Effects of Child Skills Training in Preventing Antisocial Behavior: A Systematic Review of Randomized Evaluations

Author(s): Friedrich Lösel and Andreas Beelmann

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# Effects of Child Skills Training in Preventing Antisocial Behavior: A Systematic Review of Randomized Evaluations

*By*
FRIEDRICH LÖSEL
and
ANDREAS BEELMANN

This article reports a meta-analysis on social skills training as a measure for preventing antisocial behavior in children and youth. From 851 documents, 84 reports containing 135 comparisons between treated and untreated youngsters ($N$ = 16,723) fulfilled stepwise eligibility criteria (e.g., randomized control-group design, focus on prevention). Despite a wide range of positive and negative effect sizes, the majority confirmed the benefits of treatment. The best estimated mean effects were $d$ = .38 (postintervention) and .28 (follow-up). Effects were smaller on antisocial behavior than on related social and cognitive measures. Studies with large samples produced lower effect sizes than those with smaller samples. Programs targeting at-risk groups had better effects than universal programs. Modes of treatment did not differ significantly; however, cognitive-behavioral programs had the strongest impact on antisocial behavior. More well-controlled studies with large samples, hard outcome criteria, and long follow-up periods are needed, particularly outside the United States.

*Keywords:* prevention; antisocial behavior; social skills training; evaluation; meta-analysis; childhood and adolescence

Due to single, extremely violent events (e.g., school shootings) and alarming crime statistics, antisocial behavior in children and youth has currently become an issue of grave concern in many societies. Even in its less serious forms, such problems trigger a lot of stress, suffering, and costs for victims, parents, the society, and (in the long term) the youngsters themselves. Anti-

*Friedrich Lösel is a professor of psychology and the director of the Institute of Psychology and of the Social Science Research Center at the University of Erlangen–Nuremberg, Germany.*

*Andreas Beelmann is an assistant professor at the Institute of Psychology at the University of Erlangen–Nuremberg, Germany.*

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social behavior is a particularly frequent problem during childhood and a predictor of later criminality. Depending on diagnostic criteria and degrees of severity, approximately 5 to 20 percent of youngsters exhibit such behavior patterns (Lahey et al. 1999). Although these problems are often only a temporary developmental transition, serious and persistent offending in youth and adulthood can often be traced back to early manifestations of antisocial behavior and related risks (Hawkins et al. 1998; Lipsey and Derzon 1998; Lynam 1996; Moffitt 1993). Such findings have led to increasing demands for developmental prevention measures (Loeber and Farrington 1998, 2001; McCord and Tremblay 1992; Peters and McMahon 1996; Yoshikawa 1994). The frequently small effects of offender treatment programs (Lösel 2001b) and cost-benefit analyses (Farrington and Welsh 2002) also point to the feasibility of early developmental prevention of antisocial behavior.

*Antisocial behavior is a particularly frequent problem during childhood and a predictor of later criminality.*

There are numerous approaches to this goal, for example, parent training programs, home visits, day care, family therapy, preschool- or school-based child training, teacher training, multisystemic therapy, or combinations of such programs in more complex community-oriented approaches (see, e.g., Beelmann 2000; Loeber and Farrington 2001; Sherman et al. 1997; Wasserman and Miller 1998). One strategy that is applied relatively frequently is social skills training for children. These kinds of programs have practical advantages. For example, they can reach the whole target population (e.g., at school), may trigger relatively low costs (e.g., group training delivered by regular teachers), and are less difficult to implement in everyday practice than family-oriented or combined programs (Kazdin and Wassell 1999; Offord et al. 1998; Prinz and Miller 1994). Furthermore, training of social skills is also promising from the perspective of basic research. For example, aggression-prone schemata of social information processing, insufficient self-control, deficits in social problem solving, deviant beliefs, and a lack of prosocial interaction skills are empirically sound risk factors for antisocial behavior (e.g., Akhtar and Bradley 1991; Crick and Dodge 1994; Frick 1998). These and other factors have led not only to numerous small-scale studies but also, more recently, to some large-scale, well-designed programs of child skills training (e.g., Conduct Problems Prevention Research Group 1999).

Social skills training for children typically contains a structured program with a limited number of sessions, teaching nonaggressive modes of social perception, attribution, self-control, anger management, victim empathy, interpersonal problem solving, interaction, and related skills. More comprehensive programs covering the entire preschool education and combined with parent guidance (e.g., Schweinhart, Barnes, and Weikart 1993) should not be subsumed under this category. Various reviews of outcome evaluations suggest that child skills training is a promising approach to the prevention of antisocial behavior (e.g., Beelmann 2000; Denham and Almeida 1987; Erwin 1994; Kazdin 1998; Schneider 1992; Sherman et al. 1997). However, various problems justify only a cautious optimism (e.g., Beelmann, Pfingsten, and Lösel 1994; Bullis, Walker, and Sprague 2001; Gottfredson 2001; Gresham 1998; Lösel 2002). For example, many studies in our own earlier meta-analysis did not fulfill rigorous criteria of methodological quality. Although there was an overall effect size (ES) of $d = .47$, $r = .23$, the outcome for social adjustment was smaller. The most substantial effects were found in those criteria that were relatively close to the training contents. More generalizable outcomes in everyday behavior were less impressive. Similarly, postintervention effects became very small and nonsignificant when outcome was measured after a longer follow-up.

Against this background, the present article reports a systematic and up-to-date review of randomized studies on the preventive effects of child skills training on antisocial behavior and related outcomes. Our results should provide a basis for a differentiated evaluation of the preventive potentials of child skills training, permit conclusions on the practical implementation of such programs, and offer perspectives for practice, policy making, and research.

# Method

## *Selection of the primary studies*

Primary studies were selected according to the following eligibility criteria:

1. Child skills training: the study had to contain an evaluation specifically addressing a social training program for the prevention of antisocial behavior in children and youth. All studies evaluating additional programs or program components were excluded (e.g., programs with parent training, teacher training, or home visits). Likewise, we excluded programs focusing on other areas of problem behavior (e.g., prevention of suicide, substance use, child abuse, or internalizing problems).
2. Randomized control group design: the study had to have a treatment and a control group that were compared in a truly experimental (randomized) design. Stratified modes of randomization were also included (e.g., randomized field trial, randomized block design, matching plus randomization). Preintervention and postintervention data had to be available.
3. Age: the age range of the treated individuals had to be between zero and eighteen years.
4. Focus on prevention: the program had to be preventive in the narrow sense. Studies on primary or universal prevention and on targeted prevention in at-risk groups (selective or

indicated prevention) were included. Programs for already adjudicated delinquents or other clinical groups (e.g., internalizing disorders) were excluded. Programs for youngsters with conduct disorders or oppositional-defiant disorders were included because these targeted specific at-risk groups.

5. Outcome measures: the studies had to report outcomes in measures of (1) antisocial behavior (e.g., parent report, teacher report, self-report, observational data, or official records), (2) social skills (e.g., social interaction skills, prosocial behavior), or (3) social-cognitive skills (e.g., self-control, social problem-solving skills). Data had to be reported in sufficient detail to permit an adequate computation of ESs.
6. Publication data: we included all retrievable published or unpublished reports in the English or German languages that had appeared no later than 2000. Should funding permit, studies in other languages will be analyzed in the future.

### *Literature search*

The literature search pursued three strategies. First, we carried out an intensive check of electronic databases such as PSYCHINFO, MEDLINE, ERIC, and Dissertation Abstracts. Second, the references given in existing reviews on child skills training and the prevention of antisocial behavior were checked systematically (e.g., Ang and Hughes 2002; Beelmann, Pfingsten, and Lösel 1994; Brestan and Eyberg 1998; Durlak and Wells 1997, 1998; Farrington and Welsh 1999; Greenberg 2001; Sherman et al. 1997; Tremblay and Craig 1995; Tremblay, LeMarquand, and Vitaro 1999; Wasserman and Miller 1998; Wilson, Gottfredson, and Najaka 2001). Third, the references given in already identified primary studies were analyzed for further relevant publications.

A total of 851 articles could be identified with these strategies. From these, 230 reports were excluded in a first round because they obviously did not fulfill the selection criteria. The remaining 621 articles (80 percent published and 20 percent unpublished) formed a study pool that was checked in more detail. We excluded studies step by step when they were not published in English or German (7); did not specifically address social skills training programs (148); did not aim primarily to prevent antisocial behavior (106); addressed already delinquent or other clinical groups (66); lacked an untreated control group (13); did not use a randomized design (96); were double or supplementary publications of already included studies (19); did not contain sufficient statistical data for further analysis (30); reported no outcome data on antisocial behavior, social skills, and social-cognitive competencies (39); and were unobtainable (13). This left 84 research reports that met our eligibility criteria. These studies contained $N$ = 16,723 youngsters, of whom 52.3 percent belonged to the treatment groups. A number of studies contained more than one treatment or control group or separate analyses for children and adolescents or boys and girls. Therefore, the final database for our meta-analysis was 135 treatment- versus control-group comparisons.

### *Coding and computation of ESs*

The coauthor and a trained student coded all comparisons according to a detailed scheme. This contained characteristics of publication (e.g., year, country),

methods (e.g., design, follow-up), intervention programs (e.g., type, intensity, setting), and the trained children (e.g., age, gender, risk factors). A selection of these variables is presented in the Results section (see Table 1). A subsample of twenty-four comparisons was analyzed independently by two coders. Interrater agreement varied between 81 percent and 100 percent depending on category ($M$ = 96.3 percent).

*In the analysis of postintervention outcomes, all total effects were significant.*

Because most outcomes were quantitative variables, we used Cohen's (1988) $d$ coefficient to compute ESs. When the relevant data were available, we computed the ES as the difference between the predifference/postdifference scores between the treatment group and the control group divided by the pooled standard deviation in the pretest. If no means and standard deviations had been reported, recomputation and ES estimation techniques were used (see Lipsey and Wilson 2001). If the reports mentioned nonsignificant results without details, we counted this as a zero effect. Although a nonsignificant result does not necessarily mean "no effect" (Weisburd, Lum, and Yang 2003 [this issue]), the lack of statistical data did not permit a less conservative strategy. Overall, we computed 716 independent ESs (29.6 percent by methods of recomputation or estimation). Of these, 548 (76.5 percent) referred to postintervention measurements of outcome, 156 (21.8 percent) addressed a first follow-up, and 12 (1.7 percent) a second follow-up.

### *Integration and statistical analysis*

In several comparisons, the postintervention measures were not assessed immediately after the training but several months later. Other studies had shorter follow-up periods than in these posttests. Therefore, we used a common time metric to avoid confusion. Due to the small number of studies with relatively long follow-up periods (see Table 1), we used only two categories: each ES that referred to assessments up to three months after the training were categorized as postintervention effects. Measures that had been assessed later were subsumed to the follow-up category. This strategy produced 519 posttest ESs (80.6 percent) and 125 follow-up ESs (19.4 percent). We computed a separate ES for each of the outcomes assessing a specific construct. Then, the various effects were integrated within and also across the three categories of outcome (antisocial behavior, social

TABLE 1

**DESCRIPTION OF THE 135 TREATMENT/CONTROL GROUP COMPARISONS**

| Study Characteristic and Coding | Frequency | Percentage |
|---|---|---|
| General study characteristics | | |
| Publication year[a] | | |
| Up to 1980 | 19 | 22.6 |
| 1981-1990 | 39 | 46.4 |
| 1991-2000 | 26 | 31.0 |
| Publication type[a] | | |
| Journal article | 78 | 92.9 |
| Book, chapter | 2 | 2.4 |
| Unpublished | 4 | 4.8 |
| Country[a] | | |
| United States | 71 | 84.5 |
| Canada | 4 | 4.8 |
| England | 2 | 2.4 |
| Australia | 2 | 2.4 |
| Other | 5 | 6.0 |
| Methodological characteristics | | |
| Sample size | | |
| Less than 30 | 56 | 41.5 |
| 30-49 | 42 | 31.1 |
| 50-149 | 14 | 10.4 |
| 150-500 | 10 | 7.4 |
| Greater than 500 | 4 | 3.0 |
| Type of randomization | | |
| Individual | 67 | 49.6 |
| Pairwise | 7 | 5.2 |
| Blockwise | 29 | 21.5 |
| Groupwise | 32 | 23.7 |
| Type of control group | | |
| Attention-placebo | 53 | 39.3 |
| No treatment | 82 | 60.7 |
| Follow-up | | |
| No | 76 | 56.3 |
| Yes (second) | 43 (6) | 31.9 |
| No adequate data | 15 | 11.1 |
| Time of first measurement | | |
| Less than 1 month | 119 | 88.1 |
| 1-3 months | 7 | 5.2 |
| Greater than 3 months | 9 | 6.7 |
| Time at follow-up | | |
| No follow-up | 76 | 56.3 |
| Less than 1 month | 4 | 3.0 |
| 1-2 months | 25 | 18.5 |
| 3-6 months | 9 | 6.7 |
| 12 months (second) | 15 (4) | 11.1 |
| Greater than 12 months (second) | 5 (2) | 3.7 |
| Mean dropout rate (%) | | |
| Up to 5 | 81 | 60.4 |
| 5 to 10 | 12 | 8.9 |

*(continued)*

TABLE 1 (continued)

| Study Characteristic and Coding | Frequency | Percentage |
|---|---|---|
| 11 to 20 | 27 | 20.0 |
| Greater than 20 | 15 | 11.1 |
| Type of outcome | | |
| Antisocial behavior | 89 | 65.9 |
| Social skills, social competence | 62 | 45.9 |
| Social-cognitive skills | 60 | 44.9 |
| Treatment characteristics | | |
| Type of treatment | | |
| Behavioral | 39 | 28.9 |
| Cognitive | 23 | 17.0 |
| Cognitive-behavioral | 53 | 39.3 |
| Counseling, psychotherapy, other | 20 | 14.8 |
| Number of sessions | | |
| Up to 10 | 56 | 41.5 |
| 11-30 | 45 | 33.3 |
| 31-60 | 22 | 16.3 |
| Greater than 100 | 1 | 0.7 |
| Not specified | 11 | 8.1 |
| Treatment duration | | |
| Up to 1 month | 23 | 17.0 |
| 1-2 months | 46 | 34.1 |
| 2-4 months | 38 | 28.1 |
| 4-6 months | 12 | 8.9 |
| 6-12 months | 11 | 8.1 |
| Greater than 12 months | 2 | 1.5 |
| Not specified | 3 | 2.2 |
| Format of treatment | | |
| Individual training | 14 | 10.4 |
| Group training | 105 | 77.8 |
| Individual plus group training | 8 | 5.9 |
| Self-instruction | 3 | 2.2 |
| Individual coaching | 5 | 3.7 |
| Setting | | |
| Preschool/kindergarten | 10 | 7.4 |
| School | 100 | 74.1 |
| Clinic, special education unit | 9 | 6.7 |
| Community | 6 | 4.4 |
| Other | 10 | 7.4 |
| Trainers | | |
| Teachers | 31 | 23.0 |
| Psychosocial professionals | 35 | 25.9 |
| Study authors, research staff | 22 | 16.3 |
| Supervised students | 30 | 22.2 |
| Others | 4 | 3.0 |
| Not specified | 13 | 9.6 |
| Child characteristics | | |
| Age (years) | | |
| 4-6 | 26 | 19.3 |
| 7-9 | 54 | 40.0 |

*(continued)*

TABLE 1 (continued)

| Study Characteristic and Coding | Frequency | Percentage |
|---|---|---|
| 10-12 | 38 | 28.1 |
| 13-15 | 14 | 10.4 |
| 16-18 | 3 | 2.2 |
| Gender (% male) | | |
| 0 | 7 | 5.2 |
| 40-59 | 44 | 32.6 |
| 60-79 | 28 | 20.7 |
| 80-99 | 17 | 12.6 |
| 100 | 24 | 17.8 |
| Not specified | 15 | 11.1 |
| Type of prevention | | |
| Universal | 31 | 23.0 |
| Selective | 41 | 30.4 |
| Indicated | 63 | 46.7 |
| Number of risk factors | | |
| No risks | 31 | 23.0 |
| Single | 65 | 48.1 |
| Multiple | 39 | 28.9 |
| Type of risks[b] | | |
| Antisocial behavior | 58 | 43.0 |
| Multiproblem milieu | 41 | 30.4 |
| Deficits in social skills/competence | 36 | 26.7 |
| Academic/cognitive deficits | 23 | 17.0 |
| Sociometric status | 6 | 4.4 |

a. Based on eighty-four research reports.
b. Based on 164 codings.

skills, social-cognitive skills). Accordingly, there was only one ES for each category and each comparison at the different times of measurement.

When computing mean effects, we followed Hedges and Olkin's (1985) models of weighting for sample size. At first, the fixed model was applied to integrate the single effects. When ES distributions remained heterogeneous, we finally used the random (mixed) model to estimate ES (for details, see Lipsey and Wilson 2001).

# Results

## *Descriptive characteristics*

Table 1 contains some descriptive characteristics of the 135 comparisons included in the meta-analysis. Although there was a substantial number of comparisons both before and after the 1980s, nearly half of them were conducted during this period. Most studies fulfilling our inclusion criteria were published in journals and were overwhelmingly conducted in the United States.

Small samples were in the majority. Only 10.4 percent of the comparisons had sample sizes larger than 150. One-half of the comparisons used randomization on the individual level, and 45 percent were randomized on an aggregate level (group or block). Traditional no-treatment control groups were most frequent. The majority of studies had drop-out rates of less than 10 percent ($M$ = 7.0, range 0-45.2 percent). One-half of the comparisons measured outcomes more or less immediately after the training. Follow-up measurements were much less frequent, and the time intervals were relatively short. Less than 10 percent of the comparisons with a follow-up measured the outcome later than one year after the training. Less than one-fifth had both postintervention and follow-up measurement. Two-thirds of the comparisons contained direct measures of antisocial behavior as outcome criteria (mainly teacher, parent, observer, or self-ratings; rarely official data).

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*The mean [effect size] for posttest measures of antisocial behavior was smaller than that for measures of social and social-cognitive skills.*

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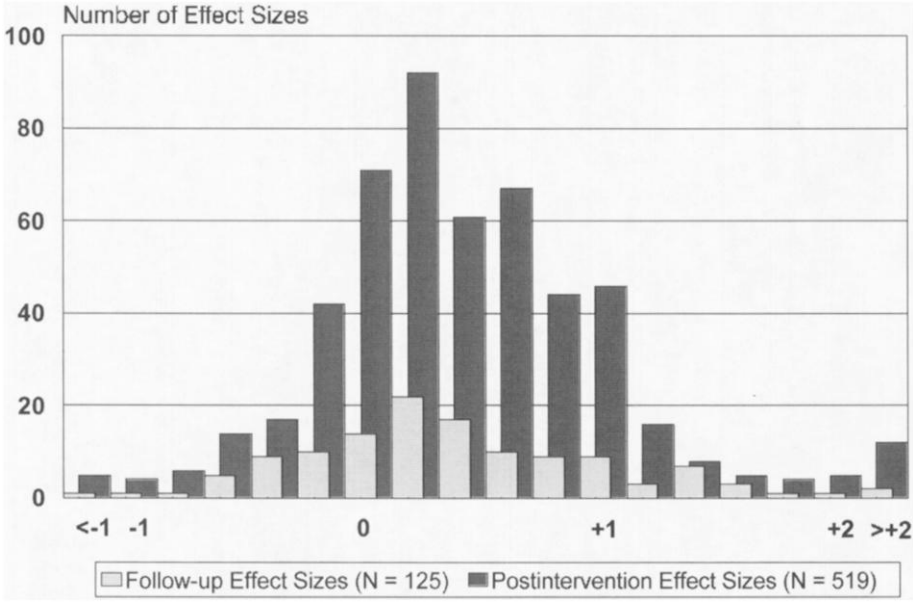
Approximately 85 percent of the programs were based on a behavioral and/or cognitive model of social learning. Cognitive-behavioral approaches that address both problematic modes of thinking and concrete patterns of social behavior were most frequent. Other programs such as counseling, psychotherapy, or intensive care were investigated less frequently. More than two-fifths of the programs were short (up to ten sessions), and three-quarters had up to thirty sessions. Most frequently, the programs lasted one to four months. The typical format was group training carried out in the school setting. There was no clear preference for a specific group of trainers.

The mean age of the trained children varied from four to eighteen years. However, there was a focus on the elementary school level. Nearly 80 percent of the comparisons addressed children younger than twelve. Most studies contained mixed samples of boys and girls. However, in line with the higher prevalence of antisocial behavior in males, boys were overrepresented. Few studies contained girls only. Programs targeting children who already exhibited some antisocial behavior (indicated prevention) or who had other risk factors (selective prevention) were more frequent than programs for unselected groups (universal prevention). More than three-quarters of the comparisons mentioned at least one risk factor. Forms of antisocial behavior were the most frequent risks followed by a multiproblem family milieu and cognitive or social skill deficits.

FIGURE 1
**DISTRIBUTION OF SINGLE EFFECT SIZES**




*Intervention effects*

Figure 1 shows the frequency distribution of the unweighted and not yet integrated ESs ($n$ = 716). These ranged between –2.39 and 2.79. Although a substantial portion of the effects were negative, the majority revealed a positive outcome (better results for the treated group). The overall mean of unweighted and partially dependent study effects was $d$ = .36. Table 2 contains the more detailed analysis and results of the integration of multiple study effects.

In the analysis of postintervention outcomes, all total effects were significant. The results remained fairly similar when the various outcomes were integrated into one ES for each comparison. The total effects became smaller when the comparisons were integrated by weighting for sample size. This indicates smaller effects in larger samples. However, in both the fixed and the random model, the total effect remained significant. Because the fixed model revealed significant heterogeneity beyond the subject-level sample error, the random model seemed to be more adequate for our data (see Lipsey and Wilson 2001). According to this model, the mean total postintervention effect was $d$ = .38 (equivalent to $r$ = .19).

The total ES of the follow-up measurements was slightly smaller than that of the postintervention measurements. However, the effects still remained significant. According to the random model, the total ES in follow-up measures was $d$ = .28 (equivalent to $r$ = 14). Although the smaller effects in the follow-up are plausible, it

TABLE 2

**RESULTS FROM 135 TREATMENT/CONTROL GROUP COMPARISONS, BY MEASUREMENT TIME AND TYPE OF OUTCOME**

| | Unweighted ES | | | | | Weighted ES | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | Single ES | | | Comparison ES | | All Comparisons | | | Comparisons with Posttest and Follow-Up | | |
| Outcome Measure | *M* | *SD* | $N_{ES}$ | *M* | *SD* | Fixed Model | Random Model | $N_C$ | Fixed Model | Random Model | $N_C$ |
| Postintervention measurement[a] | | | | | | | | | | | |
| Total | .37 | .60 | 519 | .46 | .52 | .26[b,c] | .38[b] | 126 | .25[b,c] | .37[b] | 25 |
| Antisocial behavior | .27 | .54 | 190 | .31 | .58 | .17[b,c] | .26[b] | 80 | .16[b,c] | .35[b] | 13 |
| Social skills | .42 | .70 | 146 | .46 | .52 | .29[b,c] | .39[b] | 61 | .19[b,c] | .32[b] | 15 |
| Social-cognitive skills | .43 | .55 | 183 | .49 | .44 | .29[b,c] | .40[b] | 57 | .19[b,c] | .34[b] | 10 |
| Follow-up measurement[d] | | | | | | | | | | | |
| Total | .34 | .64 | 125 | .37 | .55 | .14[b,c] | .28[b] | 34 | .23[b,c] | .32[b] | 25 |
| Antisocial behavior | .23 | .61 | 48 | .30 | .71 | .06[c] | .22[b] | 20 | .16[b,c] | .34[b] | 13 |
| Social skills | .40 | .74 | 44 | .46 | .49 | .13[b,c] | .38[b] | 16 | .26[b,c] | .45[b] | 15 |
| Social-cognitive skills | .50 | .52 | 33 | .49 | .40 | .33[b] | .33[b] | 12 | .33[b,c] | .33[b] | 10 |

NOTE: ES = effect size; $N_{ES}$ = number of ESs; $N_C$ = number of treatment/control group comparisons.

a. All ESs measured within two months after treatment ($M$ = .26, $SD$ = .42, range = 0-2).

b. ES differs significantly from zero.

c. ES shows significant heterogeneity.

d. All ESs measured three months or more after treatment ($M$ = 8.32, $SD$ = 6.93, range = 3-42).

must be taken into account that the postintervention effects were based on a different (larger) sample of comparisons. Other study features could have been confounded with the issue of measurement time. Therefore, a further analysis contained only those twenty-five comparisons with both a postintervention measurement and a follow-up measurement (right-hand side of Table 2). Here, the mean total effect was $d = .37$ in the posttest and .32 in the follow-up (random model).

As Table 2 shows, the type of outcome criteria was one source of heterogeneous effects. The mean ES for posttest measures of antisocial behavior was smaller than that for measures of social and social-cognitive skills. This was particularly the case for the few comparisons with nonreactive outcome measures such as school referral, police contact, or official delinquency ($d = .16, p < .05$). Effects on antisocial behavior were also smaller in the follow-up (particularly in the fixed model). However, the analysis of the twenty-five comparisons containing two measurement points revealed a somewhat different picture. Here, the mean effects for antisocial behavior in the posttest and in the follow-up were both similar and significant.

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*Small program effects should not be underrated in policy making and practice.*

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Because the random model of ES integration appeared to be the most adequate for our data, we shall present only the results of these computations in the further analysis of moderators (see Table 3). In some categories, there were very few or no comparisons with specific outcome criteria. Therefore, Table 3 reports only the total effects. When specific effect criteria differed consistently from the general picture, this will be mentioned in the text. For reasons of space and clarity, we also included only those categories from Table 1 that had strong theoretical or political relevance.

*General study characteristics*. Studies published in the 1990s had the smallest effects. However, tests of heterogeneity were not significant. There was no significant difference between published and unpublished reports.

*Methodological characteristics*. Sample size exerted a significant and linear effect on the outcomes in the posttest ($\chi^2(3) = 8.02, p < .05$). Comparisons based on small samples had higher ESs than comparisons based on relatively large samples.

TABLE 3

**RELATION BETWEEN STUDY CHARACTERISTICS AND MEAN EFFECT SIZE**

| Moderator/Category | Postintervention ES | | Follow-Up ES | |
|---|---|---|---|---|
| | $d$ | $N_C$ | $d$ | $N_c$ |
| Publication year[a] | | | | |
| Up to 1980 | .43[b] | 33 | .34 | 6 |
| 1981-1990 | .41[b] | 62 | .33[b,c] | 15 |
| 1991-2000 | .30[b] | 31 | .23[b,c] | 12 |
| Sample size[d] | | | | |
| Up to 30 | .48[b] | 56 | .39[b] | 14 |
| 30-49 | .41[b] | 42 | .40[b] | 7 |
| 50-149 | .34[b] | 14 | .33[b] | 5 |
| Greater than or equal to 150 | .22[b] | 14 | .16 | 8 |
| Type of randomization[d] | | | | |
| Individual, pairwise | .50[b] | 68 | .28[b] | 20 |
| Blockwise, groupwise | .25[b] | 58 | .28[b] | 14 |
| Type of treatment[a] | | | | |
| Behavioral | .37[b] | 37 | .17 | 5 |
| Cognitive | .39[b] | 25 | .36[b] | 9 |
| Cognitive-behavioral | .39[b] | 47 | .37[b] | 14 |
| Counseling, therapy, other | .36[b] | 17 | .17[b] | 6 |
| Trainers[d] | | | | |
| Teachers, psychosocial professionals | .29[b] | 59 | .27[b] | 21 |
| Study authors, research staff, supervised students | .49[b] | 50 | .40[b] | 10 |
| Age[d] | | | | |
| 4-6 | .31[b] | 24 | .74[b] | 6 |
| 7-12 | .39[b] | 85 | .20[b] | 26 |
| 13 and older | .41[b] | 17 | .78[b] | 2 |
| Type of prevention[a] | | | | |
| Universal | .38[b] | 30 | .15 | 4 |
| Selective | .30[b] | 46 | .23[b] | 18 |
| Indicated | .45[b] | 50 | .41[b] | 12 |

NOTE: ES = effect size.

a. No significant difference between categories in postintervention or follow-up ES (see text).

b. ES differs significantly from zero.

c. ES shows significant heterogeneity.

d. Significant difference between categories in postintervention or follow-up ES (see text).

A similar but nonsignificant trend was found in the follow-up. Studies that were randomized at the level of individuals had stronger effects in postintervention measures ($\chi^2(1) = 11.31, p < .001$). With respect to type of control group and drop-out rate, there were no significant differences.

*Treatment characteristics*. Although there was a tendency for smaller postintervention effects of counseling/psychotherapy and behavioral programs,

mode of treatment was no significant moderator. The nonsignificant ES in the behavioral category was based on a small number of comparisons. In line with the respective program focus, the behavioral programs exhibited their highest postintervention effect in measures of social skills ($d = .55, n = 21, p < .05$). Likewise, the cognitive programs revealed their best postintervention results in measures of social-cognitive skills ($d = .49, n = 19, p < .05$). The outcome pattern for the cognitive-behavioral programs was most robust: all three types of outcome at both times showed effects of at least .30, and five out of six effects were significant. Cognitive-behavioral programs also revealed the highest follow-up effect in measures of antisocial behavior ($d = .62, n = 7, p < .05$).

The type of trainer had a significant effect on the postintervention outcome ($\chi^2$ (1) = 6.01, $p < .05$): programs that were delivered by the authors, researchers, or trained students had larger effects. Treatment dosage was not significantly associated with ES. The same was found for the individual versus group format.

*Child characteristics*. Although age (like gender) was no significant moderator of total postintervention outcome, programs for the youngest and oldest children revealed the largest effect in the follow-up ($\chi^2$ (1) = 16.59, $p < .001$). However, the findings in the oldest group were based on only two studies. When the various outcome criteria were considered, studies with four- to six-year-olds revealed significant effects only in social and social-cognitive skills. In contrast, the postintervention ES in antisocial behavior was significant for both other groups (seven to twelve years: $d = .27$; older than twelve years: $d = .39$). In the follow-up measurements of antisocial behavior, the number of comparisons was too small to show a clear pattern.

Indicated prevention tended to have the largest effects in both postintervention and follow-up measurements. Accordingly, ES was somewhat larger in groups with multiple risk factors than in no-risk groups. This pattern became clearer when we differentiated between the various outcome categories: only indicated approaches had a significant ES in both postintervention and follow-up measures of antisocial behavior ($d = .45$ and .47, $n = 40$ and 8). Universal and selected prevention had their strongest effects in social-cognitive skills and social skills ($d$- range = .23-.80; six out of eight significant). However, indicated prevention also revealed significant outcomes in these categories ($d$-range = .29-.52; all four significant).

## Discussion

The present meta-analysis permits relatively reliable conclusions because all evaluations are based on randomized control-group designs. The average drop-out rate of 7 percent means that there are also no very severe threats to internal validity in this respect. Statistical validity is threatened by the low sample size in many of the studies. However, because our meta-analysis contains a large number of studies (see appendix) and a total of 16,733 youngsters, the findings on total effects can

be considered reliable. These indicate a generally highly significant postintervention effect of child skills training of either $d = .26$ or $d = .38$, depending on which computation model is applied. Although such ESs are small (Cohen 1988), they can well be beneficial in practice: the most adequate random model effect estimate of $d = .38$ corresponds to a correlation of $r = .19$. This would mean, for example, that given a failure rate of 50 percent in the control group, there would be approximately 19 percentage points less failure in the treatment group. This would be a reduction of 38 percent. Such effects are similar to those found for recognized methods of medical treatment (Lipsey and Wilson 1993). Cost-benefit analyses in the field of crime prevention show that they may pay off from monetary perspectives as well (Welsh and Farrington 2001). Hence, small program effects should

*[M]ore randomized studies with large samples and long follow-up periods are needed.*

not be underrated in policy making and practice. One should also bear in mind that specific risk factors for antisocial behavior rarely have a predictive power greater than $r = .20$ (Lösel 2002). Larger effects of prevention can be expected only when programs are combined and applied multimodally in various risk areas. Examples are combinations of child skills training, parent training, or school-oriented prevention (e.g., Hawkins et al. 1999; Kazdin, Siegel, and Bass 1992; Lösel, Beelmann, and Koglin 2001; Tremblay et al. 1995; Webster-Stratton and Hammond 1997). However, such approaches are normally more difficult to implement than are child skills training alone.

Nonetheless, the positive postintervention effect needs to be qualified by two differential findings. First, the measurements in the follow-up reveal a slightly lower effect, although the time periods were rarely longer than one year. Second, although outcomes for antisocial behavior were rarely based on official data, they are somewhat lower than those for measures of social and social-cognitive skills. Both findings indicate the need for caution regarding the long-term effects of child skills training on the prevention of criminal careers (see also McCord 2003 [this issue]). Overall, the postintervention effects are smaller than those in previous meta-analyses on social competence training in children (e.g., Beelmann, Pfingsten, and Lösel 1994; Denham and Almeida 1987; Erwin 1994; Schneider 1992). To some extent, this may be due to our closer focus on antisocial behavior and truly preventive programs compared with previous meta-analyses that also

included studies of clinical groups and other behavioral problems. Another reason for lower ES may be our restriction to randomized designs. For example, Weisburd, Lum, and Petrosino (2001) have shown that the weaker the design in criminal justice evaluations, the stronger the probability that a study will report a result in favor of treatment. The comparison with criminal justice research also reveals that the effects of child skills training are in the same range as those of appropriate offender treatment programs that are often based on longer follow-up intervals and official outcome measures (Lipsey and Wilson 1998; Lösel 1995; McGuire 2001). Therefore, our meta-analysis does not support a polarization between early developmental prevention and later treatment (see Lösel 2002). In other words, it is never too early and never too late for intervention (Loeber and Farrington 1998).

Because the total effect in our meta-analysis is based on heterogeneous study samples, we analyzed some moderator effects. For the following reasons, such results need to be interpreted very cautiously. (1) We performed multiple significance testing without alpha adjustments. (2) Some effects are based on only a few studies. (3) The random model is not very sensitive for moderator effects (Lipsey and Wilson 2001). (4) The moderators are to some extent confounded (see Lipsey 2003 [this issue]).

A significant moderator effect was due to sample size, with larger samples leading to smaller ESs. One explanation of this result is related to publication bias: larger samples are more likely to reveal the significance of a true low effect that may be overlooked in smaller studies (Weisburd, Lum, and Yang 2003). Due to author or editor decisions, this significant result may have been published more frequently than the same, but nonsignificant, effect in a smaller sample. Similar decisions can favor the publication of studies with small sample sizes when they have relatively large effects. Although our analysis reveals no indication of such a difference, one has to remember that we included only a few unpublished studies. This may be an effect of the randomization criterion: studies that meet such high methodological standards are not just presented in conference papers or unpublished dissertations but are also published in journals. This interpretation is supported by our finding that only a few studies from Dissertation Abstracts met our inclusion criteria, and these were relatively new references that may well be published in the future. A second explanation of the smaller effect in large studies involves issues of program implementation. In large studies, difficulties in maintaining program integrity and homogeneity of samples or treatments may reduce design sensitivity (Weisburd, Petrosino, and Mason 1993). For example, the relation between integrity and ES (Lösel and Wittmann 1989) may be responsible for our result that program delivery by the study authors, other research staff, or (their) students leads to larger effects in postintervention outcome. Probably, these trainers are particularly interested in delivering the training as planned. Similarly, Lipsey (1992) and Lipsey and Wilson (1998) found higher ESs in young offender treatment programs that had smaller samples and were monitored by the researcher. Naturally, a positive measurement bias cannot be ruled out either.

Type of randomization also moderates the outcomes: studies that are randomized on the level of individuals show larger postintervention effects than studies that are randomized on the group or block level (e.g., school classes). Because there is no similar effect in the follow-up, we suppose that the very large Fast Track Program had an influence. This highly weighted study of thousands of youngsters applied groupwise randomization and reports relatively small postintervention effects (Conduct Problems Prevention Research Group 1999). Such large studies may be a further reason why the mean effects in the 1990s are somewhat lower than before.

Unexpectedly, there is no significant effect of treatment type on the total outcome. However, the highest effects in the follow-up are found in cognitive and cognitive-behavioral programs. This is plausible insofar as they address relatively broad behavioral dispositions. There may be various reasons for the lack of significant differences: most programs are based on social learning theories and partially overlap in their contents. Their specific focus is reflected in specific outcomes

*Well-implemented, cognitive-behavioral programs targeting high-risk youngsters who already exhibit some behavioral problems seem to be particularly effective.*

(e.g., behavioral programs affect social skills most strongly, and cognitive programs affect social-cognitive skills most strongly). These differences tend to disappear in the total effects. A further reason may be that the counseling or therapeutic programs no longer contain just unstructured casework. Intensive care programs such as the Big Brother/Big Sister Program (Grossman and Tierney 1998) include elements corresponding to a cognitive-behavioral approach. Similarly, individual counseling programs also reveal relatively positive effects in the treatment of noninstitutionalized juvenile offenders (Lipsey and Wilson 1998).

When we look beyond the total outcome, there are clearer differences between the treatment modes (although these do not become significant because the number of comparisons is sometimes so small). In particular, only cognitive-behavioral programs have significant effects in both postintervention and follow-up measurements of antisocial behavior. This is in line with the finding that well-structured, multimodal, cognitive-behavioral treatments are basically most appropriate for dealing with problems of antisocial behavior (see Lösel 1995; McGuire 2001).

However, one should not pay too much attention to discriminating according to therapeutic traditions or therapeutic schools. The actual contents of the program delivery are just as important (Lösel 2001a).

A moderator effect is also found in child characteristics: programs for the youngest and for the oldest samples have the largest follow-up ESs. However, this result is based on only two studies in the adolescent group and is not consistent with the findings in the posttest and across various outcome categories. Our findings on the type of prevention seem to be more relevant: programs targeting youngsters who have already exhibited some behavioral problems (indicated prevention) have the highest ESs, whereas universal prevention has the lowest ES. This applies particularly to the outcomes in antisocial behavior in which the difference becomes significant. Analogous to this, the best follow-up effects are found in those comparisons in which the children exhibit multiple risk factors. These patterns agree with findings on the treatment of juvenile delinquency that also show larger effects in groups with higher risk (Lipsey and Wilson 1998).

Our findings on indicated prevention cannot be interpreted as an artifact of regression to the mean, because we have included only randomized studies. The best explanation is that the programs actually do influence deviant developmental tendencies in groups at high risk. Although there are also immediate learning processes in the unselected groups, most of these children would not develop serious behavioral problems even without the programs. Correspondingly, no major differences in behavior compared with untreated control groups can be expected in the long term. This is supported by the nonsignificant effect in universal prevention at the follow-up. However, the finding of higher ES in indicated prevention should not be overgeneralized to extremely high-risk youngsters such as those exhibiting early psychopathic tendencies (Frick 1998; Lynam 1996). An inverted U-shaped relationship between risk level and program effect seems to be most plausible (Lösel 2001a).

Our meta-analysis does not just provide a systematic review of program effects and moderators but also reveals deficits and blind spots in research: as mentioned above, more randomized studies with large samples and long follow-up periods are needed. More studies should also include nonreactive outcome measures of antisocial behavior. In addition, only a few studies focus particularly on girls. It is also worth noting how few randomized studies have been performed outside the United States. This is not just because we have restricted our meta-analysis to English- and German-language reports: the English-speaking countries outside the United States also reveal very few randomized studies on the effect of child skills training on the prevention of antisocial development. The situation is even worse in the German-speaking countries, and our literature search also indicates hardly any reports on relevant experiments in the Romance, Slav, or Asiatic tongues. This is a serious deficit because programs and findings from the United States cannot simply be transferred to other cultural contexts. It is to be hoped that the initiatives of the Campbell Collaboration Group will increase the methodological quality of evaluations throughout the world.

## Conclusion

Our systematic review reveals that there are a substantial number of randomized experiments addressing the efficacy of social skills training in preventing antisocial behavior in childhood and youth. These studies demonstrate a positive overall effect that is small but robust. Well-implemented, cognitive-behavioral programs targeting high-risk youngsters who already exhibit some behavioral problems seem to be particularly effective. Such programs can be recommended to policy makers. However, most findings are based on small samples, behavior ratings of outcomes, and relatively short follow-up periods after the interventions. Therefore, we still do not know how far child skills training prevents long-term criminal development. More well-controlled experiments using larger samples, hard outcome criteria, and long follow-up periods are needed, particularly outside the United States.

## Appendix
## Studies Integrated into the Meta-Analysis (Including Double, Supplementary, and Separate Follow-Up Publications)

Andrews, D. W., L. H. Soberman, and T. J. Dishion. 1995. The adolescent transitions program for high-risk teens and their parents: Toward a school-based intervention. *Education and Treatment of Children* 18:478-98.

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Beelmann, A. 2000. Prävention dissozialer Entwicklungen: Psychologische Grundlagen und Evaluation früher kind- und familienbezogener Interventionsmaßnahmen (Prevention of antisocial developments: Psychological foundations and evaluation of early child- and family-oriented interventions). Unpublished postdoctoral habilitation thesis, University of Erlangen–Nuremberg, Germany.

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