

Effective Interventions in Dropout Prevention: A Research Synthesis

The Effects of Cognitive- Behavioral Interventions on Dropout for Youth with Disabilities

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National Dropout Prevention Center
for Students with Disabilities



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NDPC-SD was established to support state education agencies in assisting local education agencies to increase school completion rates and decrease dropout rates among students with disabilities. NDPC-SD supports states through the following activities:

- Identifying evidence-based dropout prevention interventions, programs and practices.
- Producing evidence based knowledge useful to school professionals
- Providing targeted technical assistance to states in a variety of formats.
- Disseminating dropout prevention information through multiple methods.

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ABSTRACT

The relationship between cognitive-behavioral interventions/therapies (the intervention) and dropout outcomes and violent verbal or physical aggression (the outcomes) for secondary aged youth with disabilities was explored in this systematic review. A total of 16 studies intervening with 791 youth with behavioral disorders, attention deficit/hyperactivity disorders, and learning disabilities were reviewed. The findings of this review strongly support the efficacy of the use of cognitive-behavioral interventions across educational environments, disability types, ages, and gender in the reduction of dropout and correlates of dropout. A series of more detailed implications for practice are suggested as well as directions to the reader to locate more detailed descriptions of how these interventions might be implemented in their secondary educational environments.

The Effects of Cognitive-Behavioral Interventions on Dropout for Youth with Disabilities

PURPOSE

Introduction

This practice-based systematic review summarizes the scientifically-based research studies that have been produced in the past two decades from three distinct perspectives: (a) cognitive-behavioral interventions, (b) dropout or dropout-related outcomes, and (c) samples of secondary-aged youth with disabilities. By *scientifically-based research studies* we mean reports of research studies that meet minimum standards of internal and external validity. These standards include, for example, explanations of how multi-group studies have assured minimal equality of groups (through randomization, matched sampling, or statistical use of covariates), clear explanations of the intervention, and some evidence of validity and reliability of the outcome measures. These studies may have employed group-based designs, single-participant designs, or qualitative designs, but they must report adequate evidence of these two sets of validity standards. By *cognitive-behavioral interventions* we mean these original research studies must have reported on the effects of implementing an intervention that had as its defining characteristics the use of:

... the greatest emphasis on the learning process and the influence of the contingencies and models in the environment while underscoring the centrality of the individual's mediating-information-processing style (Kendall, 1993, p. 235).

By *dropout or dropout-related outcomes* we mean studies that measured actual dropout from school, or powerful correlates of dropout such as persistence in therapy or program directly designed to assist preventing dropout, or violent verbal or physical aggression. Studies by

Cairns, Cairns, and Neckerman (1989), French and Conrad (2001), and Janosz, Le Blanc, Boulerice, and Tremblay (2000), for example, have consistently found that dropout in high school was predicted by aggressive behavior in middle and high school years. Finally, by *samples of secondary-aged youth with disabilities* we mean studies whose samples were either youth with disabilities or were, in part, youth with disabilities and outcome measures for those youth with disabilities were reported separately. These youth must have been enrolled in secondary school environments or, if in non-graded residential or day treatment facilities, the studies must have reported the ages of those youth with disabilities as ages 13-22 inclusive.

The conceptual framework we used to guide our philosophical orientation to this systematic review is grounded in the ecological model of social functioning to help answer “what works” questions for preventing dropout for youth with disabilities. An ecological framework provided the necessary conceptual structure to guide the scope, the methodology, and the development of this research synthesis. The question of “what works” can be translated by the classical ecological question posed by Wachs (1987): “Under what environments (situations, programs and settings) have what kinds of persons (the diverse characteristics of all youth with disabilities) changed in what kinds of behaviors (school and therapeutic persistence, violent behavior)?”

This ecological framework focuses on the transactional relationship among persons, environments and behaviors and was first introduced in 1936 by Karl Lewin. Since Lewin’s work, the application of the ecological framework has impacted much of the theoretical and implementation strategies associated with a wide range of human services and education. The ecological approach to understanding human behavior is well documented in the field of psychology (Barker, 1968; Moos, 1976; Bandura, 1971; Wicker, 1979).

BACKGROUND

Preventing youth from dropping out of school is an enormous challenge for school systems; when effective strategies can be implemented there are with extraordinary benefits for youth, communities, and society. According to the National Center for Education Statistics (NCES), in 2001 approximately 3.8 million 16-24 year olds were not enrolled in a high school program and had not completed high school, representing approximately 11% of this age group nationally (Kaufman, Alt, & Chapman, 2004). We know that Hispanic students drop out at the highest rates and that males still outnumber females in dropping out of school. We know that a higher percentage of students in the southern U. S. drop out than in any other region in the country (NCES, 2001). What compounds this issue is that many of these students also have disabilities. The federal government has kept track of dropout/graduation rates for students with disabilities, but until 2004, they have not integrated this information into the national dropout databases (NCES, March 12, 2004).

The National Center on Secondary Education and Transition published a report discussing the issue of dropout among students with disabilities. They have reported several alarming and continuing findings from the literature. For example, in the 1999-2000 school year, just 57% of students with disabilities who completed or left school were graduated with a regular diploma (U.S. Department of Education, 2001). And we know that proportionately almost twice as many students with disabilities drop out as typical students and that the very highest dropout rates among students with disabilities is found among students with emotional and/or behavioral disorders (Blackorby & Wagner, 1996). We also have recent documentation that high school graduation requirements and high stakes exit examinations in high schools are

increasing (Johnson & Thurlow, 2004) possibly making it more difficult for these students to say in school.

A number of narrative reviews have been published in recent years addressing the problem of dropout and correlates of dropout for youth with disabilities. In an exhaustive review conducted a decade ago, Whitaker (1993) concluded that behavioral, contingency management approaches were the most effective, although he found a number of studies recommending social skills training and self-control management techniques as well. Kashani, Jones, Bumby, and Thomas (1999) examined the literature set on controlling youth violence, similar to Whitaker (1993), and concluded that a while cognitive-behavioral, parent training, and family treatment models have been shown to be effective in reducing mildly aggressive, non-violent behavior in younger youth, multisystemic therapeutic approaches were recommended for chronically violent youth. Finally Spekman, Herman, and Vogel (1993), reporting the results of a symposium held in 1991 on increasing resiliency in youth with learning disabilities, recommended among other things, mentoring systems and other external supports. The findings of this symposium were echoed by the review conducted by Nettles, Mucherah, and Jones (2000).

Summary

There are literally dozens of current reviews of dropout prevention programs for at-risk students (c.f. Martin, Tobin, & Sugai, 2002; McPartland & Jordan, 2002). Additionally, there are many reviews of cognitive-behavioral interventions (e.g., Kendall, & Panichelli-Mindel, 1995). This review adds to the literature base of reviews in several important ways. The focus (our ecological perspective notwithstanding) includes only studies that combine the use of a cognitive-behavioral treatment and measurement of one or more dropout prevention outcomes exclusively (or in large part) for secondary aged youth with identified disabilities. Most prior

reviews have focused largely on the effects on academic outcomes, and not dropout prevention outcomes. Indeed, those reviews of cognitive-behavioral interventions that have focused on the reduction of problem behaviors have described those behaviors as an impediment to learning academic content and not as a threat to school completion. Also, most prior reviews have included both studies conducted in elementary school contexts as well as secondary schools and the results of those reviews must be generalized to that broader k-12 context.

We have also required every study included in this review to meet minimum standards of internal and external validity (see Table 1 for an example of the standards and rubric used to assess the studies that employ a between groups design; similar rubrics adapted to the unique features of one group pretest/posttest designs, qualitative designs, and single participant designs are available from the review authors). The standards and assessment rubric in Table 1 were adapted from early design work completed by meta-analysts and systematic review experts at both the Evidence for Policy and Practice Information and Co-ordinating Centre (EPPI-Centre) at the University of London, and at the What Works Clearinghouse (WWC) in the U. S. Department's Institute of Educational Sciences (see their respective websites at <http://eppi.ioe.ac.uk/EPPIWeb/home.aspx> and <http://www.whatworks.ed.gov/>). Hence, our review would be considered evidence-based. Most prior reviews are exclusively narrative reviews with no attempt to screen studies with weak designs out of the review, and no attempts to calculate effect sizes.

Description of Practice

Cognitive-behavioral therapy/interventions (CBT/I) have been described in the literature for nearly 30 years. Meichenbaum's (1977) integrated approach to cognitive and behavioral interventions is generally regarded as one of the most seminal works in building this intervention

approach to address a variety of disorders such as anxiety, depression, and aggression.

Cognitive-behavioral therapies/interventions, as their name implies, typically combine meta-cognitive skill building with classic contingency management systems. Kendall and Panichelli-Mindel (1995) describe CBT/I as follows:

“CBT focuses on how people respond to their cognitive interpretations of experiences rather than the environment or the experience itself, and how their thoughts and behaviors are related. It combines cognition change procedures with behavioral contingency management and learning experiences designed to help change distorted or deficient information processing.” (p. 108)

Although the specific therapeutic or instructional nature of the cognitive and behavioral intervention components may vary greatly in reported studies of CBI/T, there are some distinct commonalities that are present in all of these interventions. First, participants in these interventions are almost always taught, in classroom or therapy environments, a sequential strategy for recognizing one or more stimuli that have historically produced anxiety, stress, or violent responses by the participant, resisting the automaticity of the historical response, and identifying and implementing an alternative strategy that is more socially or emotionally appropriate.

Etscheidt (1991) provides a prototypical description of these strategies as a series of steps in which students are trained to engage through a variety of self-monitoring processes:

“Step 1: Motor cue/impulse delay;

Step 2: Problem definition;

Step 3: Generation of alternatives;

Step 4: Consideration of consequences; and

Step 5: Implementation” (p. 111)

Often the instruction will involve training students to actively resist impulses for a period of 10-30 seconds, engage in self-relaxation and/or self-talk activities, and cycle through a series of problem-solving processes as alternative behaviors are envisioned and differential consequences of those behaviors are considered. Frequently role-playing is used as an instructional technique to train students.

The second feature of CBT/I is some form of behavioral contingency management. In the studies included in this review, this component of the CBT/I intervention were often described with much less precision. For example, Barkley, Edwards, Laneri, Fletcher, and Metevia (2001) described this component of their CBI intervention as positive parental attention to appropriate behavior, the use of a home point system, the use of grounding or privilege loss to deal with unacceptable behavior, and training parents to anticipate impending problems. In the Etscheidt (1991) study, the behavioral contingency was described as 10 minutes of listening to audiotapes in a class period as a reinforcer for an appropriate reduction of aggressive behaviors.

In general, CBT/I interventions are taught in a series of 10-20 traditional classroom periods (or therapy sessions). They can be implemented in schools, residential treatment centers, or in group, individual, or family counseling venues. And they can be implemented by teachers, therapists, peers, and family members at home (or some combination of these individuals).

SEARCH STRATEGIES

To establish the most inclusionary literature set possible, extensive systematic searches were conducted of relevant electronic databases, hand searches of selected journals, author searches, and searches of selected reference lists, especially of review articles. Two project staff members consulted with a literature search expert from the University of London to design and conduct the electronic searches. The databases that were searched included ERIC (Ovid and Cambridge), PsycINFO (Ovid), and Medline (Ovid). All possible disability, intervention, outcome, setting, and age terms were first identified using database thesauruses.

In addition to the electronic searches described above, a list of ten representative journals was developed based on the recommendations of transition experts (a sample of the most prolific in special education transition, as well as a few representing low incidence disabilities) and a random sample (20% of 520 issues) of these journals were searched by hand by four staff members, beginning with 1990 publications and inclusive of December 2003. These searches yielded 7 articles not already retrieved in the electronic search process; these were added to our database (and only 1 resulted in an article included in a review).

Search Terms

Disability terms included: disabilities, emotionally disturbed, learning disabilities, mental retardation, attention deficit disorder, autism, Deaf, Deaf Blind, physical disability, speech language disability, multiple disabilities, orthopedic impairment, special education student(s).

Intervention terms included: teaching, learning, special education, best practices, educational programs, community services, classroom discipline, school counseling, dropout prevention, job coaching, supported employment, community based instruction, behavior

management, interagency collaboration, inclusive education, assistive technology, speech therapy, vocational rehabilitation.

Outcome terms included: academic achievement, academic anxiety, education attainment level, achievement, diploma, school graduation, school expulsion, dropout, resiliency, school suspension, school retention, truancy, persistence, employment, employment status, GED, outcomes of education, treatment outcomes, outcomes of treatment, quality of life, recreation, relationships, school to work, transition, school -to -work transition, school transition, work, jobs, employment, independent living.

Setting terms included: schools, residential care facility, accelerated programs, accelerated schools, alternative education, nontraditional education, alternative programs, alternative schools, colleges, community college, correctional institutions, high schools, middle schools, secondary education, higher education, junior high schools, mainstreaming, home school, technical school, vocational school, vocational education, vocational high school.

Sources

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¹ The search parameters and procedures described here were utilized to establish the databases for several reviews being conducted by the *What Works in Transition Systematic Review Project*, of which this review represents a single case. Therefore, some of the search terms included here may not be immediately pertinent to “dropout prevention.”

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The outcome of all literature searching processes described resulted in approximately 560 studies for which we acquired full-text reports/journal articles and that appeared promising as intervention-based studies in the area of dropout prevention. These 560 studies then were screened for propriety for our meta-analysis interests – that is, that they were intervention based, that they had a measured outcome, that the sample was youth with disabilities, and that the age/grade level of those youth was between 12 and 22 years old and in secondary school environments. The reduction in the number of studies associated with this screening process was from the original 560 studies to 135 studies.

These 135 studies were then subjected to a three-stage coding process whereby a primary coder extracted all the relevant information from those studies for this review; a secondary coder completed a semi-independent coding process similar to that used by the primary coder; and a consensus process was used to settle differences in codes assigned by the primary and secondary coders. This dual coding process resulted in removal of approximately 75% of the quantitative studies from consideration in this review. By far, the most typical reason for the removal of studies from consideration was lack of sufficient data for calculating an effect size. Other less frequently encountered reasons included inadequate specification of the intervention, inadequate

specification of the outcome measure, lack of clarity on whether the subjects sampled in the study were actually youth with disabilities, and a host of design inadequacies such as an insufficient number of participants, conditions, or settings in single-participant studies, or lack of any assurances of comparability of groups in non-randomized group design studies.

The final set of studies that made up the entire database of dropout prevention studies numbered 50 studies. At this point an inductive process was used by all staff associated with this review wherein the 40 studies were sorted into a total of four common intervention constructs – cognitive-behavioral interventions, counseling interventions, applied behavior analytic interventions, and segregated facility interventions. Sixteen studies measured interventions that conformed to cognitive-behavioral theory and are the subject for this review.

Selection Criteria

Youth with disabilities, ages 12-22 and in secondary school, must have comprised exclusively the sample in these studies, or if they were only part of the sample, there must have been separate data reported for the sub-sample of youth with disabilities such that effect sizes could be calculated for this sub-sample. The only exception to this criterion were *ex post facto* studies whose samples were older than age 22, but whose focus was retrospective estimates of the efficacy of interventions that occurred while the sample was within the 12-22 age range.

Outcomes must have related directly or indirectly to a goal of keeping students in school. Direct measures included, for example, persistence in school, persistence in grade, reduction of dropout rate, etc. Indirect measures included, for example, physical and/or verbal aggression. All studies using some form of disciplined inquiry were eligible for inclusion in this review provided they conformed to the criteria above and met minimum methodological standards for

internal and external validity. These included between groups comparison studies, one group pretest-posttest studies, single participant studies, and qualitative studies.

SEARCH RESULTS

Sixteen (16) studies were located that fit the intervention, outcome, and sampling selection criteria for this review, and whose methodological features met minimally acceptable standards of internal and external validity as determined through the consensus coding (see Table 1 for coding protocol). Four of the studies were published in 2000 or more recently; eight of the studies were published in the 1990's; and the remaining four studies were published in the 1980's. Table 2 shows selected characteristics of the participants in the studies. Table 3 presents information on the design features used in the studies, as well as detailed information on the characteristics of the intervention and the outcome(s) measured in the studies.

Participants

The sixteen studies in this review explored the effects of cognitive-behavioral interventions on a total of 791 participants. These participants were largely labeled behaviorally disordered or seriously emotionally disturbed, although three studies (Freeman & Hutchinson, 1994; Larson & Gerber, 1987; Sinclair, Christenson, & Evelo, 1998) included students labeled as learning disabled in their respective studies and several other categories of student disabilities (attention deficit hyperactivity disorder, oppositional defiance disorder, other health impaired, autism, and traumatic brain injury) were represented in individual studies. One study (Knapczyk 1988) reported research on non-categorically labeled students.

Eleven of the 16 studies reported precise estimates of the average ages of the participants in the research. These estimates ranged from an average age of 13.3 years to 17 years ($M = 15.1$ [unweighted]). The remaining five studies reported age ranges for their participants, and these ranges spanned the 12 to 19 year age range. Most studies sampled participants who were either exclusively male or mostly male; two studies (Bodtker, 2001; Larson & Gerber, 1987) did not

report gender percentages; and one study (LeCroy, 1988) involved participants who were exclusively female.

Finally, the majority of studies that reported sample attrition rates (57%) lost no participants in the research process. Three studies did not report attrition rates (Bodtker, 2001; Etscheidt, 1991; LeCroy, 1988). Of the studies that did report a measurable attrition rate, these rates ranged from an 8% rate in the Dangel, Deschner, and Rasp (1989) study to a 38% rate in one of the treatment groups in the Barkley et al. (2001) study.

Research Designs

Fifteen of the 16 studies in this review were classified as quantitative in design; only the Freeman and Hutchinson (1994) used a qualitative (case study) design. Of the 15 quantitative designs, only two were single group pretest-posttest designs, five used a variety of single-participant designs, and eight were classified as between group designs (studies utilizing one or more between groups factors and within subjects factors are designated simply as between groups designs here for the sake of simplicity). All eight of these between groups studies made use of either randomized, quasi-randomized (Barkley et al. 2001), or cluster randomized (Etscheidt, 1991; Robinson, Smith, & Miller, 2002) assignment processes.

Between groups designs. Five of the between groups studies used a classic pretest-posttest control group design, although the Barkley et al. (2001) study used multiple posttests. The remaining three between groups studies were all posttest only control group designs with only the Clark, Hawkins, Sheeber, Lewinsohn, & Seeley (1995) study making use of more than one posttest measurement. Four of the eight studies utilized a “no treatment” or “treatment as usual” control group; the remaining four studies used alternative treatment groups. Two studies (Etscheidt, 1991; Larson & Gerber, 1987) used both alternative treatment groups and “treatment

as usual” control groups. In these two cases, the effect sizes calculated for this review involved the primary treatment group compared with the control group.

Single-participant designs. Of the five single-participant design studies, all were multiple baseline studies. Three of the studies incorporated multiple baseline across settings designs and two were across participant designs.

Single group pretest-posttest designs. Two studies reported on the effects training youth with aggressive behaviors on anger control/conflict education curricula (Bodtker, 2001; Dangel et al, 1989). In both cases there were substantive deviations from a traditional single group pretest-posttest design. In the Bodtker (2001) study, two cohorts of students were pretested at the beginning of a semester and posttested at the end of the semester, but in different semesters. The data were then combined for the analyses in the study. In the Dangel et al, (1989) study, the data were actually reported as a multiple baseline study, but with groups of four and eight subjects in each group. The raw data were reported for baseline, treatment, and follow-up allowing for the calculation of a change score from baseline to first posttest.

Outcomes

Most of the studies measured multiple outcomes, although many of these multiple outcomes in these 16 studies were either not amenable to effect size calculation, were not germane to dropout prevention, or were process-oriented outcome variables (e.g. capacity of the participants to implement the intervention). None of the 16 studies measured more than one outcome construct that was both relevant to dropout prevention and provided sufficient data to allow for effect size calculation. The single exception to this statement was the Sinclair et al. (1998) study in which “enrollment status” was used as an outcome variable as was “relevance of school” and “expectation to graduate”. One might make the case that these latter two perception

variables represented a demonstrably different dropout construct than the more behaviorally-oriented “enrollment status” (dropped out or not dropped out). Nonetheless, we included only the effect size associated with the “enrollment status” dependent measure in this review.

Only three studies measured retention or dropping out. The Barkley et al. (2001) measured “retention in treatment” as the outcome variable of interest for this review, and the Sinclair et al. (1998) measured “enrollment status”. The Freeman and Hutchinson (1994) study also measured a student’s perspectives on dropping out or staying in school while the student was participating in a discrete intervention program. The remaining 13 studies in this review all measured either physical or verbal aggression, correlates of dropout (as discussed).

Characteristics of Cognitive-Behavioral Interventions

Although the features of cognitive-behavioral interventions found in these 16 studies varied extensively, there were many common characteristics. Almost all of the studies discussed a specific curriculum that was delivered through a defined period of time within a school or residential treatment setting. For example, the Barkley et al. (2001) study implemented a *problem-solving communication training curriculum* which had been described as part of an earlier study described in Barkley, Guevremont, Anastopoulos, and Fletcher (1992). The Clarke et al. (1995) curriculum was an adaptation of a curriculum titled “*Adolescent coping with depression*” and had been published earlier (Clarke, Lewinsohn, & Hops 1990). The Coleman et al. (1992) study used a curriculum developed prior by Goldstein and Glick (1987). Finally, the Etscheidt (1991) study used an adaptation of a curriculum published by Lochman, Nelson, and Sims (1981). Perhaps the studies in which the best descriptions of the curricula used was embedded directly in the study narrative were the Presley and Hughes (2000) study (which was

adapted from a curriculum published by Walker, Todis, Holmes, & Horton, 1988) and the Sinclair et al. (1998) curriculum titled *Check and Connect*.

In many cases, the study authors characterized the curricula used within the broader genre of social skills competency development. These social skills often included such concepts as appropriate communication techniques, meeting people, and responding to potentially provoking stimuli. What characterized these studies as cognitive-behavioral studies, however, were the meta-cognitive skills the participants in these studies learned. In almost every study, for example, participants were taught problem-solving skills, conflict management skills, conflict resolution/negotiation skills, self-talk and relaxation skills, and emotional and anger self-awareness techniques. While these are not social skills per se, these skills promote the social skills mentioned earlier.

In those studies in which a defined curriculum was implemented, the typical duration was in the 10 – 15 week genre, with the normative intensity of implementation following a traditional 50-minute classroom period one to five times per week. However, substantial deviations from these normative instructional practices were reported and studies. For example, Dangel et al. (1989) implemented their intervention for only six weeks for about one hour per week, and the intervention reported in LeCroy (1988) lasted for only three weeks, although there were two sessions each week lasting 90 minutes.

Setting

The majority of the studies (eight) were conducted in public secondary schools, although most of these settings were in segregated (or self-contained) classes for students with disabilities. Three studies took place in residential treatment centers, and one study each involved a private

school, a segregated public school, and a correctional facility. One study also did not state the setting.

Treatment Fidelity

Several of the studies assessed fidelity or integrity of treatment implementation. Perhaps the best description of how this was accomplished was given by Robinson et al. (2002), in which 20% of treatment sessions were observed by two trained observers who used a checklist to confirm treatment fidelity. Additional debriefings were conducted with teachers implementing the treatment after all sessions that were not directly observed.

SYNTHESIS FINDINGS

Table 4 presents the meta-analytic results for the eight between groups quantitative studies in this review. One effect size (Hedges g) and accompanying weight (inverse variance method) were computed for each study. A weighted mean effect size was computed for the eight between groups studies, along with a z statistic to test for statistical significance and a Q statistic to evaluate for homogeneity of effect size estimates.

As shown in Table 4, the mean effect size for these studies was 0.55 and was statistically significant ($z = 5.6; p < .001$). This mean effect size would be considered a moderately large effect size according to Cohen's (1988) rubric. The Q -statistic for this group of eight studies, estimating the homogeneity of effect size estimates was not statistically significant ($Q = 9.17; df = 7; p = .24$) suggesting homogeneity of effect size estimates. Interestingly, every one of these between groups studies involved randomized or quasi- (or cluster) randomized participant assignment procedures. It is worth noting in Table 4 that three of the eight studies had confidence intervals associated with their effect sizes that included zero, an indication that those particular effect size estimates were not statistically significant in those three studies. The confidence interval associated with the average effect size, however, had a lower bound that was well above zero (.36) and a range that equaled the smallest range from among all of the confidence intervals in all eight studies.

Effect sizes were calculated for the single-participant and one group pretest-posttest design studies as well. With the single-participant designs, the effect sizes ranged quite broadly from 1.65 to 12.08. The average effect size was estimated to be 11.16 for these four studies. However the Q -statistic estimating homogeneity of effect size estimates was significant, reducing the confidence in the generalizability of this average effect size estimate. For example,

eliminating just the Presley and Hughes (2000) study reduced the average effect size to 2.79. However, in three of the four effect sizes, the lower limit of the confidence intervals associated with that effect size was negative, yet the average effect size was considered statistically significant. This anomaly undoubtedly has more to do with the instability of the effect size estimates (state of the field in effect size measurement for single participant designs) than as a valid representation of the effects of cognitive-behavioral interventions in single-participant study contexts.

Similarly, the fact that there were only two single group pretest-posttest design studies, as well as the relative weakness of these studies from an internal validity perspective severely limits the generalizability of the of the average effect size calculated from these two studies. It was positive, however, and was estimated to be 0.25.

Sensitivity Analyses

The single-participant and one group pretest-posttest design studies do not lend themselves to sensitivity analyses due to the extreme instability of the effect size estimates in the single-participant studies, and the limited number of one-group pretest-posttest design studies. With respect to participant characteristics the between groups studies were quite homogeneous on most characteristics such as gender, disability, and age range, again limiting the capacity for subgroup analyses. There was the opportunity to conduct a sensitivity analysis on the subgroup of three studies that employed quasi-randomized designs. These three studies employing quasi-randomization were the Barkley et al, (2001) study in which used an alternating assignment process described as follows:

The first group of 12-14 sequentially referred families meeting eligibility criteria were enrolled in PSCT alone. As this treatment phase neared completion, the next

wave of sequentially eligible families was then assigned to combined PSCT/BMP and so on in alternating waves. This method of assigning families to treatment was thus quasi-random (p. 927).

The other two quasi-randomized studies (Etscheidt, 1991; Robinson et al, 2002) used cluster randomization of intact classes to treatment conditions.

A sensitivity analysis conducted by analyzing the effect sizes of these three quasi-randomized studies and comparing the results of this analysis with an effect size analysis of the five randomized studies, yielded virtually the same mean effect size (0.53 and 0.58 respectively). The confidence intervals around the effect size estimates across these two analyses were very similar as well.

Rival Explanations

Although it is certainly possible that a number of rival explanations, dispersed across the 15 studies, could account for the effects found in this review it seems unlikely. First, the consistency of the results across all three design types (despite the instability of the effect size estimates in the single participant studies) reduces the probability of systematic or random error accounting for these findings. Second, and looking just at the strongest of the studies in this review – the between groups studies – the quality of the randomization in the designs and consistency of the magnitude, direction, and confidence intervals for those effect sizes tend to make rival explanations less defensible. Every one of these eight studies used some form of randomization – the single most important design characteristics that helps to rule out rival explanations.

Nonetheless, we feel compelled to point out several potential sources of bias that may make the results of this review less “bullet-proof” so-to-speak. The first is publication bias. All

of our studies were published in refereed journals. It is entirely conceivable that an unknown body of unpublished “file drawer” research with results counter to ours exists but was not published and hence was not accessible to us in our review. This is not a trivial issue in the conduct of systematic reviews like this one and must be considered as a potential source of downward pressure on effect size estimates that are found to be high and positive.

Second, despite the fact that we are confident in the technical validity with which we included (and excluded) single-participant studies on research design standards of internal and external validity, we are not comfortable with the interpretability of the effect size estimates derived from those studies that we did include. We recognize, too, that other meta-analysts in this fledgling area of research synthesis with single-participant studies may have used equally defensible alternative design standards for inclusion of studies, as well as other metrics for calculating effect sizes from those studies, and a different set of effect size estimates may have emerged.

Finally, from among the eight studies that used between groups designs, two (Etscheidt, 1991; Robinson et al, 2001) used cluster randomized assignment of classrooms to treatments. The appropriate statistical analysis for these types of designs is at the unit of assignment level (classrooms, rather than students). Nonetheless, both these studies reported analyses at the student level, diminishing somewhat the confidence that can be ascribed to the internal validity controls associated with the randomized nature of these designs.

CONCLUSIONS

There have been a number of anecdotal studies (c.f. Gruenhagen, 1993; Kortering, Braziel, & Tompkins, 2002; Seidel & Vaughn, 1991) youth with learning disabilities and behavioral disorders who have dropped out of school. These studies all have focused on exploring the perceptions of these students as to why they dropped out and what there might have been put in place in schools to help them resist the temptation to drop out. While many of the factors cited by these students are unalterable, one common theme across these studies is the feelings by these students of social alienation, and an underlying source of some, if not all, of this social alienation is a lack of competence in a broad range of social skills by these students, as well as situational knowledge of how to use the social skills they do have in their repertoire under stressful or challenging social pressures. The curricula most often used in the studies included in this review are social skills curricula. And the instructional techniques and situational contexts in which these curricula are introduced and reinforced in therapeutic and classroom activities are precisely these stressful and challenging scenarios – a strategy for youth called “stress inoculation” coined by Novaco (1978) nearly 30 years ago.

In the July/August (1997) issue of *Teaching Exceptional Children*, Forness, Kavale, Blum, and Lloyd published an article summarizing the results of 18 meta-analyses in special education. Among the interventions they found to be “convincing” (effect sizes larger than .66) were a meta-analysis of mnemonics (a meta-cognitive strategy) (Mastropieri & Scruggs, 1989) and of cognitive-behavior modification (Robinson, Smith, Miller, & Brwonell, 1999). Our results are remarkably similar to those found by Robinson et al, 1999, at least for those studies in our review that used a between groups design.

Hence, our conclusion, based on the studies in this review, is that cognitive-behavioral interventions work well to reduce dropout and physical and verbal aggressive behavior in youth with disabilities. We do not have convincing or adequate evidence within these studies to state with certainty what forms of cognitive interventions work best and in combination with what forms of contingency management strategies. However, given the weight of the prior positive evidence of cognitive strategies utilizing self-management/self control procedures (i.e. Harchick, Sherman, & Sheldon, 1992; Hughes & Agran, 1993; Lancioni & O'Reilly, 2001; Martin & Hrydowy, 1989), and given the consistency of intervention strategies in our group of studies, we are confident in the implications for practice directly below.

IMPLICATIONS FOR PRACTICE

Do Cognitive-Behavioral Interventions (CBI's) Work?

Yes, cognitive-behavioral interventions work if schools or residential treatment centers want to reduce dropout and the behaviors that tend to lead to dropout (violent verbal and physical aggression). We consider this to be an evidence-based judgment based on the high quality of studies in our review, and the consistency of findings within and across study designs, and across both types of outcomes measured in these studies.

How Well Do CBI's Work?

The most interpretable estimate we have is the average effect size for the studies using a between groups design, which was 0.55. This effect size is interpreted as evidence of a moderately powerful effect on the outcomes measured. Average effect sizes of this size, particularly when derived from well-designed studies and when the individual effect sizes are consistently found across those studies (see the fairly narrow confidence interval [.36-.74] surrounding this average effect size in Table 4, must be considered by teachers and education administrators as a green light to go forward and expect similar results when implemented in their schools in a manner similar as was implemented in these studies.

How Difficult is it for Youth to Learn and Use CBI's?

Our evidence on this question is very limited. In our group of studies, only the Presley and Hughes (2000) study directly measured the steps in the self-control curriculum that students learned, and our interpretation of the effect size associated with this study is confounded by the fact that the curriculum was implemented by student peers and not teachers. The effect size was very large (12.08) – much larger even than the average of the rest of the single-participant studies – suggesting ease of learning by the students in that study. Also supporting this judgment

in an indirect manner is the fact that none of the study authors reported difficulties in implementing the curricula in their respective studies.

Do CBI's Work Equally Well for Different Kinds of Youth and Settings

Our answer here is yes, they do, although the variability in types of youth with disabilities was limited largely to youth with moderate and severe behavior disorders, and who were largely male. Interestingly, one of the well-designed group studies (LeCroy, 1988) implemented a cognitive-behavioral intervention with just females and the results were among the most powerful from among our group of studies. CBI's appear equally effective with younger and older adolescents as well, and in schools as well as in residential and day treatment centers or private specialized schools.

What Happens if you Implement only the Cognitive Part of CBI's and Leave out the Behavioral (Contingency Management) Part?

We had two studies that addressed this question directly – the Barkley et al, (2001) study and the Etscheidt (1991) study. Barkley and his colleagues found a statistically significant difference in efficacy between their cognitive only intervention group and their cognitive plus behavioral group in favor of the cognitive plus behavioral group. Etscheidt (1991) found no significant differences between these two intervention groups, although she too noted that the cognitive plus behavioral treatment group evidenced greater effects (although not statistically significant) than the cognitive only group. It appears that eliminating the behavioral component reduces the efficacy of the CBI intervention.

How Long Should You Implement CBI's?

Our evidence here is not direct. Nonetheless, many study authors recommended longer duration interventions rather than those of shorter duration.

What are the Most Common Elements of the Cognitive and Behavioral Components in These Studies?

The most common curricular and instructional components of the CBI's that were implemented in these studies were as follows (Table 3 describes the intervention components in more detail).

Cognitive components. Training included curriculum in problem-solving, self-instruction, communication skills, relaxation, and situational self-awareness. Instructional techniques included mentoring, teacher and peer modeling, role-playing and behavioral rehearsal (often with cartoon characters).

Behavioral components. In addition to customary reinforcers of praise and recognition, the most common behavioral components of CBI's were token economy point systems and behavioral contracting.

Where can You Find Published Descriptions of These CBI Interventions?

While all of the studies included in this review have, by our inclusionary criteria, reasonably complete descriptions of the interventions used, these descriptions are typically not as complete as they might be, most often due to space limitations in the journals in which the studies were published. However, several of the studies cited other work where interested individuals might find much more complete descriptions of precisely how to implement these CBI's. We have not acquired all of these more detailed descriptions to affirm their comprehensiveness, but offer these additional citations for potential use by the reader, recognizing that many of these sources may be in fugitive literature as unpublished curricula and may be difficult to acquire.

In the Barkley et al, (2001) study, the authors cite: Barkley, Guevremont, Anastopoulos, & Fletcher (1992) and Robin and Foster (1989). In the Clarke et al, (1995) study, the authors cite: Clarke, Lewinsohn, and Hops, (1990). In the Etscheidt (1991) study, the author cited Lochman et al, ((1981).

Where are the Best Descriptions of CBI's as they were Implemented in These Studies?

We recommend two studies – the Sinclair et al, (1998) study for a thorough description of a school-based dropout prevention CBI – and the Barkley et al, (2001) for a more counseling/therapy oriented CBI. Both studies were well-designed, had reasonably large sample sizes, and used high-quality randomization techniques. Both can be found in the companion piece to this review titled “Bottomlines”.

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TABLE 1

Design Standards and Evaluative Rubric for Between Groups Studies

Design Standard	Evidence for a Yes Rating	Evidence for a Maybe Yes Rating	Evidence for a Maybe No Rating	Evidence for a No Rating
A. How would you rate the alignment of the intervention to commonly-held ideas of the intervention or approach?	The intervention or approach fully reflected commonly-held or theoretically derived ideas about what the intervention or approach should be.	At a minimum the intervention or approach at least somewhat reflected commonly-held or theoretically derived ideas about what the intervention or approach should be.	The intervention or approach was described only as member of broader classes (across which significant variation in content can be expected).	The intervention or approach did not reflect commonly-held or theoretically derived ideas about what it should be.
B. How would you rate the implementation and replicability of the intervention or approach?	The intervention or approach was sufficiently described at a level which would allow relatively easy and thorough replication by other implementers, and the description of the implementation of intervention was fully consistent with its defined characteristics.	The intervention or approach adequately described to allow replication of the most essential elements by other implementers, and the description of the implementation was largely consistent with it's defined characteristics.	The authors of the study omit important descriptive information concerning the essential elements of the intervention such that its replication would be impossible, OR it is plausible that the implementation of the intervention may well have been inconsistent with it's defined characteristics.	The authors of the study omit important descriptive information concerning the essential elements of the intervention such that its replication would be impossible, AND it is plausible that the implementation of the intervention may well have been inconsistent with it's defined characteristics.
C. How would you rate the adequacy with which the outcome measure was defined?	The study provided adequate evidence that the outcome measure was properly defined and appropriate for the context of the study.	Although the study did not present adequate evidence that the outcome measure was properly defined but the measure did appear to be appropriate to the content of the outcome and the context of the study.	The outcome and/or the measure used to assess the outcome were only described conceptually as a member of a broader class of outcomes/measures about which significant variation exists as to their specific content.	It is unclear what the outcome is and how it was measured.
D. How would you rate the adequacy with which participants in the comparison or	Participants were randomly assigned to conditions, and there does not appear to have been	Either randomized assignment was used but there appears to have been serious	Randomized assignment was not used and despite some steps taken to make the groups	It is unlikely or unknown that/if the participants in the groups are comparable

Design Standard	Evidence for a Yes Rating	Evidence for a Maybe Yes Rating	Evidence for a Maybe No Rating	Evidence for a No Rating
alternative treatment group(s) were made comparable to those in the treatment group?	any serious differential attrition within groups or severe attrition across groups.	differential attrition within groups or serious overall attrition across groups, or although random assignment of participants to groups was not used, there does not appear to have been serious attrition problems within or across groups and reasonable attempts were made to make the groups comparable (i.e. matched sampling, use of a covariate, etc.).	comparable, they do not appear to have been adequate.	
E. How would you rate the adequacy with which the study controlled events that happened concurrently with the intervention or approach that might have confused its effect(s)?	Concurrent processes and events that might be alternative explanations to a treatment effect have been ruled out, either explicitly or implicitly.	There were no identified processes or events that could be alternative explanations for a treatment effect, but some alternative explanations cannot be explicitly ruled out either because there was some evidence that alternative explanations might exist, or because no attention was given to ruling out an alternative explanation and it is reasonable to expect that one or more alternative explanations might exist.	There was no “Maybe No” rating in this standard.	Identifiable processes or events that are described to be occurring simultaneously with the treatment or approach may have caused the observed effect
F. How would you rate the adequacy with which the actual sample, setting, outcome(s), and measurement processes reflected	The actual sample generalizes well to the theoretical population and the setting, outcome(s) and measurement processes generalize	Most aspects of the theoretical population and common variations of settings, classes of outcomes, and data collection	Although some important characteristics of the theoretical population and typical settings, outcomes, and data collection processes	The actual sample does not adequately reflect any characteristics of the theoretical population, and the setting, outcomes,

Design Standard	Evidence for a Yes Rating	Evidence for a Maybe Yes Rating	Evidence for a Maybe No Rating	Evidence for a No Rating
the theoretical population and typical norms for settings, outcomes, and measurement processes?	well to common variations in settings, classes of outcome(s), and processes and timing of data collection.	processes and timing are represented in the study.	and timing are represented by the study, many important characteristics are not.	and data collection timing and processes have characteristics that are not within the boundaries of accepted and typical practice.
G. How broadly was the intervention tested statistically across important sub-groups of students, and across substantive variations within the intervention as a whole?	The analyses in the study examined the effect(s) of the intervention across important sub-groups of students AND included separate analyses of key sub-components of the intervention for differential effectiveness on those different sub-groups of students.	Some sub-group analyses were conducted and some estimates were made exploring differential effects of different intervention components.	Some sub-group analyses were conducted or some estimates were made exploring differential effects of different intervention components. However, significant sub-groups were omitted from the analyses, and no separate effects of different intervention components by sub-groups were explored.	Only main effects for the intervention as a whole were reported with no sub-group or intervention component analyses.
H. How thoroughly were the assumptions underlying the statistical analyses for the study reported?	It is clear from the design that the assumption of independence across groups and observations was not violated, and some evidence is provided that other important assumptions underlying the statistics for the study (i.e. homogeneity of variance) were not violated.	It is clear that the assumption of independence across groups and observations was not violated, but other information about assumptions underlying the statistics for the study are not provided.	It appears that the assumption of independence across groups or observations was likely to be met and other information about assumptions underlying the statistics for the study are not provided.	It does not appear from the study's design that the assumption of independence across groups or observations was met.
I. How adequately were the data described, analyzed, and depicted such that effect size for the outcome in this extraction is able to be calculated?	Either the effect size was reported by the authors or they provided data to allow precise calculation of effect sizes.	Sufficient statistical information was reported to allow, at a minimum, an imprecise effect size to be calculated for the outcome of this extraction.	There was no "Maybe No" rating in this standard,	Neither sample sizes nor effect sizes were reported, and insufficient data were provided to allow those effect sizes to be calculated.

TABLE 2

Sample and Participant Characteristics of 16 Studies

Study	Sample Size	Attrition Rate	Participant Characteristics		
			Handicapping Conditions	Average Age or Grade Level	Percent Male
Barkley, Edwards, Laneri, Fletcher, & Metevia (2001)	97 families	PSCT – 38% PSCT plus BMT – 18%	ADHD ODD	14.7 years	89%
Bodtker (2001)	307	*	ED OHI Autistic	16 years +/-	*
Clarke, Hawkins, Murphy, Sheeber, Lewinsohn, & Seeley, (1995)	125	26.7%	ED/BD	15.3 years	30%
Coleman, Pfeiffer, & Oaklane, (1992)	39	25%	BD	15.9 years	74%
Dangel, Deschner, & Rasp (1989)	11	8%	ED	14.1 years	70% estimated
Etscheidt (1991)	30	*	BD	*	80%
Feeney & Ylvisaker (1995)	3	0%	TBI	18.3 years	100%
Freeman & Hutchinson (1994)	1	0%	LD	17 years	100%
Knappczyk (1988)	2	0%	Non-categorical	13.5 years	100%
Knappczyk (1992)	4	0%	BD	15.5 years	100%
Larson & Gerber (1987)	20	LD – 13% Non-LD – 8%	LD	16-19 years	*
LeCroy (1988)	11	*	ED/BD	12-17 years	0%
Ninness & Fuerst	2	0%	ED	13.5 years	100%

Study	Sample Size	Attrition Rate	Participant Characteristics		
			Handicapping Conditions	Average Age or Grade Level	Percent Male
(1995) Presley & Hughes (2000)	4	0%	BD	15 years	75%
Robinson, Smith, and Miller (2002)	41	0%	BD/ED	13-14 years	100%
Sinclair, Christenson, and Evelo (1998)	94	32%	ED/BD/LD	13.3 years	68%
Smith (1992)	3	0%	ED/BD	15 years	67%

TABLE 3

Design Features, Intervention Characteristics, and Outcomes Measured for All Studies

Study	Research Design Elements	Intervention Components	Outcome(s)
Barkley, Edwards, Laneri, Fletcher, & Metevia (2001)	Pretest – posttest alternative treatment design with quasi-random assignment of families to PSCT alone treatment or PSCT plus BMT treatment	X ₁ : Problem-solving communication training (PSCT) alone, X ₂ : Problem-solving communication training (PSCT) plus behavior management training (BMT)	Retention in treatment
Bodtker (2001)	One-group pretest-posttest design	Conflict education	Student aggressive behavior
Clarke, Hawkins, Murphy, Sheeber, Lewinsohn, & Seeley, (1995)	Posttest only control group design using randomized assignment of students to either the treatment or “usual care” control group	Cognitive-behavioral group intervention titled “Coping with Stress Course”	Episodes of depressive behavior
Coleman, Pfeiffer, & Oaklane, (1992)	Pretest – posttest control group design using randomized assignment of students to treatment and “no treatment” control groups	Aggression replacement training	Aggressive behavior
Dangel, Deschner, & Rasp (1989)	Single group pretest/posttest Design	The treatment involved groups sessions with cognitive preparation (thinking about anger provoking situations, thinking about non-aggressive ways to deal with the situations, and introduction to the concepts of anger management, coping strategies, and verbal self-instruction	Verbal and physical aggression
Etscheidt (1991)	Pretest – posttest alternative treatment group design with cluster randomized assignment of intact classes to two treatment groups and a no treatment control group	The cognitive only treatment group was taught a five-step self-cueing procedure. The cognitive plus behavior treatment included training on the cognitive steps above plus a behavioral contract with contingent positive reinforcement	Teacher observation of frequency of aggressive behaviors

Study	Research Design Elements	Intervention Components	Outcome(s)
Freeman & Hutchinson (1994)	Qualitative case study design	The intervention was drawn from the first draft of module five in the Pathways program.	Personal perspectives on dropping out of school or staying in school
Knapczyk (1988)	Single-participant design – multiple baseline across settings design	The experimental treatment involved application of modeling and rehearsal procedures to the training of social skills.	Aggressive behaviors
Knapczyk (1992)	Single-participant design – multiple baseline across settings design	Treatment used modeling and behavioral rehearsal to develop alternative responses to aggression.	Frequency of aggressive behaviors
Larson & Gerber (1987)	Posttest only alternative treatment group design with randomized assignment of students to two treatment groups and a no treatment control group	Training for the treatment group included three lessons in verbal self-instruction, nine lessons in social metacognitive awareness, and ten lessons in social metacognitive control skills. taught delinquents how to think.	Behavioral variables of incident reports and progress towards goals of positive behavior
LeCroy (1988)	Posttest only alternative treatment group design with randomized assignment of students to two treatment groups	The anger management group was presented in 2 phases during each of the sessions. The first phase was educational and provided a conceptual framework for the participants. The next phase included behavioral rehearsals using cognitive and behavioral techniques of social skills training and practice in role plays with other group members.	Tendencies for aggressive action
Ninness & Fuerst (1995)	Single-participant design – multiple baseline across settings	Following explanations and demonstrations of appropriate in-class social skills and self-management behaviors, students role played under simulated conditions	Off task and disruptive behavior (combined into one outcome measure)
Presley & Hughes (2000)	Single-participant design – multiple baseline across subjects design	Using non-disabled peers to deliver the intervention, participants were instructed by their peer trainers to verbally express anger appropriately using the 11 steps of the Triple A Strategy: ASSESS, AMEND, and ACT.	Observed use of the intervention “steps”

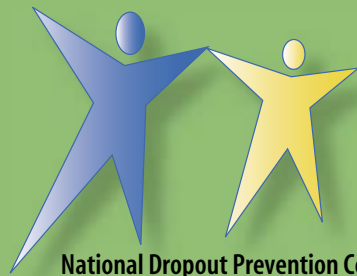
Study	Research Design Elements	Intervention Components	Outcome(s)
Robinson, Smith, and Miller (2002)	Pretest – posttest control group design with cluster randomized assignment of intact classes to groups	Cognitive-behavior intervention in anger control techniques that included the following elements in the ACC: 1. Lessons on understanding and handling anger. 2. Lessons about effective communication. 3. Introduction to relaxation techniques. 4. Presentation of problem-solving skills e.g., problem identification, choosing among alternative solutions, evaluating the outcome). 5. Modeling of the intervention steps. 6. Practice (e.g., role-playing the use of the overt and covert self-statements) paired with performance feedback from the teachers and students.	Child Behavior Checklist ratings of social, anxiety, self control, and aggressive behavior
Sinclair, Christenson, and Evelo (1998)	Pretest – posttest control group design using randomized assignment of students to treatment and “no treatment” control groups	Check and Connect intervention curriculum model.	Enrollment status
Smith (1992)	Single-participant design – multiple baseline across subjects design	The strategy included the mnemonic ZIPPER which stands for zip your mouth, investigate the problem, put off what you want to do, put yourself in charge, explore other solutions, return to what you are doing.	Aggressive behavior

TABLE 4

Meta-Analytic Results of Between Groups Studies

Study	Experimental Group			Alternative Group			Effect Size <i>d</i>	Confidence Interval	
	<i>n</i>	<i>M</i>	<i>SD</i>	<i>n</i>	<i>M</i>	<i>SD</i>		Lower	Upper
Barkley et al, (2001)	39	N/A	N/A	58	N/A	N/A	.45	.36	.74
Clarke et al, (1995)	55	N/A	N/A	70	N/A	N/A	.37	0.02	.73
Coleman et al, (1992)	24	5.93	8.20	15	1.00	8.20	.59	-.07	1.25
Etscheidt (1991)	10	37.00	29.11	10	-16.33	25.25	1.87	.78	2.97
Larson et al, (1987)	10	N/A	N/A	10	N/A	N/A	1.08	.13	2.03
LeCroy (1988)	6	N/A	N/A	5	N/A	N/A	1.25	-.10	2.61
Robinson et al, (2002)	19	4.1	14.79	22	-2.05	12.01	.45	-.17	1.07
Sinclair et al, (1998)	47	N/A	N/A	47	N/A	N/A	.56	.15	.97
Mean Effect Size							.55	.36	.74

Note: A mean and standard deviation of “N/A” indicates a study in which the effect size was calculated through an odds ratio or a non-parametric statistic and subsequently converted to Hedges *g*.



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