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The Benefits of Booster Interventions: Evidence from a Family-Focused Prevention Program

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Abstract Booster effects have been reported in few prevention and treatment studies. However, as noted by Eyberg et al. Clinical Psychology: Science and Practice, 5, 544–554 (1998), there has been no adequate random-assignment test of booster effects to address the basic question of whether boosters increase effects over initial intervention. The present study addresses this question by randomly assigning 196 families to a booster intervention (SAFEChildren II) and comparing effects 1 year after that intervention with families who had been assigned to the initial intervention only (SAFEChildren I). Both interventions were based in a developmental-ecological framework emphasizing family management of child-rearing and related challenges within an inner-city social ecology. The booster led to a relative improvement in child aggression and concentration in school for the overall sample, with additional benefit for high-risk groups in academic achievement, behavior, and family organization. The study also suggests need for more careful study of processes related to booster effects in prevention, including comparison of competing models.

Keywords Booster effects · Outcome · Family intervention

Prevention focused on parenting and family relationship characteristics and intended to reduce later antisocial behavior among children has shown robust initial effects (Dishion and Kavanagh 2000; Eyberg et al. 1998). As such evidence accumulates, interest is growing in how proximal effects on risk factors translate to lasting and substantial impact (Eyberg et al. 1998). Findings are mixed about the relation between proximal and long-term impact. Initial positive effects of some interventions are sustained, whereas others emerge only with time, and other initially positive effects fade with time (Tolan and Gorman-Smith 2002).

Factors influencing the long-term effects of initially promising interventions is an important but relatively understudied topic (Kellam and Rebok 1992). For example, a commonly held principle is that intervention early in a child's development is more effective than if occurring later. Yet, few empirical studies have tested the validity of this belief. In some cases, long-term effects of early interventions are mistakenly presented as evidence that early intervention is preferable, but this is not an actual test of the importance of timing of intervention. One study that empirically tested this contention was the Metropolitan Area Child Study. In that study, exposure to a 2-year intervention beginning in second grade was compated to similar exposure beginning in fifth grade (Metropolitan Area Child Study Research Group 2002). Benefits were limited to those with the earlier exposure.

Another frequent contention is that booster interventions can be important in realizing significant effects and/or maintaining, or enhancing initial intervention effects (Whisman 1990). As with intervention timing, few have studied booster effects (Eyberg et al. 1998). As early as 1974, Patterson reported improved maintenance and recovery of behavior with additional, "booster" professional contact (Patterson 1974). McDonald and Budd (1983) used a multiple baseline design to show that booster sessions led to reclaiming benefits lost after an effective intervention. Lochman (1992) introduced a booster session for a subsample of his randomized trial of the Coping Power intervention. The addition of 6 weeks of a cognitive-

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behavioral group for targeted children and six parent training sessions led to maintenance of initial effects on aggression but did not enhance effects over the basic intervention. A subsample within the Metropolitan Area Child Study, those who remained in their original school for the course of the study, presented an opportunity for a quasi-experimental comparison of exposure to both the early and late interventions versus exposure to early intervention alone. The second exposure substantially amplified effects from initial exposure (Metropolitan Area Child Study Research Group 2002). For example, for the full (family-focused) intervention, the reduction in aggression for those exposed to both early and late versions was about three times that from exposure to the early intervention only. This booster effect was found for putative mediators of aggression as well (Metropolitan Area Child Study Research Group 2007). Another quasi-experimental comparison suggested the Life Skills program effects could be attributed to "booster sessions." Significant differences from no-intervention controls occurred only for those who had sessions in eighth and ninth grades added to the initial seventh-grade program (Botvin 2000; Botvin et al. 1990). As with the Metropolitan Area Child Study, this comparison did not randomly assign to booster exposure. In addition, given that fidelity of implementation also moderated effects, it is unclear how booster exposure and fidelity were interrelated in explaining benefits. One study by Djikstra et al. (1999) randomly assigned study participants to booster or no-booster conditions for two promising approaches to smoking prevention. For both interventions, the booster condition led to less initiation of smoking immediately post-test, but this difference over the basic intervention dissipated by the 1-year follow-up. However, the study was hampered by low exposure to the booster among those randomly assigned, limiting confidence in the results

These studies suggest a booster may be important in realizing promising effects of initial interventions and therefore be important in prevention. Yet, there has been very little study of booster effects and the existing research has been hampered by methodological limitations. In addition, study of booster effects can be challenging because of the potential confounding of exposure and retention-related selection effects, difficulty differentiating effects of greater dosage from the specific booster program, and the sample size needed to make comparisons. Thus, the potentially important role of boosters in achieving preventive effects remains relatively unexamined. This absence of study was noted by Eyberg et al. (1998):

> To our knowledge, the impact of booster session on maintaining treatment gains following parent training has never been evaluated in a controlled study... no study in the child literature has used a randomized group design to determine if booster training is superior to no such training. The use of a randomized-control design in such a study would allow one to determine whether additional treatment sessions enhance maintenance more effectively than attempts to program maintenance during the original intervention. Systematic evaluation is clearly needed. (p. 549).

The Present Study

The present study is intended to address this need by applying random assignment to test the booster effect of the SAFEChildren (Schools and Families Educating Children) project. SAFEChildren is grounded in a developmental-ecological perspective that emphasizes age- and setting-related conditions and events that might promote risk. The approach is to help families protect against such risk factors for children growing up in communities characterized by high levels of poverty and crime and low social and economic resources (Tolan and Gorman-Smith 2002). Accordingly, SAFEChildren consisted of a 22-session family intervention to help inner-city parents manage their children's transition into school (first grade), accompanied by 20 sessions of academic tutoring for children. This program was delivered to a randomly selected sample of 55% of all incoming first graders and their parents in seven schools serving high-crime and high-poverty neighborhoods in Chicago. As reported in a prior publication, the intervention had good participation (e.g., 84% of those solicited) and showed modest but statistically significant effects at 1-year follow-up for academic achievement and parental school involvement for all participants (Tolan et al. 2004). Broader and more substantial effects were found for those families with higher pre-intervention risk as denoted by parenting practices measures and for children with higher pre-intervention risk denoted by elevated externalizing problems scale scores (Tolan et al. 2004).

To test for booster impact, we randomly assigned 50% of those randomly assigned to the initial version to receive a second version of the SAFEChildren program. The intervention was delivered during the fourth grade because this is just prior to the age when delinquency and other related social problems are emerging. The booster program applied the same conceptual approach and overall design as the initial version, with content modified to be developmentally appropriate. Similar to the initial program, we were interested in combining multiple family groups to focus on parenting, normal developmental issues at this age, and family relationship support with promotion of reading

skills. In this case, the 20-session multiple family groups were paired with a reading club and access to age-appropriate books. Families were encouraged to help set time for children to read, to read books with their children, and to bring the books they read into the multiple family group discussions. As with the initial intervention, the booster program consisted of 4–6 families per group and focused successively on a set of topics. Through a mixture of information, group discussion, behavioral practice, and group support, the program covered effective parenting practices, managing school achievement motivation and involvement in school, peer relations, and ecological challenges such as violence and safety. As with the initial program, interest was in affecting growth and level of school achievement and bonding, child behavior and social competence, and parenting practices, family relationships, and parental involvement in school. Thus, although developmentally adjusted for content, the approach was quite consistent with the initial program.

With re-consent, those initially randomly assigned to intervention were randomly assigned to either booster or no further intervention. As with the initial intervention, random assignment was at the individual level within school, such that approximately one-half of those randomly assigned to the initial intervention were randomly assigned to the booster exposure. We expected the booster intervention group to evidence significantly greater benefits in parenting practices and family relationships, parental involvement in school, child interest in school, academic achievement, and behavior (e.g., aggression, hyperactivity, social competence). We report here the relative impact of the booster for those initially exposed, with follow-up 24 months after the booster completion (end of sixth grade).

Method

Research Participants

Children and their families who had been randomly assigned to receive the initial SAFEChildren intervention were randomly assigned to either (1) receive a booster intervention or (2) participate in an assessment-only condition. Those in the control condition in the initial trial were also followed to estimate long-term effects of the initial intervention. The comparisons of long-term effects of the initial intervention are provided in a separate report (Gorman-Smith et al. 2006).

Four hundred twenty-four (424) subjects (84% of the initially solicited population) participated in the initial SAFEChildren study. Of these, 348 (82.1%) were retained for this second phase. Participants from the original study were deemed eligible because they had participated in the first phase of the study and were still residing within an hour's travel time from the original intervention locale. Ninety-five percent of this group were assessed at the outset of the second phase of the study and constitute the sample used here. Of this number, 45% had been assigned to control condition in the first phase of the study. The 196 subjects and families that had been assigned to the intervention condition in the first phase of the study were randomly assigned to either the booster intervention condition (n=101, 51%) or an assessment-only condition (n=95, 49%).

Of the families retained and assessed for the first assessment for this phase of the study, 97.9% were retained to completion. No significant differences or near significant trends resulted for those in the initial assessment and final assessment of this phase (X^2 (2,339)=0.28, *ns*). Of the families randomly assigned to the booster intervention, 80% completed a majority of the intervention sessions, and 69% completed 90–100% of the sessions. No significant differences occurred between intervention completers and non-completers on any pretest characteristics.

A little more than one-half (54.6%) of the target children in the second phase of the study were female; 42.6% were African American, 54.8% Latino (most of Mexican-American heritage), and 2.4% other ethnicity. Median family income ranged from $20,000 to $30,000 per year.

Measures

In this phase of the study, we continued the prior approach of focusing on three risk factors for antisocial behavior, on the basis of prior empirical evidence: (1) the child's school functioning; (2) the child's behavior and social competence; and (3) parenting and family relationships. Within each of these areas, data was based on several indicators from multiple sources. In addition, several important covariates were measured that might moderate effects. This report focuses on measures from the child and parents because those are complete up to the final data collection point (wave 9, 1 year post-intervention).

Child's School Functioning

Academic achievement was measured by collecting standardized scores on reading skills tests used by the schools, either the Iowa Test of Basic Skills (ITBS; Hieronymous et al. 1986) or the California Achievement Test (CTB/McGraw-Hill 1985) depending on which test a given school used. Approximately 80% of the published reliability coefficients for the CTB are at .8 or above (Salvia and Yesseldyke 1991). On the ITBS, reliabilities range from .67 to .95. As both are nationally standardized, standardized

scores were comparable for purposes of constructing a composite indicator of academic achievement.

School bonding was measured using two subscales in the child's self-report version of the Behavioral Assessment System for Children (BASC): Attitude Toward Teacher and Attitude Toward School (Reynolds and Kamphaus 1998). The BASC is a widely used measure of several dimensions of children's school functioning, with substantial evidence for reliability and validity with children as young as 8 years old. For the present sample, the subscales had internal consistency reliabilities of .62 and .57, respectively. Higher scores on these scales indicate greater levels of school bonding. Although both subscales are thought to measure aspects of school bonding, our confirmatory factor analysis indicated that they should be regarded as distinct constructs (correlations between the scales were modest: .26 to .37). Therefore, we did not combine these subscales into a single composite scale for analysis.

Child Behavior and Social Competence

Three aspects of *child behavior* were assessed using the Parent Observations of Classroom Adaptation-Revised (POCA-R; Kellam et al. 1983): aggression, hyperactivity, and concentration. This is a frequently used and validated measure of child's behavior as "observed" by the parent. Parents are given a structured interview to elicit their experience with the child and their observation of different behaviors and characteristics. The interview does not specify where the behavior was observed, but parents report their observations of child behaviors that may affect adaptation to school. Confirmatory factor analyses with our sample supported the original scale content. Pretest alpha coefficients with this sample ranged from .61 to .84. Higher scores refer to higher aggression, hyperactivity, and concentration, respectively.

Child social competence was measured using parent ratings to render scores for three subscales meant to measure social competence in the BASC (Reynolds and Kamphaus 1998): leadership (α=.70–.78), adaptability (α=.71–.81), and social skills (α=.80–.86).

Parenting Practices and Family Relationships

Parenting practices was based on parent reports on the Parenting Practices Questionnaire (Gorman-Smith et al. 1996). This 46-item scale measures two constructs—discipline practices and monitoring—which are higher-order factors derived from five scales: (1) discipline effectiveness (α=0.61); (2) discipline avoidance (α=.66); (3) positive parenting (α=.81); (4) involvement (α=.73); and (5) supervision (α=.59). Each scale is scored such that higher scores represent more effective or better parenting practices. The scales and factor structure have been validated with several samples, including initial development with a sample from the same neighborhoods (Gorman-Smith et al. 1996). Reports of parents on the first-order parenting scales were standardized and combined for these analyses.

Family relationship characteristics were based on parent reports on the Family Relationships Scale (Tolan et al. 1997), which yields three scores used in analyses: cohesion, beliefs about family, and structure (family organization). These scales are based on higher-order factors from a 35-item inventory measuring six aspects of family relationship characteristics: (1) cohesion (α=.80); (2) beliefs about family (α=.84); (3) deviant beliefs (α=.71); (4) support (α=.63); (5) organization (α=.53); and (6) communication (α=.57). Higher scores mean stronger and more functional family relationship characteristics. The scale has been validated with several samples, many of which are demographically equivalent to this sample (see Tolan et al. 1997 for details on development and initial psychometric validation). Reports of multiple informants may be combined to produce cross-source scores when available. The same factor structure has been confirmed for single sources (August et al. 2001). We did not expect effects on family beliefs, and therefore do not include that scale among the results. Single-informant scaling of two of the higher-order constructs (cohesion, structure) are used in these analyses.

Covariates The following demographic variables were measured using a background information questionnaire administered as part of the parent interview and were included as covariates in the analyses: parents' marital status, family's income, and child's gender, ethnicity, and school at the time of random assignment.

Differentiating High-Risk Families and Youth with High-Risk Behavior

Because all families and children were included in the study and eligible to be randomly assigned by virtue of where they lived, this intervention, like the first SAFEChildren study, was universal in one sense. However, other prevention trials have demonstrated that within a general population intervention effects may be limited to or be greater for high-risk groups (e.g., Hawkins et al. 1999; Olds et al. 1998; Reid et al. 2002). Similar patterns were found in the initial SAFEChildren study, with substantially greater and broader effects for high-risk families and children with high risk due to behavior (Tolan et al. 2004). In this evaluation of the effects of the booster intervention, the original

designations for high-risk families and for high-risk children were retained to estimate variations in effects related to high-risk status, in addition to testing overall effects. Families were coded as high risk in the first phase of the study if their pretest scores on composite family relationships or parenting practices scales were lower than one standard deviation below the mean. One hundred, or 23.5%, of families were classified as high risk at pretest in the initial study. Of these, 83% participated in SAFEChildren II (Fig. 1). A child was coded high risk in the initial phase if he or she had average standardized parent and teacher aggression, hyperactivity, and concentration scale pretest scores that were one standard deviation or more above the mean; 68 children (16%) were categorized as high risk in the first phase. Fifty-four of these children (79.4%) participated in the present study. Notably, the overlap of high-risk families and high-risk children was limited (less than 30%).

Procedures

For this phase of the study, four waves of data were collected for all participants to measure intervention effects, following up the five waves of data collected in the first phase. Data were collected from parents and children at waves six (pretest, beginning of fourth grade), eight (posttest, end of fourth grade) and nine (1-year follow-up, end of fifth grade). At wave 7 (mid-intervention of this phase), only teacher data were collected owing to financial constraints. Similarly, financial constraints limited our ability to fully include teacher estimates at wave 9, rendering it of limited use in estimating the sustained outcome effects.

African American and Latino interviewers recruited from communities in the study conducted interviews as two-person teams. After obtaining informed consent and explaining the procedures to the family, the interviewers talked with parent(s) individually and recorded the answers on laptop computers. The child interview was completed using paper and pencil methods and in private, although the caregivers were given the opportunity to examine the questionnaires their child would be completing before the interview began.

Prior to the study outset, interviews were piloted with families and children in English and Spanish. All measures were translated and back-translated before use in Spanish language interviews. The project coordinator and data management staff checked every interview completed for proper consent, missing data, accuracy, and out-of-range values. A random sample of 20% of the cases received a follow-up phone call from the project director to assess whether proper interview procedures were followed.

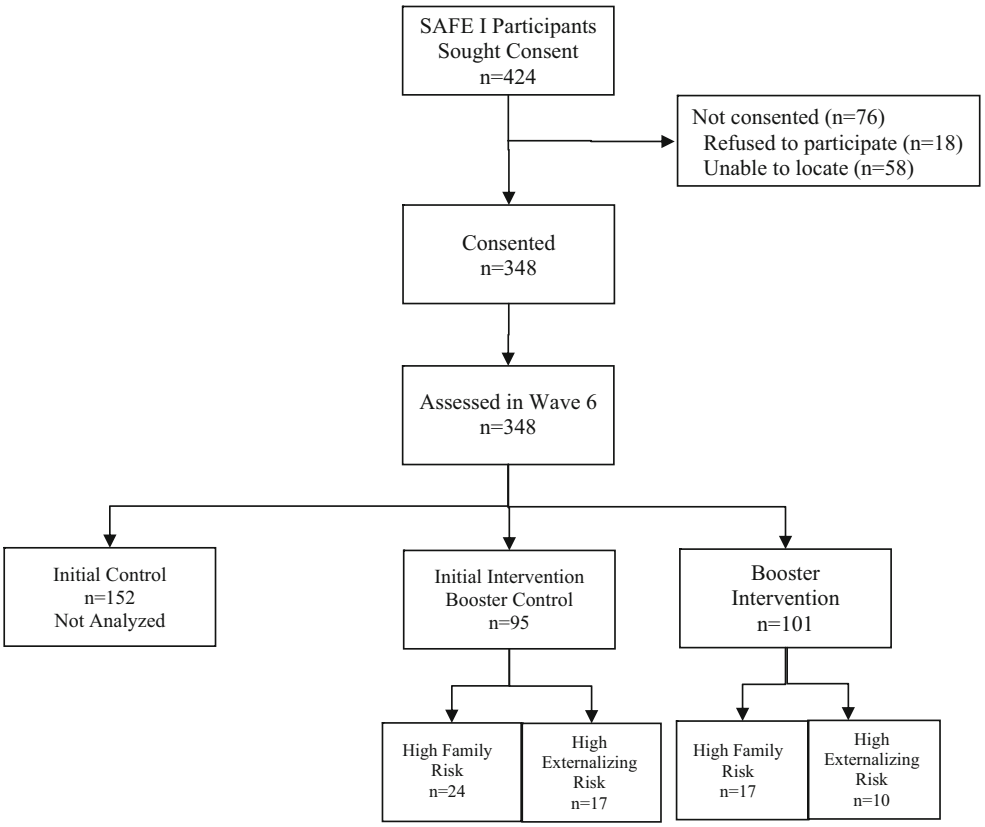

Fig. 1 Consolidated Standards of Reporting Trials (CONSORT) chart depicting the recruitment and assignment of subjects in the SAFEChildren II study. Please note that random assignment to the initial SAFEChildren intervention and classification into risk groups took place in the initial SAFE-Children I study. Also, we provide the number for each phase in each of the high-risk groups. As membership in each high-risk group was independently determined, the denotation is not meant to explain complete components of the sample for that phase (not meant to add to *N* participating in that phase)

Results

Analysis Plan

The analysis plan applied here follows the same approach used in the first SAFEChildren study (Tolan et al. 2004). Our interest in this set of analyses is the degree to which the booster intervention has an effect over and above that of the initial intervention over this duration of this study. Accordingly, the focus is on comparing those randomly assigned to the booster intervention with those assigned to receive the initial intervention without the booster. All participants who were randomly assigned to any of the three conditions (control, initial intervention only, booster + initial intervention) were included in these analyses, regardless of attrition, dosage, or missing data. Inclusion of control subjects allowed us to obtain more accurate estimates of the standard errors than would be possible with the two intervention conditions only. This "intent-to-treat" approach to analysis was used to assure that the randomness of assignment to conditions was preserved (Shadish et al. 1998), and that the comparisons reported reflect differences due to the intervention.

A random regression approach implemented through SAS PROC MIXED was applied to test effects of the intervention (Bryk and Raudenbush 1992; Gibbons et al. 1993). Random effects regression models assume that the available data at any given point of measurement estimate the group trend and each individual's deviation from the group trend at that point of measurement. This assumption permits valid estimates of effects with cases that have missing waves of data, whether or not the data are missing at random. Post-intervention intercept differences between booster and initial intervention only conditions (estimated from waves 8 and 9) were calculated controlling for pretest scores (wave 6). Outcome analyses were two-level models. The level-1 models predicted an outcome variable from an individual intercept, and the pretest value of the outcome. The level-1 equations also included terms for family income and parental marital status at each wave of measurement. Thus, marital status and income were treated as time-varying covariates in these analyses. The level-2 equations predicted the level-1 intercepts by intervention condition, gender, ethnicity, parental marital status, family income, and the child's school at the time of the beginning of the initial SAFEChildren study.

As in the initial phase, random regression models were fit to assess effects of the intervention on all subjects (overall effects model) and then refit with terms for both risk group designations, focusing on the interaction between risk group and assigned intervention condition (high-risk family and high-risk children models). However, the three-way interaction between both risk groups and intervention conditions was not tested. These models allowed testing of the effects of the intervention on high-risk families (as defined by their family relationships and parenting practices scores) and high-risk children (as defined by their externalizing scores) over and above any overall or general effects. Thus, three sets of results are reported: overall or whole sample effects, additional effects for high-risk families, and then additional effects for high-risk children.

All of these models are intent-to-treat analyses. They include data from all participants randomly assigned to condition, regardless of their level of participation in the intervention. Random error terms were entered for individual intercepts and linear slopes. For each model, Helmert contrasts were used to model the added effects of the booster intervention over and above the effects of the initial SAFEChildren intervention, as follows:

Assigned condition	Contrast	
	Initial vs. control	Booster vs. initial
Control	−1	0
Initial	.5	−1
Booster	.5	1

Planned comparisons evaluated differences in intercepts (representing differences at posttest) by intervention condition. These comparisons were linear contrasts with a single degree of freedom. The contrast of interest reported in this study is booster intervention impact compared with initial intervention only.

Pretest Differences

Pretest differences were conducted between the two groups on all covariates and outcome variables, with comparisons made overall and for each of the high-risk groupings. Two significant differences were found. Pretest (for booster) score on the POCA Hyperactivity Scale was significantly lower for the booster condition ($B=-0.22$, $t(298)=-2.54$, $p\leq.05$). Also, among those children designated as high-risk due to externalizing behavior, "attitude toward teacher" report was significantly higher at pretest. ($B=-2.09$, $t(298)=-2.53$, $p\leq.05$). There were no significant pretest differences among high-risk families and no other significant pretest differences. Given the number of comparisons and the limited size of these effects, we considered it reasonable to consider the random assignment as valid for analysis.

Booster Intervention Effects: Overall Effects

Table 1 reports findings for booster versus initial intervention only. These models were fit without terms for high-risk children or high-risk families, and without interactions

Table 1 Overall comparison booster effects

Construct		Estimate	SE	*df*	*t*	Effect size[a]
Child school functioning						
Scale	Academic achievement[b]	0.44	1.86	37	0.23	0.08
	School bonding	−0.32	0.49	327	−0.65	−0.07
Child behavior						
Scale	Aggression	−0.16	0.09	327	−1.70*	−0.19
	Hyperactivity	−0.03	0.09	327	−0.32	−0.04
	Impulsivity	−0.26	0.10	327	−2.61**	−0.29
	Concentration	0.21	0.11	327	1.86+	0.21
Child social competence						
Scale	Social skills	0.06	0.11	327	0.60	0.07
	Leadership	−0.09	0.11	327	−0.82	−0.09
	Adaptability	0.17	0.10	327	1.68*	0.19
Parenting practices						
Scale	Parent monitoring	0.05	0.10	320	0.45	0.05
	Parent discipline	0.09	0.10	320	0.99	0.11
Family relationships						
Scale	Family cohesion	0.07	0.11	327	0.61	0.07
	Family structure	0.11	0.09	327	1.21	0.13

Except for Academic Achievement, effect sizes are expressed in units of Cohen's *d*, calculated by dividing the unstandardized regression coefficient by the sum of the variance components

[a] Hypotheses tested: H_0: *Change in Booster − Change in Initial Treatment Only*=0, where change is post-intervention average controlling for pretest

[b] Estimates are booster vs. initial treatment slope differences from growth models using up to five waves of standardized test scores from 1998 to 2002

*$p \leq .10$; **$p \leq .05$

between risk groups and intervention condition. The models estimate differences between the booster and the initial-intervention-only condition on post-intervention intercepts. Table 1 also reports effect sizes for each test, in units of Cohen's *d* (Wolf 1986, pp. 26–30).

Child's School Functioning There was no significant overall difference between groups for academic achievement or school bonding.

Child Behavior and Social Competence Booster participants had better outcome than the initial-intervention-only group on impulsivity, with near significant differences for aggression, concentration, and adaptability (see Table 1).

Parenting Practices and Family Relationship Characteristics There were no significant or marginal differences for any parenting or family relationships scales.

Table 2 Booster effects as a function of family risk

Construct		Estimate	SE	*df*	*t*	Effect size[a]
Child school functioning						
Scale	Academic achievement[b]	−0.51	3.72	115	−0.14	−0.03
	School bonding	−1.25	0.99	323	−1.26	−0.14
Child behavior						
Scale	Aggression	−0.48	0.19	323	−2.59*	−0.29
	Hyperactivity	−0.11	0.18	323	−0.61	−0.07
	Impulsivity	−0.22	0.20	323	−1.10	−0.12
	Concentration	0.26	0.23	323	1.12	0.12
Child social competence						
Scale	Social skills	0.03	0.21	323	0.17	0.02
	Leadership	−0.06	0.22	323	−0.28	−0.03
	Adaptability	0.18	0.20	323	0.90	0.10
Parenting practices						
Scale	Parent monitoring	0.00	0.21	316	0.00	0.00
	Parent discipline	0.16	0.20	316	0.83	0.09
Family relationships						
Scale	Family cohesion	0.19	0.21	323	0.91	0.10
	Family structure	0.41	0.19	323	2.18*	0.24

Except for Academic Achievement, effect sizes are expressed in units of Cohen's *d*, calculated by dividing the unstandardized regression coefficient by the sum of the variance components

[a] Hypotheses tested: H_0: *Change in Booster − Change in Initial Treatment Only*=0, where change is post-intervention average controlling for pretest

[b] Estimates are booster vs. initial treatment slope differences from growth models using up to five waves of standardized test scores from 1998 to 2002

*$p \leq .05$

Table 3 Booster effects as a function of externalizing behavior

Construct		Estimate	SE	*df*	*t*	Effect size[a]
Child school functioning						
Scale	Academic achievement [b]	9.63	4.62	178	2.09*	0.31
	School bonding	0.97	1.47	323	0.66	0.07
Child behavior						
Scale	Aggression	−0.29	0.28	323	−1.03	−0.11
	Hyperactivity	0.05	0.26	323	0.19	0.02
	Impulsivity	−0.59	0.29	323	−1.99*	−0.22
	Concentration	−0.13	0.34	323	−0.39	−0.04
Child social competence						
Scale	Social skills	0.21	0.31	323	0.66	0.07
	Leadership	−0.14	0.33	323	−0.41	−0.05
	Adaptability	0.20	0.31	323	0.64	0.07
Parenting practices						
Scale	Parent monitoring	−0.10	0.31	316	−0.31	−0.03
	Parent discipline	0.13	0.28	316	0.47	0.05
Family relationships						
Scale	Family cohesion	−0.02	0.32	323	−0.05	−0.01
	Family structure	0.29	0.28	323	1.05	0.12

Except for Academic Achievement, effect sizes are expressed in units of Cohen's *d*, calculated by dividing the unstandardized regression coefficient by the sum of the variance components

[a] Hypotheses tested: H_0: *Change in Booster − Change in Initial Treatment Only*=0, where change is post-intervention average controlling for pretest

[b] Estimates are booster vs. initial treatment slope differences from growth models using up to five waves of standardized test scores from 1998 to 2002

*$p \leq .05$

Effects for High-Risk Families

Table 2 reports the intercept comparisons, their standard errors, *t* values and effect sizes between high-risk families initially assigned to the booster and initial-intervention-only conditions (N=83; 36 control, 25 initial-intervention-only, and 22 booster intervention). Findings can be interpreted as differences between booster and initial-intervention-only high-risk families when no overall effect was found and additional impact when overall effects comparisons were also significant. Only two such effects occurred. High-risk families who were assigned to the booster program reported significantly lower post-intervention child aggression and improved family structure (organization).

Effects for High-Risk Children

Table 3 summarizes results for the subgroup defined as high risk due to high levels of externalizing behavior of the children prior to the initial study (N=54; 27 control, 17 initial-intervention-only, and 10 booster intervention). As with the high-risk family comparisons, significant differences between conditions represent effects limited to this high-risk group if not found in the overall comparison, or a heightened effect if a significant difference was found in the overall comparison. Findings show that those high-risk children who received a booster intervention scored significantly higher on academic achievement and lower on impulsivity.

Discussion

This study is one of the first random assignment tests of a booster intervention and one of the most extensive assessments of such effects to date. The present study used random assignment, consistent exposure criteria, and substantial follow-up, which have not been applied in prior booster effects research (Eyberg et al. 1998). In general, these results suggest a relative boost in effects over the initial intervention only, providing modest support for the frequent contention that boosters are advantageous to consolidate or enhance initial impact of interventions. Although the findings suggest that boosters can improve behavior and affect other key risk factors for longer-term developmental trajectories of high-risk groups, effects were not found consistently for all indicators and in all cases effects were modest. Notably, the effect was not found for theoretically important mediators of long-term child risk such as parental monitoring and school bonding.

Another consideration in interpreting these results is that, although this study incorporates many design features lacking in prior booster research, the test of effects used here is still constrained by two limitations. These limitations may be inevitably part of booster comparisons: those in the booster are exposed to more intervention, and usually to more recent intervention. In any test of a booster there is greater intervention dosage than occurs in the non-booster condition. Also, the booster will occur more

recently to the endpoint for measuring effects than the prior intervention being compared. While one can imagine a research design that might mix timing of booster and initial intervention or apply an attention control to balance dosage (by defining dosage as hours of time in intervention), the practical challenges of carrying out such studies are considerable, and perhaps implausible to circumvent. Moreover, such design efforts might well carry with them more troubling validity threats. For example, if one were to mix booster assignment with initial intervention assignment there might well be effects on attendance, engagement, and other processes affecting engagement in the initial intervention that also affected booster exposure. If one were to stagger follow-up such that time since intervention was the same for those exposed to a booster and those not, then the age at outcome measurement would be confounded, imposing other interpretation problems. These limitations in certainty noted, the present study still represents the most soundly designed test of a booster effect to date, and within these limitations suggests an additive effect of a booster over early intervention only.

The present results show the most consistent impact on child behavior—characteristics that are strong predictors of later delinquency, substance use, and other antisocial behavior (Tolan et al. 2006). Thus, those assigned to the booster evidence relatively lower aggression and impulsivity and better concentration overall. As these were not significantly different at the whole population at 1-year post the initial intervention, it appears the booster may help further benefits from the initial intervention on these important behavioral indicators. There were no differences in the whole-group comparisons for academic achievement, school bonding, social competence, and parenting and family characteristics, developmental influences thought to protect against a developmental course toward problem behavior. While not increasing differences among those exposed to some intervention, the booster seems to enhance the benefits on behavior from the intervention that targeted these protective factors.

Enhanced benefits on several of these protective factors from the booster exposure were found to some extent. For example, in the booster condition high-risk families showed relative improvement in organization and high-risk children showed relative improvement in academic achievement. These significant boosts in capability of parents who were high risk at first grade entry in ability to set up clear expectations, routines, and responsibilities in family life and academic achievement of children who entered first grade with elevated externalizing problems also related to specific additional benefit for child aggression and impulsivity over and above the overall effects found. This pattern of results suggest that similar to what has been found in several prevention trials, benefits are more extensive or broader in the high-risk portion of the population than found overall (Stoolmiller et al. 2000; Tolan et al. 2004).

The pattern of results may suggest processes by which boosters are beneficial in prevention. The “boost” may arise from consolidating propensities set forth in the initial intervention or by strengthening skills that are critical for managing developmental challenges that are more figural during this time just before adolescence (Forgatch and DeGarmo 1999). It may be that the initial protective academic success and continued parental involvement in the early school years found for those assigned to the initial intervention set a stage for behavioral impact that the booster could provide. Similarly, while supporting effective parenting practices and strong family relationships, the initial interventions may have enhanced propensity that is more evident as children near adolescence and are more likely to exhibit risk behaviors and age-related elevations in aggression (Reid et al. 2002).

The need for consideration of the relation of effects on the general and high-risk populations in prevention is another area that is highlighted by the booster comparison. It appears that the booster may enhance or broaden effects on behavior in the general population from just for high-risk groups to the more general population. In addition, the effects continue to be more extensive for the high-risk groups. How this broadening of initial effects to the whole population and the extending of effects among the high-risk group are related is an important but undetermined question for further study. It may be that these different effects are actually reflective of interdependent processes. Effects for the whole population may help consolidate benefits for high-risk populations by reducing the normative level of aggression and other problem behaviors. It could be that reduced problem-behavior and improved family functioning in those in the high-risk groups due to the initial intervention decreased the exposure of the children and families (who are from the same neighborhood and classrooms) to aggression and other classroom disrupting behaviors. In addition, there is concurrent improvement in school performance and behavior that may represent an interdependency of improvement in protective factors that results in a readiness to utilize the booster.

The booster may also have acted to provide more developmental preparedness than occurred with the initial intervention only. Both interventions were designed to help address issues rearing children in the inner city, with developmentally attuned content and skills. Thus, although not affecting basic parenting and family relationship characteristics such as monitoring or cohesion, the booster intervention may have provided parents with more information and skills about managing immediate developmental and ecological challenges, such as the increasing saliency of peer relations and changing involvement of

children in the community, with attending exposure to violence and other community risks. Unfortunately, neither parental knowledge, confidence about these topics, nor issue specific strategies were measured in this trial and therefore cannot be analyzed with this study.

Implications for Prevention

This study provides a random-assignment test of the relative impact of a booster over initial intervention only, perhaps the strongest test to date of this important issue. The initial intervention showed significant effects in a random-assignment trial for many key protective processes for later substance use, delinquency, and other adolescent problem behaviors. Juxtaposed to those findings, these results suggest the booster benefit is to broaden or increase initial effects, as has been the commonly theorized benefit of boosters. In addition, the effects found for high-risk families and youth are those that are particularly relevant to risk reduction. Thus, this study suggests that prevention effects can be improved with a booster. What is less clear from this study is the process of enhancement for boosters. Thus, one implication of these results for prevention is to undertake further study and analyses that can test models of competing theories about how boosters should affect risk beyond effects of the initial or basic intervention. In addition, the pattern of effects raises some potentially valuable areas of study for understanding the relation between impact of a universal prevention effort on high-risk groups and the rest of the population. The pattern of results suggests a need to also evaluate how boosters might spread the prevalence of effects rather than simply boost initial levels of impact.

Whether boosters are, in general, critical for prevention effects cannot be answered by this or any one study. One generalization limitation is that the booster tested here was as lengthy as the initial intervention and was designed as a developmentally-attuned correspondent to the prior intervention. In addition, as with the original study, this intervention was designed for a population for whom risk is tied closely to residence and socioeconomic status. In many cases, boosters are cast as a few contacts or brief intervention meant to merely further exposure to the key features of the initial intervention. Also, in some cases boosters are considered for only those who have below-typical response to the original intervention. This test applied the booster irrespective of variation in response to the initial intervention or level of risk among those first exposed. It would be valuable to have strong tests of the benefits of boosters for those other approaches and more consideration of boosters as a potentially important part of prevention in general.

This study also supports the contention that family-focused prevention can have adequate participation and retention rates to test effects validly. This argues against the contention that family-focused prevention is not feasible. Even in what are communities with populations thought difficult to engage, a high proportion of families were willing and able to participate fully. In both the initial and the booster program, family interest and participation was quite high. It may be that this feasibility rests in some part on the utility of the intervention and on the approach of promoting effective family functioning and managing the challenges faced by families in this social ecology (Gorman-Smith et al. 2006). A major consideration in designing these interventions was to attune the content and focus of interventions to the developmental issues that the children were currently experiencing and to reflect the ecological challenges they and their families faced, such as lack of resource and issues of safety (Tolan et al. 2004). Consideration of utility and relevance may be important in affecting engagement in family prevention and the full test of its potential (Kellam and Rebok 1992).

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