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Treatment features associated with youth cognitive behavioral therapy follow-up effects for internalizing disorders: A meta-analysis

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Abstract

Objective—Our aim was to investigate whether four treatment features (i.e., the inclusion of parental involvement, goal setting strategies, maintenance/relapse prevention sessions, the addition of booster sessions) were associated with post-treatment and follow-up effect size of youth Cognitive Behavioral Therapies (yCBTs) for anxiety, depression, post-traumatic stress disorder, and obsessive-compulsive disorder in age groups spanning young children to adolescents.

Method—We conducted a random-effects meta-analysis of 106 yCBTs tested in 76 randomized clinical trials from the PracticeWise Database to examine average effects of yCBTs post-treatment and at a later follow-up assessment. We coded the use of parental involvement, goal setting, booster sessions, and maintenance/relapse prevention in each yCBT and conducted random-effects meta-regression analyses to investigate whether these treatment features were associated with yCBT effects at post-treatment as well as at follow-up.

Results—Overall, yCBTs produced large pre- to post-treatment effects ($d = 1.05$, *95% CI* = [0.94, 1.15]) and larger pre- to follow-up effects ($d = 1.29$, *95% CI* = [1.18, 1.40]). Meta-regression results indicated that parental involvement was significantly associated with larger pre- to post-treatment effect sizes as well as pre- to follow-up effect sizes. Booster sessions, goal-setting, and maintenance/relapse prevention were not significantly related to effect sizes at post-treatment or follow-up.

Conclusion—Parental involvement may be helpful for maximizing long term effectiveness of yCBT. Future studies should investigate for whom and under what conditions inclusion of yCBT treatment features is related to the durability of treatment gains.

Keywords

youth CBT; meta-analysis; treatment features; parental involvement; follow-up

Great strides have been made toward building an evidence base for the treatment of youth internalizing disorders such as anxiety, depression, post-traumatic stress disorder, and obsessive compulsive disorder (David-Ferdon & Kaslow, 2008; Higa-McMillan, Francis,

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Rith-Najarian, & Chorpita, 2016; Silverman, Pina, & Viswesvaran, 2008), which is important because these disorders are the most prevalent youth mental health problems (Merikangas et al., 2010). Advances have been possible, in part, because of the hundreds of randomized controlled trials (RCTs) of youth cognitive behavioral therapies (yCBTs) (Chu, 2012; Clark, 2009; Mansell, Harvey, Watkins, & Shafran, 2009). Although RCTs examine effects of yCBTs at the termination of treatment (herein "post-treatment"), not all studies evaluate the effects of treatment at follow-up (i.e., after the post-treatment assessment). When assessed at post-treatment, yCBTs have in general demonstrated significant effects (Chu & Harrison, 2007), and these treatments have been recognized accordingly as "evidence-based" (Chorpita et al., 2011). Notably, some RCTs report significant effects of yCBT at follow-up (e.g., Barrett, 1998; Chen et al., 2014; Jensen, Holt, & Ormhaug, 2017), whereas others report nonsignificant findings at follow-up (Rith-Najarian et al., 2017). Thus, it is important to investigate whether treatment features (i.e., structural aspects of the intervention design that are decided upon before treatment is delivered) are associated with effects of yCBTs at follow-up. We differentiate treatment features here from treatment characteristics, which are descriptive factors such as for whom and under what conditions treatment is delivered (e.g., the target of treatment, inclusion of comorbid youths, and outcome informant), that cannot be modified once the treatment has begun. Such an investigation may advance our understanding of why some yCBTs produce follow-up effects whereas others do not.

Our focus here was toward treatment features that can be intentionally included into any yCBT protocol to produce generalizable effects across treatment contexts. As such, we left out treatment features that are specific to certain yCBT targets such as exposure (which would be specific to yCBTs targeting anxiety) or behavioral activation (which would be specific to yCBTs targeting depression). Four treatment features that may relate to follow-up effects met these criteria: parental involvement, booster sessions, goal setting strategies, and maintenance and relapse prevention strategies. These treatment features are seldom empirically studied with regard to their associations with follow-up treatment effects.

Parent involvement is a good candidate predictor of follow-up effects, as parents likely continue to be with the youth after therapy is over, and their continual involvement may help with concretizing treatment gains. Theoretically, parents may act as an additional reinforcement agent and contingency manager for the youth (Khanna & Kendall, 2009; Silverman et al., 1999). However, meta-analytic findings are mixed. One meta-analysis indicated that there is a significant relationship between parental involvement and yCBT treatment gains (Manassis et al., 2014), whereas others found mixed (Creswell & Cartwright-Hatton, 2007) or non-significant relationships (Barmish & Kendall, 2005; In-Albon & Schneider, 2006; Reynolds, Wilson, Austin, & Hooper, 2012; Thulin, Svirsky, Serlachius, Andersson, & Öst, 2014). Our study will extend previous work by evaluating if parental involvement is associated with yCBT follow-up effects.

Next, booster sessions are included in many types of treatments including yCBTs because booster sessions are logically posited to reduce and prevent relapse (Beck, 2011). Booster sessions are hypothesized to maintain follow-up treatment gains by providing ongoing contact with a clinician; accordingly, researchers have investigated the beneficial effects of

booster sessions on treatment gains over the past several decades (Baker & Wilson, 1985; Eyberg, Edwards, Boggs, & Foote, 1998; Kroll, Harrington, Jayson, Fraser, & Gowers, 1996). One advantage to offering booster sessions is that these sessions can be tailored to specific difficulties the client expects to encounter post-treatment. In one meta-analysis, *r* effect sizes of 53 studies (28% of which featured booster sessions) of yCBTs for internalizing disorders were aggregated (Gearing, Schwalbe, Lee, & Hoagwood, 2013). Results indicated that pre- to post as well as pre- to follow-up effect sizes were larger for studies that included booster sessions, as compared with studies without booster sessions (Gearing et al., 2013). These findings suggest that booster sessions importantly contribute to yCBT effects at post-treatment as well as at follow-up.

Third, goal setting is a feature that involves the deliberate selection of an actionable therapeutic goal and breaking it into manageable steps. Clinicians have posited that it builds in a collaborative process that is a "gateway to maintaining long-term change" (Frank & Davidson, 2014, p. 113). Theory suggests that dissatisfaction from unmet goals increases goal-directed effort (Locke & Latham, 1990, 2002), and through repeatedly rewarded effort, goal setting is posited to increase youth self-efficacy (Bandura, 1989; Schunk, 1990). Goal setting has been included as a treatment feature in yCBTs that demonstrate significant effects at post-treatment (McCarty & Weisz, 2007; Seligman & Ollendick, 2011). Youths who are taught goal setting may continue to take action toward alleviating their own dissatisfaction after treatment has ended, increasing their self-efficacy for internal regulation, and maintaining treatment gains (Kuyken, Padesky, & Dudley, 2008). However, no study has empirically assessed whether goal setting is associated with yCBT treatment gains at later follow-up.

Finally, the inclusion of maintenance and relapse prevention strategies (herein "maintenance/relapse prevention") may also be related to effects of yCBTs at follow-up, and is commonly implemented in the yCBTs literature (e.g., Holmes, Donovan, Farrell, & March, 2014; Rosselló & Bernal, 1999). Such protocols are most formalized and well-studied in the adult addiction literature (Marlatt & Gordon, 1985), but they share many of the same principles. These sessions typically occur near the end of treatment, and the goals are for youth to consolidate learned skills and anticipate warning signs, future challenges, and to apply an action plan based on their skills for future challenges that could arise after termination. Theoretically, maintenance/relapse prevention sessions are posited to prepare youth so that they do not feel dependent on the practitioner (Beck, 2011). To the best of our knowledge, no study has assessed the relationship between the use of maintenance/relapse prevention sessions and yCBT treatment gains for internalizing disorders at post-treatment or at later follow-up, and the current study sought to fill this gap in the literature.

Rationale for Meta-Analytic Approach

In general, RCTs are needed to understand the efficacy of the aforementioned strategies, but they should be informed by generalized patterns across the relevant literature first obtained through systematic review and meta-analysis. RCTs that manipulate specific treatment features have been conducted for parental involvement (e.g., Spence, Donovan, & Brechman-Toussaint, 2000) and booster sessions (Clarke, Rohde, Lewinsohn, Hops, &

Seeley, 1999), but these studies have focused on specific treatment targets such as anxiety (Spence et al., 2000), or depression (Clarke et al., 1999), which limits the generalizability of the findings to those specific disorders. Furthermore, no RCT has investigated effects of goal setting and maintenance/relapse prevention sessions on treatment outcomes. Conducting new RCTs that fill these gaps in the literature would pose several challenges. RCTs are time-intensive and expensive, and investigations of treatment features that predict effects at follow-up assessments would likely be even more expensive and necessitate additional time. Also, effects produced by RCTs are limited to the contexts in which they are tested. In contrast, a meta-analytic approach would allow us to leverage the data of existing RCTs to fill the gaps in the literature in a more timely and cost-effective manner. Given that there is natural variation in the inclusion of these four treatment features in the hundreds of existing yCBTs, data exists that can be utilized at a meta-analytic level, regardless of the original intentions of their respective RCT. Additionally, meta-analytic findings are advantageous in that the results are more context-general than those of RCTs.

Existing Relevant Meta-Analyses

Meta-analyses on youth psychotherapy for internalizing disorders are produced frequently (e.g., Cartwright-Hatton, Roberts, Chitsabesan, Fothergill, & Harrington, 2004; In-Albon & Schneider, 2006; Ishikawa, Okajima, Matsuoka, & Sakano, 2007; A. A. James, Soler, & Weatherall, 2005; A. C. James, James, Cowdrey, Soler, & Choke, 2015; Manassis et al., 2014; Reynolds et al., 2012; Warwick et al., 2017). Of note, these meta-analyses typically focus on specific treatment targets (e.g., anxiety only) or specific age groups (e.g., adolescents only). The latest and largest meta-analysis for general youth psychotherapy to date also examined how certain aspects of treatment and study design related to follow-up outcomes (Weisz et al., 2016). Findings indicated that yCBTs produced the most robust cross-informant evidence of beneficial effects at post-treatment and at follow-up relative to other psychotherapy types such as client-centered, psychodynamic, or gestalt therapy (Weisz et al., 2016). Additionally, the target of treatment, inclusion of comorbid youths, and outcome informant was significantly associated with post-treatment and follow-up effects (Weisz et al., 2016). The importance of these characteristics cannot be understated, but these types of factors are not adjustable at the time of treatment planning. We are more interested in instead examining treatment *features*, which differs from *characteristics* in that they can be decided upon by a treatment developer or deliverer.

In terms of types of reported effects, few meta-analyses reported pre- to post-treatment or pre- to follow-up effect sizes. Rather, they commonly reported aggregated yCBTs vs. "usual care" or yCBTs vs. waitlist control group effect size comparisons instead. Consequently, we know less about the overall magnitude of symptom change one can expect from yCBTs as well as the durability of symptom change over time. Two meta-analyses for anxiety (In-Albon & Schneider, 2006; Ishikawa et al., 2007) reported pre- to post-treatment and pre- to follow-up effect sizes, evidencing Cohen's *d* effects ranging from 0.86 to 0.94, and these effects were maintained at follow-up in both studies.

Review Aims

Although much is known about the effectiveness of yCBTs for internalizing disorders, there are several limitations in the literature that must be addressed. First, yCBT meta-analyses rarely report pre- to post-treatment or pre- to follow-up effects, which prevents us from understanding the average effects of delivered yCBTs. Second, despite the purported intention behind including certain decidable treatment features in yCBT, their relationship with follow-up effects are rarely examined. Finally, existing relevant meta-analyses are typically narrow in that they examine single treatment targets or single age groups.

In the current study, we explored whether four specific treatment features (i.e., parent involvement, booster sessions, goal setting, and maintenance/relapse prevention), were associated with pre- to follow-up effect sizes. These findings may inform yCBT treatment developers and providers about the extent to which these treatment features are associated with effects at post-treatment and follow up, which is helpful for the adaptation and maximization of treatment effectiveness in the short and long-term. This also has value to consumers of treatment for whom long-term outcomes are central. To do this, we aggregated summary statistics on pre- to post-treatment effects and pre- to follow-up effects for yCBTs and report them using Meta-Analysis Reporting Standards (MARS) guidelines. These meta-analytic findings provide an up-to-date characterization of the effectiveness of extant yCBTs across several internalizing disorders. We are also more inclusive than previous meta-analyses; we included yCBTs that targeted internalizing disorders more generally (i.e., depression, anxiety, obsessive compulsive disorder, post-traumatic stress disorder) and yCBTs targeting pre-adult age groups (i.e., under 18 years old).

Method

Inclusion and Exclusion Criteria

Articles were extracted from the PracticeWise Evidence-Based Services (PWEBS) literature database, a database that summarizes youth mental health services across diagnoses (PracticeWise, 2017). Articles included in PWEBS are continuously identified through computerized searches using electronic databases (e.g., PsycINFO, Medline) by a standing team of trained professional coders and through nominations by scholars in treatment outcome research (e.g., members of Hawaii's Evidence Based Services Committee). The database included 987 youth treatment articles published from 1965 through 2017 when it was accessed in February 2017 under a research agreement with PracticeWise, LLC. All yCBTs in the featured articles are either general CBT protocols or protocols from well-known treatment manuals (e.g., Coping Cat, Trauma-Focused CBT), targeted internalizing disorders and were conducted primarily on four age groups: young children primarily aged 6 and under, youth primarily ages 7–12, adolescents pri7marily aged 13 and older, or a mix of more than one of these groups. All articles described an RCT design with no minimum sample size. Articles were included for study when they: (a) tested an active yCBT treatment (with or without medication), (b) used random assignment, (c) explicitly targeted one of the following internalizing disorders: anxiety, depression, traumatic stress, or obsessive compulsive disorder, and (d) they were delivered in individual or group sessions. Articles were excluded if: (a) they used a non-clinical youth sample, (b) they did not use a symptom

measure to assess a target problem area (e.g., they used a functional outcome only, such as "school attendance"), (c) they did not conduct a follow-up assessment (see Figure 1 for a flowchart illustrating these steps), (d) they were a single session intervention, or (e) they did not report the necessary statistics for calculating effect sizes (unless these statistics were provided when we contacted the authors). Effect sizes for each article were computed as the primary dependent variable (see section "Effect size calculation" below). Treatment features (i.e., parental involvement, goal setting, booster sessions, and maintenance/relapse prevention) were coded as primary independent variables, while treatment characteristics and research design characteristics were coded as covariates.

Search Strategies

The full text of each article from PWEBS was examined by six article coders (four Ph.D. students, one post-baccalaureate, and one undergraduate) who determined the eligibility of each article according to the above criteria. Follow-up articles were then queried from Google Scholar, Web of Science, PsycINFO, and PubMed. These articles were then examined for eligibility by the first and second author. Finally, the first and second author confirmed through manual review of the search results that no other original RCT articles not yet in our sample met the inclusion criteria.

Coding Procedures

Protocols of each treatment group per article were coded for various features based on the description provided in their respective article. Initial coding for entry into PWEBS involved two coders and a third validation judge using the PracticeWise Clinical Coding System (PracticeWise, 2012). This coding procedure has produced excellent inter-rater agreement (kappas ranging from .84 to 1.0; rs ranging from .88 to 1.0; (Cicchetti, 1994)) as reported elsewhere (e.g., Chorpita & Daleiden, 2009; Chorpita et al., 2011). See the Method section in Supplemental Materials for more details.

An additional round of coding was conducted to code some new variables and collect all of the information regarding treatment outcome data. Aforementioned article coders were extensively trained to code articles based on a coding manual developed for this meta-analysis study. The base of this study's coding manual was informed by existing article coding manuals (e.g., PracticeWise, 2012) and developed in consultation with the authors of those manuals. Independent subsets of articles were delegated to each coder, while the second author coded all study articles independently and validated her results with each coder. Disagreements were resolved through a joint reading followed by a mutual agreement made between the second author and the respective coder. These additional procedures (described in more detail in Rith-Najarian et al., 2017) produced excellent inter-rater agreement (Cohen's kappa range: .79 to .97). An overview of all variables is presented in Table 1.

For outcome coding, we examined each article's published results and aggregated target symptom outcome data for each CBT group at each assessment period. All outcome data collected was based on measures of the targeted symptoms that treatments within each study were designed to address (i.e., anxiety, depression, obsessive-compulsive disorder, post-

traumatic stress disorder). Target symptom measures were determined by identifying the problem area common to the following parameters: (a) sample selection characteristics, (b) target of the treatment design, and (c) the selected treatment outcome measures. For every target symptom measure reported, we collected the statistics (*M*s, *SD*s, *n*s) at each assessment period (pre-treatment, post-treatment, follow-up). If this data was not provided in the article, we contacted authors and obtained the necessary data directly. If data from a particular measure were available only for one or two assessment periods, then the data were not incorporated in the meta-analysis.

Statistical Methods

Effect size calculation—We calculated within-subject pre- to post-treatment and pre- to follow-up effect sizes as the primary dependent variables, using the Comprehensive Meta-Analysis software version 3 (Borenstein, Hedges, Higgins, & Rothstein, 2017). We calculated within-subject effect sizes for each yCBT rather than between-group effect sizes for two reasons. First, given that our article sample included RCTs with various types of comparison groups (e.g., waitlist, active controls) effect size formulas that incorporate comparison group data will produce effect sizes that differ as a function of the type of comparison group. Second, the majority of RCTs do not have a comparison group at follow-up assessments, and so we would have to forfeit these yCBT data if a formula required comparison group data. Therefore, we deemed within-subject effect size formulas as more appropriate for our meta-regression intentions. Effect sizes were calculated as mean differences between all pre-treatment and post-treatment assessment periods for every target symptom outcome measure in every yCBT group. Our effect sizes were calculated by:

$$Hedge's\ g_{av} = \frac{M_1 - M_2}{\frac{SD_1 + SD_2}{2}} \times \left(1 - \frac{3}{4(n_1 - 1) - 1}\right)$$

The former part represents Cohen's $d_{average}$, used for correlated groups by dividing the mean difference by their average standard deviations (Lakens, 2013).[1] This term is multiplied by the latter part of the formula representing a Hedge's correction (Lakens, 2013) . This correction adjusts for the discrepancy between our sample effect sizes and the population effect size, which exists because the sample standard deviation is only an estimate of the actual population standard deviation and is thus subject to sampling error (Hedges & Olkin, 1985). Here, *M*s stand for means, *SD*s stand for standard deviations, *n* for sample size, the subscript 1 is for pre-treatment, and the subscript 2 is for either post-treatment or follow-up period, depending on the *d* being calculated. When a yCBT reported multiple outcomes and thus multiple effect sizes, they were averaged within a time point. All follow-up effect sizes (if there was more than one follow-up assessment of the yCBT) were averaged across follow-up time points to create one follow-up treatment effect size per yCBT. This resulted

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[1]We used the $d_{average}$ as opposed to the $d_{repeatedmeasure}$ (a more conservative metric) because calculation of this metric requires the correlation coefficient of pre-treatment and post-treatment means, which most studies in our sample do not report. We decided that the costs of dropping studies that did not report this coefficient outweighed the costs of using *daverage* Alternatively, we could have substituted a standard *r* (e.g., *r* = 0.70) for all studies that did not report this statistic. However, we decided that using an arbitrary value for the calculation of most yCBT's effect sizes would not provide additive benefit, especially since it might advantage or disadvantage those few studies that did report a *r* value for their pre-post measure correlations.

in one post-treatment effect size and one follow-up effect size per yCBT group. Cohen's (1988) guidelines for the *d* value (i.e., .20 indicating small, 0.50 indicating medium, and 0.80 indicating large effects) were used to facilitate interpretation. We also report 95% confidence intervals around all effect size estimates.

Meta-Regression—To better understand how treatment features are associated with effect size, we conducted restricted maximum-likelihood based random-effects inverse variance weighted least squares meta-regressions with a Knapp and Hartung adjustment. The yCBT groups in our sample were characteristically heterogeneous (e.g., treatment target, age, delivery format) therefore necessitating a random-effects model to account for this heterogeneity. The Knapp and Hartung adjustment appropriately modified the estimation of between-group variance to be based on *t* or *F* distributions in each random-effects model (Knapp & Hartung, 2003). The restricted maximum-likelihood approach provided the least biased estimation of the true between-studies variance given that our effect sizes were normally distributed. We used the *Q* test statistic to assess between-group and within-group variance in these models, which has a chi-square distribution with $k - 1$ degrees of freedom (Hedges & Olkin, 1985).

For each of the meta-regression models, we entered three groups of predictors in separate steps and evaluated each group of predictors separately. In the first step, we examined the amount of variance explained by treatment characteristics, which included treatment duration (in days), youth sample age group (young children primarily ages 6 and under, youth primarily ages 7–12, adolescents primarily ages 13 and older, or "wide range" that may span all of these age groups), treatment target (anxiety, depression, traumatic stress, obsessive compulsive disorder), and delivery type (individual-format or group-format). In the second step of the meta-regression, we examined the amount of variance explained by the treatment features (i.e., goal setting, maintenance and relapse prevention, parental involvement, and booster sessions) over and above treatment characteristics in a combined model to arrive at more accurate association estimates. In the third step, we examined the amount of variance explained by research design characteristics such type of measure reporter (youth, caregiver, or evaluator), number of measures included within the effect size estimate, and time of follow-up assessment (one, three, six, twelve months or later).[2] Treatment feature associations are interpreted after research design characteristics are accounted for to ensure that methodological quality of the study is controlled as much as possible (Mansfield & Busse, 1977). The time to follow-up assessment was included as a covariate that investigated pre- to follow-up effect sizes as the outcome.

Assessment of bias—There are many known factors that bias effect size measurements. To protect against the well-known file drawer problem, whereby studies with null or negative findings are less likely to be published (Begg, Cooper, & Hedges, 1994; Rosenthal, 1979), we used a funnel plot (Torgerson, 2006), with standard error on the ordinate and

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[2]This arrangement of the steps does not provide an estimate of the unique variance attributable to treatment features. As an additional step, we reran these models flipping steps 2 and 3, setting treatment features as the last step. While overall, the results appear identical, we found that there was less unique variance explained by treatment features (pre to post-treatment effects: R^2 analog = 0.06, F(16, 89) = 3.93, $p < 0.001$; pre to follow-up effects: R^2 analog = 0.05, F(21, 84) = 2.33, $p < 0.003$).

effect size on the abscissa. The funnel shape produced by the plot would be asymmetrical if there was significant publication bias. This bias was tested using Egger's weighted regression test (Egger, Smith, Schneider, & Minder, 1997), for which significance would suggest biased asymmetry. When significant, we applied the trim-and-fill method (Duval & Tweedie, 2000) to examine if adjusting the effect size for bias in this way changes the effect size substantially. We also assessed for biases stemming from incomplete outcome data, and selective reporting. To assess bias stemming from incomplete outcome data, we first assigned a "data type" variable for each study, defined as the type of data from the article on which the effect size was based – raw, completer, intent-to-treat, or imputed. We ran a meta-regression with this as an independent variable associated with effect size. Finally, to assess for selective reporting bias, we ran a meta-regression with a binary "complete reporting" independent variable associated with effect size. A study was assigned a 1 on the "complete reporting" variable if all target symptoms measures listed in the methods were reported in the results; otherwise, the study was assigned a 0. Meta-regressions to assess bias were configured to match those of our main analyses: restricted maximum-likelihood based random-effects inverse variance weighted least squares with a Knapp and Hartung adjustment.

Results

Study Characteristics

Our final sample consisted of 76 articles (and 18 supplemental articles with additional follow-up data) reporting outcome statistics for 106 yCBT groups (as some studies tested more than one yCBT group). Coded articles were published from 1990 to 2017. The treatment targets for these yCBT groups were, in accordance to the designations reflected in the most current version of the Diagnostic and Statistical Manual of Mental Disorders (DSM-5, American Psychiatric Association, 2013): anxiety ($n = 61$), depression ($n = 14$), traumatic stress ($n = 24$), and obsessive-compulsive disorder ($n = 7$). Table 1 of Supplemental Materials provides detailed information of these studies and Figure 1 of Supplemental Materials provides a forest plot of effect sizes for all yCBT groups.

Treatment Characteristics

Of the 106 yCBT groups, 50 (47.17%) of these groups were delivered in group format and 56 (52.83%) were delivered in an individual youth or family format. Very few yCBTs targeted young children under 6 (3.77%), and the remaining yCBT groups delivered treatment to a wide range of ages. Treatment duration (for studies that included this information) was on average 79.56 days (SD = 31.14, range = 3–183 days). Ten studies failed to report the treatment duration, and for the purposes of meta-analysis their data was mean substituted.

Treatment Features

Parental involvement (66 yCBT groups, or 62.26%) was the most common feature used, followed by maintenance/relapse prevention sessions (58 yCBT groups, or 54.72%), booster sessions (22 yCBT groups, or 20.75%) and then goal setting (16 yCBT groups, or 15.09%). No treatment groups used all four features, and 41 yCBT groups (38.32%) did not use any

feature examined in this review. Table 2 of Supplemental Materials reports the number of yCBT groups that contained each feature organized by treatment target. The table also contains information on the number of studies that with every combination of features that exist in our study sample.

Research Study Characteristics

YCBT groups used an average of 2.31 (SD = 1.38, range = 1–9) primary outcome measures. Unsurprisingly, the majority of groups (88.68%) featured youth-response measures as one of the primary outcome measures, but 43.40% used caregiver-response measures and 21.70% used evaluator-response measures as primary outcome measures. On average, these measures were assessed 1.51 (SD = 0.88, range = 1–4) times at follow-up periods after a post-treatment assessment period. Groups featured follow-up periods ranging from 1 to 89 months post-treatment (M = 11.80 months, SD = 16.69).

Aggregated Effect Sizes

Mean post-treatment effect size was 1.05 (SE = 0.05, *95% CI* = [0.94, 1.15]) and mean follow-up effect size was 1.29 (SE = 0.06, *95% CI* = [1.18, 1.40]), both of which were normally distributed. An intercept-only (null) model, reflected a significant amount of potentially explainable between-group variance at post-treatment (I^2 = 83.75%, Tau^2 = 0.29, Tau = 0.54, Q = 646.15, df = 105, $p < .001$) and at follow-up (I^2 = 80.75%, Tau^2 = 0.28, Tau = 0.53, Q = 545.58, df = 105, $p < .001$). Table 2 reports the effect sizes by treatment feature.

Meta-Regression

Meta-regression revealed significant between-group variance explained by treatment characteristics when simultaneously tested as independent variables on pre- to post-treatment effect sizes (R^2 analog = 0.17, $F_{(8, 97)}$ = 2.56, $p = .01$). Effect sizes significantly differed by treatment target ($F_{(3, 97)}$ = 3.93, $p = .01$), being largest for obsessive compulsive disorder (B = 0.67, t = 2.65, p = 0.009), followed by depression (B = 0.45, t = 2.19, p = 0.03), as compared to anxiety. Traumatic stress (B = 0.28, t = 1.90, p = 0.06) did not differ significantly from anxiety. No associations were found for treatment delivery type, age group of the participants, and treatment duration ($ps > .05$). The goodness-of-fit test revealed that significant unexplained within-group variance remained (I^2 = 80.29%, Tau^2 = 0.24, Tau = 0.49, Q = 492.13, df = 97, $p < .001$). Next, we investigated treatment features over and above the previous set of variables, which further revealed significant between-group variance accounted for (R^2 analog = 0.29, $F_{(12, 93)}$ = 3.16, $p < .001$), but still significant remaining unexplained within-group variance (I^2 = 83.75%, Tau^2 = 0.29, Tau = 0.54, Q = 646.15, df = 93, $p < .001$). At this step of the model, parental involvement was revealed to be significant (B = 0.43, SE = .13, p = 0.001, *95% CI* = [0.17, 0.68]), but not goal-setting, maintenance/relapse prevention, or booster sessions ($ps > .05$). The significant differences by treatment target ($F_{(3, 93)}$ = 4.24, $p = .007$) remained: obsessive compulsive disorder (B = 0.59, t = 2.33, p = 0.02), depression (B = 0.55, t = 2.41, p = 0.02), and traumatic stress (B = 0.37, t = 2.62, p = 0.01), relative to anxiety.

Finally, we investigated research study characteristics over and above the previous set of variables, revealing still more significant between group variance (R^2 analog = 0.49, $F_{(16,}$

89) = 3.93, $p < .001$). Significant unexplained within-group variation remained (I^2 = 70.02%, Tau^2 = 0.15, Tau = 0.38, Q = 296.85, df = 89, $p < .001$), although parental involvement also remained significant at this step of the model (B = 0.40, SE = .12, p = .001, 95% CI = [0.16, 0.64]). Other treatment features, and the research study characteristics of outcome reporter and the number of measures incorporated into the effect size were non-significant ($ps > .05$). Treatment target differences also became non-significant at this step (p = .32). See Table 3 for details of models.

When examining pre- to follow-up effects, meta-regression did not reveal significant between-group variance explained by treatment characteristics (R^2 analog = 0.13, $F(8, 97)$ = 1.59, p = .14) while a goodness-of-fit test revealed significant unexplained within-group variance that remained (I^2 = 80.75%, Tau^2 = 0.28, Tau = 0.53, Q = 545.58, df = 97, $p < .001$). No variables from the treatment characteristic set were significantly associated with pre- to follow-up effect size ($ps > .05$). Next, we investigated treatment features over and above the treatment characteristics, which revealed significant between-group variance accounted for (R^2 analog = 0.28, $F(12, 93)$ = 2.07, p = .03), although significant unexplained within-group variance remained (I^2 = 70.99%, Tau^2 = 0.20, Tau = 0.45, Q = 320.58, df = 93, $p < .001$). At this step of the model, parental involvement was again revealed to be significant (B = 0.34, SE = .13, p = .02, 95% CI = [0.06, 0.62]), but goal-setting, maintenance/relapse prevention, and booster sessions were not related significantly to the dependent variable ($ps > .05$). Finally, we investigated research study characteristics over and above the previous set of variables, revealing still more significant between-group variance (R^2 analog = 0.47, $F(21, 84)$ = 2.33, p = .003). Significant unexplained within-group variation remained (I^2 = 65.95%, Tau^2 = 0.15, Tau = 0.38, Q = 246.71, df = 84, $p < .001$), although parental involvement also remained significant at this step of the model (B = 0.31, SE = .14, p = 0.03, 95% CI = [0.04, 0.58]). Research study characteristics - follow-up assessment time, outcome reporter, and the number of measures incorporated into the effect size - were all non-significant ($ps > .05$). See Table 5 for full details of these models.

Publication Bias

Egger's weighted regression test was significant (t = 3.03, p = .003) suggesting that larger effect sizes were disproportionately obtained, indicating potential publication bias (Jüni, Holenstein, Sterne, Bartlett, & Egger, 2002). The trim-and-fill method however, did not indicate different effect sizes, suggesting that publication bias had minimal impact on obtained results. No significant effect was found for intent-to-treat, completer, or imputed data types (post-treatment, p = .38 – .86; follow-up: p = .33 – .71) or complete versus incomplete reporting of measures (post-treatment, p = .43; follow-up: p = .78). Taken together, this suggests our obtained effect sizes were not impacted by incomplete outcome or selective reporting biases.

Discussion

Given our limited understanding of how yCBTs might produce effects that last through follow-up, the current meta-analysis achieved two major aims. First, we estimated pre- to post-treatment and pre- to follow-up effect sizes of yCBTs across a range of internalizing

disorders and age groups. Second, we investigated the magnitude of the association between four yCBT treatment features putatively important for efficacy at follow-up (i.e., goal setting, parental involvement, maintenance/relapse prevention, and booster sessions) and treatment effects at treatment termination and at follow-up.

Our findings suggest that the overall within-group effect of the 106 yCBTs for internalizing disorders from pre- to post-treatment ($d = +1.05$) and from pre- to follow-up ($d = +1.29$) are rather large. In common language effect size terms (McGraw & Wong, 1992), this means that 77.11% of youth have improved outcomes at post-treatment and 81.92% of youth having improved outcomes at follow-up compared to a random youth selected at pre-treatment by chance. Biases of these estimates introduced by publication practices, selective reporting, or incomplete outcome reporting appeared to have a minimal impact on our estimates. Taken together, these results suggest that yCBT effects for internalizing disorders appear sustained after treatment termination. Other treatment characteristics (e.g., the targeted yCBT disorder, age group, group- versus individual-format, treatment duration) collectively explained significant variance in pre- to post-treatment effect size but did not explain significant variance in pre- to follow-up effect size. Research study characteristics (i.e., type of measure reporter, number of measures included within the effect size estimate, and time of follow-up assessment) explained significant variance in pre- to post-treatment and pre- to follow-up effect sizes. No individual treatment characteristic or research study characteristic was found to be associated significantly with pre- to post-treatment or pre- to follow-up effect sizes, however.

The four treatment features examined in this study were associated significantly with pre- to post-treatment and pre- to follow-up effect sizes, over and above individual treatment or research characteristics. Specifically, these features explained 14% of the variance in effect sizes from pre- to post-treatment and 15% of the variance in effect sizes from pre- to follow-up. Despite their theoretical usefulness, more than 15% of the treatment groups reviewed (16/106) did not use any of the treatment features. Of the four features, only parental involvement was found to be associated with either pre- to post-treatment or pre- to follow-up effects. Controlling for all treatment and research characteristics, involving parents had 61.41% ($d = +.41$) of children better off by treatment termination and 58.68% of children better off at follow-up assessment ($d = +.31$), relative to a random child in a yCBT group without parental involvement in common language effect size terms (McGraw & Wong, 1992).

In the current analysis, we found overlap (6% for pre- to post-treatment effects and 10% for pre- to follow-up effects) in the variance explained by research characteristics and treatment features. Given the current state of the yCBT literature, it appears that research characteristics can also explain significant variance in treatment effects, suggesting that there is collinearity in research design and the use of certain treatment features (e.g., treatments that involve parents also include parental measures and more measures included in the effect size). Our current study design does not allow us to confidently attribute this overlapping variance to either set of variables. It may be that increased effects are produced by research characteristics that are more complex or meticulous. However, we believe that the explanation that treatment features are associated with treatment outcomes is the more

conceptually parsimonious one. Additionally, no single research characteristic was found to be significantly associated with treatment effects are post-treatment or follow-up.

Our finding that parental involvement is a significant predictor of yCBT post-treatment and follow-up effects at the meta-analytic level demonstrates that at least some involvement of parents (as opposed to none) will *in general* result in larger and more durable effects. Individual RCTs that have found otherwise (e.g, Kendall, Hudson, Gosch, Flannery-Schroeder, & Suveg, 2008) may have varied or tested parental involvement by a different definition, or their findings may have been more specific to their study context.

The current study is more inclusive and has more yCBT groups relative to previous meta-analyses on parental involvement. Thus, the results strongly corroborate the parental involvement findings of more recent meta-analyses (i.e., Manassis et al., 2014). Manassis and colleagues (2014), pointed to two potential explanations. The first is that parental involvement in therapy may emphasize and increase parental contingency management, and the second is that there may be a transfer of control from therapist to parent. These two routes are not mutually exclusive, and they may reciprocally influence one another (e.g., a transfer of control empowers the parent to use more contingency management) to support treatment effects at follow-up.

It was surprising that given the theoretical importance placed on goal setting, maintenance/relapse prevention, and booster sessions outlined in this and previous studies, these features were not found to relate independently to either pre- to post-treatment effects or pre- to follow-up effects. These findings may indicate a lack of relationship between these purported factors and effect sizes, but it is also possible that these null findings reflect limitations in the data collected at present. In the current study, our ability to detect relationships between yCBT effect sizes and goal setting or booster sessions may be hindered by the fact that no yCBT groups used goal setting alone and only one used booster sessions alone. Also, most yCBT groups ($n = 72$) described more than one feature, making it difficult to know which feature was contributing to the associations reported. It may also be possible that certain treatment effects are moderated such that they are only effective for certain targets, age groups or delivery formats.

Limitations

Inherent in the method of meta-analysis, our results are retrospective and correlational. We cannot assert causal claims to describe the link between parental involvement and yCBT effects. Furthermore, extrapolating the results from the current data to predict the factors important for future yCBTs will require great caution. Our estimates and inferences are limited by the studies sampled. We contacted authors for any missing data unreported to maximize the validity of our estimates. However, we excluded unpublished dissertations, which may have limited the representativeness of our findings by eliminating some studies with relatively lower effect sizes (McLeod & Weisz, 2004). We also only examined yCBTs that targeted one disorder; exclusion of yCBTs designed to target comorbid diagnoses might reduce the clinical generalizability of our findings, as comorbidity is common for youth presenting in clinical settings (Angold, Costello, & Erkanli, 1999; Costello, Mustillo, Erkanli, Keeler, & Angold, 2003). Confounding effects are possible given the meta-analytic

design, for example, an additional 20–40% of families drop out of treatment at follow-up (Cohen, Mannarino, & Knudsen, 2005; Flannery-Schroeder, Choudhury, & Kendall, 2005), which may inflate pre- to follow-up effect size estimates. While we did test for this potential inflation within our collective assessment of study biases, we cannot be certain of what the effect size estimates would have been had there been no drop out in studies. Relatedly, caution is required in directly comparing the effect sizes between any two individual studies, because of the variations in study implementation practices, changes in diagnostic criteria from DSM-III to DSM-5, and the nature of clinical training impacting yCBT implementation over the years (Levenson, 2014). Finally, while the studies featured in this sample are quite heterogeneous, the current study was underpowered to analyze the potential moderation of the effect of each treatment feature.

Methodological limitations regarding our data-reduction efforts should be noted. We pooled effect sizes at the level of treatment group rather than individual studies, which is potentially risky as several studies featured multiple yCBT groups. Those within-study groups are more likely to be similar to each other compared to yCBT groups across studies. Weisz, McCarty, and Valeri (2006), however, found the pooling of effect sizes at the treatment group level to be an acceptable practice, depending on the conceptual goals of the analysis. In the current review, the principal interest was establishing the effect of an average yCBT protocol by post-treatment and at follow-up, which we believe to be an acceptable use of this practice. Because many studies had multiple follow-up periods, we averaged the effect sizes of these periods, risking the loss of information (Cheung & Chan, 2004, 2008), thus possibly losing some nuanced data. At the individual RCT coding level, limitations in the definitions or the reporting of features in manuscripts may have prevented our coders from detecting them. For example, most manualized yCBTs include some form of parental involvement and goal-setting, although they may not be explicitly described in the protocols. When using a categorical variable to assess the presence of these features, we may have missed some protocols that included them to a more minor extent. Finally, as a limitation on the generalizability of our findings, youth in research clinics may not best represent youth in clinical settings (e.g., Weisz, Donenberg, Han, & Weiss, 1995). Similarly, therapists may differ in RCTs versus clinical settings in terms of level of skill and adherence to yCBT protocols.

Future Directions for Science and Practice

Our findings have implications for both clinical research and practice. Although past findings on parent involvement have been mixed (e.g., Barmish & Kendall, 2005; Breinholst, Esbjørn, Reinholdt-Dunne, & Stallard, 2012; Clarke, DeBar, & Lewinsohn, 2003; Clarke, DeBar, & Lewinsohn, 1999; Creswell & Cartwright-Hatton, 2007; Manassis et al., 2014), the current review suggests that parental involvement is associated not only with better outcomes, but also with sustainably better outcomes. In our review however, only 62.26% of yCBTs reviewed in this study involved the parents, pointing to a potential gap in implementation. Given its marked impact, we would recommend yCBT deliverers and developers consider involving parents in treatment. Future studies are needed to clarify the cost-benefit ratios involved with the implementation of parental involvement to help understand when parents may or may not be necessary in yCBT. More exploration of

treatment features' role in effect durability is warranted. First, given their theoretical importance, the three other tested treatment features in this meta-analysis that did not demonstrate statistically significant contributions to follow-up effects at this time could be examined again once the yCBT literature has further grown. For example, booster sessions have been previously reported as influential (Gearing et al., 2013), and maintenance/relapse prevention was in the majority of yCBTs, suggesting it is a feature often recognized as potentially impactful. Second, future research should examine whether interactions between treatment features, such as the combination of parental involvement and goal setting, are associated with yCBT effect sizes. As the yCBT literature grows, questions of moderation will be important to answer. Third, future meta-analyses could explore whether other features or intervention elements (e.g., delivery format, motivational enhancement) are associated with treatment gains. In the current study, we presented a proof-of-concept where treatment features considered conceptually important to follow-up outcomes were tested as correlates of yCBT follow-up effects, but we would encourage the testing of other correlates in the future. Additionally, the same type of meta-analytic research can be applied to other populations (e.g., youth with externalizing disorders, adults) or different intervention modalities (e.g., technology-based yCBTs, interpersonal therapy, prevention programming). Finally, future research should examine these features in comorbid samples, which is arguably the bulk of real-world clinical care (Angold et al., 1999; Costello et al., 2003), and existing data suggests that psychotherapy may not fare well in these cases (Weisz et al., 2016). Our meta-analytic results reinforce our belief that existing knowledge can purposefully direct future RCTs and meta-analyses to prioritize such remaining research questions.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

Acknowledgments

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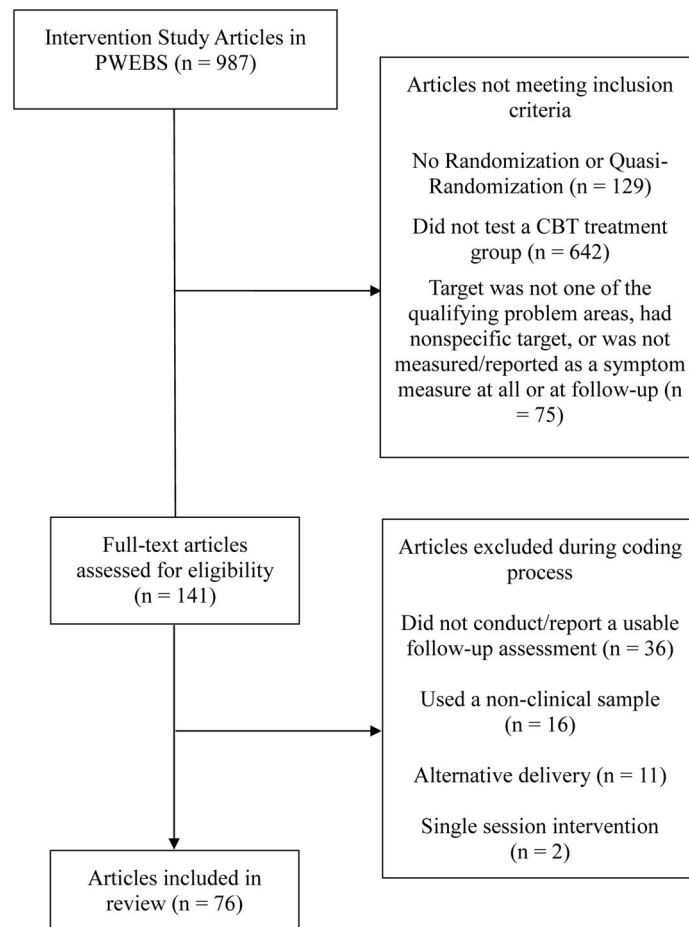
Figure 1. Article exclusion flowchart

Description. Article exclusion flowchart, outlining procedure to select final sample of articles meeting inclusion criteria. Note: Alternative delivery = computer, self, bibliotherapy, parent-delivered, audio-based

Table 1

Independent variables, their source, and their definitions.

Variable	Source	Definition
Treatment Features		
Booster Session	Additional Coding	If either (a) the treatment protocol included at least one booster session explicitly intended for all participants after the treatment ended, or (b) attendance rates were reported for at least one post-treatment booster session. Booster "materials" or unused booster sessions did not count
Goal-Setting	PWEBS	Presence of Goal-Setting is coded if a protocol describes how a therapeutic goal is selected for the youth and then repeatedly assessed for measurement of treatment progress.
Maintenance and Relapse Prevention	PWEBS	Presence of Maintenance/Relapse Prevention is coded if protocol describes exercises or conversations that are intended to consolidate skills already learned and to anticipate future challenges after treatment termination.
Parent Involvement	Translated from PWEBS	Based types of sessions that occur within the yCBT, according to RCT authors' description of session format and attendees. We credited parental involvement when a protocol included any sessions with: parent and child together, parents individually, a group of parents, or the whole family.
Other Treatment Characteristics		
Treatment Duration	PWEBS	Length of time (in days) from first day of treatment to last day of treatment
Target Age	Translated from PWEBS	Based on sample's mean age, standard deviation, range, and/or percentage of youth per age. Four categories were created: young children = primarily ages 6; youth = primarily ages 7–12; adolescents = primarily ages 13+; or "wide range" = spanning across two or more of the other age groups
Target	Translated from PWEBS	Based on coded data about the yCBT's targeted problem area: anxiety, traumatic stress, or depression. We reviewed all anxiety yCBTs to differentiate those that treated a youth sample with OCD symptoms specifically (based on a study's reported inclusion criteria).
Delivery Type	Translated from PWEBS	Based on type of sessions included in treatment delivery. Categories: individual delivery – youth only sessions, parent-child sessions, or single family sessions – or group delivery – youth group sessions, or parent-child group sessions. If treatment was delivery through multiple session types, then it was categorized by the more frequent session type
Research Study Characteristics		
Timing of Follow-Up	Additional Coding	The number of months from treatment end to follow-up assessment. Times were binned into groups: 1-month, 3-month, 6-month, 12-month, long-term. yCBTs may have been assessed multiple times.
Measure reporter	Additional Coding	Who completed the measure for which the effect size was collected. Three reporter types: youth (i.e., self-report measure), caregiver (parent or other adult responsible for youth's care), evaluator (individual who completed measure or interview as part of research study). Measures from other reporters (e.g., teacher) were not coded. Each yCBT may have been assessed by measures based on more than one reporter type.

Table 2

Comparison of effect sizes at post-treatment and at follow-up by predictor variables.

	Hedge's g	standard error	variance	lower limit	upper limit
At post-treatment					
Booster sessions					
No	0.94	0.047	0.002	0.85	1.03
Yes	1.33	0.169	0.029	1.00	1.66
Parent Involvement					
No	0.78	0.062	0.004	0.66	0.90
Yes	1.21	0.076	0.006	1.06	1.36
Maintenance/Relapse Prevention					
No	1.03	0.084	0.007	0.86	1.19
Yes	1.07	0.069	0.005	0.93	1.20
Goal-setting					
No	0.99	0.054	0.003	0.88	1.10
Yes	1.33	0.162	0.026	1.01	1.64
At follow-up					
Booster sessions					
No	1.17	0.051	0.003	1.07	1.27
Yes	1.61	0.140	0.020	1.33	1.88
Parent Involvement					
No	1.08	0.070	0.005	0.94	1.22
Yes	1.47	0.076	0.006	1.32	1.62
Maintenance/Relapse Prevention					
No	1.35	0.088	0.008	1.17	1.52
Yes	1.33	0.074	0.005	1.18	1.47
Goal-setting					
No	1.28	0.058	0.003	1.17	1.40
Yes	1.57	0.152	0.023	1.27	1.87

Table 3

Hierarchical Meta-Regression Predicting Pre- to Post-Treatment Effect Size

	Coefficient	t	F	R²analog	p
Model 1			2.56, df (8, 97)	.17	.01
Intercept	0.64	2.63			.01
Treatment Duration (Days)	0.00	0.11			.91
Age: Young Child	0.23	0.68			.50
Age: Youth	0.11	0.59			.56
Age: Adolescent	0.21	1.01			.31
Target: Depression	0.45	2.19			.03
Target: Obsessive Compulsive	0.67	2.65			.009
Target: Traumatic Stress	0.28	1.90			.06
Delivery: Individual	0.18	1.50			.14
Model 2			3.16, df (12, 93)	.29	<.001
Intercept	0.31	1.22			.22
Treatment Duration (Days)	0.001	0.37			.71
Age: Young Child	0.17	0.50			.62
Age: Youth	0.11	0.59			.56
Age: Adolescent	0.27	1.26			.21
Target: Depression	0.55	2.41			.02
Target: Obsessive Compulsive	0.59	2.33			.02
Target: Traumatic Stress	0.37	2.62			.01
Delivery: Individual	0.12	0.99			.32
Booster Sessions	0.11	0.73			.47
Parent Involvement	0.43	3.35			.001
Maintenance/Relapse Prevention	0.02	0.20			.84
Goal-Setting	−0.16	−0.76			.45
Model 3			3.93, df (16, 89)	.49	<.001
Intercept	1.09	2.93			.004
Treatment Duration (Days)	0.00	0.62			.54
Age: Young Child	−0.66	−1.75			.08

	Coefficient	t	F	R²analog	p
Age: Youth	−0.09	−0.50			.62
Age: Adolescent	0.08	0.43			.67
Target: Depression	0.43	1.88			.06
Target: Obsessive Compulsive	0.15	0.52			.61
Target: Traumatic Stress	0.11	0.72			.47
Delivery: Individual	−0.01	−0.10			.92
Booster Sessions	−0.10	−0.64			.53
Parent Involvement	0.40	3.29			.001
Maintenance/Relapse Prevention	0.04	0.35			.73
Goal-Setting	−0.09	−0.49			.63
Has youth-reported measure(s)	−0.47	−1.65			.10
Has caregiver measure(s)	−0.21	−1.51			.13
Has evaluator measure(s)	0.37	1.63			.11
Number of measures in ES	0.01	0.27			.79

Table 4

Hierarchical Meta-Regression Predicting Pre- to Follow-Up Effect Size

	Coefficient	t	F	R²analog	p
Model 1			1.59, df (8, 97)	.13	.14
Intercept	0.90	3.40			.001
Treatment Duration (Days)	0.00	−0.29			.77
Age: Young Child	0.17	0.45			.66
Age: Youth	0.18	0.94			.35
Age: Adolescent	0.34	1.52			.13
Target: Depression	0.27	1.21			.23
Target: Obsessive Compulsive	0.46	1.67			.10
Target: Traumatic Stress	0.20	1.27			.21
Delivery: Individual	0.24	1.79			.08
Model 2			2.07, df (12, 93)	.28	.03
Intercept	0.65	2.30			.02
Treatment Duration (Days)	0.00	−0.01			.99
Age: Young Child	0.11	0.31			.76
Age: Youth	0.16	0.77			.44
Age: Adolescent	0.35	1.50			.14
Target: Depression	0.34	1.35			.18
Target: Obsessive Compulsive	0.40	1.35			.18
Target: Traumatic Stress	0.29	1.81			.07
Delivery: Individual	0.19	1.42			.16
Booster Sessions	0.21	1.22			.23
Parent Involvement	0.34	2.43			.02
Maintenance/Relapse Prevention	−0.04	−0.31			.76
Goal-Setting	−0.11	−0.50			.62
Model 3			2.33, df (21, 84)	.47	.003
Intercept	1.41	3.05			.003
Treatment Duration (Days)	0.00	−0.47			.64
Age: Young Child	−0.60	−1.37			.17

	Coefficient	t	F	R²analog	p
Age: Youth	−0.08	−0.41			.68
Age: Adolescent	0.19	0.83			.41
Target: Depression	0.12	0.46			.65
Target: Obsessive Compulsive	−0.17	−0.47			.64
Target: Traumatic Stress	0.02	0.13			.90
Delivery: Individual	0.04	0.32			.75
Booster Sessions	0.06	0.33			.74
Parent Involvement	0.31	2.27			.03
Maintenance/Relapse Prevention	0.00	0.00			.99
Goal-Setting	−0.01	−0.02			.98
Has 1-month follow-up	−0.21	−0.67			.50
Has 3-month follow-up	0.03	0.21			.84
Has 6-month follow-up	−0.12	−0.90			.37
Has 12-month follow-up	0.15	0.97			.34
Has long-term follow-up	0.24	1.01			.31
Has youth-reported measure(s)	−0.27	−0.72			.47
Has caregiver measure(s)	−0.20	−1.18			.24
Has evaluator measure(s)	0.44	1.48			.14
Number of measures in ES	−0.03	−0.59			.56