 and 9% at T3, it is evenly distributed across treatment arms, and the direction of any resulting bias cannot be reliably inferred. Given the low overall rate of attrition, the absence of differential missingness by treatment allocation and the stability of model estimates in MAR sensitivity analyses, any residual bias arising from such mechanisms is likely to be minimal and would not meaningfully alter the interpretation of the ITT results. Overall, the evidence strongly supports the conclusion that the estimated impact at T3 is robust to attrition-related bias.

4 Implementation and process evaluation results

This chapter provides the findings from the IPE. The findings have been structured in line with our IPE research questions, as highlighted in section 2.5.

4.1 RQ1: How effectively has the Boys' Development Programme been implemented in schools?

4.1.1 Reach

This section addresses the following research question: 1d) Reach. How well has the BDP reached its intended cohort?

The BDP's target cohort is boys in Years 7 to 11 (aged 11 to 16) in areas of high need and deprivation, who are identified as being:

1. At high risk of exclusion from school due to poor behaviour records and/or significant behaviour incidents
2. At high risk of disengagement from school due to achieving below their potential and/or poor attendance
3. At risk of poor outcomes due to known adversity outside school, such as child poverty or previous or ongoing child protection concerns

This section explores how well the BDP has reached its intended cohort, drawing from the analysis of monitoring data and semi-structured interviews with Future Men, school stakeholders and boys.

Target cohort

Monitoring data suggest that **the BDP reached its target cohort**. As outlined in section 2.4.2, schools were provided with clear guidance on referral criteria and eligibility thresholds, including indicators of behavioural concerns, disengagement and risk of exclusion, which were used to inform decision-making. Table 31 sets out the age at the start of support, sex and referral reasons for all recruited boys. These findings demonstrate that the BDP reached its intended cohort across all aspects:

- **All recruited boys were within the target age range of 11 to 16**, with the majority (364 boys, 74.7%) starting support between the ages of 12 and 14. Only 53 boys (10.9%) started support aged 15 or above, with only one (0.2%) boy starting support aged 16.
- **All recruited young people described their sex as male** (487 boys, 100%). This is in line with the BDP's focus on boys.
- **All recruited boys were referred due to concerns about behaviour, engagement or the risk of exclusion from school, in line with the provided eligibility guidance, as set out in section 2.4.2.** The most common reasons for referral were low-level behaviour affecting school engagement (54%), behavioural challenges placing the young person at risk of exclusion (28%) and low attainment due to behavioural challenges (22%). Of those who were referred for other reasons, the most common referral reasons described included social, emotional and mental health concerns and support for the transition from primary to secondary school.

These findings were supported by interviews with Future Men and school stakeholders, who agreed that the BDP had successfully reached its intended cohort.

Table 31: Characteristics and referral reasons for recruited boys (n = 487)

Age	n	%
11	70	14.4%
12	137	28.1%
13	131	26.9%
14	96	19.7%
15	52	10.7%
16	1	0.2%
Total	487	100%
Sex	n	%
Male	487	100%
Female	0	0%
Total	487	100%
Referral reason ²¹	n	%
Low-level behaviour affecting engagement with school	262	54%
Behavioural challenges that place the young person at risk of exclusion	136	28%
Low attainment due to behavioural challenges in class	109	22%
Low attendance	62	13%
Other	56	11%

Baseline risk levels

In addition to referral reasons and stakeholder perspectives, baseline administrative and self-report data provide further insight into the risk profiles of the cohort reached by the BDP.

Administrative data indicate that a **substantial proportion of boys were already demonstrating objective indicators of exclusion and disengagement at baseline**. Approximately 30% of boys had experienced at least one exclusion prior to randomisation, and close to 40% met the Department for Education's definition of persistent absence (attendance $\leq 90\%$). Mean baseline suspension duration was approximately one day across the cohort, and baseline incident counts showed measurable variation between pupils. These school-recorded indicators suggest that a sizeable minority of participants were already experiencing behavioural and attendance difficulties associated with elevated risk of exclusion.

²¹ Please note that percentages for referral reason do not sum to 100%, as multiple reasons for referral could be selected.

Baseline self-report measures present a more nuanced picture. As described in section 4.2.3, several measures of school connectedness and emotional difficulties displayed modest clustering at the positive end of the scale. In particular, the SCQ total score and some subscales indicated relatively high reported levels of school connectedness, whereas SDQ emotional and peer difficulties were concentrated towards the lower end of their scales. These patterns may appear less severe than might be expected for a cohort targeted on the basis of behavioural and disengagement risk.

However, SDQ externalising behaviour scores showed meaningful variation across the cohort, suggesting the presence of behavioural risk even where emotional or peer difficulties were less prominent. The evidence suggests that while some self-reported measures were skewed towards the positive end of the scale, objective administrative indicators and behavioural measures confirm that the programme reached boys experiencing elevated levels of behavioural and attendance-related risk.

Boys from minoritised and marginalised backgrounds

In addition, monitoring data and interviews with Future Men and school stakeholders suggest that **the BDP successfully reached boys from a range of marginalised and minoritised backgrounds** including:

- Boys from minority ethnic backgrounds
- Boys from low-income households
- Boys with SEND

Findings relating to each group are presented below.

Boys from minority ethnic backgrounds

Table 32 sets out the ethnicity of recruited boys for the study compared with local population statistics for young people aged 10–15 in the three London boroughs where participating schools are located (ONS, 2021). This shows that the **BDP successfully reached a cohort with a higher proportion of boys from minority ethnic backgrounds** than the local populations across Lambeth, Southwark and Wandsworth.

Specifically, Table 32 shows that in each borough, boys from minority ethnic backgrounds are substantially overrepresented in the study sample compared to the 2021 Census. For example:

- In Lambeth, 71% of participants identified as Black, Black British, Black Welsh, Caribbean or African, compared with 40.6% in the local population aged 10 to 15 years.
- In Southwark, 67% of the study sample were from this group, compared with 41.3% locally.
- In Wandsworth, 34% of recruited boys were from this group, compared to 16.6% in the Census.

These patterns were also noted by school stakeholders, the majority of whom reported that the BDP successfully engaged boys from a wide range of minority ethnic backgrounds. They attributed this to both the diversity of the school populations and to the project co-ordinators' ability to build rapport and work in a culturally competent and sensitive way.

Boys from socioeconomically disadvantaged backgrounds

Monitoring data show that the **BDP successfully reached boys from socioeconomically disadvantaged backgrounds**. Table 47 (Appendix C) shows that 63.3% of recruited boys were eligible for FSM. Supporting this, the school stakeholders we spoke to stated that their schools are based in communities with high rates of socioeconomic disadvantage, with two school stakeholders reporting that over 60% and 70% of their respective school populations are eligible for FSM and pupil premium.

Boys with special educational needs and disabilities

Monitoring data indicate that the **BDP reached boys with a range of SEND**. Table 43 (Appendix C) shows that 160 boys (33.5%) recruited to the BDP were recorded as having SEND. Table 44 (Appendix C) shows the most frequently recorded needs related to social, emotional and mental health (22%), communication and interaction (13%), and cognition and learning (12%). Only a small proportion of participants (27 boys; 5.7%) had an EHCP, while the majority (94.3%) did not. This indicates that most boys presented with a lower, non-clinical level of need, in line with the programme's eligibility criteria.

These patterns indicate that the BDP effectively reached boys from groups that are disproportionately affected by school exclusion and disengagement, particularly Black boys, boys from low-income households and boys with SEND. This is consistent with the programme's theory of change and targeting strategy, which prioritises those at elevated risk due to known structural inequities.

Table 32: Comparison of ethnicity²² for recruited boys (n = 487) compared with 2021 Census data stratified by London borough

Ethnicity	Lambeth		Southwark		Wandsworth	
	Efficacy study N (%)	Census %	Efficacy study N (%)	Census %	Efficacy study N (%)	Census %
Asian, Asian British or Asian Welsh	2 (2.7%)	5.9%	13 (5.4%)	8.7%	13 (7.9%)	14.0%
Black, Black British, Black Welsh, Caribbean or African	52 (71.2%)	40.6%	161 (67.4%)	41.3%	56 (34.1%)	16.6%
Mixed or multiple ethnic groups	13 (17.8%)	14.9%	15 (6.3%)	12.6%	32 (19.5%)	13.2%
Other ethnic group	2 (2.7%)	6.7%	17 (7.1%)	6.8%	17 (10.4%)	4.6%
White	4 (5.5%)	31.9%	33 (13.8%)	30.5%	46 (28%)	51.5%
Valid total	73 (100.0%)		239 (100.0%)		164 (100.0%)	
Missing (not known/prefer not to say)	4	-	3	-	4	
Grand total	77		242		168	

²² Ethnicity categories use ONS ethnic group classification 6a; more information is available here:

<https://www.ons.gov.uk/census/census2021dictionary/variablesbytopic/ethnicgroupnationalidentitylanguageandreligionvariables/census2021/ethnicgroup/classifications>

Factors impacting reach

Supporting the above findings, the majority of school and Future Men stakeholders agreed that the programme's reach had been a success. They identified the following factors as supporting the BDP's ability to reach its intended cohort:

- **The BDP's reach was supported by strong referral, screening and triage processes** between schools and Future Men. The school stakeholders we spoke to had a consistent understanding of the target cohort and the eligibility criteria for the BDP. They valued the clear recruitment guidance provided by Future Men, stating that this had supported initial referrals. Several school stakeholders highlighted that the initial screening meetings held between schools and Future Men stakeholders were particularly beneficial. They described how these meetings involved discussions of referrals on a case-by-case basis, which enabled the majority of questions about eligibility to be raised and resolved before referrals were made.
- **Peer-to-peer recommendations also supported reach.** One staff stakeholder explained that boys who had already participated in the BDP regularly shared their positive experiences with their peers, which helped generate interest and motivation to engage with the programme. Several boys we spoke to also described first hearing about the BDP through their friends, suggesting that peer recommendations were a valuable factor in encouraging engagement across successive cohorts of the study. While boys only described recommending the programme to their friends, this peer influence also raises the possibility of contamination, whereby elements of the BDP's messages or techniques may also have been informally shared among peers outside the intervention group. While this was not raised by interviewees and was not formally measured, it is possible that such peer diffusion could have contributed to some degree of treatment contamination, potentially making the observed impact estimates conservative. Further discussion on limitations is provided in the conclusion.

While most stakeholders generally viewed the programme's reach as a success, a few challenges were identified that affected the BDP's ability to engage with its intended cohort. These were:

- **Referrals for boys at the upper and lower eligibility thresholds.** Both Future Men and school stakeholders highlighted a few cases of boys being referred to the programme who did not meet the eligibility criteria, including instances of boys being referred with both higher and lower levels of need:
 - **Lower levels of need.** Future Men and school stakeholders reported that in some schools, the profile of referrals shifted over time. Earlier cohorts tended to include boys with higher levels of identified need, while later cohorts included comparatively lower-need pupils, some of whom were described as borderline in terms of eligibility. They stated that this was particularly evident in schools with smaller year groups (e.g. 60 to 80 pupils), where it was challenging to identify 20 boys with consistently high levels of need each term. Future Men stakeholders emphasised that these pupils were still eligible for support, but they noted a downward trend in levels of need over time, reflecting the difficulties some schools faced in maintaining high thresholds. Monitoring data on referral reasons and risk indicators (see Table 31) support this perception, suggesting that all boys were within scope for the BDP. This indicates that any changes in cohort composition were a pragmatic response to school context rather than a deviation from the intended model.

- **Higher levels of need.** Both school stakeholders and the Future Men team reported a few examples of boys who were referred into the BDP but had already started the managed move process, which is an exclusion criterion for support. The Future Men team raised that this was often due to school staff not disclosing key information during the referral process, either because of capacity pressures or miscommunication. This meant that these boys were unable to complete the full BDP before their managed move.
- **Prioritising younger boys to reduce missed lesson time for older students.** To minimise disruption to exam preparation, in some schools, referrals into the study were deliberately limited to boys in Years 7 to 9. Demonstrating this, Table 31 above shows that only 53 boys (10.9%) were referred from the age of 15 upwards. In addition, Table 49 in Appendix C highlights that just 22 boys in Year 11 (4.5%) were referred to the study, with no referrals for boys in Year 11 in two schools. As a result, cohorts in these schools tended to be slightly younger than in other schools, limiting the reach of the programme for boys aged 15 to 16. Future research may wish to further explore the impact of the BDP on boys in this age group.

There were a small number of groups that stakeholders believed would benefit from support but were not currently being reached by the BDP:

- **Students with EAL.** Some school stakeholders raised that they had pupils with limited English proficiency who could benefit from the BDP but were currently unable to be referred due to language barriers. Table 46 (in Appendix C) shows that 91 (19.0%) of recruited boys were recorded as having EAL. However, both stakeholders who suggested this acknowledged the challenge for Future Men to implement this aspect of support, given their size and capacity, and that this would also require stronger joint approaches between school support teams and the Future Men team.
- **Students with significant additional learning needs.** One school stakeholder questioned the exclusion criteria relating to students with more advanced learning or communication needs. They suggested that Future Men could explore ways to enhance the programme's accessibility for these boys, who may also stand to benefit from the support it offers.
- **Students with involvement with children's social care.** Table 48 (in Appendix C) sets out the care status of the boys who were randomised into the programme. The vast majority (466 boys; 97.9%) were recorded as having no involvement with children's social care. Only a very small minority were identified as children in need (3 boys; 0.6%) or looked after children (7 boys; 1.5%), and no participants were recorded as being subject to a child protection plan. These figures suggest that boys with formal social care involvement were underrepresented in the programme, with less of a focus on reaching boys who met the third eligibility criterion, i.e. at risk of poor outcomes due to known adversity factors.

Overall, the **BDP successfully reached its intended cohort**. Both monitoring data and findings from semi-structured interviews demonstrate that the BDP successfully engaged boys from minority ethnic and low-income backgrounds. While a few challenges were noted around borderline referrals, accessibility for pupils with language or learning needs and reaching boys involved with children's social care, the BDP's strong reach was supported by clear referral and screening processes, relatable project co-ordinators and peer-to-peer recommendations.

4.1.2 Dosage

This section addresses the following research question: 1b) Dosage. How much of the BDP has been delivered? How much of the BDP needs to be delivered to have an impact?

This section explores how much of the BDP was delivered, drawing on monitoring data and perspectives from boys, Future Men and school stakeholders.

Monitoring data show that **the BDP was largely delivered as intended**. The programme manual specifies 12 sessions as the intended dosage, with the Future Men team informally considering a minimum of eight sessions necessary for completion. This threshold was operational rather than evidence-based, designed to balance the need for meaningful engagement with the practicalities of delivering the full programme within a school term.²³

Across the entire cohort (n = 245 boys),²⁴ a total of 2,362 sessions were delivered, with boys receiving an average of 9.6 sessions each. Table 33 shows that most boys came close to the intended dosage: Nearly half (49%) attended 10 to 11 sessions, while a further 12% completed the full 12 sessions. An additional 31% received eight to nine sessions, meaning that 92.6% of the cohort received eight or more sessions.

Conversely, only a small minority (16 boys; 7.4%) attended fewer than eight sessions. Within this group, five boys received four to five sessions, two boys received two to three sessions and four boys attended only one session. These findings suggest that the majority of boys (93%) received at least eight sessions, and over 60% came close to or achieved the full intended dosage. This indicates relatively strong fidelity to the programme model, with only a very small proportion unable to complete two-thirds of the intended dose.

Table 33: Number of sessions received (n = 245)

Number of sessions received	n	%
0 to 1	4	1.6%
2 to 3	2	0.8%
4 to 5	5	2.0%
6 to 7	7	2.9%
8 to 9	76	31.0%
10 to 11	121	49.4%
12 to 13 ²⁵	30	12.2%
Total	245	100.0%
Average number of sessions		9.6

²³ The evaluation team also advised that project co-ordinators should aim to deliver as much of the programme as possible, as there was no empirical evidence that eight sessions would be sufficient to achieve the intended outcomes.

²⁴ Please note that the analysis in this section relates to all 245 boys who received the BDP. This includes the 243 boys who were allocated to the BDP delivery group and two boys were initially allocated to the signposting group but received the BDP by mistake.

²⁵ Please note that only four boys received 13 sessions, i.e. 1.6% of the whole treatment group received one more session than the intended dosage.

While the overall number of sessions received was high, monitoring data shed some light on the reasons behind the variation. Of the 245 boys who took part in the BDP, most were offered a full programme of support: 82.1% of boys were offered between 11 and 19 sessions, with an average of 12.1 sessions offered to each boy (see Table 53 in Appendix C). This is in line with the agreed approach to delivering the BDP, whereby boys who had missed a few sessions over the course of the school term were provided with additional opportunities to catch up. Attendance rates were generally high, with an average attendance rate of 88.2% across the entire sample. The average number of missed sessions was 1.3 sessions per boy, and Table 34 shows that reasons for missed sessions were most often due to school absence (36.6% of all missed sessions) or school disruption²⁶ (30.3% of missed sessions), rather than due to refusal to attend (6.3%).

Table 34: Reasons that missed sessions did not go ahead as planned broken down by cohort (n = 604 sessions)

Cohort	Reasons that sessions did not go ahead as planned					Total
	Young person was absent from school	School disruption	Project co-ordinator absence	Young person was suspended	Young person declined a session	
1	78 (47%)	31 (18.7%)	24 (14.5%)	22 (13.3%)	11 (6.6%)	166 (100%)
2	67 (27.7%)	84 (34.7%)	52 (21.5%)	22 (9.1%)	17 (7%)	242 (100%)
3	35 (35%)	32 (32%)	21 (21%)	6 (6%)	6 (6%)	100 (100%)
4	41 (42.7%)	36 (37.5%)	9 (9.4%)	6 (6.3%)	4 (4.2%)	96 (100%)
Total	221 (36.6%)	183 (30.3%)	106 (17.5%)	56 (9.3%)	38 (6.3%)	604 (100%)

An analysis of length of support also shows that **most boys engaged with the BDP for the intended duration, although 20% received support for less than 10 weeks**. Just under half (49%) of boys remained in the programme for 11 to 15 weeks, reflecting the intended weekly structure of delivery.²⁷ A further quarter (25%) received support lasting 16 to 20 weeks, while a minority (5%) continued beyond 20 weeks. Cases where support lasted for more than 12 weeks were typically in cohorts where delivery was extended across school terms for pragmatic reasons. At the other end of the scale, around one in five boys (20%) received 10 weeks or fewer of support, typically due to shorter school terms, early exit from school or project co-ordinator turnover.

²⁶ Reasons for school disruption included, but were not limited to, teacher strikes, delivery rooms becoming unavailable at short notice or timetable scheduling issues beyond the control of the Future Men team (such as last minute exams) preventing the boy from leaving lessons.

²⁷ The BDP is a 12-session programme delivered weekly over 12 weeks. Within the planned delivery timescales, the team includes a few additional buffer weeks for boys who may have missed sessions due to absence to catch up as needed – hence why delivery spanned from 11 to 15 weeks in most cases.

Table 35: Length of support in weeks (n = 245)

Length of support (weeks)	Number of boys	%
0 to 5 weeks	7	2.9%
6 to 10 weeks	42	17.1%
11 to 15 weeks	121	49.4%
16 to 20 weeks	62	25.3%
21 to 25 weeks	1	0.4%
26 to 30 weeks	3	1.2%
31+ weeks	9	3.7%
Total	245	100%
Average length of support		13.6 weeks

Insight into this pattern is provided from findings from semi-structured interviews, where **the majority of boys we spoke to reported receiving the intended dosage** as outlined in the theory of change and programme manual. Most boys reported that they met weekly with their project co-ordinator across a school term, only missing sessions due to illness or absence from school. Boys shared that they appreciated seeing their project co-ordinator weekly and that the duration of the sessions was appropriate, providing time for them to check in, cover the content in the slides and discuss anything that they wanted to discuss.

Despite this broadly positive feedback, the following challenges to the overall length of the BDP were raised by the boys and school stakeholders we spoke to:

- **Concerns about the length of support.** Several boys shared that they wished the BDP had been longer and that they would have welcomed a more prolonged period of support engagement. A few boys raised that they were only starting to understand some aspects of the topics and how they could apply to their lives and noted that they might have benefitted from additional sessions.
- **Concerns about whether any change can be sustained.** A few school stakeholders suggested that the 12-week timeframe is insufficient to generate sustained change, particularly given the complexity of some boys' lives. They gave examples where boys had initially shown improvements in behaviour at the end of support but described that these improvements had gradually diminished after the programme ended.

"There's a significant impact for quite a lot of the children, but then, slowly over time, it drops unless there's regular contact...a 12-week programme is not going to...change every single child for the long term." **Female teacher, School A**

Overall, the dosage analysis suggests that the BDP was delivered with strong fidelity and high levels of participant engagement, with most boys receiving the intended dosage of at least eight weekly sessions over a school term and valuing both the frequency and duration. This is a notable achievement, given that the programme worked with a cohort of boys experiencing multiple needs within complex school environments, which likely reflects the strength of the relationships established between project co-

ordinators and boys – relationships that, as evidenced in the impact evaluation and discussed further in section 4.3.1, were a central mechanism for engagement and change.

Future Men informally aimed for every boy to complete a minimum of eight sessions. In practice, this threshold was used pragmatically to guide delivery, particularly in determining whether additional catch-up sessions should be offered when boys had missed sessions, given the timescale constraints of delivering within a school term. While this reflects a realistic and operationally feasible definition of completion, the impact evaluation findings suggest that this level of engagement may not be sufficient to realise the full benefits of the programme. The CACE analysis (see section 4.3.6) indicates a dose–response relationship, with boys who received a higher number of sessions, particularly those who attended close to the full 12-session model, demonstrating greater improvements in school engagement. This suggests that, although eight sessions may represent a meaningful level of exposure, there is additional benefit associated with receiving the full programme. As such, maintaining high attendance and maximising continuity of delivery should remain a central focus for future implementation and scale-up.

It is important to note that concerns about the sustainability of change from a few stakeholders may sit in tension with the observed improvements at T3. However, T3 captured outcomes 12 weeks after programme completion and does not provide evidence about longer-term durability beyond this period. The qualitative accounts therefore highlight uncertainty about whether gains would persist over extended periods without continued support. Longer-term follow-up would be required to determine whether effects are sustained, diminish or strengthen over time.

4.1.3 Fidelity

This section addresses the following research question: 1a) Fidelity. To what extent has support been delivered in line with the BDP’s theory of change and protocols?

This section outlines findings on the fidelity of the Future Men BDP, addressing how closely support was delivered in line with the BDP’s theory of change and programme manual.

The BDP consists of 12 weekly sessions, each lasting the length of a standard lesson (50 to 60 minutes, depending on the school) and delivered over a 12-week period. The BDP is a manualised intervention, meaning there are fixed elements that all boys receive, alongside structured flexibility that allows project co-ordinators to tailor delivery to individual needs and interests.

The BDP covers six key topics, as shown in Figure 3.

Figure 3: Topics covered by the Boys’ Development Programme



Each topic is delivered through a core session, which all boys are expected to receive. Each topic also has an associated set of supplementary sessions, which can be selected and adapted by project co-ordinators depending on the needs and interests of the individual boy. This design builds in flexibility while maintaining

a clear framework, allowing project co-ordinators to emphasise or revisit particular topics, such as conflict or emotional regulation, if especially relevant to a boy's circumstances at the time.

Table 36 indicates strong fidelity to the intended model:

- **Nearly all boys received a comprehensive version of the programme.** Across the full BDP cohort (n = 245), 92% covered at least five of the six core topics and 61% covered all six. Among boys who received at least eight sessions (n = 226), delivery fidelity was even higher, with 96% covering at least five core topics and nearly two-thirds (64%) covering all six.
- **Almost all boys received at least one session on each of the six core topics.** Table 36 shows that core coverage was very high for rules and boundaries (97%), masculinity (94%), emotions (94%), conflict (94%), communication (82%) and aspirations and goals (82%).
- **The topics most regularly covered in supplementary sessions were conflict and emotions.** Table 37 also shows that 70% of boys had at least one supplementary session on conflict, 61% on emotions and 52% on communication. Masculinity (45%), rules and boundaries (36%) and aspirations and goals (34%) were also delivered as supplementary content, albeit less commonly. This indicates that while all themes were covered, facilitators prioritised certain areas for deeper exploration.

Table 36: Number of core topics covered by the entire Boys' Development Programme (BDP) cohort (n = 245) and by boys who had received at least eight sessions (n = 226)

Number of core topics covered	Whole BDP cohort (n = 245)		Boys who received 8+ sessions (n = 226)	
	n	%	n	%
0 topics	4	1.6%	0	0.0%
1 topic	0	0%	0	0.0%
2 topics	2	0.8%	0	0.0%
3 topics	3	1.2%	1	0.4%
4 topics	11	4.5%	9	4.0%
5 topics	76	31.0%	71	31.3%
6 topics	149	60.8%	146	64.3%
Total	245	100.0%	226	100%

Table 37: Number (%) of boys who received a core session or at least one supplementary session on each of the six topics (n = 245)

Number of boys	Rules and boundaries	Masculinity	Emotions	Conflict	Communication	Aspirations and goals
Core sessions (%)	238 (97.1%)	231 (94.3%)	230 (93.9%)	229 (93.5%)	202 (82.4%)	202 (82.4%)
Supplementary sessions (%)	88 (35.9%)	110 (44.9%)	150 (61.2%)	171 (69.8%)	128 (52.2%)	82 (33.5%)

Qualitative data from interviews with Future Men stakeholders and boys also suggests high fidelity to the intended model. Project co-ordinators stated that the **majority of boys covered all core topics**. Among the boys we spoke to, there was a shared understanding of the sessions that had been covered. These aligned with those in the programme manual, including masculinity, conflict, communication, emotions and goals and aspirations. Boys reported that they were satisfied with their weekly sessions and the spread across themes, finding the intervention content to be relevant and beneficial.

The main reported challenge to fidelity was **staff turnover**, which affected the continuity of delivery in a small number of schools. While the Future Men management team responded quickly to redeploy staff, some boys experienced multiple project co-ordinators or fewer sessions than intended. This may have undermined the quality of the relationships these boys were able to build with the project co-ordinators, impacting the effectiveness of the programme. Further detail about staff turnover is provided in section 5.2.1.

Overall, the BDP was delivered with high fidelity to its theory of change. Most boys received the six core topics as intended, with supplementary sessions providing opportunities for deeper or tailored exploration. The balance between structure (manual and slides) and flexibility (adaptation to individual needs) was seen as a strength of the model. While staff turnover occasionally disrupted continuity, the programme was implemented largely as designed, with only minor gaps or adaptations.

4.1.4 Quality

This section addresses the following research question(s): 1c) Quality. How well has the BDP been delivered?

This section addresses how well the BDP has been delivered. Evidence from boys, school stakeholders and project co-ordinators suggests that the quality of delivery was strong overall, with several features consistently highlighted as strengths.

The following key findings were raised as supporting the delivery of the BDP to high quality:

- **A skilled, passionate and flexible team of project co-ordinators.** Boys and school stakeholders identified the commitment and capability of the project co-ordinators as key contributors to high-quality delivery. Project co-ordinators were praised for their skill, flexibility and dedication to supporting boys, with school stakeholders highlighting the strong relationships they built with boys. Several stakeholders described co-ordinators as particularly effective at creating a sense of respect and psychological safety for boys from a range of backgrounds, including boys from minority ethnic backgrounds, which supported honest conversations. Project co-ordinators themselves credited Future Men's reflective practice sessions (see section 4.2.1 for more detail) with helping them refine their approach and sustain quality.
- **The BDP's programme slides and manual provided structure and consistency.** Project co-ordinators also highlighted the value of the BDP's programme slides and manual in ensuring consistent delivery. They explained that the slides helped anchor sessions to their objectives while still leaving space for flexibility and tailoring. The programme manual was described as a useful resource for standardising content and ensuring all staff had access to the same materials. Several project co-ordinators noted that over time, they relied less on the manual as they became more confident in the programme structure.

- **Strong, positive working relationships between schools and Future Men.** Most school stakeholders also praised the positive working relationships between schools and Future Men. They said project co-ordinators were proactive communicators who understood the competing demands schools faced and delivered with professionalism and consistency. One school stakeholder reflected:

“[Project co-ordinators] really understood our context; schools are very busy places with competing priorities, and they have managed to work with us by asking us what is expected without them being rude; they have done that in a professional way.” **Male teacher, School F**

- **Project co-ordinators advocating for boys outside of the sessions.** Both boys and school stakeholders valued the added quality from project co-ordinators advocating for boys outside of sessions. Boys said that when they faced challenges with teachers, project co-ordinators would often raise them with their teachers, representing their perspectives and helping to resolve misunderstandings. One boy explained:

“If I have a problem with a teacher, like my maths teacher picking on me, [project co-ordinator] can speak to the teacher and tell him what helps me and then also telling me how I should behave.” **Leo,²⁸ participating boy, School B**

Several school stakeholders agreed that advocacy was an important part of high-quality delivery, particularly for boys who felt misunderstood by teachers. They also noted a few instances of project co-ordinators supporting boys outside formal sessions, for example, by joining parental meetings or offering informal check-ins. They described this as evidence of a high-quality, holistic approach to support.

Findings from interviews were broadly positive around the factors impacting the quality of delivery. However, project co-ordinators suggested that **updating programme content to reflect a wider range of themes** would further enhance the quality of delivery. Some project co-ordinators said they felt less confident delivering support on topics such as online influencers (e.g. Andrew Tate and the “manosphere”) and LGBTQIA+ identities. They emphasised the importance of regularly reviewing and updating session materials to keep pace with boys’ lived experiences.

In summary, boys, school stakeholders and project co-ordinators consistently described the BDP as being delivered to a high standard. Skilled and relatable staff, clear programme resources, strong school relationships and advocacy were central to this quality. The main identified area for improvement was ensuring that programme materials continue to evolve so that project co-ordinators are fully equipped to address the issues most relevant to boys today.

4.1.5 Responsiveness

This section addresses the following research question: 1e) Responsiveness. To what extent have boys engaged with the intervention?

²⁸ As set out in section 3.5.2, all names used in this report are pseudonyms. They have been assigned to protect the identities and privacy of the boys who participated in the research.

This section explores how well boys engaged with the BDP, drawing on monitoring data and perspectives from boys, Future Men staff and school stakeholders.

Qualitative research suggests that **overall engagement with the BDP was strong** among the boys we spoke to. Boys consistently reported attending and enjoying most sessions and finding the content relevant and accessible and described their project co-ordinators as trustworthy, relatable and easy to talk to. The majority of Future Men and school stakeholders agreed that boys were enthusiastic about the BDP sessions, stating that their attendance was consistently high, with few boys not completing the programme.

Several factors were highlighted by boys, school stakeholders and Future Men stakeholders as supporting this engagement:

- **Safe and trusting relationships.** Boys said that they valued spending time with a project co-ordinator who they trusted and could talk to openly. They said project co-ordinators listened to them and understood their circumstances, which helped build relationships that encouraged honest discussion and engagement in the sessions. One boy explained:

"[Project co-ordinator] is someone I could turn to if I needed help, like when anything happens at school, I could talk to him about it, and this was really good." **Jordan, participating boy, School C**

- **Relatable project co-ordinators.** Project co-ordinators, school stakeholders and boys all highlighted the benefits of project co-ordinators being relatable to the boys. Project co-ordinators were often closer in age to the boys than teachers, shared similar lived experiences or reflected the communities in which they worked, demonstrating cultural competence and understanding of local contexts. These qualities were consistently identified as key factors in building rapport. For some boys from minority ethnic backgrounds, this contributed to a stronger sense of respect and of being understood, which supported trust and engagement.

"I get on with [project co-ordinator] because he's relatable, and he knows what boys go through." **Ezra, participating boy, School H**

- **Enjoyable session formats.** The majority of boys described the BDP sessions as fun and enjoyable. They particularly appreciated the inclusion of warm-up games and activities at the beginning of the session, as well as the ways in which their project co-ordinators were able to interact with them in an informal way. Boys generally viewed the sessions as a welcome break from lessons and separate from any pressures that they may have been facing at school. This enjoyable atmosphere supported consistent attendance and engagement.
- **Relevant and applicable content.** Boys reported enjoying the content of the BDP sessions, valuing the relevance to their lives. Several boys appreciated how their project co-ordinator combined the content in the slides with interactive delivery methods that helped keep their attention and provide information in a more accessible manner. Several boys said project co-ordinators tailored discussions by using real-life scenarios and practical activities and showed interest in their experiences inside and outside school. They said that this helped them stay engaged and apply what they had learned.
- **Responsive and agile delivery.** Project co-ordinators said the structure of the BDP, particularly the flexibility of the supplementary sessions, allowed them to tailor content to boys' needs and interests. School stakeholders appreciated that sessions could be adapted to address recent events, for

example, focusing on conflict management after a school incident. Both groups said this responsiveness increased relevance and engagement.

Despite these strengths, some challenges with engagement were highlighted:

- **Relationship with referrers.** Some project co-ordinators highlighted that a few boys were more reluctant to engage with the BDP because of their relationship with the member of school staff who had referred them. This made them more sceptical about the programme and their reasons for being selected.
- **Consistency of onboarding processes.** Several boys said that at the start, they were unsure why they had been referred or what the programme involved. Some described their participation as feeling prescriptive or school-directed, which limited early engagement.
- **Timetabling of sessions.** A minority of school stakeholders said some school staff had expressed concerns about boys missing lessons to attend BDP sessions. Despite efforts to rotate timings, this remained a tension in some schools. A few boys also said they were less willing to attend if sessions clashed with subjects they particularly enjoyed, such as PE.

Overall, most boys, school stakeholders and project co-ordinators said engagement with the BDP was strong. The relational qualities of project co-ordinators, enjoyable and relevant content, and flexible delivery all promoted engagement. To further strengthen responsiveness, the programme could focus on clearer onboarding processes and on continuing to work closely with schools to reduce timetabling conflicts.

4.1.6 Intervention differentiation

This section addresses the following research question(s): 1f) Intervention differentiation. To what extent is the BDP sufficiently different from existing practices within schools?

This section outlines the extent to which the BDP is sufficiently different from existing practices within schools. Findings from interviews with school stakeholders and Future Men stakeholders suggested that:

- **The BDP filled a gap in support for boys at risk of disengagement.** The BDP was viewed by school stakeholders as filling a gap by providing early, targeted support to a group of boys that may have gone unnoticed until their risk of disengagement escalated. School stakeholders noted that the intended cohort of boys did not meet the thresholds required to access existing support services but exhibited key risk factors for disengagement and exclusion, particularly around challenges with communication, emotional regulation and expressions of masculinity.

“Future Men do some really great work on identity for children who are maybe quieter and wouldn't automatically fit into another intervention but have reached a certain amount of negative [behaviour] points.” **Female teacher, School D**

- **The BDP has a unique focus on boys and masculinity.** School stakeholders stated that they felt that the BDP's focus on boys and masculinity was distinct. They stated that, unlike interventions that primarily use sport or creative activities as an engagement hook, the BDP offers a structured space for boys to explore topics such as identity, relationships and emotional regulation. This approach was viewed as a unique and valuable aspect of the BDP compared to other available support.

- **There was a pre-existing varied and wide-ranging support offer within schools.** While the BDP was viewed as unique, school stakeholders also reported that boys in the signposting group had access to a wide range of existing one-to-one support within schools. The exact offer of support varied but typically included both universal and targeted support aimed at achieving similar outcomes to the BDP:
 - **Universal support.** School stakeholders noted that all boys had access to a universal support offer, typically a school-wide pastoral programme. This included aspects of the core secondary curriculum covering sexual health, mental health and wellbeing, behavioural support, Personal, Social, Health and Economic (PSHE) lessons and access to a tutor/pastoral lead, some of which have similarities to the content covered by the BDP.
 - **Targeted support.** Further targeted support for individuals or small groups was also available in all schools, although the nature and intensity of this provision varied. Many school stakeholders reported a range of additional targeted support programmes for individuals or small groups across most participating schools. These typically included mentoring and behaviour support delivered by school staff, local authorities or external organisations, sometimes using sports or creative activities as an engagement tool. However, the evaluation did not seek to quantify or measure which boys in the signposting group accessed which forms of provision or the extent of their participation in these activities.

These findings suggest that the BDP offered a distinctive form of support, with its focus on boys and masculinity setting it apart from other available programmes. The school stakeholders we spoke to believed that the programme filled an important gap by providing boys with a structured space to explore identity, relationships and emotional regulation. However, they also highlighted that boys in the signposting group had access to a broad range of existing universal and targeted provision, which, in some cases, took similar approaches and aimed to achieve similar outcomes. While this reflects the reality of delivering a trial in a school environment, the implications for the findings of this study should be considered. In particular, it is likely that any resulting differences between the BDP and BAU groups may be smaller than if the support offer for boys in the signposting group did not involve comparable mentoring programmes and one-to-one support.

4.2 RQ2: Which factors have acted as enablers of or barriers to implementation of the Boys' Development Programme?

This section examines the organisational, school and community factors that enabled or constrained implementation.

4.2.1 Organisational factors

This section addresses the following research question: 2b) Which factors have impacted implementation at the organisational level? For example, capacity, skills and training, co-ordination, and resources?

Key organisational enablers that were highlighted in the semi-structured interviews included:

- **Management and oversight from the Future Men senior leadership team.** All project co-ordinators praised the Future Men's management and senior leadership team, noting that their patience, flexibility and continued communication with school stakeholders had been a key enabler of the

successful implementation of the BDP. They gave examples of the ways in which they had overcome unexpected challenges, such as managing school relationships and staff turnover, stating that they had played a key role in the successful implementation of support.

- **Skilled and relatable delivery workforce.** Future Men stakeholders, school stakeholders and boys all pointed to the skill and relatability of the workforce as a major organisational strength. As described in sections 5.1.2 and 5.1.5, school stakeholders said that project co-ordinators were consistently able to balance professionalism with approachability, building rapport while maintaining authority. This suggests that Future Men's ability to recruit and support a workforce with the right blend of lived experience and professional skills is a critical feature of the model and one that underpins both delivery quality and responsiveness.
- **Weekly reflective practice sessions.** Several project co-ordinators also highlighted the importance of weekly reflective practice sessions facilitated by the Future Men management team. They said these sessions gave them a dedicated space to reflect on challenges from recent sessions, share experiences and problem-solve together. Project co-ordinators explained that this helped them refine their delivery and adapt to boys' needs, which they saw as critical to maintaining quality.

Key organisational barriers that impacted the implementation and delivery of the BDP were:

- **Scaling up delivery in the context of an RCT.** Future Men stakeholders described the scaling up of delivery required for Future Men to effectively deliver an RCT as organisationally demanding. Future Men stakeholders reflected that they were a small organisation with no prior experience of delivering an RCT and that they had to double their workforce and alter the structure of their organisation while implementation was ongoing. The fact that Future Men achieved this scale-up so rapidly in the context of an RCT is a considerable achievement.
- **Staff turnover.** Staff turnover also posed a challenge. Both Future Men stakeholders and school stakeholders described examples in which project co-ordinators left at short notice, often part-way through a school term. To minimise disruption, the Future Men management team redeployed project co-ordinators from other schools and reallocated staff from other projects. While this enabled delivery to continue, it placed additional strain on resources and reduced continuity for some boys. In two schools, this meant that groups of boys experienced more than one project co-ordinator during the programme, while in another, delivery was compressed to eight sessions rather than the full intended dosage. School stakeholders recognised that the management team adapted quickly and professionally to cover gaps, but noted that the discontinuity may still have affected the stability of relationships and the quality of support for a small number of boys.

At the organisational level, strong leadership, skilled project co-ordinators and structured reflective practice created a supportive foundation that enabled high-quality delivery and responsiveness to school contexts. Challenges around the rapid scale-up required for the RCT tested organisational capacity, with staff turnover and resource pressures affecting fidelity and dosage for some boys.

4.2.2 School and wider community-level factors

School level factors

This section addresses the following research question: 2a) Which factors have impacted implementation at the school level? For example, level of need, readiness for change and/or policy, practice and funding context?

This section outlines the school-level factors that influence the implementation and delivery of the BDP. Interviewees highlighted the following themes as key enablers at the school level:

- **Level of embeddedness within the school environment.** Project co-ordinators reported that it was easier to implement the BDP in schools where project co-ordinators were well embedded within the school environment. In these schools, project co-ordinators were given access to school systems (e.g. attendance registers and behavioural logs) and provided with school email addresses. Project co-ordinators said this allowed them to build stronger relationships with staff and respond quickly to any incidents experienced by boys. School stakeholders in these settings agreed that integration made delivery smoother. One school stakeholder reflected:

“Our facilitator is so skilful and fits in...with the culture of the school; they understand what we were and where we are going to; we were a school in flux, and that's not always easy to work within.” **Female teacher, School C**

- **Dedicated space to deliver sessions.** The availability and consistency of physical space for sessions varied across schools, which, at times, impacted delivery. Some schools ensured a dedicated room was always available, which supported establishing a routine for the young person. Other schools, particularly smaller schools with fewer rooms, struggled to offer a consistent space, resulting in sessions being relocated frequently or having to end prematurely if a room had been double-booked. This lack of stability was reported by Future Men stakeholders to affect delivery quality and to make it harder for project co-ordinators to provide support.
- **The capacity and authority of the main point of contact.** Project co-ordinators said implementation worked best when the designated school contact was senior enough to make decisions but also had time to engage with operational requirements. In schools where contacts were overstretched, referrals and room bookings were delayed, creating knock-on challenges. Some project co-ordinators suggested nominating a secondary contact to keep communication flowing when the main contact was unavailable. This was particularly important given the competing demands on staff, as it helped maintain smooth communication and logistical arrangements when the main contact was unavailable or had limited capacity.
- **Parent/carers engagement.** Project co-ordinators emphasised that being able to engage directly with parents and carers was a key enabler of positive outcomes. In schools where this contact was allowed, project co-ordinators built stronger relationships with families, facilitated information sharing and provided more tailored and consistent support for boys. In contrast, when direct contact was restricted, delivery became more challenging. Project co-ordinators said this limited their understanding of boys' wider contexts and made it harder to reinforce learning at home.

School-level factors played a critical role in shaping how the BDP was delivered. Implementation was strongest when project co-ordinators were embedded in school systems, had a consistent space, worked with senior and available contacts, and were able to engage with parents. Where these conditions were absent, project co-ordinators faced barriers that limited responsiveness and stability. These findings highlight that successful implementation depends on the school's capacity and willingness to create the necessary conditions for the programme to thrive.

Wider community-level factors

This section answers the following research question: 2a) Which factors have impacted implementation at the wider community level? For example, level of need, readiness for change and/or policy, practice and funding context?

Fewer community-level factors were raised in interviews as impacting implementation. However, the following themes were suggested as being important:

- **High levels of local need.** In most schools, stakeholders described the context of disproportionate rates of exclusions experienced by boys, as well as rates of serious youth violence in surrounding communities. They said boys often felt pressure to present strength and resilience in public, which was hard to set aside in school. The BDP was valued as a safe space where boys could explore these pressures and develop healthier ways of expressing themselves.
- **Cultural barriers impacting readiness for change.** One school stakeholder described stigma around mental health, mentoring and counselling in their school community. They said some parents were suspicious of such support, viewing it as conflicting with cultural or conservative values. This created barriers to securing consent for boys' participation. The stakeholder suggested that clearer communication and promotional materials from Future Men could help overcome this challenge.

4.3 RQ3: What are young people's experiences of support?

This section addresses the following research question: 3) What are young people's experiences of support?

This section explores boys' experiences of support, i.e. what they, school stakeholders and project co-ordinators said about how the programme felt in practice and how experiences differed across subgroups (including those from minority ethnic backgrounds, from low-income households and with SEND). We also consider perceptions of progress against the short-, medium- and long-term outcomes set out in the BDP's theory of change.

Across interviews, the **boys we spoke to consistently described positive experiences of the BDP**. When asked about aspects of the programme that they would change or improve, boys generally struggled to identify any aspects that could be changed for the future. Almost all boys said they would recommend the BDP to other boys, typically because they believed that the BDP had helped them achieve their objectives, and they wanted other boys to have the same opportunity. This was reflected across boys from a range of backgrounds, including boys from minority ethnic backgrounds, who often described the relationship with their project co-ordinator as a key reason they engaged with the programme.

"Yeah, I'd definitely recommend; my friend who has done it has been changed, and I've stopped getting detentions as well. [Project co-ordinator] changes people in a good way and would be able to help you if you need it." **Michael, participating boy, School F**

Project co-ordinators echoed this, saying boys' high levels of engagement and consistent attendance showed that most valued their participation. Boys, school stakeholders and project co-ordinators linked this engagement to a range of positive outcomes, including better educational engagement, improved understanding of male development and more constructive communication, changes which align closely with the short-term outcomes in the theory of change.

The next section explores the specific aspects of the BDP that boys, school stakeholders and project co-ordinators identified as driving these positive changes, shedding light on the mechanisms through which the programme supported progress. This is then followed by perceived progress towards outcomes from boys, school stakeholders and the Future Men team.

4.3.1 Which aspects of the BDP have supported positive outcomes?

This section addresses the following research question: 3a) Which aspects of the BDP have supported positive outcomes?

Across interviews with boys, school stakeholders and project co-ordinators, the following aspects of the BDP were highlighted as supporting positive outcomes.

- **Trusting relationships with project co-ordinators.** Most boys said they trusted their project co-ordinators and felt able to speak openly with them. They explained that being listened to and understood helped them share concerns they would not have shared with teachers. Several school stakeholders also observed that these trusting relationships had supported calmer responses in class and reduced conflict. One boy reflected:

"I can trust him; midway through the support I felt like I could tell him stuff I was worried about. He would listen to me when I wanted to talk and be engaged in what I was trying to say; he told me information in a funny way so I could understand it." **Benjamin, participating boy, School E**

Project co-ordinators described building this trust through a **non-judgemental approach to support**, focused on empathy and understanding. One explained:

"It's all about empathy, trying to understand where they are coming from...and just being a good listener to the young person. It's about being open and transparent and without pushing them, just allowing them to be themselves without feeling like they're being judged." **Future Men team member**

- **Goal setting and review.** Boys, school stakeholders and project co-ordinators identified the programme's approach to goal setting and progress review as a key driver of change. Project co-ordinators supported boys to set SMART²⁹, personalised goals, often around attendance, punctuality and behaviour, and reviewed progress weekly, helping to build a sense of accountability and ownership. Boys reported that setting goals gave them something constructive to focus on and encouraged greater responsibility for their actions.

²⁹ SMART stands for Specific, Measurable, Achievable, Relevant and Time-bound

"[Project co-ordinator] would set goals at the end of each session which gave me something to work towards, and if I get a warning in class, it would make me think about how I can not get a detention." **Ellis, participating boy, School F**

- **Use of behaviour records as a reflective tool.** Several project co-ordinators explained that they regularly reviewed school behaviour logs at the start of each session with boys. Boys explained that this helped them reflect on incidents, recognise progress and identify strategies for improvement. School stakeholders agreed that this reflective approach reduced repeated misbehaviour. One boy said:

"We went through my behaviour and achievement points log at the start of the session and thought about what happened in each situation and what I'd do differently another time." **Jamie, participating boy, School E**

4.3.2 Perceived outcomes from support

This section summarises boys', school stakeholders' and project co-ordinators' perceptions of the progress boys made towards the outcomes set out in the BDP's theory of change. Findings are organised into short-, medium- and long-term outcomes in line with the theory of change. It is important to note that these are qualitative perceptions drawn from a small sample and therefore should be interpreted accordingly. In addition, we did not consult with boys in the signposting group or with school stakeholders about boys in this group, so we do not know the extent of similar perceived outcomes for these boys. The extent to which these perceived changes are borne out in measurable outcomes will be explored through triangulation with the findings of the impact evaluation set out in the conclusion.

Short-term outcomes

Boys and school stakeholders reported that participants had achieved a range of short-term outcomes aligned with the programme's theory of change. These included:

- **Strategies for managing feelings and behaviours.** Many boys said they had learned practical tools for managing emotions, such as breathing exercises and stress-relief techniques. They described these as easy to use and memorable, helping them regulate their emotions more effectively in and outside lessons. Some boys added that this helped prevent situations escalating to the point of disciplinary action.

"Breathing techniques really help me to calm down. I use them when I am angry; it helps to take a moment and calm down." **Sam, participating boy, School C**

- **Strategies for dealing with conflict situations.** Boys consistently reported learning ways to de-escalate conflict, avoid retaliation and maintain control in tense situations. Some also said they had begun to use these strategies to support their peers.

"I now avoid situations where things could escalate. For example, if I was walking in the corridor and someone does something stupid, before I would lash out and punch them, but now, I would just leave it and remove myself from the situation." **Luca, participating boy, School B**

- **Improved recognition of emotions.** Several boys said they had become better at recognising and reflecting on their emotions, linking feelings with behaviours and considering consequences before reacting.

- **Improved understanding of masculinity.** Boys said the programme helped them question what it means to “be a man.” They described realising that some expressions of masculinity can be harmful or combative, with one boy saying:

“I remember that...some men are afraid and stuff because they want to be seen as the alpha or the big person. I learned that you don’t need to be afraid to show people who you are, and you don’t need to prove anything to anyone.” Tyler, participating boy, School C

Several boys described that this in turn gave them permission to express their emotions and question internalised beliefs about their perceptions of masculinity.

“I learned that it is OK for men to cry. Because the way I’ve grown up, I’ve never seen my dad cry, and I’ve kind of just brought that in.” Alex, participating boy, School F

- **Improved understanding of how communication impacts relationships.** Boys reported that participation in the BDP had helped them to better understand how communication affects their relationships, particularly with teachers and peers. They described improvements in their communication skills, including greater awareness of tone and the emotional impact of their words.

“Now when [a misunderstanding with a teacher] happens, instead of talking back, I would wait until the end of the lesson and come to the teacher about it then so I could explain what really happened. Then I don’t get into trouble as I’m not disrupting the class, so I haven’t gotten any behaviour points.” Cameron, participating boy, School H

Several boys also noted that they had developed a more thoughtful approach to how they communicate, including pausing to reflect on the situation before speaking:

“Usually, I don’t think about what I’m going to say, so I can say something wrong and then upset people. [Project co-ordinator] discussed how I need to think before I talk and read the situation, so now I try and think before saying something wrong.” Emmanuel, participating boy, School B

Medium term outcomes

Boys, school stakeholders and project co-ordinators reported some evidence of the following emerging medium-term outcomes from the theory of change:

- **Improved emotional regulation.** School stakeholders in several schools said boys were better able to manage emotions in class, with fewer incidents of boys storming out of lessons or reacting aggressively. One teacher highlighted a boy with SEND who had avoided any further internal exclusions since engaging with the BDP:

“One boy started with lots of internal exclusions because he was struggling to regulate his emotions in class. Since working with Future Men, he hasn’t had a single internal exclusion due to defiance to staff, which is a real win.” Female teacher, School D

- **Get along better with teachers.** Boys and school stakeholders agreed that communication skills learned in the BDP had supported more constructive interactions with teachers. Some boys described getting into trouble less often, while school stakeholders said they had noticed boys’ language had become less combative.

“I’m getting on better with teachers than before, I’m getting into trouble less than I was before and I’m also getting fewer behaviour points and more achievement points.” Adrian, participating boy, School A

- **Relationships with peers.** Boys expressed mixed views about the impact of the BDP on their relationships with their friends. Some boys reported that the programme had helped them strengthen their friendships by improving their communication skills and giving them practical strategies to resolve any arguments. Other boys described how the BDP had encouraged them to re-evaluate aspects of their friendships or wider relationships. For instance, some boys explained that their Future Men project co-ordinator had prompted them to reflect on the influences around them, particularly behaviours or dynamics that might lead them into trouble, and to think about how they could distance themselves from these situations. This sometimes involved reducing the time spent with certain peers or avoiding particular activities or behaviours.

"[Future Men project co-ordinator] has helped with friendships and stuff; he would see me with the people that I would get into trouble with and tell me that I should avoid hanging out with them to not get into trouble." **Joseph, participating boy, School E**

- **Reduced misbehaviour and aggression in class.** Many boys reported improvements in their behaviour at school, which were reflected in reduced behavioural points and detentions. They attributed these improvements to the regular reflection and goal-setting exercises conducted with their project co-ordinator.

"Behaviour was my worst thing at school, but since we spoke about how to improve my attitude, I don't get any lates, and I do my homework, and my behaviour is also getting better." **Riley, participating boy, School H**

In addition, several school stakeholders expressed that they had seen a difference in how boys who have engaged in the BDP are communicating with staff, reporting that boys' communication was noticeably less combative and aggressive than previously, with one explaining:

"One boy, since having [project co-ordinator], he's so calm when you tell him that this behaviour wasn't correct...Before the BDP he would be swearing and angry and argumentative, but not any more, so that's great to see." **Female teacher, School B**

- **Improved educational engagement.** Boys, school stakeholders and project co-ordinators noted improved behaviour in lessons, greater classroom participation and improved attendance. Boys reported being more focused, less distracted and more interested in lessons. This improved engagement in lessons also supported two boys in moving into higher sets.

"I'm enjoying lessons more than I was before. Before, I was always getting a detention – so I used to think, 'What's the point in me even going to the lessons?' Then I had the realisation that after I stopped getting detentions, I was doing much better in lessons than I used to." **Andre, participating boy, School H**

- **Improved attendance.** Views were more mixed on whether engaging with the BDP had improved attendance. Some school stakeholders shared that a few boys had started to attend school more regularly. This often started through boys attending school on the day of their session with their Future Men project co-ordinator and then attending school more frequently the more they engaged with their sessions. A few boys also shared that they felt excited to attend their sessions, and this increased their motivation to attend school.

"One boy in Year 10, his attendance was so awful, but if he has an appointment with [project co-ordinator], he'll be in...now he's done two of his Religious Education exams and just been in for all his revision sessions and exams." **Male teacher, School A**

However, other school stakeholders stated that they had not seen an improvement in attendance for the majority of the boys in their school who had received support. These accounts suggest that the BDP may have supported individual improvements in attendance for a minority of boys (particularly where sessions provided a motivating reason to come into school) but that these changes did not appear to translate to consistent, whole-group effects. This aligns with the quantitative findings, which showed no statistically significant difference in attendance between the BDP and signposting groups. This suggests that any attendance gains may have been localised to individual cases rather than representing a wider systematic change across the cohort.

Long-term outcomes

This section explores the extent to which the people we spoke to believed that the BDP generated longer-term impacts for participating boys, particularly in relation to exclusions and the sustainability of behavioural and educational improvements.

- **Sustainability of behavioural and educational impacts.** Many school stakeholders and boys expressed that while they believed that the BDP had led to positive short- and medium-term changes, these improvements had been difficult to sustain after the 12-week programme ended. One school stakeholder shared an example of a boy who they thought had shown significant progress during his time on the programme, including improved engagement in school and moving up two sets in English, but who had since returned to being monitored on the Head of Year's behavioural list. This was echoed by one boy we spoke to, who reported making progress toward personal goals identified with his project co-ordinator while the work was ongoing but finding it challenging to maintain that progress once the support ended.
- **Reduced exclusions.** School stakeholders expressed mixed views about whether the BDP had resulted in reduced exclusions. Some school stakeholders reported reduced internal exclusions as a result of the improved behaviour described above. However, others noted that despite engagement with the programme, a small number of boys were excluded following their participation in the programme.

These findings suggest that stakeholders perceived the BDP to deliver progress across the outcomes in the theory of change, particularly in the short and medium terms. However, they believed that sustaining these changes beyond the 12-week programme was difficult without ongoing reinforcement, and views were mixed as to whether these short-term outcomes had translated into improvements in attendance and exclusions.

It should be noted that these perceptions only partially align with the quantitative findings set out in this report, which showed statistically significant improvements in school engagement and behavioural difficulties at the longer-term follow-up (T3). In addition, boys who took part in the BDP recorded fewer exclusion incidents and shorter suspension durations in the term following programme completion than those in the signposting group. Future research with a longer-term follow-up (12 months or more) would be valuable to assess whether these emerging positive trends consolidate, emerge or decay over time and translate into sustained engagement and educational progress.

4.3.3 How have experiences of support differed across subgroups e.g. those from minoritised/marginalised backgrounds, from low-income households or with SEND?

This section addresses the following research question: 3b) How have experiences of support differed across subgroups, e.g. those from minoritised/marginalised backgrounds, from low-income households or with SEND?

As described in section 4.1.1, the BDP reached a diverse group of boys, reflecting the varied contexts of the schools in which it was delivered. This section explores how experiences of support differed across subgroups, drawing on monitoring data and interviews with boys, school stakeholders and project co-ordinators.

Dosage

Table 38 shows dosage by ethnicity, FSM eligibility, SEND status and Year group for boys who received the BDP (n = 245). This shows that:

- **Ethnicity.** Overall, dosage was broadly consistent across ethnicities, with mean sessions clustering around nine to 10 sessions, and similar proportions of boys appearing in mid- and high-dosage categories. While small differences exist, such as a slightly lower mean among boys from mixed and multiple ethnic groups and marginally higher representation of boys from Black, Black British, Black Welsh, Caribbean or African backgrounds in the upper dosage bands, these variations are modest and do not indicate systematic differences in engagement or access to the programme. This suggests that the intervention was delivered relatively evenly across ethnicities.
- **FSM eligibility.** Boys who were eligible for FSM received a very similar average number of BDP sessions to their non-eligible peers, with some variation across dosage bands. FSM-eligible boys were slightly more represented in the lowest dosage category, but the difference is small and unlikely to reflect a structural equity issue. The overall distribution indicates that socioeconomic disadvantage did not notably affect the extent to which participants accessed or received the intended dosage of the intervention.
- **SEND status.** Boys with SEND received a mean dosage comparable to boys without SEND, and the distribution of sessions across dosage bands was similarly aligned. Boys with SEND appear slightly more represented in the higher dosage categories, suggesting good engagement and consistent delivery within this subgroup. Overall, this does not suggest evidence of reduced dosage for boys with additional needs.
- **School year.** Dosage was also broadly similar across school years, with mean sessions for Years 7 to 10 all falling within a narrow band of 9.5 to 10.1 sessions and comparable proportions of boys receiving mid- to high-dosage levels. Year 11 boys showed a slightly lower mean dosage (7.73 sessions) and were somewhat more represented in the lowest dosage bands. However, this group is small (n = 11), so caution is needed in interpreting this pattern. Overall, the distribution suggests that dosage was delivered consistently across year groups, with no clear evidence of systematic differences in access related to age or school stage.

Across all four characteristics, dosage patterns appear broadly equitable, with no meaningful evidence that particular groups received substantially fewer BDP sessions. This suggests that implementation was inclusive

and that common barriers to participation (e.g. competing demands, support needs or access constraints) did not translate into systematic differences in intervention receipt. For the trial, this suggests that observed effects are therefore more likely to reflect the intervention itself rather than differential delivery. Overall, the equity of delivery provides reassurance that the programme was implemented consistently across groups, supporting a cautious interpretation of the subgroup findings.

Table 38: Dosage broken down by ethnicity, free school meal (FSM) eligibility, special education needs and disabilities (SEND) status and school year (n = 245)

Characteristic	N	Number of BDP sessions received							
		Mean	0–1	2–3	4–5	6–7	8–9	10–11	12–13
Ethnicity									
Asian, Asian British or Asian Welsh	14	9.57	1 (7.1%)	0 (0.0%)	0 (0.0%)	0 (0.0%)	3 (21.4%)	7 (50.0%)	3 (21.4%)
Black, Black British, Black Welsh, Caribbean or African	138	9.75	2 (1.4%)	0 (0.0%)	2 (1.4%)	4 (2.9%)	43 (31.2%)	72 (52.2%)	15 (10.9%)
Mixed or multiple ethnic groups	29	9.14	1 (3.4%)	1 (3.4%)	0 (0.0%)	1 (3.4%)	11 (37.9%)	10 (34.5%)	5 (17.2%)
Other ethnic group	19	9.95	0 (0%)	0 (0.0%)	0 (0.0%)	0 (0.0%)	8 (42.1%)	10 (52.6%)	1 (5.3%)
White	40	9.62	0 (0%)	1 (2.5%)	3 (7.5%)	1 (2.5%)	9 (22.5%)	20 (50.0%)	6 (15.0%)
Not known	5	9.00	0 (0%)	0 (0.0%)	0 (0.0%)	1 (20.0%)	2 (40.0%)	2 (40.0%)	0 (0.0%)
FSM eligibility									
Yes	147	9.43	3 (2%)	1 (0.7%)	5 (3.4%)	6 (4.1%)	51 (34.7%)	61 (41.5%)	20 (13.6%)
No	94	9.93	1 (1.1%)	1 (1.1%)	0 (0%)	1 (1.1%)	24 (25.5%)	58 (61.7%)	9 (9.6%)
Not known	4	10.75	0 (0%)	0 (0%)	0 (0%)	0 (0%)	1 (25%)	2 (50%)	1 (25%)
SEND status									
Yes	82	9.70	2 (2.4%)	2 (2.4%)	1 (1.2%)	3 (3.7%)	22 (26.8%)	40 (48.8%)	12 (14.6%)
No	161	9.57	2 (1.2%)	0 (0%)	3 (1.9%)	4 (2.5%)	54 (33.5%)	80 (49.7%)	18 (11.2%)
School year									
Year 7	61	10.10	0 (0%)	0 (0%)	0 (0%)	1 (1.6%)	17 (27.9%)	33 (54.1%)	10 (16.4%)
Year 8	57	9.74	1 (1.8%)	0 (0%)	2 (3.5%)	1 (1.8%)	16 (28.1%)	31 (54.4%)	6 (10.5%)
Year 9	63	9.49	1 (1.6%)	1 (1.6%)	1 (1.6%)	4 (6.3%)	19 (30.2%)	28 (44.4%)	9 (14.3%)
Year 10	52	9.58	1 (1.9%)	0 (0%)	2 (3.8%)	1 (1.9%)	18 (34%)	26 (49.1%)	5 (9.4%)
Year 11	11	7.73	1 (9.1%)	1 (9.1%)	0 (0%)	0 (0%)	6 (54.5%)	3 (27.3%)	0 (0%)

Experiences of support

Findings from semi-structured interviews with boys, project co-ordinators and school stakeholders highlighted the following differences in experiences of support, which help to contextualise and enrich the quantitative findings:

- **Boys from minority ethnic backgrounds.** Project co-ordinators and school stakeholders said boys from minority ethnic backgrounds engaged particularly well with the BDP. Boys from minority ethnic backgrounds frequently characterised their relationships with their project co-ordinators as trusting, non-judgemental and easy to sustain over time. Future Men staff emphasised that project co-ordinators' cultural competence, drawing on both professional skill and lived experience, enabled them to relate meaningfully to boys' contexts. They said this relatability created a sense of cultural relevance, which helped boys feel understood and respected and supported the development of trusted relationships and open dialogue. This perceived strong engagement aligns with the quantitative dosage analysis, which indicated broadly equitable delivery and no evidence of reduced session attendance for boys from any minority ethnic group.
- **Boys from low-income households.** School stakeholders highlighted that many participating schools served communities with high proportions of low-income households. They explained that boys in these contexts often faced additional pressures, such as parents working multiple jobs with limited time for one-to-one support. Project co-ordinators and school stakeholders agreed that this made boys especially receptive to the opportunity to build trusted adult relationships through the BDP.

In addition, school stakeholders and project co-ordinators explained how engagement with parents and carers had also supported engagement with boys from low-income households. For example, one project co-ordinator described working with a family that was unaware of their child's entitlement to FSM, stating that the boy had been going without lunch each day. Once the project co-ordinator contacted the parents and connected them with available support, the boy's behaviour and wellbeing improved substantially. These insights provide a useful explanation for the statistically significant improvements seen among FSM boys in the quantitative subgroup analysis. As dosage was similar across FSM groups, qualitative evidence suggests that the stronger outcomes for FSM boys may reflect enhanced benefits or a higher perceived value of the support, rather than differences in exposure.

- **Boys with SEND.** Experiences for boys with SEND were more mixed. Some school stakeholders said the standard pace and structure of the programme could be challenging for boys with SEND, limiting sustained engagement. Others shared positive examples of progress, including one boy who significantly reduced his internal exclusions after receiving BDP support. Project co-ordinators themselves noted that delivering the BDP to boys with SEND could be difficult without specific training and suggested that additional guidance or resources would be valuable if delivery were expanded. This suggests that while SEND boys generally accessed the same level of support, some may require tailored adaptations to maximise the programme's relevance and accessibility.

Overall, triangulation of qualitative and quantitative evidence suggests that the BDP was experienced as inclusive and engaging across key demographic groups, with particular strengths in cultural competence and support for boys facing socioeconomic disadvantage. At the same time, the mixed experiences among boys

with SEND highlight opportunities for targeted enhancements that could further strengthen equitable delivery and ensure that all pupils are able to benefit fully from the intervention.

4.4 Lessons from the Boys' Development Programme

4.4.1 Potential adaptations to the programme

This section addresses the following research question: 1g) Adaptation. Are any changes needed to accommodate context and need?

Interviews with boys, school stakeholders and Future Men project co-ordinators identified several adaptations that could potentially strengthen the BDP's alignment with school contexts and boys' needs. These suggestions build on the strong foundations of the current model, which was largely delivered with fidelity and valued by participants:

- **Consider extending the intensity and duration of support.** Project co-ordinators, boys and school stakeholders suggested that a longer programme, or follow-up booster sessions, could help sustain engagement and embed skills more effectively. The Future Men team may wish to consider extending the timeframe for higher-need boys or piloting reinforcement mechanisms after the core programme ends.
- **Updating content to address topical themes.** A few project co-ordinators said they felt less confident addressing contemporary issues, such as online influencers (e.g. Andrew Tate and the "manosphere") and LGBTQIA+ identities. Future Men may wish to strengthen training and update resources to equip staff to approach these topics confidently and sensitively, using reflective practice to test how best to frame discussions.
- **Enhance parent/carer engagement.** Several school stakeholders and project co-ordinators recommended stronger communication with families. They suggested that regular updates or joint workshops could reinforce programme messages at home and promote consistency. Future Men may wish to ensure expectations around family engagement are set clearly with schools at the outset.
- **Increase teacher engagement.** Some school stakeholders recommended more structured communication between project co-ordinators and teaching staff, for example, by linking sessions with existing PSHE provision or feeding back progress updates. This could help embed the BDP messages within wider school systems.
- **Improve accessibility for boys with EAL and SEND.** Most project co-ordinators said they would welcome further training and resources to engage boys with EAL or SEND. School stakeholders agreed that collaboration with SEND staff or teaching assistants could improve inclusivity, provided schools have the capacity to support this.
- **Consider introducing a whole-school component.** Some school stakeholders suggested that assemblies or workshops for wider cohorts could extend the BDP's influence beyond participating boys. This could reinforce messages at a cultural level, creating consistency between programme content and wider school messaging.

However, it is also clear that most of these adaptations would have cost implications. Some changes, such as minor adjustments to session sequencing or materials, could likely be achieved at low or no additional

cost, while others, such as extending the duration of support, providing structured follow-up sessions or increasing staffing capacity, would require moderate to high levels of additional funding.

This distinction is important: As with many programmes evaluated through RCTs, the BDP was delivered within tight resource constraints. The evaluation evidence therefore highlights not only what works but also what might work better if appropriately resourced. Ensuring that promising interventions such as the BDP have the potential for sustained impact may depend on support through funding, commissioning and investment models that recognise the true cost of delivering high-quality, relationship-based work with young people. The YEF, EEF and other funders are well placed to help shape this conversation about what level of investment is required for these approaches to reach their full potential.

4.4.2 Implications for future replication and scale of the Boys' Development Programme

This section addresses the following research question: 4) What are the implications for future replication, scale and spread?

The impact analysis suggests that the BDP works as intended, generating improved engagement with school, reduced behavioural difficulties and reduced frequency and duration of exclusions at 12 weeks post-completion. The IPE also suggests that the BDP is feasible to deliver in secondary schools and is highly valued by boys, school stakeholders and project co-ordinators. Any future replication should preserve the “secret sauce” that enabled effective implementation and outcomes, while also paying attention to areas for improvement identified during the trial.

The IPE has highlighted several features of the BDP that participants consistently described as its core strengths. These elements form the “secret sauce” of the programme and will be critical to protect in any future scale-up:

- **Adherence to the BDP programme manual and slides.** The BDP is a manualised intervention, with structured guidance and session plans that define its theoretical foundations, session sequence and learning objectives. The manual provides a consistent framework that ensures all project co-ordinators deliver the same high-quality, evidence-informed content while allowing flexibility to tailor sessions to individual boys' needs. Maintaining fidelity to this manual is critical to preserving the programme's integrity, implementing its theory of change and supporting reliable replication at scale.
- **Relatable, skilled project co-ordinators.** Boys consistently said they engaged because project co-ordinators were relatable, trustworthy and skilled at balancing informality with professionalism. Preserving this workforce profile, i.e. culturally competent, empathetic and skilled members of the team, enabled by a supportive management culture and reflective sessions, will be key to support delivery of the model to high quality.
- **Strong referral and screening processes.** School stakeholders valued the clear recruitment guidance and case-by-case triage meetings. These ensured the right boys were recruited and should remain central to any future rollout.
- **Focus on inclusivity and accessibility.** The BDP successfully reached boys from minority ethnic backgrounds, from low-income households and with SEND. This intentional focus on equity should remain at the heart of any future scaling.

Several practical lessons also emerged about what will be needed for replication at scale. These lessons highlight the organisational and school-level conditions that enabled success, as well as the challenges that will need to be addressed:

- **Continuity planning.** Staff turnover in a few schools disrupted delivery. Future replication should include clear continuity plans, handover protocols and staffing buffers to protect relationships between boys and their project co-ordinators wherever possible.
- **School integration.** Delivery was strongest where project co-ordinators were embedded in school systems, had access to data and were supported by a senior school contact. Replication agreements should make these conditions explicit in service level agreements between Future Men and schools.
- **Organisational capacity.** Scaling for the RCT tested Future Men's capacity; the organisation successfully doubled its workforce and maintained reflective practice. Replication will require sustainable workforce planning, ongoing training and space for reflective practice.
- **Durability of outcomes.** Boys and school stakeholders said 12 weeks often felt too short and that they would have benefitted from longer support. Future replication may need to test booster or extended models to embed skills and build on progress.

Bringing these strands together, **the evaluation shows that it is feasible to deliver the BDP in schools as intended.** To build on this implementation success, future delivery may benefit from further strengthening the core ingredients of the model, as well as introducing some adaptations. The conclusion will return to these themes, drawing on both the IPE and impact evaluation to reflect on recommendations for the future.

5 Cost information

5.1 Overview

This section presents an analysis of the costs of delivering the BDP. Our approach is in line with our efficacy study protocol and [YEF cost reporting guidance](#).

As outlined in our protocol, the analysis is based on Future Men's budget for the BDP and discussions with key Future Men stakeholders. Our approach:

- **Estimates the costs of delivery only.** Future Men have been delivering the BDP in line with the programme budget, and, therefore, the budget is representative of the costs of delivery only.
- **Derives estimates using the bottom-up principle.** Future Men reported that the BDP budget was drawn up via a bottom-up approach. This means that an approach using the BDP budget to capture the cost of delivery aligns with the bottom-up principle.
- **Estimates the cost of resources used in delivering the intervention and does not capture how costs changed relative to BAU.** The cost estimate is representative of the resources used in delivery of the BDP and does not compare costs to BAU.

As set out in the efficacy study protocol and in line with [YEF analysis guidance](#) and [YEF cost reporting guidance](#), we have used the following approaches to report cost information:

- All costs relating to both the evaluation and programme development/adaptation are excluded from cost estimates.
- All costs have been presented as constant prices for the year 2025–26 to aid interpretation. We have not discounted cost estimates based on time preferences.
- There are no durable inputs with benefits to those outside the project.
- In line with [YEF cost reporting guidance](#), all cost estimates have been generated assuming full compliance (i.e. that all participants received the full BDP dosage of 12 weekly sessions of one-to-one support). We have provided estimates on the basis of project co-ordinators operating at full capacity, i.e. assuming that project co-ordinators delivered to an average caseload of 20 boys per term across four working days per week.
- We identified estimates as set-up and recurring costs. We also set out what needs to be in place concerning pre-requisite costs – in line with [YEF cost reporting guidance](#), we have not monetised these.
- We presented two scenarios for the BDP programme: one in which support is delivered to five boys across one term and one in which support is delivered to 60 boys across a full academic year. This demonstrates the differences made by economies of scale: The first scenario reflects the costs associated with small-scale delivery and initial roll-out, while the second reflects a realistic steady-state model in which fixed costs are spread over a larger cohort, reducing the unit cost. Commissioners should interpret these as early versus mature implementation costs.

The following sets out pre-requisite, set-up and recurring costs for the BDP. This is followed by a presentation of total and average cost estimates per participant for the BDP programme.

5.2 Pre-requisite costs

In line with [YEF cost reporting guidance](#), the following lists what is expected to be in place before the Future Men BDP programme is implemented. [YEF cost reporting guidance](#) suggests that these are things that can be assumed would not need to be paid for by a funder/commissioner; as such, we have not provided costs for them. The list of pre-requisite costs includes:

Provider pre-requisite costs

It is anticipated that for an intervention like the BDP to be implemented, it would require a public, private or Voluntary Community Sector Enterprise (VCSE) organisation like Future Men to deliver it. This organisation would need to have the following in place which would incur pre-requisite costs:

- **Premises and office.** Associated costs could include rates, maintenance, utilities and depreciation.
- **Strategy and governance.** Associated costs could include audit, board expenses, membership subscriptions, consultancy, travel and subsistence.
- **Organisational support.** Associated costs could include human resources support, bank charges, payroll processing, marketing, staff recruitment, insurance and staff meetings.
- **Indirect staffing.** Associated costs could include strategy, finance, information technology, communications and business development.

It is also anticipated that the provider would have staff in place with the appetite and ability to lead the implementation of the BDP. As a result, the costs of senior leadership teams have not been included in the below estimates.

School-based pre-requisite costs

Delivery of the BDP requires several pre-requisite resources within schools. These are not direct costs of the intervention, but are a necessary pre-requisite for its implementation:

- **School facilities.** Schools must provide suitable facilities. The BDP is delivered during school hours in existing school buildings, so the availability of private rooms or designated spaces for one-to-one sessions is an essential pre-condition. These spaces must be quiet, accessible and suitable for confidential discussions. However, as schools already have these facilities, we do not assign a separate cost to physical space use.
- **Staff time.** Some school staff time is required to co-ordinate participation. Typically, a senior member of staff, such as a pastoral lead, SENCo or deputy headteacher, identifies eligible pupils and liaises with the Future Men team. Staff also arrange parental consent and room bookings and support the scheduling of sessions to minimise disruption to teaching. This represents a small but important time contribution, typically around half an hour per participant over the course of the intervention. As teacher time is already available and co-ordinating external interventions is a core part of this role, we do not assign a separate cost for teacher time.

- **School environment.** Finally, schools are responsible for providing a safe and appropriate environment for programme delivery. This includes ensuring safeguarding processes are in place and that the intervention aligns with school policies on pupil behaviour and attendance.

These represent the minimum pre-requisite conditions for the BDP to be implemented effectively in a school setting. They are therefore included qualitatively in the cost analysis but not costed separately, as they reflect existing school infrastructure and routine staff responsibilities rather than new expenditure.

5.3 Set-up and recurring costs

Table 39 provides a description of the set-up and recurring costs for delivering the BDP. Set-up costs occur at the beginning of the intervention, while recurring costs are ongoing.

Table 39: Description of items included in cost estimates

Category	Set-up or recurring	Description
Staff		
Staff recruitment costs	Set-up	The cost of recruiting a new member of staff, assumed to be £400 per person
DBS checks	Set-up	The cost of an enhanced DBS check as a requirement to work with young people, assumed to be £65 per member of staff
Staff training	Recurring	The cost of necessary training for staff (e.g. data protection, safeguarding and training on delivery), including external and bespoke training, assumed to cost £1,000 per member of staff and to recur once per year
Delivery staff	Recurring	- Cost associated with delivery time: Project co-ordinators deliver the programme; as such, this includes their salary, employer National Insurance contributions and pension costs and is assumed to total £42,779.00 per year at 1.0 FTE. - Each project co-ordinator can deliver five one-to-one sessions per day for four days per week. This means a full caseload is the equivalent of 20 boys per project co-ordinator per term.
Line management and oversight	Recurring	- The cost associated with line management and delivery oversight time from the heads of programmes. At 1.0 FTE, this is assumed to cost £59,811.00 to cover salary, National Insurance contributions and pension. - It is assumed that 0.2 FTE is required per full-time project co-ordinator, so this time has been prorated accordingly in the estimates below.
Programme		
Travel	Recurring	Travel costs for project co-ordinators to attend schools, assumed to be £40 per month of delivery.
Materials and equipment		

Category	Set-up or recurring	Description
IT hardware	Set-up	Staff laptops, tablets and mobile phones to support delivery and liaison with school colleagues, assumed to be a £950 laptop plus a £180 mobile phone.
Mobile phones	Recurring	Staff phones and internet access in order to liaise with school colleagues, assumed to be £21 per month per member of staff.
IT software	Recurring	Customer relationship management software, assumed to be £12 per month per member of staff.
Incentives		
None	N/A	N/A
Buildings and facilities		
None	N/A	N/A

Note: DBS = Disclosure and Barring Service; IT = information technology; FTE = full-time equivalent;

5.4 Detailed breakdown of cost estimates

Table 40 presents the estimated real cost of delivering the BDP in 2025–26 prices based on two delivery scenarios:

1. **Scenario 1** models a small-scale delivery to five young people across one school term (equivalent to 0.2 full-time equivalent [FTE] of staff time across four months).
2. **Scenario 2** models a full-scale delivery to 60 young people across an academic year (equivalent to 0.8 FTE staff time³⁰ across 12 months).

The estimates below suggest that the per-boy cost is £1,284 for Scenario 1 and £708 for Scenario 2. This is calculated as follows:

- **Total estimated cost** of delivery is £6,418 for Scenario 1 and £42,479 for Scenario 2. Most costs are recurring rather than one-off, accounting for around 75% of total expenditure in Scenario 1 and over 95% in Scenario 2. The main driver of costs across both scenarios is staff time, including delivery, line management and training.
- **Total set-up costs** are fixed at £1,595 across both scenarios, reflecting one-off expenditure on recruitment, DBS checks and information technology (IT) hardware required to deliver the intervention. Because these costs do not vary by cohort size, they represent a larger proportion of total expenditure when delivery is small-scale (25% of total cost in Scenario 1, compared to just 4% in Scenario 2).
- **Total recurring costs** increase with programme scale, from £4,823 for five participants to £40,884 for 60 participants. This growth is driven primarily by delivery staff salaries (£2,753 in Scenario 1 vs £34,223 in Scenario 2) and associated line management (£778 vs £4,785). Other recurring costs, such

³⁰ In line with a bottom up approach, full time delivery is costed as 0.8 FTE. This is because one day a week is reserved for training, supervision and other organisational activities within the Future Men team that are not associated with delivering the BDP.

as travel and communication, also increase with scale but remain relatively small in proportion to overall expenditure.

- Comparing the two scenarios highlights the **economies of scale** inherent in the BDP delivery model, with the average cost per participant declining from £1,284 per boy in Scenario 1 to £708 per boy in Scenario 2. This reduction is largely due to the fixed set-up costs being distributed across a larger cohort of participants.

Table 40: Estimated real cost of delivering the Boys' Development Programme, presented in 2025–26 prices.

Category	Set-up or recurring	Scenario 1 <i>Delivery to five boys across one term</i>	Scenario 2 <i>Delivery to 60 boys across three terms</i>
Staff			
Staff recruitment costs	Set-up	£400	£400
DBS checks	Set-up	£65	£65
Staff training	Recurring	£1,000	£1,000
Delivery staff costs (salary, pension, NI)	Recurring	£2,753	£34,223
Line management costs (salary, pension, NI)	Recurring	£778	£4,785
Programme			
Travel	Recurring	£160	£480
Materials and equipment			
IT hardware	Set-up	£1,130	£1,130
Mobile phone charges	Recurring	£84	£252
IT software	Recurring	£48	£144
Incentives			
None	N/A	£0	£0
Buildings and facilities			
None	N/A	£0	£0
Total costs			
Total set-up costs		£1,595	£1,595
Total recurring costs		£4,823	£40,884
Total costs		£6,418	£42,479
Cost per boy			
Set-up cost per boy		£319	£27
Recurring cost per boy		£965	£681
Total costs per boy		£1,284	£708

Note: DBS = Disclosure and barring service; IT = information technology; NI = National Insurance

5.5 Cost composition and implications

Across both delivery models, staffing remains the dominant cost component, accounting for approximately 70% to 80% of total programme expenditure. This reflects the one-to-one nature of the BDP and the

importance of experienced project co-ordinators and line management oversight in maintaining fidelity to the model. Material, IT and travel costs together represent a small proportion of overall costs.

The analysis highlights substantial economies of scale. While total expenditure increases with programme size, the average cost per participant falls from £1,284 at small scale to £708 at full capacity, as fixed set-up and management costs are spread across a larger cohort. This demonstrates that the BDP becomes considerably more cost-efficient when project co-ordinators operate at or near full caseloads (approximately 0.8 FTE supporting 60 boys per year).

Importantly, these figures represent a bottom-up estimate of real delivery costs, rather than a commissioning price. They exclude organisational overheads, accommodation, central administration and surplus, which are typically included in a contract value. Any future commissioning price would therefore be higher than these direct delivery estimates, typically by at least 20%.

When benchmarked against comparable interventions in the YEF Toolkit, the BDP sits comfortably within the expected cost range for targeted one-to-one SEL support. According to the YEF Toolkit on mentoring, delivery costs for mentoring and one-to-one support interventions typically range from around £800 to £3,500 per participant, depending on intensity, staffing model and delivery setting (YEF, 2024). Programmes at the lower end of this range are often light-touch or volunteer-led, while those at the higher end involve intensive, structured one-to-one support delivered by trained professionals. The BDP's estimated cost of £708 per participant at full capacity, therefore, places it toward the lower end of the YEF's cost spectrum for structured, professionally delivered mentoring programmes. This indicates that, for its intensity and level of project co-ordinator involvement, the BDP represents good value for money compared to similar interventions targeting boys at risk of exclusion or disengagement from school.

Overall, the cost analysis indicates that the BDP delivers good value for an intensive one-to-one social-emotional support model, with scope for efficiencies through scaling. However, a full cost-effectiveness assessment was beyond the scope of this study and should be explored through future research.

6 Conclusion

6.1 Key conclusions

Key conclusions
The BDP had a small positive impact on boys' school engagement immediately after the programme. Boys who received the BDP had slightly higher levels of school engagement than their counterparts in the control group. This result has a very high security rating .
The BDP showed a moderate positive impact on reducing behavioural difficulties, and a moderate positive impact on reducing emotional problems. It had a small positive impact on improving teacher bonding and a moderate negative impact on peer difficulties, meaning peer difficulties increased. These are secondary outcomes and should be treated with more caution.
As an exploratory analysis, the evaluation also measured the impact of the programme 12 weeks after the end of the programme. At that time point, the BDP had a moderate positive impact on school engagement, suggesting that improvements in school engagement may have strengthened after the intervention ended.
The BDP reached its intended cohort. The BDP successfully engaged boys from minority ethnic and low-income backgrounds, supported by clear referral and screening processes, relatable practitioners and engaging programme content. A few challenges emerged around referrals for boys whose needs were not clearly suited to BDP, accessibility for boys with language or learning needs, and the ability to reach boys involved with children's social care.
Exploratory analyses suggest that the quality of the relationship between boys and their BDP practitioners (known as project co-ordinators) helped explain later improvements in school engagement. This was supported by qualitative findings, in which boys described project co-ordinators as approachable, trustworthy and easy to talk to.

Note: BDP = Boys' Development Programme

6.2 Evidence to support the BDP's theory of change

The BDP's theory of change was co-developed by Future Men, Cordis Bright and the YEF during the study's set-up phase and remained unchanged throughout the efficacy study. It articulates clear pathways linking activities, mechanisms and outcomes.

Evaluation findings provide strong support for key elements of the theory of change, particularly trusted relationships and the development of social and emotional strategies to improve communication, conflict management and emotional regulation. Quantitative and qualitative evidence suggests these mechanisms contributed to measurable improvements in school engagement and behaviour over time. Other elements, such as emotional regulation and peer relationships, were only partially supported within the 24-week follow-up period. Longer-term outcomes, such as reduced exclusions, are supported by exploratory analyses of administrative data, whereas improvements in attendance were not observed within the evaluation timeframe.

Areas with the strongest support

- **Evidence base and fidelity to intended delivery.** The BDP is grounded in an established evidence base on SEL programmes (e.g. Durlak et al., 2011; Taylor et al., 2017; Valdebenito et al., 2019). It targets empirically supported risk factors, such as poor emotional regulation and weak school belonging, and seeks to build protective factors, including pro-social skills, positive identity formation, notions of masculinity, communication skills and trust in adults. The IPE found high fidelity to the intended model: Most boys received at least eight sessions covering the planned topics. This fidelity under efficacy study conditions strengthens confidence that the intended mechanisms operated as theorised.

- **Trusted relationships as the key mechanism.** Both the impact evaluation and the IPE provide consistent evidence that high-quality relationships between boys and project co-ordinators were central to delivery. SSRS scores were higher for the BDP delivery group and statistically significantly associated with later school engagement, and the results from exploratory mediation analyses were consistent with a relational mechanism of change. Qualitative findings reinforced this pathway: Boys consistently described project co-ordinators as approachable, trustworthy and easy to talk to, and stakeholders emphasised the importance of a non-judgemental, culturally competent, relational approach in helping boys feel respected and understood. Trust was built through consistent presence, empathy, advocacy and skilful communication, suggesting that relational practice was a core active ingredient of the model.
- **Improvements in overall school engagement.** The effects on school engagement (SCQ total score) at T2 were small and not statistically significant (Hedges' $g = 0.04$, $p = 0.639$). The evaluation identified a statistically significant positive effect on school engagement (Hedges' $g = 0.21$, $p = 0.027$) at T3. Further evidence from the CACE analysis demonstrated that boys who attended the majority of sessions experienced greater gains in school engagement, with a large, statistically significant effect size (Hedges' $g = 0.47$, $p = 0.046$). Qualitative insights on perceived outcomes mirror the quantitative trends and indicate that the relational and behavioural changes promoted by the BDP translated into stronger school engagement over time. The delayed effect on school engagement may suggest that change takes time to consolidate, particularly for underlying attitudes and routines that develop gradually after support ends. It is also possible that measurement timing played a role: Several scales use "last month" recall periods, meaning that T2 responses were captured while boys were midway through the intervention. By T3, questionnaires reflected boys' experiences in the weeks after completing the programme, when changes may have been more noticeable or more accurately recalled.
- **Improvements in behavioural difficulties and reduced exclusions.** Exploratory analyses found that the BDP was associated with reduced behavioural difficulties (Hedges' $g = -0.22$, $p = 0.020$) at 24 weeks post-randomisation. Exploratory analysis of administrative data from five of the eight schools pointed in a consistent direction: Boys in the BDP delivery group recorded fewer exclusion incidents (Hedges' $g = -0.24$, $p = 0.024$, IRR = 0.65) and shorter suspension durations (Hedges' $g = -0.50$, $p = 0.004$) in the two half-terms following programme completion. This pattern was reinforced by qualitative evidence from staff, who described noticeable changes in boys' conduct, including fewer incidents of conflict, detentions and classroom removals. Many boys we spoke to reported improvements to their behaviour and reductions in negative behaviour points, suggesting that the programme helped them translate emotional learning into behavioural change.
- **Equity and reach to priority groups.** The BDP successfully reached its intended target group: Boys at risk of exclusion, the majority of whom were from minority ethnic and socioeconomically disadvantaged households. Exploratory subgroup analyses indicated a large positive and statistically significant effect on school engagement at T3 for boys eligible for FSM ($g = 0.27$, $p = 0.030$). However, this finding should be interpreted with caution given the relatively small subgroup sample and the absence of a statistically significant interaction effect, meaning there is limited evidence to conclude that the BDP was differentially more effective for FSM-eligible boys. Effects by ethnicity were positive but not statistically significant, suggesting that while the programme was culturally responsive and valued across groups, there was no evidence of differential effects. Exploratory moderation by

baseline risk suggested larger point estimates for boys with ≥ 1 exclusion at baseline at both T2 ($g = 0.36, p = 0.085$) and T3 ($g = 0.38, p = 0.086$), compared with smaller estimates for boys with no baseline exclusions. While non-significant, this pattern is consistent with the possibility of greater benefit for boys at higher baseline risk. The IPE found no evidence of differential dosage by ethnicity, FSM status, SEND status or Year group, and boys from minority ethnic backgrounds viewed the programme as inclusive and relatable.

Areas partially supported

- **Emotional symptoms.** The IPE found substantial qualitative evidence that boys developed stronger emotional awareness and communication skills, such as *“thinking before reacting”* and *“stopping to calm down.”* However, quantitative measures (using the SDQ emotional symptoms subscale) did not detect statistically significant improvements within the trial period, with effect sizes smaller than the trial was powered to reliably detect. This is also reflected in the broader literature, where meta-analyses suggest that SEL programmes have small to moderate effect sizes on emotional symptoms (Durlak et al., 2011; Mahoney et al., 2021; Taylor et al., 2017).
- **Improved peer relationships.** Evidence on improved peer relationships was mixed. Quantitative analyses using the SDQ peer difficulties subscale found small, non-significant effects in a non-favourable direction at T2 and T3. However, qualitative evidence from boys, project co-ordinators and school staff suggested that boys made early progress in how they managed conflict and interacted with others. Exploratory analyses of pro-social behaviours also showed a moderate statistically significant change at T3, favourable to the BDP. These findings suggest that the BDP supported early interpersonal and pro-social change consistent with its theory of change, although these were not consistently evident across quantitative measures within the 12- or 24-week post-randomisation follow-up period.
- **Improved teacher relationships.** The BDP sought to improve pupils’ relationships with teachers by encouraging better communication, respect and trust. Quantitative effects on the SCQ teacher bonding and attachment subscale were small, favourable and non-significant, while qualitative data indicate that communication between boys and teachers became less combative and more reflective.

Areas requiring further evidence or refinement

- **Attendance outcomes.** Exploratory quantitative analyses on a subset of boys found a small, non-statistically significant, negative effect on attendance, with boys in both groups maintaining similar attendance rates across the trial period. These findings suggest that any improvements in school engagement captured through self-report measures did not translate into detectable changes in attendance over this timeframe for the subset of boys with available data. Qualitative data suggest that the BDP may have improved motivation for individual boys to attend school on session days, but these changes did not translate into a measurable overall effect. Future research should aim for standardised, school-verified attendance over longer periods.
- **School-level influences.** The IPE found that programme delivery was shaped by school environments, including the level of embeddedness within the wider school culture, the level and capacity of the main point of contact, and physical capacity to host the programme. These contextual conditions should be included explicitly in future versions of the theory of change as conditions for success and moderators of implementation quality.

Considerations for future refinement of the theory of change

The evaluation did not identify any elements of the theory of change that were invalid, but several refinements could strengthen its clarity and applicability for future trials and scale-up.

- **Temporal sequencing of change.** Future iterations of the programme's theory of change could consider incorporating clearer timeframes for when different outcomes are likely to emerge, drawing on the temporal pattern observed in this study. For example, short-term outcomes may be most realistically expected immediately post-programme, with medium-term changes emerging around three to six months after completion, and longer-term outcomes requiring sustained follow-up. These timeframes should be treated as indicative rather than prescriptive: Developmental trajectories vary across individuals, effects may unfold non-linearly and heterogeneity in boys' needs, readiness and dosage means that change may occur at different rates for different young people.
- **Pro-social behaviours.** The exploratory analysis suggested moderate, statistically significant gains on the SDQ pro-social subscale at T3, supported by qualitative data, which suggested that boys were becoming more co-operative and empathetic over time. Future iterations should consider embedding improved pro-social behaviours as a secondary outcome and testing it in future research.
- **Longer-term outcomes.** Extending follow-up to 12 months or beyond would allow testing whether short-term improvements in engagement and behaviour translate into sustained reductions in exclusions and potential future gains in attendance. This could be achieved through YEF archive analysis.
- **Fidelity and workforce development.** The evaluation found strong fidelity under efficacy study conditions with a small, experienced team. Future scale-up will require embedding ongoing supervision, reflective practice and structured training to maintain fidelity at a larger scale while preserving the relational quality that underpins impact. Future iterations of the theory of change may wish to make these organisational factors and assumptions explicit.

Overall, the evaluation provides strong support for the core assumptions underpinning the BDP's theory of change while also highlighting areas where refinements would strengthen its precision and usefulness for future delivery and evaluation. The central mechanisms, particularly trusted relationships and the development of social-emotional strategies, were clearly activated and linked to meaningful improvements in boys' engagement with school, behavioural difficulties and exclusions. At the same time, mixed findings for emotional symptoms, peer relationships and attendance underscore the value of specifying realistic timeframes for change and recognising contextual moderators. Refining the theory of change in light of these findings will help Future Men articulate a more nuanced, temporally grounded pathway and support the design of future trials capable of testing longer-term outcomes.

6.3 Interpretation

6.3.1 Answering the key research questions

The aim of the efficacy study was to address the following key research question:

Does a targeted, social–emotional learning programme for boys at risk of disengagement and exclusion improve school engagement in comparison to business as usual?

The impact evaluation found that allocation to the BDP led to statistically significant improvements in school engagement at 24 weeks post-randomisation (Hedges' $g = 0.21$, $p = 0.027$). No statistically significant difference was observed at the primary endpoint (T2), but by T3 a moderate effect had emerged, suggesting that gains consolidated after delivery or possibly reflecting questionnaire scale recall periods. Consistent with this pattern, the CACE analysis indicated a substantially larger effect at T3 for boys who attended at least 11 sessions (Hedges' $g = 0.47$, $p = 0.046$).

Exploratory administrative data analyses showed complementary patterns: While there was no measurable change in attendance, boys in the BDP delivery group experienced statistically significant reductions in both the number and duration of school exclusions in the term following completion (Hedges' $g = -0.24$, $p = 0.024$, IRR = 0.65 for incidents and Hedges' $g = -0.50$, $p = 0.004$ for suspension duration). These results provide indicative evidence that the BDP's relational and behavioural mechanisms translated into fewer disciplinary incidents, even if overall attendance rates remained unchanged. Because administrative data were available for a subset of schools (five out of eight participating schools), these findings should be regarded as indicative.

These findings indicate that the BDP improved school engagement among boys who participated fully, with effects becoming evident three months after delivery.

In addition to the primary research question, the study addressed a number of related research questions, which are discussed in Table 41.

Table 41: Key research questions and summary answers

Research question	Summary
Delivery. Can the BDP work under ideal circumstances?	Findings from the IPE indicate that the BDP was delivered largely as intended and can operate effectively under ideal circumstances. High fidelity was achieved across the eight participating schools: Almost all boys received eight or more weekly one-to-one sessions covering the core BDP topics (emotions, communication, conflict, goals, aspirations and masculinity). Project co-ordinators consistently used the BDP manual and adhered to the structured session format. Relationships between boys and project co-ordinators were consistently reported as positive, trusted and transformative and identified by both participants and school staff as the central strength of delivery. However, contextual variation was observed: Timetabling challenges, competing school commitments and limited capacity occasionally constrained flexibility, and some boys missed sessions due to competing school priorities or absence from

Research question	Summary
	school. Overall, the evidence suggests that the BDP can be implemented with fidelity under ideal conditions and is feasible for delivery within school settings, provided school-level conditions for success, such as sufficient leadership support, operational capacity and embeddedness within the school environment, are in place.
Impact. a) What is the impact of the BDP? b) Do different subgroups of young people have different outcomes, e.g. those from minoritised/marginalised groups?	The efficacy study found that the BDP had a small positive non statistically significant impact on boys' school engagement immediately after the programme. Boys who received the BDP had slightly higher levels of school engagement than their counterparts in the control group. Effects across the pre-specified secondary outcomes were mixed, with no statistically significant differences in behavioural difficulties, emotional problems, peer difficulties and teacher bonding at this timepoint. Pre-specified exploratory analyses provided additional supportive evidence at 24 weeks post-randomisation. The BDP was associated with a statistically significant improvement in overall school engagement (Hedges' $g = 0.21$, $p = 0.027$) and a statistically significant reduction in behavioural difficulties (Hedges' $g = -0.22$, $p = 0.020$), suggesting that effects may have strengthened following programme completion. Exploratory analysis using administrative data from five of the eight schools also showed statistically significant reductions in both the number of exclusion incidents and the duration of suspensions (Hedges' $g = -0.24$, $p = 0.024$; IRR = 0.65, and Hedges' $g = -0.50$, $p = 0.004$, respectively), although there was no statistically significant difference observed for attendance. Exploratory CACE analysis showed a larger, statistically significant effect on school engagement at T3 among boys who attended at least 11 sessions (Hedges' $g = 0.47$, $p = 0.046$). Exploratory subgroup analyses found no evidence that the programme's effectiveness differed systematically between groups, although a statistically significant positive effect was observed for boys eligible for FSM. Qualitative findings indicate that the BDP was viewed as culturally responsive and relevant across groups, enhancing trust and engagement. Together, the findings suggest that the BDP has the potential to produce relational, behavioural and school-engagement improvements, but the positive findings at 24 weeks and from the CACE, administrative-data and subgroup analyses should be interpreted as exploratory
Unintended consequences. a) Does the BDP have any unintentional consequences? If so,	There was no evidence of major unintended consequences of the BDP reported by boys, project co-ordinators or school staff. Boys described their involvement positively, highlighting the one-to-one relationship as supportive, safe and enjoyable. A small number expressed disappointment when sessions ended, and some wished the programme were longer. No adverse

Research question	Summary
what are these? b) Do different groups of young people experience these differently?	experiences were reported, and no differences were observed between demographic groups in how boys experienced the intervention. Overall, the evidence suggests that the BDP was acceptable and valued by participants and perceived as distinct from disciplinary or punitive interventions in school.
Iatrogenic effects. Are there any serious negative effects attributed to the BDP on any outcomes?	The evaluation found no evidence of serious negative or iatrogenic effects attributable to the BDP. No safeguarding concerns or harms were reported. On measured outcomes, all statistically significant effects were positive and favourable for the BDP delivery group. For non-significant outcomes (emotional symptoms and peer and teacher relationships), effect sizes were small and statistically uncertain. These findings indicate that the BDP did not cause harm and that participation was associated with neutral to positive impacts across all measured domains.
Mechanisms. Which factors contribute most to the observed outcomes?	The evidence suggests that the BDP's primary mechanism of change operates through the formation of consistent, trusting relationships between boys and project co-ordinators. Quantitative mediation analysis confirmed that the quality of the mentoring relationship (SSRS mentoring subscale) predicted and mediated later improvements in school engagement. Qualitative findings reinforced this pathway: Boys described their project co-ordinators as people who listened to and understood them. These relationships were described as providing a safe space to reflect on emotions, practice self-regulation and build communication skills. The relational trust and emotional support built through the BDP appears to have translated into improved behaviour, reduced exclusions and enhanced school engagement, with effects emerging once boys had time to apply these skills in school life. Dosage and school environment were identified as important moderators of impact, with higher attendance and more supportive school climates associated with stronger outcomes.

Note: BPD = Boys' Development Programme; IPE = implementation and process evaluation; SSRS = Social Support and Rejection Scale

6.3.2 Policy context and contribution to knowledge

The BDP was implemented within a policy context marked by rising school exclusions, persistent educational disadvantage for boys from minority ethnic and low-income backgrounds, and a growing recognition of the link between school disengagement, exclusion from school and later youth violence and offending. The programme was developed to fill a clear gap in provision for boys at risk of exclusion in mainstream schools, offering a structured, relational and culturally responsive intervention focused on emotional learning, communication and positive identity development.

The BDP aligns closely with national and local priorities on school inclusion and early intervention. The UK Government's Timpson Review of School Exclusion (Timpson, 2019) highlighted the disproportionate exclusion of Black Caribbean and dual-heritage Caribbean boys and called for targeted preventative support to reduce exclusion risks. Similarly, the Working Together to Improve School Attendance guidance

(Department for Education, 2022) and the Serious Violence Duty (Home Office, 2023) emphasise the importance of relational and early support for young people at risk of disengagement from education. The BDP's manualised, evidence-informed approach and its focus on trusted adult relationships align with these priorities by addressing underlying social and emotional needs that contribute to exclusion.

The findings of this evaluation contribute new and rigorous UK-based evidence to a limited but growing body of research on targeted SEL interventions delivered in school settings. International meta-analyses (Durlak et al., 2011; Corcoran et al., 2018; Mahoney et al., 2021; Taylor et al., 2017) show that well-implemented SEL programmes can produce small to moderate improvements in social behaviour, emotional regulation and academic engagement, while UK evidence remains sparse. The BDP trial adds to this evidence base by demonstrating statistically significant improvements in school engagement (Hedges' $g = 0.21$, $p = 0.027$), reductions in behavioural difficulties (Hedges' $g = -0.22$, $p = 0.020$) and statistically significant decreases in both the number and duration of exclusions (Hedges' $g = -0.24$, $p = 0.024$, IRR = 0.65 and Hedges' $g = -0.33$, $p = 0.029$, IRR = 0.55, respectively) among boys at heightened risk of exclusion.

The pattern of impact, emerging after the programme had concluded, suggests that relational and emotional mechanisms take time to embed and manifest as measurable changes in behaviour and school engagement. The mediation analysis further strengthens this interpretation, confirming that improvements in mentoring relationships predicted and mediated later school engagement outcomes.

These findings make several distinct contributions to the evidence base by:

- **Providing what is, to our knowledge, the only UK-based RCT** of a school-based, one-to-one SEL intervention designed specifically for boys at risk of exclusion
- **Demonstrating that targeted relational and emotional support** can lead to statistically significant gains in school engagement and reductions in behavioural difficulties and exclusions when delivered with fidelity
- **Strengthening the understanding of relational mechanisms** by providing quantitative mediation evidence linking mentoring relationships to later school engagement
- **Highlighting the feasibility of delivering a high-fidelity, manualised mentoring programme** in mainstream schools within an RCT design
- **Contributing to the YEF's and EEF's equity priorities** by evidencing statistically significant positive effects for boys eligible for FSM and positive effect directions for minority ethnic boys
- **Underscoring the importance of time and consistency in delivery** by indicating that sustained exposure and higher programme dosage are key to achieving intended outcomes
- **Emphasising the value of longer-term follow-up in evaluation**, as some programme effects may only emerge or consolidate beyond the immediate post-intervention period

Overall, the BDP evaluation demonstrates that trusted one-to-one relationships can meaningfully improve school engagement and behaviour among boys at risk of exclusion. It strengthens the UK evidence base on how targeted, relational interventions can be embedded within schools to support inclusion and equity in education.

6.3.3 Considerations to develop the BDP and similar programmes in the future

Findings from both the impact evaluation and the IPE provide a strong rationale for continuing and further developing the BDP and similar social–emotional interventions for boys at risk of exclusion. The two strands of evidence are mutually reinforcing: While the impact analysis shows measurable improvements in engagement, behaviour and exclusions, the IPE identifies how these outcomes were achieved in practice and how the model could be strengthened for future delivery and scale.

Key considerations to develop the BDP and similar programmes in the future include:

1. **Preserve and build on the core strengths of the model.** Both the impact analysis and the IPE confirmed that the BDP’s success rests on trusted, consistent relationships between boys and relatable, skilled project co-ordinators. These relationships were repeatedly described as the reason boys attended sessions, opened up emotionally and applied what they learned in school life. Maintaining this workforce profile of culturally competent, empathetic, supportive project co-ordinators will be essential for fidelity and impact. At scale, this will require investment in workforce development, including recruitment pipelines, line management and reflective supervision, and manageable caseloads to preserve relational quality. Maintaining fidelity to the manual and structured supervision processes should remain central to the model as it grows.
2. **Extend the duration of the programme and reinforce learning over time.** Quantitative findings indicated that full participation (≥ 11 sessions) was associated with larger statistically significant improvements in school engagement. While fidelity was generally high across the whole cohort, some missed sessions due to school absence limited the programme’s intensity for a minority of boys. To strengthen impact, future delivery could consider the following:
 - Offering extended or phased delivery for boys with higher levels of need to ensure all boys receive at least 11 sessions
 - Piloting “booster” or follow-up sessions after programme completion to sustain skills and motivation
 - Building in structured re-entry points for boys requiring renewed support after transition points (e.g. between academic years)

These adaptations are likely to improve retention and the durability of outcomes but would have moderate to high cost implications.

3. **Strengthen alignment with school systems and families.** The IPE found that school environments influenced implementation and fidelity and that delivery worked best where the BDP was embedded within the wider school environment. Embedding future delivery of the BDP within existing pastoral and inclusion systems, for example, through joint planning with PSHE, SEN and safeguarding teams, could support this further. The IPE also highlighted opportunities for stronger family and teacher engagement to reinforce messages beyond one-to-one sessions. Future iterations of the programme could include periodic communication with parents/carers, briefings for teachers or collaborative goal setting with key members of school staff. These actions would incur low to moderate additional cost but could meaningfully strengthen embeddedness with the wider school community.

4. **Enhance inclusivity and relevance.** Future Men should continue to prioritise equity and accessibility as central design principles. The IPE identified potential for improvement in engaging boys with EAL or SEND, as well as for addressing contemporary social issues (e.g. online influencers, gender identities and sexual orientations). Updating training and resources to reflect these realities would ensure ongoing cultural relevance and reach.
5. **Improve data systems and the evaluation of long-term outcomes.** Administrative data indicated reductions in exclusions but no measurable change in attendance for the subset of schools that provided this data for the evaluation. These findings highlight both the importance and the challenge of tracking longer-term outcomes. Future evaluations should consider standardising attendance and exclusion data collection across schools and including follow-up points at 12 months or more to test whether gains in self-reported school engagement translate into sustained educational participation. There is an opportunity to conduct this type of research via the YEF data archive, including analyses of the association between differences in exclusion and differences in offending behaviour.

In summary, the BDP represents a well-evidenced and well-implemented model of targeted social-emotional support for boys at risk of exclusion. Strengthening its future delivery will depend on maintaining relational consistency, increasing dosage and duration, embedding relational practice within schools, and ensuring that evaluation and scaling approaches continue to foreground equity, cultural competence and systemic context.

6.4 Limitations and lessons learned

6.4.1 Limitations of the evaluation

The BDP efficacy study comprised a theory-led, two-armed RCT with integrated IPE and cost assessment. The study was delivered to a high standard and in accordance with the efficacy study protocol and SAP. This provides strong confidence in the findings presented throughout this report. Nonetheless, several limitations should be considered when interpreting the findings:

- **Limited statistical power to reliably detect small effects and for subgroup analyses.** Although the study achieved the sample size specified in the SAP, it was powered in line with YEF guidance to detect a moderate effect size (MDES = 0.19). The observed statistically significant effects on school engagement (Hedges' g = 0.21) and behavioural difficulties (Hedges' g = -0.22) at T3 meet this threshold, indicating a meaningful impact. However, smaller, non-significant effects observed for emotional symptoms, peer relationships and teacher relationships (Hedges' g ranging from 0.05 to 0.12) were below this MDES and, therefore, unlikely to reach statistical significance even if true population effects exist. Similarly, exploratory subgroup analyses (e.g. by ethnicity) were underpowered to detect small differences across groups with confidence. Future evaluations seeking to explore subgroup variations or smaller but policy-relevant effects should use larger, equity-powered samples.
- **Potential for impact dilution due to the nature of the control condition.** Participants in the signposting group were informed about other school and local support options consistent with a BAU approach. Some of these services, such as pastoral mentoring, youth work or emotional support, may have offered comparable support to that provided by the BDP. Because the extent and quality of BAU provision were not systematically recorded, it is possible that some boys in the signposting group

accessed similar support, reducing the contrast between groups and potentially underestimating the true impact of the BDP (i.e. suggesting that the effect estimates observed in this evaluation may be conservative). This limitation is common in school-based and community evaluations, where ethical and practical considerations require ensuring that all young people have access to some form of support.

- **Potential for contamination between boys in the BDP delivery group and the signposting group.** As randomisation occurred at the individual level within schools, there was some potential for contamination across groups. Boys shared school environments and, in some cases, social networks, which may have led to informal discussions of programme content. Qualitative evidence from one school stakeholder indicated that some boys who had completed the BDP actively encouraged their peers to take part, suggesting a degree of peer-to-peer diffusion, although this was not mentioned by other stakeholders, and the prevalence of this was not measured directly. However, it is possible that this informal sharing of ideas and approaches may have reduced the contrast between groups, meaning that the impact estimates reported here may be conservative.
- **Length of follow-up.** The study's 24-week (six-month) post-randomisation follow-up period was sufficient to capture emerging changes in school engagement and behaviour but may not have been long enough to detect cumulative or sustained effects on longer-term outcomes, such as attendance. The emergence of statistically significant effects at 24 weeks suggests that changes consolidate gradually. Longer-term follow-up (e.g. 12–18 months post-randomisation) would be valuable to determine whether early improvements in school engagement and behaviour are maintained and translate into reduced exclusions and improved educational outcomes over a longer time period.
- **Generalisability and contextual variation.** The BDP was delivered under efficacy study conditions across eight secondary schools in South London. These schools shared some contextual characteristics (urban settings with high levels of deprivation and diverse pupil populations), limiting the generalisability of findings to other school contexts (e.g. rural or less diverse areas). Furthermore, the IPE identified variability in implementation linked to school-level factors. Future testing under effectiveness conditions should examine how the BDP performs across a wider range of school types and local systems to understand generalisability further.
- **Limited risk of bias from attrition and missing data.** As with any RCT designed with self-report data, participant attrition and missing data pose potential risks of bias. However, the study achieved a strong recruitment sample, and baseline characteristics were well balanced across treatment and control groups. While some attrition occurred at both 12- and 24-week post-randomisation follow-ups, overall retention rates remained strong, balanced across groups and well within acceptable limits for a school-based trial, with final samples exceeding the planned targets from power calculations at both time points. Analyses were conducted using a complete-case approach, consistent with the pre-registered SAP, and comparisons of baseline characteristics between completers and non-completers found no systematic differences. On this basis, the risk of attrition bias is considered low and effectively mitigated.

Overall, these limitations do not undermine the validity of the findings but provide important context for interpretation. The trial provides a robust and credible estimate of the BDP's impact three months after programme completion, demonstrating measurable improvements in school engagement, behaviour and exclusions under efficacy conditions.

Future studies should include longer follow-up, larger samples to detect smaller effects and subgroup differences, and expanded collection of administrative data to explore how implementation quality, school context and contamination risk influence long-term outcomes.

6.4.2 Lessons for future evaluations

This evaluation adds to a growing body of work demonstrating that **RCTs of school-based SEL programmes in the UK are both feasible and valuable**. The BDP efficacy study provides high-quality causal evidence of what works to improve school engagement and behaviour among boys at risk of exclusion. It also offers important learning about the conditions required to deliver and evaluate relational interventions within mainstream school settings.

Key lessons include:

- **An innovative and committed delivery partner.** Future Men demonstrated a strong commitment to innovation, learning and collaboration throughout the evaluation. As one of the first UK organisations to subject a targeted, one-to-one SEL programme to an RCT, Future Men showed a high degree of adaptability and professionalism in balancing delivery fidelity with research requirements. The organisation's willingness to engage openly with Cordis Bright and the YEF on study design, data collection and randomisation processes was critical to the study's success. Its leadership and reflective approach model how similar VCSE organisations can use rigorous evaluation to strengthen evidence and practice.
- **The importance of building support for the RCT.** Future Men played an active role in building and maintaining engagement from school stakeholders with the evaluation. This included working closely with senior leaders, safeguarding teams and pastoral staff to manage referrals, scheduling and data collection. Establishing strong relationships with schools was essential to achieving informed consent, minimising attrition and securing high-quality data. The delivery team's ability to communicate the purpose and value of the evaluation helped maintain buy-in and trust across all eight participating schools.
- **A collaborative and co-designed approach.** The evaluation was co-designed by Cordis Bright, Future Men and the YEF, ensuring shared ownership and alignment between delivery and evaluation objectives. This included refining the theory of change, designing data collection processes and agreeing evaluation materials and procedures. Cordis Bright provided training and ongoing guidance to Future Men's delivery and research staff on data collection, informed consent and randomisation, supported by a clear evaluation handbook. Regular communication, including weekly updates on recruitment, data collection progress and randomisation, ensured transparency and responsiveness. This collaborative approach was central to the success of the trial and the quality of the data collected.
- **Minimising attrition through collaborative design and in-person data collection.** The BDP trial achieved exceptionally low attrition rates (4% at T2 and 9% at T3) by embedding data collection within delivery processes and prioritising collaboration between the evaluation and delivery teams. Key enablers included dedicated data collection staff within the Future Men team who were not involved in delivering support, clear school agreements, in-person survey sessions during school hours and continuous communication between evaluators and delivery partners. These practices ensured high response rates across cohorts and balanced retention across trial arms. Future school-

based evaluations should plan similar resource allocation and scheduling flexibility from the outset, treating data collection as a core delivery activity rather than a separate research task.

- **Importance of real-time monitoring and reflection.** Throughout the trial, Future Men systematically recorded delivery and attendance data, providing a clear picture of dosage, fidelity and participant progress. While monitoring systems functioned effectively, future evaluations could benefit from real-time data dashboards to allow immediate oversight of session delivery, attendance and fidelity. This would support ongoing supervision and adaptive management during future studies. The inclusion of linked administrative data on exclusions also provided valuable insight and should be considered and resourced appropriately in future YEF trials, given its potential to capture behavioural change.
- **Embedding EDI within delivery.** Future Men embedded strong EDI principles in programme delivery and staff recruitment, ensuring culturally responsive delivery. This cultural competence was highlighted by boys as a factor that enhanced trust, relatability and engagement with the programme. Future programmes should continue to prioritise the inclusive practices deployed by the delivery team, and evaluators should consider formal measurement of cultural responsiveness as part of implementation fidelity.
- **Stable funding and organisational capacity.** The BDP was delivered with consistent staffing and oversight throughout the evaluation period, which supported high fidelity and low staff turnover. However, like many youth organisations, Future Men operates within short-term funding cycles. Sustained, multi-year investment would support workforce stability, reduce administrative turnover and maintain momentum for both delivery and evaluation.
- **Mixed-methods approach and triangulation.** The combination of an RCT and comprehensive IPE provided both causal evidence and rich contextual understanding. Quantitative results captured measurable changes in school engagement and behaviour and the mechanisms that supported these, while qualitative findings illuminated the mechanisms and experiences underlying those effects. This mixed-methods approach proved essential for interpreting the timing and nature of change and should remain a core element of future YEF- and EEF-funded studies.
- **Designing evaluations for statistical power and long-term follow-up.** As with other social and emotional interventions, effects were mainly modest to moderate. Future studies of similar programmes should be powered to detect small effects, which are typical of complex school-based interventions, and should include longer follow-up periods (e.g. 12–24 months) to test whether short-term improvements are sustained and whether improvements in school engagement eventually translate into attendance or attainment gains.
- **Selecting outcome measures that are appropriate, theory-aligned and sensitive to change.** This evaluation highlighted the importance of choosing short, age-appropriate measures that directly reflect the programme's theory of change and have strong psychometric properties. The SCQ and SDQ were suitable and acceptable for this cohort, but some scales showed skewed distributions, which may have limited sensitivity to small changes, particularly over short follow-up periods. Future evaluations would benefit from selecting or supplementing measures that minimise respondent burden, map clearly onto the intervention's mechanisms and are responsive enough to

detect early social–emotional shifts, especially in studies with modest expected effect sizes and younger participants.

6.4.3 Generalisability of findings

The findings of this efficacy study are likely to be most relevant to **urban secondary schools in the UK** operating in contexts of socioeconomic disadvantage and high exclusion risk. The eight participating schools were located in South London boroughs with elevated deprivation indices and diverse student populations, suggesting that the BDP may be particularly suitable for schools with similar characteristics.

However, there are important considerations for applying these findings elsewhere:

- **Delivery organisation and context.** The BDP was implemented by an experienced organisation with strong local credibility and relationships with schools. These contextual strengths supported recruitment and fidelity and may not be directly replicable in areas without similar trust and networks.
- **Population characteristics.** While the sample reflected the BDP’s intended target group (boys at risk of exclusion, predominantly from minority ethnic and low-income backgrounds), the study was not powered to detect small to moderate differences across subgroups. Further research is needed to test effectiveness for different ethnic groups, sexes or regions.
- **Programme duration and intensity.** The BDP was generally delivered over one school term, and effects were observed three months after completion. In different contexts or populations, impacts may vary depending on school readiness, delivery capacity and participant engagement.
- **School readiness and culture.** The BDP relies on school environments that are able to support implementation. Implementation and outcomes may differ in schools with different contexts, environments or cultures.

Overall, findings should be generalised cautiously. The results demonstrate clear promise under controlled efficacy conditions; further research under effectiveness conditions across a wider range of schools and regions would help determine scalability and broader applicability.

6.5 Future research and publications

The BDP efficacy study raises several important questions that can inform future evaluation, scaling and adaptation of the programme.

- **Long-term impact and sustainability.** The emergence of statistically significant effects at 24 weeks suggests that change continues after delivery. Extending follow-up to 12 to 18 months would help to determine whether these improvements in school engagement, behaviour and exclusions are sustained and whether they translate into improved attendance or academic attainment over time. There is a role for future analysis of YEF archive data in addressing some of these questions.
- **Mechanisms of change.** Further mediation analyses using longitudinal data could deepen understanding of the relational and emotional pathways through which the BDP operates, particularly how trust, emotional regulation and mentoring quality interact to influence behavioural outcomes.

- **Differential impacts and equity.** Future studies should include larger, equity-powered samples to further test subgroup differences across ethnicity (including for boys from Black Caribbean and dual Caribbean heritage backgrounds), socioeconomic background and baseline risk levels. Understanding who benefits most and under what conditions is key to ensuring equitable outcomes.
- **Comparative effectiveness.** Comparative research could situate the BDP alongside other evidence-based SEL interventions within the YEF Toolkit to assess relative cost-effectiveness and scalability.
- **Integration with school systems.** Future research should explore how programmes like the BDP can be embedded into whole-school approaches to inclusion, including how teacher–pupil relationships and school culture can sustain gains.
- **Practitioner development and workforce models.** As the BDP scales, further work could examine the optimal training, supervision and reflective practice models required to maintain fidelity and practitioner wellbeing in a range of school contexts.
- **Data linkage and mixed-method evaluation.** Integrating administrative data on attendance, exclusions and attainment alongside self-report measures offers a broader evidence base and supports stronger triangulation of findings. Continued mixed-method approaches, including participatory evaluation with young people, would ensure that future evidence remains grounded in lived experience.

These lessons reinforce the value and feasibility of rigorous RCTs in evaluating SEL programmes. The BDP study contributes robust UK-based evidence that trusted, culturally competent one-to-one support within schools can improve engagement and behaviour among boys at risk of exclusion. Future research should build on this foundation through longer follow-up, equity-powered designs and effectiveness testing across a range of school contexts to inform sustainable system-wide practice.

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8 Appendix A: Effect size estimation

Appendix table 2: Effect size estimation

Outcome	Time point	Unadjusted differences in means	Adjusted differences in means	BDP delivery group		Signposting group		Pooled variance
				n (missing)	Variance of outcome	n (missing)	Variance of outcome	
School engagement	T2	-0.01	0.09	232 (1)	6.72	234 (1)	6.07	6.40
School engagement	T3	0.40	0.48	221 (0)	6.42	219 (3)	8.13	7.27
Externalising behaviour	T2	-0.54	-0.46	232 (1)	14.04	233 (2)	15.38	14.71
Externalising behaviour	T3	-0.79	-0.63	221 (0)	18.40	220 (2)	15.70	17.03
Peer difficulties	T2	0.11	0.19	232 (1)	2.20	233 (2)	1.94	2.07
Peer difficulties	T3	0.03	0.09	221 (0)	2.80	220 (2)	2.54	2.67
Emotional symptoms	T2	-0.21	-0.20	232 (1)	5.21	233 (2)	5.55	5.38
Emotional symptoms	T3	-0.24	-0.20	220 (1)	5.57	219 (3)	5.98	5.77
Pro-social behaviours	T2	0.23	0.26	232 (1)	3.85	234 (1)	3.64	3.75
Pro-social behaviours	T3	0.32	0.37	221 (0)	3.38	222 (0)	4.19	3.79
Teacher bonding and attachment	T2	0.09	0.09	225 (8)	2.25	230 (5)	2.19	2.22
Teacher bonding and attachment	T3	0.10	0.14	211 (10)	2.34	214 (8)	2.00	2.17
Peer bonding	T2	-0.11	-0.10	229 (4)	1.27	231 (4)	1.00	1.14
Peer bonding	T3	0.10	0.13	217 (4)	0.96	214 (8)	1.34	1.15
School engagement subscale	T2	0.12	0.13	223 (10)	1.58	232 (3)	1.35	1.46
School engagement subscale	T3	0.20	0.20	209 (11)	1.45	215 (7)	1.53	1.49

9 Appendix B: Baseline distributions of outcome measures

Histograms of baseline primary and secondary outcome measures were run to assess the distribution of baseline scores, based on all participants with valid T1 data. These are provided below.

The histograms indicate variation in the extent to which measures captured a spread of responses across the cohort, which has implications for their sensitivity in detecting change. They show that:

- **Several measures showed a modest clustering of scores at the upper end of their scales**, indicating high baseline scores for many boys. The SCQ total score displayed a negative skew, with most boys reporting high levels of school connectedness at baseline. A similar pattern was observed for the SCQ Peer subscale, the SCQ Engagement subscale, and the SDQ pro-social behaviours subscale, where responses were concentrated at the positive end of each scale. These negatively skewed distributions highlight a potentially small ceiling effect across these outcomes, possibly limiting the extent to which measurable improvement can be detected for much of the cohort.
- **Emotional problems and peer difficulties (as measured by the SDQ) were concentrated at the lower end of their scales**, with most boys reporting few difficulties in these areas at baseline. This suggests that, for these outcomes, there may also be more limited room for improvement among boys.
- **Externalising behaviours scores and the SCQ Teacher subscale displayed more balanced distributions**, with scores spanning the full range and clustering around the mid-points of their respective scales. These more normally distributed measures demonstrated greater spread across low, moderate and higher scores, suggesting they may be comparatively more sensitive for detecting change. The SCQ Teacher subscale captured a broad range of teacher-related connectedness, indicating that it may better reflect meaningful variation in baseline experiences than the more skewed peer- or engagement-related subscales.

Figure 4: Baseline SCQ scores

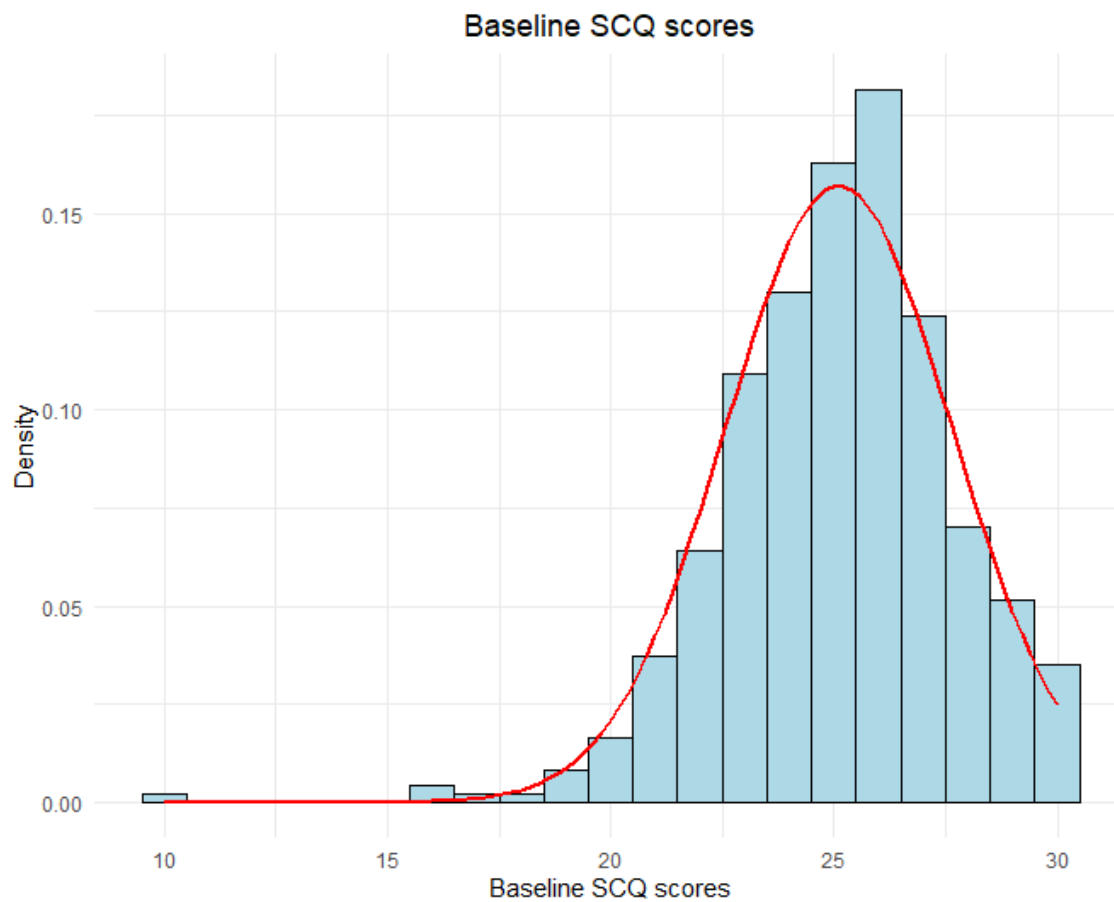


Figure 5: Baseline SDQ emotional problems scores

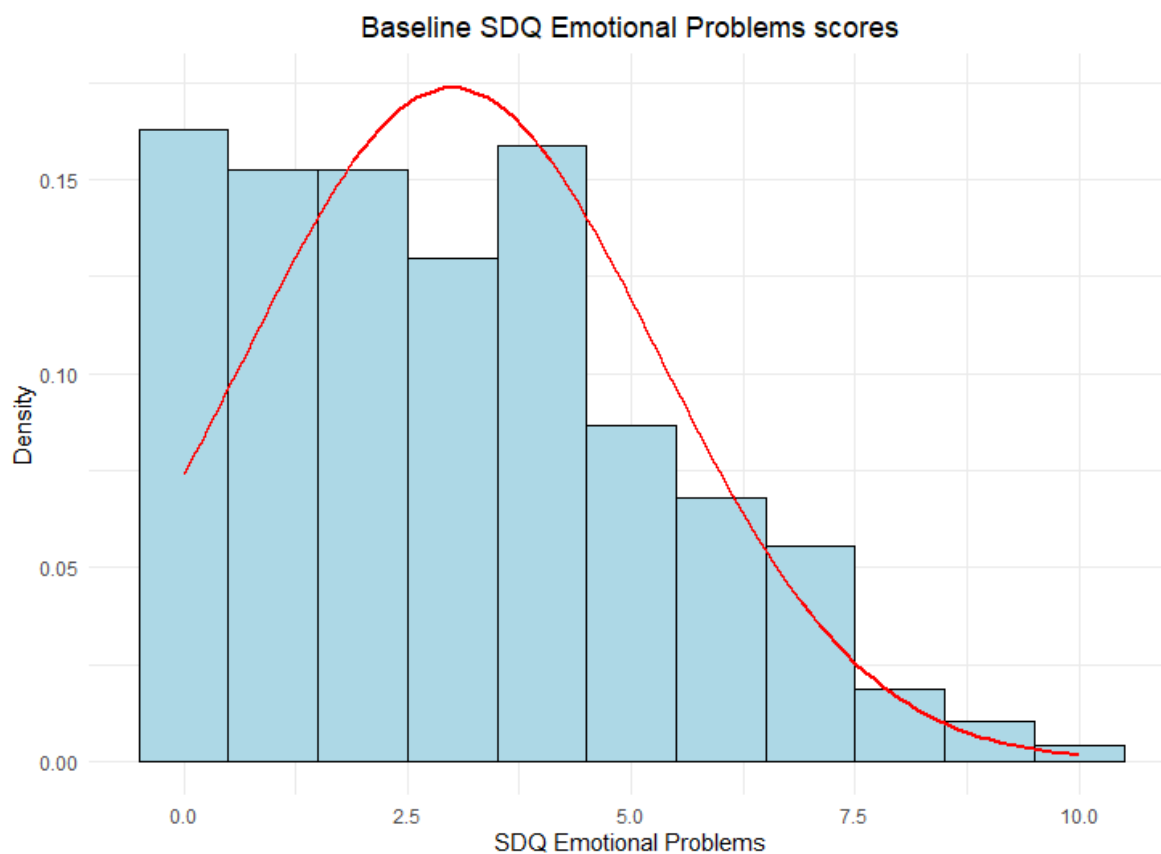


Figure 6: Baseline SDQ peer difficulties scores

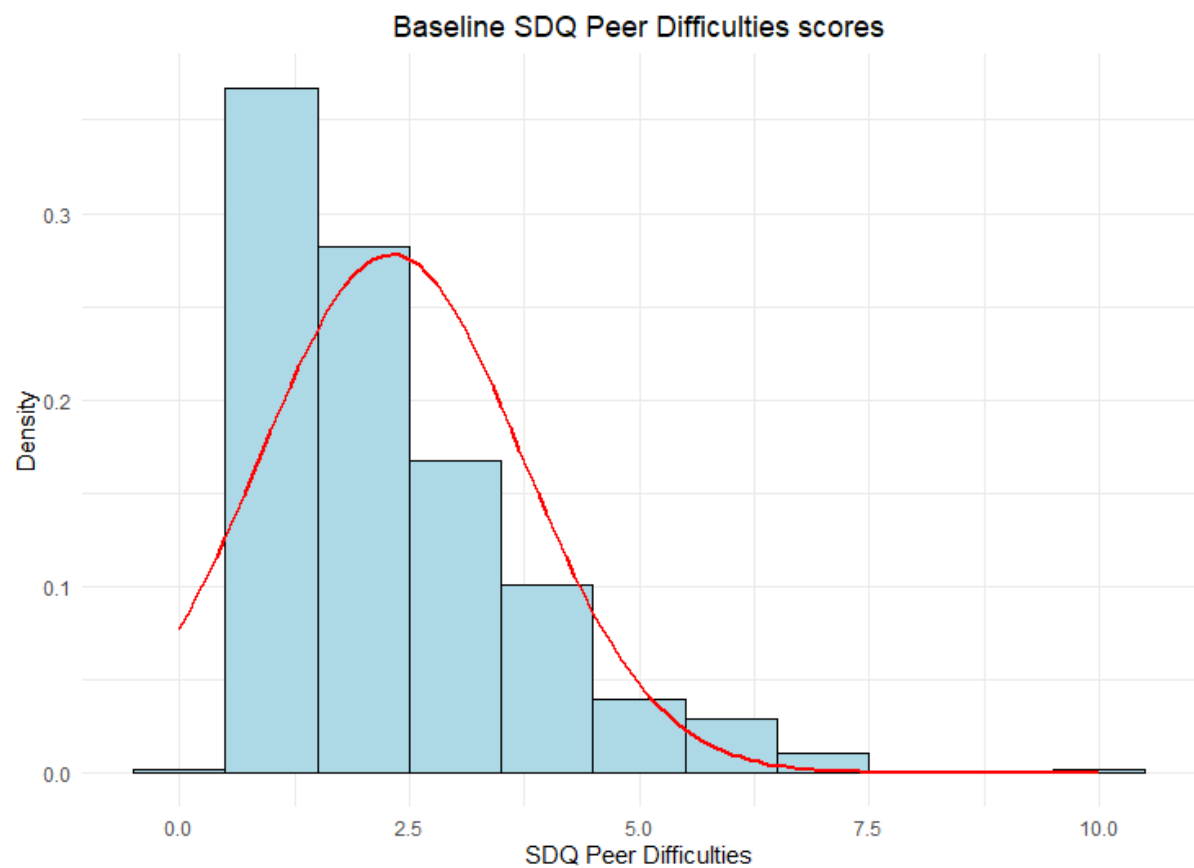


Figure 7: Baseline SDQ externalising behaviours scores

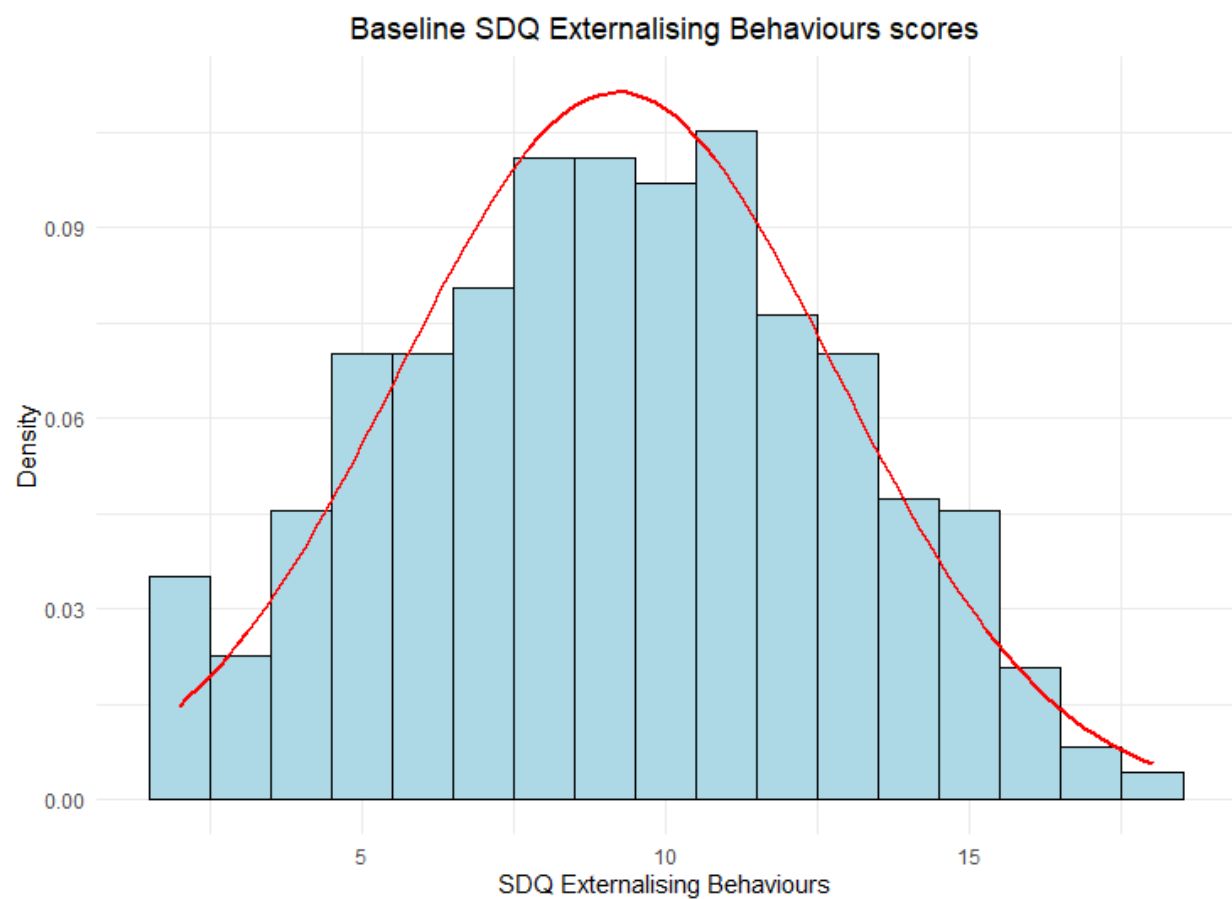


Figure 8: Baseline SDQ pro-social behaviours scores

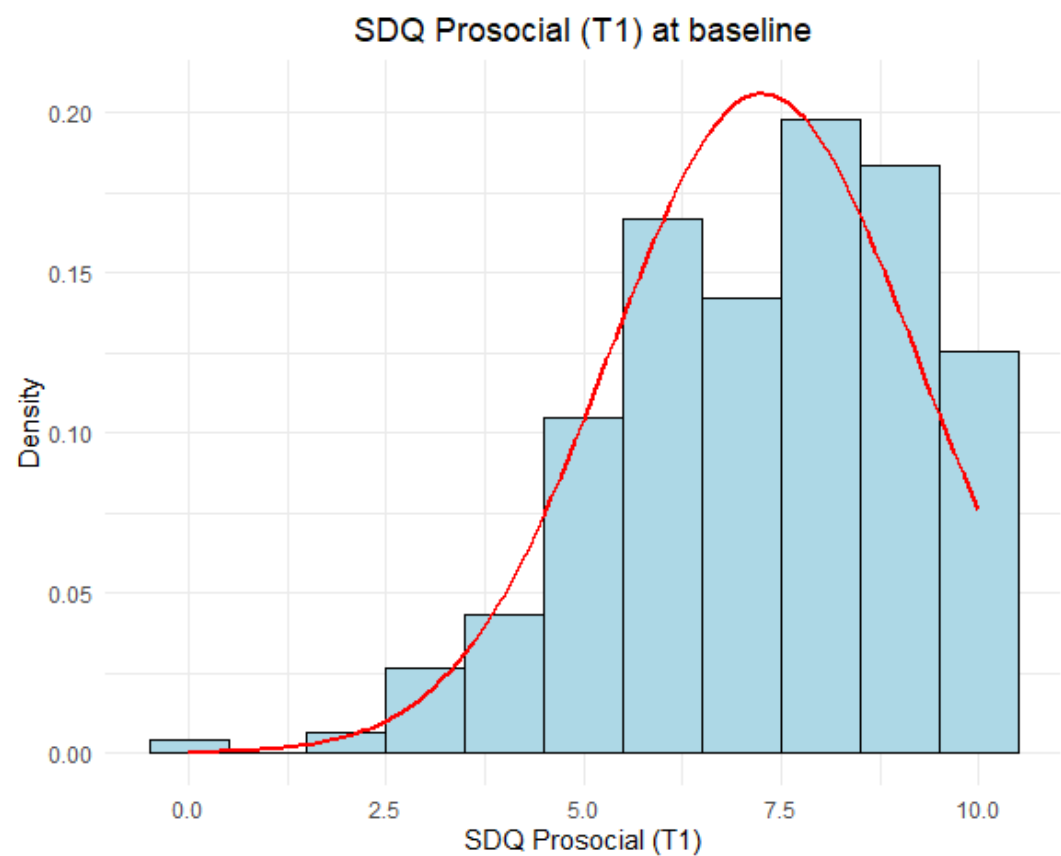


Figure 9: Baseline SCQ school engagement subscale scores

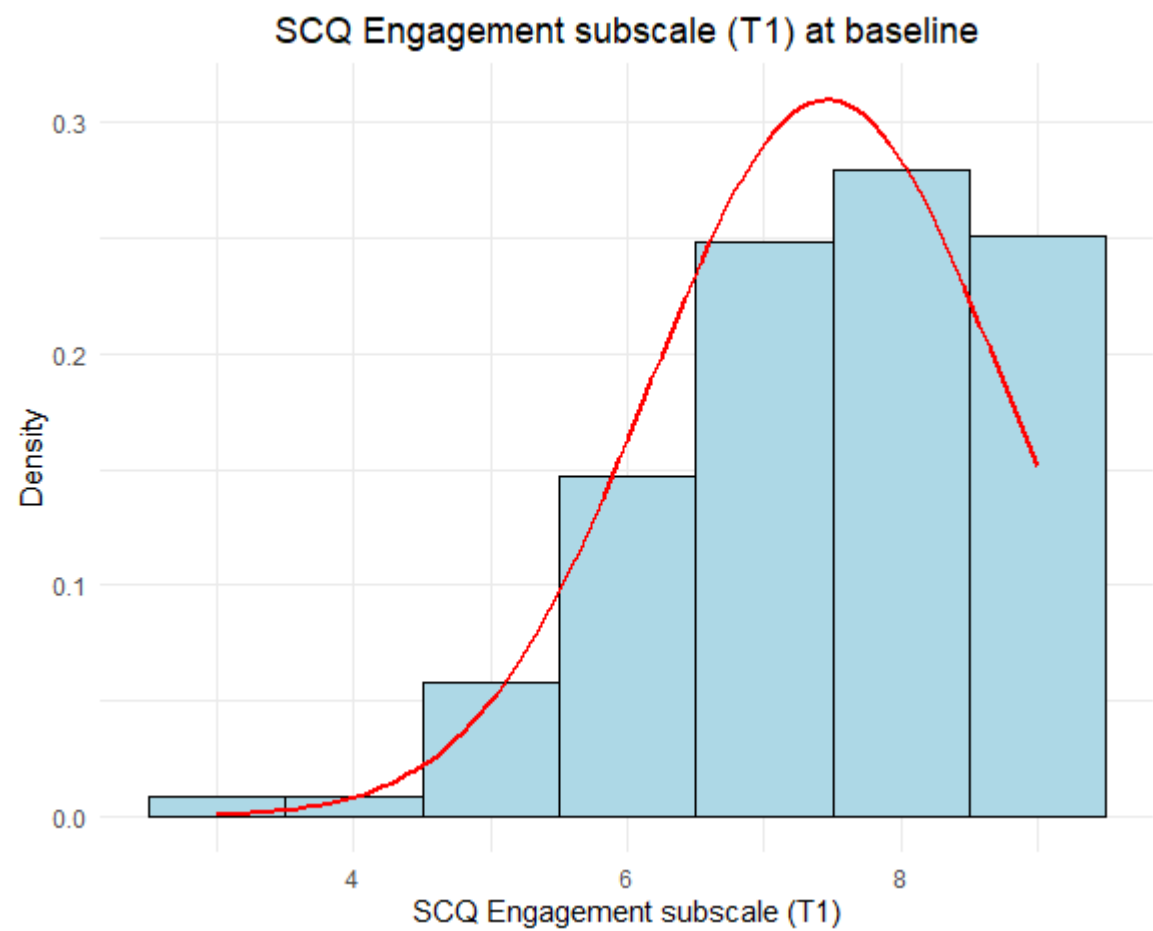


Figure 10: Baseline SCQ Teacher bonding and attachment scores

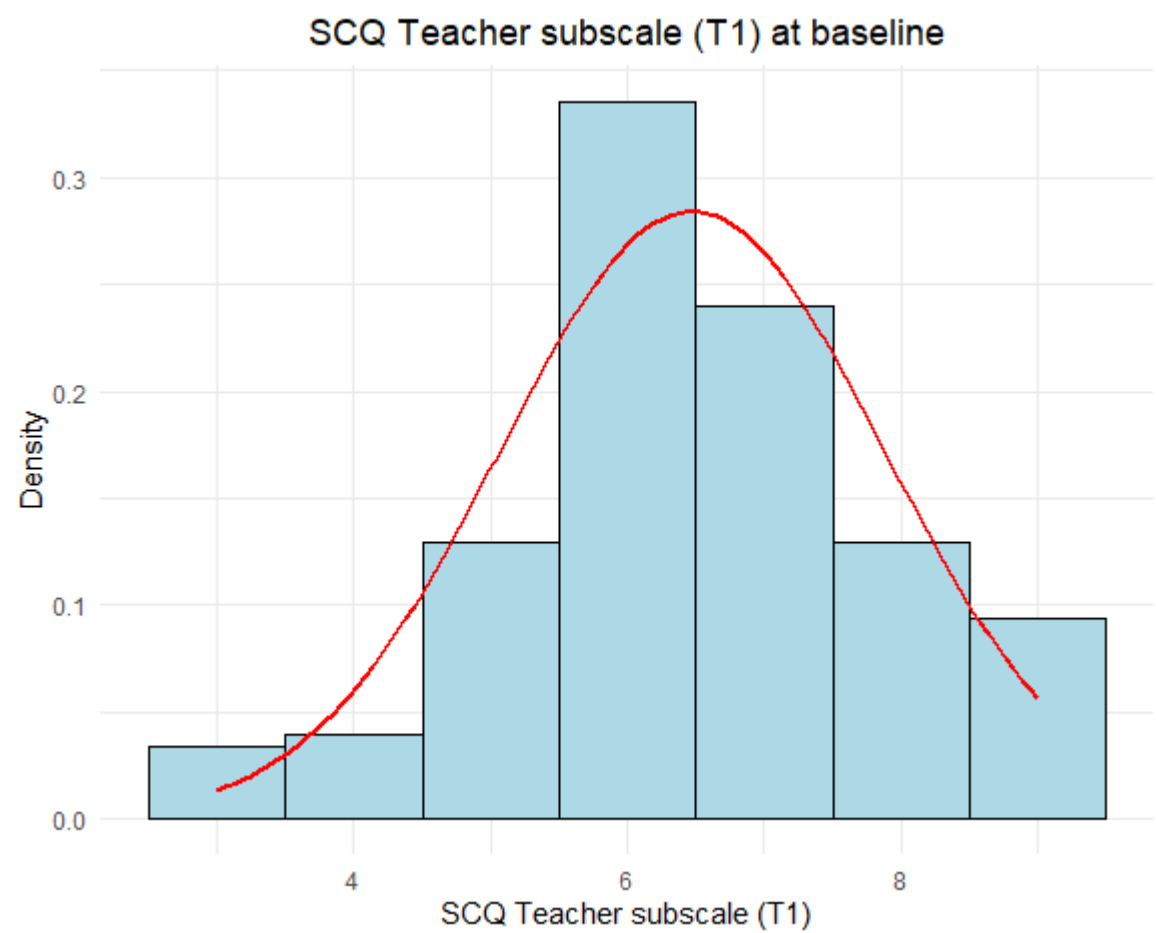
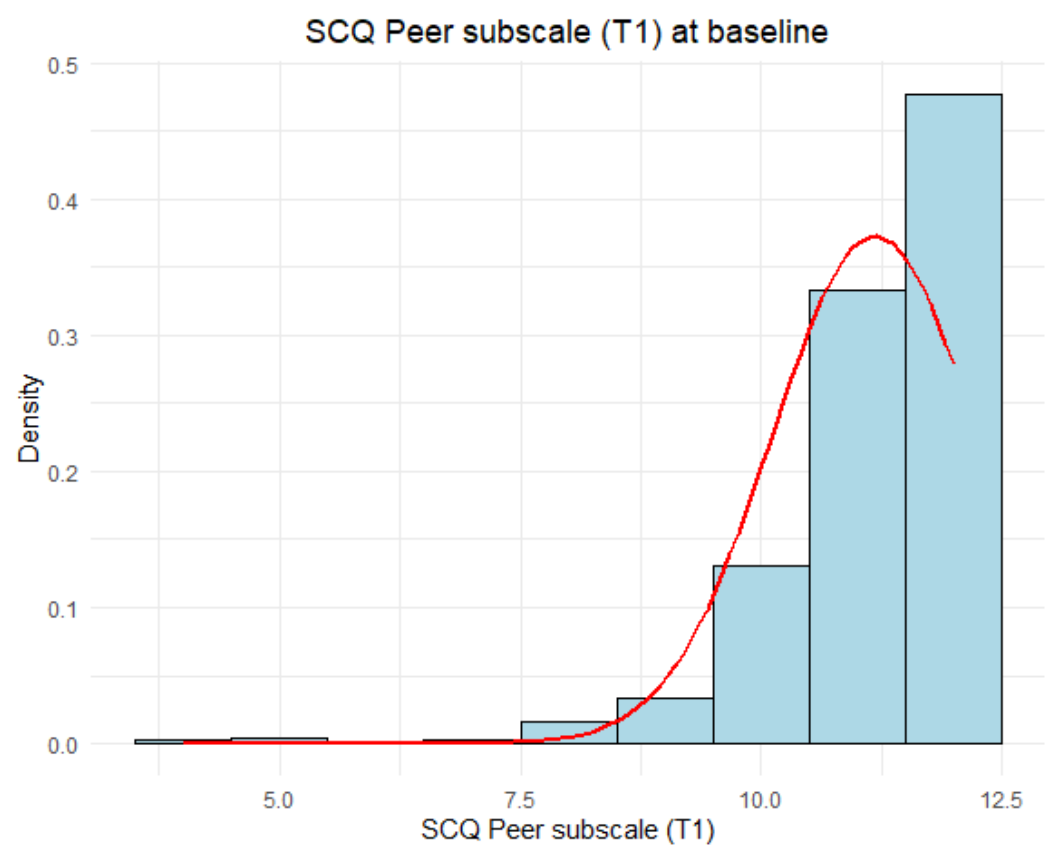


Figure 11: Baseline SCQ Peer bonding and attachment scores



10 Appendix C: Monitoring data analysis

Table 42: Ethnicity for randomised boys (n=486)

Ethnicity	n	%
Asian or Asian British	28	5.8%
Asian or Asian British: Any other Asian background	12	2.5%
Asian or Asian British: Bangladeshi	4	0.8%
Asian or Asian British: Chinese	1	0.2%
Asian or Asian British: Indian	2	0.4%
Asian or Asian British: Pakistani	9	1.9%
Black, Black British, Caribbean or African	268	55.1%
Black, Black British, Caribbean or African: Any other Black, Black British or Caribbean background	89	18.3%
Black, Black British, Caribbean or African: African	109	22.4%
Black, Black British, Caribbean or African: Caribbean	70	14.4%
Mixed or Multiple ethnic groups	60	12.3%
Mixed or multiple ethnic groups: Any other Mixed or multiple ethnic background	24	4.9%
Mixed or multiple ethnic groups: White and Asian	1	0.2%
Mixed or multiple ethnic groups: White and Black African	9	1.9%
Mixed or multiple ethnic groups: White and Black Caribbean	26	5.3%
Other ethnic group	35	7.2%
Other ethnic group: Any other ethnic group	27	5.6%
Other ethnic group: Arab	8	1.6%
White/any other White background	83	17.1%
White: Any other White background	25	5.1%
White: English, Welsh, Scottish, Northern Irish, British	58	11.9%
Not known/prefer not to say	12	2.5%
Total	486	100%

Table 43: SEND status for all recruited boys (n=487)

SEND status	n	%
No	317	66.5%
Yes	160	33.5%
Not known	10	-

SEND status	n	%
Total	487	100%

Table 44: SEND need for all recruited boys (n=487)

SEND need ³¹	n	%
Social, Emotional and Mental Health (SEMH)	109	22%
Cognition and learning	60	12%
Communication and interaction	64	13%
Sensory and or physical difficulties	3	1%
None	298	61%
Not known	15	-

Table 45: EHCP status for all recruited boys (n=487)

EHCP	n	%
No	450	94.3%
Yes	27	5.7%
Missing (Not known/prefer not to say)	10	-
Total	487	100%

Table 46: Number of recruited boys with English as an additional language (n=487)

English as an additional language	n	%
No	389	79.9%
Yes	91	19.0%
Missing (Not known/prefer not to say)	7	-
Total	487	100%

Table 47: Number of recruited boys eligible for free school meals (n=487)

Eligible for free school meals	n	%
No	176	36.7%

³¹ Referrers could select multiple SEND needs for each participant. As a result, the total number of SEND needs may exceed 487 and the percentages may not sum to 100%.

Eligible for free school meals	n	%
Yes	304	63.3%
Not known	7	-
Total	487	100%

Table 48: Care status of all recruited boys (n=487)

Care status	n	%
Child in need (CIN)	3	0.6%
Looked after child	7	1.5%
Child protection plan (CPP)	0	0
None	466	97.9%
Not known	11	-
Total	487	100%

Table 49: Cross tabulation of school year by school attended for all recruited boys (n=487)

School year	Year 7	Year 8	Year 9	Year 10	Year 11	Total
School	n (%)	n (%)	n (%)	n (%)	n (%)	
School A	17 (22.1%)	17 (22.1%)	18 (23.4%)	23 (29.9%)	2 (2.6%)	77 (100.0%)
School B	15 (18.1%)	19 (22.9%)	19 (22.9%)	25 (30.1%)	5 (6.0%)	83 (100.0%)
School C	28 (35.0%)	15 (18.8%)	22 (27.5%)	10 (12.5%)	5 (6.3%)	80 (100.0%)
School D	8 (20.5%)	11 (28.2%)	9 (23.1%)	11 (28.2%)	0 (0.0%)	39 (100.0%)
School E	10 (25.0%)	11 (27.5%)	8 (20.0%)	6 (15.0%)	5 (12.5%)	40 (100.0%)
School F	18 (41.9%)	11 (25.6%)	12 (27.9%)	2 (4.7%)	0 (0.0%)	43 (100.0%)
School G	11 (24.4%)	15 (33.3%)	15 (33.3%)	2 (4.4%)	2 (4.4%)	45 (100.0%)
School H	23 (28.8%)	17 (21.3%)	21 (26.3%)	16 (20.0%)	3 (3.8%)	80 (100.0%)
Total	130 (26.7%)	116 (23.8%)	124 (25.5%)	96 (19.5%)	22 (4.5%)	487 (100.0%)

Table 50: Number of sessions offered to each boy who received the BDP (n=245)

Number of sessions offered per boy	n	%
0 - 5	5	0.4%
6 – 10	39	15.9%
11 – 15	179	73.1%
16 – 19	22	9.0%

Number of sessions offered per boy	n	%
Total	245	100%
Average number of sessions offered		12.1

Table 51: Number of missed sessions per boy (i.e. the reason for a missed session was related to the young person, and not due to either school disruption or project co-ordinator absence) (n=245)

Number of missed sessions per boy	n	%
0	98	40%
1 – 3	125	51%
4 – 6	18	7.3%
7 - 9	4	1.6%
Total	245	100%
Average number of missed sessions per boy		1.3

Table 52: Full breakdown of the number of sessions for boys who received the BDP (n=245)

Number of sessions received	Number of boys	%
0	4	1.6%
1	0	0%
2	1	0.4%
3	1	0.4%
4	2	0.8%
5	3	1.2%
6	1	0.4%
7	6	2.4%
8	40	16.3%
9	36	14.7%
10	53	21.6%
11	68	27.8%
12	26	10.6%
13	4	1.6%
Total	245	100.0%
Average number of sessions received		9.6

Table 53: Number of sessions offered to boys who received the BDP (n=245)

Number of sessions offered	Number of boys	%
0	1	0.4%
1	2	0.8%
2	1	0.4%
3	0	0%
4	0	0%
5	1	0.4%
6	0	0.0%
7	1	0.4%
8	9	3.7%
9	11	4.5%
10	18	7.3%
11	46	18.8%
12	59	24.1%
13	39	15.9%
14	23	9.4%
15	12	4.9%
16	5	2.0%
17	11	4.5%
18	5	2.0%
19	1	0.4%
Total boys	245	100.0%
Average sessions offered		12.1

Table 54: Length of support in weeks for boys who received the BDP (n=245)

Length of support	Number of boys	%
0	4	1.6%
1	1	0.4%
2	0	0.0%
3	1	0.4%
4	1	0.4%
5	0	0.0%

Length of support	Number of boys	%
6	2	0.8%
7	5	2.0%
8	15	6.1%
9	6	2.4%
10	14	5.7%
11	41	16.7%
12	35	14.3%
13	9	3.7%
14	20	8.2%
15	16	6.5%
16	22	9.0%
17	21	8.6%
18	13	5.3%
19	6	2.4%
20	0	0%
22	1	0.4%
29	1	0.4%
30	2	0.8%
31	2	0.8%
32	4	1.6%
33	1	0.4%
34+	2	0.8%
Total	245	100.0%

Table 55: Number of topics covered by all boys who received the BDP (n=245)

Number of topics covered	Number of boys	%
0	4	1.6%
1	0	0%
2	2	0.8%
3	3	1.2%
4	11	4.5%
5	76	31.0%
6	149	60.8%
Total	245	100.0%

11 Appendix D: Subgroup analysis

Table 56: Subgroup analysis - FSM interaction on SCQ scores at T2

Predictors	Estimates	Standard error	p-value
Intercept	12.11	1.03	< 0.001
Group: BDP group	-0.05	0.33	0.883
Baseline SCQ score	0.52	0.04	< 0.001
FSM: Eligible	0.01	0.30	0.965
Group:BDP group x FSM: Eligible	0.27	0.2	0.525

Table 57: Subgroup analysis - FSM interaction on SCQ scores at T3

Predictors	Estimates	Standard error	p-value
Intercept	14.05	1.21	< 0.001
Group: BDP group	0.18	0.38	0.627
Baseline SCQ score	0.45	0.05	< 0.001
FSM: Eligible	-0.54	0.35	0.118
Group:BDP group x FSM: Eligible	0.47	0.48	0.334

Table 58: Subgroup analysis – White/Minority ethnic interaction on SCQ scores at T2

Predictors	Estimates	Standard error	p-value
Intercept	11.76	1.04	< 0.001
Group: BDP group	0.62	0.49	0.201
Baseline SCQ score	0.52	0.04	< 0.001
Ethnicity: Minority Ethnic	0.45	0.37	0.232
Group:BDP group x Ethnicity: Minority ethnic	-0.64	0.53	0.231

Table 59: Subgroup analysis - White/Minority ethnic interaction on SCQ scores at T3

Predictors	Estimates	Standard error	p-value
Intercept	13.96	1.21	< 0.001
Group: BDP group	0.69	0.56	0.214
Baseline SCQ score	0.44	0.05	< 0.001
Ethnicity: Minority Ethnic	0.16	0.43	0.717

Predictors	Estimates	Standard error	p-value
Group:BDP group x Ethnicity: Minority ethnic	-0.28	0.61	0.651

Table 60: Subgroup analysis – Black or Mixed Caribbean / Non Black or Mixed Caribbean on SCQ scores at T2

Predictors	Estimates	Standard error	p-value
Intercept	12.30	1.04	< 0.001
Group: BDP group	-0.03	0.46	0.953
Baseline SCQ score	0.52	0.04	< 0.001
Ethnicity: Non-Black or Mixed Caribbean	-0.26	0.34	0.440
Group:BDP group x Ethnicity: Non-Black or Mixed Caribbean	0.17	0.51	0.745

Table 61: Subgroup analysis - Black or Mixed Caribbean / Non Black or Mixed Caribbean on SCQ scores at T3

Predictors	Estimates	Standard error	p-value
Intercept	14.18	1.20	< 0.001
Group: BDP group	0.14	0.52	0.787
Baseline SCQ score	0.44	0.05	< 0.001
Ethnicity: Non-Black or Mixed Caribbean	-0.14	0.39	0.726
Group:BDP group x Ethnicity: Non-Black or Mixed Caribbean	0.40	0.58	0.494

Table 62: Subgroup analysis – Baseline risk (at least one exclusion at baseline) on SCQ scores at T2

Predictors	Estimates	Standard error	p-value
Intercept	11.28	1.21	< 0.001
Group: BDP group	0.08	0.29	0.782
Baseline SCQ score	0.55	0.05	< 0.001
Baseline exclusions: 1+ incidents	-0.16	0.38	0.675
Group:BDP group x Baseline exclusions: 1+ incidents	0.70	0.54	0.193

Table 63: Subgroup analysis – Baseline risk (at least one exclusion at baseline) on SCQ scores at T3

Predictors	Estimates	Standard error	p-value
Intercept	13.41	1.45	< 0.001
Group: BDP group	0.47	0.35	0.177
Baseline SCQ score	0.47	0.06	< 0.001
Baseline exclusions: 1+ incidents	-0.04	0.47	0.931
Group:BDP group x Baseline exclusions: 1+ incidents	0.49	0.66	0.457

12 Appendix E: School administrative data sample

Table 64: Comparison of attendance data sample (n=234) and exclusion data sample (n=316) to total cohort (n=486), broken down by school.

School	Total boys in trial	Attendance sample n (%)	Total incidents sample n (%)	Duration sample n (%)
School A	77	70 (91%)	77 (100%)	77 (100%)
School B	83	0 (0%)	0 (0%)	0 (0%)
School C	80	70 (88%)	80 (100%)	80 (100%)
School D	39	37 (95%)	39 (100%)	39 (100%)
School E	40	37 (93%)	40 (100%)	40 (100%)
School F	43	0 (0%)	0 (0%)	0 (0%)
School G	44	0 (0%)	0 (0%)	0 (0%)
School H	80	20 (25%)	80 (100%)	80 (100%)
Total	486	234 (48%)	316 (65%)	316 (65%)

Table 65: Comparison of attendance data sample (n=234) and exclusion data sample (n=316) to total cohort (n=486), broken down by cohort.

Cohort	Total boys in trial	Attendance sample n (%)	Total incidents sample n (%)	Duration sample n (%)
Cohort 1	123	55 (45%)	78 (63%)	78 (63%)
Cohort 2	120	57 (48%)	79 (66%)	79 (66%)
Cohort 3	123	56 (46%)	82 (67%)	82 (67%)
Cohort 4	120	67 (56%)	79 (66%)	79 (66%)
Total	486	234 (48%)	316 (65%)	316 (65%)

13 Appendix F: Recruitment documents

This appendix sets out:

- Evaluation information sheets for parents/carers.
- Evaluation information sheets for boys.
- Interview information sheets for parent/carers.
- Interview information sheets for boys.

Future Men: Boys' Development Programme Evaluation

Information sheet for parents and carers

Summary of this document

- The Youth Endowment Fund is funding a study of the Boys' Development Programme (BDP). The BDP can help your child/the child in your care with school and make sure they are safe. The study aims to understand whether the BDP improves engagement with school and reduces the number of exclusions.
- **If you agree** that your child/the child in your care can take part in the study, they will either receive around 12 one to one sessions from a BDP project co-ordinator, **or** they will receive support and signposting from the school. This will be decided randomly.
- They will also be asked to complete a questionnaire including about how they feel and their experience of school. This will happen at the start of the study; again 12 weeks later; and a third and final time after a further 12 weeks.
- All support sessions and discussions will take place within school hours.
- We will collect information from the school on attendance, behaviour and exclusions. We will use this to write a report about the impact of the BDP. More information on how we collect and use data can be found in the privacy notice provided alongside this document.
- Taking part in the study is optional. You can choose whether or not they are involved. Both you and your child have the right to change your mind about taking part at any time. However, if you do not agree to your child/the child in your care taking part in the study they will not be able to be supported by the BDP project.
- The rest of this document gives more information about (1) the study; (2) the information we would like to collect; (3) how this information will be used; (4) confidentiality; and (5) who to contact if you have any questions.
- If you agree that your child / the child in your care can take part in the study, **please complete the consent form at the end of this document.** We will also ask your child / the child in your care to agree to take part.

An evaluation of Future Men's Boys' Development Programme

Information sheet for Parents and Carers

1. What are we doing?

We are doing a study of young people taking part in Future Men's Boys' Development Programme (BDP). This is to find out how it might help young people with their wellbeing and how they are doing in school. The study is funded by the Youth Endowment Fund (YEF). For more information see: <https://youthendowmentfund.org.uk/>.



This information sheet contains details about who we are, what we are doing, and why we are doing it. It also explains how we will use your child's/the child in your care's personal information if you agree for them to take part in this study.

2. Who are we?

This study is being organised by an independent research organisation Cordis Bright. You can find more information on Cordis Bright by visiting the website www.cordisbright.co.uk.

When we collect and use your child/the child in your care's personal information as part of the study, we are the **controllers** of the personal information. This means we decide what personal information to collect and how it is used. Contact details of team members are below.

Contact

Project Manager: Emma Andersen

Email: emmaandersen@cordisbright.co.uk Tel: 020 7330 9170



Data Protection Officer: Colin Horswell

Email: Colinhorswell@cordisbright.co.uk Tel: 020 7330 9170



3. Why has your child/the child in your care been invited to take part?

Your child/the child in your care has been asked to take part in this study because they are eligible to take part in the BDP.

4. What happens if your child/the child in your care takes part?



If you agree that the child/the child in your care can take part in the study, they will fill out a questionnaire which includes questions about their wellbeing and what they think about school. A member of BDP staff will help support your child/the child in your care to answer the questions where appropriate. The questionnaire will take around 50 minutes to complete.

Once they have answered these questions, they will then **either be**:

- 1) Offered a BDP project co-ordinator who will deliver a tailored support plan over a period of about 12 weeks.

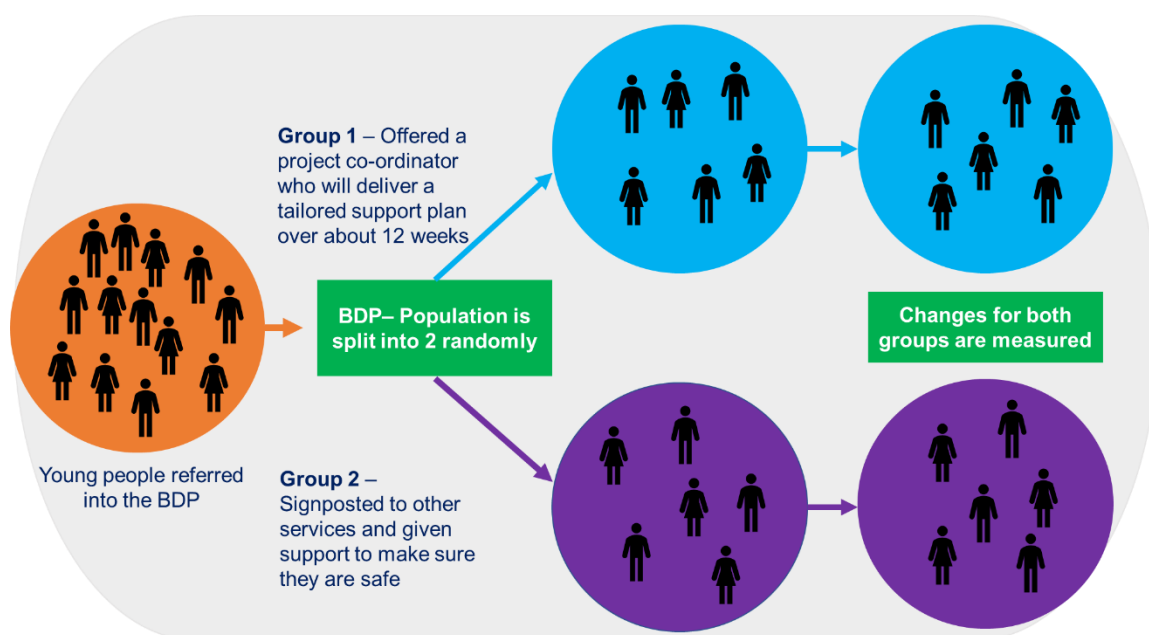
Or they will be:

- 2) Signposted to other services as appropriate and given support to make sure they are safe. This will consist of two check-in sessions (one in about 12 weeks and one in about 24 weeks' time) which will include an assessment of needs.

Whether they receive (1) or (2) will be randomly allocated. This is so that we can compare the benefits of the different support. See the picture below which explains this.

They will then be asked to fill out another questionnaire around 12 weeks and 24 weeks later to see if anything has changed. This will help us to understand the difference the support is making to young peoples' lives. These questionnaires will be administered by members of BDP staff.

If you agree that your child/the child in your care can take part in this study, we will also access records collected by the BDP. This may include information about their background and what support they have received. We may also access information collected by the school, for example on attendance.



5. Who has confirmed that this study is ethical?

This study has been reviewed and has achieved ethical approved by the Royal Holloway, University of London Research Ethics Committee. The approval ID is: REC/3756

6. Do they have to take part in the BDP study?

If you do not want your child/the child in your care to take part in the study, they do not have to. It is a decision you may want to take together.

We would like as many eligible young boys as possible to take part to improve our understanding about what makes a difference for young boys.

If your child/the child in your care chooses not to take part in the study, all the usual services will continue to be available to you. However, **the BDP will not be available** to them.

8. How do we keep your child/the child in your care safe?

Occasionally, someone may feel upset about a question or issue that arises during the study. If you or your child/the child in your care feel upset by any of the questions they are asked as part of this study, you should tell their BDP project co-ordinator, the school safeguarding lead, our study manager Emma (see box above for contact details) or our safeguarding lead Kam Kaur, who is contactable at kamkaur@cordisbright.co.uk or on 020 7330 9170).

If you or your child/the child in your care do not feel able to ask us for help, we encourage you to make contact with an external support service such as The Samaritans (Tel. 116 123, www.samaritans.org) or Childline (Tel. 0800 1111, www.childline.org.uk).

We will keep the information that your child/the child in your care shares with us confidential. However, if they tell us something that makes us think they or others might be at risk of harm we will report this to the relevant authorities. If this happens then we will try to discuss it with them first.

You can find more information in our Safeguarding Policy. See: <https://www.cordisbright.co.uk/news/safeguarding-and-protecting-children-young-people-and-adults-at-risk>

9. How will we use the personal information that we collect?

We will use the information that your child/the child in your care gives us to find out how well the BDP has worked and to write a report about our findings.



The **Privacy Notice** provided along with this information sheet provides more information about what will happen to this information after the study. This is also summarised in a picture on page 6 of this sheet.

10. What happens if you change your mind?

You and your child/the child in your care can change your minds about whether they take part in the study (and have their information sent to the YEF archive) at any time before the study comes to an end in June 2026.



To withdraw from the study, contact Emma, the Project Manager using the details provided in the box at the start of this information sheet. You do not have to give a reason and your child/the child you work with will still be allowed to take part in the BDP if they have already started work with a project co-ordinator.

If you change your mind, please tell us as soon as possible. Two weeks after the completion of the third questionnaire (at around 24 weeks) it may no longer be possible to delete the personal information we have already collected from your child/the child in your care. This is because we will have used their information and those of other participants to carry out part of our study and to write a research report. If it is too late to delete the information already collected from your child/the child in your care from the study, they can still withdraw from the rest of the study (that is, not answering any more questions) and from the YEF archive.

Once information goes into the YEF archive after June 2026 it can no longer be deleted because that would affect the quality of the archived data for use in future research.

14. Feedback, queries or complaints

If you have any questions about anything to do with the study, you can contact the:

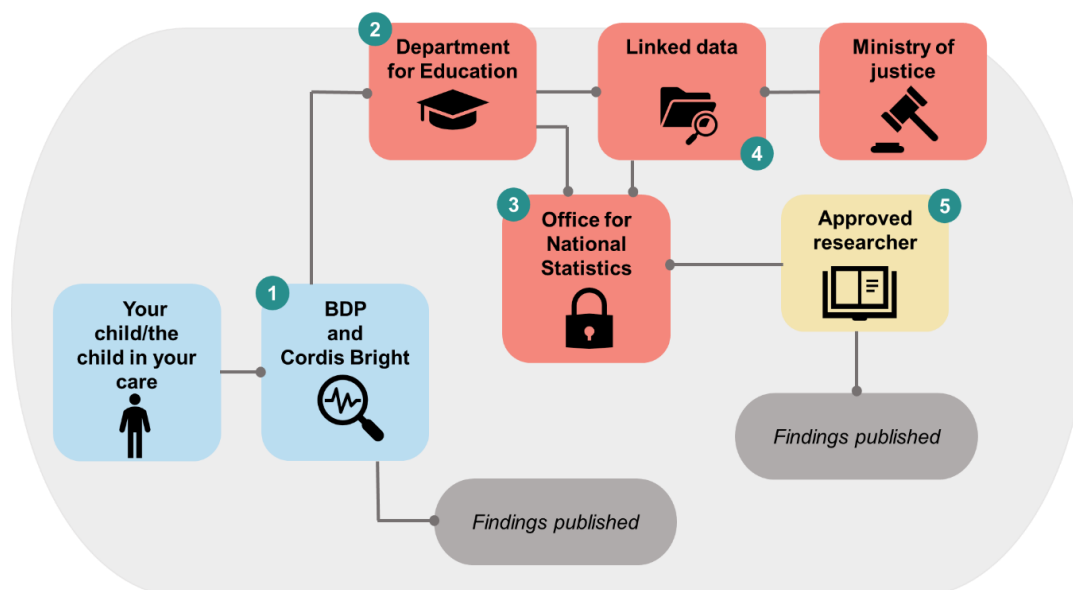


- School key contact:
- The BDP key contact:
- The Cordis Bright key contact: Emma Andersen the study project manager. Her details are in the box on the first page of this sheet.

If you have any feedback or questions about how we use personal information, or if you want to make a complaint, you can contact Colin our Data Protection Officer using the details provided in the box at the start of this information sheet.

We always encourage you to speak to us first, but if you remain unsatisfied you also have the right to make a complaint at any time to the Information Commissioner's Office (ICO), the UK supervisory authority for data protection issues: <https://ico.org.uk/make-a-complaint/> .

How information will be used



1. Information is collected from your child/the child in your care and other young people as part of the study to see if the support is helping them.
2. Personal information (like their name or date of birth) is removed from your child/the child in your care's records and they are assigned a unique identification number. After this, no one will be able to know who they are when looking at the information.
3. The information will then be held in a safe place called the YEF archive by the Office for National Statistics (ONS). No one can access it without approval.
4. The Department for Education and the Ministry of Justice will link together information on education and crime records that they hold. It will be possible to send information such as this to the ONS to safely match to your child/the child in your care's information in the YEF archive.
5. Only approved researchers will be allowed to safely access your child/the child in your care's information to see if the BDP helped people in the long term.

Confirmation statement for parents and carers on behalf of the children in their care

I confirm that: • I understand the information sheet that has been sent to me. • I have had an opportunity to ask questions about how personal information is used in the study. • I have enough information to make a decision about whether my child/the child in my care can take part in the study. • I understand that they are free to withdraw from the study at any point. • I agree my child/the child in my care can take part in the BDP and the study.	
Name of child	
Name of parent	
Date	
I agree that my child/the child in my care can take part in this study.	Yes ☐ No ☐

Evaluation of Future Men's Boys' Development Programme
Information sheet for young people

Summary of this document

- The Youth Endowment Fund is funding a study of the Boys' Development Programme (BDP).
- The BDP may help you with school and keep you safe.
- The study aims to understand how well the BDP is doing and whether it can be improved.
- **If you agree** to take part in the study, you will either receive 12 one to one sessions from a BDP project co-ordinator, or you will receive support and signposting from the school. This will be decided randomly.
- You will also be asked to complete a questionnaire about how you feel and your experience of school. This will happen at the start of the study; again 12 weeks later; and a third and final time after a further 12 weeks.
- Support sessions and filling out the questionnaires will take place during lesson time.
- The rest of this document provides more information about (1) the study; (2) what taking part would mean for you; (3) the information we will collect and how this will be used; and (4) who to contact if you have any questions.
- If you would like to take part in the study, **please complete the consent form at the end of this document.** We will also ask your parent/carers if it is okay for you to take part.

Evaluation of Future Men's Boys' Development Programme

Information sheet for young people



1. What are we doing?

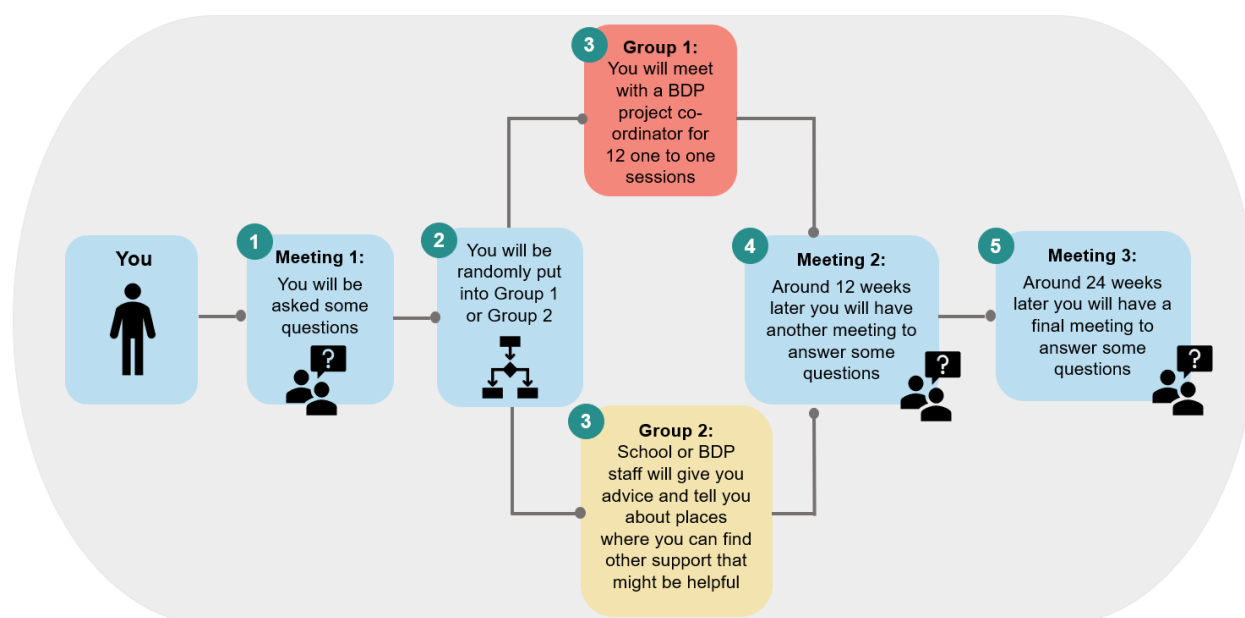
The Boys' Development Programme (BDP) is designed to help young people. It is being funded by the Youth Endowment Fund (YEF).

It may help you with school and help make sure you are safe.

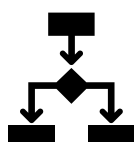
We are doing a study to see whether the BDP helps young people and how it could be improved.

2. What will you get?

The picture below shows what taking part in the research would mean for you. This is explained in more detail below.



Step 1: If you agree to take part in the study, someone from the BDP will ask you some questions about how you are feeling and what you think about school. This first meeting should take about 50 minutes.



Step 2: The study has two groups, and you will be put into either Group 1 or Group 2. This is so that we can see if there are any differences based on the support young people receive.

Step 3: What happens next depends on which group you are in:



- 3) If you are in Group 1, you will be offered a project co-ordinator who will give you 12 one-to-one support sessions to help you with school.
- 4) If you are in Group 2, you will be given advice and told about places where you may find other support that might be helpful for you. This advice will be given by a member of the BDP team and/or school staff.

Step 4: You will meet with someone from the BDP to answer questions which will ask you about how you are feeling and what you think about school. These questions will be asked around 12 weeks later.

Step 5: You will then meet with someone from the BDP one final time to answer these questions again. This will happen around 12 weeks later, at around 24 weeks following your first meeting with a BDP member of staff.

3. How will we keep you safe?

If you feel upset by any of the questions you are asked, you should tell your parent or carer, or the person from the BDP that you are working with, or a member of school staff.

The answers you give to the questions will be kept secret unless we think that you or someone else might be at risk of harm. If this happens then we will try to talk to you first about why we want to tell another person or organisation about what you told us.

4. What information will we collect?

If you agree to take part in the study, we will collect the answers you give to the questions about how you feel and what you think about school.



The BDP and your school will also give us some information about the support you receive and other information like your name and date of birth, and how often you go to school.

5. How will we use your information?

We will use the information you and other young people give us to find out how much the support has helped people.



We will write a report about what we find. The report will not include your name or any other information that could identify you.

The report will go on to the YEF's website and anyone will be able to read it. We might also put it on our website or in articles and presentations.

6. How we comply with the law

We will only use your information if the law says it's ok. Because this study is interesting and important to lots of people, the law says we can use your information.



We will always keep your information safe. During the study, we only let our research team look at your information.

7. Your legal rights

The law gives you rights over how we can use your information. You can find full details of these rights in the information sheet the BDP practitioner has given to your parent or carer and in YEF's archive privacy notice: <https://youthendowmentfund.org.uk/wp-content/uploads/2021/07/YEF-Data-Guidance-Participants.pdf>.

8. How long we keep your information for

After we have finished the study, we will take all names and other personal details out of the information held by Cordis Bright so no one will be able to know who took part in the study.

We will keep this information for six years after we have finished the final report.

9. What we do with your information once the research is over

When we finish the study, we'll give your information to YEF and they will become the 'controller' of it.

They will keep your information in a safe place called the YEF archive. This is held by the Office for National Statistics (ONS).

Information will be kept safely in the YEF archive for as long as it is needed for future research.

The picture on page 6 explains more about what will happen to your information. If you have any questions, you can ask the person from the BDP who is talking to you about this.

You can also see more information in the Privacy Notice that has been given to your parent/the person who cares for you.

10. Do you want to take part?

You can decide whether or not you want to take part in the BDP study – it is up to you. If you do not want to take part then that is okay. You can still get all the support you would normally receive. However, you won't be able to take part in the BDP.



If you do not want to take part in the study, you can tell your parent or guardian, or the person from the BDP you are working with. We will also talk to your parent or the person who cares for you, so they know we have asked you about this. We will also ask for their permission to let you take part.

11. What happens if you change your mind?

You can change your mind about taking part in the study at any time. If you change your mind, tell your parent or guardian, or contact Emma from the research team.

Once your information goes into the YEF archive it can't be deleted because it needs to be used for future research.

12. Who can you ask questions to?

If you have any questions, you can ask the person from the BDP who is talking to you about this.

You can also contact Emma from the research team, or Colin who is our Data Protection Officer using the contact details below.

Contact

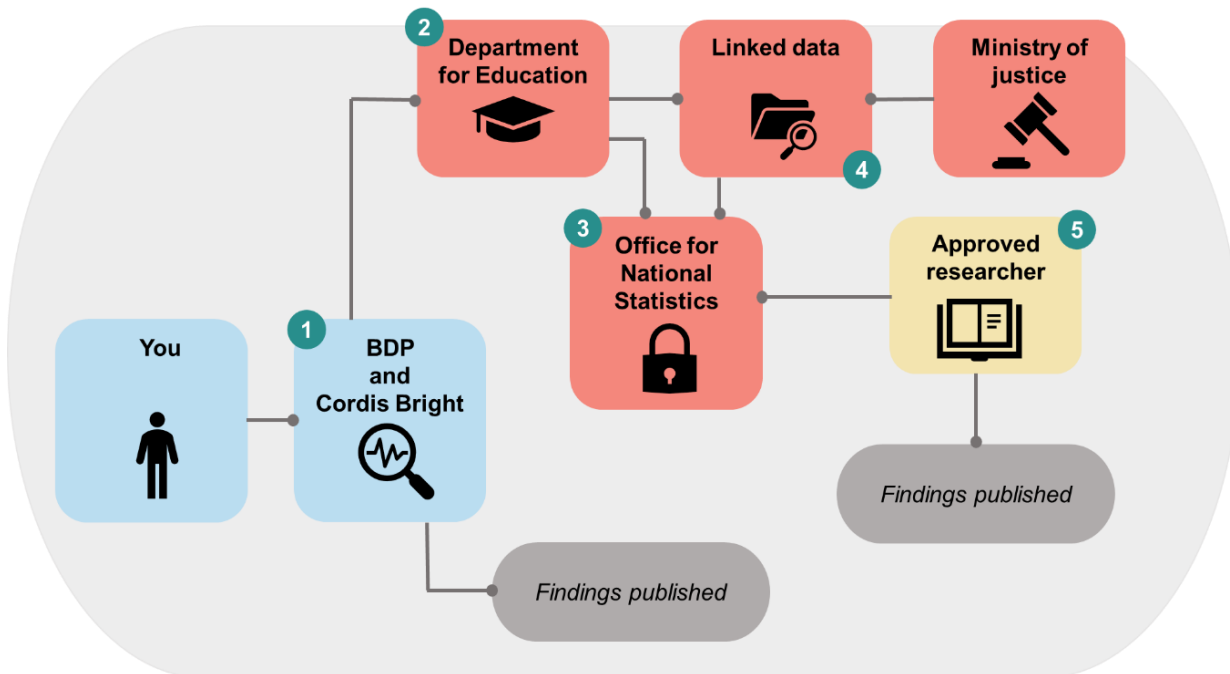
Project Manager: Emma Andersen, Email: emmaandersen@cordisbright.co.uk Tel: 020 7330 9170

Data Protection Officer: Colin Horswell, Email: colinhorswell@cordisbright.co.uk Tel: 020 7330 9170



If you have any questions or complaints please ask Emma or Colin. You also have the right to make a complaint to the Information Commissioner's Office (ICO). You can do this here <https://ico.org.uk/make-a-complaint/>

How your information will be used



6. Information is collected from you and other young people as part of the study to see if the BDP is helping you.
7. Personal information (like your name or date of birth) is removed from your records. After this, no one will be able to know who you are when looking at the information.
8. The information will then be held in a safe place called the YEF archive by the Office for National Statistics (ONS). No one can access it without approval.
9. The Department for Education and the Ministry of Justice will link together information on education and crime records that they hold. It will be possible to send information such as this to the ONS to safely match to your child/the child in your care's information in the YEF archive.
10. Only researchers that YEF work with will be allowed to safely access your information to see if the BDP has helped people.

Agreement statement for young people to take part in the evaluation of the Boys' Development Programme

I confirm that:

- I understand the information sheet.
- I have had an opportunity to ask questions.
- I have enough information to make a decision about taking part in the study.
- I understand that I am free to withdraw from the study at any point.
- I confirm that I am happy to take part in the study.

Name of young person

Date

I agree to take part in the study

Yes ☐ No ☐

Future Men: Boys' Development Programme Evaluation

Privacy Notice

Summary of this document

- The Youth Endowment Fund is funding a study of Future Men's Boys' Development Programme (BDP). The study is being conducted by Cordis Bright.
- The study will gather information to help researchers understand the long-term impact of the BDP. For example, whether it increases young people's engagement with school.
- As part of this study, we will ask young people questions using a survey at three points in time (on entrance to the programme, 12 weeks later, and 24 weeks later). We will also collect personal data (such as name, ethnicity, gender and age), and information from schools on attendance, behaviour and exclusions.
- Cordis Bright will store all information securely in line with the General Data Protection Regulation (GDPR). They will use this information to write a report on the impact that the BDP has had. They will delete all identifiable data once the study has been completed.
- Once the study is completed, identifiable information will be sent to the Department for Education. They will replace all personal information (for example, name, age, date of birth, home address) with an identification number. After this **all young people will be anonymous**, i.e. it will not be possible to identify them.
- Once all the personal information has been removed, the information will be stored electronically in the Youth Endowment Fund archive in the Office for National Statistics Secure Research Service.
- Only approved researchers can access the information to understand the long-term impact of the BDP. They will not know who took part in the study and will not have any access to personal information.
- You have the right to access the information we hold about your child/the child in your care, and to ask us to correct information which is incorrect. In some circumstances, you can also ask us to delete this information.
- The rest of this document contains more information about (1) the study; (2) the protection of personal information; (3) your data protection rights; and (4) who to contact if you have any questions, feedback or complaints.

Future Men: Boys' Development Programme Evaluation

Privacy Notice

1. What is the Boys' Development Programme and what is the study about?

We are carrying out a study of young people taking part in Future Men's Boys' Development Programme (BDP) to try to find out how the programme might help young people in the future. The study is being funded by the Youth Endowment Fund (YEF) – see www.youthendowmentfund.org.uk for more information.

This study is being organised by an independent research organisation Cordis Bright. You can find more information on Cordis Bright by visiting the website www.cordisbright.co.uk.

When we collect and use participants' personal information as part of the study, we are the **controllers** of the personal information, which means we decide what personal information to collect and how it is used.

This Privacy Notice explains how we will use and protect the personal information we collect from your child/the child you care for. Key research team members contact details are below.

Contact details:

Project Manager: Emma Andersen

Email: emmaandersen@cordisbright.co.uk Tel: 020 7330 9170



Data Protection Officer: Colin Horswell

Email: Colinhorswell@cordisbright.co.uk Tel: 020 7330 9170



2. How will we use the personal information that we collect?

Data protection laws require us to have a valid reason to use your child's/the child in your care's personal information. This is referred to as our 'lawful basis'. We rely on the public task lawful basis to use their personal information. This means we will only use more sensitive information (such as information about their wellbeing, ethnic background, school attendance, or any criminal offence information) if it is necessary for research purposes which are in the public interest.



We will use the information that your child/the child in your care gives us to find out how well the BDP has worked and to write reports about our findings.

The reports will not contain any personal information about your child/the child in your care and no one will be able to identify them from the reports. The reports will be published on the YEF's website and we might also use the reports on our website. We may also include findings from the reports in articles that we write or in presentations.

Any personal information that your child/the child you care for gives us will be stored securely and kept secret.

The only time we may share this personal information with another person or organisation is if your child/the child in your care says something that makes us concerned about them or about someone else.

Our Safeguarding Policy (See: <https://www.cordisbright.co.uk/news/safeguarding-and-protecting-children-young-people-and-adults-at-risk>) has more information about steps that we might take if this happens.

3. What happens to personal information after the study?

Once we have finished the study, we will do the following:

- Create and submit two separate datasets to the DfE and the ONS. The first dataset contains only identifying information and a unique project specific reference number for each child. The DfE will replace all information that could identify the young people (their name, gender, date of birth, home address) with an identification number³². Once this has been done, it is no longer possible to identify any individual young person from the study data. This process is called **pseudonymisation**. The DfE will transfer the pseudonymised information to the YEF archive³³.
- We will create and submit a second dataset directly to the ONS. This will contain all the evaluation data and the project specific reference numbers. This is submitted directly to the ONS, where it is also stored in the YEF archive. All young people will be **anonymous and non-identifiable** in this dataset.
- Once information is transferred to the DfE and the ONS, we hand over control to YEF for protecting your personal information. YEF is the ‘controller’ of the information in the YEF archive. By maintaining the archive and allowing approved researchers to access the information in the archive, the YEF is performing a task in the public interest, and this gives the YEF a lawful basis to use personal information.
- Information in the YEF archive can only be used by approved researchers to explore whether the BDP, and other programmes funded by YEF, had an impact over a longer period of time. Using the unique identification numbers added to the data by the DfE it will be possible to link the records held in the YEF archive to other information held by the Department for Education and Ministry of Justice. This will help approved researchers to understand the long-term impact of the BDP because they can find out, for example, whether it reduces a child’s likelihood of being excluded from school or becoming involved in criminal activity.

4. How will we protect your child/the child in your care’s information?



We will do a number of things to protect your child’s/the child in your care’s personal information during the study, including:

- Limiting access to a few researchers who need the information to conduct the study.
- Keeping personal details such as name and address separate from all other data and linking these using a unique number.
- Keeping information on a secure safe server and making sure information is regularly backed up so it is not lost.

³² The young person’s unique Pupil Matching Reference number in the DfE’s National Pupil Database.

³³ The YEF archive is stored safely in the Office for National Statistics Secure Research Service.

We will not transfer personal data outside the UK.



5. How is information in the YEF archive protected?

The YEF has strong measures to protect the information in their archive. The YEF archive is protected by the Office for National Statistics' 'Five Safes' framework. The information can only be accessed by YEF approved researchers in safe settings and there are strict rules about how the information can be used. All proposals must be approved by an ethics panel. Information in the YEF archive cannot be used by the police or the Home Office for immigration enforcement purposes.

You can find more information about the YEF archive and the Five Safes on the YEF's website <https://youthendowmentfund.org.uk/evaluation-data-archive/>. YEF's data archive privacy statement is also available here: <https://youthendowmentfund.org.uk/wp-content/uploads/2021/07/YEF-Data-Guidance-Participants.pdf>. We encourage all young people, parents and carers to read the YEF's guidance for participants before deciding to take part in this study.



6. How long will the information be kept for?

After we have given the information to YEF, we will take all names and other personal details out of the dataset held by Cordis Bright so no one will be able to know who took part in the study. We will keep this information for six years after we have finished the report.

The YEF will keep information in the YEF archive for as long as it is needed for research purposes. This is allowed under data protection laws because it is in the public interest. The YEF will carry out a review every five years to see whether it is likely that the data will be used for future research and to see whether it still makes sense to keep the information in the archive.

7. What are your data protection rights?

You/You and your child/ the child in your care have the right to:

- Ask for access to the personal information that we hold about them.
- Ask us to correct any personal information that we hold about them which is incorrect, incomplete or inaccurate.

In certain circumstances, you also have the right to:

- Ask us to erase the personal information where there is no good reason for us continuing to hold it – please read the information in section 7 below about the time limits for requesting deletion of personal information;
- Object to us using the personal information for public task purposes;
- Ask us to restrict or suspend the use of the personal information, for example, if you want us to establish its accuracy or our reasons for using it.

If you would like to do any of the above during the study period, please contact Emma our Project Manager, or Colin our Data Protection Officer using the details provided earlier. We

will usually respond within one month of receiving your request.

If you would like to do any of the above after the study has finished, please contact the YEF. Further information and their contact details are available in YEF's here:

<https://res.cloudinary.com/yef/images/v1625734531/cdn/YEF-Data-Guidance-Participants/YEF-Data-Guidance-Participants.pdf> .

If you ask us to do any of the above, we may need to ask for more information to help us confirm the identity of your child/the child you care for. This makes sure that personal information is not shared with a person who has no right to receive it. We may also ask you for more information to make sure we can respond more quickly.

8. Time limits

If you decide that you would like us to delete your child/the child in your care's information from the study, you should tell us as soon as possible.

After two weeks following completion of the second set of questions (at around 6 months – see the information sheet for parents/carers for more information) it might no longer be possible to delete the personal information we have already collected from your child/the child you care for. This is because we might have used their information, along with all of the information we have gathered from the other participants, to carry out part of our study and to write a report.

Once information goes into the YEF archive after June 2026 we can no longer delete it. You will need to apply to the YEF (see contact details in Section 6 above), who will review applications for deletion on an individual basis.

9. Other privacy information



Categories of personal information we will collect may include:

- First name
- Surname
- Date of Birth
- Home address
- Alternative address (if appropriate)
- Telephone number
- Email address



10. What personal information will be shared?

We only ever use your child's/the child in your care's personal information if we are satisfied that it is lawful and fair to do so. Section 3 above explains how we share data with the Department for Education and the YEF.

We may also share personal information with the police and local authority education teams so that they can tell us what information they have about the young person from the year before they took part in the study and in the year after they agreed to take part in the study. We will not transfer your personal data outside the UK.



11. What if I have any questions, feedback, or complaints?

If you have any feedback or questions about how we use personal information, or if you want to make a complaint, you can contact Emma, the Project Manager or Colin, the Data Protection Officer using the details provided above. We always encourage you to speak to us first, but if you remain unsatisfied you also have the right to make a complaint at any time to the Information Commissioner's Office (ICO), the UK supervisory authority for data protection issues: <https://ico.org.uk/make-a-complaint/> .

Evaluation of Future Men's Boys' Development Programme

Interview information sheet for parents and carers



Summary of this document

- The Youth Endowment Fund is funding a study of the Boys' Development Programme (BDP). The study aims to understand whether the BDP improves engagement with school and reduces the risk of exclusion.
- As part of this study, we would like to interview your child/the child in your care. This is because they are working with or have worked with a BDP project co-ordinator in the past.
- They will be asked about their experience of working with the BDP, including what they liked, what could be improved, and whether it made a difference for them.
- The interview will take 30 minutes and will happen within school hours.
- Taking part in the interview is optional. You can choose whether or not you would like your child/the child in your care to take part. Both you and your child/the child in your care can change their mind about taking part at any time.
- Everything that is said in the interview will be kept secret between your child/the child in your care and the researcher. The only exception is if they say something which makes us think they are at risk of harm. We would then need to report this to the relevant authorities.
- The rest of this document gives more information about (1) the interview; (2) what would happen; (3) confidentiality; and (4) who to contact if you have any questions.
- If you agree that your child/the child in your care can take part in the interview, [please complete the consent form at the end of this document.](#) We will also ask your child/the child in your care to agree to take part.

Evaluation of Future Men's Boys' Development Programme

Interview information sheet for parents and carers



What are we doing?

The Boys' Development Programme (BDP) is designed to help young people with their wellbeing, relationships and behaviours, and to improve their educational engagement outcomes, such as reducing the likelihood of exclusion.

We are doing a study to see whether the BDP helps young people, how it may do this and how it could be improved.

The study is being funded by the Youth Endowment Fund (YEF), for more information see: <https://youthendowmentfund.org.uk/>.

As part of the study, we would like to talk to young people who are working or have worked in the past with a BDP project co-ordinator.

You can choose whether or not you would like your child/the child in your care to be involved. You may discuss anything in this form with other people.

We will also talk about this with your child/the child in your care. They also need to agree to take part in the discussion.

Who are we?

We are part of a research organisation called Cordis Bright. If your child/the child in your care takes part, they will talk to one of the researchers called Emma, Camilla, Keiran or Karim.



Emma Andersen, Email: emmaandersen@cordisbright.co.uk

Tel: 07849 089 226



Camilla Antrobus, Email: info@antrobus.com

Tel: 07976 477 946



Keiran Manners, Email: keiran@iscre.org.uk

Tel: 07751 705 939



Karim Bukleb, Email: karimbukleb@cordisbright.co.uk

Tel: 07749 418 873



What would happen if they take part?

If you and your child/the child in your care agree that they can take part, they will talk to one of the researchers either in person or online via video or phone call.



The interview will take around 30 minutes.

A Future Men member of staff will also be around to help if they need them. Your child / the child in your care can also choose whether a member of the Future Men team sits with them when they speak to Emma, Camilla, Keiran or Karim.

They will be offered a £20 voucher for taking part.

Does my child/the child I care for have to take part?

If you decide that you do not want your child/the child in your care to take part they do not have to. It is a decision you may want to take together.



If your child/the child in your care does not take part, they will still get all the support they would normally have from their Future Men project co-ordinator.

Who is going to know about this?

The only people who will know that your child/the child in your care is involved in the research is you, the child, staff from the BDP, school staff and the researchers from Cordis Bright.

If your child meets with a researcher, only the researcher (and a member of the Future Men team, if they choose to have them in the room) will know what they say.



The answers your child/the child in your care give will be kept confidential. However, if they say something that makes us concerned about them or others being at risk of harm, we will report this to the relevant authorities. If this happens then we will try to discuss the issue with them first.

We will always keep information about your child/the child in your care safe. During the study, we only let our research team look at their information. After we have finished the research, we will delete any personal information.

You can find more information in our Safeguarding Policy, see:

<https://www.cordisbright.co.uk/news/safeguarding-and-protecting-children-young-people-and-adults-at-risk>

What will happen afterwards?

After we have spoken with your child/the child in your care, we will use the information they tell us to find out how well the BDP has helped people. We will write a report about what we find. The report will not include their name or any other information that could identify them.



The report will go on to the Youth Endowment Fund's website which are funding the research and the BDP project and anyone will be able to read it. We might also put it on our website or in articles and presentations.

What happens next?

If you are happy for your child/the child in your care to talk with a researcher from Cordis Bright, **please fill in the consent form at the end of this document.**



What happens if I change my mind?

You can change your mind about whether you are happy for your child/the child in your care to speak with us at any time. You can tell their BDP project co-ordinator or Emma the Project Manager if you change your mind. Emma's contact details are on the first page of this information sheet.

If your child/the child in your care changes their mind part way through talking with one of the researchers and they want to stop, that is also fine. They can tell the researcher and the researcher will delete any notes they have taken.

Who can I talk to or ask question to?

If you have any questions then please ask Emma Andersen, the evaluation project manager at emmaandersen@cordisbright.co.uk or 020 7330 9173, or your child's/the child in your care's BDP project co-ordinator.



Evaluation of Future Men's Boys' Development Programme

Consent form to take part in a research conversation



I confirm that:

- I understand the information in this document.
- I have had an opportunity to ask questions about what this interview will involve and how their information will be used.
- I have enough information to make a decision about whether my child/the child in my care can take part in the interview.
- I understand that they are free to withdraw from the interview at any point.

Name of child	
Name of parent	
Date	
I agree my child/the child in my care can take part in this interview.	Yes ☐ No ☐

If your child/the child you care for has any accessibility requirements, please let us know or talk to your child's Future Men worker:

This may include (but is not limited to) receiving questions in advance, additional breaks, physical or sensory adjustments to the room, interpreters or language support, providing alternative response formats, or asking for a familiar adult or support worker to be present.

Please return this form to your child's Future Men worker.

Thank you!

Evaluation of Future Men's Boys' Development Programme

Interview information sheet for young people

Summary of this document

- The Youth Endowment Fund is funding a study of the Boys' Development Programme (BDP). The study aims to understand whether the BDP helps young people with school and how it can be improved.
- As part of this study, the researchers who are doing the study would like to ask about your experience of working with the BDP.
- We would like to ask you about what you liked, what you think could be improved, and whether you think the BDP has made a difference for you.
- If you agree to take part, the conversation would take around 30 minutes, either over the phone or in person.
- The conversation would happen during school hours. You can choose if a member of the Future Men team is in the room with you or not.
- You will receive a £20 voucher as a thank you.
- Taking part in the interview is optional. You can choose whether or not you would like to take part and you can change your mind at any time. We will also ask your parent/carer if they agree you can take part.
- Everything you say will be kept secret between you and the researcher. The only exception is if you say something which makes us think you or someone else is at risk of harm.
- The rest of this document gives more information about (1) the interview; (2) what would happen if you agree to take part; (3) confidentiality; and (4) who to contact if you have any questions.
- If you would like to take part, **please complete the consent form at the end of this document.** We will also ask your parent/carer if it is okay for you to take part.

Evaluation of Future Men's Boys' Development Programme

Interview information sheet and consent form for young people



What are we doing?

We would like to talk with you and ask some questions about your work with your Future Men worker including what you think about the BDP and whether or not it has made a difference to you.

We will also talk about this with your parent or the person who cares for you. They also need to agree for you to take part in the discussion.

Who are we?

We are a research organisation called Cordis Bright which is working closely with the BDP. If you agree to take part, one of the researchers called Emma, Camilla, Keiran or Karim will talk to you.

Emma Andersen, Email: emmaandersen@cordisbright.co.uk

Tel: 07849 089 226



Camilla Antrobus, Email: info@antrobus.com

Tel: 07976 477 946



Keiran Manners, Email: keiran@iscre.org.uk

Tel: 07751 705 939



Karim Bukleb, Email: karimbukleb@cordisbright.co.uk

Tel: 07749 418 873



What would happen if I take part?

If you agree to take part, you will talk to either Emma, Camilla, Keiran or Karim either in person or virtually via video call or telephone call. The interview will take around 30 minutes.



A Future Men team member will also be around to help if you need them. You will also be able to choose whether they sit with you while you talk to either Emma, Camilla, Keiran or Karim.

You will be offered a £20 voucher for taking part.

Do I have to take part?

You do not have to talk with us if you don't want to – it is up to you. If you decide not to talk with us, you can still get all the support you would normally have from your project co-ordinator.



You can talk about anything in this form with anyone you would like to speak to. You can decide whether you would like to take part or not after you have talked it over. You do not have to decide straight away.

Who is going to know about this?

The only people who will know that you have taken part are you, your parent or guardian, staff from Future Men, school staff and the researchers from Cordis Bright. The answers you give will be kept secret unless we think that you or someone else might be at risk of harm. If this happens, we will tell the relevant authorities but we would try to discuss this with you before doing so.



What will happen afterwards?

After we have spoken with you, we will use the information you and other young people tell us to find out how well the BDP has helped young people. We will write a report about what we find. The report will not include your name or any other information that could identify you.



The report will go on to the Youth Endowment Fund's website who are funding the study and anyone will be able to read it. We might also put it on our website or in articles and presentations.

What happens next?

If you are happy to talk with us, please fill in the consent form. Your Future Men worker can help you with this. Please give the consent form to your project co-ordinator.



If you do not want to take part, then that is OK. You will still be able to work with your Future Men worker.

What happens if I change my mind?

You can change your mind about talking with us at any time. You can tell your Future Men worker. if you change your mind.

If you change your mind part way through talking with one of the researchers and you want to stop, that's also fine. You can tell the researcher and they will delete any notes they have taken.



Who can I talk to or ask question to?

If you have any questions, then please ask your Future Men worker. You could also email Emma, who is one of the Cordis Bright researchers. Her email address is emmaandersen@cordisbright.co.uk.

Evaluation of Future Men's Boys' Development Programme

Consent form to take part in a research conversation



I confirm that:	
• I understand the information sheet. • I have had an opportunity to ask questions about the interview. • I have enough information to make a decision about taking part in the interview. • I understand that I can change my mind at any point.	
Name of young person	
Date	
I agree to take part in the interview	Yes ☐ No ☐

If you have any accessibility requirements, please let us know or talk to your Future Men worker.

This may include (but is not limited to) receiving questions in advance, additional breaks, physical or sensory adjustments to the room, interpreters or language support, providing alternative response formats, or asking for a familiar adult or support worker to be present.

--

Please return this form to your Future Men worker.

Thank you!

14 Appendix G: Topic guides

This appendix sets out:

- Topic guide for boys.
- Topic guide for the Future Men team.
- Topic guide for school stakeholders.

Topic guide for boys

Note for researchers:

We will be interviewing both young people in the treatment group who have completed the BDP and who have dropped out. Before starting the interview, please confirm:

- How much of the BDP they have completed, and amend the tense and questions as needed.
- The name of their Future Men worker, and amend questions to refer to them by name throughout.

Thank you!

Introduction

Thank for you agreeing to speak to us today. We would like to ask you some questions about your work with your Future Men worker, including what you think about the BDP and whether or not it has made a difference to you.

The only people who will know that you have spoken to us is you, your parent or guardian, staff from Future Men and school staff who have helped to organise the conversation, and the researchers from Cordis Bright. We won't tell anyone what you say unless we think that you or someone else might be at risk of harm. If this happens, we will tell the relevant authorities, but we would try to discuss this with you before doing so.

After we have spoken with you, we will use the information you tell us to find out how well the Boys' Development Programme has helped young people. We will write a report about what we find. The report will not include your name or any other information that could identify you.

You don't have to answer and questions you don't want to, and we can stop the interview at any time. If you have any questions as we go through the interview, please do ask.

Does that sound OK to you? Thank you again for taking part.

Questions

About you

1. To start, I'd like to hear a little bit about you. Can you tell me about yourself?

Prompts: What do you like doing in your free time? What's your favourite subject at school at the moment?

[Note to interviewer: choose an appropriate ice breaker based on circumstances – i.e. avoid home life if this is a source of difficulty]

Support from the BDP

I'd now like to talk to you about when you first heard about the BDP (stage 1 on the map)

2. How did you first hear about the BDP? Who talked to you about it first, and how did that go?

3. What made you want to get involved with the BDP? Was the information you got about the BDP and what the sessions were going to involve easy to understand?
4. Could anything about getting involved with the BDP been improved? Could anything have been made easier?

Prompts: More/less information needed beforehand?

Great, thank you. Now let's talk about when you started working with your Future Men worker (stage 2 on the map).

5. How well do you get on with your Future Men worker? Why do you say this?

Prompts: Are they someone you can turn to for help? Do you trust them? Do you feel you can be honest with them about how you are feeling?

6. How many sessions have you had with your Future Men worker? What type of topics have you covered in these sessions?

Topics may include: boys development and masculinity, emotions, communication, conflict, goals & aspirations

7. How did you find these sessions? What went well? What could have been improved?

Prompts: Fun/boring, informative, stressful/calming, helpful/not helpful, etc. Would you have liked learning in a different way e.g. less reading, information explained through videos etc?

Note for researcher: The following questions are only for the young people who dropped out of the BDP.

8. If I've understood correctly, you stopped working with your BDP project co-ordinator. Can you tell me a bit about why you decided to stop the BDP?

Prompts: Clashing with important or fun lessons, need for alternative support, didn't like the programme, receiving support from other services, too time intensive

9. Is there anything that your Future Men worker, or anybody else, could have done to encourage you to keep taking part in the BDP?

Prompts: Different topics for sessions, scheduling flexibility, spending more/less time together

Impacts of the BDP

We would like to understand a bit more about the difference that support from your Future Men worker may have made for you (stage 3 of the map)

10. What were you trying to achieve during your time with your Future Men worker?

Prompts: Did you have any goals when you started? What were you hoping to get out of it? E.g. help with school, help with managing emotions, thinking about the future?

11. Have you noticed any differences since you started working with your Future Men worker? If yes, do you have any examples? If no, why do you think this is?

12. *[If these have not been covered above]* Have you noticed any differences in the following areas?

- a. Your emotions? *Prompts: Do you find managing your emotions easier/harder? Do you have a better understanding of how to manage difficult emotions?*
- b. How you communicate with other people? *Prompts: Do you feel able to better discuss your emotions with other people? If you disagree with someone, do you feel that you have strategies to discuss this calmly? Do you have a better understanding of how communication influences relationships?*
- c. How you're getting on in school? *Prompts: Are you enjoying school more? Are you more engaged in school? Do you get along better with your teachers? Are you getting into trouble at school less/more?*
- d. Your friendships? *Prompts: Are you getting on better/worse with friends? Do you find it easier to deal with disagreements with friends? Do you find it easier/harder to make new friends?*

13. Are there any sessions you did with your Future Men worker that you feel were most useful in helping you make changes?

Prompts: conflict management, communication, managing emotions, boys development & masculinity, goals & aspirations?

14. Is there anything else that you think has helped make these changes?

Prompts: Has having someone to talk to like your project co-ordinator helped? Have they encouraged you to make these changes in any way? Are there any particular qualities your project co-ordinator has that you think have been helpful?

15. Is there anything else you would like to have learnt or learnt more about during your time with your project co-ordinator?

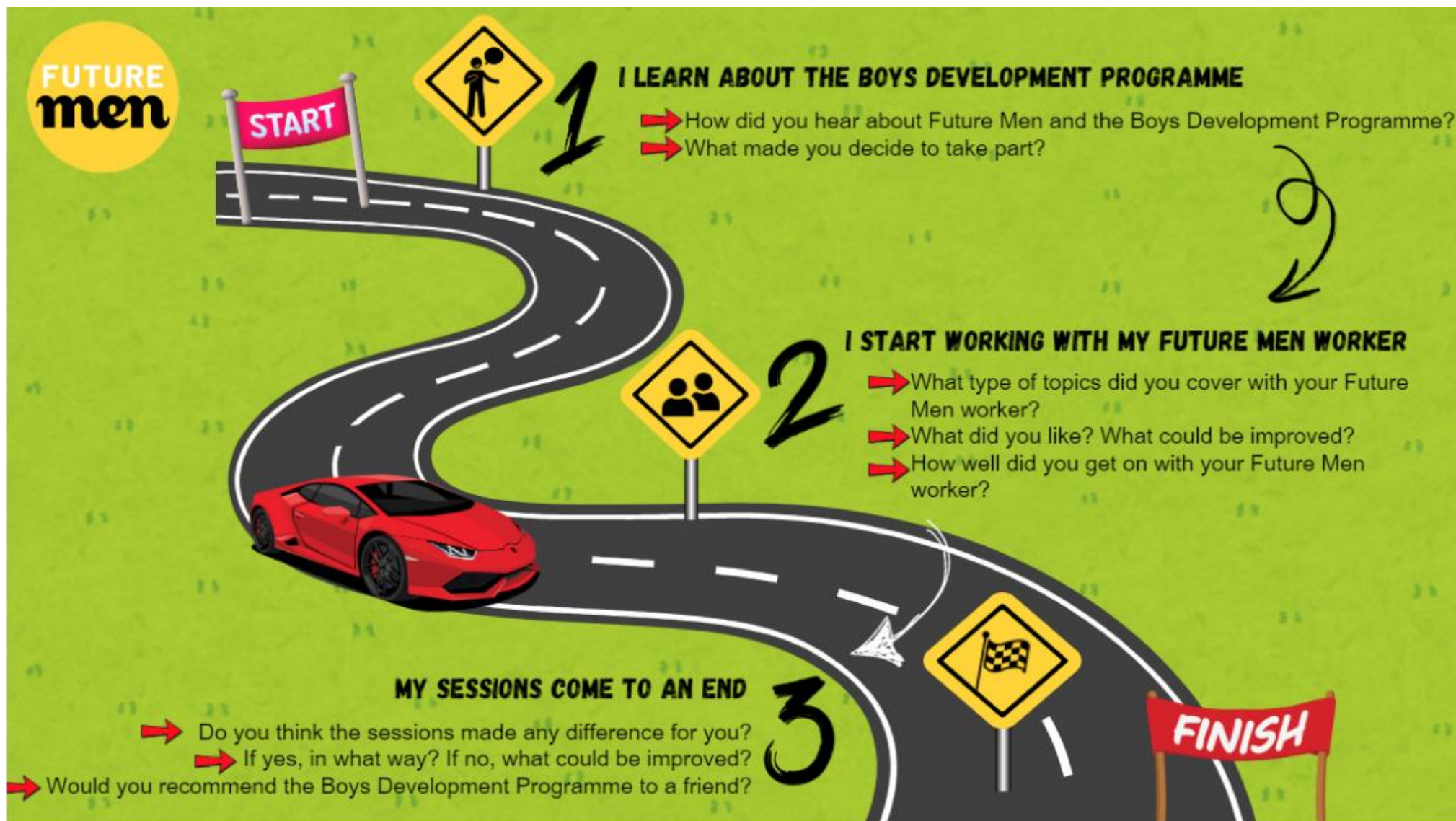
Prompts: Help with school work, relationship skills, concentration skills, anger management skills, etc.

Wrapping up

16. Finally, would you recommend working with your Future Men worker to a friend? Why do you say this?

17. Do you have anything else that you would like to tell us?

Thank you for chatting with me today and for your help.



Topic guide for the Future Men team

Introduction

Thank you for agreeing to take part in this interview, which should take about an hour.

As you may know, Cordis Bright, an independent research organisation, has been asked by the Youth Endowment Fund to conduct an independent evaluation of Future Men's Boy's Development programme (BDP) in South London.

The aim of the evaluation is to see whether the BDP improves educational outcomes in comparison to young people who receive "business as usual" services.

The aim of this interview is to inform our understanding of how effective the implementation of the BDP has been, any challenges and solutions that have arisen, and any impact the programme has had on young people.

What you say in this interview will remain confidential, unless you mention anything which raises safeguarding concerns, in which case we would have to pass this on to the relevant authorities. However, we would try and speak to you about this first. We will not attribute your views to you in any report that we produce based on this interview.

Are you happy for us to record the conversation to help us with our note-taking?

If you have any questions as we go through the interview, please do ask. Thank you again for your time and taking part in this interview.

Questions

About you

1. Please can you confirm your name, role and relationship to the BDP?

Note to researcher: We should already have this information so just confirm it with the interviewee.

The BDP model

The next few questions are designed to get an understanding of your views about the context and rationale behind the BDP model, and how young people are referred into the programme.

Context and rationale

2. What is the rationale for the BDP, and why is it needed in South London? What gap is it seeking to address?
3. Is there local demand for the programme? Why do you say this? Does this demand vary across London?

Referrals and eligibility criteria

4. Please can you describe who the BDP aims to work with?

Prompts: Boys in Years 7-11 at risk of:

- a) Exclusion (due to poor behaviour records and/or significant behaviour incidents)*
- b) Disengagement (achieving below potential and/or poor attendance)*
- c) Poor outcomes due to known adversity outside school (child-poverty, previous or ongoing child-protection concerns)*

5. Do you think that the BDP is successfully reaching these young people? Why do you say this?
- a. Do referral rates vary across the different schools that the BDP is working in? If yes, why?
 - b. Are there any other groups who you think would benefit from the BDP who are not currently being reached? Why do you think this?
 - c. How well does the BDP reach young people from minoritised and marginalised backgrounds, for example from ethnic minority backgrounds? If well, how does it achieve this? If it could be better, what actions are needed?
6. What do you think has worked well helping the BDP reach its intended audience? What could have been improved?
7. How effective are identification, referral and assessment pathways into and through the BDP? What is working well and what could be improved?

Delivery and implementation of the BDP

I'd now like to talk to you about delivering the BDP to young people.

8. Please could you describe the types of support that are delivered as part of the BDP?

Prompts: How many sessions do boys typically receive? What topics are covered and how are these topics selected? Does each session follow a similar structure?

9. If you are involved in delivering 1:1 support to young people, how closely do you follow the BDP session slides? Which aspects of the session slides are working well and which aspects could be improved?

Prompts: Have the types of topics covered, sequencing or length of support varied significantly from the BDP slides/plan? If so how and why?

10. How well has support been implemented in schools? What's working well and what could be improved? Why do you say this?
11. Have there been any key factors that have supported the effective delivery of the BDP? For example, at:
- a. An organisational level (i.e. staffing, training)?
 - b. A school level (i.e. school policies, school capacity to host the BDP)?

- c. A local or community level (i.e. level of need, readiness for change, existing referral pathways, partnership working)?
- d. A national level (i.e. YEF, government, funding, etc.)?

12. Have there been any challenges or barriers to the effective delivery of the BDP? For example, at:

- a. An organisational level (i.e. staffing, training)?
- b. A school level (i.e. school policies, school capacity to host the BDP)?
- c. A local or community level (i.e. level of need, readiness for change, existing referral pathways, partnership working)?
- d. A national level (i.e. YEF, government, funding, etc.)?

If so, how were these addressed?

13. In general, how well do you think the BDP manages to engage young people? What has worked well? What could be improved in engaging young people?

Prompts: Are there any aspects of the BDP which have been effective in engaging young people? Are some groups of young people more engaged than others? If so, why do you think this is?

14. Have any young people dropped out of the BDP during the programme? If yes, why do you think this?

Prompt: Is there anything that you think could support young people to stay engaged?

Impact of the BDP

The next set of questions focus on the impact you think the BDP may or may not be having.

15. From your perspective, what impact is the BDP having on young people? Do you have any examples you can share?

Prompts: To what extent do you think the BDP is achieving any impact on the following. Why or why not?

- *Trusted relationships with a positive role model.*
- *Improved understanding of the risks and consequences associated with their behaviours.*
- *Improved pro-social values and behaviours.*
- *Improved skills in emotional regulation.*
- *Improved engagement with school*
- *Reduced behaviour problems at school*
- *Reduced exclusions*

16. Do you think the impact of the BDP varies for different groups of young people? If so, how and why do you think this is?

Prompt: i.e., young person's ethnicity, age, gender etc. Does this vary across the different schools or areas that the BDP is working in?

17. Which aspects of the BDP make the most difference for young people?

Prompts:

- a) One to one support with a trusted adult who acts as a positive role model*
- b) Strategies to support managing emotions, effective communication or conflict management*
- c) Support thinking about goals and aspirations and future options*

18. Have there been any unintended impacts, either positive or negative, resulting from the BDP?

Prompt: If yes, do these differ depending on the young person's characteristics and/ or experiences of the programme?

Understanding business as usual

19. Please can you describe what business as usual support has looked like for boys in the control group (i.e. who do not receive the BDP), to the best of your knowledge?

- a. What, if any, other services would these boys have been referred to in each school?
- b. Has any alternative provision been made for boys in the control group in each school? If so, what has this looked like?
- c. To what extent has this varied across each of the eight schools?
- d. To what extent has this varied across each of the four Cohorts?

Future delivery

These questions are designed to understand your views on the implications of scaling up the BDP across the country.

- 20. Is there anything you'd like to change about the BDP?
- 21. What do you think would be three key factors necessary for the BDP to be successfully implemented in a new area? (either within London or beyond?)
- 22. Do you think there would be any challenges with replicating and scaling up the BDP in a new area? How could these challenges be addressed?
- 23. Is there anything else that you would like to tell us?

Thank you very much for your time

Topic guide for school stakeholders

Introduction

Thank you for agreeing to take part in this interview, which should take between 45 minutes to an hour.

As you may know, Cordis Bright, an independent research organisation, has been asked by the Youth Endowment Fund to conduct an independent evaluation of Future Men's Boys' Development Programme (BDP) in South London.

The aim of the evaluation is to see whether the BDP improves educational outcomes in comparison to young people who receive "business as usual" services.

The aim of this interview is to inform our understanding of how effective the implementation of the BDP has been, any challenges and solutions that have arisen, and any impact the programme has had on young people who have taken part.

What you say in this interview will remain confidential, unless you mention anything which raises safeguarding concerns, in which case we would have to pass this on to the relevant authorities. However, we would try and speak to you about this first. We will not attribute your views to you in any report that we produce based on this interview.

Are you happy for us to record the conversation to help us with our note-taking?

If you have any questions as we go through the interview, please do ask. Thank you again for your time and taking part in this interview.

Questions

About you

1. Please can you confirm your name, role and the school you work for?

Note to researcher: We should already have this information, so just confirm it with the interviewee.

2. Please could you describe your relationship to the BDP?

Prompts: Have you referred into BDP? Do you work with any children or young people receiving support from BDP?

The BDP model

Context and rationale

3. What is the rationale for the BDP, and why is it needed in South London? What gap is it seeking to address?
4. Is there local demand for the programme? Why do you say this? Does this demand vary across London?

Referrals and eligibility criteria

5. Please can you describe who the BDP aims to work with?

Prompts: Boys in Years 7-11 at risk of:

- d) Exclusion (due to poor behaviour records and/or significant behaviour incidents)*
- e) Disengagement (achieving below potential and/or poor attendance)*
- f) Poor outcomes due to known adversity outside school (child-poverty, previous or ongoing child-protection concerns)*

6. Do you think that the BDP is successfully reaching these young people? Why do you say this?
 - a. Are there any other groups who you think would benefit from the BDP who are not currently being reached? Why do you think this?
 - b. How well does the BDP reach young people from minoritised and marginalised backgrounds, for example from ethnic minority backgrounds? If well, how does it achieve this? If it could be better, what actions are needed?
7. Have you referred a young person to the BDP? If yes, how did you find this process? Did anything work particularly well? Could anything be improved?
8. Are the pathways for identification, referral and assessment into and through the BDP working well? Is it clear why and how decisions have been made in terms of:
 - a. Eligibility for the BDP (entrance criteria)?
 - b. Decisions for what support is provided for young people and families?

Delivering the BDP

9. Please could you describe the types of support that are delivered as part of the BDP to your knowledge?
Prompts: How many sessions do boys typically receive? What topics are covered and how are these topics selected? Does each session follow a similar structure?
10. How well has support been implemented in schools? What's working well and what could be improved? Why do you say this?
11. Have there been any key factors that have supported the effective delivery of the BDP? For example, at:
 - a. An organisational level (i.e. staffing, training)?
 - b. A school level (i.e. school policies, school capacity to support the BDP)?
 - c. A local or community level (i.e. level of need, readiness for change, existing referral pathways, partnership working)?
 - d. A national level (i.e. YEF, government, funding, etc.)?
12. Have there been any challenges or barriers to the effective delivery of the BDP? For example, at:
 - a. An organisational level (i.e. staffing, training)?
 - b. A school level (i.e. school policies, school capacity to support the BDP)?
 - c. A local or community level (i.e. level of need, readiness for change, existing referral pathways, partnership working)?
 - d. A national level (i.e. YEF, government, funding, etc.)?

If so, how were these addressed?

13. In general, how well do you think the BDP manages to engage young people? What has worked well? What could be improved in engaging young people?

Prompts: Are there any aspects of the BDP which have been effective in engaging young people? Are some groups of young people more engaged than others? If so, why do you think this is?

14. Have any young people that you work with dropped out of the BDP during the programme? If yes, why do you think this?

Prompt: Is there anything that you think could support young people to stay engaged?

Impact of the BDP

The next set of questions focus on the impact you think the BDP may or may not be having.

15. Do you have any insight into the impact is the BDP might be having on young people in your school? Do you have any examples you can share?

Prompts: To what extent do you think the BDP is achieving any impact on the following. Why or why not?

- *Trusted relationships with a positive role model.*
- *Improved pro-social values and behaviours.*
- *Improved skills in emotional regulation.*
- *Improved engagement with school*
- *Reduced behaviour problems at school*
- *Reduced exclusions*

16. Do you think the impact of the BDP varies for different groups of young people? If so, how and why do you think this is?

Prompt: i.e., young person's ethnicity, age, gender etc. Does this vary across the different schools or areas that the BDP is working in?

17. Which aspects of the BDP make the most difference for young people?

Prompts:

- d) One to one support with a trusted adult who acts as a positive role model*
- e) Strategies to support managing emotions, effective communication or conflict management*
- f) Support thinking about goals and aspirations and future options*

18. Have there been any unintended impacts, either positive or negative, resulting from the BDP?

Prompt: If yes, do these differ depending on the young person's characteristics and/ or experiences of the programme?

Time estimates for supporting the BDP

The next set of questions focus on the amount of time you think it has taken to support delivery of the BDP in your school. This is part of the cost reporting element of the evaluation, and will help us to get the full picture of what delivering the BDP involves.

19. If the BDP was delivered in your school without the randomisation component or evaluation, how much time would be needed from you to support delivery for ten young people across a term? This may involve the following processes. If you are not sure, please provide your best guess.

- a. Making referrals to the BDP (how much time does it take to complete one referral form?)

- b. Attending a screening conversation with the Future Men team (how long do these typically last?)
 - c. Achieving parent/carers consent
 - d. Supporting the co-ordination of delivery (i.e. supporting room bookings and scheduling). *Prompts: How much time is involved at the start of term to set everything up? How much time does this take per week?*
 - e. Receiving updates from the Future Men team on how delivery is going, and feeding these back to parents/carers as needed.
20. Have you spent time on any other aspects as part of delivering the BDP? If so, what were they and how much time has this taken?

Understanding business as usual

21. Please can you describe what business as usual support has looked like for boys in the control group in your school (i.e. who do not receive the BDP)?
- a. What, if any, other services would these boys have been referred to?
 - b. Has any alternative support been given to boys in the control group? If so, what has this looked like?
 - c. To what extent has this varied by Cohort?

Future delivery

These questions are designed to understand your views on the implications of scaling up the BDP across the country.

22. Is there anything you'd like to change about the BDP?
23. What do you think would be three key factors necessary for the BDP to be successfully implemented in a new area? (Either within London or beyond?)
24. Do you think there could be any challenges with replicating and scaling up the BDP in a new area? How could these challenges be addressed?
25. Is there anything else that you would like to tell us?

Thank you very much for your time

15 Appendix H. YEF Security Rating

Rating	Design	MDES Outcome: Threshold*	Attrition	 Initial score	 Adjustments	Final score
5 	Randomised design	Offending: ≤ 0.1 SDQ tot: ≤ 0.3 Other: ≤ 0.2	0-10%	5		
4 	Design for comparison that considers some type of selection on unobservable characteristics (e.g. RDD, Diff-in-Diffs, Matched Diff-in-Diffs)	Offending: 0.11 – 0.19 SDQ tot: 0.31 – 0.39 Other: 0.21 – 0.29	11-20%			
3 	Design for comparison that considers selection on all relevant observable confounders (e.g. Matching or Regression Analysis with variables descriptive of the selection mechanism)	Offending: 0.2 – 0.29 SDQ tot: 0.4 – 0.49 Other: 0.3 – 0.39	21-30%			5
2 	Design for comparison that considers selection only on some relevant confounders	Offending: 0.3 – 0.39 SDQ tot: 0.5 – 0.59 Other: 0.4 – 0.49	31-40%			
1 	Design for comparison that does not consider selection on any relevant confounders	Offending: 0.4 – 0.49 SDQ tot: 0.6 – 0.69 Other: 0.5 – 0.59	41-50%			
0 	No comparator	Offending: ≥ 0.5 SDQ tot: ≥ 0.7 Other: ≥ 0.6	>50%			

*MDES requirements vary by outcome measurement. Offending: Offending data collected through self-report or admin data; SDQ tot = SDQ total difficulties score; Other: all other outcomes, incl. SDQ externalising and internalising