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Effectiveness of Youth Psychotherapy Delivered Remotely: A Meta-Analysis

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Reports on remote psychotherapies for youth (e.g., technology-based treatment) suggest it is acceptable, feasible, and useful in overcoming logistical barriers to treatment. But how effective is remote care? To find out, PsycINFO and PubMed were searched from 1960 through 2020, supplemented by journal searches and reference trails, to identify randomized controlled trials of youth psychotherapy for anxiety (including obsessive–compulsive disorder and trauma), depression, attention-deficit/hyperactivity disorder (ADHD), or conduct problems, in which all therapeutic contact occurred remotely. Articles (N = 37) published from 1988 through 2020, reporting 43 treatment-control group comparisons, were identified. Robust variance estimation was used to account for effect size dependencies and to synthesize overall effects and test candidate moderators. Pooled effect size was .47 (95% confidence interval [CI: .26, .67], $p < .001$) at posttreatment, .44 (95% CI [.12, .76], $p < .05$) at follow-up—comparable to effects reported in meta-analyses of in-person youth psychotherapy. Effects were significantly (a) larger for remote psychotherapies supported by therapeutic provider contact (.64) than for those accessed by youths, with only logistical support (.22), (b) larger for treatments with phone contact (.65) than for those without (.25), (c) larger for treatment of anxiety (.62) and conduct problems (.78) than ADHD (–.03), and (d) smaller for therapies involving attention/working memory training (–.18) than for those without (.60). Among studies with therapeutic contact, effects were significantly larger when therapists facilitated skill-building (e.g., practicing exposures or problem solving [.68]) than when therapists did not (.18). These findings support the effectiveness of remote psychotherapies for youths, and they highlight moderators of treatment benefit that warrant attention in future research.

Public Significance Statement

The youth psychotherapies that have been tested remotely to date appear as effective as therapies delivered in-person. The effectiveness of remote psychotherapies may be enhanced when they are delivered with therapeutic provider support, especially when providers teach specific skills or discuss difficulties patients face when implementing skills. Additionally, remote youth psychotherapies were significantly more effective for anxiety and conduct problems than for ADHD.

Keywords: children, adolescents, psychotherapy, remote therapies, meta-analysis

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Across recent decades, there have been significant advances in the development, assessment, and dissemination of treatments for mental health problems in children and adolescents (hereafter referred to as *youths*). Despite this progress, there is considerable room for improvement, especially in reducing rates of mental illness and broadening access to youth mental health services (Kazdin, 2019; Weisz et al., 2017, 2019). Indeed, more than one in five youths will experience at least one psychiatric disorder that requires treatment before adulthood, and mental disorders are the leading cause of disability in youth (Erskine et al., 2015; Merikangas et al., 2010). Yet, even in high-income countries, only around one third of these youths will receive care; in low- and middle-income countries, the rate of access is much lower, likely under 10% (Merikangas et al., 2011; Rathod et al., 2017; Sweetland et al., 2014). This need-to-access gap is maintained and exacerbated by numerous structural (e.g., lack of trained professionals, economic burden) and social (e.g., stigma related to mental health problems) barriers to mental health care (e.g., Owens et al., 2002). Access to mental health care is especially limited within rural and low-income regions, which tend to be disproportionately disadvantaged by shortages of mental health professionals and facilities (Collins et al., 2011; Cummings et al., 2013; Thomas et al., 2009).

To address these key barriers to mental health service use among youths and families, researchers and clinicians have increasingly aimed to identify nontraditional methods for delivering psychotherapy. One such approach that has gained traction in recent decades involves using remote, frequently technology-based, therapies (Comer & Myers, 2016). Remote therapy (RT) is a form of psychological care in which therapeutic content is delivered to a patient through a remotely accessible platform (e.g., telephone; computerized program) instead of, or in conjunction with, face-to-face contact (American Academy of Child & Adolescent Psychiatry, 2017). Remote therapy encompasses, but is not limited to, approaches including tele-mental health, which often refers to using technology to provide real-time (i.e., synchronous) mental health care (Hailey et al., 2008). In addition to offering a potential solution for common barriers to accessing mental health care across the world, the application of RT to youth mental health is especially pertinent during contagious disease outbreaks and other crises that prevent in-person delivery of services, such as COVID-19 (Gruber et al., 2020). Restrictions on in-person interactions during the COVID-19 crisis are expected to persist in many countries for an indefinite period (Kissler et al., 2020), and experts predict similar pandemics, with comparable restrictions on face-to-face contact, will emerge in the future (Smolinski & Hamburg, 2003). Pandemics can produce abrupt shifts to remote psychiatric care (Ghebreyesus, 2020; Unützer et al., 2020), making this an especially appropriate time to evaluate evidence on the benefits of RT for youth mental health, and to identify factors that may moderate those benefits, both within and beyond the context of COVID-19 (Torous & Wykes, 2020).

Studies of RT have largely indicated that it is a feasible method for delivering youth psychotherapy (e.g., Hilty et al., 2004; K. Myers et al., 2006; K. M. Myers et al., 2007, 2010; Nelson et al., 2003). Advances in the quality and widespread availability of technology have contributed to the feasibility of RT, with 96% of adults in the United States reporting access to at least one mobile device (Pew Research Center, 2020). Rates of smartphone device ownership among young people are also growing rapidly, ranging from 19% of 8-year-old children to 91% of 18-year-old people in the United States reporting possession of a smartphone (Common Sense Media, 2019). Rates of mobile phone ownership among youths vary in developing countries, for example, 8.4% of youth in Malawi and 50.8% in South Africa report owning a mobile phone (Porter et al., 2016). Moreover, evidence suggests that remote therapies might be especially appealing to youths, who are accustomed to communicating via technology. Research testing the acceptability of RTs for youth mental health has yielded strong acceptability ratings among young people (e.g., Boydell et al., 2014; Hilty et al., 2004; K. M. Myers et al., 2007, 2010). Several trials even suggest that remote therapies are as effective as or possibly more effective than in-person therapy (Comer et al., 2014, 2017). Of note, much RT research to date has focused on acceptability and feasibility of remote audio- and video-based care, although many forms of RT have been tested for their effectiveness, ranging from working memory training (e.g., Bikic et al.,

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2017) to therapist-facilitated, computer-delivered therapy (e.g., Lenhard et al., 2017; Topooco et al., 2019).

Efforts to synthesize evidence on the potential acceptability and feasibility of RT have also often qualitatively analyzed the acceptability, feasibility, and effectiveness of audio- and video-based remote therapy approaches generally, with reviews largely underscoring the promise of these approaches for scalable mental health care for all ages (American Academy of Child & Adolescent Psychiatry, 2017; Hailey et al., 2008; Hilty et al., 2004, 2013). Although these reviews offer important information, the proliferation of a variety of RT methods applied to promote youth mental health highlights the potential value of examining effects of RT in all its current forms. Furthermore, applying a meta-analytic method to quantitatively examine the effects of RT approaches on clinical outcomes would usefully complement past research. Thus, the goal of the present article was to complement the valuable prior reviews with a meta-analytic synthesis of evidence encompassing the broad range of RT methods that have been evaluated in youth psychotherapy RCTs to date.

To this end, this present study is a meta-analysis of the overall effects of remote youth psychotherapies, assessed through RCTs conducted between 1960 and 2020; in which all therapeutic content was delivered without in-person contact. The study included a review of the characteristics of included studies and testing of moderators of psychotherapy effects on key clinical outcomes. For example, adult psychotherapy meta-analyses indicate that provider-supported remote interventions for adult depression have outperformed unsupported interventions (Andersson & Cuijpers, 2009) and that adult mental health apps have worked better with professional guidance than without (Linardon et al., 2019), therapeutic contact may similarly moderate the effects of RT for youths. The authors sought information to advance understanding of the overall potency of RT for youth mental health and inform future research and clinical practice related to this approach, both during and beyond the COVID-19 crisis.

Method

Search Strategy

PsycINFO and PubMed were searched to identify peer-reviewed articles reporting outcomes of youth psychotherapy RCTs published between January 1960 and June 2020. Search terms included those related to psychological disorders and counseling (e.g., mental disorders, psychotherapy, counseling) and limiters related to age and trial type. See the online supplemental material for full list of search terms and limiters. Additionally, studies were identified through references of youth psychotherapy systematic reviews and meta-analyses, recommendations by youth psychotherapy researchers, and journals that often publish literature on remote psychotherapy (i.e., *Journal of Medical Internet Research* [JMIR], *JMIR Mental Health*, and *Internet Interventions*). A protocol for this review was registered via PROSPERO (CRD42020180923).

Inclusion and Exclusion Criteria

Study inclusion criteria were: (a) youths treated for psychopathology in one or more of the four broad problem areas that account for most mental health referrals in youth (Weisz & Kazdin, 2017; anxiety-related disorders and problems, including obsessive–compulsive disorder (OCD) and trauma; depression-related disorders and problems; attention-related disorders and problems (attention-deficit hyperactivity disorder [ADHD]); and conduct-related disorders and problems); (b) mean participant age between 3.5 and 18.4 years to allow for comparison to recent youth psychotherapy meta-analyses that have used the same age range (Weisz et al., 2017, 2019); (c) random assignment of youths to treatment and control conditions in which at least one treatment condition consisted of psychological therapy (studies testing medication only or psychotherapy combined with medication were excluded, as were randomized trials that only included comparison conditions involving active supported treatments); (d) studies in which at least one psychotherapy condition was delivered without any in-person therapeutic contact (i.e., no psychoeducation, skill-building, rapport-building, or other therapeutic content could occur in person—only purely logistical contact, in the form of technical support, study expectation-setting, or provision of materials could occur in person); (e) mental health outcomes were assessed at pre- and postintervention in both the intervention and control groups; (f) articles were available in English (although

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studies could be conducted in any country). As in several other recent youth psychotherapy meta-analyses (e.g., Eckshtain et al., 2020; Weisz et al., 2017), psychopathology was defined broadly to include not only meeting criteria for *Diagnostic and Statistical Manual of Mental Disorders* (5th ed.; American Psychiatric Association, 2013) diagnoses, but also clinically elevated scores on standardized measures of psychopathology.

Data Extraction, Coding, and Processing Procedures

All included studies were coded for sample and study characteristics, including candidate outcome moderators (see Table 1 for outcome moderators). All study characteristics specifically related to remote therapies (e.g., media of delivery) were coded by the first and second authors, who independently double-coded a selection of 25% of studies for reliability. The fourth author coded all other study and sample variables (e.g., age, gender, outcome type). She had already achieved intercoder reliability (reported below) on these variables. Disagreements were resolved by collaborative article review and discussion.

The target problem was coded for each study and for each individual effect size (ES), with options including anxiety (including trauma and OCD), depression (including self-harm behaviors and suicidality), attention problems/ADHD, and conduct problems ($\kappa = .83$). Coding included percentage Caucasian participants (intraclass correlation [ICC] = .87), percentage male and female participants (ICC = .95), mean participant age (ICC = .99), and percentage of content delivered with therapist support (ICC = .94). Each of these continuous variables was made categorical for moderator analyses (see Table 1).

Intervention and control conditions were coded, with a focus on intervention variables relevant to remote therapy delivery. Control conditions were coded as either active control (i.e., psychotherapy placebo—active conditions resembling psychotherapy that are intended to control for the nonspecific aspects of psychotherapy) or waitlist/no treatment control ($\kappa = .83$). For all intervention conditions, therapy type was coded (youth-focused behavioral [including cognitive-behavioral] interventions, youth-focused nonbehavioral interventions, caregiver and family-focused behavioral [including cognitive-behavioral] treatment, caregiver and family-focused non behavioral treatment, and multisystem treatment; $\kappa = .83$), and presence or absence of attention/working memory training ($\kappa = .950$).

Finally, coding included candidate moderators especially relevant to remote therapies: (a) frequency of scheduled contact ($\kappa = 1.00$); (b) use of logistical contact such as tech support ($\kappa = 1.00$); (c) use of reminders ($\kappa = 1.00$); (d) presence of therapeutic provider contact ($\kappa = 1.00$); (e) type of therapeutic provider contact, including motivational enhancement (i.e., motivational interviewing meant to encourage completing therapy activities), skill building (i.e., therapeutic contact meant to teach, practice, or review therapeutic skills), and implementation support (i.e., troubleshooting implementation difficulties such as barriers to using a therapeutic skill; $v\kappa = .82–1.00$); (f) media of intervention delivery for each intervention (phone call, mobile app, computerized program, prerecorded videos, e-mail, video calls, instant messaging, text messaging, long-form written materials such as booklets, and long-form written feedback on activities such as worksheets; $\kappa = 1.00$); and (g) presence of synchronous (i.e., real-time) contact ($\kappa = .80$). Importantly, many studies used multiple media for intervention delivery, thus, it was not possible to compare these methods to each other directly; instead, the authors directly compared the effects of interventions that did and did not use each medium.

Assessment of Study Quality and Risk of Bias

Study quality variables were assessed using four indicators outlined in Cochrane's risk of bias tool and used in recent meta-analyses of psychotherapy outcomes (Cuijpers et al., 2020). The tool was used to assess studies for (a) adequate (i.e., unbiased, blinded) generation of allocation sequence ($\kappa = .58$), (b) adequate concealment of allocation sequence from the researchers ($\kappa = 1.00$), (c) adequate method of handling incomplete outcome data (any analyses using an intention-to-treat approach, in which all participants initially randomized were included in analyses, were defined as adequate; $\kappa = .67$), and (d) adequate blinding of assessors ($\kappa = 1.00$). Two independent assessors coded each study for these indicators, and disagreements were resolved through discussion.

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Data Analyses

All ES calculations for psychiatric symptoms were conducted by two trained research assistants, and disagreements were resolved by the first and second authors. Hedge's *g* (to account for small sample bias) was calculated for each comparison between a remote therapy and a control at post or follow-up. Only treatment versus control comparisons were included in this meta-analysis, to avoid muddying ES estimates with psychotherapy versus psychotherapy comparisons.

To include multiple ESs from each study, robust variance estimation (RVE) was used to account for dependence between ESs from the same sample by correcting study standard errors. Because RVE only adjusts study standard errors, it relies on fewer distributional assumptions and requires less power than other approaches such as multilevel meta-analysis. The *robumeta* package in R (Fisher et al., 2017) was used to conduct primary and moderator analyses and analyses for publication bias. Using RVE, weighted, random-effects models to were used to account for between-study variance. Although 40 studies are generally considered sufficient for a fully powered RVE analysis (Hedges et al., 2010), a small sample size correction was still used. Additionally, because there is no omnibus test that uses the RVE method, and past meta-analyses using RVE have often not employed omnibus tests (e.g., Stevens et al., 2018), for all moderator variables with more than two categories, pairwise comparisons were conducted against a reference group instead of conducting omnibus tests.

Table 1
Demographic and Logistical Moderators of Posttreatment

Moderator of effect size	*N* comparisons (*N* effect size)	ES (*g*)	*SE*	95% CI	*df*	*p*
Average age						.827
≤11	30 (136)	0.48***	0.1	[0.28, 0.68]	28.6	
>11	13 (57)	0.42	0.27	[−0.13, 0.98]	22.1	
Majority gender female						.325
Male	22 (98)	0.58**	0.15	[0.27, 0.89]	20.6	
Primary contact	20 (92)	0.38*	0.2	[−0.03, 0.79]	39.2	
Youth	17 (74)	0.17	0.19	[−0.21, 0.54]	30.9	.06
Caregiver	18 (65)	.53***	0.07	[0.38, 0.68]	16.4	
Majority ethnicity						.954
Caucasian	16 (64)	0.47*	0.23	[−0.01, 0.93]	30.6	
Not reported	27 (129)	0.46***	0.11	[0.25, 0.70]	25.7	
Study location						.082
Outside the United States	34 (157)	0.53***	0.12	[0.28, 0.78]	32.5	
Inside the United States	9 (36)	0.23	0.16	[−0.12, 0.57]	12.5	
Control type						.229
Active control	12 (49)	0.25	0.21	[−0.20, 0.71]	10.8	
Waitlist	31 (144)	0.55***	0.24	[0.05, 1.04]	19.8	
Logistical contact						.023*
Yes	21 (98)	0.24*	0.19	[−0.14, 0.62]	40.2	
No	22 (95)	0.68***	0.16	[0.34, 1.02]	20.7	
Reminders						.547
Yes	18 (72)	0.40***	0.19	[0.02, 0.78]	37	
No	25 (121)	0.52**	0.16	[0.18, 0.85]	23.6	
Therapeutic provider contact						.024*
Yes	25 (115)	0.64***	0.18	[0.28, 0.99]	36.1	
No	18 (78)	0.22*	0.09	[0.02, 0.42]	16.8	

Note. Effect size (ES) calculated with RVE with small-sample adjustment. *SE* = standard error; *g* = Hedges' g; CI = confidence interval; *df* = degrees of freedom; *p* = *p*-value.
* $p < .05$. ** $p < .01$. *** $p < .001$.

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Results

Study Selection and Inclusion

After screening for duplicate articles in PsycINFO and PubMed, trained research assistants reviewed every article abstract for inclusion, and retrieved 5,054 full-text studies for further double-screening. In total, 5,015 of these articles were excluded and two did not provide enough information to extract effect sizes (see Figure 1), leaving 43 treatment-control comparisons in 37 articles which were included in the analyses.

Characteristics of Included Studies

For detailed study characteristics and references for all included studies (see Table S1 in the online supplemental material). For a brief qualitative description of each intervention (see Table S2 in the online supplemental material). Mean youth age was 9.38 (*SD* = 4.19), mean duration of interventions was 9.14 weeks (*SD* = 4.28 weeks), and 58.14% included therapeutic provider contact. Of included studies, 39.53% targeted anxiety, 9.30% depression, 23.26% ADHD, 23.26% conduct problems, and 4.65% multiple externalizing problems. Additionally, 39.53% of studies reported the youth was the primary contact and 41.86% reported the caregiver was the primary contact; in other studies, the primary contact was unclear. Study quality indicators were as follows: Of the studies, 67.44% reported adequate random generation of allocation sequence, 58.14% reported adequate concealment of allocation sequence, 55.81% reported blindness of study assessors, and 66.12% reported adequate handling of missing data (i.e., used an intention-to-treat approach, including all those who were initially randomized in analyses).

Overall Remote Therapy Effect

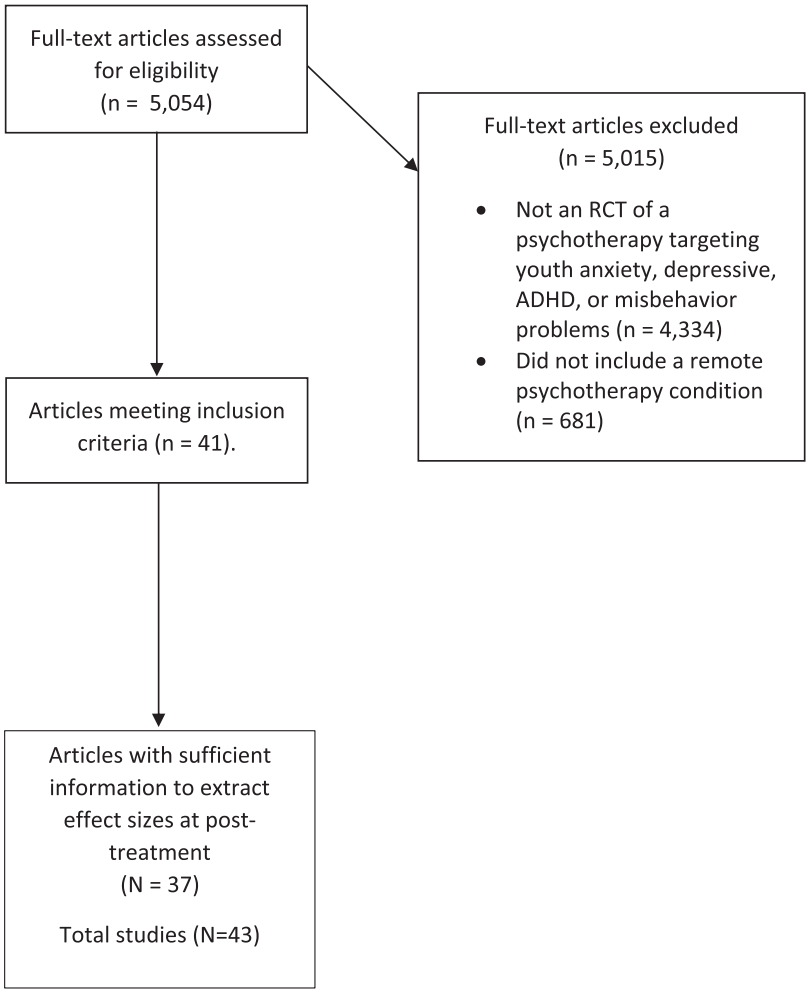
At immediate posttreatment, and separately at follow-up, correlated-effects, small-sample-corrected models were used to analyze overall effects of remotely delivered interventions compared to control conditions. The posttreatment model included 191 effect sizes and revealed an overall pooled effect size of .47 (95% CI [.26, .67], $p < .001$). Pooled effect size at follow-up (46 effect sizes; *M*[*SD*] weeks postbaseline = 48.58 [.20–.26]) was .44 (95% CI [.12, .76], $p < .05$). I^2 statistics revealed that 90.17% of variance was between studies rather than due to chance at posttreatment and 95.62% at follow-up. Thus, moderator analyses included posttreatment-only data to ensure as much statistical power for those analyses as possible. Details of moderator analyses are reported in Tables 1 through 4, and findings are summarized in the following text.

Moderation by Therapeutic Provider Contact

The presence of any therapeutic contact with a provider served as a significant moderator ($p = .024$). Studies with therapeutic provider contact had an overall ES of .64 (95% CI [.28, .99]), whereas studies without therapeutic provider contact had a significantly lower overall ES of .22 (95% CI [.03, .42]). Among interventions that included therapeutic provider contact, skill building (i.e., teaching, practicing, or reviewing therapeutic skills) was a significant moderator, with greater ESs for studies with therapeutic provider contact that included skill building ($p = .025$), the ES for these studies was estimated at .68 (95% CI [.25, 1.10]), whereas studies without this element had an ES of .18 (95% CI [.15, .51]). The moderator model with small sample correction testing presence versus absence of implementation support (i.e., troubleshooting difficulties implementing skills or principles taught in therapy) had too few degrees of freedom (<4) to be interpreted. An exploratory analysis using RVE without the small sample correction suggested that implementation support was significantly associated with a larger ES ($p = .0001$). The ES for studies with implementation support in this uncorrected model was .80 (95% CI [.39, 1.20]), compared with the ES of –.11 for studies without this component (95% CI [–.25, .04]).

The percentage of the total intervention involving contact with a provider was a significant moderator, with those interventions involving less than 25% contact showing significantly lower ESs (.17, 95% CI [.01, .33]) than those with between 25% and 50% contact (.86, 95% CI [.28, 1.42], $p = .022$), and significantly less than those with over 50% contact (.70, 95% CI [.23, 1.17], $p = .029$). The frequency of contact between providers and

Figure 1
Study Inclusion Flowchart



Note. RCT = randomized controlled trial; ADHD = attention-deficit/hyperactivity disorder.

participants and the presence of motivational enhancement were not significant moderators.

Moderation by Media

Media used to administer treatment was assessed as a moderator; these results are displayed in Table 3. Therapy administered using the phone had a significantly higher ES than therapy not administered using the phone (p = .036). The estimated effect size for therapies using the phone was .65 (95% CI [.28, 1.03]), whereas the ES for nonphone administered therapy was .25 (95% CI [.05, .45]). Using computer-based programs, e-mail, written text, long-form feedback, and prerecorded videos did not significantly moderate ES. Moderators accounting for delivery via instant messaging (df = 2.3) and text message (df = 1.1) had too few degrees of freedom, and the moderator accounting for mobile app use included only one study.

Moderation by Intervention and Control Content

Presence of attention or working memory training was a significant moderator of ES. ESs for the seven interventions with attention/working memory training (–.18, 95% CI [–.51, .16]) were significantly (p = .001) lower than for interventions without attention/working memory training (.60, 95% CI [.39, .82]).

Significant differences emerged between studies with different therapy types. In a model using youth-focused behavior therapy (YFBT; including cognitive-behavioral therapy) as the reference group, studies of YFBT performed significantly (.59, 95% CI [.22, .96], p = .008) better than studies of therapies coded as other (i.e., neither cognitive nor

Table 2
Therapeutic Contact, Contact Media, and Therapy Media Moderators Calculated With RVE With Small-Sample Adjustment

Moderator of effect size	*N* comparisons (*N* effect size)	ES (*g*)	*SE*	95% CI	*df*	*p*
Therapeutic contact						
Motivational enhancement	13 (65)	0.68*	0.32	[0.03, 1.33]	22.7	.831
No M.E.	12 (50)	0.61***	0.14	[0.32, 0.91]	10.9	
Skill building	17 (68)	0.68***	0.19	[0.25, 1.10]	10.9	.025*
No S.B.	5 (39)	0.18	0.13	[−0.14, 0.51]	5.9	
Synchronous contact						
Yes	24 (108)	0.67***	0.18	[0.31, 1.03]	38.1	.014*
No	19 (85)	0.21*	0.09	[0.03, 0.39]	17.8	
Therapy media						
Phone	23 (106)	0.65***	0.19	[0.28, 1.03]	39.6	.036*
Not phone	20 (87)	0.25*	0.1	[0.05, 0.45]	18.7	
Computer	27 (126)	0.44**	0.18	[0.08, 0.81]	31.8	.726
Not computer	16 (67)	0.51***	0.1	[0.30, 0.71]	14.8	
Email	10 (48)	0.62***	0.15	[0.31, 0.94]	15	.189
Not email	33 (145)	0.42**	0.13	[0.16, 0.68]	31.5	
Prerecorded video	19 (79)	0.65**	0.2	[0.24, 1.06]	39	.093
No video	24 (114)	0.30**	0.11	[0.09, 0.52]	22.7	
Written text	16 (132)	0.69**	0.24	[0.23, 1.15]	30.7	.123
Not via written text	27 (61)	0.34**	0.11	[0.11, 0.56]	25.8	
Long-form feedback	7 (36)	0.49*	0.22	[0.00, 0.98]	8.54	.884
Not long-form feedback	36 (157)	0.46***	0.12	[0.23, 0.70]	34.45	
Other content		−0.18				
Attention/working memory training[a]	7 (30)		0.14	[−0.51, 0.16]	8.29	.001***
No attention/working memory training	35 (169)	0.60***	0.11	[0.39, 0.82]	33.45	

Note. ES = effect size; *g* = Hedges' g; *SE* = standard error; CI = confidence interval; *df* = degrees of freedom; *p* = *p*-value; RVE = robust variance estimation; M.E. = motivational interviewing; S.B. = skill building.
[a] One of these studies was about attention-bias training for anxiety, so, for a sensitivity analysis, this study was removed from the study pool. In this sensitivity analysis, the results held up ($p < .001$), with a pooled ES of .60 for studies without attention/working memory training, compared to a pooled ES of –.23 for studies with attention/working memory training.
* $p < .05$. ** $p < .01$. *** $p < .001$.

cognitive-behavioral; –.05, 95% CI [–.50, .40]). In another model with caregiver-focused behavior therapy (CFBT) as the reference group, CFBT performed significantly better (.69, 95% CI [.40, .99], $p = .001$) than studies of therapies coded as other. No other significant differences emerged between the therapy types. Active versus waitlist controls and treatment reminders were not significant moderators.

Moderation by Demographic Factors

Mean patient age, majority gender, patient ethnicity, and location of the study (insides vs. outside the United States) were not significant moderators. Primary contact (youth vs. caregiver) was also not a significant moderator.

Moderation by Problem Type

Significant differences emerged between studies targeting different problems. First, ADHD was set as the reference group in a model comparing ESs for studies targeting other problems to studies targeting ADHD. Overall ES for studies targeting ADHD was –.03 (95% CI [–.32, .26]), significantly lower than for studies targeting anxiety (.62, 95% CI [.16, 1.07], $p = .008$) and conduct problems (.78, 95% CI [.28, 1.28], $p = .003$). Studies targeting ADHD did not have a significantly different average ES from those targeting depressions ($p = .653$). Assessing studies targeting multiple problems was not possible because there were too few degrees of freedom (*df*s < 4). When depression was set as the reference category, the model could not be interpreted because of too few degrees of freedom (*df*s < 4). Setting conduct and anxiety as the reference groups revealed no new significant differences (i.e., the only significant differences in these models were between ADHD and anxiety and between ADHD and conduct).

Significant differences also emerged between measures of different target problems. In a model using ADHD as the reference group against which the other groups were compared, overall ESs for measures of ADHD (–.03, 95% CI [–.32, .26]), which were significantly lower than for measures of anxiety (.60, 95% CI [.16, 1.04], $p = .008$), conduct (.78, 95% CI [.34, 1.21], $p = .001$), and measures of multiple internalizing problems (.87, 95% CI [–.02, 1.76], $p = .048$). Assessing measures of depression was not possible because there were too few degrees of freedom (*df*s < 4). measures of anxiety, multiple internalizing problems, and conduct were each set as the reference group in the model, no other significant differences emerged.

Moderation by Contact

Logistical contact (e.g., technical support) during treatment was a significant moderator, with studies including this form of contact demonstrating a significantly smaller effect size than those without it ($p = .023$). The ES for studies without this

Table 3
Effect Size (ES) Moderators With 3+ Levels Assessed via RVE With Small-Sample Adjustment

Moderator of effect size	*N* comparisons (*N* effect size)	ES (*g*)	*SE*	95% CI	*df*	*p*
Target problem						
ADHD[a]	10 (38)	−0.03	0.13	[−0.32, .26]	8.93	
Anxiety	17 (92)	0.62**	0.22	[0.16, 1.07]	18.72	.008**
Conduct	10 (49)	0.78**	0.24	[0.28, 1.28]	17.74	.003**
Depression	4 (10)	0.09	0.25	[−0.54, 0.72]	5.74	.653
Assessment						
ADHD[a]	(38)	−0.03	0.13	[−0.32, 0.26]	8.91	
Anxiety	(86)	0.60**	0.21	[0.16, 1.04]	19.09	.008**
Conduct	(53)	0.78***	0.21	[0.34, 1.21]	18.76	.001**
Multiple internalizing	(6)	0.87	0.35	[−0.02, 1.76]	5.43	.048*
Therapy type						
YFBT[a, b]	17 (92)	0.59**	0.17	[0.22, 0.96]	15.7	
CFBT[b]	15 (56)	0.69***	0.22	[0.24, 1.15]	29.1	.648
Other	10 (43)	−0.05	0.22	[−0.50, 0.40]	18.4	.008**
Percentage of intervention involving provider contact						
<25%[a]	23 (109)	0.17*	0.08	[0.01, 0.33]	21.5	
≥25%, <50	11 (45)	0.85**	0.27	[0.28, 1.42]	19.4	.022*
≥50%	9 (39)	0.70**	0.22	[0.23, 1.17]	14.9	.029*
Scheduled contact frequency						
<Once per week[a]	10 (36)	0.42	0.33	[−0.33, 1.17]	8.84	
Once per week	20 (94)	0.54	0.35	[−0.20, 1.29]	17.57	.732
>Once per week	12 (57)	0.37	0.35	[−0.37, 1.11]	18.98	.892

Note. YFBT = youth-focused behavioral therapy; CFBT = caregiver-focused behavioral therapy; *g* = Hedges' g; *SE* = standard error; CI = confidence interval; *df* = degrees of freedom; *p* = *p*-value; RVE = robust variance estimation.
[a] Indicates reference group. [b] Behavioral therapy encompasses cognitive behavioral approaches.
* $p < .05$. ** $p < .01$. *** $p < .001$.

contact was .68 (95% CI [.34, 1.02]), whereas the ES for studies with this contact was .24 (95% CI [–.14, .62]). Synchronous (i.e., real-time) contact was also a significant ($p = .014$) moderator of ES. The ES for studies with synchronous contact was .67 (95% CI [.31, 1.03]), which was higher than the ES for studies without of .21 (95% CI [.03, .39]).

Moderation by Duration

Duration of treatment, measured by the number of weeks for which the treatment protocol was scheduled, was a marginally significant moderator of effect size. Each additional week of treatment protocol was marginally significantly associated with an ES increase of .03 (95% CI [–.006, .07], $p = .086$).

Publication Bias

Because funnel plots are not ideal for use with very dependent data sets, the funnel asymmetry test and precision effect test (FAT-PET) test was used to assess for publication bias (Stanley & Doucouliagos, 2014). The results of the FAT-PET test for publication bias are displayed in Table 4. Using only the posttreatment ESs, both components of the FAT-PET test were insignificant ($ps > .2$), indicating that publication bias was not detected. Using the ESs from posttreatment and follow-up jointly, both elements of the FAT-PET test were again insignificant ($ps > .2$). Thus, publication bias was not detected.

Outlier Detection and Sensitivity Analyses

As in other recent RVE meta-analyses (Zelinsky & Shadish, 2018), the Grubbs test (G) was used to identify outliers. The G for outliers revealed that there were two outlier ESs (G = 7.31, $p = .003$). One of these outliers was an ES of 6.26 (Wuthrich et al., 2012), the other an ES of –1.62 (Waters et al., 2013). These could not be

Table 4
Parameter Estimates From the RVE-Modeled FAT-PET Test of Publication Bias

Test statistic	*B*	*SE*	95% CI	*df*	*p*
Posttreatment only					
FAT	−0.23	1.54	[−3.47, 3.00]	17.71	.882
PET	0.52	0.46	[−0.54, 1.58]	7.96	.287
Posttreatment and follow-up					
FAT	−0.10	1.36	[−2.97, 2.77]	16.73	.941
PET	0.48	0.40	[−0.46, 1.42]	7.58	.268

Note. *B* = beta; *SE* = standard error; CI = confidence interval; *df* = degrees of freedom; *p* = *p*-value; RVE = robust variance estimation; FAT-PET Test = funnel asymmetry test and precision effect test.

accounted for by data entry errors, nor by any other issues (e.g., floor or ceiling effects), meaning these ESs likely did not need to be removed from the dataset. Nonetheless, for a sensitivity analysis, these two ESs were removed from the dataset and models were run of overall ES at posttreatment and follow-up. Both ESs remained significantly different from zero and had nearly the same values (.48 at posttreatment and .44 at follow-up). For another sensitivity analysis, the one attention/working memory training study which focused on attention bias in anxiety was removed from the study pool to reanalyze presence of attention/working memory training as a moderator of ES. In this sensitivity analysis, the results held up ($p < .001$), with a pooled ES of .60 for studies without attention/working memory training, compared to a pooled ES of –.23 for studies with attention/working memory training. Finally, ES for studies with all risk of bias criteria coded as low were compared in a moderator analysis to ES for studies with at least one source of elevated risk of bias; there was not a significant difference between these study types ($p = .48$).

Tests of Moderator Interaction

Interaction effects were examined for all pairs of significant moderators for which there were at least five ESs in each cell. Only one of these, testing the interaction between logistical contact and therapy delivery via phone, revealed a significant interaction effect ($p = .025$). In this model, the interaction effect was dismantled from both possible directions. First, the effects of logistical contact were examined among interventions with and without therapy delivered via phone. Among interventions which were delivered at least in part via phone, presence versus absence of logistical contact did not have a significant effect on ES ($p = .621$). However, among interventions in which no part of the intervention was delivered via phone, those with logistical contact had a significantly lower ($p = .009$) overall ES (.14, 95% CI [–.39, .68]) than those without logistical contact (.89, 95% CI [.43, 1.34]). Then, the effects of phone therapy were examined among interventions delivered with and without logistical contact. Among interventions without logistical contact, presence versus absence of intervention delivery via phone was not a significant moderator ($p = .588$). For therapies with logistical contact, however, those including intervention delivery via phone showed significantly greater ESs (.89, 95% CI [.29, 1.44], $p = .015$) than those without intervention delivery via phone (.18, 95% CI [–.12, .49]).

Discussion

At posttreatment, remotely-delivered youth psychotherapies produced on average a medium effect (.47). Youth psychotherapies overall produce comparable pooled ESs of .46 to .50 according to recent meta-analyses (Weisz et al., 2017; Weisz et al., 2019)—this despite the fact that the mean duration of tested psychotherapies has been markedly longer than remote therapies (15.81 weeks in Weisz et al., 2017 vs. 9.5 weeks in the present meta-analysis). At follow-up, the pooled ES for remote youth psychotherapies (.44) appears similar to, or possibly slightly higher than, the pooled ES for mostly in-person youth psychotherapies (.36; Weisz et al., 2017), even though the follow-up period was somewhat longer for remote therapies (48.58 vs. 44.26 weeks). There are several possible explanations for why remote psychotherapies may outperform in-person psychotherapies at follow-up. For instance, youths might be more likely to engage with therapeutic content after completing a remote intervention, as most remote therapy content is online and thus available even after formal therapy ends. Moreover, learning and practicing therapeutic skills at home may help youths and families more readily integrate the skills into their daily lives. However, it is also possible that studies of remote therapy differ from studies of in-person therapy in important ways (e.g., targeted problems; specific interventions) that contribute to artifactual advantages in follow-up outcomes.

Remote psychotherapies with therapeutic provider contact significantly outperformed those without therapeutic provider contact. Relatedly, remote psychotherapies in which over 25% of the intervention involved provider contact significantly outperformed remote psychotherapies in which less than 25% involved provider contact. Overall, remote youth psychotherapies appear more effective when they incorporate meaningful contact with a provider. This finding aligns with adult psychotherapy meta-analyses (Andersson & Cuijpers, 2009; Linardon et al., 2019). Therapeutic provider contact was more helpful when it involved skill-building and discussing implementation difficulties the youths and caregivers were experiencing. Surprisingly, providing reminders and using motivational enhancement strategies was not associated with larger effects.

Importantly, some youth psychotherapies delivered via technology without any therapeutic provider support have been effective (e.g., Osborn et al., 2020; Schleider & Weisz, 2018). Thus, it will be important to investigate why some unsupported interventions have weak effects and others work well. It is possible, for example—based on content of some successful unsupported interventions—that designing interventions to be quite brief (e.g., one session) may reduce drop out, adding branching choices may increase personalization, and incorporating personal stories may help substitute for personal provider contact.

In line with past meta-analyses (Weisz et al., 2017), studies targeting ADHD had significantly lower ESs than studies targeting anxiety and conduct, and measures of ADHD showed significantly lower ESs than measures of anxiety, conduct, and multiple internalizing problems. There were too few studies targeting depression to formally analyze

depression in these models, yet, consistent with past meta-analyses, pooled ES for depression appeared meaningfully lower than for anxiety and conduct problems (Weisz et al., 2017).

Interestingly, studies with attention/working memory training showed significantly lower ESs than studies without. This finding aligns with a recent meta-analysis of attention/working memory training for typically developing children, which found that it produces reliably null effects; the authors recommended that no further resources be devoted to working memory training (Sala & Gobet, 2020). Another meta-analysis, however, found that attention/working memory training produced small but significant positive effects, and suggested that attention training might be more effective for youths and people with ADHD (Peng & Miller, 2016). While findings on this question remain inconclusive, the present meta-analysis provides no support for the efficacy of remotely delivered attention/working memory training. Relatedly, youth-focused behavioral therapy and caregiver-focused behavioral therapy showed markedly larger ESs than other types of therapies. Notably, of nine interventions using "other" types of therapies seven involved attention/working memory trainings.

Finally, several surprising moderators of ES should be examined in future meta-analyses with greater power to detect interaction effects among moderators: studies with logistical contact had significantly lower ESs than those without, and studies with phone contact showed significantly greater ESs than those without. No other media of contact significantly moderated effectiveness. It seems unlikely that logistical contact (e.g., tech support, scheduling) itself makes interventions less effective, though it is possible that programs requiring more technical support were more confusing or frustrating, leading to lower treatment compliance and efficacy. It is also possible that contact with staff without mental health training (e.g., tech support personnel) contributed to reduced effectiveness of interventions with logistical contact. It also seems unlikely that use of a phone itself makes interventions more effective, although the personal touch of phone communication may boost therapeutic alliance and efficacy. As discussed in limitations, artifactual explanations should be considered for both findings.

There are several limitations of this meta-analysis. Although the RVE method allows us to achieve much higher power for analyses than would a multilevel modeling approach, the sample size was still likely too small to detect most significant interactions among moderators and also may have been too small to detect significance of some moderator effects. These limitations make it difficult to disentangle potential confounds among variables—a common challenge in meta-analyses, and a challenge that is best addressed by the accumulation of larger study pools over time. As noted previously, it seems especially likely that the analyses of phone contact and logistical contact suffered from confounding. For example, there may be more need for logistical contact when technical issues are present, and such issues might hamper uptake of intervention content. Additionally, studies including therapeutic provider contact may be less likely to report logistical provider contact. Similarly, using a phone involves personal contact with a provider and is a common form or therapeutic contact, which is associated with larger ES. Future research should aim to disentangle these effects, discerning appropriate psychological explanations.

Another limitation is that there were too few studies using certain media (i.e., video-chat, instant messaging, and text messaging) to analyze ESs for studies using these media. Although several randomized trials have examined video-chat treatments (e.g., Comer et al., 2017), these studies have randomized participants to either video-chat treatment or a supported office-based comparison treatment (i.e., they have not included a control condition), and thus did not meet the present selection criteria. Accordingly, video-chat programs will need to be evaluated separately. More broadly, communication via technology is evolving rapidly, so the landscape of remote therapies may look quite different quite soon, necessitating an evidence-base update. Finally, some may wish to know the effects of RT for even younger children, so it may be valuable to conduct meta-analyses of psychotherapies targeting all ages, including infants.

The findings of this meta-analysis may inform hypotheses about how to optimize remote therapies. For example, researchers and clinicians may test whether effects of remote therapies are strengthened by incorporating therapeutic provider contact, teaching or reviewing skills, discussing implementation difficulties during therapeutic discussions, and prioritizing therapeutic over logistical contact Additionally, future research might focus on alternative ways of addressing ADHD remotely, given the negligible effects of remote therapies tested in RCTs on ADHD symptoms. Relatedly, a case can be made for increased development and testing of remote therapies for depression and for multiple problems, as, to date, there have been relatively few studies addressing depression or multiple problems, making it difficult to assess effects.

Overall, this meta-analysis of remotely-delivered youth psychotherapies indicates that those tested thus far are comparably effective to in-person psychotherapies for youths, both at posttreatment and at follow-up. Thus, remote therapies appear to be a promising option for youth mental health care during the COVID-19 pandemic, and in other situations and contexts where in-person intervention may not be feasible. About 90% of mental health resources worldwide are allocated to high-income countries, and about 200 times more providers are

available in high-income countries than in low- and middle-income countries (Patel, 2007). Remote therapies may help improve access to care in low- and middle-income countries and other low-resource regions (e.g., Osborn et al., 2020). Beneficiaries of remote therapy might include youths in contexts that make travel to a clinic infeasible and youths living in areas where provider organizations are not easily accessible, including low-resource and rural regions. Remote therapies appear especially effective when they incorporate therapeutic provider contact, and when they address anxiety and conduct problems. Together, the findings of this meta-analysis provide encouraging evidence that remotely-delivered youth psychotherapies could broaden access to effective, evidence-based treatments for youth in need.

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