

Social Learning Theory Parenting Intervention Promotes Attachment-Based Caregiving in Young Children: Randomized Clinical Trial

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Parenting programs for school-aged children are typically based on behavioral principles as applied in social learning theory. It is not yet clear if the benefits of these interventions extend beyond aspects of the parent–child relationship quality conceptualized by social learning theory. The current study examined the extent to which a social learning theory–based treatment promoted change in qualities of parent–child relationship derived from attachment theory. A randomized clinical trial of 174 four- to six-year-olds selected from a high-need urban area and stratified by conduct problems were assigned to a parenting program plus a reading intervention ($n = 88$) or nonintervention condition ($n = 86$). In-home observations of parent–child interactions were assessed in three tasks: (a) free play, (b) challenge task, and (c) tidy up. Parenting behavior was coded according to behavior theory using standard count measures of positive and negative parenting, and for attachment theory using measures of sensitive responding and mutuality; children's attachment narratives were also assessed. Compared to the parents in the nonintervention group, parents allocated to the intervention showed increases in the positive behavioral counts and sensitive responding; change in behavioral count measures overlapped modestly with change in attachment-based changes. There was no reliable change in children's attachment narratives associated with the intervention. The findings demonstrate that standard social learning theory–based parenting interventions can change broader aspects of parent–child relationship quality and raise clinical and conceptual questions about the distinctiveness of existing treatment models in parenting research.

As a result of many successful trials of social learning theory–based parenting interventions, the field has moved beyond questioning whether or not such interventions can be effective to considering broader issues about the generalizability of effects and the mechanisms of change. The aim of the current study is to examine if

the improvements in parenting from a standard social learning–based intervention generalized to parental sensitivity and children's attachment representations, two features of an attachment theory–based assessment model that were not targeted by the intervention. In addition, we examine if changes in parental sensitivity

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might be a mechanism for understanding change induced by the intervention.

Social learning theory proposes that children's real-life experiences and exposures directly or indirectly shape behavior; processes by which this learning occurs can be diverse, and include imitation and reinforcement (Gardner, Burton, & Klimes, 2006; Hood & Eyberg, 2003; Kazdin, 2005; Patterson, DeBaryshe, & Ramsey, 1989; Scaramella & Leve, 2004; Stormshak, Bierman, McMahon, & Lengua, 2000; Wahler & Meginnis, 1997). Children's strategies for managing emotions, resolving disputes, and engaging with others are learned from experience and carried forward across setting and time. For younger children especially, the primary source of these experiences is the parent-child and family relationship environment. Accordingly, interventions to improve child behavior have focused on altering the quality of parenting. Specific parenting behaviors targeted for assessment and treatment are positive attention and praise for the child's desirable behavior, contingency of parental response, directions and instructions that are clear and set limits on undesirable behavior, and criticism. Numerous randomized clinical trials show that parenting interventions that improve these parenting dimensions can reduce child negative behavior (DeGarmo, Patterson, & Forgatch, 2004; Dumas, 1984; Forgatch & DeGarmo, 1999; Reid, Webster-Stratton, & Beauchaine, 2001; Scott & O'Connor, in press; Scott, Sylva, et al., 2010; Webster-Stratton, 1984; Webster-Stratton, Reid, & Hammond, 2004).

Attachment theory (Ainsworth, Blehar, Waters, & Wall, 1978; Bowlby, 1978; Cicchetti, Rogosch, & Toth, 2006; Sroufe, Carlson, Levy, & Egeland, 1999) is an alternative model for understanding the nature and origins of parent-child relationships and the mechanisms by which they shape psychological development of the child. Not surprisingly, then, an attachment theory-based assessment and treatment model differs from social learning theory. According to attachment theory, children who experience sensitive-responsive caregiving will, over time, internalize a sense of the attachment figure as responsive and available and of themselves as worthy and lovable; these children are described as developing a secure internal working model of the self and other. In contrast, children who have experienced insensitive parenting develop an insecure working model and an insecure base for exploration. In the current study we target parental sensitive responding behavior and the child's internal working model as core components of an attachment theory-based parenting assessment.

Differences between social learning and attachment theory are more conceptual rather than empirical in nature (O'Connor, 2002; Scott, Briskman, Woolgar, Humayun, & O'Connor, 2011; Speltz, DeKlyen, Greenberg, & Dryden, 1995). The possibility that the

two models may be reconciled is found in a recent meta-analysis of attachment interventions (Bakermans-Kranenburg, van IJzendoorn, & Juffer, 2003). They reported that the most positive attachment outcomes were found in studies using specific behavioral techniques. That finding may imply more about technique than conceptual model but does underscore the need for further research on the distinctiveness of interventions and the mechanisms of change implied.

The question we ask in this study is whether or not a standard social learning theory intervention produces change in relationship constructs central to attachment theory that were not targeted by the intervention. Research of this kind is conceptually important because it tests the conceptual-methodological overlap between theories. For example, if social learning and attachment relationship constructs were distinct, then it might follow that the social learning theory-based intervention effects might not extend to attachment theory-based measures. An alternative possibility is that, if there were change in both social learning and attachment theory measures, then there might be little overlap in changes observed in each type of measure; on the contrary, there might be substantial overlap in change observed because changes in one set of constructs and measures would not be distinguishable from change in the other. We consider these possibilities in the current study.

In addition to examining change in specific parental behaviors, we provide a further test of the broader impact of a social learning theory intervention on attachment outcomes by assessing the security of children's internal working model. A child's internal working model, which is reliably assessed through established paradigms (Bretherton, 1988; Futh, O'Connor, Matias, Green, & Scott, 2008; Schechter et al., 2007), is thought to be a mechanism accounting for why children who experience sensitive/responsive care display greater emotion regulation and social competence. Limited evidence exists that children's internal working models are altered by interventions, with the only existing examples deriving from attachment-based treatments (Toth, Maughan, Manly, Spagnola, & Cicchetti, 2002). The current study examines if children's internal working models are altered by a social learning theory-based parent training program that does not target this specifically and, moreover, does not include the child in the treatment setting.

More practically, research of the type proposed here is significant because it addresses the clinical concern sometimes expressed that parent training programs would have little impact on the dynamic and mutual parent-child *relationship*. That is, reluctance of some clinicians to endorse social learning theory-based programs derives from the suspicion that changes in parenting behavior brought on by the training would be limited to superficial acts somewhat mechanically performed, which would

not impinge upon more fundamental aspects of the relationship. We test that directly in the current study.

A further component of the current study's focus on generalizability of treatment effects is a consideration of whether or not gains in parenting quality are observed across multiple interaction tasks. Observational methods are the "gold standard" for assessing parent-child relationship quality in treatment studies. In the current study, we employ a standard practice of assessing parent-child interactions pre- and posttreatment across diverse tasks. These interaction tasks—free play, structured challenge task, and tidy-up session—are commonly employed because they vary in the demands put on the parent and child. Sampling behavior across different tasks would, in principle, provide a more reliable representation of parenting change than any single task (Kotler & McMahon, 2004). However, with few exceptions, studies assessing child behavior or parent-child interactions in multiple settings collapse ratings across interaction tasks, despite the significant mean differences in target behavior that exist (Wakschlag et al., 2008). If there are sizable differences in the extent to which interaction tasks elicit target parent and child behaviors, then it would follow that there may be differences across task in the extent to which parent behavior is altered by the intervention. A novel feature of this study is that we consider if treatment effects are equally evident across diverse tasks that vary in the structure and demand they impose on the dyad. We test the exploratory hypothesis that changes in interaction quality would be more evident in the less structured compared to the more experimentally structured settings.

We previously reported that a social learning theory-based intervention was effective in improving standard social learning theory-based measures of parenting in an ethnically diverse, psychosocially deprived sample (Scott, O'Connor, et al., 2010). The current article expands that report by testing the novel hypothesis that the parenting improvements would extend to attachment-based assessment model and by examining the generalizability of parenting improvement across diverse interaction settings.

METHOD

The trial took place from 2001 to 2004 in all four primary schools in the most disadvantaged ward within a deprived inner-city London borough. All 672 reception (kindergarten equivalent) and Year 1 (Grade 1 equivalent) pupils were screened for emotional and behavioral difficulties by questionnaire, yielding 665 (99%) teacher reports and 532 (79%) parent reports. The screening measure used was the Strengths and Difficulties Questionnaire (Goodman & Scott, 1999), supplemented

by the eight *Diagnostic and Statistical Manual of Mental Disorders* (4th ed. [DSM-IV]; American Psychiatric Association, 1994) items used to diagnose oppositional-defiant disorder. Parent and teacher scores from the conduct items of the Strengths and Difficulties Questionnaire and the oppositional-defiant disorder items were summed. The cutoff for high risk was a total score of five on the Strengths and Difficulties Questionnaire conduct items or 10 on the *Diagnostic and Statistical Manual* items, corresponding to antisocial behavior reached by the highest 18% of the population in England; those who scored below were deemed low risk. A consort diagram of the study sample and means for conduct symptoms by reporter is provided as Figure 1.

Method of Randomization and Concealment

This was a group randomized controlled trial; random allocation of classrooms to condition was carried out by a statistician independent to the project using the permuted block randomization method. The four schools had eight classes, so over 3 years a total of 24 classes were randomized. Each had an average of 28 pupils, giving 672 children in the study. All families in the class were assessed for eligibility criteria using information supplied by the class teacher: ability to understand English and absence of clinically apparent severe global developmental delay. This led to 16 children being excluded: 10 on language grounds and six due to delay. Recruitment to the trial was as follows. There were five to nine high-risk children per class, who were randomized in a ratio of 2:1 to be approached for the study. There were 18 to 23 low-risk children per class, who were randomized in a ratio of 1:2. There were 233 parents who were sent letters describing the study and inviting them to participate; 174 (75%) agreed. Written informed consent was obtained from parents; the local research ethics committee approved the study.

Recruitment to the parenting groups was conducted by the intervention team. An invitation letter was sent to parents, who were invited to coffee mornings held at the school to describe the groups. Where possible, parents of high-risk children were met personally. The strategy of including equal numbers of parents of high- and low-risk children was intended to maximize recruitment of children at risk while minimizing stigma. The vast majority (>90%) of primary caregivers participating in the study were mothers; too few fathers or grandparents participated to justify separate analyses, and so we combined all caregivers in analyses presented next.

Procedure

All pre- and posttreatment parent-child interaction data were collected from home visits by two trained developmental researchers blind to intervention status;

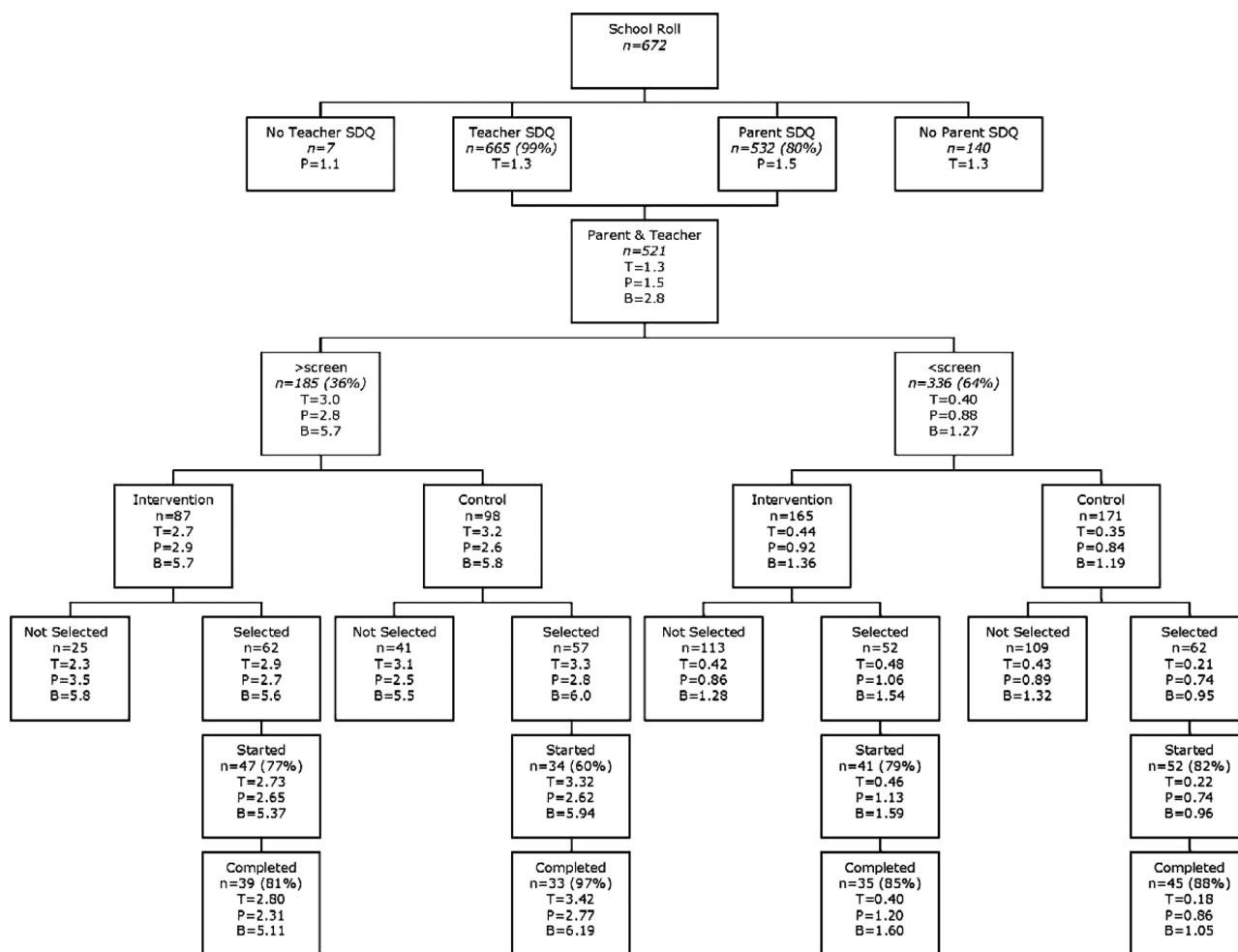


FIGURE 1 Consort diagram for primary age learning skills treatment trial with means (by reporter) of conduct symptoms. *Note:* The numbers of people who declined to take part is the difference between the number “selected” and “started,” 25% overall. There were 272 of the 288 “not selected” due to the randomization process, 10 due to inadequate English, and six due to marked developmental delay, in similar proportions across the four groups. T = the score on the Teacher Strengths and Difficulties Questionnaire (SDQ) Conduct Problems scale (range = 0–10); P = score on the Parent SDQ Conduct Problems scale (range = 0–10); B = the sum of both teacher and parent scores; Selected = selected by randomization to be offered the opportunity to take part in the study; Started = consented to be part of the trial.

follow-up measures were collected 1 year later (approximately 6 months after the end of treatment), when 152 families of the original sample size of 174 families (87%) were successfully reassessed (Figure 1).

During the home visit the primary caregiver and the child were videotaped during three commonly used observational tasks with early school-aged children. The first task was a 10-min free play session with a designated set of toys brought by the interviewer; no specific instructions were given to the parent by the experimenter except “to play as you normally would.” The second task was a 10-min highly structured challenge in which parent and child were instructed to construct a difficult LEGO object from a picture; in this task the parent was not allowed to touch the LEGO bricks (i.e., only verbal instruction was allowed). The third task was a toy clean-up session in which the first 5 min were coded; minimal instructions

were provided to the parent (“We’d like you to get your child to tidy up the toys before we move on to the next thing”). Coders who rated the parent–child home observations were blind to all identifying information and did not take part in the home observation data collection. All coders had at least a college degree and several years of research experience or some graduate training in psychology. Training in the observational rating systems (see next) were carried out by senior investigators with considerable experience with each system; training continued throughout the study period to prevent coder drift. Each coder provided ratings for each of the three interaction tasks. On a separate visit, which took place in the child’s school, children were individually administered the Manchester Child Attachment Story Task (see next) in a private setting with a trained developmental researcher.

Intervention

Eighteen sessions were offered, interleaving a 12-week parenting program with a 6-week literacy program.

Parenting program. The parenting program was the basic, 12-week Incredible Years (IY; Webster-Stratton, 1984) school-age program, which addresses the parent-child relationship and child behavior by intervening with the parents in a group format. IY focuses on how parents can bring the best out of their child; it includes observing videotapes showing scenes of parents and children in a variety of common situations, with the parents sometimes behaving in a way that leads to the child being calm and obedient and sometimes to being miserable and having tantrums; parents and child from diverse ethnic groups are included in the videos. Through observation and group discussion, the elements of parental behavior that lead to successful child outcomes are drawn out. Then parents practice the new techniques in role-plays, are instructed to practice the new skills at home, and are telephoned by the group leader midweek to solve difficulties.

Literacy program. This was a shortened 6-week version of the SPOKES manualized program (Sylva, Scott, Totsika, Ereky-Stevens, & Crook, 2008). It begins with a "whole language" approach, where parents are encouraged to discuss the child's book and link the text to the child's everyday experiences. They are encouraged to play rhyming games with their children 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 s; if the child does not succeed, the parent gives a specific prompt and then praises the child for complying. Other elements included role-play, homework, and a home visit.

Group treatment model. Each of the 11 groups of eight to 10 parents consisted of a leader and a coleader. The main leader (for eight groups) had a psychology degree and a master's in child development. She was trained in IY by (a) attendance at a 3-day accredited training, (b) observation of a 12-week group, with (c) attendance at weekly supervision led by mentors, (d) leading eight groups, and (e) accreditation from the program developer. The leader for the remaining three groups had a psychology degree and training in the program but not experience prior to the trial, or certification. Coleaders were child mental health professionals in training without certification or trainees with psychology degrees. Treatment fidelity was emphasized and was addressed by (a) training described previously, (b) completing treatment adherence schedules weekly, (c) acting

on weekly feedback from group participants, and (d) weekly supervision meetings with an IY mentor. During supervision, videotapes of the last group were shown and therapeutic techniques discussed and practiced. The aim of shortening the program to a maximum of 18 weekly sessions was instituted so it would last one and a half school terms and could be run twice a school year, making it less costly to run than our previous 28-week program from which it was adapted (Scott, Sylva, et al., 2010).

Control Group

No active intervention was offered.

Help Available to All Participants in Both Arms of the Trial

For all families, a general practitioner, school-based drop-in service, and specialist mental health service were available.

Measures

The Coding of Attachment-Related Parenting (Matias, Scott, & O'Connor, 2006) is a global measure of parent-child interaction quality that was derived from attachment theory-based and related assessments in young school-aged children (Kochanska & Aksan, 2004; Kochanska & Murray, 2000) that has been used in prior research (Bisceglia et al., 2012). Level and intensity were considered in deriving a score on a 7-point Likert scale. Two attachment-related parenting behaviors are the focus of this report: (a) Sensitive Responding, which assesses the degree to which the parent shows awareness of the child's needs and sensitivity to his or her signals, promotes the child's autonomy, adopts the child's psychological point of view, and physically or verbally expresses warmth toward the child, and (b) Mutuality, which reflects the degree to which each member of the dyad seems to willingly accept and seek the other's involvement in a joint activity, build on each other's input and coordinate their efforts/actions while conducting a task together, maintain shared attention and fluid conversation, reciprocate positive affectionate behaviors, and keep physical proximity/closeness when interacting with one another. Data supporting the validity of the Sensitive Responding and Mutuality scales include (a) 1-year stability of intraindividual differences (in the nontreatment condition, 1-year stabilities were $r = .66$ for Sensitive Responding and $r = .56$ for Mutuality), (b) significant correlation with security of the child's attachment narrative from a story stem procedure ($r_s = .32$ and $.20$, $p < .05$, for Mutuality and Sensitive Responding, respectively), and (c) prediction of peer nomination

ratings of being liked by peers ($r_s = .27$ and $.29$, $p_s < .05$, for Mutuality and Sensitive Responding, respectively; Matias, Scott, & O'Connor, 2006). Interrater reliability was conducted on 30 tapes; intraclass correlations were $.73$ for Sensitive Responding and $.81$ for Mutuality.

The Parent Behavior Coding Scheme (Aspland & Gardner, 2003; Scott, Sylva, et al., 2010) is based on existing social learning-based observational measures of parent behavior such as the Behavior Coding Scheme (Forehand & McMahon, 1981). The Parent Behavior Coding Scheme is an event-based observational measure in which specific parenting behaviors are coded as frequency counts; the frequency of each coded behavior was adjusted by the total time to create a scale of behaviors per minute. Specific parenting behaviors were grouped into child-centered behaviors and child directive behaviors. Child-centered behaviors were (a) praising the child's behavior, (b) attending to and commenting on the child's activities, (c) making encouraging comments to the child, (d) putting requests to the child as questions in the conditional sense, (e) seeking the child's cooperation by induction, (f) making facilitative remarks, and (g) making plausible reference to the child's thoughts or feelings. Child-directive behaviors were (a) clear commands of the child, (b) vague or uncertain commands to the child, (c) prohibitions, and (d) criticisms. Child-centered and child-directive behaviors formed two factors accounting for 29% and 24% of the total variance, respectively. Scores were standardized before being composited into the child-centered and child directive factors. The median intraclass correlation across codes, based on a set of 30 tapes, was $.75$.

The Manchester Attachment Story Task (Green, Stanley, Smith, & Goldwyn, 2000) is a narrative story stem task to elicit attachment representations in young school-age children. Using dyadic play scenarios with a target child doll, mother doll, and dollhouse, the child's attachment representations are evaluated from doll characters' behavior and the organization and coherence of the child's narrative to four story stems (nightmare, hurt knee, feeling ill, and being lost in a store). Following the story completion, the interviewer uses structured probes that help in clarifying the intention, degree of assuagement and mental state attributions behind the play. A detailed coding manual is used to score videotapes. The rater makes an overall determination of attachment classification coded as Secure, Insecure-Avoidant, Insecure-Ambivalent, and Insecure-Disorganized. Previous studies support the construct validation of the measure (Futh et al., 2008; Green et al., 2000). On 20 randomly selected tapes from the current sample, average intraclass correlation was $.5$ and there was 80% agreement on the four-way classification ($\kappa = .66$, $p < .01$).

Information on parental education, family structure, income, and ethnicity were derived from parent interview.

Statistical Analysis

After reporting descriptive data, we report correlations and means for parenting measures across task. Treatment effect results for observed parent-child interaction behavior and attachment narratives are reported using an intention-to-treat and last value carried forward approach; this is among the most conservative approaches for analyzing treatment effects. Treatment effects on observed parenting behavior are tested using the analysis of covariance method, with pretreatment scores and high-risk status (because of its role in sampling) included as covariates. Effect size (ES) estimates are derived from the difference in pre- and post-treatment means between the groups divided by the pooled standard deviation. We then test the hypothesis that changes in social learning theory-based measures mediated change in attachment-based measures using the joint significance test (MacKinnon, Fairchild, & Fritz, 2007). We focus on social learning theory measures as a mediator because they are targeted by the intervention, whereas the attachment theory measures are not. Supplementary analyses were carried out for children whose parents attended five or more of the 18 sessions to examine if those parents who received at least a minimal exposure to the intervention changed (a "per protocol" analysis); supplementary analyses also examined if the treatment effects varied by ethnic background. Preliminary analyses using multilevel model analyses indicated that there was no variance in parenting variables at the classroom level or group leader level (i.e., clustering). Accordingly, we analyze the data using the standard approach just noted.

RESULTS

Demographic data (Table 1) indicate that the sample was ethnically diverse and at high risk for social problems, but there were no differences between the intervention and comparison groups on these risk factors. It was not possible to obtain in-home observational data in every case; as a consequence of a variety of practical and logistical limitations, we were able to obtain valid observational data on 145 of the sample (83%) at pretreatment. In addition, of the 174 participants who began the trial, we obtained some postintervention data on 152 (87%), including 141 at the 1-year follow-up. Of the 22 families for whom no postintervention data were available (i.e., 174 – 152), 14 were no longer on the school roll and had moved away and the remaining eight said they were now too busy with work or too ill. Compared to those on whom we gathered some postintervention data, those whom we were not able to contact did not differ significantly on ethnicity, pretreatment conduct problem

TABLE 1
Participant Characteristics

	Intervention ^a	Control ^b	M Values for England
Child Age (M in Months)	66.4 (5.9)	65.7 (5.5)	
Child Male	49% (43)	44% (38)	51%
Primary Caregiver Ethnicity			
White British	24% (21)	24% (21)	
Black African	43% (38)	48% (41)	
Black African-Caribbean	22% (19)	15% (13)	
Other	11% (10)	13% (11)	
Total in Minority	76% (67)	76% (65)	9%
Lone Parent	56% (49)	50% (43)	22%
Mother Ended Education by 16 Years	24% (21)	26% (22)	13%
Council or Housing Association home	82% (72)	77% (66)	17%
Household Income £175 per Week or Less	43% (38)	34% (29)	5%

^an = 88.
^bn = 86.

score, or parenting measures. The average number of sessions attended in the intervention group was five (SD = 5.7), with a median of two and a range of zero to 18.

Correlations between pretreatment parent-child relationship measures are provided in Table 2; means (standard deviations) for each measure are also provided. Findings show that there is modest to moderate overlap between child-centered and Sensitive Responding and Mutuality; correlations between these scales and child-directive behavior are negligible. Table 2 also shows that, despite similarity in rank order of parenting

across task, the means for each of the parenting measures varied modestly to substantially across the three tasks settings at pretreatment. For child-centered parenting there was a significant multivariate effect of interaction task, $F(2, 142) = 46.50, p < .001$. Follow-up paired *t*-test analyses indicated that all three comparisons were statistically significant: free play > LEGO, $t(143) = 6.11, p < .001$; free play > tidy up, $t(143) = 9.68, p < .001$; LEGO > tidy up, $t(144) = 5.34, p < .001$. Similarly, for child-directed parenting behavior, there was a significant multivariate effect of interaction task, $F(2, 142) = 82.05, p < .001$. Follow-up paired *t*-test analyses indicated that all three comparisons were significant: free play < LEGO, $t(143) = -12.73, p < .001$; free play < tidy up, $t(143) = -6.95, p < .001$; LEGO > tidy up, $t(144) = 7.75, p < .001$. Across-task variation was also substantial for Sensitive Responding, $F(2, 141) = 13.09, p < .001$. Follow-up paired *t*-test analyses indicated significant differences: free play > tidy up, $t(143) = 2.91, p < .01$, and LEGO > tidy up, $t(142) = 5.11, p < .001$. Last, Mutuality also showed substantial variation across task, $F(2, 141) = 34.28, p < .001$. Follow-up paired *t*-test analyses indicated significant differences: free play > tidy up, $t(143) = 7.00, p < .001$, and LEGO > tidy up, $t(142) = 7.49, p < .001$. The sizable differences across task in parenting behaviors support the analyses of change in behavior across task.

Intervention Effects across Task: Social Learning Theory Measures

Means (standard deviations) for child-centered and child-directive parenting measures across Task × Treatment condition are provided in Table 3.

TABLE 2
Correlations Between Parenting Constructs Across Task: Pretreatment

	Child-Centered			Child Directive			Sensitive Responding			Mutuality			M (SD)
	FP	LEGO	TU	FP	LEGO	TU	FP	LEGO	TU	FP	LEGO	TU	
1.	—												.51 (.31)
2.	.61**	—											.38 (.26)
3.	.49**	.59**	—										.27 (.27)
4.	.14	.08	.16	—									.76 (.63)
5.	.12	.08	.11	.47**	—								1.92 (1.22)
6.	.15	.11	.19*	.55	.56*	—							1.24 (.99)
7.	.51**	.43**	.43**	-.09	-.13	-.05	—						3.87 (1.57)
8.	.43**	.62**	.46**	-.09	.07	-.04	.58**	—					4.04 (1.51)
9.	.43**	.51**	.64**	.02	.00	.06	.51**	.64**	—				3.48 (1.61)
10.	.49**	.36**	.39**	-.04	-.06	-.04	.79**	.54**	.50**	—			3.52 (1.61)
11.	.34**	.45**	.32**	-.09	.11	-.10	.51**	.75**	.53**	.60**	—		3.54 (1.51)
12.	.25**	.37**	.44**	.00	.00	-.01	.49**	.53**	.68**	.55**	.58**	—	2.68 (1.50)

Note: The scale for child-centered and child directive behaviors are events per minute; Sensitive Responding and Mutuality are based on a 7-point Likert scale. FP = free play session; LEGO = structured LEGO task session; TU = tidy-up session.

* $p < .05$. ** $p < .01$.

TABLE 3
Means (Standard Deviations) of Parenting Measures as a Function of Observational Task and Treatment

	Pretreatment						Posttreatment					
	Control			Treatment			Control			Treatment		
	FP	LEGO	TU	FP	LEGO	TU	FP	LEGO	TU	FP	LEGO	TU
Child-Centered	.57 (.33)	.41 (.27)	.29 (.27)	.45 (.28)	.34 (.25)	.26 (.26)	.48 (.27)	.39 (.23)	.22 (.23)	.51 (.33)	.40 (.26)	.24 (.27)
Child Directive	.74 (.62)	1.88 (1.13)	1.21 (1.03)	.79 (.64)	1.96 (1.32)	1.27 (.93)	.64 (.43)	1.88 (1.11)	.99 (.72)	.77 (.81)	1.81 (1.51)	1.10 (1.00)
Sensitive Responding	3.87 (1.54)	4.07 (1.47)	3.58 (1.64)	3.87 (1.61)	4.01 (1.55)	3.38 (1.57)	3.61 (1.33)	4.22 (1.48)	3.20 (1.43)	4.00 (1.53)	4.07 (1.57)	3.49 (1.55)
Mutuality	3.77 (1.60)	3.68 (1.55)	2.67 (1.53)	3.25 (1.59)	3.39 (1.47)	2.68 (1.49)	3.59 (1.61)	3.82 (1.42)	2.78 (1.65)	3.68 (1.59)	3.57 (1.45)	2.67 (1.50)

Note: Means (standard deviations) based on intention-to-treat and last value carried forward data. See text for significant treatment effects and effect size estimates. FP = free play session; LEGO = structured LEGO task session; TU = tidy-up session.

Child-Centered Parenting

In the free play setting, we obtained a significant effect of treatment, $F(1, 139) = 4.77, p < .05, ES = .50$, and pretreatment parenting behavior, $F(1, 139) = 54.17, p < .001$; no significant effects were observed for high-risk status, $F(1, 139) = 1.03, ns$, or the Treatment \times High-Need Status interaction, $F(1, 139) = .85, ns$. In the structured LEGO setting, we obtained a marginally significant treatment effect, $F(1, 140) = 2.79, p = .097, ES = .32$, and significant pretreatment parenting behavior effect, $F(1, 140) = 104.61, p < .001$; no significant effects were observed for high-risk status, $F(1, 140) = .01, ns$, or the Treatment \times High-Need Status interaction, $F(1, 140) = .45, ns$. Finally, in the tidy-up setting, a significant effect was found for pretreatment parenting behavior, $F(1, 140) = 68.22, p > .001$, but not for treatment, $F(1, 140) = 1.11, ns$; high-risk status, $F(1, 140) = 1.21, ns$; or the Treatment \times High-Need Status interaction, $F(1, 140) = .00, ns$.

Child-Directive Parenting

No significant effects of treatment were obtained for child-directive parenting in the free play, $F(1, 139) = 1.21$; LEGO, $F(1, 140) = .39$; and tidy-up settings, $F(1, 140) = .30$.

Intervention Effects across Task: Attachment Theory Measures

Means (standard deviations) for Sensitive Responding and Mutuality measures across Task \times Treatment condition are provided in Table 3.

Sensitive Responding

In the free play setting, we obtained a significant effect of treatment, $F(1, 139) = 4.11, p < .05, ES = .27$, and pretreatment parenting behavior, $F(1, 139) = 61.13, p < .001$; no significant effects were observed for high-risk status, $F(1, 139) = .36, ns$, or the Treatment \times High-Need Status interaction, $F(1, 139) = 2.23, ns$. In the structured LEGO setting, we obtained a significant

effect of pretreatment parenting behavior, $F(1, 138) = 145.66, p < .001$, but no significant effects were observed for treatment, $F(1, 138) = .28, ns$; high-risk status, $F(1, 138) = .41, ns$; or the Treatment \times High-Need Status interaction, $F(1, 138) = .05, ns$. Finally, in the tidy-up setting, we obtained a significant effect of treatment, $F(1, 140) = 3.98, p < .05, ES = .31$, and pretreatment parenting behavior, $F(1, 140) = 87.25, p < .001$; no significant effects were observed for high-risk status, $F(1, 140) = .26, ns$; or the Treatment \times High-Need Status interaction, $F(1, 140) = .29, ns$. In summary, significant treatment effects of one fourth to one third of a standard deviation were detected in the two less structured settings (free play, tidy up) but not in the structured LEGO setting.

Mutuality

Using the analysis of covariance analytic approach, no significant treatment effects were observed in the free play, $F(1, 139) = 1.59, ns$; LEGO, $F(1, 138) = .32, ns$; or tidy up, $F(1, 140) = .36, ns$, settings.

Narrative Assessment

For this analysis, we considered the attachment classifications recoded as secure or insecure (combining the insecure-avoidant, ambivalent, and disorganized groups) and used a logistic regression. In an analysis with treatment and high-need group and their interaction as predictors and controlling for baseline secure classification, there was not significant evidence that the intervention significantly increased the likelihood of a secure classification at posttreatment (for the treatment effect, odds ratio = 1.21), 95% confidence interval [.49, 2.97], *ns*.

Do Changes in Social Learning Theory Measures Covary with Changes in Attachment Measures?

The next set of analyses examined the degree to which treatment changes in social learning theory measures were independent of, or overlapped with, changes in attachment-related parenting measures. We focus these

analyses on child-centered parenting and sensitive responding, the two measures of parenting for which there were reliable treatment effects across setting. We first examined this question by assessing correlations between change scores. Results suggested modest but not substantial overlap. Specifically, correlations between change scores (Time 2 – Time 1) in child-centered parenting were modestly correlated with change scores in Sensitive Responding within each task, free play, $r(144) = .38$, $p < .01$; LEGO task, $r(143) = .23$, $p < .01$; tidy up, $r(145) = .37$, $p < .01$.

Formal tests of mediation were limited because we only had pre- and posttreatment data, that is, we did not meet the temporal requirement that the hypothesized mediator was collected during the course of the treatment. Nonetheless, we applied a joint significance test using the regression model (MacKinnon et al., 2007). We did not have a strong a priori hypothesis about the primacy of one conceptual-measurement model driving change in the other; on the other hand, given that the intervention was based on social learning theory, we considered the changes in social learning theory measures to be primary (because they were targeted by the intervention) and mediators of changes in attachment theory measures to be secondary (because they were not targeted by the intervention).

Following the joint significance test to assessing mediation (MacKinnon et al., 2007), we first assess (a) the intervention effect on the mediator, child-centered parenting, controlling for pretreatment scores and high-risk status; we then examine (b) the effect of the posttreatment child-centered parenting on posttreatment sensitive responding, controlling for pretreatment sensitive responding. Partial mediation is supported if both paths are significant (MacKinnon et al., 2007). Mediation analyses were conducted for parenting behavior in the free play and LEGO sessions because only in those two settings was there a significant effect of treatment on child-centered parenting, the mediator (see earlier).

In the free play setting, there was not significant evidence that child-centered parenting mediated change in sensitive responding; that is, although treatment did predict change in child-centered parenting (see earlier), posttreatment child-centered parenting did not predict significant variance in posttreatment sensitive responding (controlling for pretreatment sensitive responding; $B = 1.15$, $SE = .73$, ns). In the LEGO setting, posttreatment child-centered parenting did not predict significant variance in posttreatment sensitive responding, although the effect was marginal ($B = 1.28$, $SE = .70$, $p = .07$).

Supplementary Analyses

The first supplementary analysis was conducted to examine the treatment effects when we limited the

treatment group to those who received at least five intervention sessions, a per protocol analysis. Two measures showed a significant effect in the intention-to-treat analyses, child-centered parenting, and sensitive responding; the treatment effect and effect sizes for these variables were similar in the intention-to-treat and per protocol analysis. Specifically, for child-centered parenting, a per protocol analysis for the free play session yielded a significant treatment effect, $F(1, 96) = 5.98$, $p < .05$, $ES = .60$; for the LEGO task, the treatment effect in the per protocol analysis was not significant, $F(1, 97) = 2.35$, ns , $ES = .10$; for the tidy-up task, the treatment effect in the per protocol analysis was nonsignificant, $F(1, 97) = 3.83$, ns , $ES = .30$. For Sensitive Responding, a per protocol analysis indicated a significant effect in the free play setting, $F(1, 96) = 6.07$, $p < .05$, $ES = .28$; for the LEGO setting, the treatment effect remained nonsignificant, $F(1, 95) = .01$, ns , $ES = 0$; for the tidy-up session, the treatment effect was significant and somewhat larger, $F(1, 97) = 6.54$, $p < .05$, $ES = .55$. Analyses of the three other measures—child-directive parenting, Mutuality, and child attachment narrative—did not indicate a significant treatment effect in the intention-to-treat analysis; the treatment effect was also nonsignificant in a per protocol analysis for these measures. Specifically, for child-directive parenting, the treatment effect ranged from $F(1, 96) = .64$, ns , $ES = .27$, in the free play setting to $F(1, 97) = .34$, ns , $ES = .18$, in the tidy-up setting. For Mutuality, the treatment effect ranged from $F(1, 96) = 1.67$, ns , $ES = .29$, in the free play setting to $F(1, 97) = .14$, ns , $ES = -.08$, in the tidy-up setting. For child attachment narrative, there was no significant increase in the likelihood of a secure classification at posttreatment using a per protocol analysis (for the treatment effect, odds ratio = .66), 95% confidence interval [.18, 2.29], ns . We also reexamined the analyses of the whole sample and used number of sessions as a covariate; it was not significant, indicating the variation in number of sessions did not predict outcome.

A second set of supplementary analyses examined if the treatment effects reported earlier varied across ethnic group or gender. We found no significant evidence that either gender or ethnicity moderated the treatment effects reported earlier, although the power to detect significant effects was modest given the sample size.

DISCUSSION

We used the leverage of an RCT design to test questions about the distinctiveness of two models of parent–child relationships that dominate theoretical and clinical work, and yet are relatively empirically isolated from one another. The key finding was that the positive

impact of a standard social learning theory intervention extended to attachment theory measures: parents receiving the Incredible Years (plus reading program) intervention demonstrated improvements in their sensitive responding to the child, as rated by researchers blind to treatment status. There was a strong suggestion that the treatment effects may be dependent on the interaction context, as described next. In addition, there was evidence that the changes observed in the social learning theory measure, child-centered parenting, were at least partly distinct from those observed in the attachment-based measures of sensitive responding. However, we found no evidence that the intervention produced a positive change in the children's attachment representations derived from a narrative measure.

Before considering the conceptual and clinical implications of the results, we first note the study limitations. First, the study was based on a high-risk, ethnically diverse sample. Whether or not the conclusions extend to less high-risk and diverse samples is not clear, and our ability to detect even moderate moderation findings according to ethnicity or other factors was limited (and was not a focus of the study). Second, although we included representative measures of social learning theory-based and attachment theory-based models, we did not—and could not have—included exhaustive measures of each model. It may be that the findings obtained here are limited to the particular measures assessed. Third, our mediation analyses were limited because we did not have measures of the proposed mediating parenting variables between pre- and posttreatment assessments. Finally, we note that the mean number of sessions attended was a modest percentage of the total program. Although this is not unfamiliar in community prevention studies, it does confound conclusions about the intervention and whether or not the dose received was sufficient. That limitation may not be significant because moderate treatment effects were obtained, but the low number of sessions experienced may have confounded analyses of the coordinated change in relationship measures from social learning and attachment theories. These limitations are offset by several strengths of the study, including the randomized design, the intensive observational parenting assessments, the diverse and community sample nature of the study, and the good retention rate.

The first message from the study is that a standard social learning theory intervention, with a focus on behaviorally based social learning theory parenting practices of increasing praise, making directives clear, and reducing hostility, altered parental sensitivity. These benefits in attachment theory-based measurement constructs are an unanticipated “side effect” of the intervention, which did not target these constructs. The empirical evidence offered here is important in two related ways. Specifically, it rejects a bias among some clinicians that

parent training program effects would be “superficial” or alter only “mechanical” features of parental behavior and would be unable to alter a more affectively charged component of the parent–child relationship. And it provides some of the only evidence for a contrary view, from other clinicians, that good social learning theory-based approaches do have a deeper impact on the relationship between the parent and child—even if that is not especially evident in how change is recorded in the research.

One explanation for this generalized effect is that there are aspects of the intervention that might be seen as emphasizing attachment-based notions, as in the aim of getting parents to *respond to the child* by following his or her lead during play. On the other hand, other aspects of the program emphasize components of the parent–child relationship that are not featured in attachment interventions, such as disciplinary practices. Some attachment-based interventions have been specifically altered to include disciplinary components traditionally allied with social learning theory, as in the case of one program for 1- to 3-year-olds (Van Zeijl et al., 2006). Of course, most effective parenting interventions seek to alter multiple dimensions of the parent–child relationship; we are unable to determine in this study which “active ingredient” in the social learning theory intervention may have promoted sensitive responding. Another explanation may simply be that the specific parenting measures derived from social learning and attachment theory overlap (see Table 2), and this accounts for the apparent generalizability of effect to the attachment measures. However, that is not an adequate explanation, because other findings suggested that there was not strong evidence of coordinated change between the child-centered and Sensitive Responding parenting measures: (a) correlation between change scores was modest; (b) there was not significant evidence of mediation for Sensitive Responding; and (c) whereas changes in child-centered behaviors were most evident in the free play and LEGO challenge interaction tasks, changes in Sensitive Responding were strongest in the tidy-up task. That implies that the social learning theory-based intervention produced *additional* and not merely overlapping changes in attachment-related caregiving.

Social learning and attachment theories propose their own particular set of social-cognitive processes, mechanisms, and targeted focal behaviors for assessment and treatment. Nevertheless, that does not guarantee that they are functionally distinct models. Indeed, there are examples of how distinct treatment models nevertheless produced nonspecific, generalized effects, as in the case of cognitive-behavioral therapy and interpersonal therapy for depression (Imber et al., 1990). In other words, a distinct conceptual treatment model does not imply specificity of treatment effects. That kind of question

was raised by a meta-analysis of attachment-based interventions by Bakermans-Kranenburg and colleagues (Bakermans-Kranenburg et al., 2003) who reported that briefer, behaviorally based attachment interventions were associated with reliably larger gains in attachment-based outcomes. Our findings offer something of a complementary conclusion.

It may be noteworthy that the strength of the findings for attachment-based measures was significant and robust across task for parent-focused behaviors (i.e., Sensitive Responding) and weaker and nonsignificant for dyadic-level behaviors (i.e., Mutuality) and child attachment narratives. The tendency to find that the effects were strongest for parent behavior may not be surprising given that parents were the focus of the intervention. The lack of effect for children's attachment narratives could be because the potentially broad reach of the intervention did have limits, or for the more practical reason that the mean number of sessions attended is insufficient to bring about large and wide enough parenting changes that will alter a child characteristic; modest reliability of the attachment narrative measure is an alternative explanation. It might also be that insufficient time had elapsed to detect changes in children's dyadic behavior or mental representations that might be induced by changes in parental sensitivity. Given the growing clinical interest in using attachment narratives (Futh et al., 2008; Page et al., 2011; Toth, Cicchetti, Macfie, Maughan, & VanMeenen, 2000), further intervention studies are needed to examine what kinds and what durations of interventions are needed to alter young children's attachment representations and cognitions (e.g., expectations and attributions) of parents, and whether or not this mediates behavioral change.

A second major finding is that the beneficial effects of the intervention were not equally evident across all interaction tasks. We found sizable differences in parent behavior across tasks that differed widely in their demands and structure (and perhaps stress) imposed on the parent and child. It would then be similarly expected that the likelihood of detecting change in targeted parenting behaviors might also vary across task; this is what we found. Changes in both child-centered and sensitive responding were robust, that is, evident in more than one setting, but the effects varied moderately. In the case of sensitive responding, for example, improvements associated with treatment were evident in the two less structured settings and not at all in the highly structured LEGO building task setting. It may be that the structure imposed on the dyad by the LEGO building task was too restricting to observe behavioral change.

Some authors (Grusec & Davidov, 2010) have emphasized domain-specific nature of parenting phenotypes and tasks, although that notion has not yet been widely integrated into parenting interventions. If supported in

subsequent research, the nongeneralizability of intervention effects across task has several practical and clinical applications; the most obvious is that all interaction tasks are not equally sensitive to parenting dimensions and changes in parenting. It is acknowledged that children's behavior varies across setting (De Los Reyes, Henry, Tolan, & Wakschlag, 2009) and the possibility that there may be a context-specific nature of parent-child interaction quality has been appreciated for some time; however, those lessons are only slowly being incorporated into clinical settings.

There is a growing interest in comparative effectiveness research, that is, research examining if one form of treatment is more effective and operates through similar mechanisms than another form of treatment. Lessons from such studies should help practitioners who need to decide which of several evidence-based interventions may be most suitable to them, their skills sets, and the families they seek to help. Although we did not compare contrasting interventions, our findings are relevant to the divide in the clinical community that has come to mirror that in the academic research literature between social learning theory and attachment theory interventions. We have found that the parallel paths pursued by social learning theory and attachment theory may be somewhat artificial. There are generalizable effects from treatment, even if there is evidence of distinctiveness from the meditational analyses. Clinical and theoretical progress is most likely where there are further efforts to identify areas of overlap and distinction between these competing models.

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