

# Randomised controlled trial of a parenting intervention in the voluntary sector for reducing child conduct problems: outcomes and mechanisms of change

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**Background:** To test effectiveness of a parenting intervention, delivered in a community-based voluntary-sector organisation, for reducing conduct problems in clinically-referred children. **Methods:** Randomised controlled trial, follow-up at 6, 18 months, assessors blind to treatment status. Participants – 76 children referred for conduct problems, aged 2–9, primarily low-income families, randomised to treatment vs. 6-month wait-list group. Retention was 93% at 6 months, 90% at 18 months. Interventions – Webster-Stratton Incredible Years video-based 14-week group programme, teaches cognitive-behavioural principles for managing behaviour, using a collaborative, practical, problem-solving approach. Primary outcomes – child problem behaviour by parent-report (Eyberg) and home-based direct observation; secondary outcomes – observed positive and negative parenting; parent-reported parenting skill, confidence and depression. **Results:** Post-treatment improvements were found in child problem behaviour, by parent-report (effect size (ES) .48,  $p = .05$ ) and direct observation (ES .78,  $p = .02$ ); child independent play (ES .77,  $p = .003$ ); observed negative (ES .74,  $p = .003$ ) and positive (ES .38,  $p = .04$ ) parenting; parent-reported confidence (ES .40,  $p = .03$ ) and skill (ES .65,  $p = .01$ ), using ANCOVA to control for baseline scores. Maternal depression did not change. Consumer satisfaction was high. At 18-month follow-up, although no randomised comparison was possible, changes appeared to maintain, with no significant change toward baseline level on any measure. Change in observed positive parenting appeared to mediate change in child problem behaviour ( $p < .025$ ). **Conclusions:** Findings suggest that a group-based cognitive-behavioural parenting programme, delivered by well-trained and supervised staff, can be effective in a community voluntary-sector setting, for reducing conduct problems and enhancing parenting skills. Change in parenting skill appears to be a key mechanism for change in child behaviour. Findings have implications for feasibility of translating evidence-based programmes, even for clinically-referred conduct problems, into less specialised community settings, likely to have lower costs and be more accessible for families. **Key-words:** Parenting, conduct problems, trial (randomised), mediator, voluntary sector. **Abbreviations:** FNN: Family Nurturing Network.

Conduct problems in children and young people are a serious, common and costly problem. A recent UK national survey, using ICD-10 criteria for conduct disorder, found a prevalence of 5% in 5–15-year-olds, with no evidence that these problems might be transient (Meltzer, Goodman, & Ford, 2002). Using less conservative definitions, studies in several countries have put the figure between 5 and 10%. An economic analysis showed that combined costs to health and social services, justice and education are up to ten times higher than for children without conduct disorder (Scott, Knapp, Henderson, & Maughan, 2001a). These problems can be detected early and tend to persist over time. They tend to develop in the context of harsh, inconsistent parenting, and low levels of positive parenting (Gardner, Sonuga-Barke, & Sayal, 1999; Gardner, Ward, Burton, & Wilson, 2003) and place young people at high risk for later problems, including school exclusion, delinquency, mental ill-

ness, partner violence, poor literacy, unemployment and raised mortality (Fergusson, Horwood, & Ridder, 2005; Robins, 1991; Rydelius, 1988).

There is clear evidence from randomised trials (Scott, Spender, Doolan, Jacobs, & Aspland, 2001b; Webster-Stratton, 1998a) and systematic reviews (Barlow & Stewart-Brown, 2000; Woolfenden, Williams, & Peat, 2001) that conduct problems can be prevented and treated with cognitive-behavioural parenting interventions. These interventions help parents to learn more effective skills, including reducing harsh, negative parenting, enhancing play and supportive interactions, and employing more consistent discipline and encouragement for good behaviour. However, many intervention trials have been carried out in specialist clinics, with only a handful located in 'real-world' child mental health services (Scott et al., 2001b).

Despite high levels of public concern about anti-social behaviour, and evidence of potentially effective treatments, surveys show that only a small propor-

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tion of children access services for conduct disorder (Ford, Sayal, & Goodman, 2005). Furthermore, despite many 'efficacy' trials, studies suggest that relatively few everyday services have a firm evidence-base. Therefore a key policy question is how services can reach larger numbers of families, through provision that is effective, yet accessible and low-cost. Recent policy guidance from the UK Department of Health (2004), National Service Framework, and from the US Centers for Disease Control (2004) recommend greater use of parenting interventions for preventing youth violence and conduct disorder. They stress the need for interventions to start early, and to be locally based and accessible, particularly given that families most at risk may find it hard to access conventional services. To achieve this, they emphasise partnership between health services and community-based organisations, including the voluntary sector.

In keeping with these policies, parenting interventions for troubled families and children are increasingly being provided by the voluntary sector, for example as part of the UK 'Sure Start' initiative. These typically aim to provide community-based services, in many cases to reach families who are marginalised. Voluntary organisations have distinctive goals and philosophies, and may bring remarkable energy and innovation into services. On the other hand, common challenges for the sector may include insecure funding and employment; partial reliance on volunteers; neighbourhood-based facilities which, although accessible for families, may be experienced by staff as dispersed, inconvenient and poorly equipped. Staff are less likely to have formal professional qualifications, raising issues about what is likely to be appropriate training and supervision for carrying out complex interventions.

Given these contextual differences between interventions in the voluntary sector and those in more specialised clinic settings, it is necessary to test whether evidence-based programmes can be translated into such settings, and still be effective. To our knowledge, this is the first randomised trial of a parenting programme in the voluntary sector, in this case, one that is aimed at children referred for conduct problems.

Testing effectiveness is vital for policy; however, investigators have pointed to the clinical and theoretical importance of examining intervention mechanisms (Rutter, 2005; Weersing & Weisz, 2002). Knowing which ingredients of a complex intervention predict outcome is important for refining implementation, and for testing causal hypotheses about parenting influence on child behaviour. Qualitative studies of parents' views suggest that key ingredients of intervention from a user's perspective may be increased social support and confidence gained from sharing problems with others in the group (Stewart-Brown et al., 2004). On the other hand, secondary analysis of randomised trials

(Forgatch & DeGarmo, 1999; Gardner, Shaw, Dishion, Burton, & Supplee, in press; Reid, Webster-Stratton, & Baydar, 2004) and longitudinal data (Gardner et al., 1999, 2003) suggest that change in observed positive parenting skill may be an important predictor of child outcome.

The aim of the present study is to test the effectiveness of an intervention with a strong evidence-base for reducing children's conduct problems, the Webster-Stratton 'Incredible Years' programme, delivered in multiple neighbourhood sites, by a voluntary sector organisation. Specifically we hypothesise that, for children referred for treatment of conduct problems, the intervention will be effective in reducing conduct problems and increasing parenting skill and confidence. We go on to explore putative intervention mechanisms, by examining change in parent mood and confidence, and observed positive parenting skill, as predictors of child problem behaviour outcomes.

## Methods<sup>1</sup>

### *The intervention*

The Webster-Stratton (Webster-Stratton, 1998a, 1998b; Webster-Stratton & Herbert, 1994) parenting programme employs a collaborative approach, building on parents' strengths and expertise. The sequence of topics includes parent-child play, praise, incentives, limit-setting, problem-solving and discipline. Video clips are used to illustrate different strategies parents use to manage children. Parents are encouraged to discuss their children's behaviour and role plays are used to find solutions and practise skills for managing their child. Each week parents practise tasks at home; telephone calls are made to encourage progress. The 14-week intervention was delivered weekly to groups of 10–12 parents in two-hour sessions. Partners and grandparents were encouraged to attend. Children did not participate, but were offered supervised child care.

*Setting.* The trial took place in a small charity, the Family Nurturing Network, specialising in offering Webster-Stratton's 'Incredible Years' intervention, and serving up to 200 families per year. Group interventions operated in nine sites across one county, including community and family centres and church halls, with a central office-base in Oxford. Sites for the trial were five urban sites with the highest numbers of referrals. Three sites were located in housing estates classified as the 10–20% most deprived UK wards (DETR, 2000). At all sites, referred families tended to live in conditions of social disadvantage (Table 1).

<sup>1</sup> Reporting conforms to CONSORT guidelines for randomised trials.

**Table 1** Participant characteristics at recruitment

	Intervention group N = 44	Control group N = 32
<b>Child</b>		
Gender (% boys)	34 (77)	22 (69)
Age (SD) Range 2–9	6.0 (2.1)	5.8 (1.9)
Eyberg problem score (SD) (cut-off 10)	20.8 (6.5)	20.3 (7.0)
<b>Family</b>		
Headed by single parent (%)	23 (52)	13 (42)
Father main carer (%)	2 (4.5)	2 (6.3)
Age main carer (SD)	31 (6.7)	30 (4.7)
Left full-time education age 16 or earlier (%)	34 (64)	18 (56)
Welfare benefits (%)	30 (68)	18 (56)
Rented accommodation (%)	30 (68)	22 (69)
SES of main earner (%)		
Non-manual	16 (36)	10 (31)
Manual	27 (61)	20 (63)
Never employed	1 (2)	2 (6)
<b>Depression (%)</b>		
Moderate – Severe	12 (27)	10 (31)
Severe depression	4 (9)	7 (22)
Sibling above Eyberg cut-off (%) (N = 43)	14 (54)	10 (59)

All group differences NS.

**Training and treatment integrity.** The programme was delivered by six trained group leaders, assisted by a co-leader. Leaders had backgrounds in nursery nursing, teaching, adult education, and the child and family voluntary sector, but none had specialist professional training or experience in child mental health. The supervisor was an experienced clinical psychologist. Supervisor and group leaders received training from accredited Webster-Stratton trainers over 6 months, consisting of workshops; training meetings; learning by acting initially as a co-leader; and weekly supervision. Training was then 'cascaded' down to a second and third generation of leaders. Ongoing training was also provided by Webster-Stratton who ran three workshops during the project period. The supervisor was involved in all training and supervision. To enhance treatment integrity, all intervention sessions were videotaped and viewed during weekly two-hour group supervision meetings. The manual was used extensively to guide group sessions, and in training and supervision. Group leaders adhered to protocols of content and teaching methods, including weekly use of video material, homework and role-plays.

### Design

In this randomised controlled trial, all families were included in the analysis according to trial allocation, irrespective of level of uptake of intervention. Families were randomly allocated to the next available parenting group in their area or to a wait-list control group. Control group families were offered intervention 6 months later. All assessments were conducted

in the home by researchers who were unaware of families' allocation. These took place at three points: time 1, pre-intervention and pre-randomisation; 2, post-intervention (6 months later); 3, follow-up (18 months post-recruitment). The main group comparison by allocation is made at post-intervention. At 18-month follow-up, the progress of intervention children is described on all outcomes. However, randomised comparison with the control group is no longer possible, as the wait-list period is ended.

**Randomisation method.** A computer-generated list was used for random allocation of families. The administrator, therapists and researchers were unaware of the randomisation sequence. The sequence was stored in numbered, opaque, tamper-proof envelopes, held by an administrator who was not involved with recruitment, therapy or evaluation. Following research assessments, names of newly recruited families were passed to the administrator, who allocated families strictly in order of recruitment, according to the next envelope in the sequence.

### Participants

**Inclusion criteria.** These included i) child aged 2–9; ii) referred for help with conduct problems; iii) score above clinical cut-off (>10 problems) on Eyberg 'problem' scale (Robinson, Eyberg, & Ross, 1980); iv) parent able to attend group and communicate in English. Exclusion criteria included i) child severely disabled; ii) in temporary care; iii) parent drug addicted; iv) previous attendance at FNN programme.

**Recruitment.** Families were referred by primary health care staff (37%), social workers (16%), or other professionals (18%). Twenty-nine per cent were self-referred. Following referral, FNN group leaders visited the home for a practitioner assessment and introductory meeting. The research was explained to eligible families. With their consent, families interested in the trial were visited by a researcher for full information and consent. Consenting families entered the trial, and the research assessment was completed. Randomisation took place after this baseline assessment. All parents who agreed to the initial research visit consented to participate in the trial.

Power calculations, based on means and SDs of Eyberg scores from a trial using the same intervention and primary outcome (Webster-Stratton & Hammond, 1997), suggested a total sample size of 44 (power .80,  $p < .01$ ) would suffice. To be cautious, given treatment effects might be weaker when translated to the voluntary sector, and to allow for attrition, we felt 76 would be adequate.

**Sample.** Seventy-six families were randomised; Table 1 shows characteristics of the sample by allocation. A high proportion were boys living in

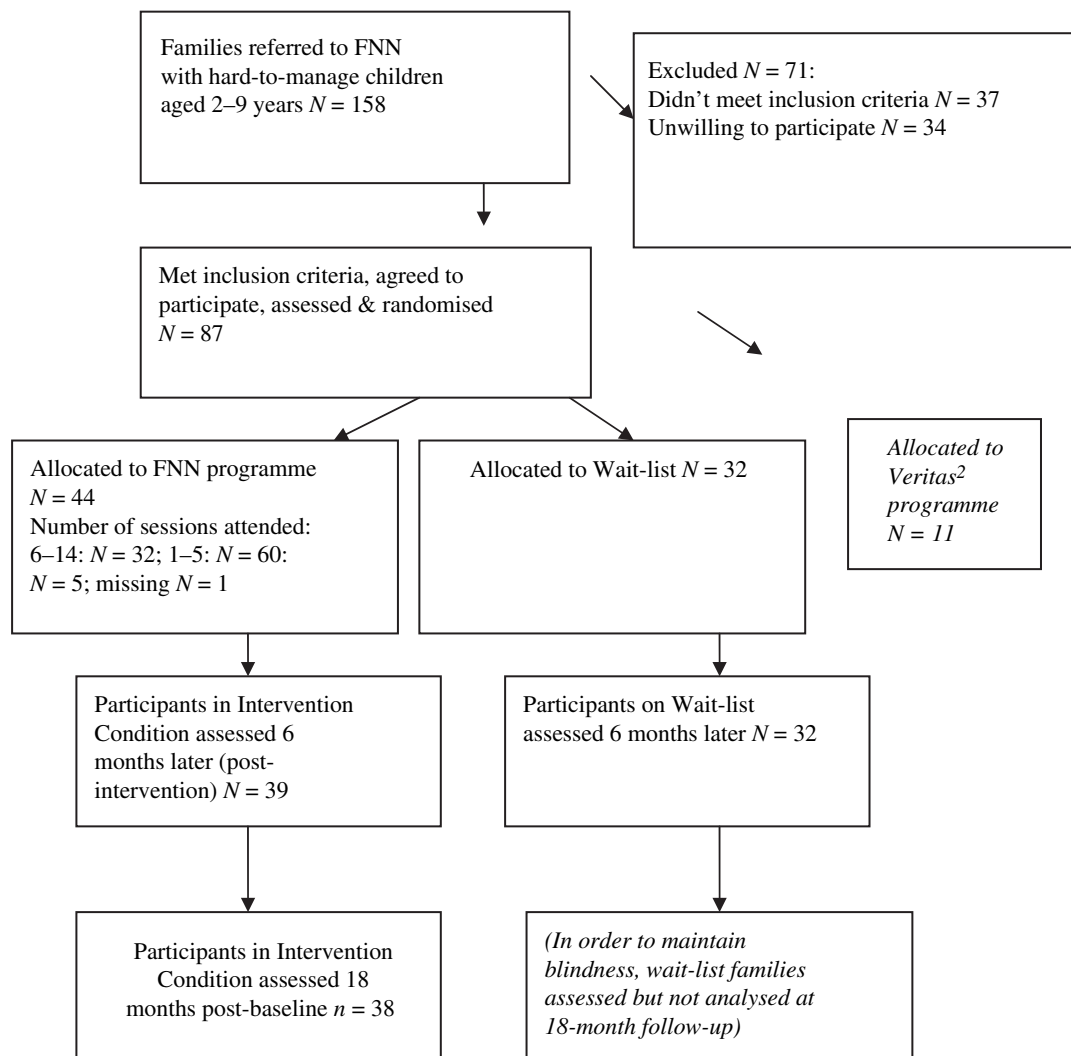
poverty, with a lone parent showing signs of depression. As expected, conduct problem scores were high (mean 21, SD 6.7, Eyberg problem scale, compared to clinical cut-off of 11, population average, 4–5; Robinson et al., 1980). Figure 1 charts the flow of families through the study.

### Overview of measures

All assessments were conducted at home, using standardised instruments, drawing on those validated in similar trials. Importantly, both parent-report and direct observations were used to measure child and sibling behaviour, parenting skill, confidence and mood. Procedures and measures were the same for each group and at each time point. Several strategies were used to enhance blindness of researchers: families were reminded by letter, phone and at each visit not to reveal intervention status.

Researchers did not administer consumer satisfaction questionnaires; these were mailed to a different researcher for analysis. Wherever possible, staff coded observation tapes of families they had not themselves visited.

*Observational procedures and measures.* Parent-child interaction was recorded using a small video-camera, in six structured settings in the home. These were designed to sample different types of everyday situations, allowing assessment of a range of child and parent behaviours in both pleasurable and more stressful settings. These involved varying degrees of task demand and amounts of parental attention, including: Child watches video for 5 mins, then has to switch it off; parent-child play with farm set, 10 mins; parent gets child to clear up, 5 mins; child plays skittles game, 5 mins; clears up, 5 mins; parent busy completing questionnaires, with



**Figure 1** Participant flow chart

<sup>2</sup> Originally the study was designed as a 3-arm trial, with allocation in equal proportion to FNN, control and to a second parenting intervention, commonly used in the voluntary sector, the 'Veritas'. After 6 months it proved impractical to recruit enough families in each centre to run two groups, and from then on, new families were allocated to FNN vs. control, in a 2:1 ratio. By using the same allocation list throughout, we ensured that families had the same probability of allocation to an intervention condition as in the original protocol. Eleven families allocated to Veritas in the early months of the trial were not included in the analysis

unstructured time for child, 20 mins. Parents were asked to prohibit TV and computers. Observations were coded from each 50-minute videotape, according to a coding system validated in studies of parenting and conduct problems (Gardner, 1987, 1989, 1994; Gardner et al., 1999, in press). Variables are event-coded categories, expressed as frequency per 50-minute session. These aimed to capture changes in: i) child problem behaviour, ii) positive and iii) negative parenting skills, that are the focus of the intervention. Inter-rater reliability checks on 20% of families showed high intra-class correlations (mean  $r = .95$ ), listed for each observational category below.

### Measures of child behaviour

*Eyberg Child Behaviour Inventory (ECBI)*. Thirty-six-item inventory of child conduct-problems. Both the 'problem' (i.e., total number of behaviours parent sees as problematic) and 'intensity' (i.e., frequency of occurrence) scales were used (Robinson et al., 1980).

*Observed child negative behaviour*. Defined as total frequency of behaviours: non-comply, hit, yell, destructive, rude, threaten. Intra-class correlation,  $r = .96$ .

*Observed child independent play*. Constructive solitary play by child; excludes wandering, fiddling, TV, computers (Gardner, 1987). Intra-class correlation,  $r = .94$ .

*Sibling behaviour problems*. Parent completed Eyberg scales for the sibling they saw as most difficult.

### Measures of parenting skill, confidence, mood

*Observed positive parenting*. Included praise, positive and proactive discipline (Gardner et al., 1999); joint play and talk, intra-class correlation,  $r = .95$ .

*Observed negative parenting*. Included hit, yell, rude, threaten, negative command, intra-class correlation,  $r = .97$ .

*Parenting Scale* (Arnold, O'Leary, Wolff, & Acker, 1993). Thirty items measuring parental discipline style and techniques; subscales include Laxness, Verbosity, Over-reactivity; total score reported here.

*Parent Sense of Competence* (Johnston & Mash, 1989). Sixteen items, subscales measure efficacy and satisfaction in parenting.

*Beck Depression Inventory* (BDI; Beck, 1972). Twenty-one items measuring depressive symptoms, correlates with clinical ratings and behavioural measures of depression. Score range 10–18 indicates mild-to-moderate, 19–29 moderate-to-severe, 30–63 severe depression.

*Consumer satisfaction, Therapy Attitude Inventory* (Brestan, Jacobs, Rayfield, & Eyberg, 1999). Ten-item scale assessing parental satisfaction with intervention, confidence about discipline and learning new skills.

## Results

### Analysis

*T*-tests for continuous, and chi-sq for categorical, variables revealed no significant differences between groups at baseline (Table 1) on demographic factors, parent-reported parenting skills, depression or child behaviour problems. However, since there was some variation between groups, particularly on observational measures, we controlled for baseline scores when analysing intervention effects, using ANCOVA, with baseline scores as covariates for each corresponding post-intervention score (Vickers & Altman, 2001). For skewed variables, analyses were repeated using a nonparametric test for group differences. As there is no nonparametric equivalent of ANCOVA, Mann-Whitney tests were used to compare the two groups on change scores from baseline to post-intervention. At 18-month follow-up the control group have been offered intervention, and can no longer be compared with the intervention group. Instead, maintenance of change, from post-intervention to 18-month follow-up, within the intervention group is examined, using paired *t*-tests and their nonparametric equivalent. Mediation analyses are described in the appropriate results section.

### Participant flow

Figure 1 shows participant flow and follow-up. All parents were offered intervention or waiting-list in accordance with their allocation. There was 93% retention at 6 months and 90% at 18 months. Families lost to follow-up did not differ significantly from those retained. Parents in the intervention condition attended an average of 9 (SD 5.1) parenting sessions. Information was missing for 4 parents. Of the remainder, 75% attended six or more, 12% attended 1–5, and 12% attended no sessions. One family in the control and none in the intervention group received other professional help during the intervention phase of the trial.

### Intervention effects post-intervention (Table 2)

*Child conduct problems*. ANCOVA comparing groups at time 2 indicated significant intervention effects on Eyberg problem and intensity scales, and on observed child negative behaviour. Using nonparametric tests on change scores for observational variables produced the same pattern of results. There were significant intervention effects on frequency of observed child independent play.

**Table 2** Intervention outcomes

	Baseline		Post-intervention			ANCOVA‡		18-month follow-up, maintenance of change Intervention group only		
	Mean	SD	Mean	SD	<i>N</i>	<i>F</i> , <i>p</i> -value	Effect size <sup>c</sup>	Mean	SD	<i>p</i> -value <sup>d</sup>
Child behaviour										
Eyberg Problem Score										
Intervention	20.8	6.5	12.4	7.8	37	4.1	.48	12.9	9.3	.96 ns <sup>a</sup>
Control	20.3	7.0	16.3	8.6	30	.05				
Eyberg Intensity Score										
Intervention	152.7	39.2	130.7	29.9	34	6.1	.55	134.0	41.0	.95 ns <sup>a</sup>
Control	156.1	32.9	148.5	34.7	26	.01				
Independent play‡										
Intervention	11.3	9.9	19.0	10.6	37		.77	14.8	11.0	.07 ns <sup>b</sup>
Control	18.6	10.9	17.1	11.6	29	.003				
Observed negative behaviour‡										
Intervention	58.5	50.6	30.3	28.6	37		.78	30.0	27.7	.65 ns <sup>b</sup>
Control	39.9	37.0	35.3	31.5	29	.02				
Parenting										
Parent Sense of Competence Total										
Intervention	51.2	9.3	60.0	11.8	37	4.9	.40	59.8	10.4	.55 ns <sup>a</sup>
Control	52.1	10.9	55.5	10.2	28	.03				
Parenting Scale Total										
Intervention	3.5	.63	3.1	.68	38	6.3	.65	3.1	.58	.89 ns <sup>a</sup>
Control	3.6	.71	3.5	.55	29	.01				
Beck Depression Total										
Intervention	15.5	10.7	11.7	11.3	39	.06	.35	10.3	9.0	.77 ns <sup>a</sup>
Control	20.4	11.3	15.5	10.7	28	.8 ns				
Observed positive strategies										
Intervention	29.6	12.6	36.8	11.3	37	4.4	.38	36.3	13.7	.71 ns <sup>a</sup>
Control	31.1	13.1	32.3	13.1	29	.04				
Observed negative strategies‡										
Intervention	22.2	24.1	8.8	10.1	37		.74	9.7	8.9	.24 ns <sup>b</sup>
Control	12.6	12.4	13.0	14.0	29	.003				

Main analyses use ANCOVA with baseline score as covariate, except: ‡ where distributions skewed: Mann-Whitney-test on change scores baseline to post-intervention.

<sup>a</sup>Paired-samples *t*-test, post-intervention to 18-month follow-up, intervention group only.

<sup>b</sup>Equivalent nonparametric test, post-intervention to 18-month follow-up, intervention group only.

<sup>c</sup>Cohen's *d* effect size: small (.15-.40); medium (.40-.75); large (>.75)

<sup>d</sup>Non-significant (ns) *p*-value represents no change (i.e., maintenance of treatment effect) at 18-month follow-up.

*Parenting skill, confidence and mood.* ANCOVA comparing groups at time 2 indicated significant intervention effects on parents' observed use of negative strategies, as well as increases in positive parenting. Using nonparametric tests on change scores for observational variables produced the same pattern of results. ANCOVA showed significant intervention effects on parents' sense of competence. Examining the two subscales separately, there was an effect on parents' sense of efficacy, but not on feelings of satisfaction as a parent. There were also effects on self-reported parenting skills, including Parenting Scale total score, particularly the 'reactivity' scale, which measures harsh, coercive style. There were no intervention effects on parent depression.

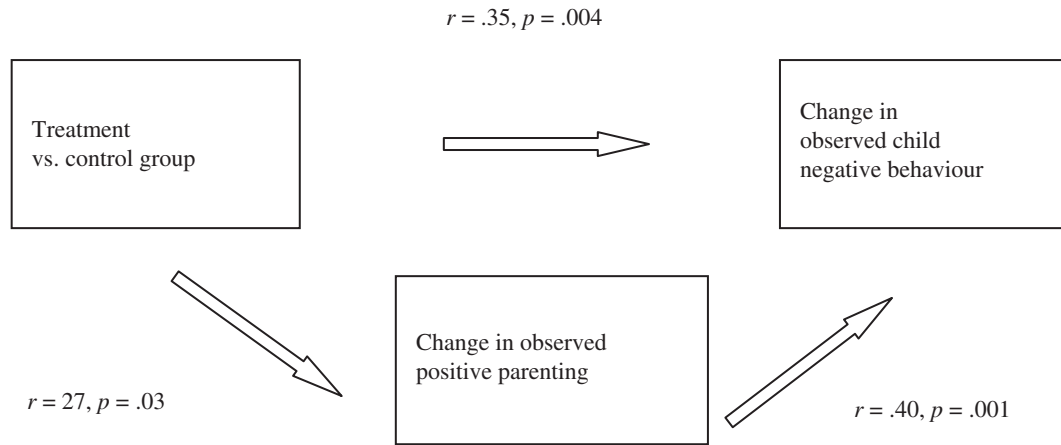
*Sibling behaviour problems.* Data were available for 31 siblings (20 girls, mean age 6 years, range 2-13). ANCOVA comparing groups at time 2 indicated intervention effects on sibling Eyberg problem scores,  $p = .03$ ,  $ES = .68$ .

*Maintenance of change at 18-month follow-up, intervention group (Table 2).* On all measures, changes were maintained at follow-up, as evidenced by no significant change from post-intervention to follow-up on within-group paired-sample *t*-test.

*Consumer satisfaction.* Ninety-one per cent of intervention parents ( $n = 42$ ) liked the group, 75% felt behaviour problems had improved, 97% felt they had learned useful skills, 88% felt the group helped with other family problems.

#### *Are intervention mechanisms identifiable?*

We tested whether observed positive parenting is a mediating mechanism for treatment change. First, it is necessary to show that all three variables are correlated (Baron & Kenny, 1986). Figure 2 shows this is the case; improvement in observed positive parenting correlated with improvement in observed child negative behaviour ( $r = .40$ ,  $p = .001$ ); treatment status correlated with change in positive



**Figure 2** Mediation model

parenting ( $r = .27, p = .03$ ) and change in child negative behaviour ( $r = .35, p = .004$ ). Positive parenting thus conforms to current definitions of a mediator within randomised trials (Kraemer, Wilson, Fairburn, & Agras, 2002). Secondly, hierarchical multiple regression was employed, with change in child negative behaviour as the DV. Table 3 shows IVs entered as follows: Step 1, effect of treatment on negative behaviour, and Step 2 shows that that this effect is attenuated when positive parenting is introduced as a potential mediating variable. The significance of this change was tested using the PRODCLIN program, a powerful test of mediation effects (MacKinnon, Fritz, Williams, & Lockwood, in press), and showed a significant partial mediation effect ( $p < .025$ ). Change in child negative behaviour was not correlated with change in parent sense of competence ( $r = .17, NS$ ) or depression ( $r = .02, NS$ ). Thus parent confidence and mood variables were not further investigated as mediators.

## Discussion

These findings show that the Incredible Years parenting programme can be effectively translated into a community-based service in the UK voluntary sector, for reducing conduct problems in referred children. Using both parent-report and direct observational methods, significant intervention effects were found on child problem behaviour, play, sibling behaviour, positive and negative parenting. Compared to sam-

ples of referred children in other parenting treatment trials (Scott et al., 2001b; Taylor et al., 1998; Webster-Stratton & Hammond, 1997), our sample showed similar or slightly higher levels of social disadvantage and problem behaviour, and broadly comparable medium-to-large effect sizes.

In this waiting-list control group design, it was not possible to compare the randomly allocated intervention group with controls beyond the first follow-up. However, we were able to examine, in an uncontrolled comparison, maintenance of change at 18-month follow-up. On all measures that showed intervention effects from 0–6 months, no significant change was found in the reverse direction, from 6–18 months, suggesting the changes in child and parent behaviour were maintained, and were not merely transient effects.

Change in positive parenting skill appeared to partially and significantly mediate change in observed child problem behaviour, whereas change in parent mood or sense of competence did not contribute to child outcome. These data are important in providing a direct test of the notion that skill change may be the most salient ingredient of effective parenting programmes (Hutchings, Lane, & Gardner, 2004), rather than changes in parental mood or confidence. These findings are somewhat at odds with qualitative data from users, who tend to perceive these non-specific factors as crucial, perhaps because they contribute to enjoyment and a sense of engagement in the programme, but not necessarily to behaviour change. Mediation effects are rarely studied in intervention trials (Rutter, 2005), but are vital for understanding processes underlying treatment, and causal influences on child behaviour. These findings add to a body of work suggesting that positive parenting may play a causal role in development and change in children's conduct problems (Gardner et al., 1999, 2003, in press).

Although numbers were small, findings for sibling behaviour are noteworthy, as few studies have reported impact on other family members. Moreover, they suggest that more than one child per family may

**Table 3** Regressions for mediation analysis. Dependent variable = change in observed child negative behaviour

Variable	Adjusted $R^2$ , sig. $F$ change	Beta	$t$ , 2-tailed $p$ -value
Step 1	.11, $p = .004$		
Treatment vs. control		.35	3.0, $p = .004$
Step 2	.20, $p = .006$		
Treatment vs. control		.27	2.3, $p = .02$
Change in observed positive parenting		.33	2.9, $p = .006$

benefit from a group parenting intervention. These effects may be due to the fact that parents were encouraged to use strategies consistently for all children in the family. The lack of effect on maternal depression was surprising, as many trials have found benefits (Barlow, Coren, & Stewart-Brown, 2003). It may be that some depressed parents need additional help from a programme focusing specifically on depression, such as the Webster-Stratton (1998b) ADVANCE training. In the present study, there is no suggestion that moderate depression prevented parents from making changes in parenting and child problem behaviour. Many parents were depressed, yet, in the intervention group, there was no correlation between high initial level of depression and low change in child conduct problems ( $r = +.25$ , NS, trend toward more depressed parents improving more), or positive ( $r = .05$ ; NS) and negative ( $r = .06$ ; NS) parenting. Although depression scores may not have changed, there is no reason not to include depressed parents in such interventions, as depression does not appear to impede their ability to make important changes in family interaction.

The study has a number of strengths. All participants who could be traced were included in the analysis, irrespective of their uptake of the intervention. Steps were taken to enhance allocation concealment. Outcomes were measured using two distinct, independent sources, parent-report and home observation of parent-child interaction, during 50-minute sessions. This means that this study is the first independent replication of the Webster-Stratton programme for treating children with conduct problems, using observational assessment of child and parenting outcomes on the whole sample. Observational measures are vital for providing an independent assessment of outcome, in a field where participants cannot be blind to their intervention condition. Where measures are collected only from participants, there is a higher risk of bias. At the same time it is recognised that even with observational measures, complete blindness is rarely possible. We took a number of steps to maximise blindness of our coders to intervention status of the families. One strategy was to use repeated reminders to families not to reveal allocation by any means, but despite this, one videotape showed an attendance certificate from the programme on the wall. Limitations include a modest sample size; however, it is comparable with many other treatment trials for children with identified conduct problems (Webster-Stratton & Hammond 1997). Despite extensive tracing efforts, 7% were lost to follow-up, and for other families there were missing data on some outcomes; however, these families did not appear to differ from those retained in terms of demographics or level of behaviour problems. There were some baseline differences between groups, which, in the case of outcome variables, were controlled for in the analysis. However, only one baseline difference was significant

(observed child independent play). In a small sample, where careful steps were taken to ensure allocation concealment, these differences are likely to be due to chance. Furthermore, since allocation took place after the baseline assessment, differences are unlikely to be due to researcher bias.

In terms of policy implications, these findings add to a growing body of knowledge suggesting that a well-structured parenting intervention, the Incredible Years programme, based on collaborative and cognitive-behavioural principles, can be translated across cultural groups (Reid, Webster-Stratton, & Beauchaine, 2001), countries (Scott et al., 2001b; Mørch et al., 2004) and service settings. Moreover, it can help socially disadvantaged families whose children have high levels of conduct problems. It is important to note that staff in this study, although lacking a specialist mental health background, were well trained and supervised weekly. As programmes are diffused more widely into community and voluntary sector settings, it is vital to know that they are still effective; this is all the more important given evidence that reminds us that family-based programmes can do harm as well as good, even with well-meaning interventions, which are highly acceptable to clients (Dishion, McCord, & Poulin, 1999).

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