

The long-term effectiveness of parenting interventions in early childhood for reducing young people's involvement in violence



Statistical analysis plan

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YEF statistical analysis plan

Project title*	The long-term effectiveness of parenting interventions in early childhood for reducing young people's involvement in violence
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Trial design	Two two-armed randomised parallel effectiveness trials with randomisation at the level of the child
Trial type	Effectiveness
Evaluation setting	Child and Adolescent Mental Health Services; Schools

* Please make sure the title matches that in the header and that it is identified as a randomised trial as per the CONSORT requirements (CONSORT 1a).

Target group	3-7 year old children with high levels of antisocial behaviour who were followed up in adolescence
Number of participants	252
Primary outcome and data source	Offending: Self-report Delinquency questionnaire ¹
Secondary outcome and data source	See design overview table

SAP version history

Version	Date	Changes made and reason for revision
1.2 [<i>latest</i>]		
1.1	20.02.26	Revisions in response to peer review
1.0 [<i>original</i>]		

Any changes to the design or methods need to be discussed with the YEF Evaluation Manager and the developer team prior to any change(s) being finalised. Describe in the table above any agreed changes made to the evaluation design. Please ensure that these changes are also reflected in the SAP (CONSORT 3b, 6b).

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Introduction

Preventing young people from becoming involved in violence is a national priority. Many cost estimates for antisocial behaviour by young people have been reported, dating back to analyses by our research group in the BMJ² more than two decades ago. Since then, estimates of costs have grown with the appreciation of the impact on justice, education, and health systems^{3,4} and the cost of causal factors have also begun to be identified^{5,6}.

Understanding why some but not other young people become involved in violence requires careful study of early behavioural characteristics and social and familial adaptations over time. Early antisocial behaviour⁷ and familial-level risk factors such as poor parent-child attachment⁸ and low parental supervision⁹, increase risk for antisocial behaviour in adolescence. Parent training, particularly when delivered in early childhood, may serve to prevent involvement in violence and delinquency, either directly or indirectly via differential exposure to subsequent risk and protective factors in later childhood.⁷

Parent training programs are typically delivered to parents of children aged 11 or younger¹⁰. They aim to improve parents' ability to respond to the physical, emotional, and social needs of their children¹¹, and may include practical components relating to praise and rewards, play, discipline, limit setting, and boundaries. These interventions are generally associated with improvements in child externalising behaviour in indicated samples (e.g., children with diagnosed conduct disorders)¹², with slightly smaller effects found in universal/prevention studies¹³. Although parent training can be effective, not all children and families benefit equally. Research examining moderators of parent training outcome have yielded inconsistent findings, although some evidence suggests that boys^{14,15}, younger children¹⁴, and those with more severe baseline antisocial behaviour^{15–18} derive more benefit, while those from racial/ethnic minorities¹⁹ and from lower socioeconomic status backgrounds^{17,18,20} derive less benefit.

Whilst evidence-based parent training programmes for children have demonstrated effectiveness for reducing behavioural problems, evidence for their long-term effectiveness is very limited. Our team is unique in reporting some of the only available long-term follow-up data on behavioural problems in the UK²¹. A number of key questions remain regarding the impact of early parent training on crime and violence in adolescence; this is the focus of the current analyses.

Specifically, basic questions remain about the long-term developmental paths followed by individuals who engage in criminality and violence in adolescence. One gap in the literature results from a reliance on a community sampling frame. That approach has the benefit of enrolling representative samples but has the downside of under-enrolling very high-risk and low-resource families. That is because high-risk and low-resource families are notoriously

difficult to engage in longitudinal research; they are not found in sufficient numbers unless they are specifically targeted. Furthermore, they are also difficult to retain in longitudinal studies even where tailored efforts are applied.

A second gap is that most studies of developmental pathways leading to involvement in violence and delinquency derive from observational studies that lack causal leverage or, in most cases, detailed data on interventions received, health economics, and encounters with health, education, and youth justice systems. Additionally, parenting intervention studies using randomized controlled designs do have causal leverage but are typically short-term, and unable to that examine if, and how, the benefits extend into adolescence to reduce criminality and violence. Parenting interventions in early childhood have wide-spread acceptability and growing accessibility. Findings from the current proposal will provide novel and much-needed information about if and how parenting interventions reduce serious offending later in life and how current parenting intervention practices may be improved to promote healthy families/young adult development.

To summarise, early parenting interventions have demonstrated benefits on immediate and short-term behavioural health, but very little research has examined their long-term impacts, particularly on offending in adolescence and the mechanisms of action. The present project examines the long-term effects into adolescence of early parent training on youth offending. It consists of two treatment trials of the Incredible Years (IY) parenting programme delivered to parents of 3–7-year-old children from a clinical and from a community sample and a follow-up study with these children in adolescence, aged 9 to 17 years.

A novel feature of this project is that it examines if, and by what mechanisms, early interventions may have persisting beneficial effects on adolescents and their families – especially as regards subsequent involvement in violence and delinquent behaviour. The early intervention posed a sizable manipulation of the early environment, and we know it was successful from early childhood post-treatment analyses^{22,23}. We now ask how successful that manipulation was for preventing the onset of involvement in violence and delinquent behaviour in adolescence. We compare children and families whose early experiences differed in an important way to see if that significantly prevents violence involvement years later. Several large-scale and ambitious early interventions have been carried out in recent years^{24,25}; many of these efforts seek to reduce anti-social and delinquent behaviour, among other key target outcomes. These efforts are important in emphasizing the early years but may, perhaps as an unintended side-effect, somewhat over-attribute direct effects to the early experiences *per se*. A significant hypothesis to be tested in this project is that the impact of early interventions for violence and delinquency in adolescence must be understood in terms of shift in exposures to risk- and resilience-promoting experiences in later childhood. We are well-positioned to address this important issue because of our collective of cohorts

that have been exposed to early parenting interventions and, critically, were followed up long after the intervention ended to assess developmental pathways to violence and delinquency.

Primary Research Question

1. Are there long-term benefits of an early parenting intervention on involvement in violent and delinquent behaviour more than a decade later in mid/late-adolescence? Specifically, do children of parents who received early parent training report less violence and delinquency in adolescence, as assessed using the Self-Reported Delinquency Questionnaire, than children of parents who did not receive parent training?

Secondary Research Questions

2. Why do some but not other children derive a long-term benefit of early parenting interventions?
 - a. Child gender: Do boys, who are at greater risk for delinquent/aggressive outcomes, derive greater benefit of parenting interventions than girls?
 - b. Child race/ethnicity (White British vs. Other race/ethnicity[†]): Do children from non-White British backgrounds derive less benefit because the interventions were originally designed for families of White [British] backgrounds?¹⁸
 - c. Child age at trial entry (assessed continuously): Do younger children derive more benefit than older children?¹⁷
3. Socioeconomic status at trial entry (as indexed by maternal education level): Do families at lower socioeconomic/resource backgrounds derive less long-term benefit than those from higher socioeconomic backgrounds?^{2,3}
4. By what mediating mechanisms do early interventions reduce violent and delinquent behaviour in adolescence?
 - a. Do improvements in parenting quality, assessed using direct observation at post-intervention, account for the intervention effects on long-term reductions in violence and delinquency in adolescence?²⁶
 - b. Do improvements in peer relationships (assessed using the Peer Problems subscale of the Strengths and Difficulties Questionnaire at post-intervention) and friendship quality (assessed using the Friendship Quality Questionnaire in adolescence) account for the intervention effects on reductions in violence and delinquency in adolescence?²⁷

[†] Our sample size precludes more fine-grained comparisons relating to race/ethnicity.

- c. Do changes in neighbourhood context, assessed using the Neighbourhood Questionnaire in adolescence, account for the intervention effects on reductions in violence and delinquency in adolescence?
- d. Does increased health service use, assessed using a composite index derived from the Client Service Receipt Inventory at post-intervention and adolescence account for the intervention effects on reductions in violence and delinquency in adolescence?

Table 1. How will the questions be addressed at each stage?

Question Number [‡]	Interim report	Final report
1	• Initial descriptive summaries (e.g., of participant characteristics) completed. • Examination of distribution of self-reported delinquency variables; decision made regarding strategy for handling skewed distributions, if indicated. • Summary of missing data for variables of interest completed, missingness mechanisms tests run, and decision made regarding missing data strategy.	• Formal modelling and interpretation of results. • Write-up of findings for final report.
2a-d	• Examine distributions of moderator variables (i.e., age, gender, race/ethnicity, maternal education)/data familiarisation.	• Formal modelling and interpretation of results. • Write-up of findings for final report.
3a-d	• Examine distributions of mediating variables (i.e., parenting quality, peer relationships, service use, neighbourhood context)/data familiarisation.	• Formal modelling and interpretation of results. • Write-up of findings for final report.

[‡]Question numbers should follow the ordering in the section above.

Methods

Clinical sample

We previously reported the outcome of an RCT of group parent training delivered to parents of clinic-referred children with high levels of antisocial behaviour.²⁸ We have also reported results for longer-term follow-up assessments (approximately 18 months post-trial entry) for families who received the parent training.¹⁵ We summarise the trial methods below.

Participants. Participants were children aged three to eight years who were referred to one of four NHS child and adolescent mental health services (three in South London, one in West Sussex) for antisocial behaviour. The trial ran from 1995 to 1999. Exclusion criteria were clinically apparent major developmental delay, hyperkinetic syndrome, or any other condition requiring separate treatment. Parents had to be able to understand English and be available to attend group sessions. Participant baseline characteristics are provided in the Analysis section of this SAP. Briefly, most children were boys from non-ethnic minority backgrounds, with an average age of approximately six years. Children had high levels of conduct problems at baseline compared to population norms, with mean scores above the 97th percentile.

Randomisation. In each centre, participants were assigned to the intervention or control group using a permuted block design. Each block consisted of a consecutive three-month period during which all eligible referrals were allocated to one arm of the trial. This design was chosen with the aim of recruiting at least six cases per parenting group, with the assumption that 5-15 cases were referred in each block. The planned overall ratio of intervention to control blocks was 2:1. Each centre had to have at least two intervention blocks per year to ensure that there were enough cases to run a group even after any dropouts. Also, intervention blocks had to be more than three months apart to ensure there were sufficient cases to run a group within six months of a child being assessed. Therefore, we determined the sequence nonrandomly for each centre annually in advance with the advice of a statistician. This gave 52 blocks: 35 intervention and 17 control (ratio 2.06:1). The sequence was kept in a locked cabinet.

Parenting intervention. The Incredible Years basic videotape program²⁹ was offered to groups of parents of 6-8 children over 13-16 weeks. Sessions were two hours long and delivered on a weekly basis. The program has a strong evidence base²² and emphasises engagement with parental emotions, rehearsal of behavioural strategies and understanding its scientific rationale. Program components covered play, praise and rewards, limit setting, and handling misbehaviour. In each session, videotaped scenes of parents and children together are shown, depicting “right” and “wrong” ways of handling children. Parents are supported while they practise alternative ways of managing their children; homework is set and telephone

calls were made to parents to encourage progress. Therapists held regular jobs in their local service and were trained over three months. Intervention sessions were videotaped and discussed in weekly supervision meetings to ensure adherence to the program manual.

On average, parents attended nine sessions ($SD = 4$), 74% of those offered. 82% attended five or more which was considered reasonable compliance, and 18% attended four or fewer, which was considered dropping out. Families allocated to parent training showed a large reduction in child antisocial behaviour after treatment compared with controls (effect size = 1.1 standard deviations), and improvement was maintained at 18-month follow-up.

Control intervention. Control participants received individualised treatment in child mental health clinics by trained mental health staff. Typically, both the parent and child were seen together and separately, using a counselling model. Parents received supportive psychotherapy, and children received help in understanding why they might feel angry or frustrated and in exploring strategies to change this. On average, families attended six sessions, 63% of those offered. Five children in this arm received methylphenidate medication; none in the parenting group arm did. Participants in the control arm were offered the parent training intervention after the completion of the trial.

Community sample

We previously reported the outcome of an RCT of group parent training delivered to parents of children identified as having high levels of antisocial behaviour via screening in schools.³⁰ We summarise the trial methods below.

Participants. Participants were children identified as having high levels of antisocial behaviour, based on a screening program of children in reception and year one classes across eight schools in Lambeth, London. Teachers and parents completed the Conduct Problems subscale of the Strengths and Difficulties Questionnaire and rated the child on the eight DSM-IV ODD criteria (from 1 = *not true* to 4 = *very much true*). Parent and teacher scores were summed for both measures; the cutoff score for eligibility was $\geq$ for the SDQ Conduct Problems subscale and $\geq$ for DSM-IV ODD criteria. In addition, children had to be free of clinically apparent developmental delay. Parents had to understand English, be available to attend group sessions, be interested in attending, and accepting of the RCT study.

Randomisation. After screening, 8 to 16 cases per cohort were assessed and then the trial coordinator forwarded cases to the trial statistician who, blind to any other information, randomised them individually to the intervention or control group.

Parenting intervention. This was the 12-week Incredible Years basic videotape program³¹, supplemented by sessions on how to read with the child. The intervention was delivered in schools to 4-8 parents for two and half hours one morning per week. Twenty-eight weeks of

intervention were offered across three terms: the 12-week parent training program followed by the 10-week literacy program, then a six-week revision period. The Incredible Years program included videotape clips of parents with their children, covering promotion of desirable behaviour and on-task attending through play, praise and rewards, handling misbehaviour, applying consequences, and time-out. Groups engaged in discussions and role play to practice parenting behaviours. Many therapists were from the clinical sample trial; training and supervision procedures were the same (see above). The literacy program³² encouraged parents to discuss the child's book, to link the text to the child's everyday experiences, and help the child 'predict' what might happen next and to 'discover' print in their ordinary environment. It then teaches the Pause Prompt Praise approach to reading. When a child encounters an unknown word, the parent is taught to pause for 5 seconds; if the child doesn't succeed, the parent gives a specific prompt and then praises the child for complying. Other elements included role-play and homework, family literacy workshops, and two home visits.

Mean attendance was 15 sessions, 55% of those offered. Families allocated to the parent training intervention showed a moderate reduction in child antisocial behaviour after treatment compared with controls (effect size = 0.6 standard deviations).

Control intervention. Control parents were offered a telephone helpline manned by the same staff, advising them how to access regular services. Seven parents contacted the helpline, 1 attended a local clinic and received counselling. No child in either trial arm received psychotropic medication.

Design overview

Please ensure all details are in line with the latest version of the protocol.

Table 2. Design Overview

Trial design, including number of arms		Two two-armed randomised parallel efficacy trials
Unit of randomisation		Individual child
Stratification variables (if applicable)		n/a
Primary outcome	Variable	Volume of self-reported delinquency
	measure (instrument, scale, source)	Self-Report Delinquency Questionnaire ¹ (total volume score)
Secondary outcome(s)	variable(s)	Variety of self-reported delinquency
	measure(s) (instrument, scale, source)	Self-Report Delinquency Questionnaire (total variety score)
Baseline for primary outcome	variable	N/A
	measure (instrument, scale, source)	N/A
Baseline for secondary outcome	variable	N/A
	measure (instrument, scale, source)	N/A

Other variables

We provide information about other variables to be used in analyses (e.g., moderators; mediators; covariates etc.) below. T1 refers to the pre-intervention assessment, T2 to the post-intervention assessment, and T3 to the adolescent follow-up.

Table 3. Childhood Timepoint Variables

	Measure	Variable description	Time
Demographics	Parent interview		T1
Child gender		Male vs. female	
Child age		Decimal years	
Child race/ethnicity		British White vs. Other	
Maternal education level		Left school younger than 16 vs. Left school 16-18 or technical/secretarial qualification vs. professional training, no degree vs. degree	
Eligibility for free school meals		Yes vs. no	
Child antisocial behaviour	Parent Account of Child Symptoms interview ³³	Antisocial behaviour score (average of severity and frequency ratings; range = 0-3)	T1
Parenting	Direct observation across three tasks, coded using the CARP and PBCS		T1, T2
Parental intrusion		Mean score across three tasks	
Parental negative affect		Mean score across three tasks	
Criticism		Count of parental criticisms (expressed as rate per minute, averaged across three tasks)	

Sensitive responding		Mean score across three tasks	
Mutuality		Mean score across three tasks	

Table 4. Adolescent Timepoint Variables

	Measure	Variable description
Child age	Parent interview	Decimal years
Service use	Client Service Receipt Interview ³⁵	Composite index reflecting use of mental health and education services.
Parenting		
Corporal punishment	Alabama Parenting Questionnaire ³⁶ (APQ, parent- and child-report)	Subscale score
Inconsistent discipline		Subscale score
Involvement		Subscale score
Poor monitoring and supervision		Subscale score
Positive parenting		Subscale score
Sensitive responding	Direct observation across three tasks	Mean score across three tasks
Mutuality		Mean score across three tasks
Peer relationships		
Peer delinquency	Peer Delinquency Questionnaire ¹ (YP-rated)	Peer delinquency total score (range = 0-18)

Friendship quality	Friendship Quality Questionnaire ³⁷ (YP-rated)	Total score (range = 0-54)
Neighbourhood context	Neighbourhood Questionnaire (parent-rated)	Total score (higher scores = more positive maternal perceptions of neighbourhood)

Sample size calculations overview

Below we provide a table of power calculations. Power calculations were run using GPower, supplemented with statistical programs (SPSS, R), and online resources. It is essential in these calculations not to create a single estimate but rather to provide a range of conditions – that is equally so for planning a study and for secondary data analysis.

The primary population of interest is primary school children with elevated behaviour problems scores; children from both studies were sampled from low resources communities and so that may be a factor shaping generalizability. As further background, we note that analyses from these trials have consistently yielded large effects (.5 or greater), which we attribute to the well-conducted trial and the sensitive measurement models. On the other hand, for the novel analyses proposed for this project we cannot presume medium or large effects and so we provide estimates based on small effects (.2-.3). Experience shows that having an impact on clinical practice and policy in this area almost always requires effect sizes that are medium or large. As power functions are decidedly non-linear, the adequate power we will have for small effects means that we will have far more than adequate power to detect the medium or large effect sizes that attract attention for those shaping practice and policy.

We adopt the n of 252 (across 2 cohorts); this n reflects the number of participants, across the two trials, who were randomly allocated to either the intervention or control arms.

Provided below in Table 5 are also several conditions for computing power, including p value (.05 and the more conservative .01); 1- vs 2-tailed test (for every hypothesis we propose we have a clear *a priori* expectation, and so the 1-tailed test is justified, but we nevertheless provide the 2-tailed condition); effect size (a small effect is typically defined as .2-.3, and so we provide both conditions). We would argue that the best single indicator of power would be the first column, where we estimate $> .80$ power. The one condition where there is sizable less than .80 power consider the most stringent and, we think, least plausible conditions (2-tailed, $p < .01$, .2 ES).

Table 5. Summary of power across scenarios

Parameters								
<i>n</i> (total)	252	252	252	252	252	252	252	252
<i>p</i> -value	0.05	0.05	0.05	0.05	0.01	0.01	0.01	0.01
1 vs 2-tailed	1-tailed	2-tailed	1-tailed	2-tailed	1-tailed	2-tailed	1-tailed	2-tailed
Effect size	0.2	0.2	0.3	0.3	0.2	0.2	0.3	0.3
Power	> .80	> .70	> .96	> .93	> .67	> .56	> .88	> .83

		Protocol	Randomisation
Minimum Detectable Effect Size (MDES)		n/a	0.2
Pre-test/ post-test correlations	level 1 (participant)	n/a	.20
	level 2 (cluster)	n/a	n/a
Intraclass correlations (ICCs)	level 1 (participant)	n/a	n/a
	level 3 (cluster)	n/a	n/a
Alpha [§]		n/a	0.05
Power		n/a	0.80
One-sided or two-sided?		n/a	1-sided (we have a strong a priori hypothesis, based on our previous findings)

[§] Please adjust as necessary for trials with multiple primary outcomes, 3-arm trials etc. when a Bonferroni correction is used to account for family-wise errors.

		Protocol	Randomisation
			regarding the intervention outcome)
Average cluster size		n/a	n/a
Number of clusters**	intervention	n/a	n/a
	control	n/a	n/a
	total	n/a	n/a
Number of participants	intervention	n/a	150
	control	n/a	102
	Total	n/a	252

As this is a secondary data analysis, sample size was constrained by the original trial. Power was calculated under alternative MDES, alpha, and 1- vs. 2-tailed scenarios. We will pursue missing data strategies using, e.g., multiple imputation, but the sample size provided represents observed/measured cases. We have 80% power to detect a minimum effect size of 0.2 with an alpha of .05 and a total sample size of 252, when using a 1-sided test. We consider this the most likely scenario out of those presented above.

The sample size provided, $n = 252$, is across both RCTs to be analysed and represents the number of cases randomly allocated in the original trials.

As noted, the sample size was previously established (this is a secondary data analysis); the power to detect an effect is provided in the table.

Analysis

Methods of analyses were chosen after data were collected (by definition, as this is a secondary data analysis). We will use SPSS, supplemented with AMOS software for structural equation modelling (SEM). All syntax documenting the analysis strategy for the project funded by this grant will be made available. All analyses will be run using an intention-to-treat (ITT) approach, whereby random assignment to either the parent training or control condition in

** Please adjust as necessary e.g., for trials that are randomised at the setting, practitioner or participant level.

the original trials will be used to indicate treatment allocation (i.e., not the amount the intervention received).

Primary Outcome Analysis

We will use ordinary least squares (OLS) regression for our primary outcome analysis. OLS regression falls within the general linear model (GLM) framework and provides a flexible approach to modelling. We will regress total self-reported delinquency volume (assessed using the SRD) on treatment allocation (parent training vs. control) and a number of covariates, including child gender (male vs. female), age at trial entry (decimal years), race/ethnicity (White British vs. other race/ethnicity), indicators of SES (i.e., maternal education, eligibility for free school meals) and study/sample (clinical vs. community sample; which serves as a proxy for child antisocial behaviour at baseline).

The OLS regression equation for our primary outcome analysis is outlined in Equation 1.

$$y = \beta_0 + \beta_1 X_1 + \beta_2 X_2 + \cdots \beta_6 X_6 + e$$

Equation 1

Where:

y is the estimated SRD volume score

β_0 is the intercept

$\beta_1 X_1$ is the coefficient for treatment allocation (intervention vs. control)

$\beta_2 X_2 + \cdots \beta_6 X_6$ are the coefficients for the covariates age, gender, race/ethnicity, maternal education, eligibility for free school meals, and sample

e is the error term

Self-reported delinquency data are often characterised by right skewed distributions, and it may be necessary to transform or scale the primary outcome variable to ensure GLM assumptions are met. We will visually inspect frequency histograms of model residuals to determine whether transformation is required. It is worth noting that OLS regression is reasonably robust to violations of normality, provided sample size is adequate, and data are not highly skewed. In the presence of highly skewed data, we will consider transforming the SRD volume score to better approximate a normal distribution (e.g., using a log or square root transformation). In instances where transforming outcome variables does not contribute to more normally distributed residuals, we will use generalised linear models with log link functions and Poisson distributions to account for skewed distributions.

Secondary Outcome, Moderation, and Mediation Analyses

A separate model will be estimated for our secondary outcome of interest: self-reported delinquency variety, also assessed using the SRD. The model equation will be the same as Equation 1 above; but y will represent the estimated self-reported delinquency variety score.

OLS regression is also well-suited to address our secondary research questions, as interaction terms between treatment allocation and moderator variables can be included to estimate the effect of treatment allocation, depending on the level of the moderator. Moderator variables of interest include child gender, age at entry, race/ethnicity, SES (using maternal education level as a proxy), and sample. Equation 2 is the equation for the OLS regression for moderator analyses.

$$y = \beta_0 + \beta_1 X_1 + \beta_2 X_2 + \beta_3 X_1 X_2 + e$$

Equation 2

Where:

y is the estimated SRD volume score

β_0 is the intercept

$\beta_1 X_1$ is the coefficient for treatment allocation (intervention vs. control)

$\beta_2 X_2$ is the coefficient for the moderator variable: child gender (male vs. female), age, race/ethnicity (British White vs. other race/ethnicity), SES (maternal education), and sample (clinical vs. community). Separate regressions will be run for each moderator.

$\beta_3 X_1 X_2$ is the coefficient for the interactions between treatment allocation and child gender

e is the error term

The model will also adjust for any covariates included in primary analyses (see above).

In addition to examining moderators, we will also use SEM to examine mediators of intervention outcome, using AMOS. We will construct separate models for each mediator (i.e., parenting, peer relationships, neighbourhood context, and service use) and adjust each for the same covariates included in primary analysis models. Each model will be evaluated using standard indices of model fit (e.g., Comparative Fit Index (CFI); Root Mean Square Error of Approximation (RMSEA)).

Subgroup analyses

We will not perform subgroup analyses (i.e., all analyses for primary and secondary outcomes will include the full complement of participants; $n = 252$).

Further analyses

We will check model assumptions to ensure inferences are valid. For OLS regression models, this includes normal distribution of model residuals; linearity; homoskedasticity; and independence of observations.

Interim analyses and stopping rules

N/A

Longitudinal follow-up analyses

N/A

Timepoints

Below we provide a summary of the baseline (pre-intervention), post-intervention, and follow-up assessment points from the two trials.

Table 6. Summary of Assessments

Study	Baseline (year)	Post-Intervention	Adolescent follow-up
Clinical sample	1995-1999		2005-2007
	Trial entry (prior to randomisation)	5 to 7 months post-randomisation	5.6 to 10.5 years later ($M = 7.8$, $SD = 1.1$)
Community sample	1999-2001		2005-2007
	Trial entry (prior to randomisation)	1-year post-randomisation	4.2 to 7.7 years later ($M = 5.8$, $SD = 0.8$)

Imbalance at Baseline

We provide summary statistics (means and standard deviations for continuous variables; counts and percentages for categorical variables) for participant baseline characteristics below. Specifically, we present summary statistics for key demographic (i.e., child gender and age, parent race/ethnicity), socioeconomic (i.e., maternal education, income), and clinical (i.e., severity of conduct problems) characteristics.

Table 7. Sample Characteristics at Baseline

	Clinical sample				Community sample			
	Intervention (<i>n</i> = 89)		Control (<i>n</i> = 51)		Intervention (<i>n</i> = 61)		Control (<i>n</i> = 51)	
Characteristic	<i>n</i>	%	<i>n</i>	%	<i>n</i>	%	<i>n</i>	%
Male gender	67	75	39	77	42	69	37	73
Parent ethnic minority	16	21	2	5	25	41	19	38
Mother left school aged 16 or younger	46	57	28	61	21	34	18	35
Income < £175/week	16	18	13	25	7	14	7	19
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Age	5.4	1.5	5.8	1.6	5.2	0.3	5.2	0.3
Antisocial behaviour score (interview)	1.6	0.5	1.6	0.3	1.2	0.4	1.1	0.5

Missing Data

We will first investigate the extent and mechanisms underlying missing data by providing descriptive summaries of missing data rates for variables of interest and examining associations between missingness and other observed variables in the dataset. To this end, we will generate descriptive summaries of missing data rates for key variables (e.g., SRD delinquency scores) and run simple chi-square and *t*-test analyses to explore variables (for which complete or virtually complete data are available, e.g., demographic characteristics) that might predict missingness.

Our strategy for handling missing data will depend on both the extent of, and mechanism underlying, this missingness. For low rates of missing data (i.e., < 5%), complete case analysis is thought to be sufficient, regardless of the mechanism underlying the missingness. In this scenario, we will run complete case analyses only and will not explore missingness mechanisms.

For rates of missing data greater than 5%, we will first consider plausible mechanisms underlying the missingness. There are three different scenarios under which data may be missing: Missing Completely at Random (MCAR), Missing at Random (MAR), and Missing Not at Random (MNAR). MCAR means that there are no systematic differences between those with missing and non-missing values, based on either the observed or unobserved data. MAR means there are systematic differences, and these differences are explained by the observed data. MNAR means there are systematic differences, but these differences are explained by unobserved data, not the observed data. We will use results from the preliminary chi-square and *t*-test analyses (see above), as well as our team's knowledge of the area to make judgements about likely missingness mechanisms.

If data are judged to be MCAR or MAR, we will use multiple imputation techniques. These techniques involve generating multiple plausible values using the observed data, computing parameter estimates separately for each imputation, then combining the results and

calculating variation in parameter estimates. We will use variables within our primary analysis model (i.e., intervention allocation, age, gender, race/ethnicity, SES, sample, and SRD outcome; see Analysis section above) to produce 10 imputed datasets. We will use results from analyses using multiple imputation as the basis for answering our research questions. However, we will also run sensitivity analyses using complete cases only. If findings from imputed and complete case analyses are materially different (i.e., if interpretations differ based on the analysis) then our findings will need to be interpreted with this caveat in mind.

Other Potential Limitations or Sources of Bias

Table 8. Threats to Internal Validity

Threat	Risk level	Comments
Confounding variables	Low	Confounding variables (i.e., variables associated with both self-reported delinquency and trial arm) may threaten internal validity by introducing other potential explanations (i.e., other than receipt of early parent training) for observed group differences in self-reported delinquency in adolescence. The data to be analysed in this project are rich and span multiple time points, including information relating to demographic (e.g., age, gender, race/ethnicity), socioeconomic (e.g., parental education, income level), child (e.g., antisocial behaviour severity, IQ), family (e.g., parenting), social (e.g., peer relationships), and wider contextual (e.g., neighbourhood context) factors, and we will thus be able to account and adjust for a range of potential confounding variables. Additionally, the randomisation of participants to trial arm protects against the possibility of unobserved confounds (i.e., those that cannot be adjusted for statistically) influencing results.
Concurrent interventions/contamination	Low	It is possible that children and families in the control groups (i.e., who did not receive parent training as part of the initial trials) took part in parent training or other interventions intended to reduce child antisocial behaviour in the interim between post-intervention assessments and long-term follow-up. At the adolescent follow-up, information was gathered about families' use of a range of health services since participation in the original trial. We will thus be able to examine the extent to which control participants were exposed to similar interventions, and account for this if necessary.
Sample representativeness	Moderate	High-risk and under-resourced families are notoriously difficult to engage in longitudinal research and have historically been underrepresented in population-based observational studies investigating pathways to violence

		and delinquency. This includes children and families from racial/ethnic minority backgrounds. Nearly one third of children in the combined dataset identify as Black, Asian, and Ethnic Minority; many of whom identify as Black Afro-Caribbean. As such, we will be able to examine whether there were differential effects of early parent training for children and families from racial/ethnic minority backgrounds, which will inform further understanding regarding the extent to which interventions are effective for these groups. However, due to the relatively smaller proportion of participants identifying with an ethnic minority, and the diversity of ethnic backgrounds identified with, we cannot examine more fine-grained comparisons regarding the role of race/ethnicity. This could obscure important differences, and thus findings will need to be interpreted more cautiously considering this.
Intervention fidelity and compliance	Low	Intervention fidelity and compliance were examined in the original trials. Among the clinical sample, 82% of parents attended five or more sessions, which was considered reasonable compliance. Among the community sample, 75% of parents attended five or more sessions, while the remaining 25% dropped out. Additionally, therapists delivering the parent training were trained and accredited in the program and received regular supervision to ensure intervention fidelity. Given the reasonably high rate of compliance and the efforts undertaken to maximise intervention fidelity, risk of bias arising from issues related to fidelity and compliance may be relatively low.

Compliance

Treatment allocation is defined at random assignment conducted at the initial trial; we will continue to use the random allocation to indicate treatment, providing an intent-to-treat analysis. In the original trial containing the clinical sample, reasonable compliance was defined as attendance at five or more sessions (82% of parents); drop-out was defined as attendance at four sessions or fewer (18%). Parents attended 9 sessions on average, 74% of those offered. In the original trial containing the community sample, reasonable compliance was defined in the same way. 75% of parents had reasonable compliance and 25% dropped out. Parents attended 15 sessions on average, 55% of those offered.

Presentation of Outcomes

Cohen's d will be used to quantify the magnitude of differences between the intervention and control groups on continuous outcome variables (i.e., SRD scores). Cohen's d is a widely used standardised mean difference (SMD) metric and is largely comparable to Hedges' g for samples > 30 . We will calculate SMDs and 95% CIs to estimate the degree of uncertainty around the point estimate. The general formula for Cohen's d is provided in Equation 3. We

will calculate SMDs based on estimated marginal means from the model (i.e., accounting for covariates included in modelling).

$$d = \frac{(M_1 - M_2)}{S_{pooled}}$$

Equation 3

Where:

d = the SMD in SRD scores between the intervention and control groups

M_1 = the mean SRD score for the control group

M_2 = the mean SRD score for the intervention group

S_{pooled} = the pooled standard deviation, calculated using the formula in Equation 4.

$$S_{pooled} = \sqrt{\frac{(S_1^2 + S_2^2)}{2}}$$

Equation 4

Where:

s_1 = the SD associated with mean SRD score for the control group

s_2 = the SD associated with mean SRD score for the intervention group

Ethics and Registration

Approval from the relevant ethics committees was sought and received for both original trials, and for the adolescent follow-up study. Parents were provided with information about the study and provided written informed consent if they wished to take part. The trials were registered with the ISRCTN register (<https://www.isrctn.com/>; identification numbers 94713762 and 77566446).

Timeline

Table 8. Summary of Timeline

<i>Dates</i>	<i>Activity</i>	<i>Staff responsible/ leading</i>
30.05.26	First draft of interim report submitted	All
13. 06.26	Final interim report submitted	All
11.12.26	First draft of final report submitted	All
16.01.27	Final draft submitted	All

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