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Objective: We conducted a meta-analysis of cognitive–behavioral treatment (CBT) studies for adult attention-deficit/hyperactivity disorder (ADHD), examining effects versus control and effects pre-to-post treatment to maximize the clinical and research utility of findings from this growing literature. ***Method:*** Eligible studies tested adults meeting criteria for *Diagnostic and Statistical Manual of Mental Disorders* ADHD as determined by interview or using a standardized rating scale and measured ADHD symptoms or related impairment at baseline and posttreatment. We analyzed data from 32 studies from published and unpublished sources available through December 2015. Effect size calculations included up to 896 participants. ***Results:*** Using a random effects model, we found that CBTs had medium-to-large effects from pre- to posttreatment (self-reported ADHD symptoms: $g = 1.00$; 95% confidence interval [CI: 0.84, 1.16]; self-reported functioning $g = .73$; 95% CI [0.46, 1.00]) and small-to-medium effects versus control ($g = .65$; 95% CI [0.44, 0.86] for symptoms, .51; 95% CI [0.23, 0.79] for functioning). Effect sizes were heterogeneous for most outcome measures. Studies with active control groups showed smaller effect sizes. Neither participant medication status nor treatment format moderated pre-to-post treatment effects, and longer treatments were not associated with better outcomes. ***Conclusions:*** Current CBTs for adult ADHD show comparable effect sizes to behavioral treatments for children with ADHD, which are considered well-established treatments. Future treatment development could focus on identifying empirically supported principles of treatment-related change for adults with ADHD. We encourage researchers to report future findings in a way that is amenable to meta-analytic review.

What is the public health significance of this article?
1. From pre- to posttreatment, cognitive–behavioral treatments (CBTs) for adult ADHD are associated with medium-to-large effects on ADHD symptoms and related impairment. 2. Smaller but significant effects in controlled studies suggest that the effects of CBTs for adult ADHD may extend beyond nonspecific factors. 3. Effects of CBT for adult ADHD were comparable for participants on and off medication and for a variety of treatment formats (i.e., group vs. individual).

Keywords: adult ADHD, cognitive–behavioral therapy, meta-analysis, treatment outcome

Supplemental materials: http://dx.doi.org/10.1037/ccp0000216.supp

Attention-deficit/hyperactivity disorder (ADHD) in adults is a disorder of inattention and hyperactivity-impulsivity, with an approximately 4.4% prevalence rate in the U.S. population that is associated with impairment across multiple domains of functioning and with increased risk for psychiatric comorbidity (Barkley, Murphy, & Fischer, 2008; Kessler et al., 2006). Although medications—in particular, stimulant medications—are considered first-line treatments for ADHD in adults, not all clients respond to medication treatment, while those considered responders according to the standards used in many medication studies (30% or more reduction in symptoms; Steele, Jensen, & Quinn, 2006) may continue to experience significant, impairing symptoms. An accelerating number of studies over the past 20 years have investigated the efficacy of nonmedication approaches for adults with ADHD, including cognitive–behavioral treatments (CBTs), which involve training clients in cognitive and behavioral skills to address symptoms. Skills incorporated into CBT approaches for adult ADHD range from organization, planning and time management skills to cognitive reappraisal strategies and mindfulness meditation skills. While prior reviews of the literature suggest that structured psy-

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chosocial approaches for adults with ADHD may be efficacious (Mongia & Hechtman, 2012), the magnitude of treatment effects reported across studies appears to be heterogeneous (Knouse & Safren, 2010).

To provide researchers and clinicians with a detailed and useful quantitative summary of the current state of the evidence for CBT for adult ADHD, we conducted a meta-analysis of treatment studies. Our analysis was guided by the following goals: (a) estimate the magnitude of the acute effects of CBTs for adult ADHD on symptoms and functioning, (b) determine whether effects across studies are indeed heterogeneous, and (c) test potential moderators of treatment effects, including study design features and treatment characteristics.

Studies of treatments for adult ADHD vary not only in their observed effect sizes, but also in study designs used, constructs measured, and characteristics of the treatments tested. Diversity across these dimensions presents a substantial decision-making challenge to the meta-analyst (Fabiano, Schatz, Aloe, Chacko, & Chronis-Tuscano, 2015). Research designs range from randomized controlled trials with attention-matched controls to small, open pilot studies. Measurement methods for ADHD symptoms and functional impairment vary from study to study, as do the specific treatment strategies used with clients. In particular, it is important to recognize that "cognitive–behavioral therapy" (CBT) is a general term that describes a family of interventions, each of which might employ a package of treatment strategies (Craske, 2010). CBTs developed for adult ADHD appear to vary, for example, in the extent to which they cover a range of topics related to adult ADHD versus focusing on skills that target core symptoms of the disorder (Knouse & Safren, 2010, 2014), and few treatment manuals have been tested in more than one or two studies.

In a meta-analysis, managing these sources of complexity—different research designs, measures, and treatments characteristics—requires thoughtful decision-making guided by the overall goals of the analysis (Card, 2012). Previous published meta-analyses of nonmedication treatments for adult ADHD have taken divergent approaches to handling the complexity. Linderkamp and Lauth (2011) searched the literature in 2010 and meta-analyzed 12 studies of psychotherapy for adult ADHD. They calculated an effect size that combined effects from controlled studies and open trials, treating effects compared with a control group and effects compared with baseline as comparable to one another. They also combined effect sizes from measures, regardless of construct. Using this approach, the authors calculated a weighted effect size across studies and measures of $d = 0.84$. While this analytic approach reduces the apparent complexity of findings, it may also mask important differences across study designs, measures, and treatments. The limitations of this analysis led Moriyama, Polanczyk, Terzi, Faria, and Rohde (2013), when reviewing the available meta-analytic evidence for adult ADHD treatments in 2013, to state that, "No conclusions about the impact of psychosocial interventions can be drawn based on meta-analyses so far" (p. 296).

More recently, Jensen, Amdisen, Jørgensen, and Arnfred (2016) reported on a systematic review and meta-analysis of controlled trials of CBT for adult ADHD. The goal of the analysis was to inform evidence-based national clinical guidelines for treatment of adults with ADHD in Denmark. The meta-analysis included only randomized trials in which CBT was compared with treatment-as-usual, but not other types of control conditions (e.g., active or "placebo" comparison groups). The definition of CBT used in the analysis also appears to have been restrictive, as the work of, for example, Solanto, Marks, Mitchell, Wasserstein, and Kofman (2010) was excluded for falling in the category of "psychological interventions different from CBT" (Jensen et al., 2016, p. 4) even though Solanto and colleagues' (2010) intervention uses, "cognitive-behavioral principles and methods to impart skills and strategies in time management, organization, and planning and to target depressogenic and anxiogenic cognitions that undermine effective self-management" (p. 958). After searching the literature in March 2014, Jensen et al. identified two eligible studies for their meta-analysis (Emilsson et al., 2011; Safren et al., 2005) with a total n of 63. The weighted effect size across these studies was $d = 1.00$ for self-reported symptoms and $d = 0.96$ for investigator-rated symptoms. Although this meta-analysis included a homogeneous set of studies and reduced ambiguity in the interpretation of the results—a desirable quality for official national clinical guidelines—a substantial amount of the relevant literature may have been excluded.

Most recently, Auclair, Harvey, and Lepage (2016) conducted a meta-analysis of adult ADHD CBT randomized, controlled trials. They identified 12 eligible studies according to their criteria and reported a large effect size (Hedges' $g = .95$) compared with control across self-reported and investigator-rated ADHD symptoms. However, effect sizes based on comparisons without appropriate control conditions appear to have been included in this estimate. For example, an effect size from Weiss et al. (2012) was calculated by comparing CBT plus pill placebo versus CBT plus active medication treatment. Since nonmedication treatment was held constant, between group differences in effects would reflect the influence of medication, not CBT. In another example, an effect size based on comparison of two active CBT conditions from Pettersson, Soderstrom, Edlund-Soderstrom, and Nilsson (2014) was included. Given the difficulties in interpreting these findings, they cannot be considered conclusive regarding the effects of CBT for adult ADHD in controlled trials.

Our purpose in conducting a meta-analysis of this literature was to address the limitations of these previous meta-analyses and to provide a meaningful, comprehensive summary of the current state of the evidence for CBT for adult ADHD that would be useful for clinical decision-making and informative to clinical researchers. Given the importance of gleaning the best possible information from the current literature, meta-analysis is the best analytic strategy available (Valentine, Pigott, & Rothstein, 2010). With respect to research, a detailed quantitative summary of the current state of the literature provides critical information toward designing future clinical trials, which are costly and time-intensive. Researchers can more clearly assess the range of research designs and measures used in past studies in this area when planning their own studies and resources could be directed toward treatments with the most promising results in past studies. With respect to clinical decision-making, the results of a meta-analysis of treatment studies can help clinicians identify interventions with features associated with the most promising outcomes and inform them of the plausible range of treatment effects to anticipate in their practice. Furthermore, discussion of the limitations of the current evidence base will inform future research and help to make clinicians aware of important boundaries on current knowledge regarding the effects of CBT for adult ADHD.

In response to feedback from clinical researchers as we planned our study, we aimed to provide readers with as much information as possible about which treatments show what effect sizes for which constructs and under what circumstances (e.g., study designs), to maximize the information that could be gleaned from this growing literature. We calculated separate effect sizes for each of several constructs, including both ADHD symptoms and functioning, for self- and investigator-reported results. We used a relatively inclusive definition of CBT to represent the full range of cognitive–behavioral interventions available. Our criteria for inclusion did not impose date restrictions, and we attempted to obtain as many unpublished studies as possible to make our sample of studies more representative of the population of studies (Card, 2012; Fabiano et al., 2015). To address both internal validity and clinical utility, we calculated two sets of effect sizes. We used controlled trials to calculate effect sizes between treatment and control groups and used data from both controlled trials and open trials to calculate pre-to-post effect sizes for CBT treatment groups. Results of controlled studies allow for the strongest inferences about effects specific to each treatment, while pre-to-post analyses allow readers to evaluate the range of baseline-to-outcome effect sizes they might anticipate in clinical practice. Thus, our analyses were designed to address the needs of psychologists in both their scientist and practitioner roles.

In addition to summarizing the literature by calculating overall effect sizes, we also assessed possible moderators of treatment effect size, including study design features and treatment characteristics. For controlled trials, we tested whether heterogeneity in effect sizes could be attributed to two key clinical trial design features: type of control condition and use of randomization. Control condition type is important to assess because active control conditions, such as attention-matched comparison treatments, would be expected to produce change over time because of nonspecific factors (e.g., social support of a therapist) whereas time-matched-only conditions, such as waitlist control, might be associated with smaller changes over time. Therefore, all other things being equal, a greater treatment effect size compared with control would be predicted in studies that used a nonactive control condition. Studies that lack randomization procedures may be associated with different effect sizes because of the possibility of systematic biases in group assignment. For example, a study that allowed patients to choose whether to receive the experimental intervention might show stronger effect sizes because of participant self-selection.

Among pre-to-post treatment measurements of change, where we had the largest sample size of studies and therefore the most power, we examined moderators related to treatment characteristics including treatment type (e.g., skills-based treatment vs. mindfulness meditation training), treatment format (e.g., group vs. individual), length of treatment, and the medication status of participants at the time of participation. For medication status, one plausible hypothesis is that psychosocial approaches may be more effective when participants are also taking medications. Although the idea is intuitively appealing, studies that have empirically examined this question have not found that prepsychotherapy medication status moderated the effects of psychosocial interventions (see Knouse & Safren, 2014, for a review). Given the clinical importance of this question, we examined medication status as a moderator in this meta-analysis. Likewise, we included treatment content, format, and length as possible moderators of effect sizes so that both researchers and clinicians can assess which treatment features may be emerging as the most efficacious.

To address our research questions, we conducted a systematic literature search and meta-analysis in order to summarize current findings about the magnitude of the acute effects of cognitive–behavioral treatment for adult ADHD on symptoms and functioning. We examined controlled studies and pre-to-post effects to maximize both internal validity and clinical utility. We then examined study design features and treatment characteristics as moderators of treatment effect.

Method

Study Inclusion and Exclusion Criteria

Studies were *included* if they met the following criteria: (a) participants age 18 and older; (b) participants met criteria for *DSM–III–R*, *DSM–IV* or *DSM–5* ADHD as determined by interview with study staff *or* using a standardized, normed rating scale incorporating *DSM* criteria; (c) study tested a cognitive or behavioral psychosocial treatment[1] (i.e., purports to change a cognitive or behavioral process); (d) treatment goal was to reduce ADHD symptoms or related impairment; (e) symptoms or related impairment measured in the study, new data reported, outcomes not confined to laboratory tasks; (f) outcomes measured at pre- and posttreatment. We included controlled and uncontrolled studies but meta-analyzed controlled and uncontrolled (pre-to-post) effect sizes separately. We included controlled studies that did not use random assignment and tested this feature as a moderator. Case studies were not included in this meta-analysis. We did not restrict studies based on date of completion or publication.

To isolate the effects of CBT, studies were *excluded* if participants received new medication as part of the study. We also excluded data from a study that had a nonmedication treatment + pill placebo group (but no pill placebo control group) because of the possible influence of the placebo effect (Weiss, Hechtman, & the Adult ADHD Research Group, 2006; Weiss et al., 2012). In another study, we included data from a nonmedication treatment + pill placebo group because an appropriate comparison group was available ("control" nonmedication treatment + pill placebo; Philipsen et al., 2015). However, we only included these data in controlled trial effect size calculations because of possible impact of the placebo effect on pre-to-post results. Because study staff were only fluent in English, studies were *excluded* if English versions were not accessible.

Search Strategy

Database searches. In June 2015, we completed systematic searches of PSYCInfo, PubMed, Dissertation Abstracts International, and Google Scholar. We searched the full text of database entries using multiple combinations of search terms (see online supplemental materials). For the first three databases, we reviewed

[1] We initially included cognitive training and neurofeedback in our literature search. We located only three eligible studies of cognitive training and two of neurofeedback training and therefore focus this meta-analysis on CBT only.

titles and abstracts of all hits. Because the purpose of including Google Scholar searches was to identify very recent studies not yet indexed in the other databases, and because of the nature of Google Scholar results (i.e., thousands of results returned with relevance becoming rapidly more tenuous further down the list) we reviewed only the first 100 Google Scholar hits, sorted by relevance, for each combination of search terms.

When we became aware of work published subsequent to our searches in June 2015, these studies were considered for inclusion. Finally, to ensure maximum inclusion of relevant work, we conducted a final series of Google Scholar searches in late December 2015 focusing on newly published work since June 2015. We used every combination of initial search terms, sorted results by recency, and reviewed any new hits.

Other search strategies. To obtain unpublished studies (Card, 2012), we queried members of the ADHD Special Interest Group within the Association for Behavioral and Cognitive Therapies (ABCT) via e-mail and corresponded with authors to obtain sufficient data for evaluation and coding. We also contacted authors of conference presentations cited in the literature. We reviewed programs from the International Society for Research in Child and Adolescent Psychopathology that were accessible to us (two most recent meetings) and unsuccessfully attempted to obtain abstracts from past meetings of ABCT. As we coded studies that met inclusion criteria, we reviewed their introductions for citations relevant to our search and reviewed the reference lists of relevant literature reviews. We obtained feedback on the completeness of our list of eligible studies from three experts representing psychosocial treatment of ADHD across the life span. One suggested searches based on alternative ADHD terminology, but repeating searches in PubMed using, "minimal brain dysfunction," "residual," "hyperkinetic," and "attention deficit disorder," yielded no additional eligible studies.

Determination of Study Eligibility

We obtained full text of studies that appeared to meet our criteria based on initial review of the title and abstract. For unpublished studies without full manuscripts, we obtained information from study authors to determine eligibility. Studies were then evaluated with respect to inclusion and exclusion criteria (see Figure 1 for reasons for exclusion). For studies obtained during the June 2015 literature search, each study was first evaluated by either the second or third author using a checklist form with detailed operational definitions of each criterion. Next, the first author reviewed the manuscript and the checklist and made the final determination for inclusion and discussed her determination with the initial judge. The first author is a PhD-level clinical researcher with extensive experience reviewing the literature on psychosocial treatments for adult ADHD. The second and third authors are undergraduate research assistants trained by the first author during a summer research fellowship (see descriptions of training procedures, below). For studies obtained after June 2015, the first author and either the second or third author independently completed the evaluation form for each study and then met to discuss their determinations (90% agreement for these studies; Cohen's *kappa* = .78). Discrepancies were resolved by examining the criterion in question and making a determination jointly.

Study Coding

Moderator codes. The following moderator codes were applied to eligible studies. See online supplemental materials for detailed code definitions and coding results for each study (online supplemental Table S1a and S2a). *Randomization* (controlled studies only; 100% agreement; Cohen's *kappa* = 1.0) was coded *Yes*, *No*, or *Study does not indicate*. *Control group type* (controlled studies only; 89% agreement; Cohen's *kappa* = .76) was coded *Time-matched only (not active)* or Attention-matched (active). *ADHD Medication status* (91% agreement; Cohen's *kappa* = .81) was coded All on medication, Mixed, or Unmedicated. *Treatment type* (83% agreement; Cohen's *kappa* = .64), that is, treatment content, regardless of format, was coded Skills-Based Training, Skills-Based Training with Mindfulness, Mindfulness Meditation Training, or Other. *Treatment format* (81% agreement; Cohen's *kappa* = .71) was coded Individual only, Group only, Individual + group, Group + Support Contact, Internet platform, or Other. *Treatment length in weeks* (83% agreement; Cohen's *kappa* = .77) was number of weeks participants engaged in treatment from baseline to the first posttreatment assessment. *Number of treatment sessions* (86% agreement; Cohen's *kappa* = .84) was total number of face-to-face sessions participants received or, for Internet platform treatments, number of online modules participants could access.

Procedures for moderator coding. Coder training involved collaborative development of moderator codes followed by practice coding of several manuscripts and refinement of the coding scheme until reasonable convergence was achieved. Studies were independently coded by the first author and the third author and interrater agreement statistics were calculated. Coders reviewed disagreements and collaboratively decided upon the most appropriate code.

Procedures for effect size coding. Statistics for effect size calculations were extracted independently by the first author and the second author. Coders first engaged in extensive practice coding of several manuscripts, after which they discussed results and addressed sources of inconsistency until reasonable convergence was achieved in practice coding. Next, the first and second author independently extracted statistics for each study and these were double-entered into the Comprehensive Meta-Analysis Program. Coders collaboratively identified and reviewed each disagreement and corrected any errors. If required statistics were not available in published manuscripts or in unpublished materials, we contacted the authors to obtain the necessary information. In all but two cases, authors provided the requested information.

Treatment Outcome Measures

We calculated effect sizes separately by measure type and reporter. First, we tallied the number of studies yielding effect sizes for each type of measure (inattentive symptoms, hyperactive-impulsive symptoms, ADHD symptoms, clinical global impression, functioning or quality of life, executive functioning, etc.) and reporter (self-report, other-report, blinded assessor report, unblinded assessor report; online supplemental Tables S1b and S2b). We calculated effect sizes for measures/reporters when there were five or more effect sizes available for that measure/reporter, resulting in controlled trial effect size estimates for the following self-report measures: inattentive symptoms, hyperactive-impulsive

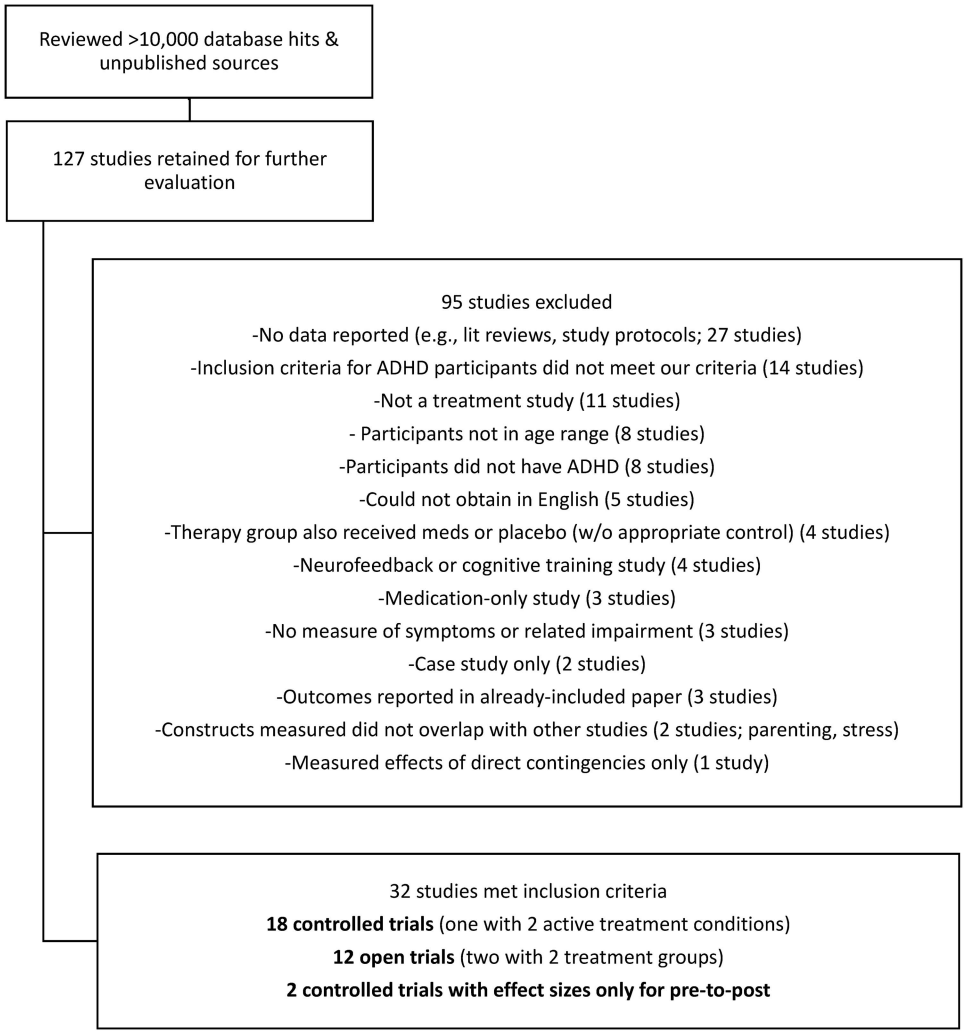

Figure 1. Flowchart of systematic literature search. Note that results from database searches were not mutually exclusive.

symptoms, ADHD symptoms, and global functional impairment or quality of life. Pre-to-post effect sizes were calculated for all of these and for self-reported executive functioning. Controlled trial effect size estimates were calculated for blinded assessor ADHD symptoms and clinical global impressions (CGI) severity scores. For pre-to-post effect sizes, effect sizes for assessor-rated ADHD symptoms and CGI were also calculated.

Statistical Methods

We used Comprehensive Meta-Analysis Version 3 to conduct all statistical analyses and to generate data plots. For each study and measure listed previously, we calculated Hedges *g* and 95% confidence intervals (CIs) using means (*M*s), *SD*, and sample sizes (*N*) in each group. We selected Hedges' *g* because several studies had small sample sizes and Cohen's *d* can overestimate the standardized mean difference in small samples. Hedges' *g* is calculated by multiplying *d* by a correction factor (*J*) to remove this bias (Borenstein, Hedges, Higgins, & Rothstein, 2009, p. 27). For controlled studies, we calculated effects based on the difference between treatment and control groups at study outcome. For pre-to-post treatment change, we calculated Hedges *g* for pretreatment versus posttreatment scores using *M*, *SD*, and *N* at each time point for the treatment group only (Borenstein et al., 2009, p. 29). When a study reported more than one effect size for a measure/reporter category, the effects for each measure were averaged together to yield one effect size contributed by that study. If a study only reported inattentive and hyperactive-impulsive self-report subscales separately, we included the mean of these subscales in calculations for self-reported ADHD symptoms.

We used a random effects model to calculate Hedges *g* for each group of studies, because the true effect likely varies from study to study and the effect sizes from available studies represent a random sample of those possible effects. The random-effects model weights the effect size from each study by the inverse of its variance, including both within-studies variance and an estimate of between-studies variance (Borenstein et al., 2009). 95% CIs were calculated around each effect size estimate.

To assess the possible impact of publication bias and selective reporting, we report the standard fail-safe *N* and Orwin's fail-safe *N*—specifically, the number of studies with an effect size of zero that would bring the estimated aggregate effect size to a trivial level, in this case $g = .20$. We also examined funnel plots for each grouping of studies and used trim and fill to assess the possible impact of missing studies on the point estimate of each effect size (Duval & Tweedie, 2000). We did not make statistical adjustments to our effect size estimates based on possible data censoring. To assess the possible influence of statistical outliers, we present forest plots so that the reader can visually assess the possible influence of outliers.

To assess heterogeneity of effect sizes, we report *Q*-statistics and their accompanying *p* values as well as τ and τ^2. We also examined I^2 for evidence of true, nonerror variance among the effect size estimates across studies. We proceeded with moderator analyses when there was compelling evidence of true heterogeneity among study effect sizes not because of measurement error. For categorical moderators, we conducted random effects model *Q*-tests based on Analysis of Variance (Borenstein et al., 2009, p. 177) using pooled estimates of τ^2 across subgroups (Borenstein et al., 2009, p. 162). For continuous moderators, we used linear metaregression.

Results

We completed over 10,000 database record reviews including review of information on unpublished manuscripts and retained 127 studies for further evaluation of inclusion based on preliminary review. As shown in Figure 1, 95 of these studies were excluded, leaving 32 studies that met inclusion criteria. Of the studies included, 18 were controlled trials, one of which had two active treatment conditions in addition to a control group. Another 12 were open trials of which two had two active treatment conditions. Finally, we retained two additional controlled trials from which data were only included in our analysis of pre-to-post effect sizes. First, we used only the pre-to-post data from Hesslinger et al. (2002) because the control group received new medication treatment after the baseline assessment and attrition was high in this group (three of seven patients retained). Second, we included only the pre-to-post data from the CBT-only group of one study because the comparison group received CBT plus new medication (Cherkasova et al., 2016). For Philipsen et al. (2015) we included only the DBT + pill placebo group versus the clinical management (control) + pill placebo group in the analyses for controlled studies.

Effect Sizes: Treatment Versus Control Groups

Estimated effect sizes (Hedges' *g*) across outcome measures for treatment versus control ranged from small to medium (Table 1). 95% CIs did not include zero. Publication bias statistics indicated minimal impact of bias on the aggregate effect size estimate for most measures (Table 1). One exception is for the estimated effect size for self-reported hyperactive-impulsive symptoms, where the number of null result studies needed to bring the estimated effect size to zero was 29 and the number of null result studies needed to bring the estimated effect size to .20 was only five. In addition, trim-and-fill statistics indicated substantial reduction in effect size (from .33 to .16) after the imputation of possibly missing studies. For most measures, however, the estimated effect sizes appear robust to publication bias.

Moderator Analyses: Treatment Versus Control Groups

Heterogeneity statistics are presented in the top portion of Table S3 in the online supplemental materials. *Q*-test results indicate that, for all measures except for self-reported hyperactive-impulsive symptoms, these studies are unlikely to share a common effect size (i.e., there is significant heterogeneity among study effect sizes). Estimates of I^2 suggest that a substantial proportion of the variance among study effect sizes is real and not because of measurement error; therefore, indicators of heterogeneity justified moderator analyses to identify the source of this heterogeneity. In the case of self-reported hyperactive-impulsive symptoms, this estimate of important variance was lower ($I^2 = 14.52$) and did not justify moderator analyses for this set of effect sizes.

Randomization as a moderator. Randomization status of studies (randomized vs. nonrandomized; one study that did not report randomization status was excluded from this analysis) was tested as a moderator for measures that had heterogeneous effect sizes **and** for which there were effect sizes available from nonrandomized studies, including self-reported inattentive symptoms, self-reported ADHD symptoms, and self-reported functioning. Randomization status was only a significant moderator of effect size for self-reported functioning, where the effect size for a nonrandomized study ($g = 1.29$; $N = 1$) was larger than for randomized studies ($g = .40$; $N = 9$), $Q(1) = 4.96$, $p = .026$. Otherwise, study randomization status was not a significant moderator of effect size.

Table 1
Effect Sizes and Publication Bias Statistics: Treatment Versus Control

Outcome measure	Hedges' *g*	95% CI	*k*	Fail safe *N*	Orwin's fail safe *N*	*k* studies trimmed	*g* after trim and fill
Self-report: Inattentive	.77	[.48–1.07]	11	188	23	4	.56
Self-report: Hyperactive-impulsive	.33	[.13–.53]	9	29	5	5	.16
Self-report: ADHD symptoms	.65	[.44–.86]	19	376	35	5	.47
Blind assessor: ADHD symptoms	.57	[.25–.89]	6	41	8	2	.42
Blind assessor: CGI	.51	[.13–.88]	5	19	5	1	.44
Self-report: Functioning	.51	[.23–.79]	10	54	14	0	n/a

Note. CI = confidence interval; *k* = number of studies contributing to each effect size; ADHD = attention-deficit/hyperactivity disorder; CGI = Clinical Global Impressions. For fail safe *N*, $p = .05$ for a 2-tailed test. Orwin's fail safe *N* represents the number of studies with effect size of zero that would bring the overall estimate to a trivial level, specified by the authors as $g = .20$.

Table 2
Moderator Analysis for Control Group Type

Outcome measure	*Q*	*p*	Nonactive controls *g* (*SE*)	*k*	Active controls *g* (*SE*)	*k*
Self: Inattentive	14.36	<.001	.95 (.12)	8	.26 (.14)	3
Self: ADHD	5.31	.021	.79 (.12)	14	.35 (.16)	5
Blind assessor: ADHD	9.01	.003	.78 (.15)	4	.20 (.13)	2
Blind assessor: CGI	9.35	.002	.80 (.18)	3	.13 (.13)	2
Self: Functioning	.25	.619	.48 (.17)	8	.67 (.34)	2

Note. ADHD = attention-deficit/hyperactivity disorder; CGI = Clinical Global Impressions Scale.

Control group type as a moderator. Control group type (Not Active vs. Active; see Method for definitions) was a significant moderator of treatment effect size for all measures except for self-reported functioning (Table 2). Thus, across self-reported and blinded-investigator rated measures of ADHD symptoms, studies in which the treatment was compared with an active control group showed significantly smaller effect sizes (g = 0.13–0.67) than studies in which treatment was compared with a control group that was not active (g = 0.48–0.95). Figure 2 presents the forest plot of effect sizes for self-reported ADHD symptoms separated by control group type and Figure 3 presents this analysis for blinded assessor-rated ADHD symptoms.

Because moderator analyses based on control group type indicated that the results from studies with active control groups should not be compared with other designs (e.g., waitlist control) and because partitioning studies by control group type reduced the overall *N* of studies, we did not proceed with analyses of treatment features as moderators. Separating studies by control group type and again by treatment features yielded very small numbers of studies in each cell, increasing concerns about power for moderator analyses. Thus, we conducted moderator analyses of treatment features within the pre-to-post data, since cell sizes were larger.

Effect Sizes: Pre-to-Post Treatment

Estimated pre- to posttreatment effect sizes (Hedges' g) for psychosocial treatments across outcomes ranged from medium to very large and 95% CIs did not include zero (Table 3). A forest plot for self-reported ADHD symptoms appears in Figure 4 and plots illustrating results for inattentive and hyperactive-impulsive symptoms and self-reported functioning appear in the online supplemental materials. Publication bias statistics suggested minimal impact on the effect size estimates (Table 3), with trim-and-fill indicating minimal reduction in adjusted effect size. Thus, the estimated effect sizes are likely robust to publication bias.

Moderator Analyses: Pre-to-Post Treatment

Statistical indicators of heterogeneity among effect sizes within each group of measures are presented in the bottom half of Table S3. Q-test results and estimates of I^2 indicated significant heterogeneity among study effect sizes for three of the outcome measures, including self-reported inattentive symptoms, self-reported ADHD symptoms, and self-reported impairment/functioning, justifying moderator analyses. Of note, study design (controlled vs. open trial) was a not significant moderator for any of the pre-to-post effect sizes, suggesting that our decision to combine results from these study designs was justified.

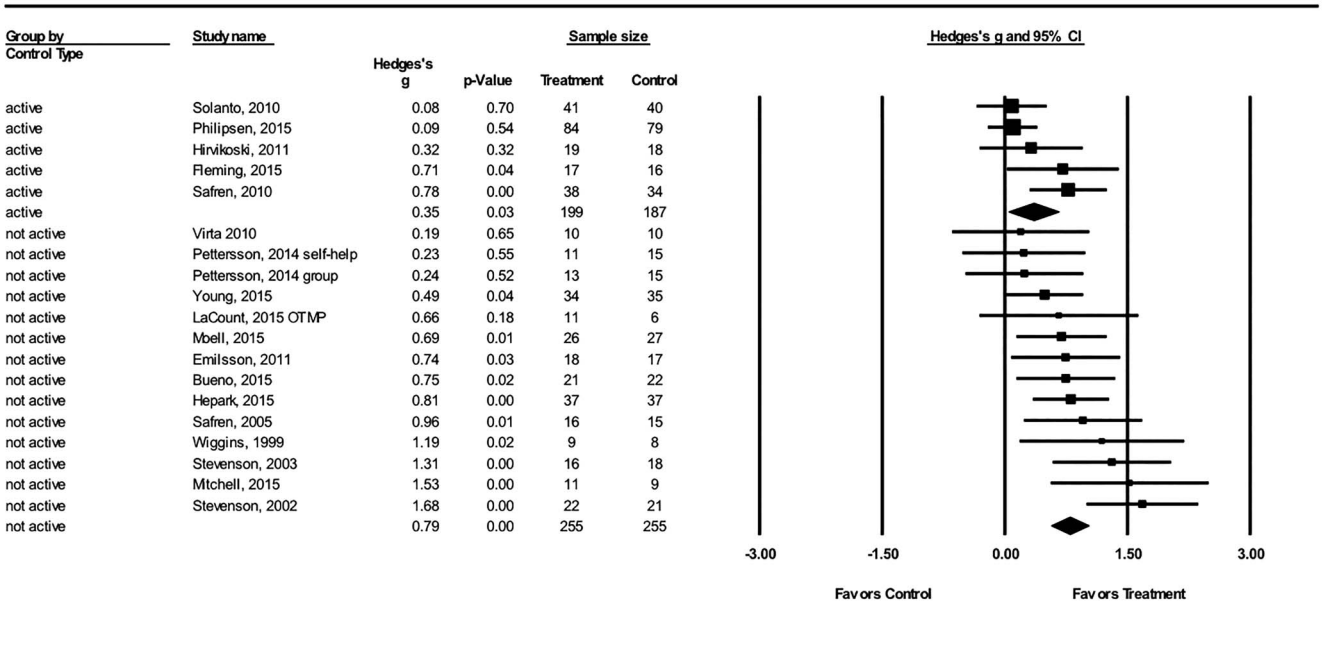

Figure 2. Forest plot for effect sizes (Hedges's g) for self-reported attention-deficit/hyperactivity disorder (ADHD) symptoms grouped by control group type (active vs. not active).

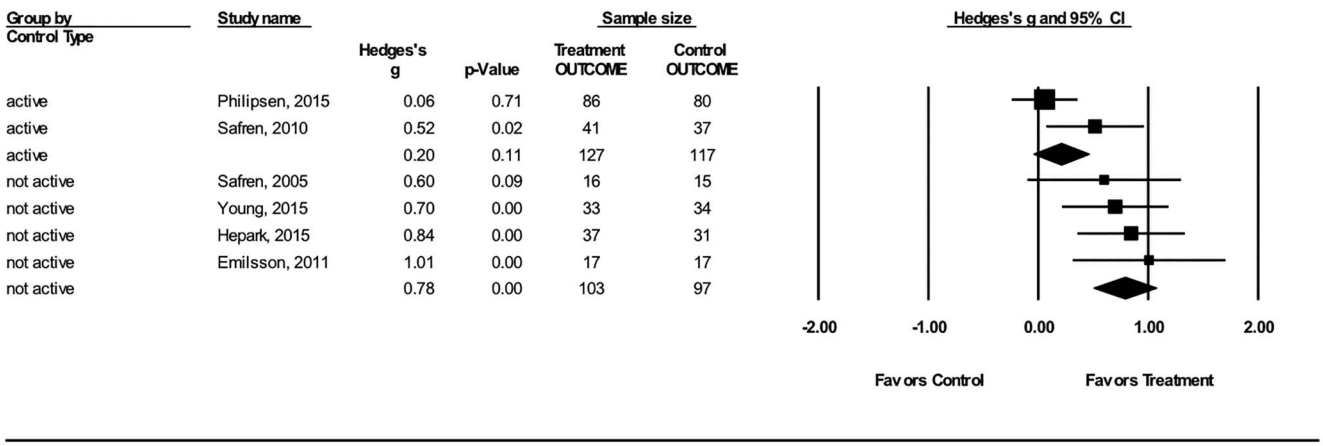

Figure 3. Effect sizes (Hedges's *g*) for blinded assessor-rated attention-deficit/hyperactivity disorder (ADHD) symptoms grouped by control group type (active vs. not active).

Because analyses of treatment feature moderators required separating studies into up to four subgroups, we conducted moderator analyses for the outcome measure that encompassed the largest number of studies and would therefore yield maximum statistical power—self-reported ADHD symptoms (Table 4). Results for two other outcome measures (self-reported inattentive symptoms, functioning) were, for the most part, comparable and are presented in the online supplemental materials.

For self-reported ADHD symptoms, medication status of study participants approached significance as a moderator of treatment effect size ($p = .11$; Table 4). Three studies treating unmedicated participants ($g = 1.33$) and six studies treating only medicated participants ($g = 1.20$) showed larger effect sizes than 22 studies treating mixed medicated and unmedicated participants ($g = .89$). In Table 4, neither treatment type nor treatment format significantly predicted effect size. However, given the possible outlier status of the results from Hesslinger et al. (2002; Figure 4), we ran the moderator analysis of treatment type excluding this small ($N = 8$) but influential ($g = 2.53$) study. We found a marginally significant moderation effect ($p = .07$) of treatment type on ADHD symptom effect size with mindfulness meditation ($g = 1.06$) and skills-based treatment without mindfulness ($g = 1.08$) showing larger effect sizes than DBT-based treatments including both skills and mindfulness ($g = .64$). Finally, neither treatment length nor number of sessions predicted effect sizes for self-reported ADHD symptoms. In other words, longer treatments were not associated with larger effects on symptoms.

Discussion

Skills-based psychosocial treatments targeting cognitive and behavioral processes for adult ADHD had medium-to-large effects on ADHD symptoms from pre- to posttreatment (e.g., $g = 1.00$ for self-reported ADHD symptoms), with controlled studies showing small-to-medium effects ($g = .65$). These results are likely robust to impact of publication bias. For effect size estimates for nearly all constructs, however, substantial heterogeneity among effects paints a more complex picture.

As in other treatment study meta-analyses (e.g., Olatunji, Davis, Powers, & Smits, 2013), effect sizes compared with active, "placebo" control conditions were smaller than effect sizes compared with nonactive, waitlist or continued medication control groups. Effects for self-reported ADHD symptoms in studies with active control groups were significantly different from zero (Figure 2),

Table 3
Effect Sizes and Publication Bias Statistics: Pre- to Posttreatment

Outcome measure	Observed Hedges' *g*	95% CI	*k*	Fail safe *N*	Orwin's fail safe *N*	*k* studies trimmed	*g* after trim and fill
Self-report: Inattentive	1.16	[.94–1.38]	20	1120	91	3	1.07
Self-report: Hyperactive-impulsive	.68	[.48–.87]	18	329	41	2	.61
Self-report: ADHD symptoms	1.00	[.84–1.16]	31	2231	119	2	.97
Assessor: ADHD symptoms	1.40	[1.10–1.71]	7	224	41	0	n/a
Assessor: CGI	1.12	[.79–1.43]	7	134	33	0	n/a
Self-report: Functioning	.73	[.46–1.00]	17	384	35	0	n/a
Self-report: Executive functioning	.99	[.61–1.36]	6	81	21	3	.75

Note. CI = confidence interval; *k* = number of comparisons contributing to each effect size; ADHD = attention-deficit/hyperactivity disorder; CGI = Clinical Global Impressions. For fail safe *N*, $p = .05$ for a 2-tailed test. Orwin's fail safe *N* represents the number of studies with effect size of zero that would bring the overall estimate to a trivial level, specified by the authors as $g = .20$.

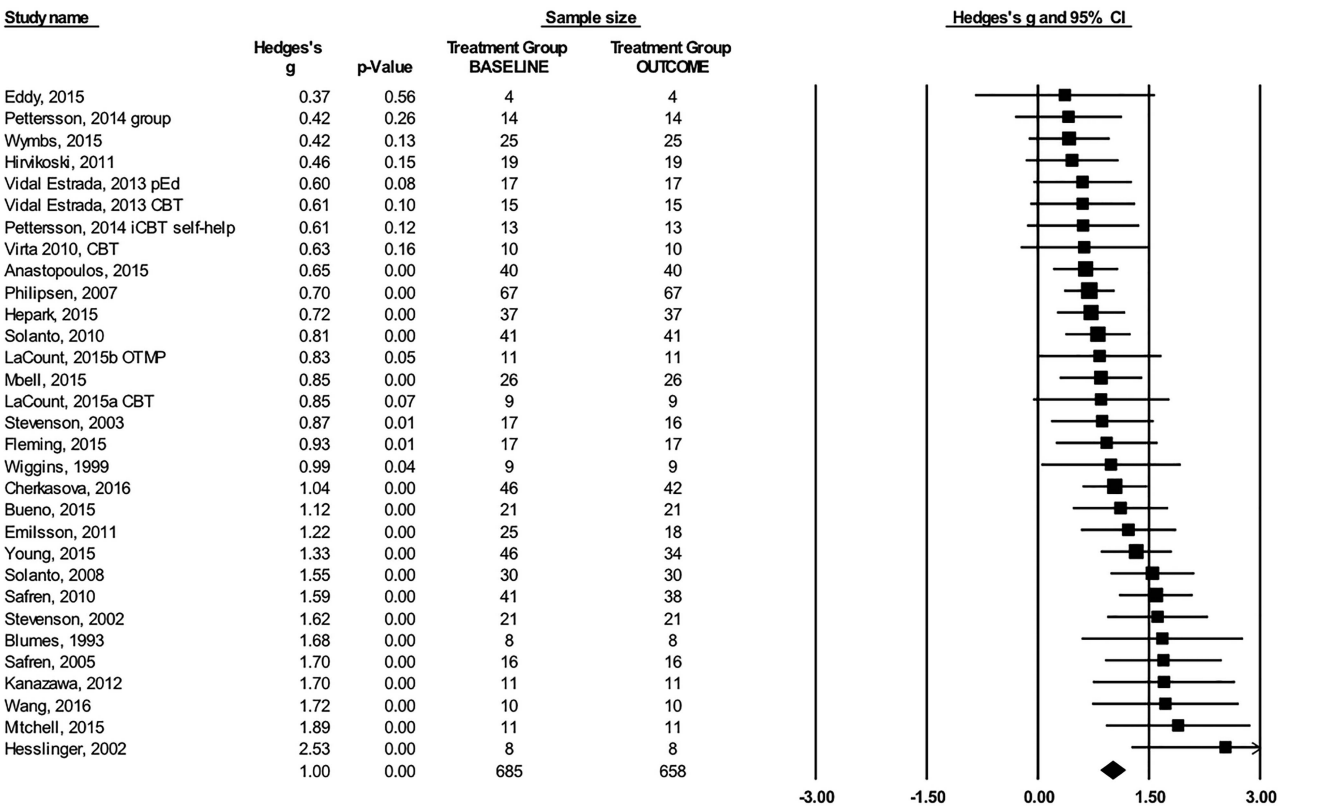

Figure 4. Effect sizes (Hedges's *g*) for self-reported attention-deficit/hyperactivity disorder (ADHD) symptoms from pre- to posttreatment.

but only two studies used attention-matched control groups and reported blinded assessor ratings of total ADHD symptoms. Thus, additional tightly controlled studies of CBT for adult ADHD are needed. In addition, future meta-analyses should continue to consider control group type as moderator.

Before discussing additional results from moderator analyses, we offer some words of caution regarding the interpretation of these data. First, associations between moderators and effect sizes are observational and not experimental so that moderator variables may be confounded with one another. Second, the current study may be underpowered to detect all effects associated with moderators. Although we addressed this issue by conducting treatment feature moderator analyses within the largest group of studies (pre-to-post treatment self-reported ADHD symptoms), the reader should keep in mind that failure to find a significant effect does not indicate that no effect exists (Borenstein et al., 2009).

Table 4
Moderator Analysis for Pre-to-Post Self-Reported Attention-Deficit/Hyperactivity Disorder (ADHD) Symptoms: Treatment Features

Moderator	*Q*	*p*	*g* (*SE*, *k*)	*g* (*SE*, *k*)	*g* (*SE*, *k*)	*g* (*SE*, *k*)
Medication status	4.41	.11	MED 1.20 (.17, 6)	MIX .89 (.09, 22)	UMD 1.33 (.26, 3)	
Treatment type	2.47	.29	MMT 1.08 (.25, 3)	SBT 1.09 (.10, 21)	SBT + M .76 (.13, 6)	
Treatment format	4.57	.21	GRP .91 (.12, 14)	GRP + SUP 1.01 (.12, 8)	IND 1.38 (.21, 6)	INT .75 (.30, 2)
	Coeff.	*SE*	*LL*	*UL*	*p*	*k*
Treatment length	.01	.03	−.05	.07	.37	31
Number of sessions	.01	.02	−.02	.04	.35	31

Note. MED = all on medication; MIX = mixed medicated and unmedicated; UMD = unmedicated; MMT = mindfulness meditation training; SBT = skills-based treatment, no mindfulness; SBT + M = skills-based training with mindfulness (DBT); GRP = group treatment; GRP + SUP = group treatment plus support contact; IND = individual treatment; INT = Internet-based treatment.

Within pre-to-post effect size estimates, CBT conducted with participants already taking medication was not necessarily associated with larger therapy effects. There was a trend toward larger effects for studies examining exclusively medicated or unmedicated participants, perhaps suggesting that treatments tailored to the medication status of clients may be more efficacious. In our analysis, longer treatments were not associated with larger effect sizes and treatment format (group, group with supportive contacts, individual, Internet) was not significantly associated with effect size among the available studies—although it should be noted that there were fewer studies of Internet-based interventions available for analysis ($N = 2$). For treatment type, effect sizes for mindfulness meditation interventions and self-regulation skills based treatments were comparable. Our results suggest that treatments based on the DBT skills training model first tested by Hesslinger et al. (2002), which includes both self-regulation skills and mindfulness, may not be associated with effects as large as other treatment types. However, this result should be interpreted with caution given that the result did not reach statistical significance, the relatively small number of studies, the challenge in grouping treatments into meaningful categories, and the small number of studies using the same treatment manual.

Taking a broader view of the results of this meta-analysis reveals some other interesting patterns. First, for both controlled trials and pre-to-post effect sizes, treatments appear to have larger effects on inattentive symptoms than hyperactive-impulsive symptoms.[2] Importantly, some CBTs are specifically designed to target inattentive symptoms of the disorder (e.g., Solanto et al., 2010) and inattentive symptoms may tend to be more persistent and problematic in adulthood than hyperactive-impulsive symptoms (Barkley et al., 2008; Stavro, Ettenhofer, & Nigg, 2007). Yet clinical researchers may need to develop treatment elements that more effectively target hyperactive-impulsive symptoms for clients for whom these symptoms remain problematic. Second, where data were available, effects on assessor-rated ADHD symptoms did not appear to be substantially smaller than self-reported symptoms. Of course, assessor ratings are often based upon the client's self-report. Unfortunately, few studies reported alternative methods of measurement (e.g., collateral report). Finally, effect sizes on measures of inattentive symptoms appeared to be larger than on measures of functional impairment/quality of life. However, this result should also be interpreted with caution because fewer studies reported nonsymptom measures. Yet these results suggest that current treatments may have a larger effect on the proximal treatment target of inattention problems in daily life than on "downstream" measures such as functional impairment.

It seems reasonable to compare our results to results obtained in meta-analyses of medication treatment for adult ADHD. Making such a comparison accurately, however, is more difficult than it may first appear. Randomized, blinded, placebo-controlled investigations of medication for adult ADHD are much more plentiful than the analogue design in psychotherapy research—comparison to an active, attention-matched control condition with ratings by a blinded assessor. As mentioned previously, only two psychosocial treatment studies in our meta-analysis reporting a total ADHD symptom score had these characteristics—those by Philipsen et al. (2015) and Safren et al. (2010) displayed in Figure 3. The effect size across these studies was $g = 0.20$ with only Safren showing a nonzero effect ($g = .52$). By way of comparison, Cunill, Castells, Tobias, and Capellà (2016) recently found a standardized mean difference of 0.45 for randomized controlled trials of medication for ADHD using primarily clinician ratings of symptoms, with stronger effects for stimulant medications. Thus, there are simply not enough relevant data to draw firm conclusions about the relative effects of CBT versus medication for adult ADHD at this time.

It also seems reasonable to compare our results to those of nonmedication treatments for ADHD in children. Fabiano et al. (2009) conducted a meta-analysis of treatments for ADHD considered to be "primarily behavioral in nature" (p. 131). They reported a mean unweighted between-groups effect size of 0.39 for parent ratings of ADHD symptoms among 11 studies and 0.79 for teacher ratings of ADHD symptoms among 8 studies compared with no-treatment control. These effect sizes for children are comparable to the aggregate effect sizes in controlled trials in our meta-analysis of $g = 0.65$ for self-rated ADHD symptoms and 0.57 for blinded assessor-rated symptoms (Table 1), although effect sizes on impairment measures in child studies may be slightly stronger (0.84 for parent ratings and 0.55 for teacher ratings of impairment in Fabiano et al., 2009 compared with $g = 0.51$ for self-rated impairment in our meta-analysis). For pre-post designs not including the results of controlled studies, Fabiano et al. (2009) obtained unweighted effect sizes of 0.90 for parent-rated symptoms in 21 studies and 0.79 for teacher-rated symptoms in 12 studies. These effect sizes in children appear somewhat smaller than pre-to-post effects in our meta-analysis of 1.00 for self-rated ADHD symptoms and 1.40 for assessor-rated symptoms (Table 3), but results were comparable between child and adult studies for pre-to-post ratings of impairment (0.74 for parent ratings and 0.78 for teacher ratings of impairment in Fabiano et al., 2009, compared with $g = 0.73$ for self-rated impairment in our study).

In a more recent analysis, Sonuga-Barke et al. (2013) included only randomized, controlled trials of behavioral interventions for childhood ADHD and obtained an overall standardized mean difference of 0.40 for parent- or teacher-rated ADHD symptoms—again, comparable to or even slightly less favorable than our result of $g = 0.65$ for self-rated ADHD symptoms and 0.57 for blinded assessor-rated symptoms in controlled trials. While caution should be exercised in comparing results across child and adult studies, particularly for results based on adult self-report, our meta-analysis suggests that CBTs for adults with ADHD may be at least as efficacious as behavioral interventions for children with the disorder.

The findings of this meta-analysis must be interpreted in light of the limitations of this analysis and the literature upon which the analysis is based. First, as mentioned previously, findings in moderator analyses are observational and not experimental so that associations between factors such as treatment type, format, length and so forth may be confounded with other important factors. Second, while the size of this literature is growing, it is still relatively small compared with research on other adult disorders. Smaller sample size of participants and studies limits statistical power—especially for moderator analyses. We attempted to ad-

[2] The possibility of restriction of range should be considered as well. Baseline scores for inattentive symptoms ($M = 24.49$, $SD = 4.51$) tended to be higher than for hyperactive-impulsive symptoms ($M = 18.55$, $SD = 5.78$) in studies where both symptom subscale scores were available.

dress this limitation by using the measure and research design with the most studies in our moderator analyses; however, as mentioned previously, it is important to recognize that given limited power, a nonsignificant result indicates that no effect was detected, and not that one does not exist. Third, heterogeneity in the studies included in this meta-analysis complicates interpretation of the results. We attempted to code cognitive–behavioral intervention types in a meaningful way but it is likely that there is still significant heterogeneity among the treatments we coded as "skills-based training." As a result, our analysis cannot capture the possible impact of more subtle differences between intervention types. Additional treatment study replications using the same manual would help to clarify many of the promising effects observed in single studies. Fourth, because only 10 of the included studies reported data for follow-up time points, we only examined treatment effects at acute outcome and therefore no conclusions can be drawn regarding the durability of the effects of cognitive–behavioral interventions for adult ADHD from the current analysis. Fifth, the studies included in this analysis provide evidence regarding efficacy of interventions in clinical research settings and cannot speak to the extent to which findings would generalize to clinical practice settings in the community. Finally, the absence of a substantial number of treatment studies using active, attention-matched control groups and blinded assessors limits the conclusions we can draw regarding the effects of cognitive–behavioral treatment for adult ADHD above and beyond nonspecific effects of treatment.

Our results provide reason for cautious optimism among clinicians and their adult clients with ADHD regarding cognitive–behavioral interventions. If the cognitive–behavioral treatments reviewed here maintain their efficacy outside of clinical research settings, clinicians might anticipate that, on average, clients who complete treatment may show self-reported symptom reduction of about one standard deviation from pre- to posttreatment. Second, our data indicate that clinicians and clients have some comparably "good choices" among cognitive–behavioral treatments. More than one cognitive–behavioral treatment was associated with larger effects and our moderator analyses did not provide evidence to suggest that one treatment format (e.g., group vs. individual) was associated with greater efficacy, although caution might be exercised with regard to Internet-based treatments because of fewer available studies of this format. The available evidence also suggests that shorter-term treatments can be associated with comparable acute effects in adults with ADHD compared with longer ones. However, the range of effect sizes, such as those illustrated in Figure 4, do suggest that some treatment packages may be more efficacious than others and clinicians should carefully review the evidence for each treatment when making decisions about which approaches to use. Although our analysis, and meta-analysis in general, has limitations, we hope that our results will be useful to clinicians wishing to use cognitive–behavioral interventions for adult ADHD.

The results of this meta-analysis and our experience conducting it support several recommendations for clinical researchers in their future investigations. First, we agree with Fabiano et al. (2015) in their call for more standardized and uniform assessment protocols for studies of ADHD treatment outcome. The diversity of measurement instruments available complicates the interpretation of results across treatment studies. If researchers could agree upon a set of "gold standard" measures of ADHD symptoms, comorbid symptoms, and functional impairment—ideally including self-report, blinded assessor-rated, and collateral report—it would be easier to compare and contrast results from studies of different treatments. Second, we recommend that clinical researchers consistently measure and report outcomes related to functional impairment—globally and with respect to key domain in which adults with ADHD are known to experience impairment such as managing daily responsibilities, fulfilling duties at work, and relating to family and friends. Rating scales that tap multiple domains may be useful (e.g., Weiss Functional Impairment Rating Scale; Canu, Hartung, Stevens, & Lefler, 2016; Weiss, 2000), as well as measures of functioning in specific domains. Third, we recommend that study authors clearly and carefully report key statistics needed for meta-analytic comparisons including sample size, unweighted means, and standard deviations for each measure in their studies, for each group, at each time-point. In particular, researchers should clearly report the number of participants assigned to treatment, the number who started and completed treatment, and the number of participants whose data were analyzed. Researchers should consider following the Consolidated Standards of Reporting Trials guidelines (CONSORT; www.consort-statement.org) when writing up their results—even, to the extent possible, for studies that are not randomized controlled trials. Fourth, we encourage study authors to clearly describe the major features of their cognitive–behavioral treatments. If these details cannot be included in the manuscript because of space constraints, we recommend that authors provide easy access to supplementary information about the intervention. Fifth, as mentioned previously, we encourage different research groups to conduct replications of treatment studies using the same treatment manuals to ensure that treatment effects obtained by one research group generalize to other settings, as described in the criteria for Empirically Validated Treatments developed by Division 12 of the American Psychological Association (Chambless & Ollendick, 2001).

Finally, we suggest that the field might benefit from shifting emphasis from entirely de novo cognitive–behavioral interventions for adult ADHD to identifying the processes that account for treatment-related change (mediators) and the client characteristics that predict response to treatment (moderators) so that interventions can be tailored and targeted. In particular, identifying empirically supported principles (ESPs) of change in CBT for adults with ADHD might aid dissemination and adaptation of treatments to the needs of clients (Abramowitz, 2006; Deacon, 2013). We hope that our findings contribute to the next phase of treatment development for adults with ADHD, which may benefit from increased collaboration and conversation among researchers in the field as well as careful consideration of the strengths and limitations of existing treatments.

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