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A meta-analysis of behavioral treatments for attention-deficit/hyperactivity disorder

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ABSTRACT

There is currently controversy regarding the need for and the effectiveness of behavior modification for children with attention-deficit hyperactivity disorder (ADHD) despite years of study and multiple investigations reporting beneficial effects of the intervention. A meta-analysis was conducted by identifying relevant behavioral treatment studies in the literature. One-hundred seventy-four studies of behavioral treatment were identified from 114 individual papers that were appropriate for the meta-analysis. Effect sizes varied by study design but not generally by other study characteristics, such as the demographic variables of the participants in the studies. Overall unweighted effect sizes in between group studies (.83), pre-post studies (.70), within group studies (2.64), and single subject studies (3.78) indicated that behavioral treatments are highly effective. Based on these results, there is strong and consistent evidence that behavioral treatments are effective for treating ADHD.

Contents

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1. Introduction

Attention-deficit/hyperactivity disorder (ADHD) is a prevalent and chronic mental health disorder associated with adverse outcomes through the life span. These adverse outcomes include severe disruptions in relationships with parents, teachers, peers and siblings during childhood, academic problems throughout the school years, and delinquency and substance abuse in adolescence and adulthood (Barkley, 2006). With a prevalence rate of 2% to 9% in the U.S. and world-wide (Froehlich, Lanphear, Epstein, Barbaresi, Katusic, & Kahn, 2007), it is one of the most common problems encountered in mental health, primary care, and educational settings. Due to its associated impairments, adverse outcomes, and prevalence, ADHD is a costly problem for society. Its estimated annual cost in the U.S. is more than 50 billion dollars, approximating the societal cost of major depression and stroke (Pelham, Foster, & Robb, 2007), making it a major public health concern.

Since the early 1990s, emphasis has been placed on identifying evidence-based treatments for psychological disorders, including ADHD (Chambless & Ollendick, 2001; Lonigan, Elbert, & Johnson, 1998; Weisz, Jensen Doss & Hawley, 2006). As part of this movement, behavior modification has been identified as an evidence-based treatment for ADHD (DuPaul & Eckert, 1997; Pelham & Fabiano, 2008; Pelham, Wheeler, & Chronis, 1998). However, no current and comprehensive review of the magnitude of behavioral treatment effect size for children and adolescents with ADHD exists.

Behavior modification (i.e., clinical behavior therapy, contingency management) is grounded in learning theory and includes principles of classical conditioning, operant conditioning, cognitive-behavioral theory, and social learning theory. Many approaches focus on operant procedures wherein the antecedents (e.g., commands) and consequences (e.g., time out) of child behaviors are manipulated to increase the desired behavior (e.g., compliance) and decrease undesirable behavior (e.g., noncompliance). These principles have been successfully employed to treat childhood externalizing problems for more than 40 years (e.g., O'Leary, Becker, Evans, & Saudargas, 1969; Patterson, 1974). Typical behavior modification procedures involve working with parents and teachers to program behavioral contingencies into the child's home, school, and recreational environments. Beginning in the 1970s, behavior modification procedures were successfully employed for children described as “hyperactive” (e.g., O'Leary, Pelham, Rosenbaum & Price, 1976; O'Leary & Pelham, 1978; Pelham, 1977), and presently to children described as ADHD (APA, 1994).

In the past decade, a number of systematic reviews have attempted to synthesize the behavioral treatment literature for ADHD. For example, in a review of treatment for disruptive classroom behavior, Stage and Quiroz (1997) reported a mean effect size of .78 for studies investigating the use of behavioral interventions for ADHD in the classroom. This synthesis is limited, however, in that only five studies were included in its calculation, far fewer than are available in the literature. Furthermore, the studies included were heterogeneous in terms of their subject composition and designs, limiting interpretability.

DuPaul and Eckert (1997) also focused on the behavioral treatment of ADHD in classroom settings. In their review, they computed separate effect sizes for single-subject, within-subject and between-group design studies. Mean behavioral treatment effect sizes of between-group (.45), within-subject (.64), and single-subject (1.16) designs indicated that behavioral interventions for ADHD in the classroom were effective. However, this research synthesis did not include treatments employed in the home by parents or those used in recreational settings with peers. In addition, many ADHD treatment studies have been published since this meta-analysis was conducted (see Pelham & Fabiano, 2008), making an updated research synthesis necessary. In another meta-analysis of group design studies of behavioral interventions for ADHD, Van der Oord, Prins, Oosterlaan, and Emmelkamp (2008) reported pre-post effect sizes ranging from .19 (academic outcomes) to .87 (parent ADHD ratings) with a median effect size of .66. However, this research synthesis did not include the range of study designs used to assess treatment outcome in the literature (e.g., single subject studies).

General reviews on behavioral parent training (BPT) for externalizing behavior problems support the use of BPT for children described as ADHD, oppositional, antisocial, and/or disruptive (e.g., Brestan & Eyberg, 1998; Eyberg, Nelson, & Boggs, 2008; Lundahl, Risser, & Lovejoy, 2006; Serketich & Dumas, 1996). Meta-analyses also yield positive effects for BPT. Corcoran and Dattalo (2006) reported effect sizes of .40 and .36 for ADHD and externalizing symptoms, respectively, in their meta-analysis of between-group studies of parent-involved treatments for ADHD. Purdie, Hattie, and Carroll (2002) reported an effect size of .31 for BPT for ADHD. Lundahl et al. (2006) reviewed between-group BPT studies for children described as disruptive and reported effect sizes ranging from .42–.53 for child and parent outcomes following intervention. Serketich and Dumas (1996) included only group design studies and reported an overall effect size of .86 for BPT interventions. Thus, behavioral treatments evaluated in group design studies result in moderate to substantial improvement for children with a variety of disruptive behavior problems.

Complementing DuPaul and Eckert's and Van der Oord et al.'s meta-analyses and the BPT reviews, Pelham, Wheeler, and Chronis (1998) qualitatively reviewed the entire behavioral treatment literature on ADHD, and included studies that occurred in the home or in school or both. BPT and classroom contingency management met criteria for empirically supported treatments. This review was updated by Pelham & Fabiano (2008), and additional studies added to the review firmly established BPT, contingency management strategies in schools, and peer-relation-focused behavior modification implemented in recreational settings (i.e., summer treatment programs) as well-established treatments according to evidence-based treatment task force guidelines (Lonigan et al., 1998).

The Pelham et al. (1998) and Pelham & Fabiano (2008) reviews were conducted in accordance with the child task force criteria for identifying evidence-based treatments guidelines (Lonigan et al., 1998) that include a consideration of within- and single-subject studies. Including these studies in reviews is very important for a number of reasons. First, the majority of the literature on behavior modification interventions for ADHD uses such methodology (see DuPaul & Eckert, 1997; Pelham & Fabiano, 2008; Pelham et al., 1998). Second, major reviews of medication effects include within-subject studies (e.g., 21 out of 29 Type 1 studies reviewed by Greenhill & Ford, 2002). Indeed, the majority of studies of stimulant medication are also short-term studies utilizing crossover designs (Conners, 2002), but that fact is rarely recognized in the literature and the treatment guidelines that discuss medication effects. Finally, sole reliance on randomized, controlled clinical trials in the construction of practice parameters is particularly puzzling because such trials have been criticized for an inability to generalize to individual cases (Jacobson & Truax, 1991; Kendall & Grove, 1988). Indeed, it is this generalization to individual cases that is a core goal for the entire enterprise of the scientific study of interventions.

Importantly, the above-mentioned literature must be considered in light of two reports commissioned by government agencies in the United States and Canada that came to different conclusions than the reviews discussed above. A report published by the Canadian Coordination Office of Health and Technology Assessment (CCOHTA; Miller, Lee, Raina, Klassen, Zupancic, & Olsen, 1998) reviewed and synthesized the between-group treatment literature on ADHD, and it concluded that “psychological/behavioural therapies were not consistently efficacious.” A separate report, commissioned by the Agency for Healthcare Research and Quality (AHRQ; Jadad, Boyle, Cunningham, Kim, & Schachar, 1999) in the United States to compare behavioral treatments to stimulant medication in head-to-head comparisons, also concluded that, “despite the limitations in the individual studies, the results indicate consistently that stimulants are more effective than non-pharmacological interventions when compared head-to-head.” Similar conclusions have been reached by qualitative reviews (e.g., Hinshaw, Klein & Abikoff, 2002).

There are potential explanations for these differing views in the literature. First, the CCOHTA and AHRQ reports included only between-group studies; whereas Pelham (Pelham & Fabiano, 2008, Pelham et al., 1998) and DuPaul and Eckert (1997) considered the entire evidence-base for behavioral treatments, including between-group, within-subject, and single-subject study designs. *Because so many of the studies of behavioral treatment of ADHD employ them, including some of the early classic studies, the exclusion of within-subject and single-subject study designs in the CCOHTA and AHRQ reports resulted in the omission of the majority of studies in the literature*. Furthermore, the authors of these reports only included behavioral treatment studies that also had medication conditions, a small subset of the literature, resulting in the omission of additional studies. Moreover, the CCOHTA and AHRQ reports did not discriminate between behavior modification treatments and other treatments such as cognitive therapy (e.g., self-control training), a distinction made in the Pelham et al. (1998) and DuPaul and Eckert (1997) reviews. This is an important distinction, because cognitive interventions have not been shown to be effective treatments for ADHD and their inclusion may dilute behavior modification study-related treatment effects (Abikoff, 1991; DuPaul & Eckert, 1997; Hinshaw, 2000; Pelham & Fabiano, 2008).

The importance of the CCOHTA and AHRQ reports and related reviews is that they have been heavily relied upon when practice parameters have been published by influential professional societies, including the American Academy of Pediatrics (AAP, 2001) and the American Academy of Child and Adolescent Psychiatry (2007). Both of these guidelines state that behavioral treatments have limited effectiveness relative to medication. In the latter practice parameter, behavioral interventions are recommended as last-line treatments to be employed only if the acute response to all FDA-approved medications is insufficient. Behavioral treatments are therefore relegated to a role in treatment equivalent to non-FDA-approved medications.

In summary, although there is support for behavior modification in the treatment of ADHD based on the evidence synthesized in the past decade, there is nonetheless considerable debate about the extent of the supportive evidence and therefore the role of behavioral approaches in treatment. This situation could be clarified with a comprehensive, systematic meta-analysis on the effectiveness of behavior modification for ADHD that includes all relevant studies to date. However, no such report exists. This review aims to improve on the current state of the literature by presenting a comprehensive, quantitative report on the magnitude of the effectiveness of behavioral treatments for ADHD that can serve as an up-to-date reference for ongoing efforts in treatment guideline development.

2. Method

In conducting this analysis, we attempted to follow recommendations made in standard texts on research synthesis (Cooper & Hedges, 1994; Hunter & Schmidt, 2004). When dealing with issues not covered in such texts (e.g., effect sizes from multiple types of designs), we have clearly described our procedures so as to enable replication and have highlighted these issues in the discussion.

2.1. Literature review

Studies included in this review were identified using multiple techniques. First, literature searches using PsycInfo were conducted. PsycInfo is an online database that comprehensively indexes scholarly and professional journal articles and book chapters from 1967 to the present. Search criteria entered into the database included: behavior modification, contingency management, behavior therapy, parent training, attention deficit hyperactivity disorder, hyperactive, and attention deficit disorder. Based on the results of the computerized search, articles were identified that met the inclusion criteria described below. Each identified article's reference section was then systematically analyzed, and additional studies were added to the review in this way. Also, serial searches of tables of contents in journals known to publish treatment studies were conducted (serial searches began at the year 1968, and the following journals were searched: *Behavior Modification, Behavior Therapy, Cognitive and Behavioral Practice, Journal of Abnormal Child Psychology, Journal of the American Academy of Child and Adolescent Psychiatry, Journal of Applied Behavior Analysis, Journal of Child Psychology and Psychiatry, Journal of Consulting and Clinical Psychology, Journal of School Psychology,* and *School Psychology Review).* Dissertations were identified using the same search terms in the PsycInfo database as well as the ProQuest Dissertation Database. Additionally, researchers known to conduct treatment studies on children with ADHD were contacted via email and asked to send a reprint or preprint of any recent treatment study they conducted. Thus, we made every effort to include both published and unpublished studies, given that the standard for research syntheses is now to include the entire literature appropriate for a review, not simply the published studies (McAuley, Pham, Tugwell., Moher, 2000; Rosenthal, 1994). The literature search was terminated in December 2006.

2.2. Inclusion criteria

A study was included in the initial collection based on specified search criteria: (1) the participants must be diagnosed with ADHD or significantly well-described to suggest the characteristic behaviors of ADHD (e.g., “hyperactive,” “off-task”). In studies that focused on treatment for children with externalizing behavior problems (e.g., ODD, CD, aggressive behavior), over 50% of the participants must have been diagnosed with ADHD or characterized as such; (2) the participants must not have an IQ reported to be below 80; (3) the participants must be under 18 years of age; (4) the participants may not have their condition better explained by a documented organic cause (e.g., brain trauma); (5) for between-groups designs, at least one treatment group must use an intervention that is primarily behavioral in nature (e.g., parent training with an emphasis on social learning principles and behavior modification techniques and/or behavioral classroom interventions) – for within-group and single subject designs, the primary treatment must be behavioral in nature (although there can be heterogeneity in the package of interventions and specific procedures used in the studies reviewed below, behavioral interventions generally use a consistent set of principles; for expanded considerations of this idea see Chorpita, Daleiden, & Weisz, 2005 and Garland & Hawley, 2008); (6) the study must include information that would permit the calculation of effect sizes; and (7) studies must be primarily treatment-outcome studies – laboratory investigations of behavior modification or combined treatments were not considered in this review. In total, 174 studies (counting each case in a single-subject publication as a study) that met these seven criteria were identified from 114 separate reports.

2.3. Study characteristics

Each study collected for the review was coded on a number of domains. These domains included: the study design, subject characteristics, setting, a description of the treatments, and the results. Coders completed a standardized form for each study, and coders met frequently to discuss coding and negotiate solutions to

Table 1
Summary of demographic and study characteristics by study design

Category	Between group	Pre-post	Within-subject	Single-subject
	Mean (SD) *% of studies reporting*	Mean (SD) *% of studies reporting*	Mean (SD) *% of studies reporting*	Mean (SD) *% of studies reporting*
Number of studies	20	30	23	101
Percent peer reviewed	90.0% (N/A) *100%*	60% (N/A) *100%*	95.7% (N/A) *100%*	77.2% (N/A) *100%*
Percent boys	78.0% (9.5%) *90%*	82.5% (12.5%) *83.3%*	82.0% (24.9%) *100%*	74.5% (N/A) *93%*
Age (years)	7.1 (2.4) *90%*	8.2 (2.6) *93.3%*	8.9 (1.4) *48%*	8.5 (2.3) *81%*
Percent Caucasian	74.9% (28.7%) *50%*	85.3% (5.2%) *43.3%*	77.1% (29.8%) *22%*	73.3% (N/A) *30%*
IQ	98.1 (5.7) *25%*	107.0 (6.6) *30.0%*	108.5 (3.3) *22%*	108.7 (12.1) *22%*
Percent two-parent family	68.7% (15.9%) *50%*	80.4% (14.8%) *40.0%*	65.3% (2.4%) *8%*	64.7% (N/A) *13.0%*
Comorbidity[a]				
ODD	42.2% (21.3%) *40%*	61.5% (24.7%) *33%*	45.6% (9.7%) *26%*	6.0% (N/A) *6%*
CD	9.2% (7.7%) 20%	33.4% (20.6%) *30%*	35.8% (17.2%) 26%	4.0% (N/A) 4%
Internalizing	20.5% (18.2%) *25%*	19.8% (21.6%) *20%*	27.00% (0%) *4%*	2.0% (N/A) *2%*
Learning disability	78.8% (N/A) *5%*	N/A *0%*	71.5% (27.5%) *17%*	8.0% (N/A) *8%*
Other	2.3% (2.1%) *15%*	5.3% (7.4%) *7%*	6.0% (0%) *4%*	3.0% (N/A) *3%*
Recruitment[a]				
Clinic referrals	73.7% (N/A) *95%*	86.2% (N/A) *97%*	50.0% (N/A) *87%*	53.3% (N/A) *92%*
School referrals	38.9% (N/A) *90%*	38.0% (N/A) *97%*	70.0% (N/A) *87%*	46.7% (N/A) *92%*
Advertisements	31.6% (N/A) *95%*	31.0% (N/A) *97%*	15.0% (N/A) *87%*	0.0% (N/A) *92%*
Setting				
Regular class	10%	13%	0%	45%
Special education class	0%	0%	57%	5%
Home	5%	3%	4.3%	11%
University-based clinic	65%	77%	0%	11%
Hospital/doctor office	15%	3%	4%	2%
Private practice	0%	0%	0%	5%
STP	5%	3%	35%	22%
Treatment[a]				
Parent-based	85% (N/A) *100%*	100% (N/A) *100%*	34.8% (N/A) *100%*	28.7% (N/A) *100%*
Number parent sessions	9.41 (2.74) *82%*	10.8 (5.7) *93%*	7.8 (1.6) *26%*	8.9 (2.6) *12%*
Teacher-based	26.3% (N/A) *95%*	40% (N/A) *100%*	82.6% (N/A) *100%*	65.4% (N/A) *100%*
Number teacher sessions	7 (3.6) *15%*	8.0 (4.8) *27%*	– (–) *0.0%*	13 (0.0) *6%*
Child-based	35.0% (N/A) *100%*	37% (N/A) *100%*	45.5% (N/A) *96.2%*	9.3% (N/A) *96%*
Number child sessions	10.0 (3.0) *35%*	16.0 (12.5) *37%*	40 (0.0%) *50%*	N/A

[a]A single study could be counted in multiple categories so percentages may not sum to 100%. ODD/CD = Oppositional Defiant Disorder/Conduct Disorder. STP = Summer Treatment Program.

discrepancies. A subset of studies was coded by two raters to permit the calculation of reliability estimates. As noted by Orwin (1994) reliability statistics were computed for each coding category, not each study, to provide a meaningful indication of the consistency across raters. A phi of 1.00 indicates perfect agreement and a phi of 0.00 indicates no agreement (Hartmann, 1977). As a rule of thumb, phi statistics between .00 and .40 are poor, between .40 and .59 are fair, between .60 and .74 are good, and between .75 and 1.00 are excellent (Orwin, 1994). Phi statistics ranged from .67–1.00 with an average phi of .88 (mode = 1.00).

2.3.1. Study design

Studies were classified into one of four design categories. A study was considered a between-group design if the study contained an active treatment group and a no-treatment control group (e.g., Pisterman et al., 1992). Studies were classified as pre-post designs if they contained only a treatment group assessed at pre and post treatment (e.g., Pelham & Hoza, 1996), or compared the relative effects of multiple treatments without a no-treatment control group (e.g., Barkley, Guevremont, Anastopoulos, & Fletcher, 1992). Studies were classified as within-subject designs if the treatment group received multiple treatments over time in a crossover fashion (e.g., Kolko, Bukstein, & Barron, 1999). For some studies, data were presented for individual subjects and also aggregated across subjects. In these instances, these were classified as within-subject studies and effect sizes were calculated for the aggregate report. A study was classified as a single-subject design if data were made available for a single participant in either a multiple baseline or ABAB or similar design (e.g., Rapport, Murphy, & Bailey, 1980). In the event a paper contained information that presented data individually for multiple participants, and not aggregate data, each participant was considered to be an independent single-subject study.

2.3.2. Subject characteristics

Coders recorded demographic information on the participants included in the studies. This information included age, IQ, race, gender, the marital status of the parents, diagnosis, comorbidity, and the recruitment process. Summary descriptives are presented in Table 1.

2.3.3. Description of treatment

Detailed information was recorded on the treatment interventions utilized in the studies. Coders recorded the participants in treatment, the number of sessions between the clinician and participant, and the setting. Summary descriptives are presented in Table 1.

2.4. Effect size calculations

Effect sizes were calculated for each dependent measure included in the identified studies and averaged across measures to derive an effect size for each study. Measures that were employed inconsistently or were unrelated to core deficits of ADHD were not included (e.g., child-reported symptoms, internalizing symptoms, parent and teacher stress ratings). For the purposes of this report, a positive effect size indicates an improvement in functioning and a negative effect size represents deterioration in functioning. Because the magnitudes of effect sizes are not comparable across design types, effect sizes were calculated separately for each of the four treatment designs. In the few cases where null results were reported, but means and standard deviations were not, an effect size of 0.00 was entered. Specific information on the procedures for calculating the effect size for each study design is listed below.

2.4.1. Between group designs

For calculating the effect sizes of treatments using between- group designs, Cohen's *d* effect size was used (e.g., Hunter & Schmidt, 2004).

The post-treatment mean of the control group was subtracted from the post-treatment mean of the treatment group, and the difference was divided by the pooled standard deviation of the groups at post-treatment.

2.4.2. Pre-post designs

Effect sizes for pre-post design studies were calculated by subtracting the post-test mean from the pre-test mean and dividing by the standard deviation of the pre-test mean. In instances where a behavioral treatment package and its component parts were included in a study (e.g., parent training + school intervention, Parent training alone, school intervention alone), the package (i.e., parent training + school intervention) was used to compute the effect size.

2.4.3. Within-subject designs

Effect sizes in within group designs were calculated by subtracting the post-intervention mean from the pre-intervention mean and dividing the difference by the pre-intervention standard deviation. In instances where multiple levels or combinations of behavioral treatment were included in a study, the combination treatment was used to calculate effect sizes.

2.4.4. Single subject designs

The majority of single subject designs did not include means and standard deviations. However, nearly all presented graphs of individual data points. A procedure recommended by Busk & Serlin (1992) and White, Rusch, Kazdin, & Hartmann (1989), and used by Stage & Quiroz (1997), was utilized for computing effect sizes for single subject design studies. Individual data points from each graph were estimated when means and standard deviations were not provided. Reliability checks were conducted on 20% of randomly selected studies and a reliability index was calculated by dividing the number of agreements by the total number of data points. A rating was considered in agreement if the two raters were within one point of each other. The percentage agreement averaged 85%.

Some methodological issues are important to consider when calculating effect sizes from single subject studies. Many studies utilized a reversal design wherein treatment was implemented after an initial baseline phase, and systematically withdrawn and reinstated to demonstrate experimental control (i.e., an ABAB design). Behavior during a reversal was not always comparable to baseline, due to generalization or learning that occurred during treatment. Therefore, reversal conditions were not included in the computation of the means and standard deviations for the control conditions. Furthermore, some single subject studies included an assessment of the effectiveness of different components of behavioral interventions (e.g., rewarding alone, response cost alone, rewarding plus a response cost component). Consistent with the approach for other study types, the combined behavioral treatment conditions (e.g., reward and response cost components), and not the component parts, were used to calculate the effect size estimates.

3. Results

Results are presented by study design. For analyses of effect size homogeneity and moderator effects, effect sizes within a study were averaged to yield a single study effect size to maintain the assumption of independence in the analyses. Table 1 lists some of the characteristics of each study and the percentage of studies that reported information on each of the categories. Table 2 includes the unweighted effect sizes for each study domain of measurement, as well as the overall unweighted effect size for each study design. Notably, there were no significant differences in the magnitude of effect size for peer-reviewed versus non-peer-reviewed studies (i.e., book chapters, dissertations; $p > .05$) so all studies were grouped together. A detailed table that summarizes the characteristics of all studies included in the meta-analysis may be reviewed at http://ccf.buffalo.edu.

3.1. Between-group designs

A total of 23 between-group treatment studies for ADHD were identified. Publication year ranged from 1976–2006 (Three studies were not included in the meta-analysis because methodological features of the study design precluded the computation of effect sizes; Fallone, 1998; Loney, Weissenberger, Woolson, & Lichty, 1979; Wolraich, Drummond, Salomon, O'Brein, & Sivage, 1978). Thus, 20 between-group design studies were entered into the meta-analysis. The total number of participants who received behavior therapy was 523. The average, unweighted effect size for between-group studies was 0.83 (SD = 0.54; Range = 0.05–1.91). The 95% confidence interval for this effect size was 0.57–1.08. Several of the study characteristics shown in Table 1 were examined to determine their correlations with effect sizes across studies. Neither total *N*, average IQ, age, percentage of boys included in each study, percentage of Caucasian participants, percentage of comorbid ODD or CD participants, percentage of two-parent families, or the number of parent, teacher, or child treatment

Table 2
Unweighted effect sizes across study designs and types of measures

		Total	Parent ratings				Teacher ratings			Observations				Academics	
Design			ADHD Sx	Ext Sx	Imp	Parenting	ADHD Sx	Ext Sx	Imp	Child clinic	Child natural	Parenting clinic	ITBC	Productivity	Ach
Between group	*N* of studies	20	11	9	12	8	8	6	8	5	3	4	0	1	3
	M (SD)	.83(.54)	.39 (.46)	.33 (.60)	.84 (.74)	.70 (.44)	.79 (.81)	.50 (.48)	.55 (.51)	.19 (.41)	.56 (.20)	1.05 (.68)	–	.63 (N/A)	.32 (.35)
	Range	.05–1.91	−.45–.98	−.21–1.80	−.21–2.12	.17–1.42	0–2.37	.05–1.19	.07–1.45	.16–.90	.42–.78	.27–1.87	–	N/A	−.04–.66
Pre-post	*N* of studies	30	21	17	19	8	12	9	6	5	8	6	4	1	5
	M (SD)	.70 (.31)	.90 (.45)	.76 (.41)	.74 (.46)	.56 (.24)	.79 (.78)	.33 (.27)	.78 (.38)	.96 (1.00)	.64 (.56)	5.08 (11.54)	.71 (.11)	.43 (N/A)	.11 (.23)
	Range	.20–1.38	.05–1.70	.10–1.63	−.27–2.36	.28–.93	−.19–2.09	−.11–.77	.12–1.15	.00–2.58	−.04–1.63	−.08–28.63	.55–.81	N/A	−.28–.28
Within-subject	*N* of studies	23	1	1	0	0	3	3	2	0	22	0	2	8	0
	M (SD)	2.64 (3.71)	.92 (N/A)	1.54 (N/A)	–	–	.51 (.73)	.45 (.14)	.41 (.11)	–	2.16 (2.93)	–	1.57 (.18)	1.91 (4.67)	–
	Range	−.37–13.41	–	–	–	–	−.26–1.20	.30–.57	.33–.48	–	−.37–12.62	–	1.44–1.70	−.61–13.41	–
Single-subject	*N* of studies	101	4	0	0	0	0	0	0	8	89	9	4	26	0
	M (SD)	3.78 (4.88)	3.70 (2.65)	–	–	–	–	–	–	1.06 (.97)	4.38 (6.15)	2.60 (3.87)	1.70 (.31)	3.33 (4.93)	–
	Range	−.75–29.38	.67–7.05	–	–	–	–	–	–	−.82–2.12	−.43–33.91	−1.19–12.08	1.34–2.01	−.30–23.31	–

Notes: ADHD = Attention-deficit/hyperactivity disorder. Sx = Symptoms. Ext=Externalizing. Imp = Impairment. ITBC = Individualized target behavior checklist. Ach = Academic Achievement Testing. *Effect size for between group studies weighted by the inverse of the variance in a random effects model = .74.

sessions, were significantly ($p > .05$) correlated with effect size. There was a negative correlation between effect size and publication year ($r = -.62$, $p < .01$).

Multivariate statistics were used to examine the overall effect size of the between-group studies. To remove the bias that is associated with the sample size used in each study, each effect size was weighted by the inverse of its variance. Once the studies were corrected for sample size, the weighted average unbiased effect size was .67 with a 95% confidence interval of .54–.80. These effect sizes were entered into a fixed effects model to test whether the effect sizes were homogeneous. The Q statistic was significant ($Q = 51.41$, $p < .001$), indicating that the effect sizes entered into this model were heterogeneous. As Table 1 indicates, there were few potential moderator variables that were reported consistently across studies, making moderator analyses difficult to perform. Because of this, and the lack of significant correlations between study effect sizes and potential moderator variables, a random effects model was calculated. The weighted random effects average effect size was .74 (95% confidence interval = .52–.95), indicating a moderate to large effect of behavior modification treatment.

To ascertain the robustness of the effect size estimate, a fail safe N procedure was calculated (Orwin, 1983). Based on the calculation of the fail safe N, 63 additional studies yielding a small effect size (ES = .20) would be required to render the unweighted effect size of .83 a small effect. Given that only 20 between-group studies were included in the meta-analysis, this suggests the results are robust.

3.2. Pre-post designs

A total of 30 pre-post design treatment studies for ADHD were identified. Publication year ranged from 1978–2008. All identified studies were included in the calculation of effect sizes. One outlier effect size (White, 2004) was windsorized (Lipsey & Wilson, 2001), and the value of the effect size was set to 1.38. The number of participants who received behavior therapy in these studies was 1,077. The average, unweighted effect size for pre-post design studies was 0.70 (SD = 0.31; Range = .20–1.38). The 95% confidence interval ranged from .59–.82.

To permit the analysis of the effect sizes in a fixed-effects model, procedures as outlined by Becker (1988) were used. Specifically, the control group N was imputed to be the same size as the treated group. For the purposes of these analyses, it was assumed that maturation and repeated assessments did not result in improvement for untreated individuals (see DuPaul & Eckert, 1997 for another example of this). Multivariate statistics were used to examine the overall effect size of the between-group studies. To remove the bias that is associated with the sample size used in each study, each effect size was weighted by the inverse of its variance. Once the studies were corrected for sample size, the weighted average unbiased effect size was .63 with a 95% confidence interval of .54–.71. These effect sizes were entered into a fixed effects model to test whether the effect sizes were homogeneous. The Q statistic was non-significant ($Q = 22.14$, $p > .05$), indicating that the effect sizes entered into this model were homogenous. Thus, in the pre-post study designs, the average effect size of .63 appears to be a robust and replicated effect. Based on the calculation of the fail safe N, 75 additional studies yielding a small effect size (ES = .20) would be required to render the unweighted effect size of .70 a small effect.

3.3. Within-subject designs

A total of 24 within-subject treatment studies for ADHD were identified, and a single study was excluded due to an inability to compute effect sizes (Kasier, 1992). Publication year ranged from 1981–2006. The total number of participants treated with behavior therapy was 386. For within-subject studies, the unweighted effect size for behavioral treatments averaged 2.64 (SD = 3.71; Range = −.37–13.41). The 95% confidence interval ranged from 1.03–4.24. Based on the calculation of the fail safe N, 281 additional studies yielding a small effect size (ES = .20) would be required to render the unweighted effect size of 2.64 a small effect.

3.4. Single-subject designs

A total of 44 reports that included at least one single-case design study were identified. A total of 108 single-subject treatment studies for ADHD were included in these reports. Of these, one was not included due to the child having an IQ less than 80 (Northup et al., 1999) and six were not included in the effect size calculations due to insufficient information (Chronis, Fabiano, Gnagy, Wymbs, Burrows-MacLean, & Pelham, 2001; Pelham, 1977; one case from Pollard, Ward, & Barkley, 1983; Waschbusch, Kipp, & Pelham, 1998). Publication year ranged from 1968–2006. For single-subject studies, the unweighted effect size for behavioral treatments averaged 3.78 (SD = 4.88; Range = −.75–29.38). The 95% confidence interval for the mean ranged from 2.82–4.74. Based on the calculation of the fail safe N, 1808 additional studies yielding a small effect size (ES = .20) would be required to render the unweighted effect size of 3.78 a small effect.

3.5. Effect sizes across study designs and measures

Table 2 displays unweighted effect sizes for each study design by rater and domain assessed. When effect sizes are presented in this way, the heterogeneity across study designs in the approach to measurement is apparent. For example, group design studies generally rely on parent and teacher ratings whereas within-subject and single-subject design studies emphasize direct observational measures. It is also clear that even within designs, there are large differences in effect size magnitude depending on the type of measure used. For example, in between group studies, laboratory observations of parenting behaviors yielded an average effect size of 1.05, whereas child behavior in the same setting was .19.

4. Discussion

This meta-analysis represents the first comprehensive research synthesis of the literature on behavioral treatments for ADHD that spans all behavior modification treatments and study designs since the first identified ADHD treatment paper in 1976. One hundred, seventy-four studies from 114 separate reports with 2094 participants were included. The results clearly support the effectiveness of behavioral treatments for ADHD. Results were consistent across study methods and designs, which suggests the generalizability of the findings (Sidman, 1960). The magnitude of between-group effects from 20 studies approaches the range classified as “large” by Cohen (1992), and the results are similar to the effect sizes reported in other meta-analyses of child treatment (Weisz & Weiss, 1993) and stimulant medication (Conners, 2002). Supporting the between-group synthesis are the results of the analyses for other study designs. Across designs, behavioral treatments are clearly better than control conditions, and the effects of the intervention are substantial. The fail-safe N estimates show that the number of studies with minimal results that would be needed to make these large effects become small is considerable, supporting the robustness of the effect size estimates across studies. We will discuss the results of the meta-analyses for each study design, and the overall results, in turn. We will then discuss several methodological and interpretive issues in this analysis and the literature as a whole.

The weighted effect size of .74 for between-group studies indicates that behavioral interventions, implemented in the home, school, or peer setting, result in substantial improvement. These results are comparable to or greater than those obtained by other meta-analyses of the behavioral treatment literature for ADHD. For example, DuPaul and Eckert (1997) reported an overall between-group effect size of .45 for classroom-based contingency management approaches. The present effect size is also larger than those reported in Lundahl et al. (2006; ES = .42–.53 for child and parent outcomes) and smaller in magnitude than that reported by Serketich & Dumas (1996; ES = .86) for BPT interventions for disruptive children. The effect size reported herein represents the most comprehensive indicator of behavioral treatment compiled to date, including behavioral treatment across domains of functioning (home, school), targets of treatment (parent, teacher, child), unpublished studies, and across childhood and adolescence. The fact that neither study N, sample demographics (gender, race, number of parents in household), child comorbidity, nor number of sessions of treatment was associated with study effect size gives support to the generalizability of the results across several important dimensions.

The pre-post effect sizes support and extend the results of the between-groups analysis. The fixed effects analysis yielded an effect size of .63, and this effect size was found to be homogeneous across the 30 studies included. Thus, the effects of behavior modification result in a moderate-to-large effect at post-treatment. Importantly, rather than using the active-treatment control group to compute effect sizes, studies were classified as pre-post designs if they included an *active* treatment control group (e.g., MTA Cooperative Group, 1999). To have done otherwise and computed effect sizes using an active treatment as the control would have resulted in uninterpretable results for the present goal – to document the magnitude of effects attributable to participation in behavioral treatment. By way of example, in the Multimodal Treatment Study for ADHD (MTA Cooperative Group, 1999), if one uses the community comparison group (of whom approximately two-thirds were medicated with stimulants prescribed by community physicians and the majority of whom received behavioral interventions in the classroom from their teachers) as a control group the between group effect size is −.01, suggesting behavior therapy is equivalent to the community comparison. Notably, the effect size of the medication group in the MTA study, when calculated in reference to the comparison group at post-treatment is also modest (effect size = .26). In contrast, the pre-post change effect size for behavioral treatment equaled .55. Thus, depending on the calculation of the effect size, one would reach quite different conclusions regarding the effectiveness of behavior therapy in this study.

Relatively larger effects were apparent in the within-subject and single-subject studies relative to group design studies. Inclusion of these designs added many studies conducted in school and recreational settings (see Table 1), extending the results of the between-group studies to other settings. Further, these studies accounted for most of the objective measures of outcome (e.g., independent observations vs. subjective ratings of improvement). Perhaps one reason for the larger effect sizes in these studies was that subjects served as their own controls, and variability was minimized. It is also possible that these studies illustrate larger effects because of procedural differences. For example, relatively more intensive behavioral procedures (e.g., contingency management; Pelham & Murphy, 1986) were used in many of the within-subject and single-subject studies, compared to the between-group and pre-post design studies, where a clinical behavior therapy approach (e.g., clinical parent training programs; consultation with teachers on behavior modification strategies) was more likely to be used (Pelham et al., 1998). Contingency management approaches typically produce considerably larger gains than clinical approaches (e.g., Fabiano et al., 2007; Hinshaw, Klein & Abikoff, 2002). Further, a number of the within- and single-subject design studies were conducted in analogue settings or special classrooms where treatment integrity was carefully monitored and supported and may have contributed to larger effects. It is noteworthy that DuPaul and Eckert (1997) employed a different method for computing effect sizes in their analysis of crossover and single-subject school studies, but our effect size estimates are similar to theirs.

Several methodological aspects of this meta-analysis are important to note. First, the inclusion of within-subject and single-subject design studies greatly increased the number of studies reviewed. In fact, had only randomized, controlled between-group studies been analyzed, only 12% of treatment studies on behavioral treatment for ADHD would have been included, and many of the classic behavioral intervention studies would have been excluded. Most previous systematic reviews of ADHD treatments (cf. Jadad et al., 1999; Miller et al., 1998) have chosen to include only randomized, controlled, clinical trials, and numerous policy and treatment recommendations have been made based on such results (e.g., AAP, 2001). The present review documents that these prior reviews and guidelines were limited by their use of a small sample of the entire behavioral treatment literature for ADHD. In addition, the Jadad et al. and Miller et al. reviews included cognitive-behavioral treatments along with behavioral interventions in the same meta-analyses. Since there is a clear difference in efficacy between behavioral treatments and cognitive treatments for children with ADHD, with the latter interventions having little support for efficacy with ADHD samples (DuPaul & Eckert, 1997; Hinshaw, 2000; Pelham & Fabiano, 2008), the inclusion of cognitive-behavioral treatments would have reduced the effect size estimates. Therefore, only behavioral treatments were included in this meta-analysis. Both of these methodological differences build and improve upon previous meta-analyses of behavioral treatments for ADHD. Further, the fact that there was no association between the effect sizes of published and unpublished literature suggests that broad inclusionary criteria in systematic reviews are appropriate and should be implemented to provide a comprehensive picture of a clinical literature. In the case of this literature, the “file-drawer” problem did not reduce the magnitude of the effect size but did contribute to the size of the study sample and thus its representativeness.

Notably, as Table 1 illustrates, the samples included in the studies reviewed appeared to be heterogeneous and generally representative of children and adolescents with ADHD. The ADHD literature reviewed in these studies includes non-Caucasian subjects and girls, with considerable comorbidity, recruitment primarily from school or clinic referrals, and conducted in a variety of treatment settings (see Table 1). The major lack of heterogeneity is with respect to age, which is early-to-middle elementary school, with only a handful of studies with preschoolers and adolescents. In summary, the studies are done in a variety of settings with heterogeneous samples of ADHD children, and, as noted above, the results were not systematically associated with a variety of sample characteristics. Thus, across different study designs, these results would appear to be generalizable to the larger population of children with ADHD in clinic and elementary school settings. That the between-group and pre-post effect sizes are similar also suggests that behavioral treatments are efficacious as well as effective.

As noted in the Method, weighted effect sizes were computed for between group and pre-post design studies in order to place these effect sizes within the context of other meta-analyses. These weighted effect sizes control for the sample size of each study, since larger sample sizes produce effect sizes that are more reliable. Weighted effect sizes were not calculated for the within- and single-subject study designs because the subjects in these studies served as their own controls. Therefore, any analyses that weighted these effect sizes by sample size would have relied on the imputation of a control group sample size that may have over-estimated the effect of the treatment and violated assumptions of independence between the control group and treatment group. Further, while the Cohen (1992) guidelines for interpreting the magnitude of an effect size can be used to provide a general guideline for interpreting the between-group effect size in the present meta-analysis, it is not clear that this guideline applies to pre-post, within-subject, and single-subject designs. An important area of future work in the meta-analysis field is developing and implementing statistical procedures for weighting and analyzing effect sizes generated from studies that utilize designs other than the traditional between-group approach and enable integration of effect sizes across designs (for an example see Morris & DeShon, 2002).

The results presented in Table 2 have interesting implications for treatment outcome studies for children with ADHD. As can be observed from the table, there is considerable heterogeneity across measures and raters of outcome as well as across study designs.

Consider for instance observational measures conducted in between-group studies. The average effect size was .19 for child behavior observed in the clinic, .56 for child behavior observed in natural settings (e.g., classrooms), and 1.05 for observations of parenting behavior. Observations of the behavior of children with ADHD in clinic settings have little predictive validity (Roberts & Hope, 2001), and it is not surprising that their use resulted in minimal effect sizes. Observations of behavior in natural settings (e.g., school) – the settings where treatment is active and the child presents impaired functioning – results in a moderate treatment effect, and observations of parenting behaviors (the proximal outcome of behavioral parent training) show very large effects. Observations are widely considered the gold standard of outcome measures in the field of ADHD (Pelham, Fabiano, & Massetti, 2005). More routine use of observations in between-group studies or routine inclusion of within-subject studies in meta-analyses would yield larger and arguably more valid effect sizes.

Similarly, parent ratings of ADHD symptoms in between-group studies yielded a much smaller effect size (.39) relative to parent ratings of impaired functioning (.84). Studies and/or analyses that focus on the former as the primary outcome measure (e.g., MTA Cooperative Group, 1999) will show considerably smaller behavioral treatment effects, for example relative to medication, than studies and/or analyses that focus on the latter (e.g., Wells et al., 2006). Notably, measures of parenting, peer relationships, and academic functioning in school are better validated as outcome measures predictive of long-term functioning than are measures of DSM symptoms (Pelham, Fabiano, & Massetti, 2005). Future studies should focus on measures with good ecological validity, that are evidence-based for ADHD, and are logically linked to the functional outcomes that treatment targets (Pelham et al., 2005).

Finally, it should be noted that the effect sizes in Table 2 show that the impact of behavioral treatments is robust across a variety of measures from different sources in a variety of settings. This is not surprising, as the components of the behavioral interventions in the studies reviewed typically included a focus on parents (behavioral parent training), teachers (classroom management), and children directly (peer-relation-focused interventions). As Table 2 illustrates, all of these components produced substantive effect sizes. At the same time, many if not most of the studies included components focusing on multiple agents (parent *and* teacher *and* child), and neither the majority of studies reviewed or our approach involved dismantling treatments into their component parts (Pelham & Fabiano, 2008). It is also important to note that the most proximal and important outcome in a given study may be dependent on the focus of the intervention used.

The results of this quantitative synthesis of outcomes for children with ADHD treated with behavioral interventions needs to be considered in the context of the literature as a whole, as well as some specific individual studies in the literature. One issue that has been discussed has been what dose or intensity of behavioral treatment is required to produce clinically meaningful effects for ADHD children (Pelham & Fabiano, 2008). Although our correlations showed that number of sessions was not associated with study effect size, the range of sessions was relatively narrow. Relatively more intensive behavioral interventions in the peer domain (e.g., Chronis et al., 2004) produce much larger effects than studies using less intensive interventions (e.g., Antschel & Remer, 2003). However, individual differences have not been investigated in such studies to determine which children need relatively more intensive treatments and which children can improve with a lower dose of behavioral treatment. For example, children in the Bor et al. (2002) study improved equally across standard and enhanced groups. Only one recent study systematically manipulated the intensity/dose of behavior modification treatment (Fabiano et al., 2007; Pelham et al. submitted for publication-a,b), so we could not perform a meta-analysis of dose.

Another issue to be considered is generalizability (Pelham & Fabiano, 2008). As discussed above, the results suggest behavioral treatments are effective across different settings and across subjects with diverse characteristics. On the other hand, we were not able to analyze generalization over time—that is maintenance—due to a dearth of studies reporting it. ADHD is currently conceptualized as a chronic disorder requiring treatment throughout childhood and adolescence (American Academy of Pediatrics, 2001). Because behavioral interventions are highly palatable and preferred over medication by parents and teachers of children with ADHD (Pelham et al., submitted for publication-c), and because palatability is an important mediator of treatment sustainability for a chronic disease, behavioral interventions may be an essential component of long-term treatment for ADHD. This review does not provide information on the most effective means of sequencing behavioral treatment. Recent studies employing innovative designs have found beneficial effects of behavioral interventions when employed as first line interventions, on the need for future use of medication in treatment (e.g., Dopfner, Breuer, Schurmann, Metternich, Rademacher, & Lehmkuhl, 2004).

An important implication from the current meta-analysis is that the few recent studies that have been interpreted as showing that behavioral treatment is ineffective (e.g., Abikoff et al., 2004; MTA Cooperative Group, 1999) are not an accurate reflection of the literature as a whole. It is notable that such studies appear to have been given differential weight in some prominent reviews (e.g., Hinshaw, Klein, & Abikoff, 2002) and in the most influential treatment guidelines for ADHD in North America (e.g., AAP, 2001; AACAP, 2007) emphasizing the first-line use of medication in ADHD treatment and chronic management, while de-emphasizing behavioral treatments or casting them as a third-line or adjunctive treatments. These present results suggest that professional guidelines and recommendations should be modified to reflect the current state of the entire literature on behavioral treatments for ADHD, and that conclusions in other meta-analyses (e.g., Jadad et al., 1999) should be revised given this more comprehensive synthesis. Based on the strength of the evidence and a risk:benefit analysis, a recent task force report from the American Psychological Association has recommended that behavioral treatments be first-line interventions for ADHD (Brown et al., 2007).

4.1. Conclusion

Across study designs and including different settings (e.g., home, school, recreational), a consistent pattern of results emerged – behavioral treatments improve the functioning of children with ADHD. Prevalence rates place at least one child with ADHD in every classroom in America (APA, 1994; Froehlich et al., 2007), making it one of the most prevalent mental health disorders of childhood. Because of its prevalence and its refractory course, childhood ADHD results in considerable costs for society (Forness & Kavale, 2002; Pelham, Foster, & Robb, 2007), highlighting the need for effective interventions. This research synthesis provides a quantitative validation of recent reviews (Brown et al., 2007; Pelham & Fabiano, 2008), demonstrating that behavioral interventions are a viable and effective intervention for ADHD. Our results suggest that efforts should be redirected from debating the effectiveness of the intervention to disseminating, enhancing, and improving the use of behavioral interventions in community, school, and mental health settings.

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